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RESEARCH ARTICLE

The expansion of early childcare and transitions to first and second birth in Germany

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Abstract

We use quasi-experimental expansion of publicly funded childcare slots for children under the age of three from Germany and exploit regional variations of this large-scale expansion to account for endogenous and selective fertility decisions. To account for left and right censoring, we implement this quasi-experimental framework into the setting of the semiparametric Cox hazard model. By using spatial data on childcare provision at the level of counties and microdata from the German Socio-Economic Panel (SOEP) from 1998 to 2012, we find a significant increase in the transition probability to first birth by 11.9% for native childless couples who were in the labor force before childbearing. With regard to transition to the second birth, however, no significant effect is found from the increase in childcare slots. With a particular focus on the transition to first birth, the effects are demonstrated not to be driven by selective residency choices and internal migration patterns. Furthermore, a large set of robustness checks is applied to show that highly educated mothers react the most, while effects are not attributable to the upper decile of income distribution.

KEYWORDS

Cox hazard model, early childcare, family policy, fertility

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1 | INTRODUCTION

Since the 1970s, low female labor market participation has been observed together with reduced fertility and increasing childlessness in Germany. Childlessness is identified as a major problem due to the need to finance Germany's pension system, which is suffering from increasing longevity and decreasing birth numbers (Dorbritz, 2008; Wood et al., 2016). Driving factors of childlessness in Germany are, alongside increasing participation in labor markets by women across decades, the lack of appropriate instruments of family policy, such as a low level of early childcare, traditional role models regarding the tasks of mothers and fathers, as well as a lack of paternal involvement. Although childlessness is a different concept compared to the transition to the first birth, it is clear that the transition to the first birth can be seen as a hurdle that is hard to clear. A particular focus on first births is thus challenging and of major political interest.¹

Expanding and subsidizing childcare has become a popular instrument for easing work–family conflict, particularly when fertility and female employment are low. While Norway and Sweden began a smooth and long-term expansion of subsidized childcare for children under the age of three at the beginning of the 1970s (Andersson et al., 2004; Kravdal, 1996), Germany expanded childcare for children aged three and above in 1996. Following this trend characterized by increasing childcare possibilities for parents, several policy changes stimulated childcare coverage for children under the age of three. The German government started to expand childcare slots for children under the age of three in 2005. The Day Care Expansion Law (Tagesbetreuungsausbaugesetz, TAG) from 2005 and the Law on Support for Children (Kinderförderungsgesetz, KiföG) from 2008 increased childcare coverage for children under the age of three for West Germany from almost zero to 18.8% in 2012. We study whether those two reforms and the subsequent expansion of early childcare increased birth transitions in Germany and cleared the hurdle of childlessness.

Usually, the identification of the effect of using childcare on fertility patterns suffers from endogeneity. Birth choices and the utilization of childcare are both correlated with characteristics of potential mothers and fathers, for example, with income and education. In addition, the choice of bearing a child and the possibility of using childcare are reverse correlated, whereas the effective direction is not unambiguous. Furthermore, the local supply of childcare slots is endogenous and might be larger in counties where the work–family conflict is more pronounced and where the employment of (potential) mothers is higher. However, the reforms created exogenous heterogeneities across German counties and time, which are uncorrelated with individual characteristics and which allow the problem of endogeneity to be dealt with.

We extend the current state of research and provide several contributions. First, although several papers have already studied the relationship between childcare supply and fertility for Germany, there is no distinct answer to the question of how much of increasing employment of women and fertility can be explained by the expansion of early childcare. We tackle this research gap by focusing on the two laws from 2005 and 2008 and separate the effects of those reforms from others in the field of family policy, such as the parental leave reform of 2007, reforms regarding childcare fees, and home care subsidy. In particular, the parental leave reform of 2007 and the abolition of childcare fees are relevant in our context and have fitted well into the development of a family friendly policy since the mid-2000s in Germany.

