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Expanding Public Early Childcare: Effects on Maternal Employment and Desired Working Hours in Germany

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Abstract

This study investigates how an increase in subsidized early childhood care influences maternal participation in the labor market. It extends existing research by considering not only employment rates but also changes in both agreed-upon working hours and preferred working hours. Using semi-parametric difference-in-differences (DiD) techniques applied to German Microcensus survey data, the analysis finds positive impacts on employment as well as on agreed and preferred working hours, reaching up to 20% of the pre-reform mean. Since agreed and preferred hours tend to adjust together, expanding early childhood care can unlock labor potential beyond that of mothers who are currently underemployed. Conditional analyses indicate that the reform particularly affects mothers with higher education and those who are not single.

Keywords: Early childhood care, Maternal labor supply, Semi-parametric difference-in-differences, Subsidized childcare, Working hour preferences

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Introduction

Employment levels and working hours in developed nations show pronounced gender differences, often shaped by family circumstances [1]. Male labor trajectories tend to be less influenced by life events, whereas women frequently reduce work hours or temporarily exit the labor force after childbirth [2]. Policymakers therefore advocate expanding public childcare to enhance employment potential, especially in aging societies. Countries with well-established childcare systems, such as Scandinavian nations, tend to have higher female employment rates [3]. Nonetheless, empirical evidence on the causal effects of subsidized childcare on women's employment is mixed. This paper evaluates not only the impact of affordable childcare on employment rates and agreed weekly hours but also sheds light on mothers' underlying preferred working hours.

Discrepancies between agreed and preferred working hours are common in industrialized economies [4-10]. Assessing whether subsidized childcare can reduce such mismatches is important, as aligning working hours with preferences benefits labor market participation and overall wellbeing [5, 11]. Investigating preferred hours alongside actual hours provides insight into the motivations behind labor supply decisions. While short hours can contribute to the gender wage gap [12], they may also reflect a voluntary choice by mothers seeking greater work-life balance.

In 2008, Germany introduced legislation to expand subsidized care for children aged one to three (Kinderförderungsgesetz, KiföG), establishing a legal right to a childcare spot from August 2013 onward. This paper exploits the exogenous variation created by the 2013 reform, comparing districts with substantial increases in childcare coverage (treated/high-expansion) to those with minor changes (control/low-expansion). The methodology follows Bauernschuster *et al.* [13], Felfe *et al.* [14], and

Havnes and Mogstad [15], who used regional differences in childcare expansion across Germany, Spain, and Norway, respectively. Treatment and control groups are defined by the median percentage point change in coverage rates. The DiD approach compares labor market outcomes for mothers of children up to age three across districts with higher versus lower childcare growth before and after the reform. Unlike prior studies where initial coverage was close to zero in both groups [13-15], both groups in this setting already had some childcare availability, but districts with higher expansion experienced a "catch-up effect" by 2015. While earlier work analyzed prior phases of Germany's childcare expansion [13, 16], this paper focuses on 2013, the year when all children gained a legal entitlement to a childcare slot.

I investigate the German labor market as a case study for the persistence of conventional employment patterns. Around 25% of women employed part-time report that caring for children or dependent relatives is the primary reason for their work arrangement [17]. Therefore, the 2013 childcare reform had significant potential to increase female labor participation on both the extensive and intensive margins, particularly for mothers who were previously underemployed and may have expanded their agreed working hours.

Rather than using a standard linear OLS model, I employ a two-stage semi-parametric difference-in-differences (DiD) approach suggested by Abadie [18]. This method relaxes the linearity assumption in the outcome equation and allows exclusion of observations that lack overlap in covariates. It also enables estimation of heterogeneous treatment effects. The analysis uses rich data from the German Microcensus, a 1% representative sample of households [19, 20]. The repeated cross-sections provide information on household composition, socio-economic characteristics, and employment, making it possible to analyze both over- and underemployment as well as individual preferences for working hours.

The intention-to-treat estimates reveal positive effects across both employment margins. In districts with large increases in childcare coverage, mothers of children aged up to three experience a 5.7 percentage point higher employment rate after the reform compared to districts with smaller coverage increases. Both agreed and preferred working hours rise by approximately five hours per week on average, changing in tandem so that the mismatch between them remains unchanged. The effects are stronger for mothers with higher education levels, and the alignment between agreed and preferred hours varies for cohabiting mothers.

The paper is structured as follows: Section 2 reviews the literature on childcare and maternal employment. Section 3 outlines the institutional background of the German childcare system, details the 2013 reform, and presents the data and estimation strategy. Section 4 reports the results, and Section 5 concludes.

Childcare access and Maternal Labor Supply

Identifying the causal impact of publicly subsidized childcare on maternal employment faces several challenges. One issue is that childcare costs and access to informal care within families are often inadequately recorded [15]. Another concern is that childcare availability and employment outcomes may be endogenous. As a result, many studies rely on quasi-experimental designs exploiting exogenous variation from policy changes or instrumental variables [3]. Findings differ widely across countries depending on pre-reform conditions, the population studied, and how childcare—public, private, or informal—is organized. The effects of more generous childcare provisions range from significantly positive to negligible or statistically insignificant.

Table 1 summarizes previous studies examining the impact of childcare availability or reduced costs on maternal employment, including the geographic context, methodology, and main findings.

Table 1. Main findings of evaluation studies on childcare and maternal employment

Article	Country/Region	Method	Key Findings
Andresen and Havnes [21]	Norway	DiD	Cohabiting mothers with children under 3 years show positive effects, with a shift towards full-time employment.
Baker *et al.* [22]	Quebec	DiD	Female employment rises by 7.7 percentage points.
Bauernschuster and Schlotter [23]	Germany	DiD	Eligibility for kindergarten is associated with a 36.6 percentage point increase in labor force participation and a 14.3-hour rise in average weekly work hours.
Berlinski *et al.* [24]	Argentina	RDD	Employment probability increases, including full-time work, with weekly hours rising by 7.8 if the youngest child attends kindergarten.
Berlinski and Galiani [25]	Argentina	DiD	Mothers of children aged 3–5 experience positive employment effects.
Fendel and Jochimsen [26]	Germany	DiD	Short-term positive effects on maternal labor force participation due to the August 2013 child care reform, including legal entitlement and home care allowances.
Fitzpatrick [27]	US	RDD	Kindergarten attendance positively affects single mothers' employment.
Gelbach [28]	US	IV	Public school enrollment increases employment rates and weekly hours for single mothers, with slightly smaller effects for married mothers.
Givord and Marbot [29]	France	DiD	Effects are near zero for mothers of preschool children, with stronger effects for larger families.

Havnes and Mogstad [15]	Norway	DiD	Minimal effects for mothers of children aged 3–6 years.
Lefebvre and Merrigan [30]	Quebec	DiD	Positive effects on maternal employment and working hours.
Lundin *et al.* [31]	Sweden	DiD	Effects close to zero; no significant differences across subgroups (child age, maternal education).
Müller and Wrohlich [16]	West Germany	DiD	Each 1 percentage point rise in child care slots is associated with a 0.2 percentage point increase in labor market participation, mainly via part-time employment and mothers with medium-level education.
Nollenberger and Rodríguez-Planas [32]	Spain	DiD	Maternal employment increases.
Schlosser [33]	Israel	DiD	Free public preschool raises employment for Arab mothers with children aged 3–4 by 8.1 percentage points and weekly hours by 2.8.

