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Spillover Effects of Public Childcare Expansion in Regional Labor Markets*

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Abstract

This paper examines labor market spillover effects of expansions in the supply of publicly subsidized childcare for children under the age of three. Using German administrative data, I exploit two major reforms that generated cross-county variation in the expansion of early childcare supply. I implement a matched Difference-in-Differences design to identify causal effects. The results show that counties that experienced a sudden and sharp increase in early childcare coverage had an average employment rate that was 0.55 percentage points higher than comparable control counties over seven years after the expansion. Excluding mothers and childcare workers, who are directly affected by the childcare expansion, does not significantly reduce the estimated effect. This suggests the presence of labor market spillover effects beyond the directly affected groups. The employment effect does not appear to be primarily driven by migration between treated and control counties.

Keywords: early childcare, local labor markets, local labor market spillovers
JEL Codes: J13, J21, R23

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1 Introduction

With populations aging, large cohorts retiring, and fertility rates remaining below replacement level, many developed economies face growing labor shortages and a shrinking working-age population. Increasing women's labor force participation has therefore become a central policy goal to mitigate the economic consequences of demographic change. Governments have increasingly invested in publicly subsidized childcare as a key policy instrument to reduce barriers to maternal employment, promote gender equality, and mobilize untapped labor supply (OECD, 2017; OECD, 2025).

Despite this development, the economic literature has focused predominantly on the direct effects of childcare expansion on maternal labor supply, while largely overlooking its potentially broader implications for local labor markets. The expansion of childcare facilities not only constitutes a substantial labor supply shock by increasing maternal employment (see e.g., Bauernschuster et al., 2015 and Müller and Wrohlich, 2020) and potential peer effects, but also potentially creates new jobs in the childcare sector and raises household incomes of families. These changes may generate broader effects in local labor markets by increasing labor demand and household consumption (see Helm, 2020; Gathmann et al., 2020). This raises the question of whether the benefits of expanding early childcare are confined to those directly affected or whether they also generate spillover effects for other workers. By shifting the analytical perspective from individual treatment effects to regional labor market outcomes, this paper highlights an important but largely overlooked effect. Consequently, the overall economic effects of childcare expansions may extend well beyond the directly treated population. This study contributes to the policy debate by documenting these regional spillovers and demonstrating that evaluations focusing exclusively on mothers' employment are likely to underestimate the full economic returns to investments in early childcare, as they neglect broader general equilibrium adjustments and local multiplier effects.

This paper provides novel evidence on local labor market spillovers arising from expansions in early childcare provision in Germany and connects two strands of literature that have largely developed separately. While existing research has primarily focused on the direct labor supply responses of mothers, considerably less attention has been paid to indirect effects operating through local labor markets. To estimate these spillover effects, I use German administrative data and apply a matched Difference-in-Differences (DiD) design. The identification exploits regional and temporal variation in the expansion of childcare provision for children under the age of three at the county-year level, which was largely driven by institutional reforms and supply-side constraints rather than short-run local labor market conditions. To assess the sensitivity of the findings, I conduct several robustness analyses.

I find a statistically significant average increase in the probability of being employed in treated

counties of 0.55 percentage points over seven years after treatment relative to the control group. The estimated effect remains only slightly smaller when mothers and childcare workers are excluded from the sample, suggesting that the overall employment effect is not primarily driven by these groups. This interpretation is further supported by the finding that the estimated effect is larger for men than for women. In addition, the employment increase appears to be primarily driven by the local labor force, as the estimated effect only slightly decreases when excluding individuals who moved between control and treatment counties after treatment. While no statistically significant effect on daily wages is found for the overall sample, a negative wage effect is observed for workers in low-skilled occupations. Additionally, the employment effect appears to be strongest for those with stronger attachments to local labor markets and with established networks, as the estimated effect is the largest for individuals over the age of 50.

This paper contributes to the literature on labor market effects of publicly subsidized early childcare expansions. A substantial body of evidence shows that expansions in subsidized childcare for toddlers can significantly increase maternal labor supply.[1] For Italy, Norway, and Germany, Carta and Rizzica (2018), Andresen and Havnes (2019), and Müller and Wrohlich (2020), respectively, document sizable increases in mothers' employment following expansions in childcare availability. However, the evidence is not uniformly positive. In other institutional contexts, several studies report only limited or negligible effects of childcare expansions on maternal labor supply (see Kleven et al., 2024; Attanasio et al., 2022; Rabaté and Rellstab, 2022).[2] However, the existing literature on Germany suggests a positive labor market effect on mothers (see Bauernschuster et al., 2015; Bauernschuster and Schlotter, 2015; Müller and Wrohlich, 2020; Duletzki and Lim, 2026). This paper adds to this by investigating effects exceeding directly affected individuals.

While childcare expansions are typically studied as labor supply interventions, they also represent a substantial increase in local employment in the childcare sector and may therefore affect regional labor markets through demand-side channels. This paper thus relates to the growing literature on how regional labor markets adjust to local labor demand shocks. Studies exploiting plant closures show that local employment effects are not confined to directly affected industries but generate substantial spillovers across the local economy (see Gathmann et al., 2020; Vom Berge and Schmillen, 2023). Similarly, Helm (2020) finds that positive labor demand shocks generate employment gains beyond

[1] Müller and Wrohlich (2020) suggest that an investment in the publicly subsidized childcare provision increases the relative cost of informal and private care provision. This leads to more parents utilizing public childcare as its relative price decreased, causing the share of childcare coverage provided informally by mothers to decrease. This consequently decreases the opportunity costs of participating in the labor market, leading to a higher labor market participation rate of mothers with children under the age of three years.

[2] Havnes and Mogstad (2011) argue that such variation is not solely driven by differences in local labor market conditions or population characteristics, but also by the availability and cost of informal childcare. If informal care is widely accessible and affordable, the marginal impact of subsidized public childcare may be limited.

the initially affected industries. These findings are supported by many studies that suggest that despite worker mobility reducing employment effects, regional employment differences remain after labor demand shocks (see Monras, 2015; Dao et al., 2017; Amior and Manning, 2018; Gathmann et al., 2020). Together, these studies demonstrate that local labor demand shocks can trigger broader regional adjustment processes through changes in labor demand, labor supply, and worker mobility.

Yet unlike private-sector labor demand shocks, employment growth in the childcare sector is largely driven by government investment, moving it conceptually closer to public employment expansions. The limited existing evidence on the aggregate employment effects of public sector employment is mixed. Faggio and Overman (2014) find that increases in public employment primarily reallocate employment from the tradable to the non-tradable sector without increasing overall employment. Becker et al. (2021) similarly document a decline in employment in tradable goods industries but find that employment gains in the non-tradable sector are sufficiently large to generate a positive net employment effect. Jofre-Monseny et al. (2020) argue that these employment gains do not translate into lower local unemployment because labor demand attracts additional workers through migration.

This paper extends the literature by studying childcare expansion as a distinct type of local labor market shock. Unlike previous studies, childcare expansion simultaneously creates employment in the childcare sector and increases maternal labor supply by reducing barriers to employment. Consequently, it combines a labor demand shock with a labor supply shock, creating an additional channel through which local employment spillovers may arise. In particular, greater childcare availability could generate spillovers through several mechanisms: the increased local labor demand resulting from the expansions of childcare facilities and in complementary sectors (e.g., food and entertainment) would cause both wages and local employment to increase. This would be further reinforced by increasing maternal labor supply and correspondingly higher household incomes. Household consumption increases in response, partially in the non-tradable service sector, which would create demand for local labor.[3] By examining these broader regional effects, the paper contributes to the literature on local labor market adjustment and public employment by considering a policy intervention that operates through both demand- and supply-side mechanisms.

Based on these considerations, I hypothesize that large expansions in early childcare provision generate positive spillover effects on local labor market outcomes. Counties experiencing substantial

[3] I follow the modified framework of Moretti (2010) by Faggio and Overman (2012), who adapted the model that focused on local multipliers of tradable sector employment to incorporate the public sector and develop a model for multipliers caused by public sector employment. They argue that the size of the labor market multipliers (i.e., the ratio of additional jobs generated to jobs created in the public sector) depends on the labor supply elasticity, while assuming neither perfect elasticity nor perfect inelasticity. They also argue that assuming that the local demand for the tradable sector relative to the total demand is insignificant, a crowding out effect from the tradable into the non-tradable sector should be observable.

increases in childcare availability are expected to exhibit higher employment among the broader local working-age population relative to comparable counties without such expansions. Importantly, if childcare expansion generates broader local labor market spillovers, these effects should not be confined to mothers of young children or workers directly employed in the childcare sector, but should extend to the wider local working-age population.

