Can Early Public Childcare Reduce Child Penalties? - Evidence from Germany*

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Abstract

This paper studies the impact of the expansion of public childcare for children under age three on mothers' child penalties, exploiting policy reforms implemented in Germany. Leveraging county-level variation in the timing and intensity of the expansion, we find that a 20-percentage-point increase in early childcare coverage during the reform period reduced mothers' earnings penalties by 4.5 percentage points, accounting for about 56% of the total 8-percentage-point decline in earnings penalties over the same period. The effect is long-lasting and driven by increases in employment and days worked, especially in full-time and non-marginal positions, as well as by higher daily wages and a greater likelihood of working in higher-paying firms and occupations. We identify two key contextual factors explaining the positive effects of early childcare: the initial level of child penalties and reliance on informal care. In settings with high child penalties and limited alternatives, early public childcare can yield substantial lasting benefits for mothers' careers. Our study underscores the importance of context for evaluating public childcare provision.

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1 Introduction

Persisting gender gaps in modern labor markets can largely be attributed to the divergence in career paths experienced by women following childbirth, commonly referred to as the "child penalty" (Bertrand et al., 2010; Angelov et al., 2016; Kleven et al., 2019b). Publicly funded childcare provides an alternative to maternal childcare, helping mothers stay connected to the labor market. (Olivetti and Petrongolo, 2017). In particular, public childcare in the early years after birth may help support mothers' career continuity (Carta and Rizzica, 2018) by bridging the gap between parental leave and the near-universal childcare available from age three in many countries.

Given its importance, a growing body of literature has investigated the effects of public childcare provision for very young children (specifically for those under three, hereafter referred to as "early childcare") on maternal labor supply. However, because early childcare expansion has occurred more recently and in fewer countries, there are fewer studies on this topic than on public childcare for children aged three and above (OECD, 2017). Furthermore, existing studies on early childcare provision have reported mixed results, which raises the question of whether and under which conditions expanding early childcare reduces child penalties.

In this paper, we address this question by examining the effects of early childcare expansion in West Germany.[1] Between 2005 and 2014, the German federal government significantly increased public childcare supply for children under age three. This expansion was particularly pronounced in West Germany, where traditional gender norms led mothers to stay home and care for children, resulting in underdeveloped formal childcare services and low maternal labor supply (Lewis, 2009; Campa and Serafinelli, 2019; Lippmann et al., 2020). Leveraging the variation in the timing and magnitude of early childcare expansion at the county-year level, we analyze how early childcare expansion affects mothers' child penalties. To address concerns about endogeneity in the expansion process, we employ a matched event-study design that exploits expansion shocks likely driven by supply factors. Through matching, we construct a control group of counties with similar pre-trends in variables that may be correlated with childcare demand. Furthermore, we conduct robustness checks to address potential issues with selection, regional economic shocks, and parallel reforms. Because the data set does not include information on individual childcare take-up, we identify the intention-to-treat (ITT) effects of early childcare availability on maternal labor supply, which allows us to evaluate the scalability of expanding childcare supply.

Using rich administrative data from Germany, this paper first documents the trend of child penalties in West Germany since the 2000s. We identify all first-time mothers in the data who gave birth between 2001 and 2014 and estimate yearly child penalties for earnings and days worked, as

[1] From 1945 to 1990, the country was divided into two parts: socialist East Germany (GDR) and capitalist West Germany (FRG). We refer to them as East and West Germany from here on.

well as monthly penalties for employment, daily wages, full-time and non-marginal employment. We observe that child penalties in all outcomes have decreased substantially throughout this time period. Motivated by this finding, we analyze whether and how providing early childcare affected mothers' child penalties.

Our main results show that expanding early childcare significantly reduces mothers' child penalties. Our estimates indicate that a 10-percentage-point increase in the early childcare coverage rate reduces the maternal earnings penalty by approximately 2.3 percentage points during the three years following childbirth. Considering that the early childcare coverage rate in West Germany increased by 20 percentage points and that the earnings penalty decreased by around 8 percentage points during the reform period, our estimates suggest that approximately 56% of this reduction is attributable to early childcare expansion. The effect is driven by increases in labor supply at both the extensive and intensive margins, accompanied by higher daily wages and a greater likelihood of working in higher-paying firms and occupations. While the effects on employment and days worked occur primarily between one and three years after childbirth, the impacts on earnings and wages persist for up to seven years after birth. Our results suggest that early childcare can help mothers advance their careers beyond the childcare eligibility period by enabling them to remain in full-time, non-marginal, and higher-paying positions.

To better understand in which context childcare expansion can produce positive effects, we conduct regional analyses within Germany, leveraging within-country variation. Our findings suggest that early childcare expansion increases maternal labor supply, particularly in regions with high initial child penalties and limited alternative childcare options. In East Germany, where more egalitarian gender norms and established childcare infrastructures predated the reforms, the effects are about one-fourth as large as those in West Germany. Within West Germany, counties with higher initial child penalties experience greater effects. Our analysis also reveals crowding-out effects: states with greater pre-reform reliance on informal childcare, such as care provided by grandparents, show less pronounced benefits from early childcare expansion.[2]

This paper contributes to the literature on early childcare provision, which has produced mixed evidence for maternal labor supply. While Goux and Maurin (2010), Attanasio et al. (2022), Kleven et al. (2024), and Rabaté and Rellstab (2022) report no significant effect of early public childcare on maternal labor supply in France, Brazil, Austria, and the Netherlands, respectively, several other studies document positive effects in different contexts, including Kunze and Liu (2019) and Andresen and Havnes (2019) in Norway, Baker et al. (2008) in Québec, Carta and Rizzica (2018) in Italy, and Nollenberger and Rodríguez-Planas (2015) in Spain.

We contribute to this literature in three main ways. First, we examine a range of labor market

[2]Grandparental care is a common form of informal childcare and produces "grandchild penalties" itself (Gørtz et al., 2020).

outcomes beyond labor force participation, including daily wages, full-time employment, mini-jobs, and average wage levels across occupations and firms. The existing literature focuses predominantly on labor force participation, with some studies showing effects on part-time versus full-time employment or hours worked (Müller and Wrohlich, 2020; Andresen and Havnes, 2019; Bettendorf et al., 2015).[3] The effects at the occupation and firm level provide valuable insights into the quality of positions mothers return to when public childcare is available. This, in turn, offers a deeper understanding of the mechanisms underlying child penalties and their interaction with family policies (Kleven et al., 2024; Rabaté and Rellstab, 2022).

Second, we investigate the effects on all outcomes for up to seven years after birth, providing an analysis on the persistence of these effects. While some studies show estimates for different cohorts relative to the childcare expansion (Nollenberger and Rodríguez-Planas, 2015; Kleven et al., 2024), others show the individual-level persistence of the effects (Kleven et al., 2024; Rabaté and Rellstab, 2022).[4] However, those persistence analyses focus only on earnings and employment. We contribute by examining long-term effects both across cohorts relative to the childcare expansion and within individuals relative to birth for all outcomes. This extensive analysis sheds light on how short- and long-term improvements interact. While effects on employment and days worked are concentrated in the eligibility period, effects on earnings, daily wages, and full-time employment persist for up to seven years after childbirth. These findings suggest that early childcare provision can help mothers remain attached to the labor market, particularly in better-paying jobs and more stable full-time positions, and thereby mitigate long-term earnings losses following childbirth.

Finally, we examine two factors that may explain the varying effects of early public childcare found in the existing literature. We find that effects of early childcare expansion are larger in regions with higher initial child penalties and lower reliance on informal childcare. Previous studies are set in very different contexts, ranging from countries with minimal child penalties, such as Norway (Kunze and Liu, 2019; Andresen and Nix, 2019), to settings with substantial penalties, such as Spain (Nollenberger and Rodríguez-Planas, 2015) and Germany.[5] There is also considerable variation in the reliance on informal childcare (OECD, 2021). Norway, Spain, and Germany show relatively low levels of informal care, whereas the Netherlands, Austria (Kleven et al., 2024), and Italy (Carta and Rizzica, 2018) have a much higher reliance. Some studies present evidence suggesting that crowding out from informal to public childcare may help explain the negligible

[3] Regarding the context of West Germany, our paper is closely related to Müller and Wrohlich (2020). However, important distinctions arise in terms of data, identification strategy, and outcomes. While Müller and Wrohlich (2020) find positive effects mainly on part-time employment using a household panel dataset (Microcensus) and a generalized difference-in-differences design, our study identifies positive effects across a broader set of outcomes, drawing on administrative data and a matched event-study design. Our paper also reveals two contextual factors that help explain the heterogeneous effects of early public childcare provision.

[4] Attanasio et al. (2022) assess the effects at three points in time after enrollment.

[5] Kleven et al. (2023) present child penalties in employment across countries.

effects they observe (Attanasio et al., 2022; Kleven et al., 2024). In contrast, Nollenberger and Rodríguez-Planas (2015) find no evidence for crowding-out. We contribute to the literature by examining how treatment effects differ along these two dimensions using regional variation within a country. While comparisons across countries are challenging because they rely on different policies, institutional contexts, and identification strategies, our analysis examines these factors within a homogeneous setting and provides internally consistent evidence for their importance, highlighting the crucial role of context in determining policy effectiveness.

2 Institutional Background

German family policy has shifted from reinforcing the male-breadwinner model to facilitating dual-earner constellations and work–family reconciliation. This transition has been particularly evident in West Germany, where the near absence of early childcare provision fostered prolonged maternal employment interruptions (Hook, 2015). Shortly after the division, both East and West Germany introduced maternity leave laws that entitled mothers to six weeks of leave before and six weeks after childbirth (now eight weeks after birth), with full wage replacement and employment protection. However, the socialist regime in East Germany established a more gender-egalitarian family policy in the following years, offering longer paid leave and near-universal childcare. Most mothers took around 12 months of leave, and childcare coverage for children aged one to three exceeded 80% (Boelmann et al., 2025; Brückner et al., 2020; Fauser et al., 2025). After reunification, substantial differences in early childcare became particularly apparent: although the public childcare coverage for children aged 0-2 declined markedly in East Germany (from over 55% in 1989 to around 35% in 2002), it remained far higher than in West Germany, where the coverage was only about 2% (Brückner et al., 2020).[6] Given the very low level of coverage in West Germany compared to East Germany and other European countries,[7] there was broad public support for the early childcare expansion (Sharma and Steiner, 2008; Wrohlich, 2008).

To increase the supply of early childcare, especially in West Germany, the German government embarked on a series of reforms. In 2005, the federal daycare expansion law (*Tagesbetreuungsausbaugesetz*) aimed to expand childcare for children under three by creating 230,000 additional slots, thus increasing coverage to 17% by 2010.[8] In 2007, the federal and local governments agreed to reach a coverage rate of 35% by 2013. While expanding early childcare, Germany also reformed

[6] Kindergarten for children aged 3–6 was relatively well developed, and the coverage rate reached almost 90% in the early 2000s in both East and West Germany (Bauernschuster and Schlotter, 2015).

[7] Other Western European countries exhibited higher coverage rates during this period, including Belgium (81%), the Netherlands (35%), and Luxembourg (19%) (Felfe and Lalive, 2012).

[8] The official policy goals were to address the growing demand for childcare, increase maternal labor supply as well as fertility rates, and meet Western European standards for the quantity and quality of public childcare (Bauernschuster et al., 2016; Felfe and Lalive, 2012; Müller and Wrohlich, 2020).

its parental leave system in 2007. The new scheme replaced a means-tested allowance with a universal benefit equal to 65% of the previous year's average net earnings for 12 months, plus two additional months if the second parent takes leave (up to 14 months total).[9] This system was implemented simultaneously across all regions and remained unchanged during our sample period (2007–2014), covering the key birth cohorts targeted by the childcare expansion since 2007.[10] In 2008, a legal entitlement to childcare slots for 1–2-year-olds was introduced and implemented in stages, with full implementation by 2013 (*Kinderförderungsgesetz*). Following the introduction of this legal claim, the reform-induced expansion of early childcare officially concluded in 2014.

As a result, the early childcare coverage rate increased by more than 20 percentage points in West Germany during the reform period. Figure 1 shows that the early childcare in West Germany increased from very low levels to around 30% over the 2005–2014 expansion period.[11] In contrast, the coverage in East Germany started from levels already exceeding 30% and further increased by approximately 12 percentage points. Figure 2 depicts the striking contrast in early childcare provision between East and West Germany at the county level. The coverage rates in West Germany were still low across all regions in 2006 but increased significantly throughout the reform period.

Early childcare provision in Germany is primarily organized by municipalities and non-profit providers, with private for-profit centers accounting for only a small share of facilities.[12] Both public and non-profit childcare are heavily subsidized by federal and local governments, with funding managed through local youth welfare offices (Müller and Wrohlich, 2020). Fees are typically income-based and reduced for families with multiple children (Stürmer-Heiber, 2022; Bauernschuster et al., 2016). In 2018, the average cost of a full-time early childcare slot was about €250 per month (Hoernig, 2023). Parents can choose the number of hours their child attends care. Out of all children under age three who attend childcare, roughly 75 percent spend more than 30 hours per week in care (Mills et al., 2014).[13] Childcare slots are generally allocated locally by municipalities, with youth welfare offices assigning slots based on assessed need, giving priority to working single parents and families with siblings already enrolled. While waiting lists are common, all families

[9]Parental leave benefits before 2007 were means-tested and not particularly generous. Only low-income households (€30,000 net per year for couples, €23,000 for single parents) would receive benefits (€300 per month for up to 24 months, or €450 per month for up to 12 months). Since 2007, all parents who worked before birth have become eligible for benefits. The wage replacement amount has a minimum level of €300 and is capped at €1,800.

[10]In our main analysis, we include birth cohorts only from 2007 to have a sample of mothers under the same parental leave scheme. Because the parental leave policy remained unchanged during our sample period, our estimates should be identified from variation in childcare expansion. Nevertheless, we test whether this parallel reform confounds the estimates and find consistent results in robustness checks (see Section 6.1.3).

[11]Data on coverage rates are available in 2002, and then from 2006 on a yearly basis (Regionalstatistik, 2023).

[12]For example, in 2022, for-profit providers covered less than 5% of the childcare market in Germany (Destatis, 2023). Private childcare provision is rare in Germany due to strict regulation, high entry costs, low profitability, and persistent staff shortages, as fees and wages are publicly regulated and operating without subsidies would make childcare unaffordable (Muehler, 2010).

[13]The number of hours in childcare comes from Eurostat, using the variable called "$ilc_caindformal$".

are legally entitled to a childcare place. Quality standards are enforced nationally through regulations on staff–child ratios and qualification requirements (Felfe and Lalive, 2018; Stürmer-Heiber, 2022).

3 Data and Sample

Our study relies on data from multiple sources. We obtain labor market data using the Integrated Employment Biographies (IEB version 16.00.01), which is a German administrative social security data set provided by the Institute for Employment Research (IAB). The IEB is a longitudinal data set encompassing the employment records of all German employees covered by social security, excluding self-employed individuals, military personnel and civil servants. It comprises complete employment biographies from 1975 to 2021 and includes information on daily wages, part-time employment, firm identifiers,[14] and detailed occupation codes. To incorporate employer characteristics, we merge firm-level variables from the Establishment History Panel (BHP) with the IEB using the firm identifier associated with each employment spell.

To identify mothers in the IEB, we follow the approach proposed by Müller and Strauch (2022) as the administrative data do not contain direct information on childbirth. Employers in Germany are legally required to report employment interruptions due to maternity leave in the employment notifications. Since mothers are entitled and obliged to take at least 14 weeks of maternity leave (six weeks before and eight weeks after childbirth), we can approximate the date of childbirth using the date on which the mother was de-registered from employment. In line with the child-penalty literature, which focuses on women's divergence in labor market trajectories after first childbirth, we restrict our sample to births identified as a woman's first birth in the data.[15] Using this procedure, we extract all women identified as having a first birth between 2001 and 2014, resulting in approximately 240,000 mothers per year in our sample.

With respect to information on the early childcare coverage rate, we use data provided by the German Statistical Office (Regionalstatistik, 2023). Dividing the number of children under age three enrolled in public childcare by the overall number of children under age three gives us a measure for early childcare coverage at the county-year level. Although the childcare reform was implemented between 2005 and 2014, we consider the sample period from 2007 to 2014 for

[14] The data are based on establishment information. In this paper, we use the term "firm" interchangeably.

[15] A limitation of this approach is that we can identify births if the mother was employed prior to childbirth. For first births, we cross-validate our sample using the German Socio-Economic Panel (SOEP) survey showing that 80% of women were employed before their first birth during 2007–2014, indicating that our sample is highly representative. Moreover, we confirm that the number (about 240,000 per year) of first births identified in our administrative data covers on average 75% of first births reported in official statistics. For higher-order births, we cannot fully capture cases in which mothers do not return to work before having another child. However, as discussed in Section 5.2, we suggest that our estimates based on first births likely provide a lower bound of the effect.

several reasons. First, data on early childcare coverage rates at the county level are available for 2006 onward on yearly level. Second, the exclusion of 2005 and 2006 ensures greater consistency in the sample period, especially considering the changes in the parental leave regime that took effect in 2007. Additionally, the childcare expansion resulting from the reforms was slow in the early years due to a lack of preparedness as shown in Panel (a) of Figure 2.

Additionally, we use survey data from the German Socio-Economic Panel (SOEP), which is a nationally representative longitudinal survey that has been conducted annually since 1984. It contains rich information on individuals' demographics, employment, income, and family circumstances. We use the SOEP primarily to construct a measure of reliance on informal childcare, based on survey questions asking whether families use grandparents, neighbors, friends, or others for their child under age three. The sample includes households with children under age three between 2001 and 2014.

4 Identification Strategy

4.1 Identifying Assumptions

During the childcare reform period in Germany, counties experienced substantial regional variation in the timing and intensity of early childcare expansion. Appendix Figure A.1 shows that counties experienced childcare expansion in different years and to varying extents. We leverage this regional variation to identify the impact of early childcare expansion on child penalties for mothers.

For causal identification, it is crucial that this regional variation is exogenous to other determinants of maternal labor supply. Several government reports document a persistent gap between the demand for and the availability of early childcare (BMFSFJ, 2016, 2020), suggesting that observed enrollment patterns were primarily driven by supply constraints rather than demand factors. Wrohlich (2011) simulates potential childcare demand using survey data and show that demand exceeded supply in every year prior to 2013. Likewise, Hubert et al. (2014) report that demand for public childcare for children under age three consistently surpassed available slots, with a remaining gap of 12.4 percentage points even in 2013.

Furthermore, these supply factors were exogenous to mothers' labor market outcomes or childcare demand in several respects (Müller and Wrohlich, 2020). A number of institutional and administrative factors contributed to this exogenous variation in supply. First, shortages of suitable construction sites and qualified staff created persistent bottlenecks in meeting demand for early childcare in a timely manner (BMFSFJ, 2011, 2012, 2013).[16] Municipalities and local childcare

[16]Germany's shortage of early childhood educators amounted to about 45,000 workers. This gap is particularly severe considering that only around 20,000 individuals complete training each year, while approximately 15,000 workers

providers often struggled to develop adequate proposals for funding because of these supply factors (Stürmer-Heiber, 2022). This suggests that the expansion of early childcare did not necessarily track local demand. In other words, the counties that expanded childcare provision earlier were not simply those with the greatest or most urgent need.

Second, the approval process for federal funds generated idiosyncratic shocks to childcare expansion. Although expansion targets were defined at the federal level, funding was administered by state-level youth welfare offices. Counties and municipalities had to apply for these funds based on projected childcare demand (Hüsken, 2011; Bauernschuster et al., 2016). Once applications were submitted, counties entered a lengthy and unpredictable approval process over which they had no control (Stoy, 2015; Felfe and Lalive, 2018). As a result, the timing of approvals varied across counties in a way that produced idiosyncratic shocks to expansion. Moreover, when applications for childcare funding exceeded available budgets, recipients in the current and subsequent years were determined through lotteries or waiting lists (Felfe and Lalive, 2018). All these features generated supply-driven variation that was plausibly exogenous to the factors shaping mothers' labor supply decisions.

However, despite the significant role of supply shocks in creating heterogeneity in the timing of childcare expansion across counties, demand-side factors may still have contributed to regional variation, potentially introducing endogeneity. To address this concern, we employ a matched event-study design, focusing on cases where a county experiences a sudden, sizable increase in early childcare to better isolate exogenous timing in the expansion. We further construct a control group through matching to ensure that treated and control counties exhibit similar pre-trends in factors potentially related to childcare demand.

4.2 Event Study Analysis with Matching

In our event study analysis, we focus on episodes where a county experiences a large and sudden increase in early childcare in a single year, following Kleven et al. (2024). This approach allows us to isolate expansions that are more likely driven by exogenous supply shocks rather than by demand-side factors. We define our event as a case where the early childcare coverage rate of a county increases by more than 5 percentage points in a single year, which corresponds to the top 5 percent of changes in the early childcare coverage rate at the county-year level between 2007 and 2014.[17] Among the 324 counties in West Germany, 109 counties experienced the expansion event at some point between 2007 and 2014, and these counties are considered our treated group.[18] We

exit the profession annually, leaving only a small net inflow (Felfe and Lalive, 2018).

[17] Among 2268 (324 counties $\times$ 7 periods) county-year-level changes, 132 cases exceed the threshold of a 5-percentage-points increase (see Appendix Figure A.2). In Section 6.2, we conduct sensitivity analysis using different thresholds and find consistent results.

[18] Appendix Figure A.3 shows how many events occurred in each year during the reform period.

regard the year when the event occurred as the expansion year of each treated county.[19]

To further ensure that the childcare expansion was not driven by demand shocks, we select control counties comparable to treated counties through propensity score matching based on covariates that are potentially related to childcare demand.[20] For each treated county, we identify a control county, drawn from counties that never experienced the childcare expansion event, with the closest propensity score.[21] The propensity scores are estimated using a logit model that predicts treatment with matching variables, including the early childcare coverage rate, fertility rate, population share of women aged between 20 and 40, female employment rate, kindergarten enrollment rate, and population size in the year prior to the expansion,[22] all within the same urban/rural status. This approach ensures that the treated and control counties are comparable in terms of the matching variables prior to the childcare expansion event.