DiD: difference-in-differences; RDD: regression discontinuity design; IV: instrumental variable

Some mixed findings in earlier research may be due to neglecting mothers' underlying work-hour preferences. For example, Lundin *et al.* [31] and Givord and Marbot [29] observe little effect in countries with already high maternal employment, possibly because preferred and actual working hours largely coincide. In contrast, countries with lower female employment show positive responses to subsidized childcare, likely because underemployed women adjust agreed hours to better match their preferences. Several studies emphasize that work-hour preferences shift after major life events like childbirth [4, 34, 35]. Reynolds and Johnson [35] find that, in the US, the arrival of a first child reduces women's preferred hours more than their actual hours, with negligible effects for men. Drago *et al.* [4] also report that Australian women are more sensitive than men to such life changes. Zimmert and Weber [36] highlight that insufficient institutional childcare can contribute to mismatches between preferred and agreed hours. However, most previous studies do not directly examine the effects of subsidized childcare on maternal working hours or overlook the adjustment process between agreed and preferred hours.

Institutional Context, Methodology, Data, and Descriptive Overview

Institutional context

Germany's childcare landscape is marked by distinctive features, including considerable variation across regions and a diverse set of providers [37]. These differences are not limited to urban-rural divides but also reflect the historical division between the former East Germany (GDR) and West Germany. For instance, in 2016, the proportion of children in formal childcare reached 51.8% in East Germany but only 28.1% in the West [38].

Childcare provision is primarily organized at the municipal level, with over 11,000 municipalities contributing to pronounced disparities in availability and costs. The private childcare sector is relatively small, as stringent quality standards and high setup costs limit entry. In 2010, only 164 out of 1,386 institutions (around 12%) operated on a for-profit basis **(Figure 1)**. By 2015, the number of profit-oriented providers increased to 261, maintaining a similar share of roughly 13%. Non-profit organizations, frequently affiliated with religious groups and publicly subsidized, make up about two-thirds of all childcare facilities. Notably, publicly run institutions experienced the largest relative growth over the period covered **(Figure 1)**.

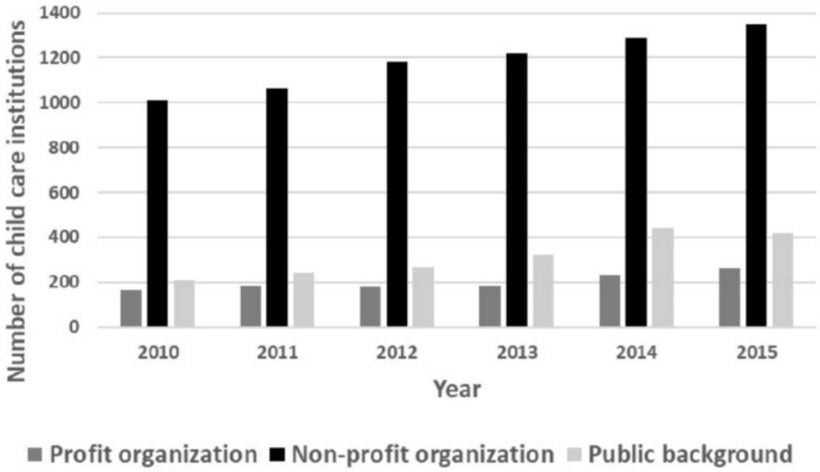

Figure 1 . Distribution of childcare institutions in Germany by provider type. Note: Data refer to March 1st of each year. Source: Federal Statistical Office [39, 40]

Early childcare expansion

The first major push to expand early childcare began in 2005 with a plan to create 230,000 additional childcare slots by 2010 under the Tagesbetreuungsausbaugesetz. This target was reinforced in 2007, aiming for a 35% coverage rate by 2013 (Krippengipfel). In 2008, the KiFöG law established a legal right to childcare for children aged 1–3 starting in August 2013, emphasizing child development rather than parental employment, similar to the 1996 kindergarten entitlement for children over two.

Funding and administration of childcare is handled at the municipal level, with financial support from federal states and the federal government. By 2014, total federal expenditure for expanding childcare reached 5.4 billion Euros, followed by an annual allocation of 845 million Euros beginning in 2015 [41].

Regional disparities persist, reflecting historical differences: in the former GDR, childcare was historically treated as a public responsibility, resulting in persistently high coverage rates. In 2011, 49% of children under three in East Germany attended subsidized childcare, compared to only 20% in the rest of the country [42]. The 2013 reform led to a substantial increase in childcare access: by 2015, 28.2% of children in West Germany and 51.9% in East Germany were enrolled in subsidized childcare [43].

Despite the five-year lead time before the legal claim became active, a projected shortage of 80,000–100,000 slots in July 2013 indicated nearly full utilization. Generally, early childcare provision is guided by available supply rather than parental demand [37, 41]. While municipalities consider population growth in planning, other factors influencing demand are largely ignored. **Table 2** presents the take-up ratios for several federal states with available statistics. By March 1st, 2013, most states reported ratios close to one. After the legal claim was implemented in 2014, ratios declined slightly, indicating alleviated scarcity, though considerable local variation remains, with many urban areas still experiencing undersupply.

Table 2. Take-up ratio of childcare

Federal State	Child Age Group	2013 Take-up Ratio	2014 Take-up Ratio
Baden-Wuerttemberg	0–3	0.942	0.879
Bavaria	0–3	0.977	0.872
Hamburg	All ages	0.849	0.802
Hesse	0–3	0.939	0.840
Mecklenburg-Vorpommern	0–3	0.968	0.983
Lower Saxony	0–3	0.895	0.864
North Rhine-Westphalia	0–3	0.946	0.876
Saarland	0–3	0.930	0.882
Saxony-Anhalt	All ages	0.881	0.880

Definition: Take-up ratio = actual enrollment ÷ authorized slots

Methodological framework

The 2013 childcare reform offers a quasi-experimental setting suitable for a difference-in-differences (DiD) analysis. Beyond time variation, the reform produced considerable spatial differences in the expansion of subsidized childcare, which are used to distinguish treatment and control groups. Following the methodology of Bauernschuster *et al.* [13], Felfe *et al.* [14], and Havnes and Mogstad [15], districts are divided based on the fourth and sixth deciles of the increase in childcare coverage for children under three. Thus, the treatment group represents a shift from lower to higher coverage rates, rather than from zero to available childcare. The resulting estimates correspond to intention-to-treat effects, as treatment assignment does not indicate whether children actually attended a slot. Since the German Microcensus has not recorded childcare attendance since 2005, it is not possible to directly link the estimates to actual usage. Additionally, as highlighted by De Chaisemartin and D'Haultfoeuille [44], Wald-type DiD estimators require strong identification assumptions. Nonetheless, the analysis can reliably indicate the direction of the reform's impact.

While a regression discontinuity design could exploit the reform's cutoff date, DiD estimation has the advantage of accounting for seasonal variation, which is particularly relevant in this context. Early childcare enrollment typically follows the school year, beginning in August or September. Older children are more likely to secure a spot, meaning that mothers with children born just before the cutoff are more likely to return to work at the start of the school year. For this reason, DiD is commonly preferred in studies evaluating German family policy reforms, such as parental leave changes, where cohort effects must be addressed [45-48].