¹ This is the reason why we focus on first birth transitions and why we do not consider transitions to third births. However, because the quantity of children is also of interest, we also consider transitions to second births. Moreover, regarding the incidence of third and fourth births, our data source is not optimal in terms of sample size.

Second, we apply a flexible model to estimate the effect of childcare provision on transitions to first and second birth. The Cox hazard model allows us to account for right censoring concerning women's decision to bear a child. Moreover, it allows a parity-specific analysis. Third, by analyzing the individual level, we can follow individuals' residency and demonstrate that analyzing internal migration patterns as reactions of childcare expansion is important. If the childcare reforms stimulate internal migration patterns and if counties with a large expansion in childcare provision attract couples with high preferences for fertility, our estimation results would be biased. Fourth, the theory is ambiguous regarding the heterogeneous effects in the relationship between childcare provision and fertility in terms of wages. A higher wage for women increases the opportunity costs of having a child, while the relative price of (external) childcare becomes lower (Ermisch, 1989; Hazan & Zoabi, 2015). We examine heterogeneity in terms of qualifications, earnings, and occupations, something which is not possible with aggregated data at the level of counties.

By using spatial data on childcare provision at the level of counties and microdata from the German Socio-Economic Panel (SOEP) from 1998 to 2012, our difference-in-differences (DiD) estimates reflect a marginal increase in first birth probability by 11.9% for the target group, that is, couples with German citizenship where both parents were employed before childbearing.² No significant effect on second births is found from the expansion of early childcare. We conducted a large battery of robustness checks, the most important of which as part of our main contribution is the examination of selective migration patterns. We demonstrate that internal migration patterns do not drive our effects and that the transition probability of the first birth is also significantly increased for couples who never moved during the observation period. This result legitimates the examination of heterogeneities where we can conclude that effects are mainly driven by highly educated women, and that the effects are not attributable to the upper decile of the income distribution. Further robustness checks illustrate that other policy reforms do not drive the results.

The structure of the paper is as follows. The next section presents lessons from previous empirical research. Section 3 outlines the development of fertility in Germany and describes the system of subsidized childcare in Germany and the reforms of interest in more detail. After the justification of our identification strategy and the presentation of the applied data set in Section 4, Sections 5 and 6 present the main empirical results and robustness checks, followed by our conclusion in Section 7.

2 | PREVIOUS RESEARCH

Theoretical groundwork predicts that the effect of childcare provision on the transition probability to first and second birth is ambiguous, but the positive income effect outperforms the negative substitution effect if the price of childcare is low (for a more detailed description of the theoretical model, see DiPrete et al., 2003; Bauernschuster et al., 2016). To understand this, first note that the opportunity costs of giving birth consist of the loss of wages and the depreciation of human capital during the period outside the labor market. Considering the transition to the first and second birth, larger availability of subsidized childcare allows the mother to return to the labor market earlier,

² We define this group as the target group, although this group does not coincide with the group of eligibility regarding the Law on Support for Children from 2008. However, the two characteristics (citizenship and employment) characterize the group that is the likeliest affected by the reforms (for a discussion of the external validity of our results, see Section 7).

which increases the reentry wage after giving birth.³ By assuming that the quantity of children is a normal good, a higher income from higher reentry wages implies that the demand for children increases (Becker, 1960; Becker & Lewis, 1973). This positive income effect makes the transition to the first and second birth more likely. However, the increased reentry wage also produces a substitution effect, which suggests that the decrease in working hours increases opportunity costs affecting the higher-order births (DiPrete et al., 2003).⁴

When estimating the causal effect of childcare provision on fertility, endogeneity is the major issue. The decision to bear a child is endogenous and correlated with characteristics such as income and education. Furthermore, the supply of childcare is larger where the work–family conflict is more pronounced and where the employment rate of (potential) mothers is larger. Initial empirical studies from Europe estimate correlations between subsidized childcare coverage and birth probability, which result in inconclusive findings.⁵ The reasons for the inconclusiveness of previous studies are economic factors and methodological issues.