This paper is structured as follows. Section 2 outlines the institutional setting and the utilized policy reforms. Section 3 describes the data used as well as the sample construction. Section 4 presents the empirical strategy, detailing the identification approach, key assumptions, potential threats to identification, and the matching procedure, as well as the resulting analytical sample. Section 5 reports the main results, investigates heterogeneity, and presents robustness checks. Section 6 concludes with a discussion of the policy implications.

2 Institutional Background

Historically, fertility rates in West Germany have remained low since the late 1960s and early 1970s, stabilizing at around 1.45 children per woman. Meanwhile, fertility in East Germany increased to approximately 1.9 in the late 1970s, partly reflecting policies aimed at supporting families and encouraging higher fertility. This divergence persisted until reunification, when fertility in East Germany declined sharply to around 0.5, before increasing again and converging toward a similar level as in West Germany by the 2000s. In 2008, Germany's overall fertility rate was 1.38, which was significantly lower than in countries such as France, the United States, and Sweden and substantially below the EU-27 average of 1.6 (Dorbritz, 2008; Bauernschuster et al., 2015).

These demographic developments increased the focus of German family policy on improving the compatibility of parenthood and employment. In response, the German government introduced a legal entitlement to publicly provided childcare for children between three and six years of age in 1996, achieving full provision of half-day care in West Germany (Bauernschuster and Schlotter, 2015). However, childcare provision for younger children remained limited, and both female labor force participation and the utilization of early childcare continued to be significantly lower in West Germany than in East Germany (Müller and Wrohlich, 2020).

The German government enacted two reforms in the 2000s that substantially expanded the supply of publicly subsidized childcare for children under the age of three. The Day Care Expansion Act (*Tagesbetreuungsausbaugesetz*), which came into force on January 1, 2005, aimed to substantially expand childcare provision for children under the age of three. The reform required municipalities to increase the availability of childcare places, particularly in West Germany where childcare coverage

was substantially lower than in East Germany, by reserving places for children under the age of three whose parents are employed, in vocational training, or studying.[4] In addition, the reform introduced measures to improve childcare quality and strengthened support for family day care providers. To ensure this, the law requires the latter to improve its provision by 2010 and report annually on the progress of the expansion (BMFSFJ, 2004). The Childcare Funding Act (*Kinderförderungsgesetz*), which came into force in December 2008, further accelerated the expansion of childcare. Most importantly, it introduced a legal entitlement to publicly subsidized childcare for all children aged one to three from August 2013 and provided 5.95 billion euro for expanding childcare capacity (BMFSFJ, 2018). Together, the two reforms substantially increased incentives and financial resources for expanding publicly subsidized childcare, particularly in West Germany.

These reforms were followed by a substantial increase in childcare coverage. Figure 1 illustrates the evolution of childcare coverage across West German counties between 2007 and 2014. In 2007 the coverage of early childcare was between 2.2 and 28.9 percent, primarily clustered in the center of West Germany and in some urban areas. Meanwhile in 2014, the coverage had increased to between 14 and 46.7 percent. Additionally, while still being higher in the center, more of the counties with a high childcare coverage rate were now located in the north of the country. When comparing the variation in the increase in the childcare coverage rate by counties in Figure 2, one can see that the magnitude of childcare expansion exhibits spatial heterogeneity and is not concentrated exclusively among counties with either initially high or initially low childcare coverage. This substantial regional variation in the timing and magnitude of childcare expansion provides the basis for the empirical identification strategy employed in this paper.

Simultaneously with the two reforms, the German government reformed the parental leave system in 2007. It replaced the previously means-tested and capped allowance for parents at 300 euro with an entitlement of 65 percent of the average net earnings in the previous year for 12 months. This is extended by an additional two months if the second parent also takes part of the leave (Duletzki and Lim, 2026). The implications of this simultaneous reform for the empirical analysis are discussed in Chapter 4.4. Given its nationwide implementation, the reform is unlikely to affect the estimation results.

The institutional framework of childcare in Germany is closely linked to the country's post-war history. Following World War II, Germany was divided into two states that developed fundamentally different political, economic, and social systems. These differences were particularly pronounced with respect to family policy, gender roles, and the organization of childcare. In the former German

[4]The law aimed at creating 230,000 additional early childcare slots and increase the childcare coverage rate to 17 percent in West Germany by 2010. Additionally, in a summit in 2007, the federal government, the states, and municipalities agreed to further increase the childcare coverage rate to 35 percent by 2013 (Bauernschuster et al., 2015).

Democratic Republic (GDR), high female labor force participation was considered essential for the functioning of the centrally planned economy. Consequently, the state assumed primary responsibility for childcare provision and developed an extensive system of publicly funded childcare facilities designed to enable full-time employment for mothers. Childcare was therefore an integral part of both family and labor market policy. West Germany, on the other hand, followed a markedly different approach. Family policy was shaped by the traditional male breadwinner model, under which childcare was primarily regarded as a private family responsibility. Public childcare for children under the age of three remained scarce, while tax and transfer policies reinforced financial incentives for mothers to reduce or interrupt employment after childbirth. As a result, female employment was considerably lower than in East Germany and was more frequently characterized by career interruptions and part-time work. These contrasting institutional arrangements also shaped social norms. While full-time employment among mothers was widely accepted in the GDR, more traditional attitudes towards maternal employment prevailed in West Germany. Although German Reunification unified the legal framework in 1990, these historical differences in childcare provision, labor market institutions, and gender norms continued to influence childcare use and female employment until today (see Campa and Serafinelli, 2019; Lippmann et al., 2020; Müller and Wrohlich, 2020). To avoid conflating the effects of childcare expansion with persistent institutional and cultural differences between East and West Germany, this analysis is restricted to West Germany.

Because Germany is a federal state, several levels of government share the responsibility for childcare expansion. Municipalities are primarily running and expanding childcare, which allows them to respond to regional differences and demands. The federal government is responsible for promoting the expansion of facilities and their services and supporting local public youth welfare organizations in fulfilling their responsibilities. In this way, the federal states set the framework without determining the actions of the municipalities. This allows local actors to act independently and react to local needs and preconditions (Stoy, 2015).

An important feature of childcare provision for children under the age of three is that it requires substantially more personnel per child than childcare for older children, creating an important supply constraint (Statistisches Bundesamt, 2025). This indicates that the expansion of publicly subsidized childcare is not only contingent on political will, but also on the provision of substantial financial resources. It requires considerable financial investment, both in the short and long term, which provides an additional barrier to expansion.

3 Data and Sample Construction

To study local labor market spillovers from expansions in publicly subsidized early childcare, this paper uses the Sample of Integrated Labour Market Biographies (SIAB), a weakly anonymized 2 percent sample of the population covered by German social security records. It consists of German administrative employment and social security data and contains the complete labor market history within the German social security system of more than 2 million individuals between 1975 and 2023. The data covers individuals employed and subject to social security contributions as well as registered unemployed individuals receiving unemployment benefits. Individuals who are self-employed or civil servants are therefore not included. This is also the case for people who are not available to the labor market as they are retired, in school, or not in employment but not dependent on payments due to them being stay-at-home parents or having other forms of income. Consequently, the data consists only of people who are either in the labor market or available for it (Schmucker and vom Berge, 2023).

I restrict the sample by excluding individuals in (partial) retirement, school, university, vocational training or internships, as those do not necessarily move into unemployment when losing their job. Additionally, I excluded cross-county commuters, as when employed they would be counted in the county they work as, but when going unemployed, they would be counted for the county they live in. This creates a problem if both are not the same as labor market effects in one county would be attributed to another one. Lastly I excluded both people unable to work and those not permanently employed.

To isolate spillover effects of childcare expansion, it is necessary to identify individuals who are directly affected by the expansion. These include childcare workers, whose employment probability is directly positively affected by the expansion of childcare supply, and mothers of children under age three, who are the primary beneficiaries of increases in childcare availability. While the SIAB allows for the identification of childcare workers, identifying mothers is more challenging because it does not contain information on childbirth. To address this issue, I follow the approach and program code provided by Müller et al. (2026), who employ a multi-step procedure. They identify the start of paid maternity leave, restricting the sample to women aged at most 38 years for a first birth and 40 years for a second or subsequent birth. These restrictions are necessary because the maternity leave identifier is not exclusive to maternity leave and can also indicate long-term illness. Müller et al. (2026) argue that these two events are concentrated in different age groups: childbirth predominantly occurs among younger and middle-aged women, whereas long-term illness is comparatively rare at these ages and more common later in life. In subsequent steps, they validate the identified births by checking that the duration of maternity leave is sufficiently long and by verifying the spacing between siblings to reduce misclassification. With this method, they manage to identify between 25 and 75

percent of all mothers, depending on the year and the age cohort of the mother, with the majority being above 50 percent. This procedure implies that some mothers may remain unidentified.