The pre-reform period is defined as 2011, while the post-reform period is 2015, as this marks the largest observed increase in childcare slots following the legal entitlement [41]. Growth in coverage slows after 2015. Sensitivity checks using 2014 as the post-reform period yield consistent findings.

Mothers whose youngest child is under three and who reside in districts with an increase in coverage above the sixth decile (8.0 percentage points) between 2011 and 2015 constitute the treatment group. Those with children under three living in districts where coverage increased below the fourth decile (6.5 percentage points) are classified as the control group. Districts with intermediate increases or those affected by territorial reforms are excluded, leaving 317 districts in the analysis. While evaluating longer-term outcomes—for example, children who were three or older in 2015—could provide further insights, this study focuses on short-term impacts on mothers of younger children.

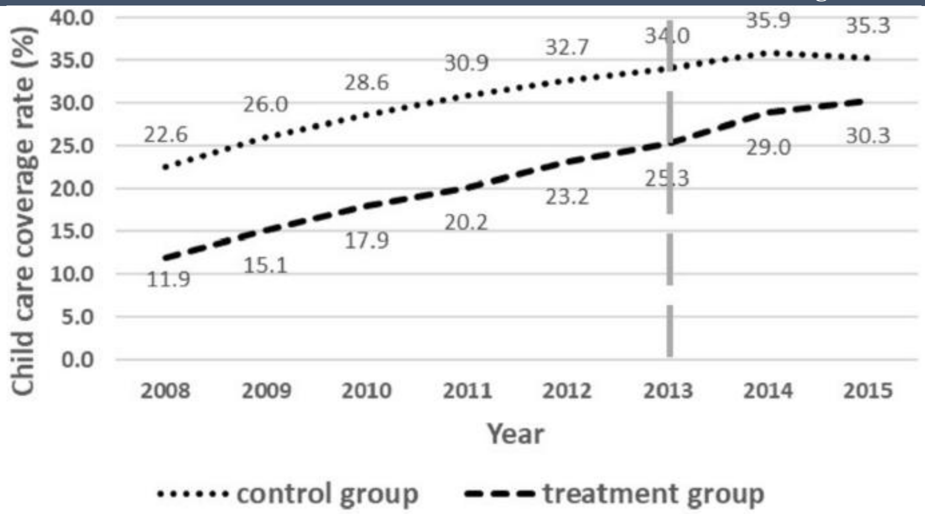

Figure 2. Trends in childcare coverage (%) for treatment and control districts. Notes: Coverage refers to children up to three in subsidized care relative to the respective birth cohort. Vertical line: reform year 2013. Source: Federal Statistical Office [39, 40, 42, 43, 49-63], own calculations

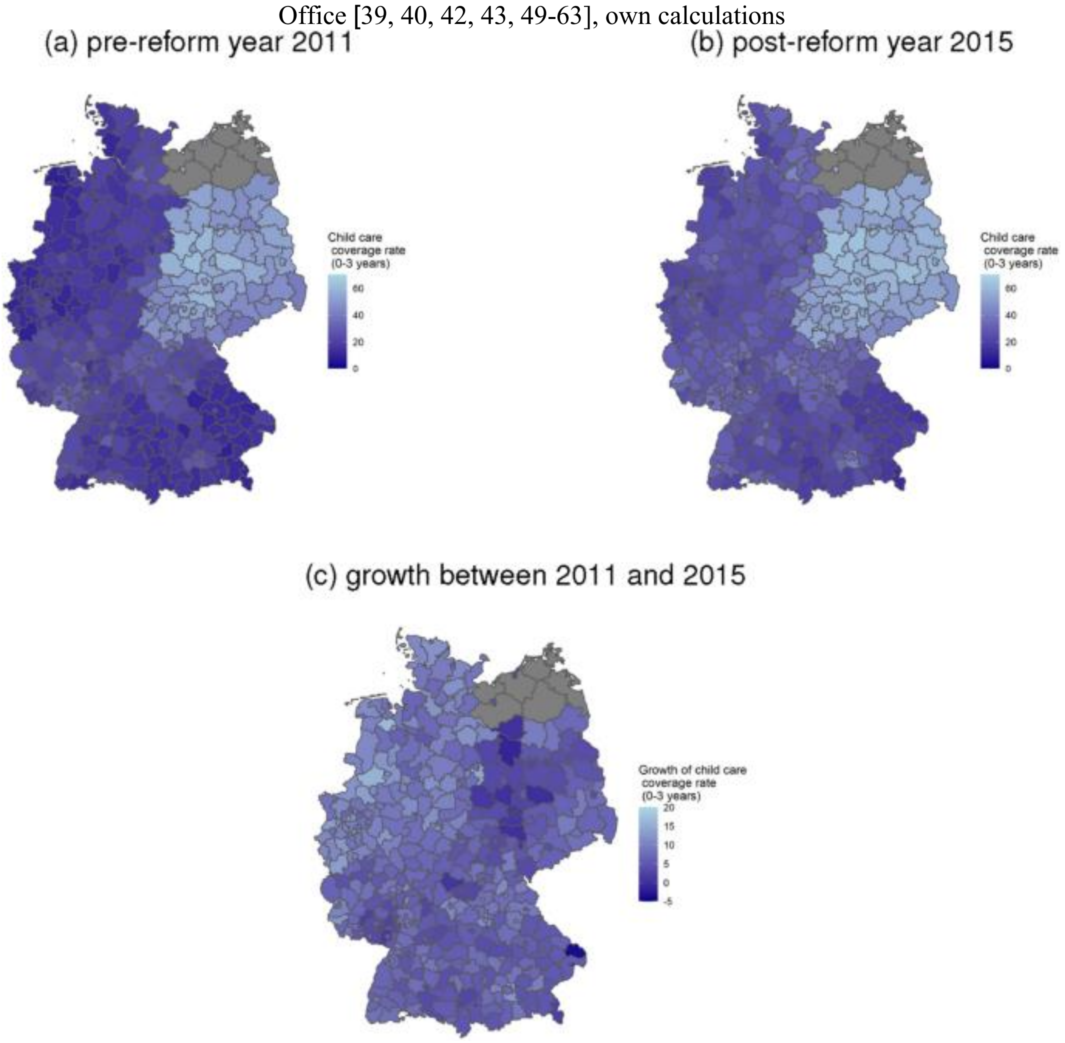

Figure 3. Childcare coverage for children under three across districts. Notes: Gray areas indicate districts affected by territorial reforms, which are excluded. Cutoff date: March 1st. Source: Federal Statistical Office [43, 52], author's illustrations

Figure 2 illustrates the trajectories of coverage rates in treated and control districts. Initially, low-expansion districts had higher coverage, but trends were nearly parallel until August 2013. From 2014 onward, the gap narrows, likely reflecting a catch-up effect in high-expansion districts, while low-expansion districts had less urgency to increase slots. Unlike earlier studies [13-15], where both groups started near zero coverage, in this setting both treatment and control groups already had established childcare systems, though districts with larger expansions started from lower initial levels through 2015.