In our setup, we construct a stacked data set using only control counties that have never been treated as in Cengiz et al. (2019) and Arnold et al. (2024). Since we use only never-treated counties as our control group for each event, our matched difference-in-differences design avoids the underidentification problems or negative weight issues that arise in difference-in-differences designs with staggered timing raised by recent studies regarding two-way fixed effects (e.g. Borusyak et al. 2024; Goodman-Bacon 2021).

Figure 3 shows that the two groups of counties followed nearly identical trends in early childcare coverage prior to the expansion event, but only the treated group experienced a sharp increase in the expansion year. The early childcare coverage rate increased by approximately 5.5 and 1.5 percentage points in the treated and control counties, respectively, during the year of the event. Figure 4 presents the trends of the matching variables that could be correlated with early childcare expansion. Panels (a)-(d) demonstrate that the pre-trends of the fertility rate, population share of young women, female employment rate and kindergarten coverage rate are highly similar between treated and control counties, suggesting that the sudden increase in early childcare in treated counties is unlikely to be driven by demand shocks. Although they are not used as matching variables, we also confirm that the pre-trends of other variables, including GDP per capita and the employment share of construction sector, are similar between the control and treated counties, as shown in Figure A.4. Using these variables as matching variables does not change our main results.[23]

[19] Out of the 109 treated counties, 98 counties experienced the childcare expansion event only once during the reform period. For counties that experienced the event twice or more during the reform period, we consider the year of the first event as their expansion year.

[20] This matching approach closely resembles the method used in several recent studies employing a dynamic difference-in-differences design (e.g. Goldschmidt and Schmieder 2017; Arnold et al. 2024; Schmieder et al. 2023).

[21] 80 counties were chosen as control counties with replacement in the matching process.

[22] That is, the year $t-1$ in control counties corresponds to the same calendar year $t-1$ in their matched treated counties, where t denotes the year of expansion.

[23] See Section 6.2 and Table A.2 for more details.

Table 1 compares county-level characteristics and the individual characteristics of mothers between the treated and control counties. Panel A shows that the treated and control counties had similar characteristics, not only for the matching variables but also for other variables, including population density and the unemployment rate (one year prior to the childcare expansion event). Moreover, Panel B indicates that mothers who had their first child one year before the expansion event exhibit similarities in terms of age, education level, and pre-birth earnings.

To examine the effect of a sudden increase in early childcare on mothers' child penalties, we conduct an event study analysis using matched treated and control counties. The specific regression model used is as follows:

$$Y_{ist}^{e} = \sum_{k=-7(\neq -4)}^{4} \beta_k^E \cdot \mathbf{I}[t = t^* + k] * Expansion_{is} + \sum_{k=-7}^{4} \beta_k \cdot \mathbf{I}[t = t^* + k] + Age_i + Year_s + County_c + \nu_{ist} \tag{1}$$

where Y_{ist}^{e} is the outcome observed in calendar year s, e years after the childbirth of individual i who gave birth in year $t = s - e$. t^* denotes the year of childcare expansion event, and $Expansion_{is}$ is a binary indicator equal to one if individual i lived and gave birth in a treated county in the year of expansion t^*.[24] The indicator $I[t = t^* + k]$ equals one if the mother gave birth k years relative to the expansion year. We control for the mother's age, year, and county fixed effects.

The coefficient β_k^E should capture the effects of early childcare expansion on mothers' labor market outcomes over time relative to the expansion event. This specification allows us to estimate the impact on outcomes in each year $e = 0, 1, \ldots, 6$ after childbirth or average outcomes over relevant childcare eligibility periods for mothers who gave birth in year t, which corresponds to k years relative to the expansion year (t^*). We adopt this specification because it yields straightforward estimates corresponding to our descriptive results and is well-suited for analyzing long-run effects.[25]

[24] Although parents are not legally required to take a spot in their county of residence, cross-county attendance is rare because funding and priority rules are tied to residence. Under §24 SGB VIII and the corresponding state implementation laws, the local youth welfare authority (Jugendamt) of the child's residence municipality is responsible for providing the entitlement to a childcare place. Therefore, mothers residing in counties where the expansion occurred are considered treated.

[25] In a similar event study setting, Kleven et al. (2024) estimate the effects of public childcare expansion in Austria using a stacked estimation model. They combine the difference-in-differences model in equation (1) for different years (e) relative to childbirth into a single stacked specification. However, we use equation (1) as our main specification because it provides more intuitive and flexible estimates. Instead of using two event times (one relative to childbirth and the other relative to childcare expansion) interacted with each other, we only consider the childcare expansion as the event for simplicity and conduct the event study analysis for each year e relative to childbirth. Moreover, using equation (1), we can utilize the averaged outcomes during childcare eligibility period as our main outcomes. We present the results obtained using Kleven et al. (2024)'s stacked estimation model in panels (b), (d), and (f) of Appendix Figures A.5-A.7 and confirm that the results are qualitatively similar to the findings obtained using our specified model.

Since mothers who gave birth three years before the expansion event could send their kids under three to childcare when the expansion occurred, while those who gave birth four years before the expansion could not, we use $k = -4$ as the reference year and omit the event time dummy for this year. Mothers who gave birth four or more years before the expansion thus serve as a reference group that was not exposed to the policy. Standard errors are clustered at the county level.

When we define child penalties relative to the predicted outcome absent a child, it is similar to normalizing the child penalties relative to mothers' pre-birth outcomes, assuming that the average earnings path in the absence of childbirth would be smooth (Kleven et al., 2019b). To enhance interpretability and comparability, we normalize the event-study coefficients by the mean outcome one year before childbirth for mothers giving birth in each respective year. These normalized coefficients are plotted in the event study graphs.

5 Results

5.1 Trend of Child Penalties

Before investigating the effects of early childcare expansion, we first analyze how child penalties developed over time in West Germany around the reform period. Specifically, we estimate the impact of having a first child on mothers as described in Appendix B,[26] employing an event study methodology proposed by Kleven et al. (2019b).[27] Since the German administrative data are spell data, we estimate the child penalty for annual earnings and days worked by aggregating monthly information to the yearly level, while estimating the child penalty for other outcomes on a monthly basis.[28] This approach allows us to show how child penalties developed over time with greater precision and detail.

To examine the trend in child penalties, we estimate them separately for mothers who had their first child in the periods 2003-2006, 2007-2010, and 2011-2014. Figure 5 suggests that child penalties for mothers in West Germany decreased significantly over time for annual earnings, employment, the number of days worked, daily wages, full-time, and non-mini job employment. To control for birth cohort effects, we also estimate child penalties at the yearly level by interacting

[26] While some studies define the child penalty as the effect of having children on mothers compared to fathers, we consider the loss in outcomes relative to their pre-birth status as the child penalty for mothers given that the effect of having children on fathers is negligible in Germany (Kleven et al., 2019a). This approach aligns with Kleven et al. (2024) and Rabaté and Rellstab (2022), who focus on the mother-specific child penalty when examining the effects of public childcare expansion on child penalties in Austria and Netherlands, respectively.

[27] To ensure valid comparisons across mothers with different ages at first birth, we follow a more conservative estimation approach suggested by Melentyeva and Riedel (2023). Specifically, we restrict the comparison groups to a maximum age difference of four years and apply a stacked estimation design. The estimates are comparable to those from the standard approach (See Appendix Figure A.9).

[28] The penalty in earnings is based on labor market earnings, not including transfers.

birth cohort dummies with event time dummies, as in Kleven et al. (2019b). Figure A.8 displays the average child penalty over three years following childbirth by birth cohort, illustrating a decreasing trend. Panel (a) of Figure A.8 indicates that mothers' earnings penalties decreased by roughly 8 percentage points (from -0.75 to -0.67) during our sample period (2007-2014). This implies that mothers who had their first child in 2014 earned 8 percentage points more of their pre-birth earnings during the first three years following childbirth compared with those who gave birth in 2007. Given Germany's early childcare expansion during this period, this trend raises the question of whether and how this expansion contributed to the decline in mothers' child penalties.

5.2 Effects of Early Childcare Expansion on Child Penalties

5.2.1 Main Results

In our main analysis, we examine the effects of early childcare expansion on mothers' child penalties, specifically during the period when children are under age three and thus eligible for childcare. We begin by presenting descriptive evidence to provide an intuitive illustration of the impact. To do so, we estimate child penalties for mothers who gave birth in each year around the childcare expansion, separately for treated and control counties, as we did in Figure 5 for each group. We then plot the average child penalties for the three years following childbirth for each group, as shown in the left panels of Figure 6.[29]

Panels (a), (c), and (e) suggest that the trends of child penalties of mothers who gave birth 4-7 years before the childcare expansion event were parallel between the treated and control counties for annual earnings, days worked, and daily wages, respectively. However, the child penalties begin to diverge among the groups of mothers who gave birth from three years before the childcare expansion event. This finding suggests that mothers whose first child was eligible for early childcare at the time of the expansion experienced a more substantial reduction in their child penalties.[30] Over time, this divergence in child penalties becomes even more pronounced for all three outcomes.

In Panels (b), (d), and (f) of Figure 6, we present the results when using $\overline{Y}_{it}$ as our outcome variable in equation (1), where $\overline{Y}_{it}$ represents the average outcome over the three years following the childbirth of an individual i who gave birth in year t.[31] Since we do not observe the exact year in which mothers enroll their children in childcare, we use the average outcome over the three years

[29] Child penalties are calculated as the average over three years following childbirth, computed as $(\frac{1}{3}\sum_{e=0}^{2} P_e)$, where P_e is defined as in equation (4).

[30] Depending on the month of childbirth, some children born three years before the childcare expansion year might not be eligible for childcare. This could explain why the divergence in child penalties becomes more apparent for mothers who gave birth two years before the expansion, while the divergence begins to appear for those who gave birth three years before the expansion.

[31] In this specification, we control for age and calendar year at childbirth to account for age and year effects.

following childbirth as our primary outcome to capture effects during the period when children are eligible for childcare. For detailed year-by-year results spanning from 0 to 2 years after childbirth, see panels (a), (c), and (e) of Appendix Figures A.5–A.7.

The three right-hand panels of Figure 6 display normalized coefficients that estimate the impact of childcare expansion on mothers' average child penalties over the three years following childbirth. These coefficients represent difference-in-differences estimates that compare treated and control counties over time, corresponding to the descriptive trends in the left-hand panels. Panel (b) shows that the expansion begins to have a positive effect on mothers' earnings, apparently for those whose first child was under age three at the time of the expansion. Panels (b), (d), and (f) suggest a further decline in child penalties for post-expansion births in treated counties for all three outcome variables.

To better interpret the magnitude of these effects, we estimate pooled effects for three time periods using a modified version of equation (1) as follows:

$$\overline{Y}_{it} = \sum_{k=2}^{3} \beta_k^E \cdot \mathbf{I}[t - t^* \in g_k] * Expansion_{is} + \sum_{k=2}^{3} \beta_k \cdot \mathbf{I}[t - t^* \in g_k] + Age_i + Year_t + County_c + \epsilon_{it} \tag{2}$$

Rather than using individual dummies for each year relative to the childcare expansion, we include indicator variables for three time periods: a reference period (4-7 years before the expansion, $g_1 = \{-7, -6, -5, -4\}$), a right-before-expansion period (0-3 years before the expansion, $g_2 = \{-3, -2, -1, 0\}$), and after-expansion period (1-4 years after the expansion, $g_3 = \{1, 2, 3, 4\}$). We omit the reference period as the baseline. Then, the coefficient of the right-before-expansion dummy (β_2^E) should capture the effect of childcare expansion on mothers who had a 0-2-year-old child when the expansion occurred.[32] In short, it reflects the impact on mothers who gave birth right before the expansion and therefore benefited directly from a sudden large childcare expansion.

Table 2 presents the results for our main outcome variables. The estimates for β_2^E represent the effects of early childcare expansion on mothers' labor market outcomes (relative to their pre-birth outcomes) for those who gave birth 0-3 years before the childcare expansion, meaning that their child was 0-2 years old when the expansion occurred. The estimates for β_3^E represent the effects on mothers who had their first child after the expansion occurred.

Consistent with the event study results in Figure 6, all coefficients are positive and statistically significant. The effects are greater for mothers who gave birth after the expansion, reflecting the

[32] Depending on the months of birth and childcare expansion, some kids born three years prior to the expansion or in the year of the expansion could be eligible for a childcare slot. This is why the time frame of 0-3 years before the expansion is considered as the "right-before-expansion" period, rather than 0-2 years or 1-3 years prior to the expansion.

growing divergence in child penalties between treated and control counties over time. However, these post-expansion estimates may be overestimated, as they could capture selection effects from birth timing or migration responses, or reflect continued divergence in childcare coverage after the expansion event. Therefore, we consider the effects for mothers who gave birth 0–3 years before the expansion as conservative estimates.

We interpret the magnitude of the coefficients to indicate the effect of a 4-percentage-point increase in the early childcare coverage rate because the rate increased by approximately 5.5 and 1.5 percentage points in the treated and control counties, respectively, during the year of the event. These findings imply that a 20-percentage-point increase in the childcare coverage rate during the reform period would reduce the earnings penalty by 4.5 percentage points. Given that the earnings penalty decreased by 8 percentage points during the reform period, early childcare expansion accounts for approximately 56% of this reduction.

In Section 6.2, we conduct various sensitivity analyses of our event study analysis results. We define the childcare expansion event using different thresholds, construct control counties using different matching variables and methods, and choose control counties among a more restricted pool. All these analyses confirm the robustness of our results.

5.2.2 Channels

In this section, we explore the channels through which the expansion of early childcare affects mothers' child penalties in earnings and daily wages. The observed increase in earnings can be attributed to multiple factors, including an increase in labor supply, influenced by both the total number of days worked and working hours, as well as an overall increase in wage levels. Similarly, the increase in daily wages can result from extended working hours or an increase in hourly wage rates. To understand the working hours channel, we investigate the impact on days spent in full-time and non-mini job employment. Additionally, we explore the effects on firm and occupational wage levels to examine the wage level channel.

To investigate the working hours channel, we rely on information about part-time[33] and marginal employment (mini-job)[34] since the administrative data lack specific information on working hours. By utilizing these variables, we can calculate the number of days spent in full-time and non-mini-job employment per year. Columns (1) and (2) of Panel B in Table 2 demonstrate that early

[33]There was a significant change in the part-time variable *teilzeit* in the IEB in 2011 as employers have been required to report their workers' part-time status more precisely since 2011. We used the imputation method proposed by Fitzenberger and Seidlitz (2020) to ensure consistency of the part-time variable over time.

[34]Mini-jobs (marginal employment) in Germany are jobs defined by a maximum income of currently 538 euros, which makes 8 hours per week the usual working hours for minimum wage (Federal Employment Agency, 2024) and exempted from social security contributions and income tax. A large share of them are held by women who have children (Drechsel-Grau et al., 2022).

childcare expansion increases mothers' days in full-time and non-mini job employment, indicating an increase in overall working hours among employed mothers. This suggests that the availability of early childcare increased mothers' labor supply not only at the extensive margin but also at the intensive margin.

To examine whether childcare expansion affects mothers' sorting into higher-paying firms and occupations, we utilize the wage levels of the firms and occupations where mothers are employed each year. Specifically, we measure firm and occupational wages using the mean wage of full-time regular workers at each firm and at the 3-digit occupational level, respectively. Columns (3) and (4) of Panel B indicate that early childcare expansion positively affects both firm- and occupation-level wages. Given that many mothers shift to lower-paying jobs or part-time work after childbirth (Kleven et al., 2019a), these findings suggest that access to early childcare helps mothers remain in higher-paying firms or occupations and reduces downward mobility following childbirth.

5.2.3 Persistence

Early childcare can positively impact mothers' labor market outcomes when their children are young and eligible for childcare by enabling them to return to work earlier. However, as shown by Kleven et al. (2019b), child penalties persist for many years after childbirth in most countries. Therefore, investigating whether early childcare can lead to lasting improvements in mothers' labor market outcomes is crucial. To examine this possibility, we conduct a persistence analysis using mothers' outcomes in each year after childbirth (Y^e_{ist}) in equation (2) and present estimates (β^E_2) for mothers who had a child under three when the childcare expansion occurred.[35]

Figure 7 shows the effects of early childcare expansion over the seven years following childbirth. Panel (a) shows persistent positive effects on both earnings and daily wages. The impact on earnings is strongest during the years of early childcare usage, and the effects on both outcomes remain positive in the following years.

In contrast, Panel (b) shows that the effects on employment is concentrated in the years when children are eligible for early childcare. One possible explanation is that most mothers return to work when their child turns three, as kindergarten coverage for children aged 3–6 is nearly universal (above 90%) in West Germany. Therefore, the marginal effect of early childcare on employment may be limited to the pre-kindergarten years. Another possible explanation is that greater access to early childcare may encourage higher fertility, leading to additional employment interruptions. Although we cannot directly test this possibility due to limitations in identifying higher-order births in the data, Bauernschuster et al. (2016) find that early childcare expansion increased fertility rates in West Germany, particularly at the intensive margin. If the expansion

[35] We consider the event time up to 6 because the IEB data set covers the period until 2021, allowing us to estimate the child penalty up to 7 years following childbirth for mothers who had their first child in 2014.

indeed led to more second or higher-order births, our medium-run estimates likely represent a lower bound of the effects, as additional childbirths would temporarily reduce maternal employment.[36]

Panel (c) shows persistent effects on full-time and non-mini job employment. Such sustained attachment to positions with longer working hours likely contributes to the long-run improvements in earnings and daily wages. These findings indicate that the early childcare expansion has a stronger long-term impact on labor supply at the intensive margin than at the extensive margin.

Panel (d) further shows that the positive effects on firm- and occupation-level wages persist beyond the childcare eligibility period, although their magnitude and significance diminish from five years after birth.[37] The initial sorting into higher-paying firms and occupations around the eligibility period could be important for understanding long-run effects on earnings. Mothers who remain in higher-paying positions shortly after childbirth can continue to benefit through higher earnings trajectories due to accumulated experience and tenure in those positions. Early access to childcare can help mothers sustain long-run earnings gains by enabling them to stay in higher-paying firms and occupations after childbirth.

6 Robustness and Sensitivity Analysis

6.1 Robustness of the Main Results

6.1.1 Addressing Selection Problems in the Sample of Mothers

Depending on how women make decisions regarding childbirth in response to early childcare expansion, there could be some selection issues in the sample of mothers. First, regarding the choice of where to have and raise their children, selective migration may influence the composition of mothers in each group of counties. For instance, mothers who are more attached to the labor market might be inclined to move to counties with more public childcare available around the time of childbirth. While this migration decision could be considered part of the effects of childcare expansion, it could bias our estimates upward, potentially reflecting changes in the characteristics of the population of mothers. To address this concern, we restrict our sample to mothers who remained in the same county from one year before to three years after childbirth.[38] Column (3) of Table A.3 shows that the results barely change.

[36] Although we cannot precisely distinguish between mothers with one child and those with multiple children using our data set, estimates based on our sample, which includes both groups, likely provides a lower bound of the medium-and long-run effects. This is because mothers who have a second child experience an additional employment interruption, which contributes to larger observed child penalties.

[37] This pattern may partly reflect lower variation in firm- and occupation-level wages. Moreover, as more mothers re-enter the labor market once their children reach kindergarten age, convergence may occur between treated and control mothers in terms of the median wage of firms and occupations.

[38] About 15% of the observations are dropped after this restriction.

Second, any selective timing of childbirth or changes in fertility could also affect the characteristics of mothers in the sample. If mothers who are more attached to the labor market adjust their timing of birth in anticipation of the childcare expansion, it may alter the composition of the sample of mothers around the expansion timing, potentially biasing our estimates. Moreover, if mothers who are more attached to the labor market and who would not have had a child without the childcare expansion now decide to give birth in anticipation of higher childcare availability, it could change the population characteristics of mothers.

We address these concerns by controlling for the fertility rate and mothers' characteristics in several ways. In our event study analysis, we use the fertility rate as a matching variable, ensuring a similar pre-trend in fertility rates between treated and control counties before the expansion event, as shown in Figure 4. Furthermore, we show that mothers in treated and control counties were very similar in terms of age, education, and pre-birth earnings in Table 1.

Third, we ensure that our effects are not driven by an increase in mothers working in childcare centers. Column (10) of Appendix Table A.3 shows the effects when mothers who ever worked in a childcare center are excluded from our sample, indicating that the effects are not driven by this group. We do not expect mothers to sort into childcare jobs after birth because of strict occupational licensing requirements in Germany. Additionally, childcare workers only account for a small proportion in our sample (4% ever worked in a childcare center).

6.1.2 Controlling for Regional Economic Shocks

One of the important factors contributing to the regional variation in early childcare expansion is the shortage of suitable construction sites or qualified staff, which are considered supply shocks. However, what if these supply shocks are correlated with regional economic changes over time? For example, if economic booms in certain regions lead to increased construction activity and motivate more mothers to return to work, this could bias our estimates.

To address this concern, in our event study analysis, we examine the trends in GDP per capita, the log wage level, and the employment share of the construction sector between treated and control counties. We demonstrate similar pre-trends for these variables before the childcare expansion event, as illustrated in Figure A.4. Furthermore, Column (4) of Table A.3 shows consistent results when we control for GDP per capita, the unemployment rate, and the employment share by sector in our main event-study analysis.

6.1.3 Addressing Potential Confounding Effects of Parental Leave

In our main analysis, we focus on mothers who gave birth from 2007 onward, ensuring that all are subject to the same parental leave regime. While this approach allows us to compare mothers under

a consistent family-policy environment, there remains the possibility that heterogeneous responses to the 2007 parental leave reform could confound the effects of childcare expansion if they are systematically correlated with childcare expansion paths. We address this concern in several ways.