Table 1 shows the individual statistics for both the control and treatment group after matching. Overall, the two groups exhibit similar characteristics across all observable dimensions. Employment shares, average age, and the share of women are nearly identical between treatment and control groups. The share of university graduates is slightly higher in treatment counties , while the shares of childcare workers, mothers with a youngest child aged three or younger, and individuals who moved across counties are small and comparable across both groups. The sample comprises approximately 1.72 million individuals in each group. For daily wages, the sample size decreases to around 1.31 million in the treatment group and 1.29 million in the control group, as wage information is only available for individuals in employment; average daily wages are similar across both groups.

4 Empirical Strategy

4.1 Identification Strategy

This paper exploits the previously mentioned two policy reforms that caused a significant expansion in publicly subsidized childcare for children under the age of three between 2007 and 2014. Importantly, the extent of the expansion that can be utilized for the analysis varies across counties (see Figure 2). Therefore, it is essential that this variation is exogenous to other determining factors of maternal labor market outcomes. There is a strong indication for this being the case, as several government reports and studies provide evidence for the variation between counties being primarily driven by local supply shocks, exogenous to mothers' labor market outcomes (Müller and Wrohlich, 2020; Stürmer-Heiber, 2022). In the following, I will give an overview of the three main factors, as put forward by Duletzki and Lim (2026), that indicate this.

Firstly, a shortage of qualified staff as well as suitable construction sites for new childcare facilities limited the extent to which counties were able to meet the demand for childcare (BMFSFJ, 2011, BMFSFJ, 2012, BMFSFJ, 2013). This resulted in counties and other regional childcare providers having problems finding both suitable locations and the necessary staff in order to create proposals that were suitable for childcare funding, slowing down the expansion of publicly subsidized early childcare (Stürmer-Heiber, 2022). Secondly, long approval processes at different administrative levels created frictions in the expansion of early childcare facilities (Stoy, 2015). Additionally, the budget introduced another insecurity in planning. When the funding needed exceeded the available budget, the recipients were determined through either waiting lists or lotteries (Felfe and Lalive, 2018). The third factor is

idiosyncratic errors that occur in the projection of childcare demand and population developments in a county, which contribute significantly to the heterogeneity in childcare funding approval probabilities (Hüsken, 2010; Hüsken, 2011). As the funding was additionally distributed based on other factors such as the existing supply of childcare and the quality of the submitted proposal by the providers (Felfe and Lalive, 2018), errors in the local projection of demand and the corresponding distribution of funds possibly occurred. Due to that, the demand often exceeded the supply of childcare (Hüsken, 2010, Hüsken, 2011).

After the two policy reforms, counties varied in the extent of the expansion in publicly subsidized childcare in terms of the timing and intensity of the expansion. I utilize this variation to identify local labor market adjustments to these investments. Although the previously mentioned reasons suggest that the variance in timing and magnitude of the expansion of childcare across counties was driven by exogenous supply shocks, demand-driven factors potentially contribute to the regional variation. Addressing this, I conducted a DiD analysis combined with matching. Here, the intervention is a large and sudden increase in early childcare in a single year, following the approach proposed by Kleven et al. (2019). I use the threshold suggested by Duletzki and Lim (2026) of an increase by more than five percentage points in the early childcare coverage rate in a single year.

4.2 Construction of the Treatment Group

I classify counties into two categories: those that experienced a significant increase in childcare coverage during the study period and those that did not. The definition of a significant increase follows the approach proposed by Duletzki and Lim (2026), who consider an increase of more than five percentage points in the early childcare coverage rate within a single year as significant. They motivate this cutoff on empirical grounds, as in the dataset, only about five percent of county–year observations exhibit such a large year-on-year rise in childcare coverage. The five-percentage-points threshold therefore identifies counties that experienced unusually rapid expansion relative to the typical variation observed across time and space. To ensure comparability with their work, I examine how the data used aligns with this benchmark. In my sample, the year-county observations that exceed this threshold make up about 5.6 percent of all observations. This indicates that the rule effectively isolates the upper tail of childcare coverage growth in my setting as well. For both conceptual consistency and ease of comparison with the existing literature, I therefore adopt the ”more than five percentage points per year increase” criterion when designating counties as having experienced a significant increase in childcare coverage. Figure 3 shows that this definition classifies only the most extreme increases as treatment. Such large, discontinuous jumps in coverage within a single year are unlikely to reflect gradual demand-

side adjustments, since demand-driven processes tend to evolve incrementally over time. In contrast, discrete jumps of this magnitude are more consistent with supply-side events such as the opening of new facilities or the implementation of policy reforms, which by their nature generate lumpy, one-time increases in capacity rather than smooth adjustments. The threshold therefore isolates expansions that are more likely to reflect exogenous supply shocks rather than gradual, demand-driven responses.

The control group consists of all counties that never experienced an increase in early childcare coverage by more than five percentage points in a single year between 2007 and 2014. This pool of counties is then used in the matching. As argued by Duletzki and Lim (2026), this avoids comparisons between earlier- and later-treated counties and therefore circumvents the under-identification and negative weighting problems that may arise in staggered treatment settings.

4.3 Difference-in-Differences Approach

To estimate the causal effect of the significant and sudden increase in the supply of childcare for children under the age of three years, I utilize a DiD approach, which compares changes in outcomes over time between the control and treatment group. I estimate the following DiD specification:

$$Y_{it} = \alpha + \beta_1 Treated_i + \beta_2 Post_t + \beta_3 (Treated_i \times Post_t) + \beta_4 X_{it} + \gamma_t + \theta_c + \epsilon_{it} \quad (1)$$

I analyze two separate outcome variables, represented here with Y_{it}. These are a dummy variable equal to 1 if a person is employed and 0 if not and the deflated daily wage of an employed person. $Treated_i$ is a binary indicator equal to 1 if the person i is in the treatment group and 0 if not. $Post_t$ is a binary indicator with a value of 1 if the time t of the observation is after treatment, defined here as a significant increase in early childcare, and 0 if before that. Both binary indicators are interacted, providing an interaction term that equals one only for treated units after the treatment and zero for any other observation. The resulting DiD estimator β_3 captures the causal treatment effect and is therefore the estimator of interest in this analysis. X_{it} are individual-level covariates, consisting of age, age squared, gender, and having a university degree. In addition, I include time fixed effects (γ_t) and county fixed effects (θ_c). ϵ_{it} measures the error term. To account for autocorrelation, I cluster standard errors at the county level.

4.4 Key Assumptions and Threats to Identification

There are three main threats to identification that must be taken into account. The first threat are differential pre-treatment trends between treated and control counties due to macroeconomic trends that

are not related to the significant investment in childcare. One of these could be that, as the two policy reforms utilized call for a demand-oriented expansion of public childcare, counties that are economically worse off already face a disproportionately high shortage of childcare, and therefore have a larger expansion than economically better-off counties. To address this, I utilize the previously mentioned propensity score matching, which improves the comparability of treated and control counties. Figures 4a and 4b provide visual evidence that both groups followed similar pre-treatment trends after matching.

The second threat are potential changes in the composition of the groups within the observed time. This means that parents who are more prone to demand childcare and to work, potentially self-select into counties with a higher supply of childcare, as they invest more in the expansion. Additionally, these parents potentially lobby or vote for more local childcare policies in line with their preferences, thereby increasing political pressure compared to counties that were not initially as attractive to such parents. Simultaneously, childcare providers potentially relocate into those counties that have a higher demand for childcare, further increasing their share of toddlers in care. To control for these changes in composition and the corresponding potential threat to identification, I include individual controls.

A third risk to identification are county-specific and aggregate macroeconomic shocks. To account for those, both time and county fixed effects are included. These fixed effects also ensure that the previously mentioned simultaneous reform of the parental leave system in 2007 is unlikely to confound the estimation results. This would only be the case if the effect of this reform would systematically vary across counties over time, which is controlled for with the matching approach. Therefore, the policy reform is presumably not going to affect the estimation results.