The geographic variation that defines the treatment and control groups is shown in **Figure 3**. It displays district-level childcare coverage in 2011 and 2015, along with the percentage point increase over this period. Coverage is consistently highest in East

Germany, while the southern and northwestern regions of the country exhibit the lowest rates. Examining the changes between 2011 and 2015 reveals the largest expansions occurred in western districts, particularly in North Rhine-Westphalia, Lower Saxony, and parts of Baden-Württemberg near the French border.

Table 3 details how treated districts are distributed across the federal states. Most northern and western districts are part of the treated group, which experienced coverage growth above 8.0 percentage points. In contrast, southern districts show a less clear separation between treatment and control, while most East German districts fall into the control group with smaller increases. One might worry that the predominance of former GDR districts in the control group could bias results; however, a robustness check that excludes East German districts produces estimates similar to the full sample.

Table 3. Number of districts by treatment and federal state

Federal state	Treatment group	Control group
West Germany		
Baden-Wuerttemberg [64, 65]	11	20
Bavaria [66, 67]	25	50
Bremen	1	0
Hamburg [68, 69]	1	0
Hesse [70, 71]	10	7
Lower Saxony [72, 73]	31	6
North Rhine-Westphalia [74, 75]	47	1
Rhineland-Palatinate	8	21
Saarland [76, 77]	2	1
Schleswig-Holstein	12	0
East Germany		
Berlin	0	1
Brandenburg	3	13
Mecklenburg-Vorpommern [78, 79]	0	2
Saxony	3	6
Saxony-Anhalt [80, 81]	0	14
Thuringia	4	17

Average effects

The DiD approach compares the outcomes of districts exposed to the reform with those of unexposed districts, both before and after the reform takes effect. Identification of the average treatment effect on the treated (ATET) relies on several conditions: parallel trends in the absence of treatment, no anticipatory behavior, the stable unit treatment value assumption (SUTVA), and common support. These assumptions are described below.

Assumption 1 – Parallel Trends

$$\mathrm{E}[\mathrm{Y}^0(1)|\mathrm{D} = 1,\mathrm{X}] - \mathrm{E}[\mathrm{Y}^0(0)|\mathrm{D} = 1,\mathrm{X}] = \mathrm{E}[\mathrm{Y}^0(1)|\mathrm{D} = 0,\mathrm{X}] - \mathrm{E}[\mathrm{Y}^0(0)|\mathrm{D} = 0,\mathrm{X}] \quad (1)$$

Here, Y0(t)Y^0(t)Y0(t) represents potential outcomes without the reform at time T=tT=tT=t, with T=0T=0T=0 for 2011 (pre-reform) and T=1T=1T=1 for 2015 (post-reform). Y1(t)Y^1(t)Y1(t) is the potential outcome under the reform. DDD is the binary treatment indicator, and XXX represents a set of covariates. Including variables related to the mother, her household, and regional economic characteristics makes the parallel trends assumption more credible (see Sect. 3.3 for the complete list). As a further check, a placebo exercise is conducted by assuming the reform occurred in 2011. While this does not directly test parallel trends, it provides indicative evidence about their validity. **Figure 4** illustrates the unconditional means of key outcomes for the treatment and control groups between 2010 and 2015. Employment shares and agreed and preferred working hours follow similar trajectories for both groups prior to 2013, supporting the parallel trends assumption. For full- and part-time employment, pre-reform years (2011–2012) show particularly similar patterns.

Previous research [13, 16] has analyzed earlier phases of the German childcare expansion. Although appropriate for their contexts, a major parental leave reform in 2007 [82] might threaten the parallel trends assumption if its effects differed between treatment and control districts.

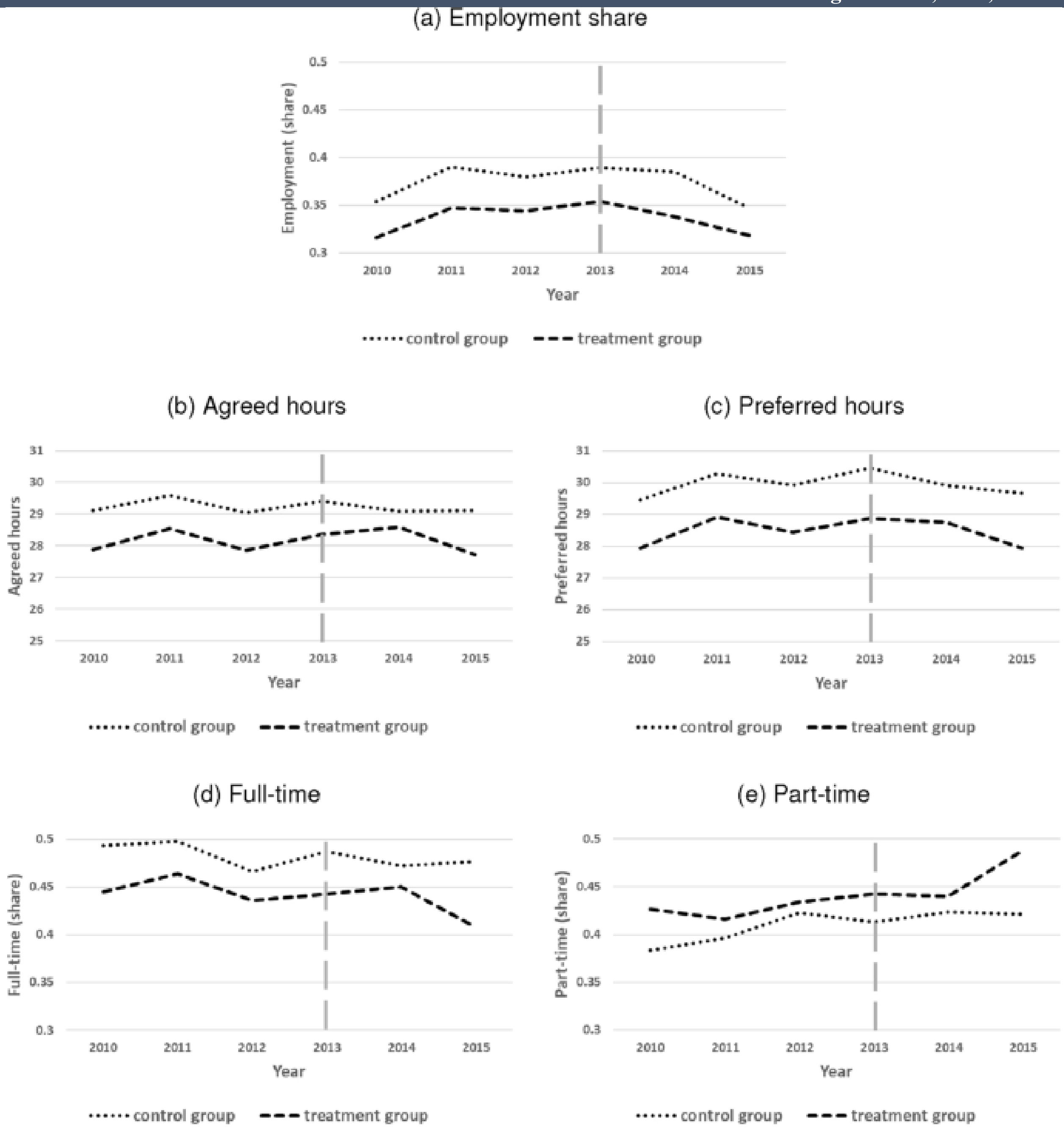

Figure 4. Trends in outcome variables. Notes: Means are calculated from the full pre-trimmed sample. Vertical line indicates the reform year, 2013. Source: Federal Statistical Office [42, 43] and Research Data Centre [19, 20, 83-86], own calculations