First, we control for the county-year share of highly educated women who are considered to be the most affected by the reform (Raute, 2019; Welteke and Wrohlich, 2019) in our main event study specification. As shown in column (5) of Table A.3, our estimates remain consistent, and panel (d) of Figure A.4 confirms parallel pre-trends in this variable between treated and control counties. Second, we control for fathers' parental leave take-up rates in our event-study analysis, as cross-county differences in paternal leave could affect mothers' child penalties. Column (6) of Table A.3 shows that the coefficients remain similar, and Panel (e) of Figure A.4 indicates that this variable also exhibits parallel pre-trends between treatment and control counties. Finally, we examine the effects of the early childcare expansion on mothers who gave birth before January 2007, who were exposed to the expansion but not to the new parental leave scheme. To obtain an effect for this group, we only focus on expansion events in 2007 and assess the effects for mothers giving birth before the expansion. Column (7) of Table A.3 shows that these mothers experienced even slightly larger effects. Although this estimate needs to be interpreted carefully, as it reflects earlier expansion effects and thus mixes pre–parental leave and early-expansion influences, it is broadly in line with the literature.[39] As shown in the simulations by Jakobsen et al. (2022), abolishing leave policies would increase maternal labor supply, but at the cost of lower fertility.[40] In general, parental leave policies give an incentive to extend employment interruptions, while securing positions for mothers (Olivetti and Petrongolo, 2017; Rossin-Slater, 2017). Therefore, in the absence of parental leave, childcare may produce even larger effects on maternal labor supply. Taken together, this evidence suggests that early childcare expansion also produces positive effects in the absence of the parental leave reform.

6.2 Sensitivity Analysis of the Event Study Analysis Results

6.2.1 Defining the Childcare Expansion Event Using Different Thresholds

In our event study analysis, we define the episode in which a county experiences an increase in the early childcare coverage rate exceeding 5 percentage points within a single year as the expansion event. That is, we use a threshold of 5 percentage points to define the childcare expansion event. To test the robustness of our results with respect to the chosen threshold, we also consider alternative cut-offs and obtain similar findings, as reported in Table A.1.

[39] Bergemann and Riphahn (2023) shows that the overall effects of this 2007 parental leave reform on maternal employment are small.

[40] The authors simulate the effects for the absence of maternity leave in Denmark.

6.2.2 Constructing Control Counties Using Different Matching Variables and Methods

To construct a comparable control group, we choose counties matched in terms of variables relevant to the demand side of early childcare expansion, including the fertility rate, the population share of young women, the female employment rate, the kindergarten enrollment rate, population size, and urban status. To assess the sensitivity of our results to the choice of matching variables, we explore whether different combinations of these variables produce any changes in our results. In Table A.2, we show that excluding or adding more variables for matching does not substantially alter our primary results.

Instead of using propensity score matching, we also choose control counties using a multivariate-distance (Mahalanobis distance) matching method and obtain similar result, as suggested in column (8) of Table A.3.

6.2.3 Choosing Control Counties among a More Restricted Pool

When constructing the control group of counties, we choose counties among *all* counties that never experienced the expansion event. This approach implies that we might include counties with an increase in the early childcare coverage rate just below the 5-percentage-point threshold. In such instances, our estimates could underestimate the impact of early childcare expansion because control counties may also have experienced a considerable increase in childcare (e.g., a 4.9-percentage-points increase). To address this issue, we choose control counties exclusively from those that never encountered an increase in the early childcare coverage rate exceeding 2 percentage points. We obtain similar results as suggested in in column (2) of Table A.3. However, this approach comes at the cost of reduced overall matching quality and diminished comparability between treated and control counties.

7 In Which Contexts Is Early Public Childcare Effective?

Although promoting maternal employment is one of the key objectives of early public childcare provision, the existing literature reports mixed results. Table 4 summarizes these findings by comparing the estimated effects of early childcare on mothers' employment across studies. For example, Goux and Maurin (2010); Attanasio et al. (2022); Kleven et al. (2023), and Rabaté and Rellstab (2022) report no significant effect of early public childcare on maternal labor supply in France, Brazil, Austria, and the Netherlands, respectively.[41] In contrast, this paper finds that a 10-percentage-point increase in early childcare coverage increased maternal employment in West

[41] In a previous study, Bettendorf et al. (2015) analyze the same childcare reform in the Netherlands and similarly report negligible effects on maternal labor supply.

Germany by about 1.5 percentage points, a finding comparable to the effects reported by Müller and Wrohlich (2020). Several other studies also document positive effects in different contexts, including Kunze and Liu (2019) and Andresen and Havnes (2019) for Norway, Baker et al. (2008) for Québec[42], and Nollenberger and Rodríguez-Planas (2015) for Spain. These differences raise important questions: Why are the effects of early childcare more pronounced in some countries, and under which contexts can early childcare be more effective?

In this section, we discuss two factors that may explain the varying effects of early childcare on maternal labor market outcomes. First, we propose that early childcare is likely to be more effective in contexts where mothers face higher child penalties prior to the expansion. Notably, Germany had one of the largest child penalties among the countries analyzed in studies on early childcare effects as shown in column (5) of Table 4. When focusing on West Germany, the estimated child penalty becomes even larger (as shown in Appendix Figure A.10). Comparing the effects between East and West Germany, as well as across counties within West Germany, we show that early childcare tends to have stronger effects where initial child penalties are higher, as there is greater potential for their reduction.

Second, reliance on informal childcare can be a crucial factor shaping the effectiveness of early childcare expansion. In contexts where informal care is widely used, the expansion of formal childcare may substitute rather than supplement existing arrangements, simply shifting families from informal to formal care and thereby limiting its impact on maternal labor supply, a pattern known as the crowding-out effect. In Germany, private childcare is rare and reliance on informal care is relatively low compared to other countries, as shown in column (6) of Table 4, which may account for the strong effects of early public childcare observed in West Germany. Table 4 also indicates that countries with low informal care usage, such as Norway, show strong childcare effects, whereas those with greater reliance on informal care, such as Austria and the Netherlands, exhibit smaller effects. Kleven et al. (2024) also suggest that Austria's limited childcare impact could be attributed to a crowding-out effect. Similarly, Attanasio et al. (2022) find that in Brazil, public childcare primarily increases the labor supply of grandparents and teenagers rather than mothers. To provide evidence of this crowding-out effect, we exploit regional variation in informal childcare within West Germany and examine how reliance on, or changes in, informal childcare relate to the magnitude of the early childcare effects.

Cross-national analyses often face challenges due to substantial differences in economic conditions, childcare policies, and social environments across countries. Moreover, comparing results derived from different data sources and estimation methods is not straightforward. In contrast, analyzing regions within a single country allows us to assess the effects of a uniform policy within

[42] Haeck et al. (2015) also show positive effects of this policy on mothers labor supply, analyzing the effects on children in the ages of one up to five.

a relatively homogeneous institutional setting while still capturing variation across local contexts. By conducting a regional analysis within Germany, we provide evidence on the contextual factors under which childcare expansion tends to generate stronger positive effects on maternal labor supply.

7.1 Initial Level of Child Penalties and Early Childcare Effects

Before the early childcare expansion reform, East and West Germany exhibited distinctly different child penalties. Appendix Figure A.10 shows that about 60% of mothers who had their first child between 2003 and 2006 did not return to work until three years after childbirth in West Germany. In contrast, almost 70% of mothers in East Germany returned to work within two years after childbirth. This disparity provides a unique context to compare the effects of early childcare in two regions with varying initial child penalties within the same country.

To estimate the effects of childcare expansion in East Germany, we use the full variation in childcare coverage rates as a continuous treatment variable, given that exploiting expansion shocks in East Germany is not feasible. As shown in Figure 1, early childcare coverage was already high in East Germany before the reform, and the post-2005 expansion proceeded more gradually based on an existing infrastructure (Gebauer, 2010). We provide the details of this generalized difference-in-differences approach in Appendix C, and also show that the estimates from this continuous-treatment approach are similar to those obtained from our matched event-study design for West Germany. The results in Table 3 indicate that the effects of early childcare on mothers' earnings penalty in East Germany are roughly one-fourth the size of those in West Germany, which suggests that early childcare expansion yields stronger positive impacts where there is greater potential for increases in maternal labor supply.

While the comparison of East and West Germany provides a unique setting to study identical childcare reforms within one country, these two regions exhibit very different gender norms. Therefore, to analyze the relationship between initial child penalties and the effects of early childcare within a more homogeneous context, we examine the effects across counties within West Germany that had similar early childcare coverage rates before the reform, as depicted in Panel (a) of Figure 2.

To account for county-specific characteristics that are potentially correlated with pre-reform child penalties, we extend our main event study analysis by including an interaction term between the post-treatment indicator and the pre-reform child penalty level.[43] Building on equation (2), we

[43] We include a linear interaction term because the initial child penalty level and the effects of early childcare expansion exhibit a roughly linear relationship when we conduct the event-study analysis across county groups classified by their initial child penalty levels. We provide further details on the justification for using a linear interaction in Appendix D.

interact the post-treatment indicator with the earnings penalty in 2002, as shown in equation (9) in Appendix D.[44] We estimate the effect of early childcare on employment since this outcome does not require to normalize coefficients relative to their pre-birth mean for interpretation. Moreover, as employment (or labor force participation) is the standard outcome in the literature, this makes our estimates directly comparable to prior studies.

Figure 8 presents the average marginal effects at different initial child penalty levels, derived from the interaction term coefficient reported in column (2) of Table A.4. The results indicate that counties with higher initial child penalties experienced larger effects of early childcare expansion.[45]

To understand the mechanisms behind this result, we first examine the characteristics of counties with high initial child penalties. Figure 9 shows that counties with higher initial child penalties also had lower early childcare coverage rates prior to the expansion. This could reflect stronger norms against early childcare and maternal employment, or a delayed policy response for these regions. In fact, several studies document significant unmet demand before the reform. BMFSFJ (2012) indicates that demand for early childcare generally exceeded supply even before the expansion began. Similarly, Felfe and Lalive (2018) and Bien et al. (2007) report that in 2005, counties could offer places for only 7% of children aged 0–2, while 36% of parents had sought a place. This suggests that the expansion met substantial latent demand and generated larger effects in areas where maternal labor supply had been particularly constrained by limited formal childcare availability.

Nonetheless, high child penalties could also reflect social norms that discourage mothers' return to work. In that case, the larger effects observed in high-penalty areas suggest that these norms may have evolved during the expansion period. Indeed, Zoch and Schober (2018) report that early childcare expansion caused a shift in norms regarding maternal employment, making them more progressive, particularly in West Germany.[46] Moreover, the behavior of mothers shortly after birth seems to be somewhat malleable. Welteke and Wrohlich (2019) show that mothers adjust their leave-taking behavior strongly to that of their coworkers. Similarly, exploiting moves after reunification, Boelmann et al. (2025) show that West-German mothers adapt their returning behavior to that of their East German coworkers, who typically return to work after 12 months. This relatively rapid adjustment is consistent with research showing that economic decisions, such as the duration

[44] We define the initial child penalty as the average earnings penalty over the first three years after childbirth for mothers whose youngest child was under age three in 2002. We choose 2002 because it allows us to estimate child penalties for the three years following childbirth prior to the start of the childcare reform in 2005. Moreover, as data on childcare coverage rates are available only for 2002 before the reform, this choice allows us to correlate the initial child penalty and initial early childcare levels in the same year. County-level child penalties are calculated following Kleven (2022), as described in equation (6) in Appendix C.

[45] Although the interaction term coefficient is negative, as the child penalty variable itself is negative, we interpret this as indicating that the effect becomes larger when the initial child penalty is higher in magnitude (for example, moving from -0.7 to -0.8).

[46] Ellingsæter et al. (2017) find similar effects of shifting norms through childcare expansion for Norway.

of employment interruptions, are shaped by individuals' desire to conform to perceived peer norms (Benabou and Tirole, 2011; Bursztyn et al., 2020; Bašić and Verrina, 2024). As public childcare expanded, behavioral change among early adopters likely helped shift local expectations, encouraging more mothers, especially those who already preferred an earlier return, to take advantage of the newly available childcare services.

7.2 Informal Childcare and Crowding-Out Effects

To assess the potential crowding-out effects, we analyze how the effects of the early childcare expansion vary with regions' pre-existing reliance on informal childcare. Using the SOEP data, we calculate state-level informal childcare usage rates for two periods (2001–2004 and 2015–2018), representing the years before and after the childcare expansion reform.[47] This measure is calculated as the ratio of households that report using childcare provided by relatives, friends (neighbors), or private babysitters among those with a first child under three. As with the analysis for initial child penalties, we interact the post-treatment indicator with the pre-reform informal childcare rate, as specified in equation (9) in Appendix D.[48]

Panel (a) of Figure 10 plots the average marginal effects implied by the interaction term,[49] showing that early public childcare has smaller impacts where informal care is more prevalent and substitution is likely. Panel (b) of Figure 10 provides direct evidence of crowding-out effects. Here, we interact the post-treatment indicator with the change in informal childcare usage between 2001–2004 and 2015–2018 in equation (9). Panel (b) shows a negative relationship between early-childcare effects and the change in informal childcare, indicating that counties with greater declines in informal care experienced smaller gains from the expansion. Panel (c) further demonstrates that states with greater decreases in informal care also recorded greater increases in early public childcare coverage. Panel (d) shows that overall reliance on informal childcare in Germany has declined since the childcare reform period. Together, these patterns suggest that the crowding-out effect, shifting from informal to public care, plays an important role in shaping the overall impact of childcare provision.

8 Conclusion

In this paper, we examine the effects of expanding public childcare for children under age three on mothers' child penalties. As extensively documented in the literature, employment interruptions

[47] We aggregate on state level because the SOEP provides too few county-level observations for a reliable measure. Four years are pooled for each period to ensure sufficient sample sizes.

[48] We provide further details on the rationale for the linear interaction term in Appendix D.

[49] The corresponding coefficient is reported in Column (3) of Table A.4 in Appendix D.

after birth can lead to long-term divergences in women's careers. The lack of formal childcare options between the end of parental leave and the start of kindergarten for three-year-olds creates an institutional gap. It is therefore crucial to understand how early public childcare provision can support mothers in continuing their careers and, in turn, reduce these penalties.

Our main estimation results suggest that a 20-percentage-point increase in the early childcare coverage rate reduces mothers' earnings penalty by approximately 4.5 percentage points for the three years following birth. This reduction occurs through various channels including more full-time and non-marginal work, higher daily wages, and a sorting into higher-paying firms or occupations. Moreover, the effects on earnings, wages, and working full-time persist even after mothers no longer utilize early childcare.

Given that the literature reports mixed results across countries, a key contribution of our study is that it demonstrates how early childcare provision can substantially boost maternal labor supply in contexts such as West Germany, where child penalties are large and alternative childcare options are limited. Using within-country variation in these two dimensions and treatment effects, we show that regions with higher initial child penalties and less informal childcare experienced a stronger impact of childcare expansion. This highlights the importance of contextual factors when evaluating childcare policies. Because child penalties are widespread and early childcare provision remains limited in many settings, our findings have broad relevance. They provide detailed insights into the conditions under which childcare expansion can be most effective.

As of 2024, there remains a shortage of 306,000 childcare places for children under age three in Germany (Geis-Thöne, 2024). This situation implies that there is still room for further expanding early childcare and reducing the child penalty for mothers. Considering the long-lasting impact of early childcare on mothers' labor market outcomes, the overall effects of early childcare expansion could be more significant than those estimated in our paper if we consider a longer time span in the future. It could affect mothers' career advancement in the long run, including promotion to managerial positions, and have a broader impact on firm culture and overall gender inequality in the labor market, which warrants further study.

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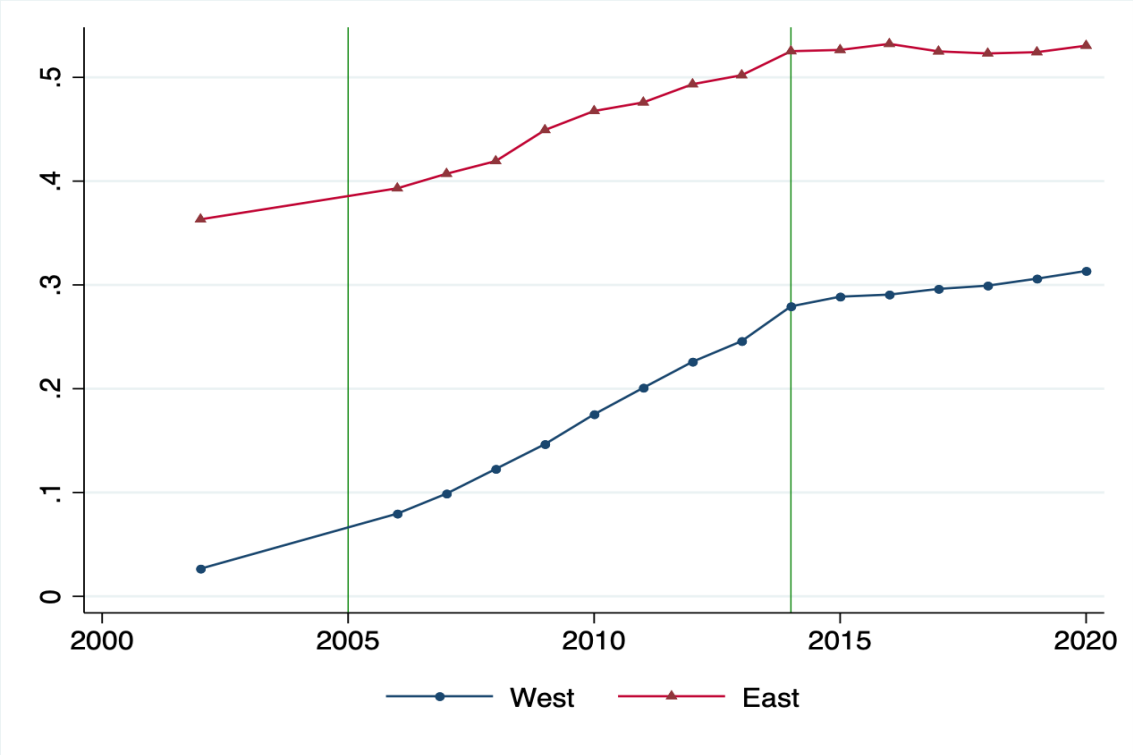
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Figures

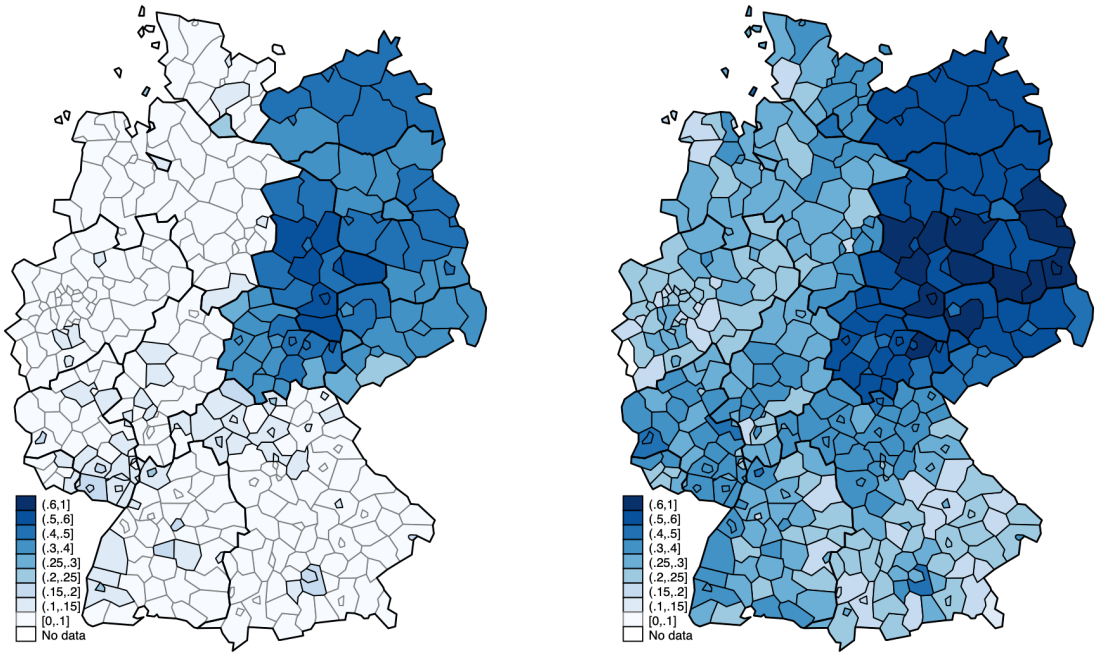
Figure 1: Early Childcare Coverage Rate in Germany



Note. The figure illustrates trends in early childcare coverage rates in East and West Germany. The coverage rate is calculated as the number of children under age three enrolled in public childcare divided by the total number of children under three in each year (Source: Regionalstatistik (2023)). The childcare reform began in 2005 and was officially concluded in 2014.