4.5 Matching Approach

A key identification challenge, as previously explained, is that treated and untreated counties may differ systematically in observable characteristics. To improve comparability of treated and control counties, I combine the DiD approach with propensity score matching. Treated counties are matched to never-treated counties using several county-level characteristics related to childcare demand, following Duletzki and Lim (2026). These include the childcare coverage rate for children under the age of three, the fertility rate, the female employment rate, the share of women aged 20 to 40 in the population, the childcare coverage rate for children aged three to six, and the total population. These are measured in the year immediately preceding treatment to avoid conditioning on post-treatment outcomes and to ensure that the matching variables are unaffected by the childcare expansion. Additionally, I include a measure for the level of urban development of a county by including the share of area utilized for settlement and transport. Only never-treated counties are eligible as controls. This avoids contamination from later-

treated counties and ensures that the control group is not affected by future treatment. A logit model is estimated to predict treatment assignment based on the matching variables. Each treated county is matched to the nearest never-treated county without replacement using the estimated propensity score.

Panel 1 of Figure 5 shows that prior to the expansion, both the treatment and control group had a similar level and trend in the share of children under the age of three being cared for. Yet, only those counties in the treatment group experienced a substantial increase in the childcare coverage rate in the treatment year. The sharp increase confirms the treatment definition, according to which treated counties experience a substantial expansion in childcare coverage while control counties do not. Panels 2–6 show the trend of the variables used for matching that could potentially be correlated with the expansion of early childcare. One can observe that trends are highly similar between both groups, with confidence intervals largely overlapping. The main exception to this is the childcare coverage for children between 3 and 6 years, where the difference between treated and control counties increases after treatment. One possible explanation is that municipalities expanding childcare for children under three also continue to expand childcare services for older children. Since the two childcare systems are closely linked, this pattern is not unexpected.

Table 2 reports the mean values of the variables used for matching for the treatment and control groups after the matching procedure. The normalized differences are substantially below the commonly used threshold of 0.1 for all variables (see Austin, 2009).

5 Results

5.1 Main Results

Table 3 presents the main results. Counties that experienced an increase in the early childcare coverage rate of more than five percentage points within a single year exhibit an average increase in the employment rate of 0.32 percentage points over the three years post-treatment period, relative to comparable control counties. The average effect increases when extending the post-treatment period. Over a five-year period, the average employment effect amounts to 0.42 percentage points, increasing to 0.55 percentage points over a seven-year post-treatment period. The estimated effect appears to level off at longer post-treatment horizons, remaining at 0.55 percentage points when extending the horizon from seven to ten years.[5] This pattern suggests that the labor market effects of childcare expansion increase gradually rather than immediately. To put this into context, when accounting for

[5]These findings are similar to the results from the analysis without propensity score matching in Table 4, although the estimated effects there are larger and exhibit higher levels of statistical significance.

the average pre-treatment employment rate of 85.2 percent in the treatment group, this comes down to an average relative increase in the employment rate of 0.65 percent. Although this effect appears modest, this corresponds to an average reduction in the unemployment rate of approximately 3.7 percent relative to the corresponding pre-treatment non-employment rate of 14.8 percent, indicating economically meaningful labor market effects.

When excluding mothers of children under the age of three and childcare workers, so the direct beneficiaries of the reforms, the estimated employment effect declines only slightly. The effect decreases to an average 0.54 percentage point higher employment rate in treated compared to control counties over ten years after treatment. This suggests that the observed increase in employment is not solely driven by additional childcare employment or higher maternal labor force participation, but also reflects broader spillover effects in local labor markets. Additionally, contrary to the findings of Jofre-Monseny et al. (2020), excluding individuals who migrated between treatment and control counties does not substantially reduce the estimated employment effect, as it is also only slightly reduced to 0.53 percentage points. This suggests that the observed employment gains are unlikely to be primarily driven by migration from untreated regions. This is supported by Figure 6, which indicates no substantial change in migration patterns between treatment groups after the treatment. These results suggest that employment gains are primarily driven by the pre-treatment local workforce, although other forms of migration, such as from other countries, cannot be ruled out.

In contrast to the employment effects, I find no statistically significant average effects of childcare expansion on wages (see 3). Across the different post-treatment horizons and when excluding mothers of children under the age of three and childcare workers as well as movers, the estimated coefficients remain small and statistically insignificant. Thus, while childcare expansion is associated with persistent increases in employment, these effects appear to not be accompanied by corresponding changes in average wages.

Figure 4 illustrates the dynamic evolution of both outcome variables relative to the treatment period. Figure 4a shows that treated and control counties follow similar pre-treatment trends, while employment in treated counties starts to bridge the gap between both groups after the treatment. Despite the largest jumps start appearing three years after the treatment, an initial decrease in difference can be observed in the first year after treatment. This indicates that the positive employment effects observed take time to consolidate. In contrast, Figure 4b indicates that wage trajectories remain broadly parallel throughout the observation period, consistent with the absence of statistically significant wage effects.

To better understand where the employment gains occur, Table 5 reports heterogeneous effects by occupational skill level. Although none of the subgroup estimates for employment are statistically significant, the point estimates are positive across all skill groups, with the largest estimated effect

observed for skilled occupations. This pattern is consistent with the hypothesis that childcare expansion generates local labor market spillovers that increase demand particularly for skilled labor. However, these results should be interpreted cautiously given the lack of statistical significance. For wages, I find a negative and statistically significant effect for unskilled occupations, while the estimates for skilled and high-skilled occupations are smaller and statistically insignificant. This may indicate differential effects across skill groups, although the underlying mechanisms remain unclear.

The results also show heterogeneity by gender (Table 6). The estimated employment effect is positive and statistically significant for men, amounting to 0.77 percentage points, whereas the effect for women is smaller and statistically insignificant. Since mothers are the group directly affected by childcare expansion, the presence of positive employment effects among men suggests that the estimated effects are not solely driven by increased labor supply among mothers. Instead, the results are consistent with the existence of broader local labor market spillovers.

Finally, Table 7 shows heterogeneous effects across age groups. The largest employment effect is found among individuals aged above 50, for whom the estimated effect is 0.71 percentage points and statistically significant. The effects for younger individuals and those aged 30 to 50 are smaller and statistically insignificant. This suggests that childcare expansion may generate employment gains beyond the group typically expected to benefit directly from childcare availability.

5.2 Robustness Tests

I conduct several robustness tests to assess whether the results presented above are sensitive to the specific design of the treatment definition and matching procedure. In the baseline specification, a county is classified as treated if it experiences an increase in the childcare coverage rate of more than five percentage points in a single year. However, some counties classified as controls may have experienced increases just below this threshold. If these near-treated counties are included in the control group, the estimated treatment effects may be weakened, leading to a potential bias towards zero in the estimated treatment effect.

To address this concern, an alternative specification restricts the pool of potential control counties to those that never experienced an increase in childcare coverage exceeding four percentage points during the observation period. While imposing an even stricter threshold would further reduce the risk of contamination, it would also substantially shrink the pool of eligible control counties, thereby potentially reducing the availability of suitable matches and increasing the risk of poorer variable balance. The same matching procedure and covariate specification as in the baseline analysis are applied. Table 8 presents the estimated treatment effects under this alternative specification, showing

that the results remain statistically significant and similar in scope for the employment probability. The estimated effect remains at an average of 0.33 percentage points over a three-year post-treatment period, but increases to 0.85 percentage points over a ten-year post-treatment period. The larger estimates in this specification are consistent with the possibility that partially treated counties in the baseline control group bias the estimated effect towards zero.

A second robustness exercise examines the sensitivity of the results to the treatment definition itself. Since the five percentage point threshold is based on the distribution of childcare expansions rather than a clear cutoff, I examine whether the results are sensitive to alternative treatment definitions. I choose a slightly lower threshold of 4.5 percentage points to assess whether the results are sensitive to marginal changes around the baseline cutoff. The results are displayed in Table 9, showing that a statistically significant effect remains. The estimated effect is smaller than in the baseline specification, which may partly reflect the different composition of treated and control counties and the reduced pool of potential matches. Lowering the threshold may include counties in which childcare expansions are more closely related to underlying local demand developments rather than discrete supply-side expansions, potentially affecting the interpretation of the treatment effect. Still, these findings suggest that the estimated effect is not due to the specific treatment definition.