Assumption 2 – No Anticipation

$$E[Y^1(0) - Y^0(0) | D = 1, X] = 0 \quad (2)$$

Although the 2013 reform was announced as early as 2008, potential anticipation by mothers could take two forms: delaying conception or timing childbirth to benefit from the new policy (eligible births from August 2012 onward). To assess this, **Figure 5** compares monthly births in 2012 with those in 2010, 2011, 2013, and 2014. The data reveal no unusual spike beginning in August 2012. Instead, the increase observed in the latter half of 2012 appears consistent with an overall upward trend in birth numbers. This suggests that anticipatory selection into treatment is likely limited.

Controlling for covariates XXX strengthens the assumption, since variables such as maternal education can account for characteristics that might influence timing decisions. Furthermore, only pre-reform data from 2011 are included in the analysis, covering potential births between February 2008 and December 2011. It is improbable that mothers would defer conception by more than six months to take advantage of the reform, implying that the pre-reform sample is unlikely to be biased by selective timing.

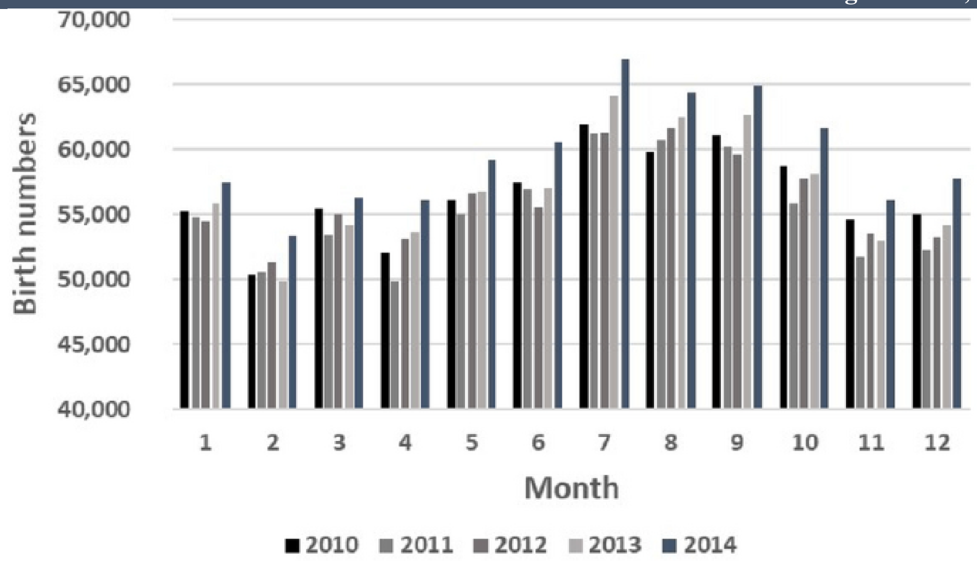

Figure 5. Monthly birth counts. Source: Author's visualization based on Official Birth Registers from the Federal Statistical Office (2010a–2014a)

Assumption 3

SUTVA

$$Y(t) = \begin{cases} Y^{0}(t) & if \quad D(t) = 0 \\ Y^{1}(t) & if \quad D(t) = 1 \end{cases} \tag{3}$$

Another important condition is the Stable Unit Treatment Value Assumption (SUTVA), which requires that outcomes for individuals in one group are not influenced by the assignment of others. This is particularly relevant when families relocate between districts classified as control or treated. Although repeated cross-sectional data do not allow complete exclusion of such movers, the analysis accounts for households that changed residence within the past 12 months.

Potential bias could also arise from other policy changes occurring during the observation period. For instance, the 2007 parental leave reform encouraged mothers to return to employment after their benefits ended [82, 87-89]. Later, in July 2015, modifications made part-time work more attractive while receiving benefits, though overall employment effects remained limited [48]. To ensure robustness, mothers of children younger than one year—who could be directly impacted by these changes—were excluded in a sensitivity check, producing results consistent with the baseline estimates.

Assumption 4 – Common Support

$$\mathrm{P(D = 1|X) < 1 \text{ where } P(D = 1|X) = E[D|X]} \tag{4}$$

Common support requires that no covariate perfectly determines whether an individual belongs to the treatment group. In the empirical implementation, observations with propensity scores near the minimum or maximum are excluded according to the trimming procedure proposed by Imbens and Wooldridge [90].

Given Assumptions 1 through 4, the average treatment effect on the treated (ATET) can be consistently estimated as

$$\begin{aligned} &\mathrm{ATET = E[Y^{1}(1) - Y^{0}(1)|D = 1]} \\ &\mathrm{E[Y^{1}(1) - Y^{0}(1)|D = 1, X|D = 1],} \\ &\mathrm{= E[\, E[\, Y(1) - Y(0)|D = 1, X\, 1]} \\ &\mathrm{-E[Y(1) - Y(0)|D = 0, X]|D = 1]} \end{aligned} \tag{5}$$

This is typically implemented through an outcome model estimated via ordinary least squares (OLS). However, Abadie [18] demonstrates that the ATET can also be obtained using an alternative formulation:

$$\mathrm{ATET = E\left[\frac{P(D = 1|X)}{P(D = 1)} \rho 0 Y\right]} \tag{6}$$

Where

$$\rho 0 = \frac{\mathrm{T} - \lambda}{\lambda(1 - \lambda)} \frac{\mathrm{D - P(D = 1|X)}}{\mathrm{P(D = 1|X)P(D = 0|X)}} \tag{7}$$

and _ being the share of post-treatment observations (see Abadie [18], for details). This implies a two-step estimation procedure for the sample analogue of the estimand $\mathrm{E\left[\frac{P(D=1|X)}{P(D=1)} \rho 0 Y\right]}$, i.e.

$$\frac{1}{N}\sum_{i=1}^{N}\left[\frac{\widehat{P(D_1 = 1|X_1)}}{\widehat{P(D_1 = 1)}}\widehat{\rho_{0,1}Y_1}\right] = \frac{1}{N}\sum_{i=1}^{N}\left[\frac{\widehat{P(D_1 = 1|X_1)}}{\widehat{P(D_1 = 1)}}\frac{Ti - \hat{\lambda}}{\lambda(1-\hat{\lambda})}\frac{D_i - \widehat{P(D_1 = 1|X_1)}}{\widehat{(D_1 = 1|X_1)\, P(D_1 = 1|X_1)}}Y_i\right] \quad (8)$$

for the entire sample. The first stage involves estimating the propensity score. Abadie [18] suggests either parametric or non-parametric techniques; for simplicity, this paper applies logistic regression. In the second stage, weighted non-parametric mean differences are calculated as a plug-in version of equation (2).