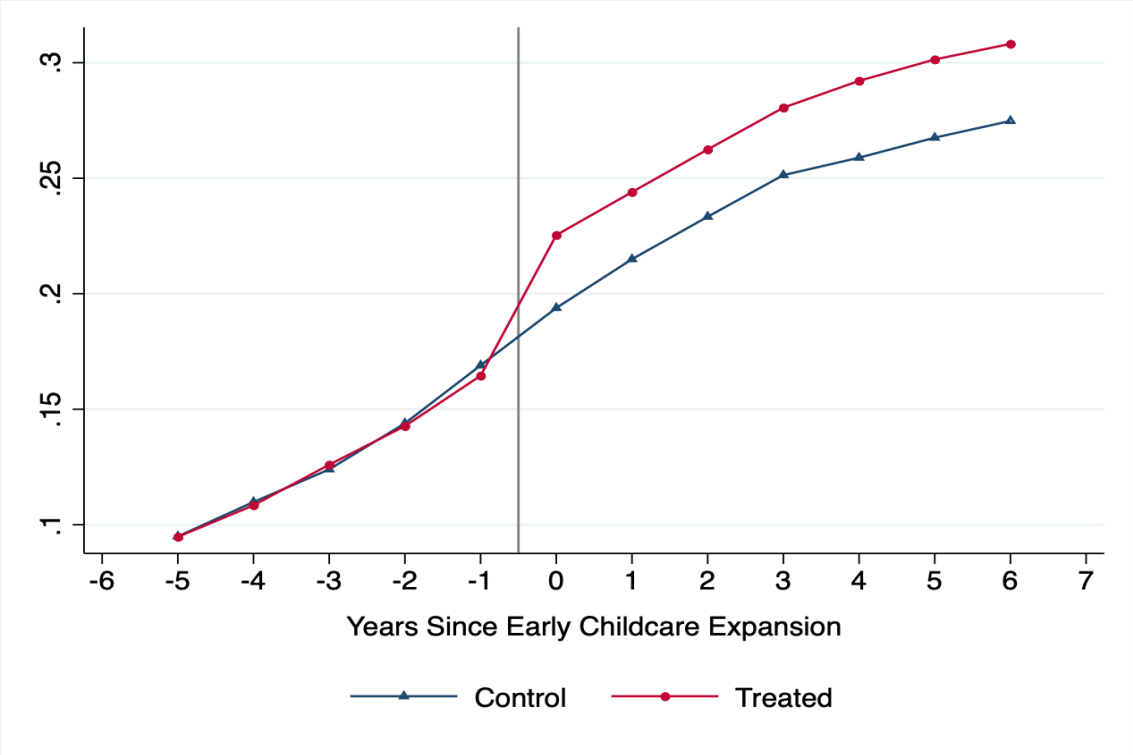
Figure 2: Expansion of Early Public Childcare

(a) 2006 (b) 2014



Note. Panels (a) and (b) illustrate the early childcare coverage rate in each county for the years 2006 and 2014. The coverage rate is calculated as the number of children under three enrolled in public childcare divided by the total number of children under three in each year for each county (Source: Regionalstatistik (2023)).

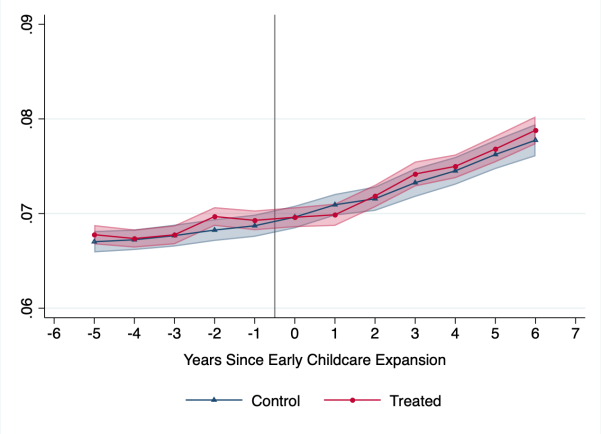
Figure 3: Early Childcare Coverage Rate in Treated vs. Control Counties



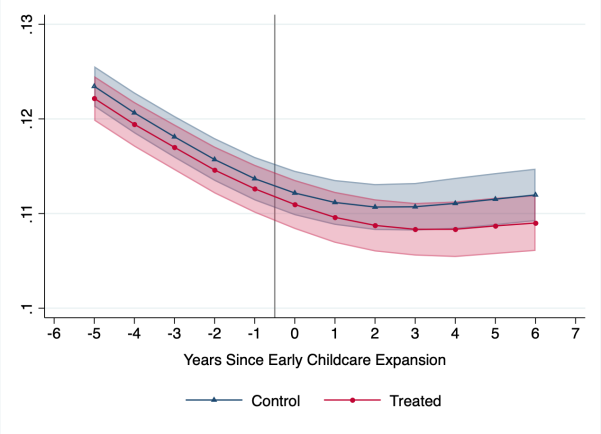
Note. This figure illustrates the changes in early childcare coverage rates in both treated and control counties. In our matched difference-in-differences design, treated counties are those that experienced an increase in the early childcare coverage rate by more than 5 percentage points within a single year, while control counties are selected from those that never experienced such an increase using propensity score matching. The year $t = 0$ on the x-axis represents the year when the childcare expansion took place in treated counties. Control counties are matched based on several variables, including public childcare coverage rates for children under three and between three and six, fertility rate, population share of women aged 20-40, population size, and urban status in the year $t = -1$.

Figure 4: Trend of Variables Used for Matching Around the Childcare Expansion Event

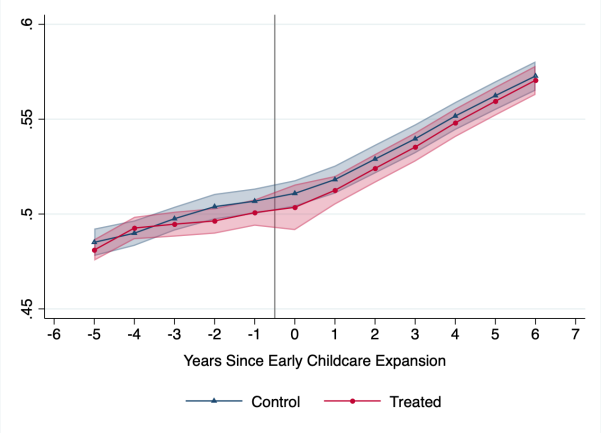
(a) Fertility Rate



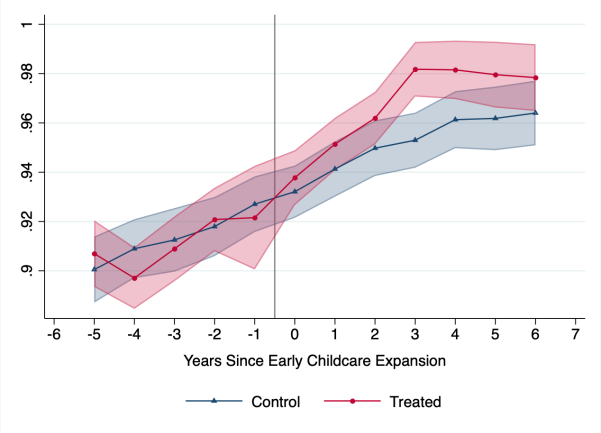
(b) Population Share of Women Aged 20-40



(c) Female Employment Rate



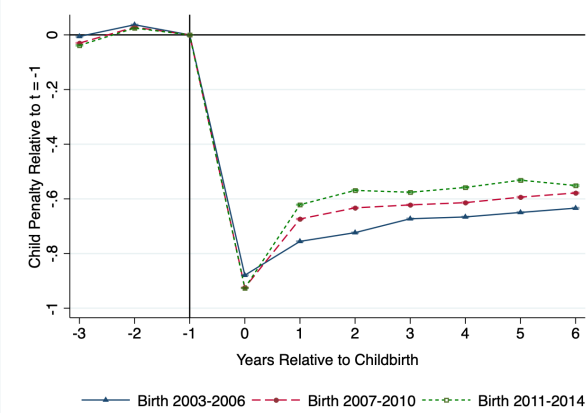
(d) Kindergarten Enrollment Rate



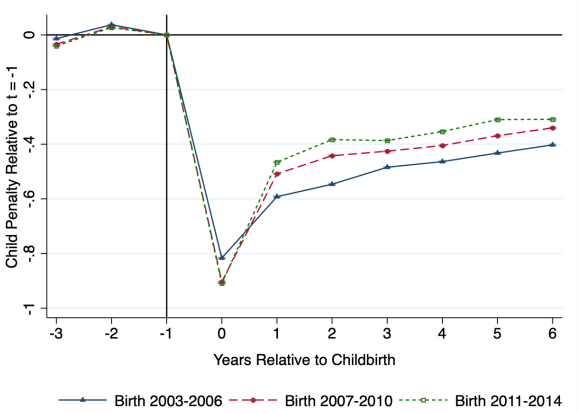
Note. The figure illustrates the trends of variables that could potentially be correlated with the early childcare expansion event and thus used for matching. Panels (a)-(d) compare the trends of the fertility rate, population share of women aged 20-40, female employment rate among women aged 20-65, and the public childcare coverage rate for children aged 3-6 between the treated and control group of counties.

Figure 5: Trend of Child Penalties in West Germany

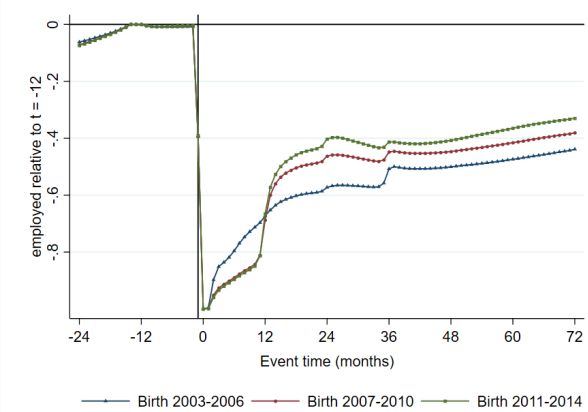
(a) Annual Earnings



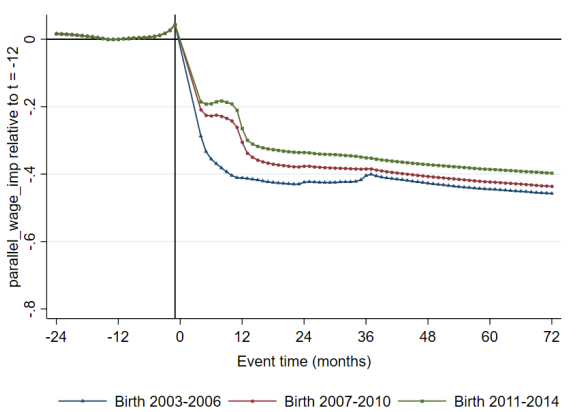
(b) Days Worked



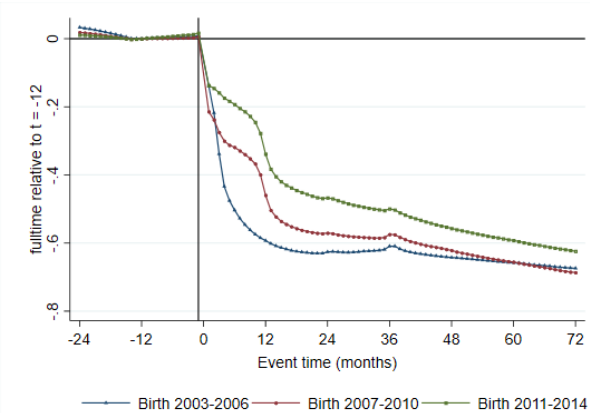
(c) Employment



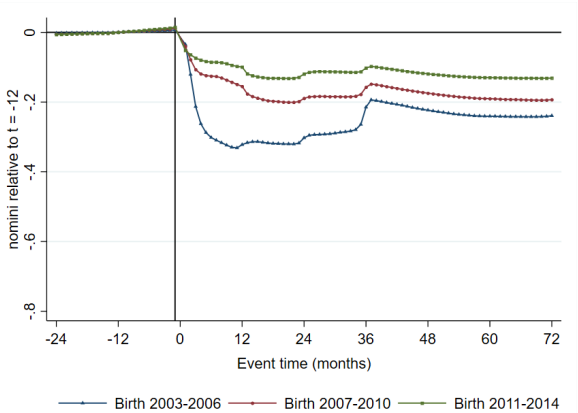
(d) Daily Wages



(e) Working Full-time



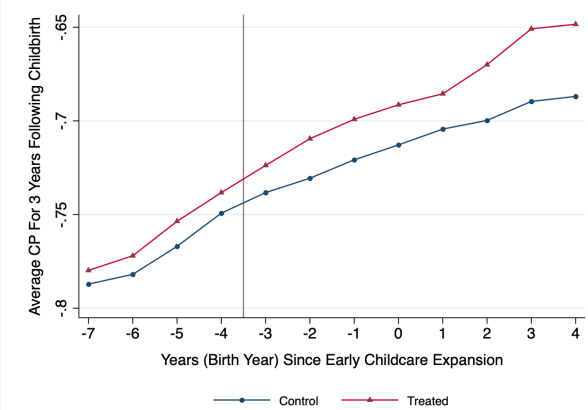
(f) Non-mini Job



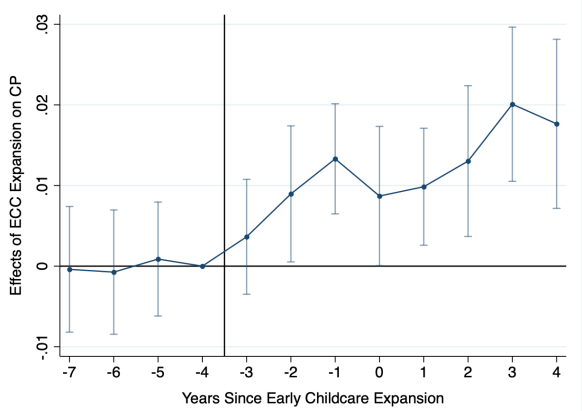
Note. The figure depicts child penalties for mothers in West Germany, who had their first child during 2003-2006, 2007-2010, and 2011-2014. Panels (a) and (b) show yearly child penalties estimated using equation (3) and normalized as in equation (4), while Panels (c)-(f) illustrate monthly child penalties. For annual earnings and days worked, child penalties are estimated unconditional on employment status, assuming zero earnings and days worked for the unemployed. For daily wages, working full-time, and non-mini job employment, child penalties are estimated for those employed. 95% confidence intervals are presented.

Figure 6: Effects of Early Childcare Expansion on Mothers' Child Penalties

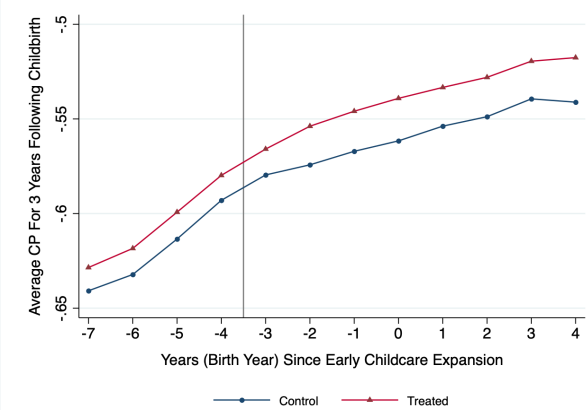
(a) Average Child Penalty in Earnings



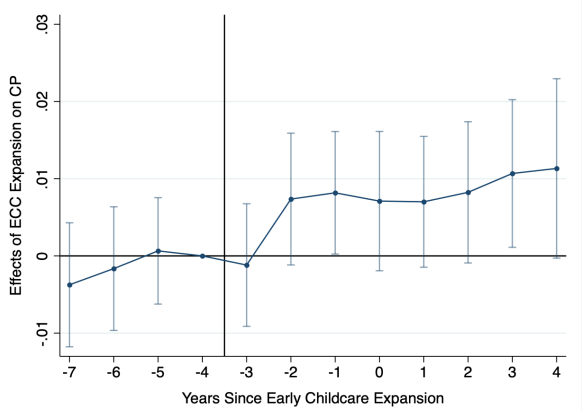
(b) Earnings: event study coefficients



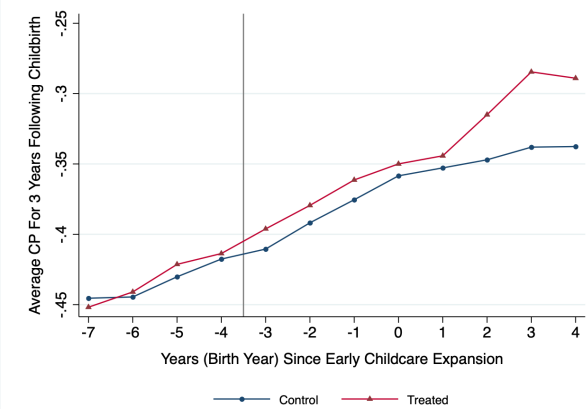
(c) Average Child Penalty in Days Worked



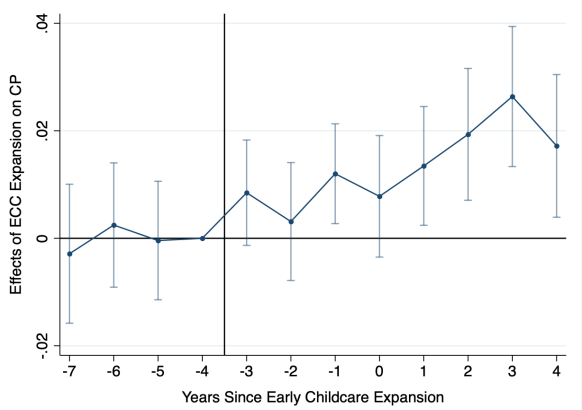
(d) Days Worked: event study coefficients



(e) Average Child Penalty in Daily Wages



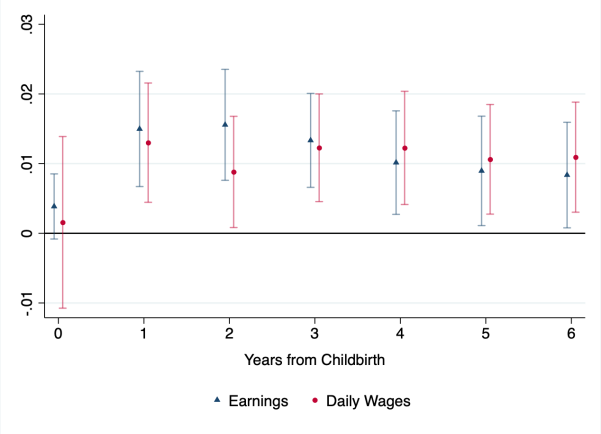
(f) Daily Wages: event study coefficients



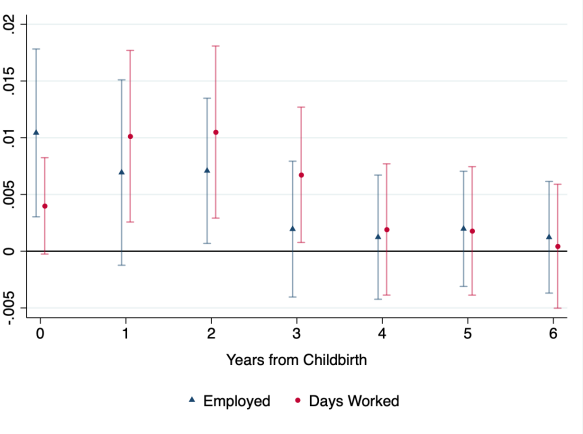
Note. Panels (a), (c), and (e) display the average child penalty over three years following childbirth (i.e. $\frac{1}{3}\sum_{e=0}^{2} P_e$, where P_e is defined as in equation (4)) for mothers who gave birth in each year around the childcare expansion event. In our event study context, while $t = 0$ on the x-axis corresponds to the year of the expansion, $t = -4$ is considered as a baseline year because children of mothers who gave birth in year $t = -4$ were not eligible for the childcare slot when the expansion occurred, while those of mothers who gave birth from year $t = -3$ onwards were eligible. Panels (b), (d), and (f) plot coefficients (with 95% confidence intervals) estimated using $\overline{Y}_{it}$ (the average outcome over three years following childbirth of an individual i who gave birth in year t) as an outcome in equation (1) and normalized with respoect to pre-birth baseline.

Figure 7: Persistence of the Effects

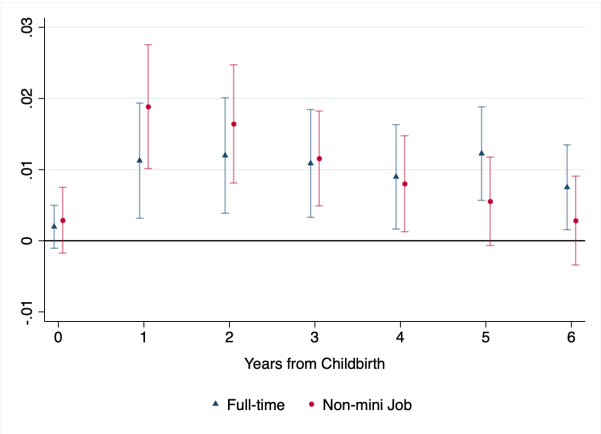
(a) Effects on Earnings and Wages



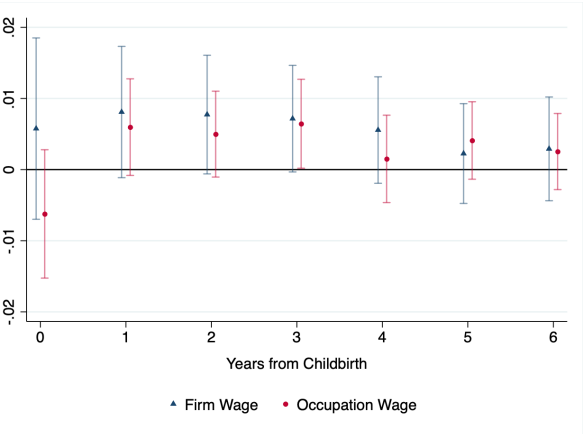
(b) Effects of Labor Supply



(c) Working Hours Channel

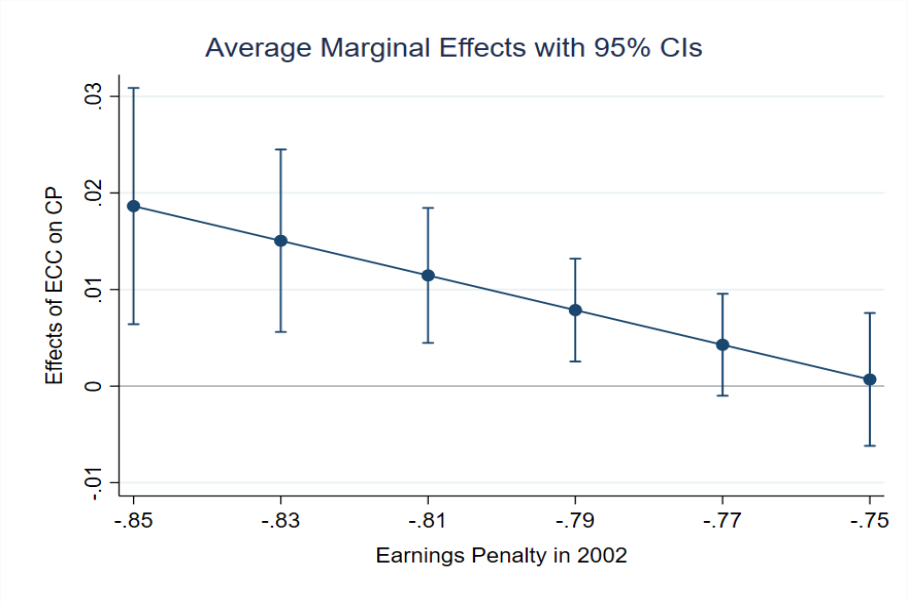


(d) Firm/Occupation Wage Channel



Note. This figure depicts the impact of early childcare expansion on mothers' child penalty in each year over a span of seven years following childbirth. In other words, we use mothers' outcomes in each year after childbirth (Y^e_{ist}) in equation (2) and present estimates (β^E_2) for mothers who had a child under three when the childcare expansion occurred. Child penalties in earnings, employment, and days worked are estimated unconditional on the employment status, while those in daily wages, full-time, non-mini-job, and firm/occupation wage are estimated conditional on being employed. 95% confidence intervals are presented.