6 Conclusion

This paper examines potential spillover effects of expansions in publicly subsidized childcare for children under the age of three, using German administrative data. The empirical strategy combines a DiD framework with propensity score matching. The identification relies on two policy reforms that generated cross-county variation in the increase of childcare coverage rates for children under three. The results indicate a statistically significant and economically meaningful rise in employment. Counties that experienced a sharp increase in childcare coverage, defined as an expansion of more than five percentage points within a single year, experienced an average increase in employment of 0.55 percentage points relative to comparable control counties over a seven-year post-treatment period. Importantly, when excluding mothers of children under three and childcare workers, two groups directly affected by the reforms, the estimated employment probability still increases by 0.54 percentage points. These findings provide evidence of broader labor market spillovers affecting individuals who did not directly benefit from the policy. Additionally, the effect appears not to be driven by migration into treated counties as excluding individuals moving between treatment groups only slightly reduces the estimated effect to 0.53 percentage points. A robustness analysis suggests that the estimated effects are likely conservative, as methodological constraints may bias the results

downward. Hence, the true effect may be larger than reported.

The absence of an average wage effect provides additional insights into the mechanisms underlying the observed employment gains. While the expansion of childcare appears to increase employment in local labor markets, these effects do not translate into systematically higher wages. One possible explanation is that the employment effects are primarily driven by adjustments in labor quantities rather than changes in the price of labor. Increased local economic activity may generate additional labor demand that is absorbed through previously unemployed or inactive workers, thereby increasing employment without creating substantial upward pressure on wages. Alternatively, the absence of wage effects may reflect the fact that local labor supply adjusts alongside labor demand. In particular, improved childcare availability increases the potential labor supply of parents, which could partly offset wage pressures arising from increased labor demand. Taken together, the results suggest that childcare expansion primarily affects the extensive margin of the local labor market, increasing employment opportunities without substantially changing average earnings.

The heterogeneous effects show a significantly stronger increase in employment probabilities for men than for women. As men are underrepresented among the groups directly affected by the expansion, this pattern supports the presence of spillover effects rather than solely direct treatment effects. The comparatively stronger response among men suggests that the childcare expansion had broader labor market implications beyond the immediately affected groups. Furthermore, the increase in employment probability is more pronounced for individuals over the age of 50. This indicates that the effects of the reform vary across age groups and that older employees benefit disproportionately from the expansion. One potential explanation could be that the effect is strongest for those with stronger attachments to local labor markets and with established networks

The results suggest that investments in publicly subsidized childcare for children under the age of three generate positive labor market effects that extend beyond the commonly assumed scope. While such investments increase maternal labor force participation following childbirth and create employment opportunities in the childcare sector, my findings indicate that they also stimulate additional employment in the broader regional labor market. Taken together, these results imply that the overall labor market impact of childcare expansion has likely been underestimated, as previous assessments may have focused primarily on the directly affected groups and neglected broader local labor market effects. Yet to better understand them and therefore make better policy decisions, a more holistic understanding is not just helpful but also necessary.

Data

Graf, Tobias; Grießemer, Stephan; Köhler, Markus; Lehnert, Claudia; Moczall, Andreas; Oertel, Martina; Schmucker, Alexandra; Schneider, Andreas; vom Berge, Philipp (2025): "Weakly anonymous Version of the Sample of Integrated Labour Market Biographies (SIAB) – Version 7523 v2". Research Data Centre of the Federal Employment Agency (BA) at the Institute for Employment Research (IAB). DOI: 10.5164/IAB.SIAB7523.de.en.v2

The data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) and remote execution.

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Tables

Table 1: Individual Characteristics

	Treatment	Control
Employment Share	0.8570	0.8622
	(0.3501)	(.8623)
Age	41.5230	41.6034
	(12.5992)	(12.6593)
Ratio of Women	0.5210	0.5179
	(0.5000)	(0.4997)
Ratio of University Graduate	0.0855	0.0741
	(0.2797)	(0.2619)
After Treatment	0.5320	0.5294
	(0.4990)	(0.4991)
Childcare Worker	0.0197	0.0202
	(0.1391)	(0.1407)
Mother (Youngest Child ≤ 3)	0.0079	0.0074
	(0.0883)	(0.0855)
Moved	0.0027	0.0026
	(0.0515)	(0.0154)
N	1,722,111	1,721,967
Daily Wage	69.6891	70.9736
	(49.7986)	(49.7377)
N	1,310,644	1,294,701

Notes: The Table displays the mean values of West German individuals who lived in one of the counties included in the analysis after matching ten or fewer years before or after the sudden and significant expansion of childcare between 2007 and 2014. Excluded are people in training, interns and students, people in (partial) retirement, cross-county commuters, people unable to work or not permanently employed. All values were rounded to four decimals.

Table 2: Characteristics of Treated and Control Counties After Matching

	Treatment	Control	Normalized Difference
Early Childcare Coverage Rate	0.1694	0.1664	0.0486
	(0.0635)	(.0562)	
Kindergarten Coverage Rate	0.9255	0.9248	0.0154
	(0.0451)	(0.0455)	
Fertility Rate	0.0683	0.0682	0.0244
	(0.0061)	(0.0068)	
Female Employment Rate	0.4798	0.4799	-0.0027
	(0.0375)	(0.0412)	
Share of Women Aged 20-40	0.1147	0.1153	-0.0415
	(0.0170)	(0.0128)	
Population Size	153,375.5	150,368.8	0.0375
	(88,110.4)	(71,363.17)	
Number of Observations	105	105	

Notes: The Table displays the mean values of county-specific characteristics for both the treated and control groups of counties one year prior to the early childcare expansion separately. The early childcare coverage rate and the kindergarten coverage rate are both calculated as the share of childcare enrolled in publicly subsidized childcare among all children aged 0-2 years and 3-6 years in each county and for every year, respectively. The fertility rate is calculated as the number of newborn babies per woman between 20 and 40 years of age. The female employment rate is calculated containing all women between the ages 20 and 65. The normalized differences between the treatment and control groups were calculated following the approach proposed by Imbens and Rubin (2015). All values were rounded to four decimals.

Table 3: Difference-in-Differences Estimates with Propensity Score Matching

	Panel A: Baseline			
Employment	0.0032*	0.0042**	0.0055***	0.0055**
	(0.0017)	(0.0019)	(0.0021)	(0.0028)
Pre-Treatment	0.8417	0.8395	0.8431	0.8524
Observations	1,176,064	1,850,584	2,521,953	3,444,078
Daily Wage	-0.1036	-0.1006	-0.0130	-0.1809
	(0.1599)	(0.1813)	(0.2035)	(0.2389)
Pre-Treatment	67.1452	67.4379	67.9859	68.9157
Observations	1,005,218	1,581,064	2,045,384	2,605,345
	Panel B: Excluding Direct Beneficiaries			
Employment	0.0032*	0.0040**	0.0052**	0.0054*
	(0.0017)	(0.0020)	(0.0022)	(0.0029)
Observations	1,144,974	1,801,253	2,453,847	3,350,462
Daily Wage	-0.1281	-0.1587	-0.0892	-0.2717
	(0.1616)	(0.1854)	(0.2082)	(0.2449)
Observations	976,687	1,535,892	1,987,558	2,533,535
	Panel C: Excluding Movers			
Employment	0.0030*	0.0040**	0.0053**	0.0053*
	(0.0017)	(0.0019)	(0.0021)	(0.0027)
Observations	1,173,879	1,846,640	2,515,958	3,434,936
Daily Wage	-0.1045	-0.0993	-0.0155	-0.1881
	(0.1589)	(0.1807)	(0.2026)	(0.2380)
Observations	1,004,171	1,579,059	2,042,620	2,601,814
Max. Dist.	3	5	7	10
Controls	YES	YES	YES	YES
County FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Notes: The sample consists of individuals who lived in one of the West German counties that experienced an increase by more than five percentage points in a single year or were chosen as matches. Excluded are people in training, interns and students, people in (partial) retirement, cross-county commuters, people unable to work or not permanently employed. Panel A shows the results for the total sample, Panel B excludes direct beneficiaries, so childcare workers and mothers of children under the age of 3, and Panel C excludes people that moved between treatment groups after the treatment. All values were rounded to four decimals; Employment: probability of being employed; Pre-Treatment: mean of the respective outcome for the treatment group across all pre-treatment years; Daily Wage: daily wages, deflated to 2010; Max. Distance: maximum distance of the observations to the expansion in years; FE: fixed effects; * $p<0.10$, ** $p<0.05$, *** $p<0.01$; standard errors are presented in parentheses and clustered at the county-level