This method offers three key advantages. First, it does not impose a specific functional form in the second stage, providing flexibility that is particularly beneficial for binary outcomes. Standard linear probability models used in parametric DiD cannot properly handle the scale of binary outcomes, while nonlinear models assuming standard parallel trends may yield inconsistent estimates [91]. Second, it addresses the common support issue between treated and control groups. Observations lacking overlap with the other group can be removed, improving comparability—a benefit typically overlooked in outcome-based models. Third, the estimator's structure allows for the estimation of heterogeneous treatment effects, which is explored in the following section.

Heterogeneous effects

Policymakers are often interested not only in average effects across the full population but also in the impact on specific subgroups. Consequently, previous research has analyzed effects for particular groups (e.g., Cascio [92]; Havnes and Mogstad [15]), although this approach can lead to multiple testing issues. The problem becomes more severe as the number of hypotheses—or heterogeneities examined—increases. Abadie [18] addresses this by proposing a least-squares approximation for estimating conditional treatment effects.

$$E[Y^1(1) - Y^0(1)|D = 1, Z] \quad (9)$$

given by $g(Z; \gamma)$ where $Z \subseteq X$, i.e., Z is a subset for the heterogeneity variables of interest:

$$\gamma_0 = \arg\min_{y \epsilon \tau} E[P(D = 1|X)\{\rho_0 Y - g(Z; \gamma)\}^2] \quad (10)$$

$\widehat{\gamma_0}$ solves the weighted least squares problem for the sample analogue

$$\arg\min_{y\epsilon\tau}\frac{1}{N}\sum_{i=1}^{N}\widehat{P(D\_1 = 1|X_1)}\left[\widehat{\rho_{0,1}}Y_1 - Z_1{}'Y\right]^2 \quad (11)$$

It also provides a direct measure of how the average effect changes with ZZZ, allowing for joint inference using ordinary least squares without requiring adjustments for multiple hypothesis testing.

Data and descriptive findings

The analysis is based on the German Microcensus, a dataset representing 1% of all German households. Conducted annually by the Federal Statistical Office, the Microcensus provides detailed information on employment, family structure, and other individual attributes. A key strength of this dataset is the ability to link child and partner characteristics to the primary unit of interest, which in this study are mothers with a youngest child aged 3 years or younger. The sample is restricted to mothers aged 18 to 45 who reside in private households as their main residence.

An important feature of the Microcensus is that it records individuals' preferred working hours in addition to their current agreed hours. Unlike other surveys, such as the German Socio-Economic Panel (GSOEP), the Microcensus first asks whether the respondent wishes to adjust weekly working hours, conditional on an earnings change, before specifying the exact number of hours. This enables the identification of underemployment (desire to increase hours) and overemployment (preference to reduce hours). Respondents are instructed to consider only factors that could realistically affect their work in the next two weeks, and the question about reducing hours is voluntary. Holst and Bringmann [93] note that voluntary reporting may lead to underestimation of overemployment. Only individuals responding to these items are included, which is unlikely to meaningfully bias the sample of young mothers.

The Microcensus data are merged with district-level statistics on child care coverage for children aged up to 3 years, provided by the German Federal Statistical Office [42, 43, 52, 56, 59, 62]. Coverage is measured on March 1st and counts children in subsidized care who are not simultaneously enrolled in another program, as well as children in alternative care arrangements. After merging, the final sample comprises 11,640 mothers, including 3,505 currently employed, all with children aged 0–3 years.

Table 4 presents descriptive statistics for the variables used to estimate the propensity score, including characteristics of mothers and families, along with interview-related details. Observations with extreme propensity scores—those close to the minimum or maximum—are excluded following Imbens and Wooldridge [90]. This trimming removes 5,192 cases in the full sample (control vs. treated) and 1,710 in the employed sample, helping ensure that the common support assumption is more likely to hold.

Using repeated cross-sectional data introduces a potential concern: mothers might enter employment after the reform, which could bias estimates. To assess this, covariate balance over time is checked. **Table 4** reports means, standard deviations, and standardized mean differences (mean differences divided by the square root of the average variance; Rubin [94]). All differences fall below the 0.25 threshold for a large imbalance, suggesting minimal selection over time. Comparisons between mothers in high- versus low-expansion districts show generally small differences, with the exception of regional characteristics, which vary as expected given the district-level treatment assignment.

Table 4. Descriptive statistics of covariates by treatment group

Variable	Pre		Post		Post–Pre	Control group		Treated group		Treated-control group
	Mean	SD	Mean	SD	SD mean diff.	Mean	SD	Mean	SD	SD mean diff.
Individual age	32.297	5.638	32.395	5.048	0.018	32.270	5.283	32.427	5.426	0.029
Age of youngest child	0.986	0.812	0.969	0.810	− 0.020	0.980	0.810	0.975	0.811	− 0.007
Number of children	1.943	1.028	1.857	0.993	− 0.084	1.877	0.969	1.925	1.056	0.047
Migration background										
None	0.851	0.357	0.835	0.371	− 0.042	0.868	0.339	0.816	0.387	− 0.142
From EU country	0.041	0.198	0.057	0.231	0.074	0.041	0.199	0.057	0.231	0.072
Not from EU country	0.109	0.311	0.108	0.310	− 0.002	0.091	0.288	0.127	0.333	0.116
Quarter of interview										
1	0.250	0.433	0.240	0.427	− 0.022	0.251	0.434	0.238	0.426	− 0.032
2	0.247	0.432	0.239	0.426	− 0.020	0.243	0.429	0.244	0.429	0.003
3	0.247	0.431	0.246	0.431	− 0.002	0.244	0.429	0.250	0.433	0.015
4	0.256	0.436	0.275	0.446	0.043	0.262	0.440	0.268	0.443	0.014
Interview part										
Head of household	0.726	0.446	0.683	0.465	− 0.093	0.712	0.453	0.696	0.460	− 0.035
Self-reported	0.189	0.392	0.202	0.402	0.033	0.185	0.388	0.208	0.406	0.058
No information	0.085	0.279	0.114	0.318	0.098	0.141	0.103	0.096	0.295	− 0.023
Educational degree										
Lower secondary school	0.254	0.436	0.225	0.418	− 0.069	0.247	0.431	0.232	0.422	− 0.033
Middle secondary school	0.353	0.478	0.356	0.479	0.007	0.373	0.484	0.335	0.472	− 0.079
High school	0.393	0.488	0.419	0.493	0.053	0.381	0.486	0.433	0.495	0.106
Partner										
No partner living in household	0.171	0.377	0.124	0.330	− 0.132	0.153	0.360	0.142	0.349	− 0.031
Activity										
Inactive	0.047	0.212	0.047	0.211	− 0.002	0.042	0.201	0.052	0.221	0.045
Active	0.782	0.413	0.829	0.377	0.119	0.805	0.397	0.806	0.395	0.004
Educational degree										
Lower secondary school	0.260	0.439	0.243	0.429	− 0.040	0.263	0.440	0.239	0.426	− 0.056
Middle secondary school	0.225	0.417	0.240	0.427	0.036	0.239	0.426	0.225	0.418	− 0.031
High school	0.344	0.475	0.393	0.488	0.101	0.345	0.476	0.394	0.489	0.100
Degree of urbanization										

Urban	0.373	0.484	0.345	0.475	− 0.059	0.266	0.442	0.460	0.498	0.412
Middle	0.459	0.498	0.406	0.491	− 0.108	0.476	0.499	0.386	0.487	− 0.182
Rural	0.168	0.374	0.249	0.433	0.202	0.259	0.438	0.154	0.361	− 0.261
East Germany	0.145	0.352	0.121	0.326	− 0.070	0.185	0.389	0.076	0.265	− 0.329
N	5847		5793			6052		5588		

The sample is restricted to mothers aged 18–45 with children up to 3 years old. Federal states are included instead of a simple East Germany indicator. The standardized mean difference (SD mean diff.) is calculated as the mean difference divided by the square root of the average variance [94].