First, economic factors are numerous and show why previous studies are hard to compare. Next to the initial level of fertility and general labor market attachment of women, the existence of a private childcare market (Bauernschuster et al., 2016), the initial level of childcare and the extent of the expansion of childcare triggered by the respective policy reform affect the relationship between subsidized childcare and birth probability. Moreover, differences in other family-related policies affect the link between childcare availability and fertility, such as the system of parental leave and childcare fees (see Section 6.3). Last but not least, different studies are hard to compare because they differ by focusing on various age groups of children.

Second, previous papers treat methodological issues differently. Rindfuss et al. (2007, 2010) tackle most of these econometric challenges. By having access to full administrative data, they follow birth cohorts from the beginning at the age of 15 until the end of their childbearing years and allow the error term to be correlated between different birth cohorts. Their Norwegian setting looks at a smooth and long-term expansion over decades of subsidized childcare. This approach leaves time and space for endogenous and internal migration reactions. Couples with a high preference for bearing a child may anticipate the reform and choose their residency with respect to the childcare supply across counties (see Section 6.1). Furthermore, as in Ronsen (2004) and Lappegard (2010), the institutional environment (for instance, the system of parental leave) in Rindfuss et al. (2007, 2010) has been shaped several times during their observation period. The consideration of the German case handles these two methodological issues. First, the system of family policy remained relatively constant during the observation period (see Section 6.3). Second, the

³ Throughout the paper, we use the term extensive margin of fertility to describe the entry into motherhood, while the term intensive margin of fertility refers to the number of children or the probability of having higher-order births.

⁴ Based on Becker (1960), recent research asks whether greater childcare provision affects the quality of the first child, rather than making the birth of a second more likely (Aaronson et al., 2014; Bar et al., 2018). Theoretically, it is assumed that children need a minimum of their mothers' time. According to this argument, the increase in childcare provision affects the quality of the first child, something which could also affect the likelihood of having a second child. In this context, Baker et al. (2008) find negative health effects of external childcare, showing an effect of childcare use on child quality. Due to our focus on the effects on the transition to the first birth and our empirical method, the effect of childcare on the quality of children is not considered further. For an overview of previous research on the impact of childcare on children's development, see Carta and Rizzica (2018).

⁵ Del Boca (2002) and DiPrete et al. (2003) find a positive link. On the contrary, applications of discrete-time hazard models with a logistic modeling of birth probability from Scandinavia (Andersson et al., 2004; Lappegard, 2010) and Germany (Hank & Kreyenfeld, 2003; Hank et al., 2004) do not find significant effects. However, after controlling for full-time care instead of care availability, Hank et al. (2004) find some indications of a positive relationship.