Table 4: Difference-in-Differences Estimates without Propensity Score Matching

	Panel A: Baseline			
Employment	0.0057***	0.0067***	0.0093***	0.0116***
	(0.0017)	(0.0018)	(0.0021)	(0.0026)
Observations	2,527,166	3,987,437	5,436,915	7,559,151
Daily Wage	0.3801**	0.4756**	0.5577**	0.5352**
	(0.1794)	(0.1933)	(0.2192)	(0.2519)
Observations	2,138,534	3,366,408	4,556,787	5,793,142
	Panel B: Excluding Direct Beneficiaries			
Employment	0.0059***	0.0067***	0.0093***	0.0117***
	(0.0017)	(0.0019)	(0.0021)	(0.0027)
Observations	2,464,721	3,886,994	5,297,269	7,361,157
Daily Wage	0.3312*	0.4024**	0.4742**	0.4477*
	(0.1874)	(0.1973)	(0.2238)	(0.2573)
Observations	2,081,597	3,275,112	4,431,538	5,637,694
	Panel C: Excluding Movers			
Employment	0.0080***	0.0093***	0.0122***	0.0149***
	(0.0017)	(0.0018)	(0.0021)	(0.0026)
Observations	2,519,693	3,974,122	5,416,579	7,527,465
Daily Wage	0.4006**	0.4997**	0.5867***	0.5610**
	(0.1783)	(0.1930)	(0.2187)	(0.2513)
Observations	2,134,684	3,359,321	4,546,331	5,780,251
Max. Dist.	3	5	7	10
Controls	YES	YES	YES	YES
County FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Notes: The sample consists of individuals who lived in one of the West German counties that experienced an increase by more than five percentage points in a single year or were chosen as matches. Excluded are people in training, interns and students, people in (partial) retirement, cross-county commuters, people unable to work or not permanently employed. Panel A shows the results for the total sample, Panel B excludes direct beneficiaries, so childcare workers and mothers of children under the age of 3, and Panel C excludes people that moved between treatment groups after the treatment. All values were rounded to four decimals; Employment: probability of being employed; Daily Wage: daily wages, deflated to 2010; Max. Distance: maximum distance of the observations to the expansion in years; FE: fixed effects; * $p<0.10$, ** $p<0.05$, *** $p<0.01$; standard errors are presented in parentheses and clustered at the county-level

Table 5: Heterogeneity by Skill Level

	Unskilled	Skilled	High-skilled
Employment	0.0014	0.0017	0.0009
	(0.0038)	(0.0022)	(0.0025)
Observations	579,450	2,193,629	471,851
Wages	-1.2246*	-0.2080	-0.1051
	(0.6690)	(0.2476)	(0.6964)
Observations	411,146	1,791,412	374,318
Composition	-0.0040	0.0042	-0.0002
	(0.0029)	(0.0031)	(0.0022)
Observations	2,931,190	2,931,190	2,931,190
Controls	YES	YES	YES
County FE	YES	YES	YES
Years FE	YES	YES	YES

Notes: The table shows the cumulative effect for five years prior and after the treatment. The sample consists of individuals who lived in one of the West German counties that experienced an increase by more than five percentage points in a single year or were chosen as matches. Excluded are people in training, interns and students, people in (partial) retirement, cross-county commuters, people unable to work or not permanently employed. Observations are grouped by skill level required for the current or last job, with Unskilled being unskilled or semiskilled tasks, Skilled including skilled tasks, and High-skilled being complex or highly complex tasks. All values were rounded to four decimals; Employment: probability of being employed; Daily Wage: daily wages, deflated to 2010; Max. Distance: maximum distance of the observations to the expansion in years; FE: fixed effects; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors are presented in parentheses and clustered at the county-level

Table 6: Heterogeneity by Gender

	Male	Female	All
Employment	0.0077**	0.0034	0.0055**
	(0.0030)	(0.0032)	(0.0028)
Observations	1,654,984	1,789,094	3,444,078
Wages	-0.5120	0.0210	-0.1809
	(0.3995)	(0.2331)	(0.2389)
Observations	1,226,322	1,307,213	2,605,345
Controls	YES	YES	YES
County FE	YES	YES	YES
Year FE	YES	YES	YES

Notes: The table shows the cumulative effect for five years prior and after the treatment. The sample consists of individuals who lived in one of the West German counties that experienced an increase by more than five percentage points in a single year or were chosen as matches. Excluded are people in training, interns and students, people in (partial) retirement, cross-county commuters, people unable to work or not permanently employed. All values were rounded to three decimals; Employment: probability of being employed; Wage: daily wages, deflated to 2010; Max. Distance: maximum distance of the observations to the expansion in years; * $p<0.10$, ** $p<0.05$, *** $p<0.01$; standard errors are presented in parentheses and clustered at the county-level

Table 7: Heterogeneity by Age

	<30	30-50	>50
Employment	0.0047	0.0040	0.0071**
	(0.0041)	(0.0033)	(0.0033)
Observations	750,121	1,708,681	985,276
Wages	-0.3138	-0.0322	-0.1525
	(0.4180)	(0.3741)	(0.5158)
Observations	550,812	1,336,781	717,752
Controls	YES	YES	YES
County FE	YES	YES	YES
Year FE	YES	YES	YES

Notes: The table shows the cumulative effect for five years prior and after the treatment. The sample consists of individuals who lived in one of the West German counties that experienced an increase by more than five percentage points in a single year or were chosen as matches. Excluded are people in training, interns and students, people in (partial) retirement, cross-county commuters, people unable to work or not permanently employed. All values were rounded to three decimals; Employment: probability of being employed; Wage: daily wages, deflated to 2010; Max. Distance: maximum distance of the observations to the expansion in years; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors are presented in parentheses and clustered at the county-level

Table 8: Robustness (Control Group no increase ≥4pp)

	Panel A: Baseline			
Employment	0.0033**	0.0050***	0.0072***	0.0085***
	(0.0017)	(0.0019)	(0.0021)	(0.0027)
Observations	1,363,503	2,146,245	2,925,648	3,999,790
Daily Wage	-0.0888	-0.0277	0.0370	-0.1205
	(0.1585)	(0.1924)	(0.2237)	(0.2602)
Observations	1,160,540	1,825,824	2,362,464	2,996,699
	Panel B: Excluding Direct Beneficiaries			
Employment	0.0031*	0.0048**	0.0070***	0.0084***
	(0.0017)	(0.0019)	(0.0021)	(0.0028)
Observations	1,328,376	2,090,482	2,848,828	3,894,093
Daily Wage	-0.1176	-0.0943	0.0102	-0.0866
	(0.1677)	(0.1988)	(0.2222)	(0.2454)
Observations	1,176,040	1,854,570	2,410,907	3,085,088
	Panel C: Excluding Movers			
Employment	0.0035**	0.0053***	0.0076***	0.0090***
	(0.0016)	(0.0019)	(0.0020)	(0.0027)
Observations	1,360,992	2,141,740	2,918,791	3,989,423
Daily Wage	-0.0822	-0.0235	0.0411	-0.1173
	(0.1580)	(0.1921)	(0.2232)	(0.2599)
Observations	1,159,311	1,823,521	2,359,411	2,992,962
Max. Dist.	3	5	7	10
Controls	YES	YES	YES	YES
County FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Notes: The sample consists of individuals who lived in one of the West German counties that experienced an increase by more than five percentage points in a single year or were chosen as matches (not exceeding an increase by more than 4 percentage points in a single year). Excluded are people in training, interns and students, people in (partial) retirement, cross-county commuters, people unable to work or not permanently employed. Panel A shows the results for the total sample, Panel B excludes direct beneficiaries, so childcare workers and mothers of children under the age of 3, and Panel C excludes people that moved between treatment groups after the treatment. All values were rounded to four decimals; Employment: probability of being employed; Wage: daily wages, deflated to 2010; Max. Distance: maximum distance of the observations to the expansion in years; FE: fixed effects; * $p<0.10$, ** $p<0.05$, *** $p<0.01$; standard errors are presented in parentheses and clustered at the county-level