Table 5 reports average values for the child care coverage rate and the key outcome variables, along with standard deviations and mean differences between treated and control districts before and after the reform. Before the reform, subsidized care included fewer than 25% of children in high-expansion districts. In contrast, low-expansion districts already had a higher usage of subsidized care, creating a statistically significant negative difference. However, after the reform, high-expansion districts experienced notable catch-up.

The outcomes analyzed comprise both the extensive and intensive margins: a binary employment indicator, agreed and preferred weekly working hours, the mismatch between these hours, and indicators for full-time (more than 30 hours/week) versus part-time (12–30 hours/week) employment. Employment is defined according to the International Labour Organization standard (at least one paid hour or self-employment during the week prior to the interview), including mothers on maternity or parental leave, who are coded as not employed to reflect actual labor market participation.

Among mothers in high-expansion districts, roughly one-third were employed, with an average of 25.5 weekly hours and a small desired increase of approximately one hour. Most mothers reported matched agreed and preferred hours, with only 13.8% underemployed and 2% overemployed. About 35% worked full-time, while nearly half were employed part-time. The final columns of **Table 5** show mean differences between treated and control districts before and after the reform. While employment rates initially differed significantly, post-reform differences largely disappeared. For intensive margins, changes across groups were minor, although part-time employment slightly increased in high-expansion districts. Overall, these descriptive statistics suggest that expanding subsidized child care is linked to higher employment rates, but its impact on weekly hours is limited.

Table 5. Descriptive Statistics on Child Care Coverage and Maternal Outcomes

Variable	Treated Group (Pre-Reform)	SD	N	Treated–Control Difference	After Reform Difference
Coverage rate (%)	20.16	8.24	158	−10.74***	−5.02***
Employed (share)	0.348	0.476	2721	−0.042***	−0.009
Agreed working hours	25.50	13.66	862	0.81	1.16**
Preferred working hours	26.96	13.71	862	0.95	1.00*
Mismatch (hours)	1.46	6.34	862	0.14	−0.16
Full-time employment (share)	0.348	0.477	862	0.006	0.011
Part-time employment (share)	0.470	0.499	862	0.021	0.042*

The sample includes 18–45 years old mothers of up to 3-year-olds. Agreed and preferred hours are measured on the weekly basis*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

Estimation Results

Baseline effects

Table 6 presents baseline estimates for the effect defined in Eq. (1) for the full sample, alongside several robustness checks. Bootstrapped standard errors (in parentheses) account for the two-step estimation procedure and district-level clustering.

High-expansion districts experienced increases in both employment and working hours compared to low-expansion districts. Employment rose by 5.7 percentage points, equivalent to approximately 16% of the pre-reform mean. Agreed and preferred weekly hours increased by 5.1 and 5.3 hours, respectively, corresponding to roughly 20% of the pre-reform mean. Notably, the increase in agreed and preferred hours was almost identical, leaving the mismatch largely unchanged. **Table 9** in the appendix shows that shares of under- and overemployed mothers were unaffected. This indicates that the observed rise in hours is not only due to previously underemployed mothers aligning agreed and preferred hours but reflects a shift in the overall distributions.

Figures 8 and 9 in the appendix illustrate that the main driver of the effect is a movement from marginal employment (up to 12 hours/week) to part-time work (12–30 hours/week). A minor reduction at the high end of the working hour distribution contributes little to the average effect because similar trends occurred in low-expansion districts. Therefore, the increase in weekly hours is largely driven by transitions from marginal to part-time employment, while full-time employment remains stable.

Table 6. Main Estimation Results and Sensitivity Analyses—ATET

Panel / Specification	Employment	Mismatch (hours)	Preferred hours	Agreed hours	Part-time	Full-time
Panel A: Baseline	0.057**	0.213	5.303**	5.089**	0.126**	0.063
Standard errors	(0.028)	(0.790)	(2.580)	(2.382)	(0.063)	(0.048)
N	11,640	3,505	3,505	3,505	3,505	3,505
Relative effect size (vs. pre-reform mean)	0.164	0.146	0.197	0.200	0.268	0.182
Panel B: Common trend (Placebo reform)	−0.007	0.814	0.127	−0.687	0.008	−0.031
Standard errors	(0.032)	(0.605)	(2.006)	(2.417)	(0.067)	(0.048)
N	11,307	3,638	3,638	3,638	3,638	3,638
Panel C: Sample composition (Median split)	0.069***	0.307	4.130**	3.823**	0.119**	0.027
Standard errors	(0.023)	(0.403)	(2.006)	(1.929)	(0.050)	(0.037)
N	16,203	5,113	5,113	5,113	5,113	5,113
Alternate specification	0.057**	0.097	3.360	3.263	0.090	0.037
Standard errors	(0.025)	(0.380)	(2.247)	(2.179)	(0.055)	(0.044)
N	15,919	5,142	5,142	5,142	5,142	5,142
Panel D: Sample composition (Robustness checks)						
West Germany only	0.066*	1.246	6.562**	5.316*	0.184**	0.025
Standard errors	(0.038)	(0.865)	(3.268)	(3.027)	(0.080)	(0.058)
N	10,618	3,196	3,196	3,196	3,196	3,196
Excluding children <1 year	0.110***	−0.534	6.087**	6.621***	0.142**	0.097*
Standard errors	(0.039)	(0.647)	(2.564)	(2.460)	(0.068)	(0.051)
N	7,695	3,001	3,001	3,001	3,001	3,001
Excluding childminders	0.052*	0.184	4.687*	4.503*	0.111*	0.058
Standard errors	(0.029)	(0.767)	(2.700)	(2.491)	(0.065)	(0.051)
N	11,438	3,441	3,441	3,441	3,441	3,441
Excluding families who moved	0.066**	0.785	6.186**	5.401**	0.163**	0.054
Standard errors	(0.030)	(0.791)	(2.743)	(2.521)	(0.068)	(0.053)
N	10,330	3,177	3,177	3,177	3,177	3,177

Estimates refer to the parameter described in Eq. (1). Standard errors are bootstrapped with 1000 replications and clustered at the district level. The sample consists of mothers aged 18–45 with children aged 0–3 years. Weekly agreed and preferred hours are used as outcome variables. Propensity score control variables are listed in **Table 4**.