Figure 8: Relationship Between Initial Child Penalties and Effects of Early Childcare

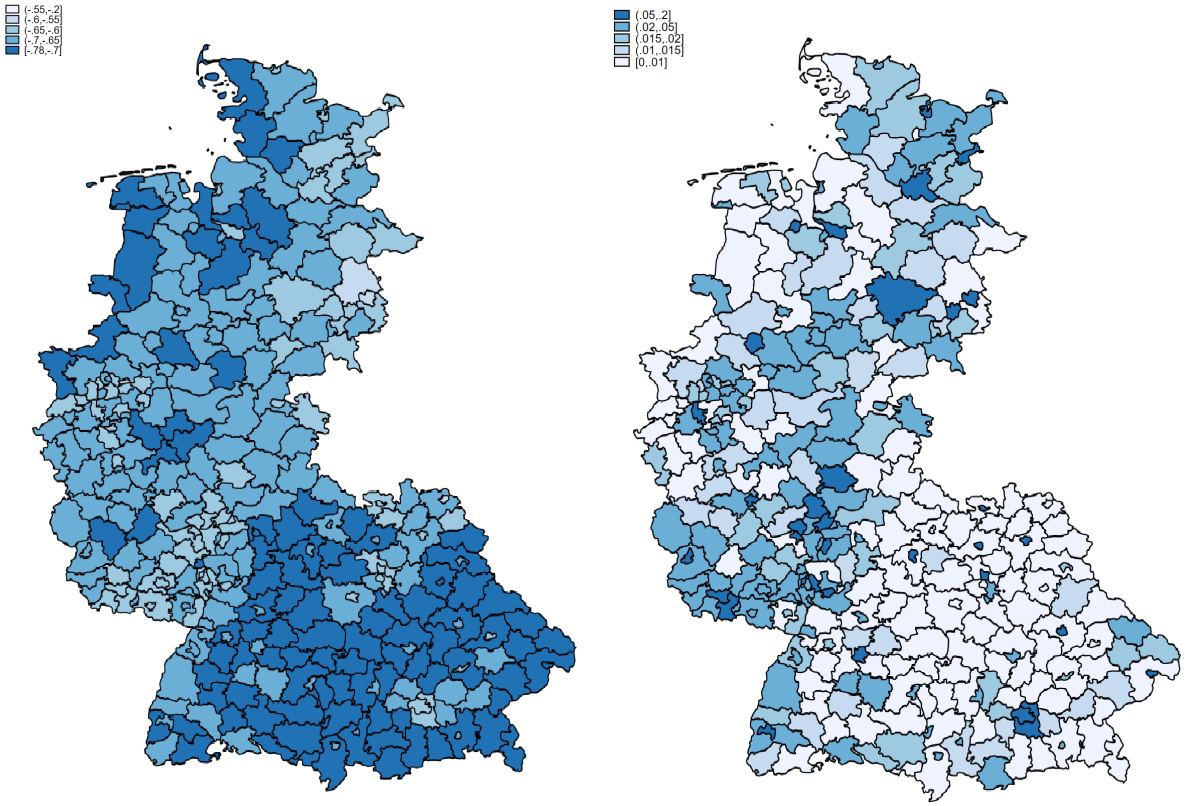


Note. This figure shows the average marginal effects at different initial child penalty levels, which are defined as the average earnings penalty over the first three years after childbirth for mothers whose youngest child was under age three in 2002. The regression is based on equation (9) in Appendix D, where the treatment indicator is interacted with the child penalty level in 2002. The coefficient of the interaction term is presented in the column (2) of A.4.

Figure 9: Child Penalties and Early Childcare Coverage Rates in 2002

(a) Child Penalties in Earnings

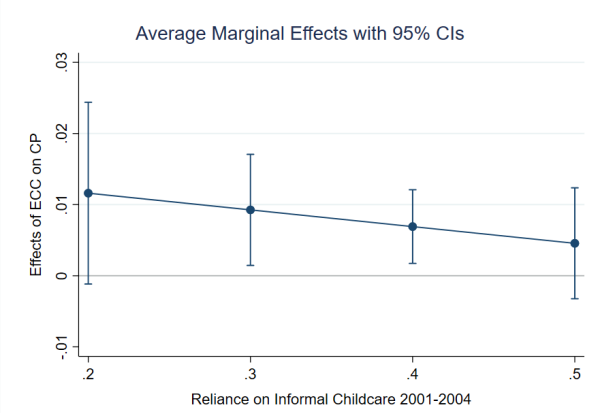
(b) Early Childcare Coverage Rate



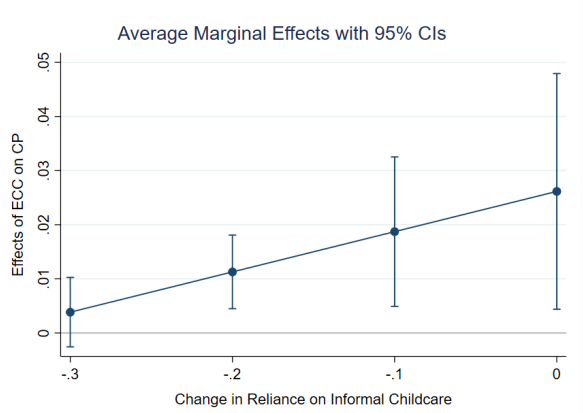
Note. Panel (a) shows the child penalties in earnings for the first three years after birth in each county for the year 2002. Child penalties are calculated as explained in Appendix B. Panel (b) shows the early childcare coverage rate in each county for the year 2002. The coverage rate is calculated as the number of children under three enrolled in public childcare divided by the total number of children under three in each year for each county (Source: Regionalstatistik (2023)).

Figure 10: Relationship Between Reliance on Informal Childcare and Effects of Early Childcare

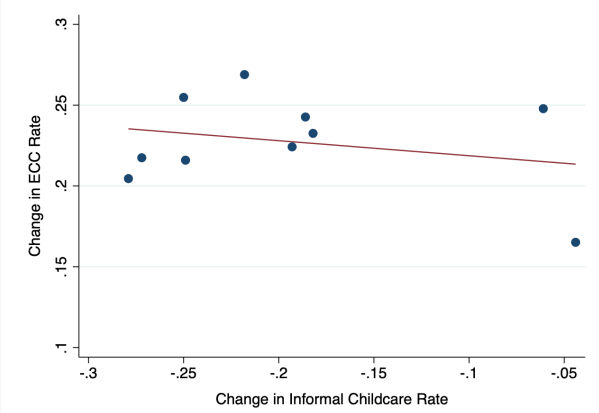
(a) ECC Effects By Reliance on Informal Childcare



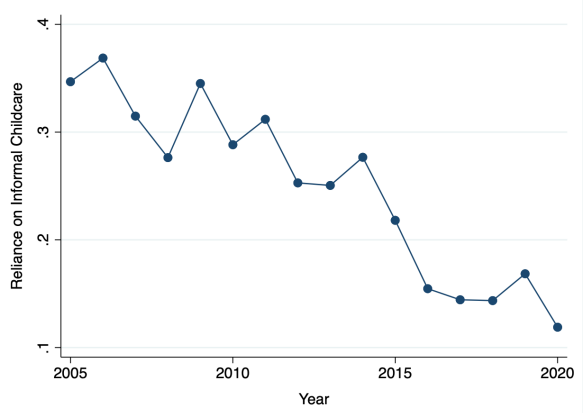
(b) Evidence for Crowding-out Effects



(c) Crowding-out of Informal Childcare By ECC



(d) Decline of Informal Childcare



Note. Panel (a) shows the average marginal effects at different levels of initial informal childcare reliance. Using equation (9), we estimate the effects of early childcare by interacting the treatment indicator with informal childcare reliance at the state level. We calculate the ratio of households reporting the use of childcare provided by relatives, friends (neighbors), or private babysitters among those with a first child under age three during 2001–2004 using the SOEP. Panel (b) shows the results from interacting the treatment indicator with the change in informal childcare reliance between 2001–2004 and 2015–2018 (the pre- and post-expansion periods), which relates our treatment effects to the crowding out of informal childcare. Panel (c) plots the relationship between changes in informal childcare reliance and changes in early childcare coverage rates, showing point estimates for each state in West Germany. Panel (d) shows the overall decline in informal childcare in West Germany over the childcare expansion period.

Tables

Table 1: Characteristics of Counties and Mothers in Treated and Control Counties

	One Year Prior to Childcare Expansion		
	Treated	Control	Normalized Difference
	(1)	(2)	(3)
Panel A. County-level Characteristics			
Early Childcare Coverage Rate	0.143	0.144	-0.01
	(0.056)	(0.055)	
Kindergarten Enrollment Rate	0.92	0.92	0.00
	(0.07)	(0.06)	
Fertility Rate	0.069	0.069	0.00
	(0.006)	(0.006)	
Female Employment Rate	0.50	0.51	-0.19
	(0.04)	(0.04)	
Population Share of Women Aged 20-40	0.11	0.11	0.00
	(0.06)	(0.07)	
Population Size	140,523	144,507	-0.03
	(86,721)	(81,952)	
Population Density	351.3	372.0	-0.03
	(420)	(435)	
Unemployment Rate	6.37	5.50	0.28
	(2.17)	(2.18)	
N of Observations	109	109	
Panel B. Individual Characteristics of Mothers			
Age	28.7	28.6	0.02
	(4.4)	(4.4)	
Ratio of College Graduates	0.11	0.11	0.00
	(0.31)	(0.31)	
Pre-birth Annual Earnings (Euros)	27,682	28,259	-0.03
	(13,590)	(13,582)	
N of observations	34,092	36,091	

Notes. Panel A displays the mean values of county-specific characteristics for the treated and control groups of counties one year prior to the early childcare expansion event. The early childcare coverage rate and kindergarten enrollment rate are calculated as the share of children enrolled in public childcare among children aged 0-2 and 3-6 in each county and year, respectively. The fertility rate is calculated as the number of newborn babies per women aged between 20-40, and the female employment rate is calculated for women aged 20-65. Population density indicates the number of people per square kilometer. Panel B presents the mean values of characteristics of mothers who had their first child one year before the childcare expansion event in the treated and control groups of counties. Pre-birth annual earnings represent the earnings during the one year before childbirth. Numbers in parentheses represent the standard deviations of each variable. We calculate the normalized difference between the treated and control groups of counties following Imbens and Rubin (2015).

Table 2: Effects of A Sudden Expansion in Early Childcare on Mothers' Child Penalties

Dependent Variable: $\overline{Y}_{it}$ (Average Outcome Over Three Years Following Childbirth)				
Panel A				
Main Outcomes:	Earnings	Employment	Days Worked	Daily Wage
	(1)	(2)	(3)	(4)
Effect of ECC Increase By 4pp on Outcomes of Mothers (relative to their pre-birth outcomes)				
1) Whose First Child Was 0-2-years Old When the Expansion Occurred (β_2^E)	0.009***	0.006**	0.007**	0.008**
	(0.003)	(0.003)	(0.003)	(0.003)
2) Who Gave Birth to Their First Child After the Expansion Occurred (β_3^E)	0.017***	0.011***	0.010***	0.019***
	(0.004)	(0.004)	(0.004)	(0.005)
N	815,952	815,952	815,952	730,044
Expected Effect of $\Delta ECC = 20pp$	0.045	0.030	0.035	0.040
$\Delta\overline{CP}_{2007-2014}$	0.08	0.05	0.05	0.14
Panel B				
Channels:	Full-time	Non-mini Job	Firm Wage	Occ Wage
	(1)	(2)	(3)	(4)
Effect of ECC Increase By 4pp on Outcomes of Mothers (relative to their pre-birth outcomes)				
1) Whose First Child Was 0-2-years Old When the Expansion Occurred	0.008**	0.005**	0.007*	0.006*
	(0.005)	(0.004)	(0.004)	(0.003)
2) Who Gave Birth to Their First Child After the Expansion Occurred	0.010**	0.006***	0.012**	0.005
	(0.006)	(0.005)	(0.006)	(0.004)
N	730,044	730,044	730,044	730,044
Expected Effect of $\Delta ECC = 20pp$	0.040	0.025	0.035	0.030
$\Delta\overline{CP}_{2007-2014}$	0.10	0.08	0.05	0.05

Notes. The table presents the coefficients estimated using equation (2). The effects for earnings and days worked are estimated unconditional on the employment status, while those for daily wage, full-time, non-mini job, firm-, and occupation wage are estimated conditional on being employed. Since the treatment is defined as a 5 pp increase in the coverage rate and the control group exhibits an average increase of 1 pp, the treatment reflects a 4 pp increase in early childcare coverage. Thus, with an overall increase of 20 pp in the coverage rate over the expansion period, the expected effect can be obtained by multiplying our main estimates for β_2^E by 5. Standard errors are clustered at the county level and are reported in parentheses (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Table 3: Effects of Early Childcare on Mothers' Child Penalties: East vs. West Germany

Dependent Variable: $\overline{CP}_{ct}$ (Average child penalty for three years following childbirth)				
A. East Germany				
Outcomes of Interest:	Earnings	Employment	Days Worked	Daily Wage
	(1)	(2)	(3)	(4)
Early Childcare Coverage Rate	0.04***	0.04***	0.03**	0.05***
	(0.01)	(0.01)	(0.01)	(0.01)
Z_{ct}, Year FE, County FE	Yes	Yes	Yes	Yes
N	597	597	597	597
Mean of Dep. Var.	-0.46	-0.23	-0.38	-0.18
B. West Germany				
Outcomes of Interest:	Earnings	Employment	Days Worked	Daily Wage
	(1)	(2)	(3)	(4)
Early Childcare Coverage Rate	0.16***	0.13***	0.14***	0.10***
	(0.02)	(0.03)	(0.03)	(0.03)
Z_{ct}, Year FE, County FE	Yes	Yes	Yes	Yes
N	2592	2592	2592	2592
Mean of Dep. Var.	-0.73	-0.49	-0.60	-0.31
Event Study Estimates	0.23***	0.15**	0.18**	0.20**

Notes. The table presents the estimates for the impact of early childcare increase on mothers' average child penalty over a three-year period following childbirth. These estimates are obtained from the regression equation (8) in Appendix C. Child penalties in earnings and days worked are estimated unconditional on the employment status, while those in other outcomes are estimated conditional on being employed. Firm wage is the mean wage of all full-time workers within each firm, while occupation wage is the average wage level of each occupation (three-digit level). Standard errors are clustered at the county level and are reported in parentheses (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). The regressions are weighted using the population size of each county in the year 2007.

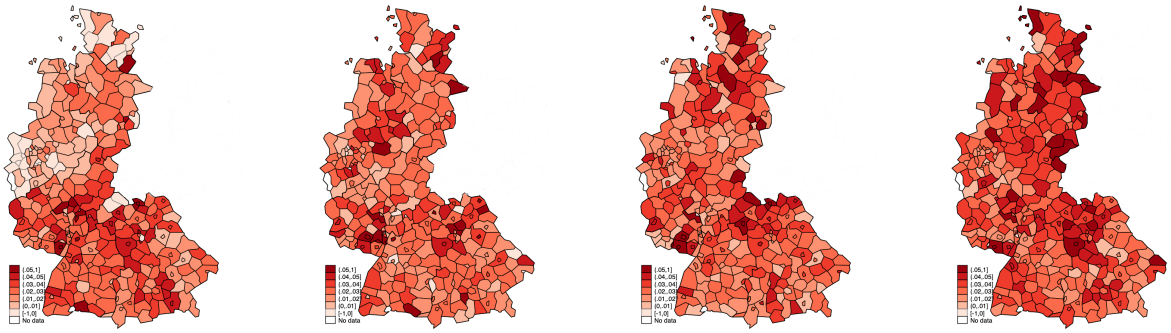
Table 4: Studies on the Effects of Early Childcare Expansion

Paper	Country	Age of Eligible Children	Method	Child Penalty (Kleven et al., 2023)	Informal Childcare (2019)	Childcare Effects (Coefficients)	Baseline	Normalized (Coef/Baseline)
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Baker et al. (2008)	Canada (Québec)	0–4	DDD	23	NA	0.077***	0.53	0.15
Goux and Maurin (2010)	France	2–3	RDD	25	18.2	0.025	0.7	0.01
Nollenberger and Rodríguez-Planas (2015)	Spain	3	DiD	38	8.9	0.028*	0.293	0.10
Carta and Rizzica (2018)	Italy	2	RDD	33	32.0	0.06*	0.573	0.10
Andresen and Havnes (2019)	Norway	2	DiD, IV	3	5.5	0.15***	0.63	0.24
Kunze and Liu (2019)	Norway	1–2	DiD	3	5.5	0.247**	0.72	0.34
Müller and Wrohlich (2020)	Germany	0–2	DiD	41	8.7	0.219*	0.46	0.48
Attanasio et al. (2022)	Brazil	0–3	RCT, IV	37	NA	0.007	0.77	0.01
Rabaté and Rellstab (2022)	Netherlands	0–2	DiD	20	38.8	0.005	0.90	0.01
Kleven et al. (2024)	Austria	0–2	DiD	34	30.6	0.03	0.30	0.10
This paper	Germany	0–2	DiD	41	8.7	0.15***	0.51	0.29

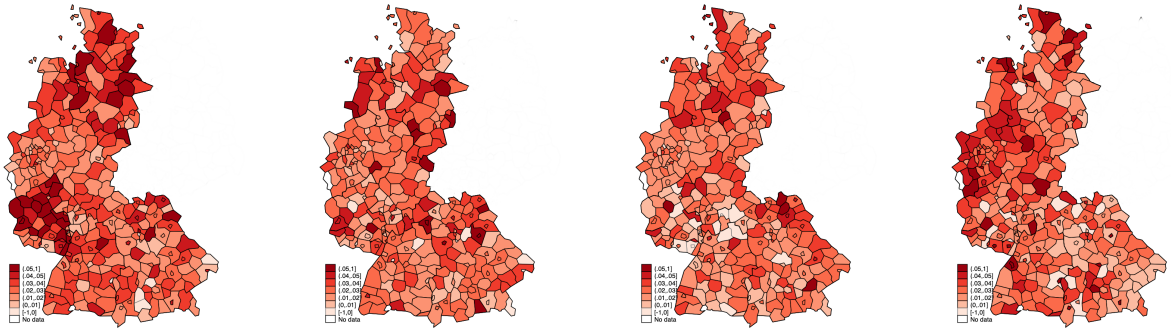
Note. Column (5) indicates the child penalty in employment as suggested by Kleven et al. (2023) for each country. Here it is important to note that child penalties in West Germany are even higher (57% for our observation period). Column (6) shows the proportion (%) of children aged 0–2 using informal childcare arrangements during a typical week in 2019, as reported by OECD (2021). Column (7) presents coefficients for the effects of early childcare expansion on maternal employment or labor force participation, potentially comparable to our matched event-study estimate. Column (8) shows the baseline employment(or LFP rate) in the period prior to the respective reform. These numbers are taken directly from the cited papers. Column (9) shows the coefficients normalized by this baseline level. Our estimate reflects the effect of a 1 percentage point (pp) increase in early childcare coverage on child penalties in employment, suggesting that a 100 pp increase could raise maternal employment rates by 15 pp among those attached to the labor market prior to birth. The baseline employment rate for 0–2 years after birth is 0.51. Baker et al. (2008) assess a childcare program in Québec that improved access and affordability. The coefficients from Goux and Maurin (2010) and Carta and Rizzica (2018) provide RD estimates for the effects of childcare eligibility on maternal employment. Because these are RD estimates and the treatment differs, it may be difficult to precisely compare their magnitudes with our estimates. Rabaté and Rellstab (2022) analyze a reform in the Netherlands that increased childcare availability by expanding subsidies. Nollenberger and Rodríguez-Planas (2015) assess the effects of an expansion for only three-year-olds. We include the triple-difference coefficient here. The coefficient from Andresen and Havnes (2019) represents a reduced-form estimate, more comparable to our intention-to-treat (ITT) DiD estimates. Their IV estimate, which measures the effect of actually using full-year childcare, is 0.32***. Kunze and Liu (2019) present effects on employment using a DiD, also presenting an ITT. Müller and Wrohlich (2020) use a DiD, exploiting the coverage rate as a continuous treatment. Attanasio et al. (2022) perform an RCT in Brazil where childcare spots are randomly assigned. For parental labor market outcomes, they estimate an OLS coefficient which also represents an ITT. The coefficient from Kleven et al. (2024) indicates the effect of a 1 pp increase in early childcare coverage on the child penalty in earnings.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix A