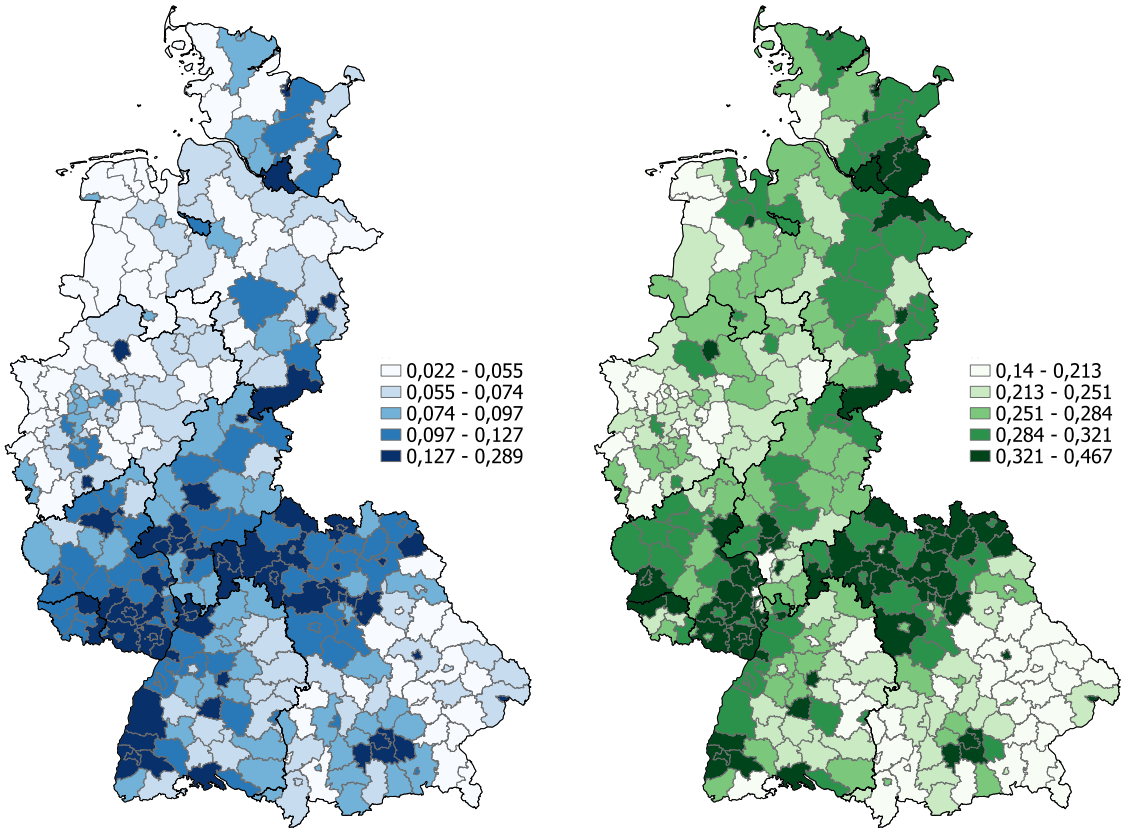
Table 9: Robustness (Treatment Threshold of 4.5 pp)

	Panel A: Baseline			
Employment	0.0035**	0.0033*	0.0042*	0.0038
	(0.0016)	(0.0019)	(0.0022)	(0.0025)
Observations	1,933,591	3,039,845	4,143,118	5,650,076
Daily Wage	-0.0957	-0.0225	0.0543	-0.1052
	(0.1342)	(0.1672)	(0.1905)	(0.2237)
Observations	1,651,873	2,598,557	3,355,985	4,287,043
	Panel B: Excluding Direct Beneficiaries			
Employment	0.0032*	0.0031	0.0040*	0.0037
	(0.0017)	(0.0020)	(0.0023)	(0.0026)
Observations	1,883,889	2,961,191	4,034,560	5,500,776
Daily Wage	-0.1015	-0.0448	0.0173	-0.1504
	(0.1400)	(0.1728)	(0.1965)	(0.2283)
Observations	1,606,476	2,526,644	3,263,953	4,172,030
	Panel C: Excluding Movers			
Employment	0.0037**	0.0036*	0.0045**	0.0042*
	(0.0016)	(0.0019)	(0.0022)	(0.0025)
Observations	1,928,023	3,029,811	4,127,990	5,627,324
Daily Wage	-0.0860	-0.0084	0.0646	-0.0980
	(0.1341)	(0.1677)	(0.1906)	(0.2236)
Observations	1,649,026	2,593,158	3,348,825	4,277,866
Max. Dist.	3	5	7	10
Controls	YES	YES	YES	YES
County FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Notes: The sample consists of individuals who lived in one of the West German counties that experienced an increase by 4.5 percentage points or more in a single year or were chosen as matches. Excluded are people in training, interns and students, people in (partial) retirement, cross-county commuters, people unable to work or not permanently employed. Panel A shows the results for the total sample, Panel B excludes direct beneficiaries, so childcare workers and mothers of children under the age of 3, and Panel C excludes people that moved between treatment groups after the treatment. All values were rounded to four decimals; Employment: probability of being employed; Wage: daily wages, deflated to 2010; Max. Distance: maximum distance of the observations to the expansion in years; FE: fixed effects; * $p<0.10$, ** $p<0.05$, *** $p<0.01$; standard errors are presented in parentheses and clustered at the county-level

Figures

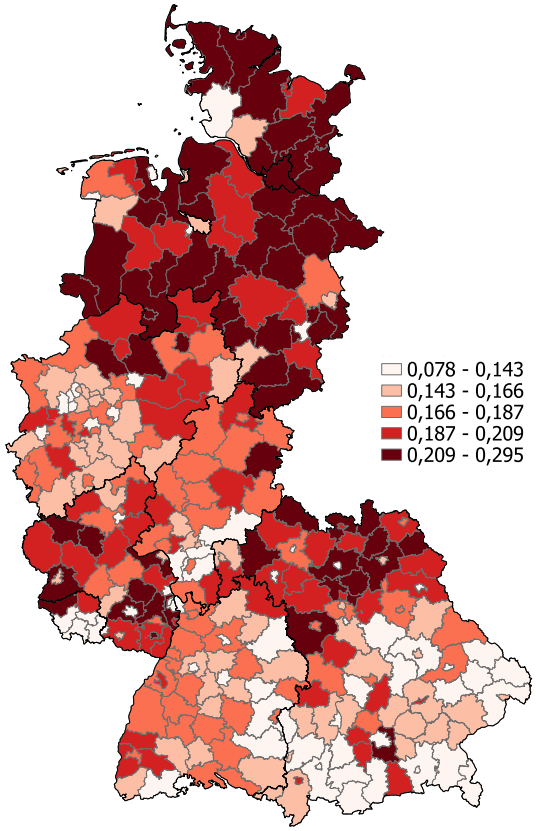
Figure 1: Childcare Coverage Rate by County



(a) Beginning of 2007 (b) End of 2014

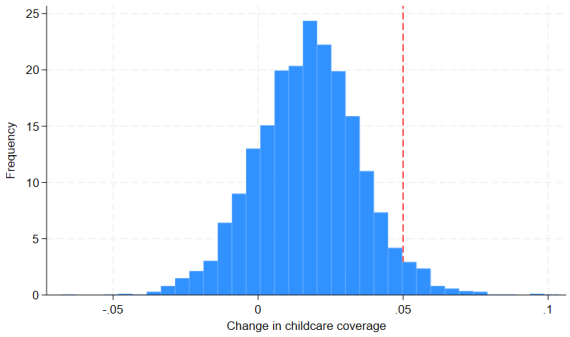
Note: This figure depicts the early childcare coverage in each West German county in 2007 (a) and 2014 (b). The childcare coverage is calculated by dividing the number of children under the age of three utilizing childcare in a county by the total number of children in a county younger than 3 years.

Figure 2: Change in Childcare Coverage Rate between 2007 and 2014



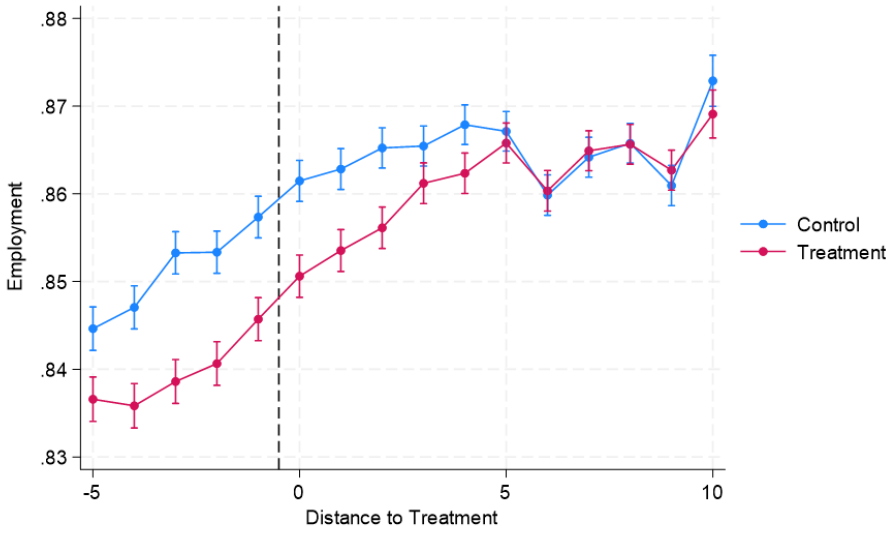
Note: This figure depicts the change in the early childcare coverage rate between 2007 and 2014 by county. It is calculated by subtracting the childcare coverage in 2007 from that in 2014.