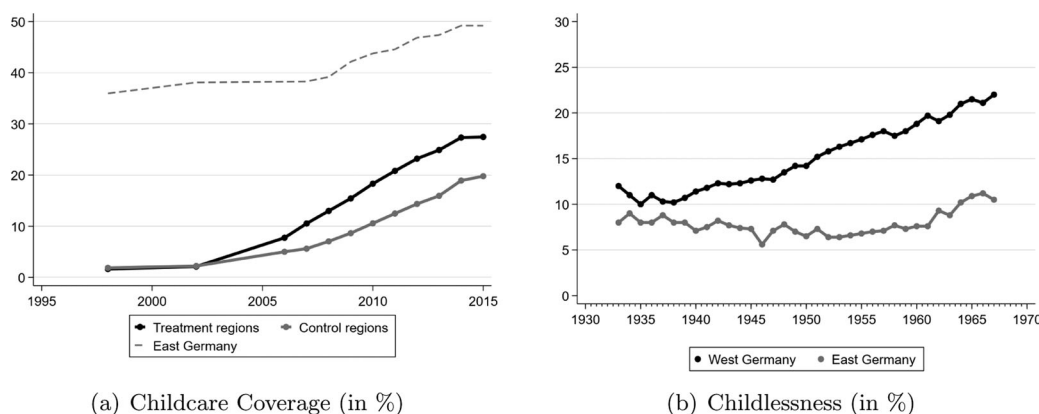


FIGURE 1 Subsidized childcare and childlessness across time

Note: Panel (a) shows childcare coverage for children under the age of three in West Germany, separated by treatment status. Treatment regions are counties in West Germany that experienced an increase in childcare coverage from 2002 to 2010 (measured in percentage points) that is at least as high as the median increase. In addition, coverage is displayed for East Germany (formerly the German Democratic Republic, GDR). Based on the German Microcensus (Federal Statistical Office, 2017), Panel (b) presents the share of childless women by birth cohorts separated for West and East Germany. Note that the parity of births is only recorded within present marriages, so that the number of children born to unmarried women and the presented numbers on childlessness are based on estimations (Dorbritz, 2008).

Source: Statistical Offices of the German Federal States; Federal Statistical Office (2017).

level of childcare coverage has been triggered significantly within only a few years, which leaves less space for endogenous and internal migration reactions (see next section and Figure 1).

There are three studies in existence that examine the same expansion in subsidized childcare for children under the age of three in Germany, which we analyze (for more details on the institutional setting, see the next section). Bick (2016) simultaneously models labor market participation and preference for birth and childcare within a life cycle model and calibrates this with SOEP data from the 1984–2005. While Bick (2016) does not find any indications of increased fertility, Bauernschuster et al. (2016) estimate that a 10 percentage point increase in childcare coverage increases the number of births per 1000 women by 1.2 (2.8 percentage). By applying a DiD model with spatial data at the level of counties from 1998 to 2009, they show that the effects are larger at the intensive margin than at the extensive margin.

The third paper emphasizes the importance of studying heterogeneities. Haan and Wrohlich (2011) model intertemporal interactions between employment and birth decisions in a dynamic structural panel model with SOEP data from 2000 to 2007 and understand both decisions as being nonseparable. Although they do not find a significant effect for the full sample, two groups benefit from the simulated introduction of a legal claim to a subsidized childcare slot for the working population and from an implicit increase in subsidized childcare to full coverage. Conditional on employment, highly educated women increase their birth probability by 7% and childless women by 9%.

In summary, it should be underlined that so far there is only one study that uses quasi-experimental data (Bauernschuster et al., 2016). Moreover, the results of the two papers that calculate simulations (Haan & Wrohlich, 2011; Bick, 2016) differ from the results of that study. The fact that those three papers come to inconclusive results in overall terms legitimates our study that uses individual level data and exploits the quasi-experimental character of the

institutional setting. We develop an analysis with quasi-experimental data at the individual level and apply a flexible duration model that makes it possible to take right censoring into account when women decide to have a child. This makes it possible to address certain problems that cannot be solved by an analysis at the aggregated level of counties. First, we can track individual residence in our data to examine whether counties with high childcare coverage attract people with a high desire for children and childcare. The extent to which childcare reforms stimulate internal migration patterns is important in determining the extent to which our estimates are undistorted. So far, no paper has addressed the topic of internal migration in the context of childcare and fertility decisions.

Furthermore, we analyze heterogeneous effects in terms of qualifications, earnings, and occupations, something which is not possible with aggregated data at the county level. This is important because the question which socioeconomic groups react to the reform is important when designing family policy instruments.

3 | INSTITUTIONAL BACKGROUND

In this section, we illustrate the development of birth patterns in Germany and describe the childcare reforms in Germany. This should help to identify important features of the institutional system and empirical issues that should be taken into account in the econometric method.