*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

The overall results align well with prior studies on Germany. Bauernschuster and Schlotter [23] report intention-to-treat effects for kindergarten eligibility ranging from five to eight percentage points on maternal employment and about 2.5 hours on weekly working time. Fendel and Jochimsen [26] find that maternal employment increased by roughly eight percentage points due to the combined effects of the legal claim to child care slots and the introduction of home care allowances. Müller and Wrohlich [16], analyzing earlier phases of child care expansion in West Germany, also document positive effects on maternal employment, largely attributable to increased part-time work. Collectively, these results for Germany appear robust and consistent with evidence from countries with relatively low maternal labor force participation [22, 24, 25, 30, 32, 33].

A key insight concerns the adjustment of agreed and preferred working hours. While both measures increase, this study contrasts with Reynolds and Johnson [35] by showing that agreed and preferred hours tend to rise in parallel. Moreover, the average effect on the proportion of under- and overemployed mothers is not statistically significant. These findings indicate that the mismatch remains largely unchanged, suggesting that the effects are not solely driven by previously underemployed mothers aligning their agreed and preferred hours. Instead, access to affordable child care appears to shift working hour preferences across a broader range of mothers, reflected in a general upward adjustment of both agreed and preferred hours. This shift is primarily driven by mothers moving from marginal employment to part-time work.

The subsequent panels of **Table 6** present various robustness checks. First, to assess the parallel trends assumption, I test whether the pre-reform time trends for districts with high versus low increases in coverage were comparable. A placebo specification is introduced, setting 2010 as the pre-reform year and 2011 as the post-reform year. Estimates are near zero (Panel B), indicating that treated and control districts followed similar trends before the reform.

Next, Panel C redefines treatment and control groups using the median increase in coverage rate. Results remain consistent with the main specification. Changing the post-reform year to 2014 yields similar point estimates, though effects on the intensive margin are smaller and only marginally significant, suggesting potential time dynamics in working hour adjustments.

Panel D explores the impact of sample composition. Restricting the analysis to West German districts strengthens the effect: employment rises by 6.6 percentage points in high-expansion West German districts, primarily due to part-time work. Preferred hours increase slightly more than agreed hours, yet as the ATET parameter captures average effects, overall findings remain comparable between the full and West German samples. Including East German districts in the baseline does not meaningfully alter results, aside from changing the composition of the control group.

Excluding mothers with children under 1 year slightly increases effects across outcomes. In particular, full-time employment rises among mothers with children older than one. This suggests that the 2015 parental leave reform, affecting mothers of infants, does not drive the results.

Further checks exclude mothers employed in child care facilities, which minimally alters the estimates. Similarly, controlling for selective migration by removing individuals who moved within the last 12 months has little impact.

Table 7. Heterogeneity Analysis—Variation in Effects

Heterogeneity	Employment	Mismatch (hours)	Preferred hours	Agreed hours	Part-time	Full-time
Education (ref: lower secondary school)						
Middle secondary school	0.044	−0.616	5.600	6.216	−0.113	0.161
Standard errors	(0.063)	(1.904)	(6.881)	(6.250)	(0.154)	(0.139)
High school	0.122*	−0.684	9.773	10.457*	0.116	0.182
Standard errors	(0.069)	(1.979)	(6.673)	(6.051)	(0.159)	(0.130)
Number of children	−0.004	1.087	2.550	1.462	0.055	0.014
Standard errors	(0.025)	(1.059)	(2.912)	(2.629)	(0.074)	(0.053)
Partner status (ref: no partner living in household)						
Partner living in household	0.052	−3.095*	−0.454	2.641	0.104	0.027
Standard errors	(0.073)	(1.834)	(7.343)	(6.840)	(0.160)	(0.140)
N	11,640	3,505	3,505	3,505	3,505	3,505

The results reflect estimates of as defined in Eq. (3), indicating differences relative to the reference category for categorical variables or a one-unit increase for continuous variables. Standard errors reported in the columns are obtained via 1,000 bootstrap replications and account for clustering at the district level. The analysis focuses on mothers aged 18–45 with children up to 3 years old. Agreed and preferred hours are recorded on a weekly basis. The covariates used for estimating the propensity score are listed in **Table 4.** Statistical significance is denoted as *p < 0.1, **p < 0.05, ***p < 0.01.

Heterogeneous effects

Table 7 presents the variation of effects across subgroups following Abadie [18]. Estimates reflect differences compared to the reference group for categorical variables or per one-unit change for continuous ones. For instance, mothers holding a high school diploma experience an employment increase of twelve percentage points relative to mothers with a lower secondary school degree. Effects on the intensive margin are also larger for higher-educated women, though these estimates exhibit substantial variance. These results are consistent with Müller and Wrohlich [16] and Havnes and Mogstad [15], both of which report stronger effects for better-educated mothers. The smaller effect in the latter study may be due to the overall weaker reform impact. One explanation is that external child care costs remain relatively high for mothers with lower educational attainment. Additionally, higher opportunity costs for reducing hours or leaving the workforce may prevent better-educated mothers from withdrawing entirely, consistent with findings from Zimmert and Zimmert [48].

While the average effect does not differ by number of children, the presence of a partner yields notable patterns. Although overall adjustments of agreed and preferred hours are similar, cohabiting mothers show significantly higher increases in agreed hours relative to preferred hours. As underemployment declines within this group, the reform appears particularly effective for families following a more traditional employment pattern by aligning agreed hours with desired hours. Comparable findings are reported for Norway by Andresen and Havnes [21], who show that cohabiting mothers respond to 2-year-old children entering child care by raising full-time employment, while most (63%) previously held part-time positions.

Discussion and Conclusion

This study offers empirical evidence on the causal effects of subsidized early child care on maternal labor market outcomes. It exploits the staggered expansion of child care in Germany, culminating in the 2013 legal entitlement to a child care slot. The semi-parametric intention-to-treat estimates indicate a substantial increase of 5.7 percentage points in maternal employment and roughly 5 hours in both agreed and preferred weekly working hours. The proportion of full-time employed mothers does not show significant changes, which may reflect either limited availability of full-time child care slots or parental preference for part-time care. Although the share of children attending full-time care (more than 7 hours per day) nearly doubled from 2011 to 2015 in high-expansion districts, only around 10% of children were enrolled full-time post-reform [42, 43]. These figures, however, cannot disentangle whether constraints are supply-driven or preference-driven, as detailed data on full-time slots is lacking.

Evidence of dynamic effects emerges, with working hours increasing more in 2015 than 2014, potentially reflecting shifting attitudes: as more mothers utilize subsidized child care and raise their hours, others may follow suit. Future research could explore this peer effect further, requiring more granular data on both child care provision and parental preferences.

Conditional average effects highlight two key points. First, mothers with a high school degree show large positive responses, whereas women with lower educational attainment respond less, potentially due to high child care costs. This suggests that income-based parental contributions could improve accessibility, as some communities have already implemented. Second, cohabiting mothers, who may previously have supplemented a partner's income, exhibit higher increases in agreed hours than preferred hours, reducing underemployment. These findings underscore that adjustments of agreed and preferred hours can diverge and depend on dissatisfaction with current work arrangements. Considering underlying working hour preferences is therefore critical for evaluating reforms aimed at increasing female labor supply.

Despite overall positive effects, some groups—particularly mothers with lower education and single mothers—show limited responses. Future studies could investigate the mechanisms behind these differences. Additionally, understanding long-term effects is important, as many mothers initially return in part-time roles; follow-up research could assess how these working patterns evolve over time.

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