Figure 3: Distribution of Annual Childcare Coverage Increases

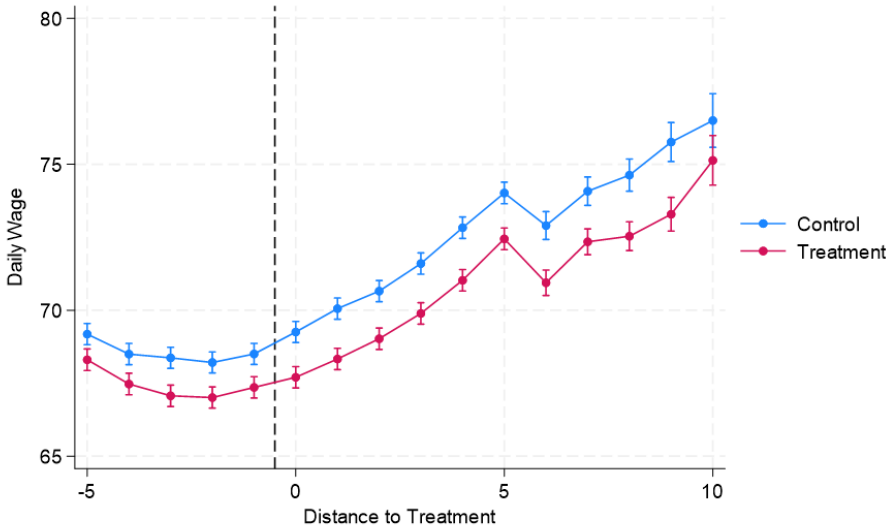


Note: This figure depicts the distribution of childcare increases per year in West German counties between 2007 and 2014. The red line indicates the threshold of 0.05 for treatment.

Figure 4: Trend Prior and After the Treatment



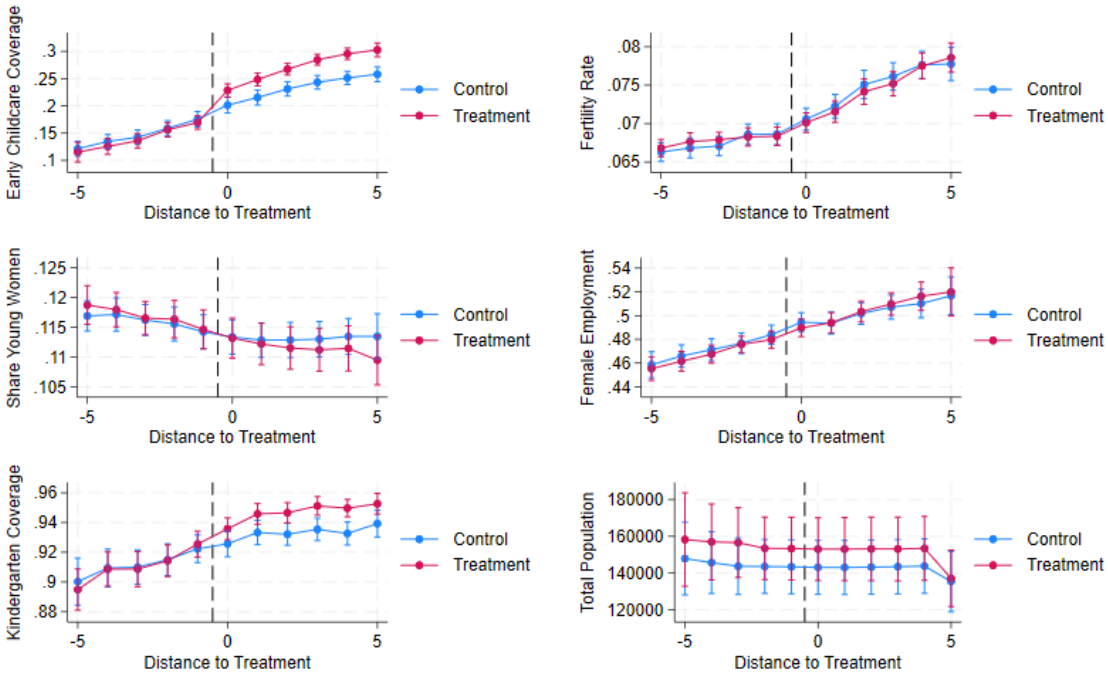
(a) Trend of Employment



(b) Trend of Daily Wages

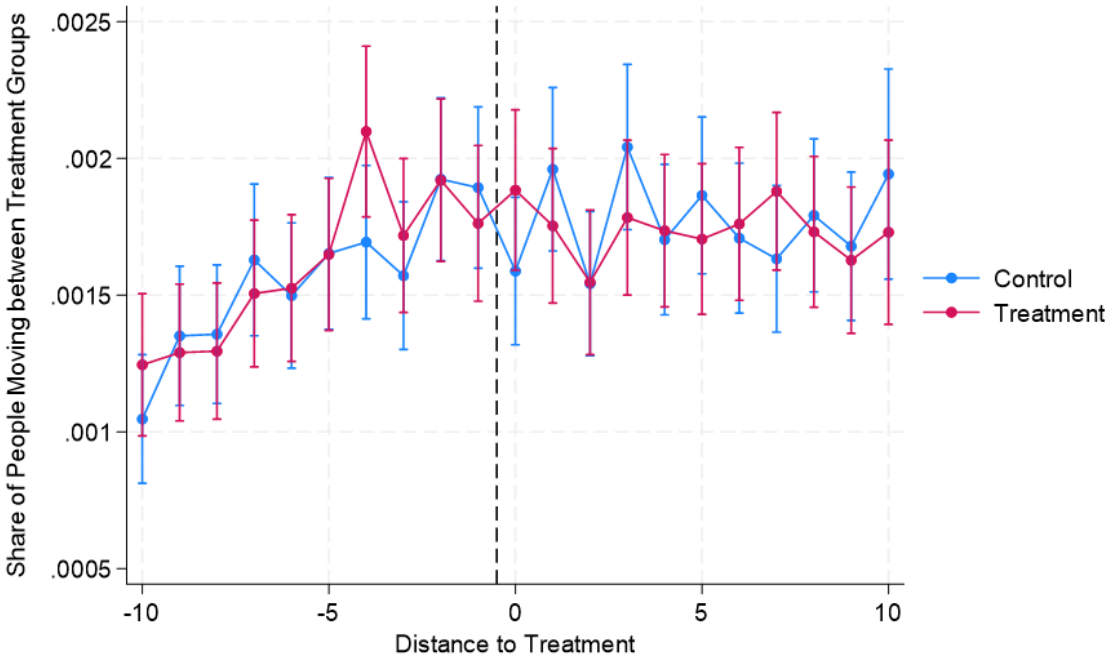
Note: These figures depict the trend of the two outcome variables over time. Treated counties are those that experience between 2007 and 2014 a sudden and significant increase in the childcare coverage rate for children under the age of 3 by more than five percentage points within a single year. The first time they experienced this increase marks the treatment at time 0. The control counties consist of those counties that never experience such an increase and were chosen as matches for the treated counties.

Figure 5: Trend of the Variables used for Matching



Note: This figure depicts the trends of the variables used for matching, differentiating between control and treated counties. Treated counties are those that experience between 2007 and 2014 a sudden and significant increase in the childcare coverage rate for children under the age of 3 by more than five percentage points within a single year. The control counties consist of those counties that did never experience such an increase and were chosen as matches for the treated counties with propensity scoring. Early Childcare measures the share of children under the age of 3 in childcare,

Figure 6: Movement into and out of Treatment



Note: This figures depict the share of people moving between treatment and control group over time. Treatment indicates the share of people moving from control into treatment counties and control the share of people moving from treatment into control counties.