Figure A.1: Yearly Change in Early Childcare Coverage by County, 2006–2014



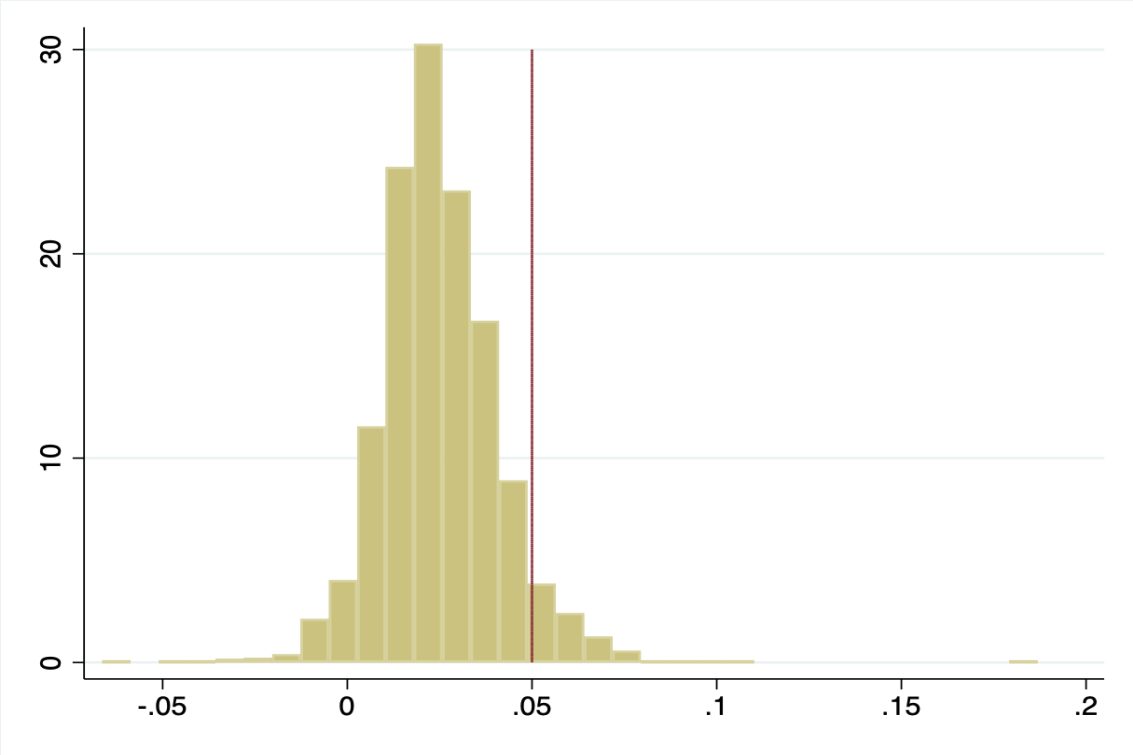
(a) 2006–2007 (b) 2007–2008 (c) 2008–2009 (d) 2009–2010



(e) 2010–2011 (f) 2011–2012 (g) 2012–2013 (h) 2013–2014

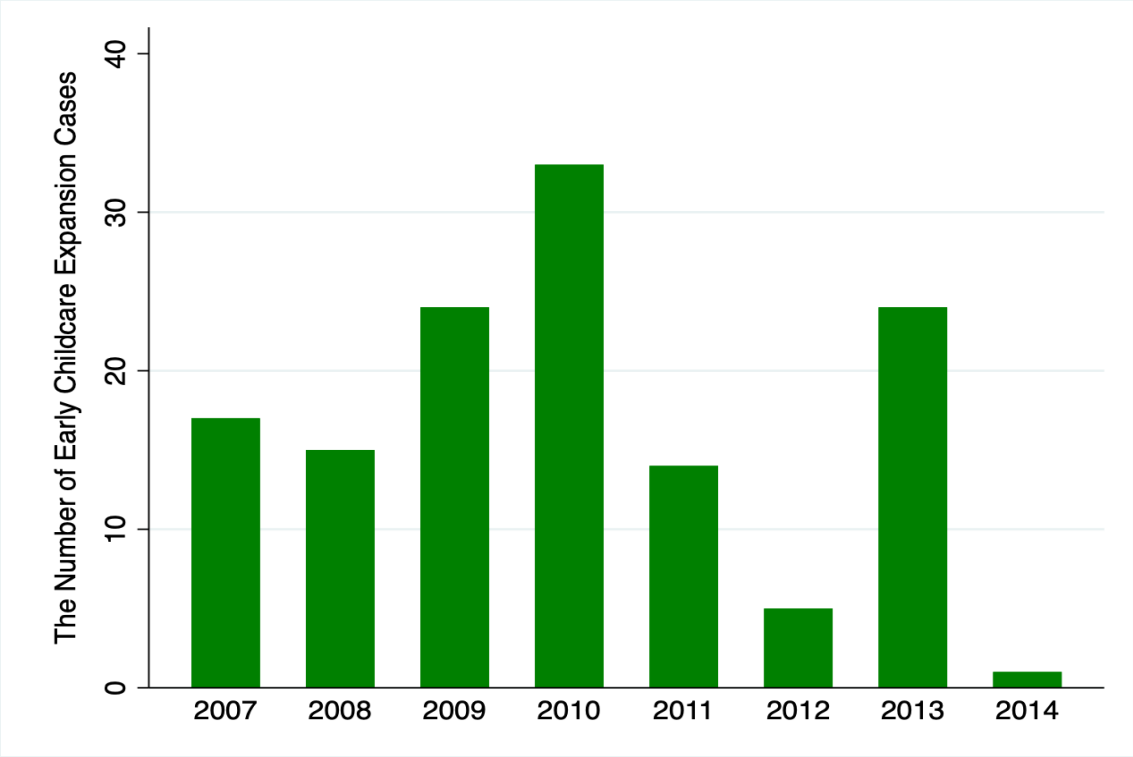
Note. The figure illustrates the yearly change in the early childcare coverage rate in each county between 2006 and 2014. The coverage rate is computed as the number of children under age three enrolled in public childcare divided by the total number of children under three (Data source: Regionalstatistik (2023)).

Figure A.2: Distribution of Changes in Early Childcare Coverage Rate



Note. The figure illustrates the distribution of annual changes in the early childcare coverage rate for 324 counties in West Germany during the period between 2007 and 2014. An increase by more than 5 percentage points represents the top 5 percent of the distribution, with 132 cases out of 2268 exceeding this threshold. This cutoff value is used to define the early childcare expansion event.

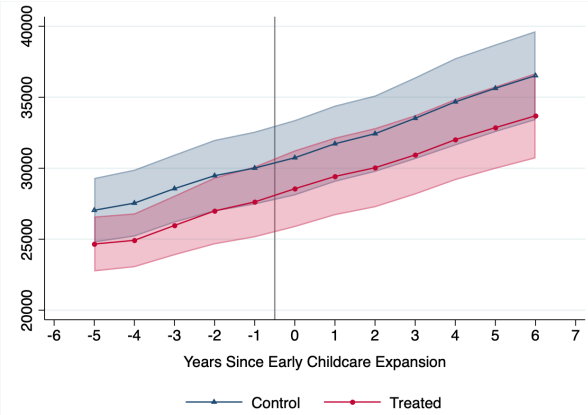
Figure A.3: The Number of Cases of Early Childcare Expansion Event



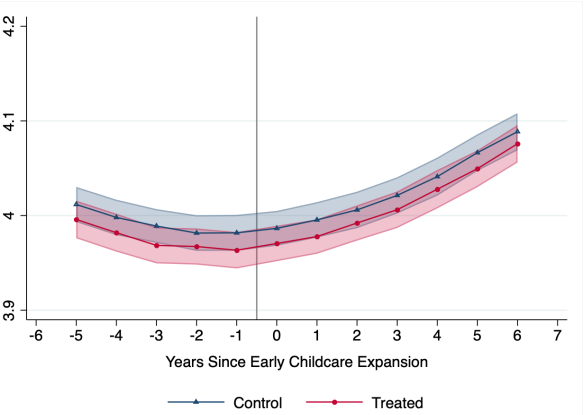
Note. The figure displays the number of early childcare expansion events that occurred across different years. An expansion event is defined as a county experiencing an annual increase in the early childcare coverage rate by more than 5 percentage points. In other words, if a county had a single-year increase in early childcare coverage rate exceeding 5 pp, it is categorized as a treated county.

Figure A.4: Trend of Variables Potentially Related To Childcare Expansion

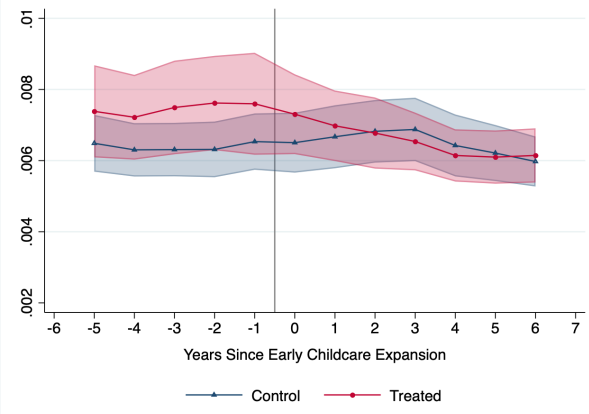
(a) GDP per Capita



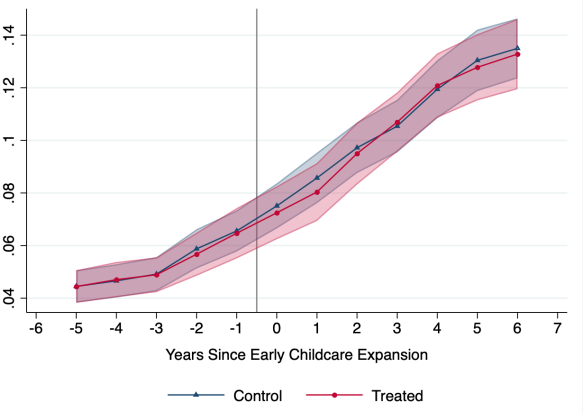
(b) Log Wage



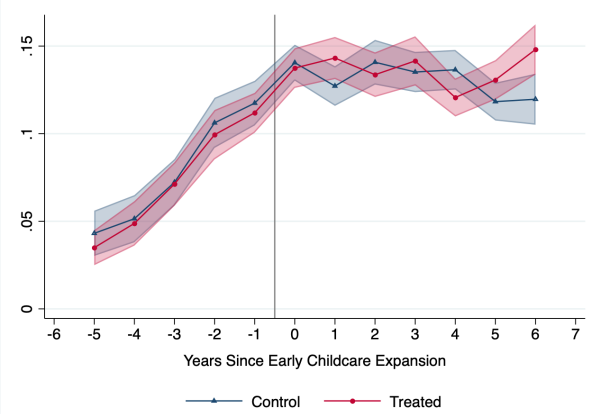
(c) Employment Share of Construction



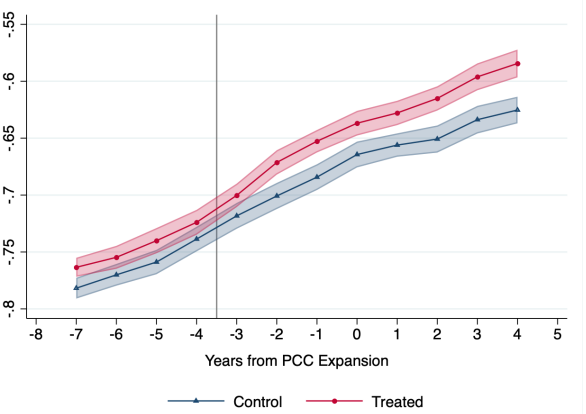
(d) Ratio of High-Edu Women Aged 25-40



(e) Paternity Leave Take-up Ratio



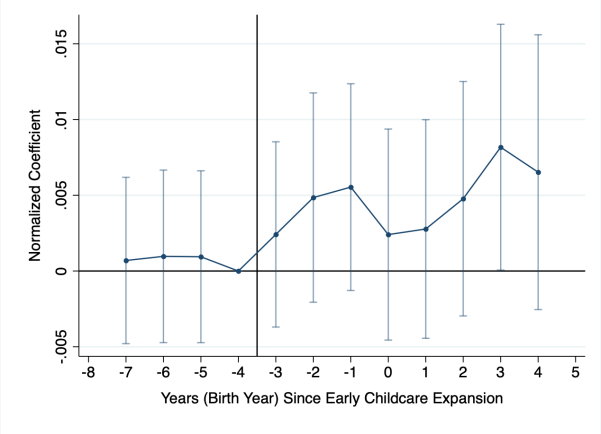
(f) CP When a Child is 1-2-years-old



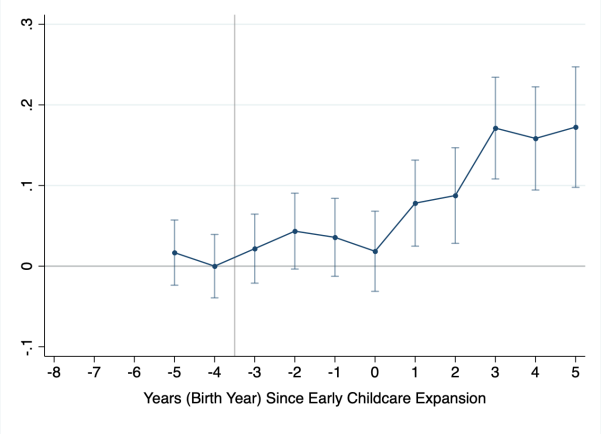
Note. The figure illustrates the trends of variables that could potentially be correlated with the early childcare expansion event but were not used for matching in the main event study analysis. Panels (a)-(f) compare the trends of GDP per capita, log(wage), employment share of the construction sector, ratio of college graduates among women aged 25-40, fathers' parental leave take-up rate, and the child penalties when a first child is 1 and 2 years old between the treated and control counties. These variables were used for matching to test for robustness and produced consistent results, as explained in Section 6.2.2.

Figure A.5: Effects of ECC Expansion on Child Penalties in the First Year Following Childbirth

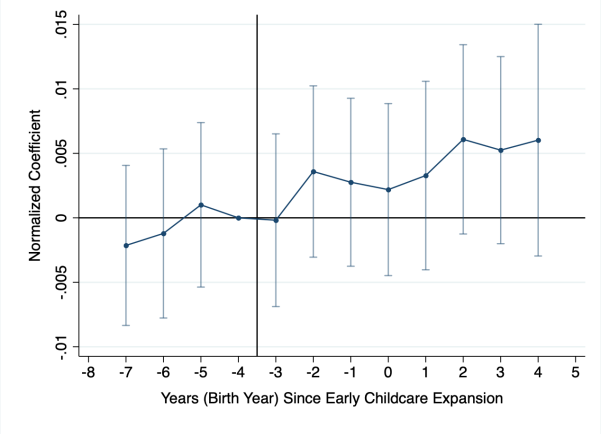
(a) Earnings: Main Specification



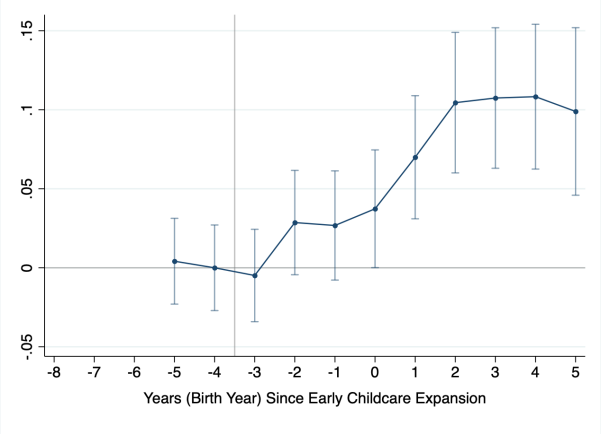
(b) Earnings: Kleven et al(2024) Specification



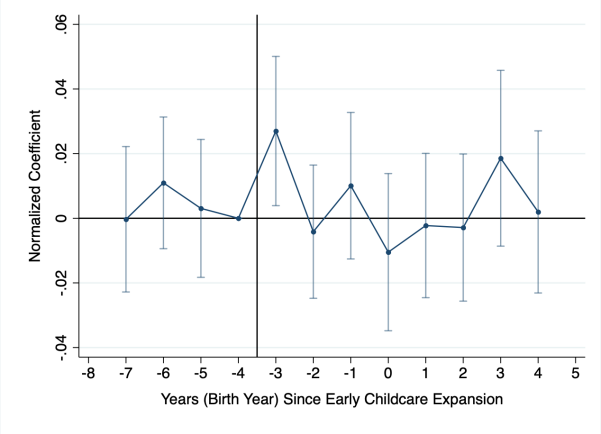
(c) Days Worked: Main Specification



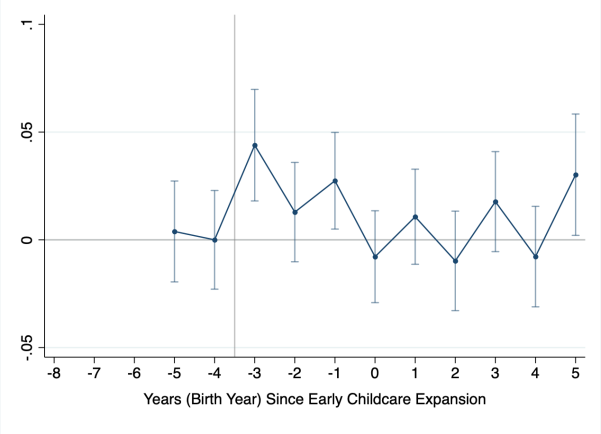
(d) Days Worked: Kleven et al(2024) Specification



(e) Daily Wages: Main Specification



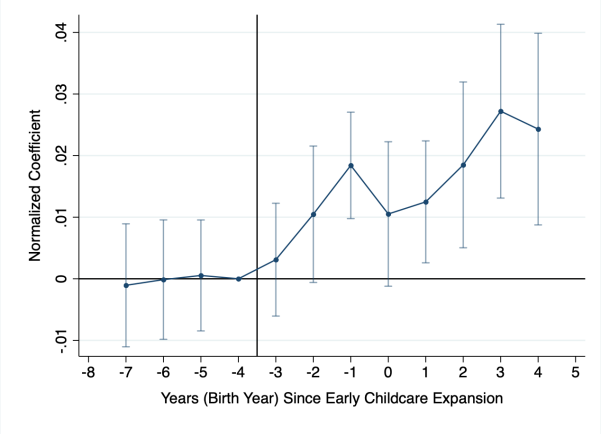
(f) Daily Wages: Kleven et al(2024) Specification



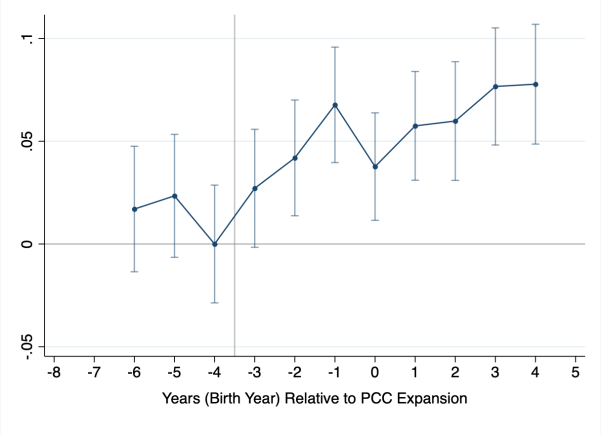
Note. This figure compares our event-study estimates to those obtained from applying Kleven et al. (2024). Panels (a), (c), and (e) plot coefficients estimated using $Y_{ist}^{e=0}$ as an outcome in equation (1). Panels (b), (d), and (f) plot the coefficients estimated using the estimation model where the equation (1) for different years e is stacked into a single specification as in Kleven et al. (2024). To make the results comparable to those in Panels (a), (c), and (e), we present all effects relative to the four years prior to childcare expansion. These coefficients are normalized to the expected outcome in the absence of a child as in Kleven et al. (2024).

Figure A.6: Effects of ECC Expansion on Child Penalties in the Second Year Following Childbirth

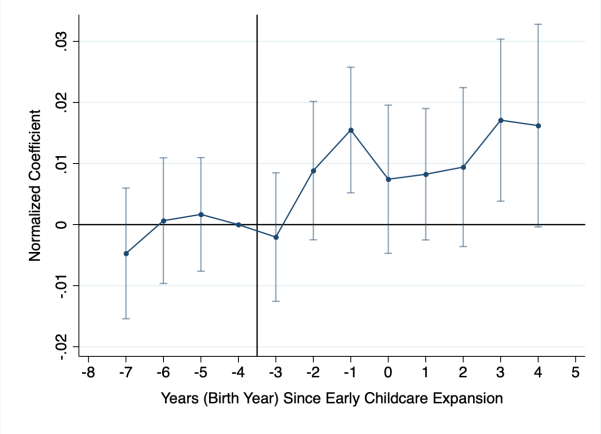
(a) Earnings: Main Specification



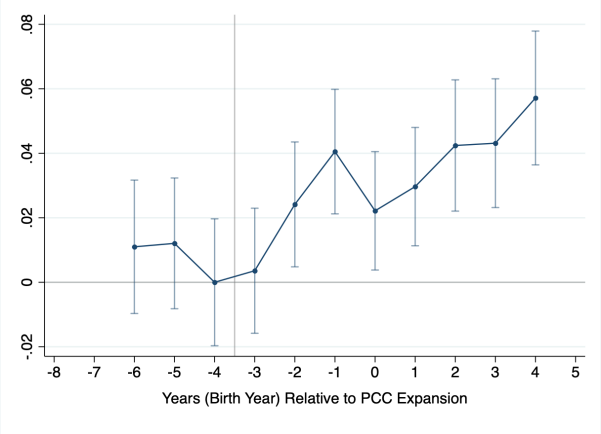
(b) Earnings: Kleven et al(2024) Specification



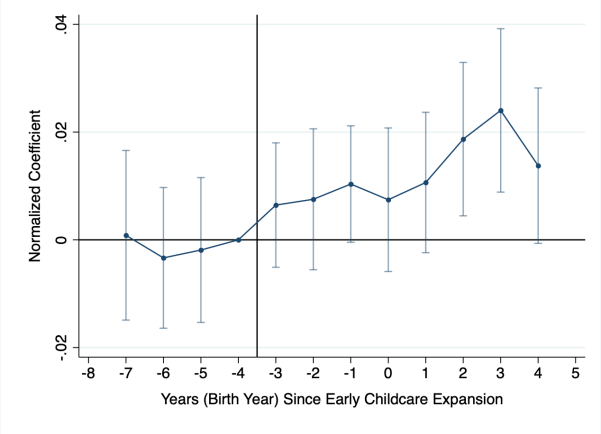
(c) Days Worked: Main Specification



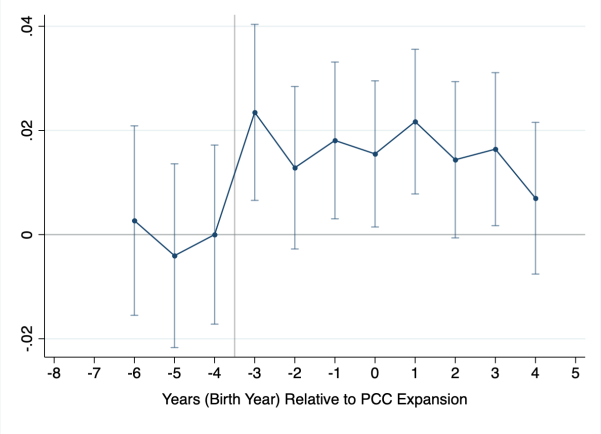
(d) Days Worked: Kleven et al(2024) Specification



(e) Daily Wages: Main Specification



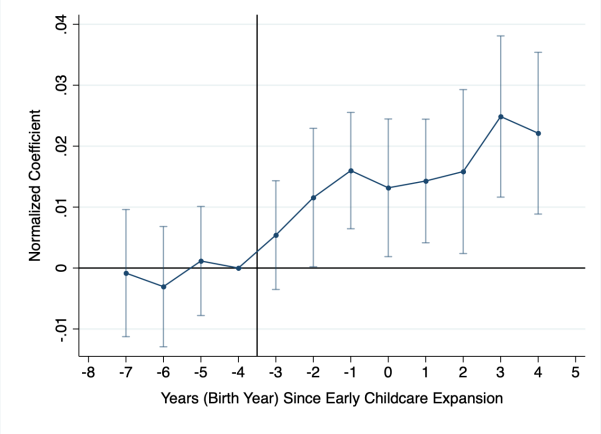
(f) Daily Wages: Kleven et al(2024) Specification



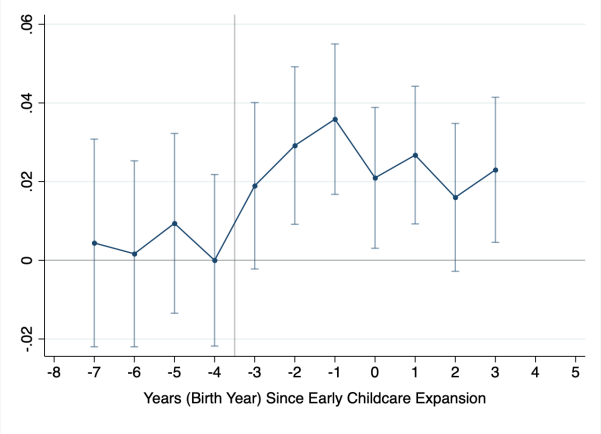
Note. This figure compares our event-study estimates to those obtained from applying Kleven et al. (2024). Panels (a), (c), and (e) plot coefficients estimated using $Y_{ist}^{e=1}$ as an outcome in equation (1). Panels (b), (d), and (f) plot the coefficients estimated using the estimation model where the equation (1) for different years e is stacked into a single specification as in Kleven et al. (2024). To make the results comparable to those in Panels (a), (c), and (e), we present all effects relative to the four years prior to childcare expansion. These coefficients are normalized to the expected outcome in the absence of a child as in Kleven et al. (2024).

Figure A.7: Effects of ECC Expansion on Child Penalties in the Third Year Following Childbirth

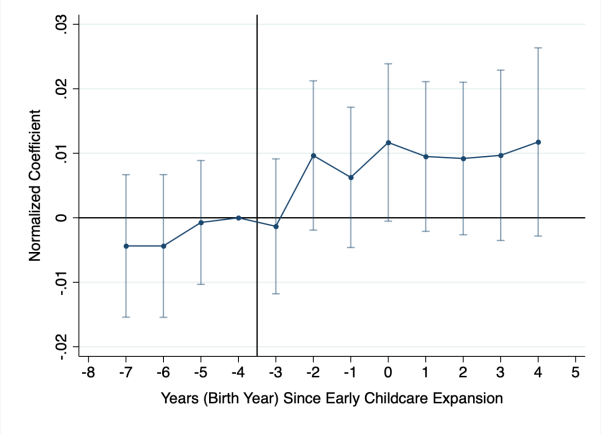
(a) Earnings: Main Specification



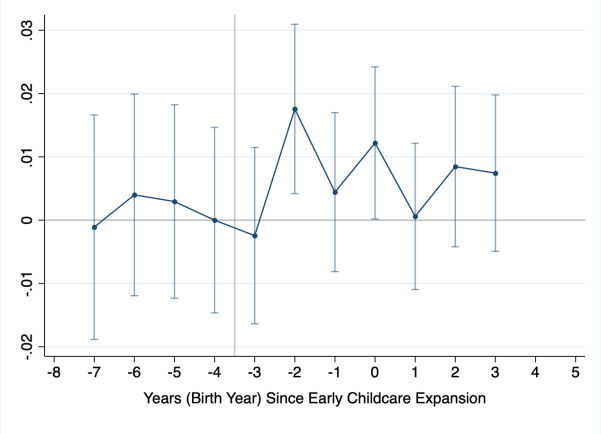
(b) Earnings: Kleven et al(2024) Specification



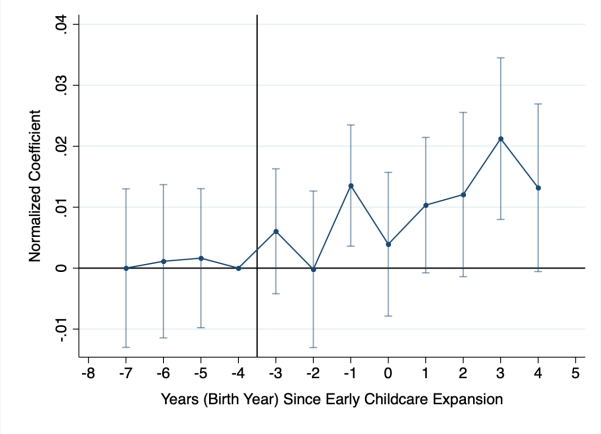
(c) Days Worked: Main Specification



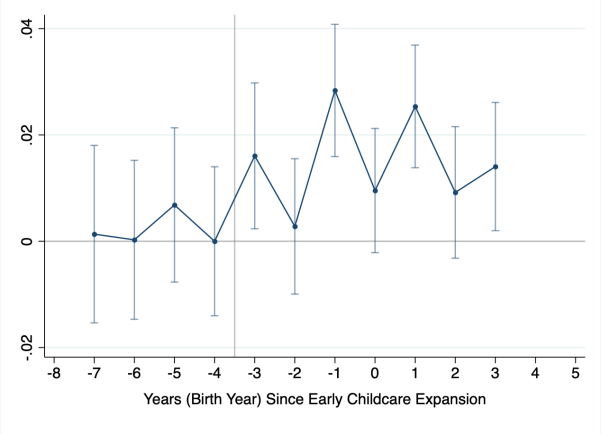
(d) Days Worked: Kleven et al(2024) Specification



(e) Daily Wages: Main Specification



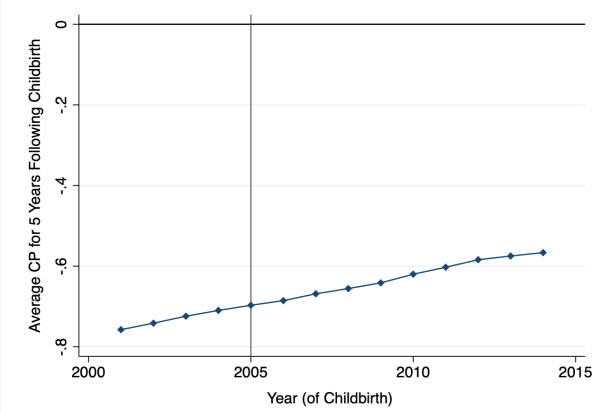
(f) Daily Wages: Kleven et al(2024) Specification



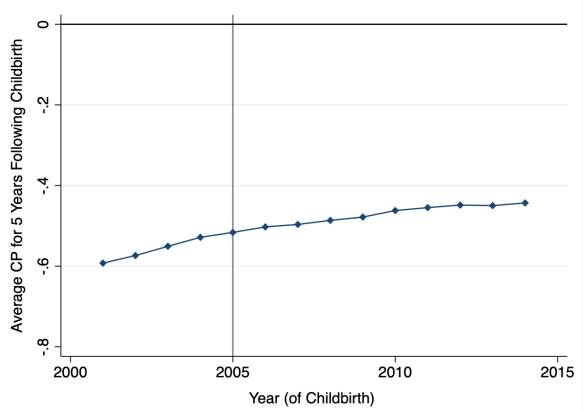
Note. This figure compares our event-study estimates to those obtained from applying Kleven et al. (2024). Panels (a), (c), and (e) plot coefficients estimated using $Y_{ist}^{e=2}$ as an outcome in equation (1). Panels (b), (d), and (f) plot the coefficients estimated using the estimation model where the equation (1) for different years e is stacked into a single specification as in Kleven et al. (2024). To make the results comparable to those in Panels (a), (c), and (e), we present all effects relative to the four years prior to childcare expansion. These coefficients are normalized to the expected outcome in the absence of a child as in Kleven et al. (2024).

Figure A.8: Yearly Trend of Child Penalties in West Germany

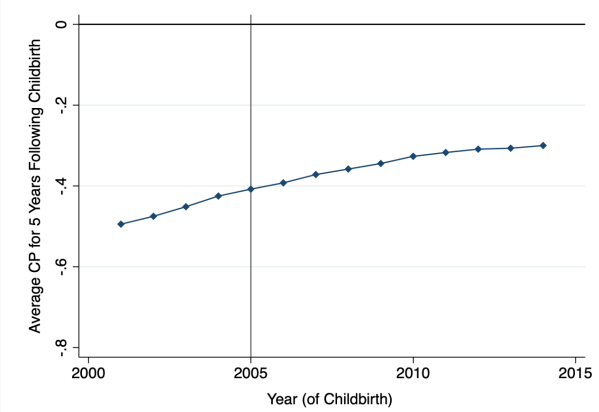
(a) Earnings



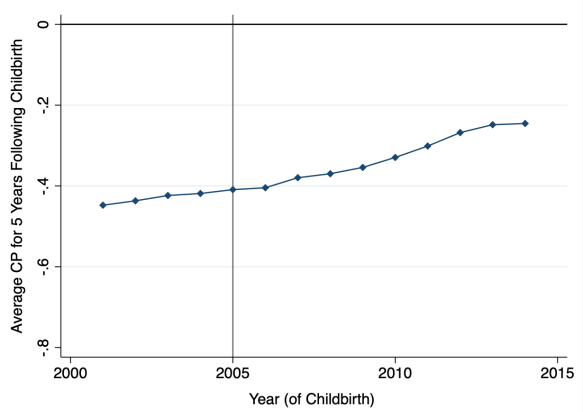
(b) Days Worked



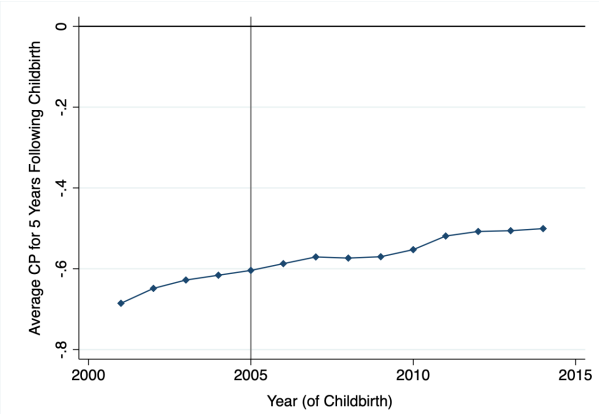
(c) Employment



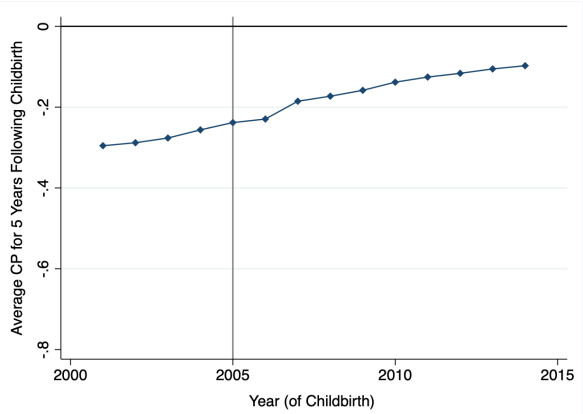
(d) Daily Wages



(e) Working Full-time

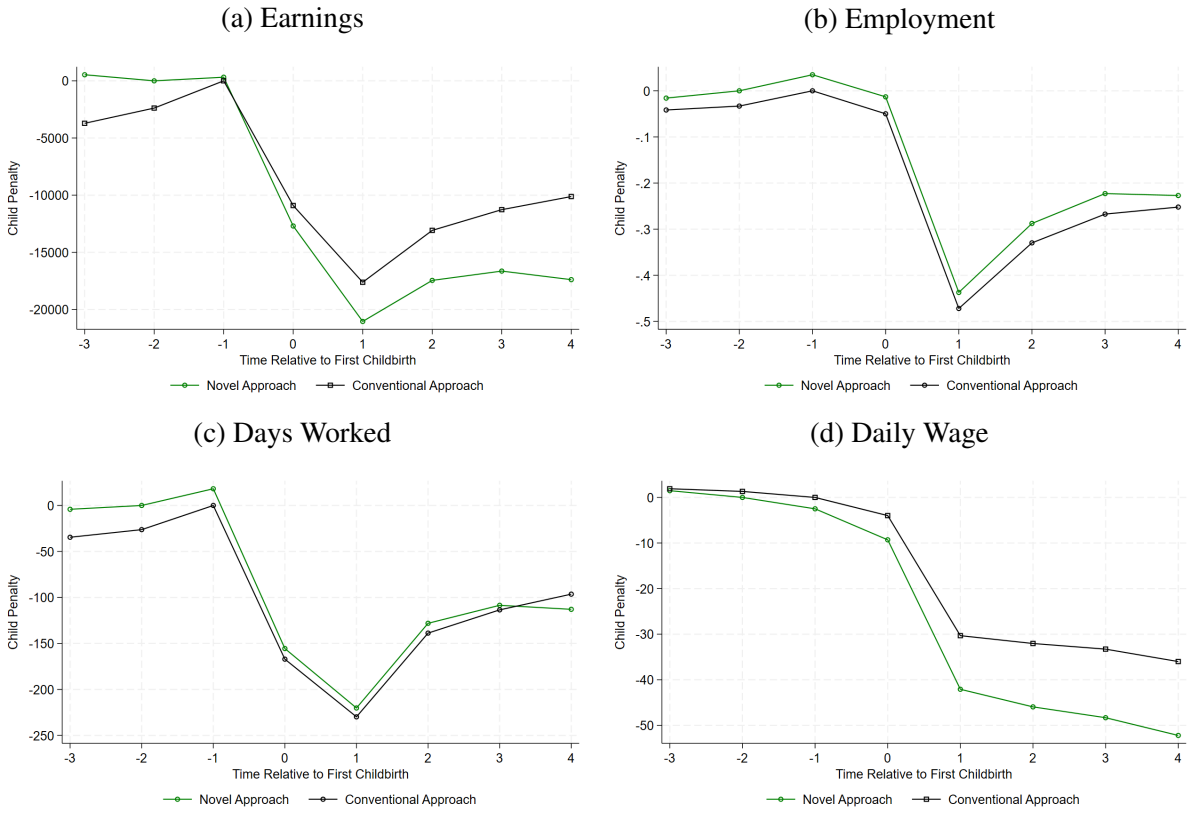


(f) Non-mini Job Employment



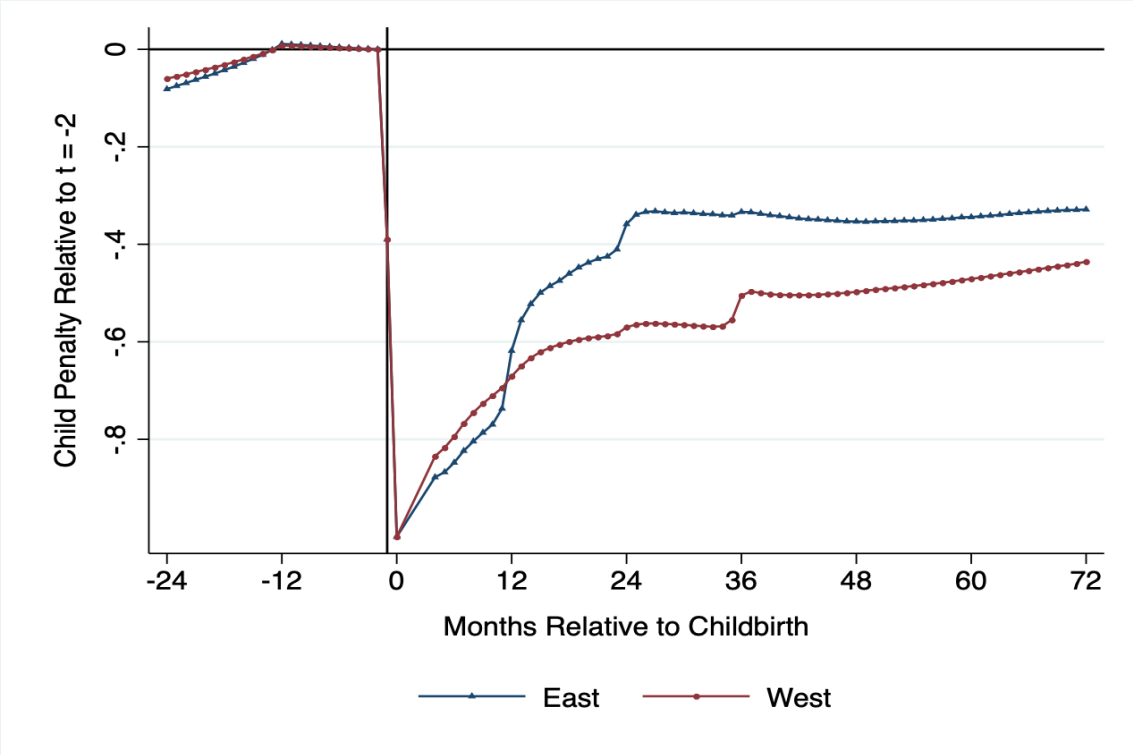
Note. The figure plots the average child penalties over the three years following childbirth by birth cohort. We estimate cohort-specific penalties by interacting birth-cohort dummies with event-time dummies in equation (3) as in Kleven et al. (2019b), and then average these penalties over the three years after childbirth for each cohort. For earnings and days worked, child penalties are estimated without conditioning on employment status (being employed for at least 30 days per year), assuming zero earnings and days worked for the unemployed. For other outcomes, child penalties are estimated conditional on being employed.

Figure A.9: Robustness of Child Penalty Estimates



Note. The figure shows the replication of Melentyeva and Riedel (2023) using our sample. We follow the same preparation steps and sample restrictions as explained in Melentyeva and Riedel (2023). Consistent with their approach, the coefficients are not normalized to pre-birth outcomes. Standard errors are clustered on individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.10: Child Penalty for Mothers Who Gave Birth Between 2003 and 2006



Note. The figure displays the monthly child penalty in employment for mothers who had their first child between 2003 and 2006 in East and West Germany, respectively. The estimation of child penalties is based on equation (3) described in Appendix B.

Table A.1: Event Study Results When Defining Childcare Expansion Using Different Thresholds

Dependent Variable: $\overline{Y}_{ist}$ (Average Earnings Over Three Years Following Childbirth)					
	(1)	(2)	(3)	(4)	(5)
Threshold:	4%	4.2%	5%	5.8%	6%
Effect of ECC Increase on Outcomes of Mothers (relative to their pre-birth outcomes)					
1) Whose First Child Was 0-2-years Old When the Expansion Occurred	0.007**	0.011***	0.009***	0.009***	0.007
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
2) Who Gave Birth to Their First Child After the Expansion Occurred	0.008	0.017***	0.017***	0.016***	0.019***
	(0.005)	(0.005)	(0.004)	(0.005)	(0.006)

Note. The table presents the coefficients for the effects of early childcare expansion for the cases when using different thresholds for defining a childcare expansion event. When we define our event as a case where the early childcare coverage rate of a county increases by more than 4, 4.2, 5, 5.8, 6 percentage points in a single year, it corresponds to the top 12, 10, 5, 3, 2.5 percent of changes in early childcare coverage rate at the county-year level between 2007 and 2014. Standard errors are reported in parentheses (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Table A.2: Event Study Analysis Results When Constructing Control Counties Using Different Matching Variables

Dependent Variable: $\overline{Y}_{ist}$ (Average Earnings Over Three Years Following Childbirth))								
Effect of ECC Increase By 0.04 on Outcomes of Mothers (relative to their pre-birth outcomes) Whose First Child Was 0-2-years Old When the Expansion Occurred								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coefficients	0.009***	0.010***	0.007**	0.011***	0.010***	0.012***	0.010***	0.005*
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Matching Variables								
Early Childcare Coverage Rate	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Kindergarten Enrollment Rate	Yes	Yes	Yes	Yes		Yes	Yes	Yes
Fertility Rate	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Population Share of Women 20-40	Yes	Yes		Yes	Yes	Yes	Yes	Yes
Female Employment Rate	Yes		Yes	Yes	Yes	Yes	Yes	Yes
Population Size	Yes	Yes	Yes	Yes	Yes	Yes		Yes
Population Density							Yes	
Unemployment Rate						Yes		Yes
GDP per Capita						Yes	Yes	Yes
Empl. Share of Construction								Yes
Urban Status	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note. The table presents the coefficients for the effects of early childcare expansion on earnings for the cases when constructing a control group of counties using different combinations of matching variables. Column (1) shows the result of our main specification. We choose control counties among never-treated counties using a propensity score matching in terms of numerous variables suggested above. Standard errors are reported in parentheses (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Table A.3: Robustness and Sensitivity Analysis Results for Event Study Analysis

Dependent Variable: $\overline{Y}_{ist}$ (Average Earnings Over Three Years Following Childbirth)									
	(1) Baseline	(2) Strict Cont.	(3) Non-Movers	(4) Economic Shocks	(5) High-edu Women	(6) Paternity Leave	(7) Pre-2007	(8) Mahal. Distance	(9) Excl. Care Workers
Effect of ECC Increase on Outcomes of Mothers (relative to pre-birth outcomes)									
1) First child aged 0–2 at expansion	0.009***	0.008***	0.009***	0.006**	0.009***	0.009***	0.012**	0.007**	0.007***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.006)	(0.003)	(0.002)
2) First child born after expansion	0.017***	0.015***	0.017***	0.012**	0.015***	0.015***		0.014***	0.014**
	(0.004)	(0.005)	(0.004)	(0.005)	(0.003)	(0.003)		(0.005)	(0.007)
Additional controls									
GDP per capita, Empl. share of industry				Yes					
Share of highly-educated women					Yes				
Paternity leave take-up						Yes			
N	815952	803756	815952	730044	815952	815952	69,210	815952	741,004

Note. Column (1) presents the main coefficients for the impact of early childcare expansion on mothers' child penalty in earnings, estimated using equation (2). Column (2) reports the results when the control counties are restricted to those that never experienced an increase in early childcare coverage exceeding 2 percentage points. Column (3) shows the estimates when limiting the sample to mothers who remained in the same county from one year before to three years after childbirth. Column (4) presents results controlling for GDP per capita and the employment shares in manufacturing, IT, public administration, education, and health. Column (5) and (6) report results when controlling for the share of highly educated mothers and fathers' parental leave take-up rates, respectively. Column (7) shows the results when restricting sample to mothers who gave birth before 2007 and focusing solely on the 2007 expansion shock. Column (8) shows the results when using a multivariate-distance (Mahalanobis) matching method to construct the control group. Column (9) reports results when excluding mothers employed in the childcare sector from our sample. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix B

Estimation of Child Penalties

To estimate the child penalty for annual earnings and days worked, we aggregate the monthly information into yearly level and estimate the child penalty using the estimation equation as follows:

$$Y_{ise} = \sum_{\tau=-3(\neq -1)}^{\tau=6} \alpha_\tau \cdot \mathbf{I}[e=\tau] + \sum_a \beta_a \cdot \mathbf{I}[age_{is}=a] + \sum_y \gamma_y \cdot \mathbf{I}[s=y] + \epsilon_{ise} \tag{3}$$

where Y_{ise} represents the outcome for individual i, in calendar year s, and at event time e. To precisely estimate the child penalty in each year following birth, we construct yearly panel data from monthly information, with the 12 months right before childbirth serving as a baseline year $(e=-1)$. The event time $e=0$ corresponds to the first 12 months following childbirth, and s indicates the rounded calendar year.[50] We consider event times from -3 to 6 for our main analysis[51] and omit the event time dummy at $e=-1$ so that the event time coefficients capture the impact of having a child relative to the year right before childbirth. To control for life-cycle and time trends, we include a full set of dummies for age and calendar year. As the child penalty is estimated relative to pre-birth outcomes, we restrict our sample to mothers who were employed for at least one year before childbirth.[52]

Converting the estimated level effects into percentages, we define the child penalty as

$$P_e \equiv \frac{\hat{\alpha}_e}{E[\tilde{Y}_{ise}|e]} \tag{4}$$

where $\tilde{Y}_{ise} \equiv \sum_k \hat{\beta}_a \cdot \mathbf{I}[age_{is}=a] + \sum_y \hat{\gamma}_y \cdot \mathbf{I}[s=y]$ represents the predicted outcome absent a child. In other words, we define the child penalty as the impact of having a child relative to individuals' pre-birth outcomes, assuming a smooth earnings trajectory in the absence of childbirth.

We estimate child penalties at the monthly level for employment, daily wages, working full-time, and non-mini job employment.[53] To do this, we modify equation (3) by replacing yearly event time dummies with monthly-level dummies as follows:

[50] For example, if earnings are calculated ranging from March 2012 to February 2013, 2012 is considered as the calendar year, and age is calculated based on this calendar year.

[51] We consider the time span up to 7 years (84 months) following birth because our data extends until 2021, and seven years is the maximum for mothers who gave birth in 2014, the last year of the childcare expansion period.

[52] We consider those employed for at least 30 days per year as being employed in our yearly level analysis.

[53] For daily wages, working full-time, and non-mini job employment, child penalties are estimated conditional on employment.

$$Y_{isq} = \sum_{m=-24(\neq -12)}^{m=72} \alpha_m \cdot \mathbf{I}[q = m] + \sum_a \beta_a \cdot \mathbf{I}[age_{is} = a] + \sum_y \gamma_y \cdot \mathbf{I}[s = y] + MonthFE + \epsilon_{isq} \quad (5)$$

where Y_{isq} represents the outcome for individual i in calendar year s, and q months since childbirth, with $q = 0$ corresponding to the month of childbirth. We consider event times ranging from 24 months before to 72 months after childbirth for our analysis. We omit the event time dummy at $q = -12$ to capture the impact of having a child relative to one year before taking maternity leave. To account for life-cycle, time trends, and seasonal effects, we include a full set of dummies for age, calendar year, and calendar month. Similar to the yearly level analysis, we convert the estimated level effects into percentages relative to the predicted outcome absent a child as in equation (4).

Appendix C

Generalized Difference-in-Differences Estimation

Conducting a generalized difference-in-differences estimation, under the assumption that regional variations in childcare expansion are driven by factors exogenous to mothers' employment conditional on specific covariates, offers several benefits. First, it allows for the comprehensive utilization of all available variations in childcare expansion across counties and over time. Second, it yields straightforward and intuitive estimates, providing an elasticity of child penalties with respect to childcare coverage rate, which enables a more practical and policy-relevant interpretation of the effects.

In our generalized difference-in-differences estimation, we directly examine the effects of early childcare expansion on child penalties. To do so, we first estimate child penalties at the county level building on the approach of Kleven (2022), as follows:

$$\begin{aligned} Y_{isec} = \sum_{z} \sum_{\tau \neq -1} \alpha_{\tau}^{z} \cdot \mathbf{I}[e = \tau] \cdot \mathbf{I}[c = z] + \sum_{z} \beta_{c} \cdot \mathbf{I}[c = z] + \\ \sum_{a} \gamma_{a} \cdot \mathbf{I}[age_{is} = a] \cdot State_{s} + \sum_{y} \delta_{y} \cdot \mathbf{I}[s = y] \cdot State_{s} + \nu_{isec} \end{aligned} \tag{6}$$

where Y_{isec} is the labor market outcome for individual i in county c, at event time e, and in calendar year s. In this specification, the event is childbirth, and the event time dummies are interacted with county dummies. Following the approach of Kleven (2022), we also interact state dummies with age and calendar year dummies to account for life-cycle and time trends at the state level. The event time $e = 0$ corresponds to the birth year (the first 12 months after childbirth), and we consider event times ranging from years -3 to 6. The child penalty for mothers in county c at event time e (i.e., e years after giving birth) is defined as:

$$P_{c}^{e} \equiv \frac{\hat{\alpha}_{c}^{e}}{E[\tilde{Y}_{isec}|e]} \tag{7}$$

where $\tilde{Y}_{isec} \equiv \hat{\beta}_{c} + \sum_{a} \hat{\gamma}_{a} \cdot \mathbf{I}[age_{is} = a] + \sum_{y} \hat{\delta}_{y} \cdot \mathbf{I}[s = y]$ is the predicted outcome in the absence of a child. To estimate the child penalty for each year t during the sample period 2007-2014, we run the regression in equation (6) separately for mothers whose first child was under age three in year t, and define P_{ct}^{e} as the child penalty in the e-th year after childbirth for mothers who had a child under age three in year t in county c.

Using the child penalty estimates at the county-year level obtained above as outcome variables, we conduct a generalized difference-in-differences estimation as follows:

$$CP_{ct} = \alpha + \beta ECC_{ct} + \gamma Z_{ct} + Year_t + County_c + \epsilon_{ct} \tag{8}$$

where CP_{ct} is the average child penalty for three years ($\overline{P}_{ct} \equiv \frac{1}{3}\sum_{e=0}^{2} P_{ct}^{e}$) or the child penalty in the e-th year (P_{ct}^{e}) after childbirth for mothers in county c whose child is 0-2 years old in year t. ECC_{ct} denotes the early childcare coverage rate in county c and year t, Z_{ct} represents a set of county-year specific characteristics, including the average age of population, the population ratio of female, college graduates and women aged between 25 and 40, the employment share of each industry, population density, and GDP per capita. $Year_t$ and $County_c$ are year and county fixed effects, respectively. The regression is weighted using the population size of each county in the year 2007 and standard errors are clustered at the county level.[54]

[54]Because we use child penalty estimates obtained from equation (6) as dependent variables, equation (8) is a two-step estimation. Since the second-step estimated standard errors could be incorrect (Murphy and Topel, 2002), we estimate bootstrap standard errors with 500 replications and find the similar results.

Appendix D

Estimation of Context Analysis

To analyze how the effects of early childcare expansion vary depending on the level of initial child penalties ($InitialCP_c$) and reliance on informal childcare ($Informal_b$), we interact each variable with the post-treatment indicator building on equation (2) as follows:

$$\begin{aligned} \overline{Y}_{it} = \beta^{Interact} \cdot \mathbf{I}[t - t^* \in g] * Expansion_{is} * InitialCP_c(Informal_b) \\ +\beta^E \cdot \mathbf{I}[t - t^* \in g] * Expansion_{is} + \beta \cdot \mathbf{I}[t - t^* \in g] + Age_i + Year_s + County_c + \epsilon_{it} \end{aligned} \tag{9}$$

where $g = \{-3, -2, \ldots, 4\}$, which combine $g_2 = \{-3, -2, -1, 0\}$ and $g_3 = \{1, 2, 3, 4\}$ in equation (2). We aggregate the two event-time treatment indicators into a single post-treatment indicator, $\mathbf{I}[t - t^* \in g]$, and interact it with $InitialCP_c$ or $Informal_b$ for ease of interpretation.[55]

We include the linear interaction term based on the following evidence. First, for the initial child penalty, we classify counties (treated–control pairs) used in the event-study analysis into four groups based on the level of initial child penalties (earnings penalties in 2002) of the treated counties and estimate the effect of early childcare expansion separately for each group. In other words, we apply our event-study analysis to each group. Figure A.11 below plots the estimated effects across these groups and shows an approximately linear relationship.

Second, for informal childcare usage, we estimate the effects of early childcare expansion separately for each state by grouping the treated counties (and their control pairs) according to state classification. Panel (a) of Figure A.12 below shows a roughly linear relationship between initial reliance on informal childcare and the magnitude of early childcare expansion effects. Panel (b) plots the estimated effects against the change in informal childcare usage between the pre- and post-expansion periods by state, also revealing an approximately linear relationship. Taken together, this evidence supports our inclusion of a linear interaction term to capture heterogeneity by initial child penalty levels and reliance on informal childcare in the event-study analysis.

Column (1) of Table A.4 reports the result without the interaction term (i.e., the pooled estimate of $\beta_2^E + \beta_3^E$). Column (2) presents the result from the specification including the interaction between the post-treatment indicator and the initial child penalty level. The coefficient on the interaction term is negative and statistically significant, indicating that the early childcare expansion has larger

[55] If we instead interact each variable separately with the two event-time treatment indicators in equation (2), $\mathbf{I}[t - t^* \in g_2]$ and $\mathbf{I}[t - t^* \in g_3]$, the regression specification becomes overly complex and statistical power decreases. We confirm that the results are similar when presenting the coefficient on the interaction term with β_2 (that is, for mothers who gave birth right before the expansion and therefore benefited directly from the early childcare expansion) as shown in Figure A.13. Panel (a) of Figure A.13 corresponds to Figure 8, and panels (b) and (c) correspond to panels (a) and (b) of Figure 10.

effects in regions with higher initial child penalties.[56]

Column (3) presents the result when including the interaction between the post-treatment indicator and the initial reliance on informal childcare. The coefficient on the interaction term is negative, indicating that the early childcare expansion has larger effects in regions with lower reliance on informal childcare. Column (4) reports the result when interacting the post-treatment indicator with the change in informal childcare between the pre-reform and post-reform periods. The positive coefficient on this interaction term suggests that early childcare had a larger impact in regions that experienced greater declines in informal childcare usage, consistent with a crowding-out effect.

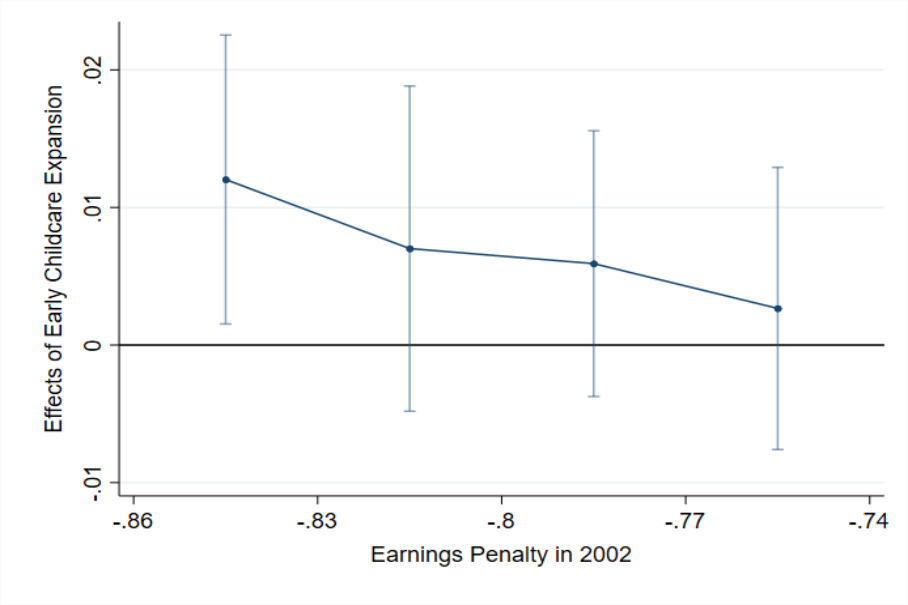
Table A.4: Heterogeneous Effects of Early Childcare Expansion Depending on Initial Child Penalty and Informal Childcare

Dependent Variable: $\overline{Y}_{it}$ (Average Employment Over Three Years Following Childbirth)				
	(1)	(2)	(3)	(4)
ECC Expansion	0.008***	-0.13**	0.017	0.026**
	(0.003)	(0.06)	(0.012)	
ECC Expansion X CP_{2002}		-0.18**		
		(0.08)		
ECC Expansion X $\text{Informal}_{2001-2004}$			-0.026	
			(0.030)	
ECC Expansion X Δ $\text{Informal}_{0104-1518}$				0.074*
				(0.043)
X_{ct}	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
N	815952	776217	776217	776217

Notes. Column (1) presents the baseline coefficient for the early childcare expansion effect without the interaction term in equation (9). Columns (2)–(4) report the coefficients on the interaction term from equation (9) when interacting the post-treatment indicator with, respectively, the initial child penalty level (earnings penalty in 2002), the initial reliance on informal childcare (informal childcare usage rate during 2001–2004), and the change in informal childcare usage between the pre- and post-reform periods (2001–2004 vs. 2015–2018). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[56] Although the interaction term coefficient is negative, we interpret it as indicating larger effects in areas with higher initial child penalties, since the child penalty variable itself is negative. Moreover, although the coefficient on the post-treatment indicator itself is negative, this is a mechanical result of the specification. For example, when evaluated at the mean initial child penalty of 0.8, the overall effect becomes positive, as illustrated in Figure 8.

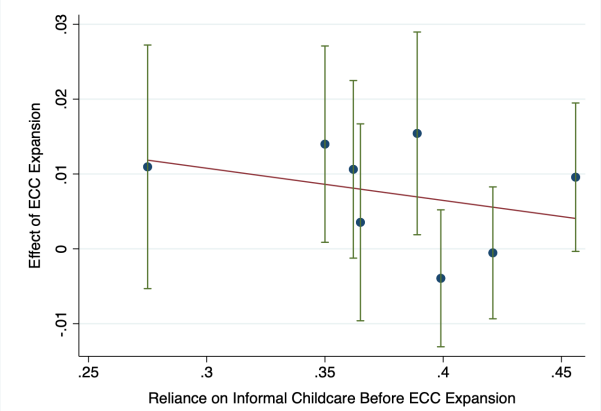
Figure A.11: Linear Relationship Between Initial Child Penalties and Effects of Early Childcare



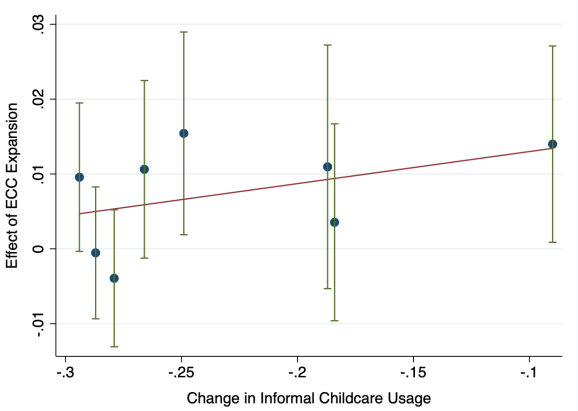
Note. This figure shows how the effects of early childcare expansion vary depending on the level of initial child penalties. We classify counties (treated–control pairs) used in the event-study analysis into four groups based on the level of initial child penalties (earnings penalties in 2002) of the treated counties and estimate the effect of early childcare expansion separately for each group. In other words, we apply our event-study analysis using equation (2) to each group and plot the main estimates (β_2^E) for each group. 95% confidence intervals are presented.

Figure A.12: Relationship Between Informal Childcare Usage and Effects of Early Childcare

(a) ECC Effects By Reliance on Informal Childcare



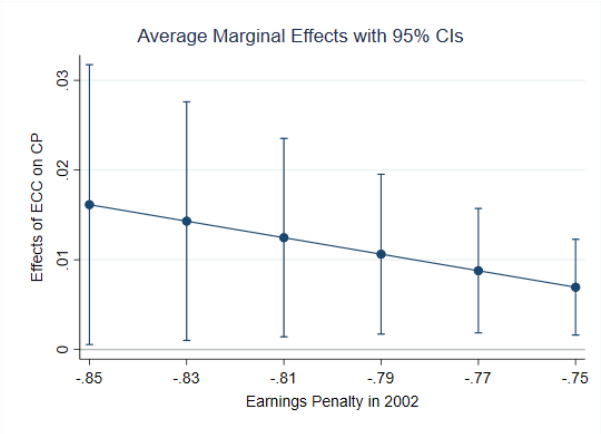
(b) Evidence for Crowding-out Effects



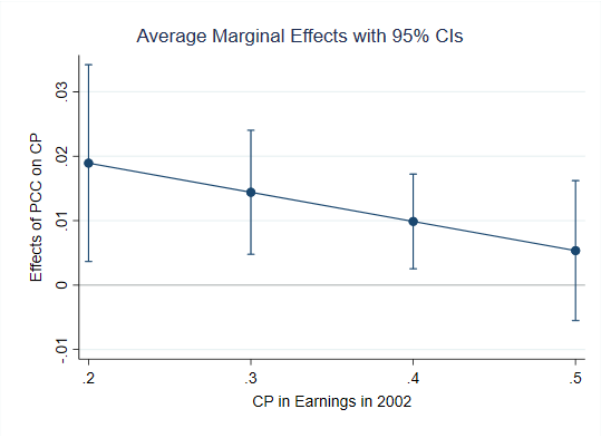
Note. In Panel (a), we plot the effects of early childcare expansion alongside the informal childcare rate for the 10 states in West Germany. We estimate the effects of early childcare expansion using equation (2) separately for each state, grouping the treated counties (and their control pairs) by state. Panel (b) plots the estimated ECC effects against the change in informal childcare usage between the pre- and post-expansion periods. To construct this measure, we calculate the ratio of households reporting the use of childcare provided by relatives, friends (neighbors), or private babysitters among those with a first child under age three during 2001–2004 and 2015–2018 using the SOEP. The x-axis in Panel (a) shows the initial informal childcare rate, while the x-axis in Panel (b) shows the change between the two periods. 95% confidence intervals are presented.

Figure A.13: Heterogeneous Effects of Early Childcare By Initial Child Penalty levels and Reliance on Informal Childcare

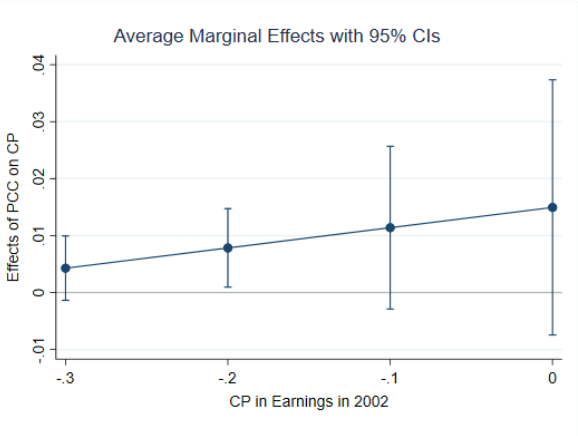
(a) ECC Effects By Initial CP Levels



(b) ECC Effects By Reliance on Informal Childcare



(c) Evidence for Crowding-out Effects



Note. Panel (a) replicates the results from Figure 8 by interacting the initial child penalty levels with the event-time dummies g_2 and g_3 in equation (2), and presents the coefficient on the interaction between g_2 and the initial child penalty levels. Panels (b) and (c) replicate the results from Figure 10 using the same approach.