As a country characterized by a high level of childlessness at the beginning of our observation period in 1998 and a low level of female labor participation, Germany is of particular interest. Fertility patterns in Germany are affected by many things. The increasing female participation in labor markets and the educational expansion across the last decades, the lack of appropriate instruments of family policy, traditional role models regarding the tasks of mothers and fathers, and the lack of paternal involvement are possible determinants of fertility patterns.⁶ In Panel (b) of Figure 1, completed childlessness in West Germany increased from 12.0% for women born in 1933 to 22.0% for women born in 1967. Considering women with an academic degree, the share of childless women of the cohort of 1967 is larger by 7 percentage points compared to women of this cohort with nonacademic degrees (Federal Statistical office 2017; pp. 18–19). Furthermore, the level of early childcare coverage is fairly low in West Germany (Panel a). In 2002, only 2.2% of children under the age of three is minded in subsidized childcare facilities.⁷ On the contrary, in East Germany, childlessness is lower and fertility is higher, while childcare coverage already exceeded 30% before the year 2000. Figure 1 thus raises the question of whether the apparent correlation between childcare coverage and childlessness is of a causal nature.

To counteract the trend of increasing childlessness, the legal claim to a slot for all children from their third birthday until school entry was introduced as a first step in 1996 (Child and Adolescent Support Law; Kinder- und Jugendhilfegesetz, KJHG). This law pushed the kindergarten

⁶ In this context, research on the economic determinants of childlessness is relevant (e.g., Gobbi, 2013). In particular, recent research focuses on the causal effect of education on childlessness (e.g., Aaronson et al., 2014; Amin & Behrmann, 2014; Baudin et al., 2015). Kreyenfeld and Konietzka (2017) provide a nice overview on trends in childlessness across European countries and its economic determinants. However, a closer consideration of this literature strand would result in less focus on our leading research question.

⁷ In detail, Panel (a) shows childcare coverage for children under the age of three in West Germany, separated by treatment status. Treatment regions are counties in West Germany that experienced an increase in childcare coverage from 2002 to 2010 (measured in percentage points) that is at least as high as the median increase (for more details, see Section 4.1).

attendance rate from 33% in the year 1991 to 55% in 2003 for children of the age of three (Bauernschuster & Schlotter, 2015). Although we fully focus on childcare for children under the age of three, this first law considering childcare for older children is the starting point of a new era of family policy.

Subsidized childcare for children under the age of three has long been exclusively available in East Germany (formerly the German Democratic Republic, GDR). However, since the mid-2000s, several bills have also promoted its expansion in West Germany. The Day Care Expansion Law enacted in 2005, aimed to provide a 17% coverage for early childcare in West Germany by 2010 (Bauernschuster et al., 2016). In response, a common conference by the German Government, federal states, counties, and towns scaled up this target in 2007 to 35% which should be reached by 2013 (Bauernschuster et al., 2016). The conference also agreed on distributing investments, construction, and operating costs for this expansion between the different administrative authorities (Bauernschuster et al., 2016). As a third step in 2008, the Law on Support for Children has promised to promote childcare for children aged one and above to single parents; or, if both parents are employed or parents are in education, on job search or obtain unemployment assistance or if a child needs childcare due to developmental reasons. In 2013, parents received the legal claim to a childcare slot for children aged one or two independent of their socioeconomic or employment status.

Although this explicit formulation of the eligibility groups came into force in 2008, the prioritization of groups with the most urgent need for childcare has already been implicitly practiced before. Before 2008, childcare providers allocated slots based on the first-come-first-served concept with exclusions for employed and single parents (Bien et al., 2006; Felfe & Lalive, 2012, 2018).⁸

In the following, the expansion of subsidized childcare for children under the age of three from 2005 until 2012 is our subject of research. We thus evaluate the accumulated effect of the Day Care Expansion Law from 2005 and of the Law on Support for Children from 2008. We restrict the observation period until 2012 because too long an observation period leaves space for internal migration reactions and time for adjustment to shaping institutional environment. Moreover, by doing this we ensure that the home care subsidy introduced in 2013 does not affect our estimates. This reform introduced a monthly subsidy of 100–150 euros for families who do not use subsidized childcare (for greater details and evaluations of this reform, see Fendel & Jochimsen, 2017; Collischon et al., 2020).

Although general reforms regarding the German labor market are also likely to affect the labor supply of women and their fertility, such as the Hartz reforms, the consideration of those reforms is beyond the scope of this paper because we want to focus on the field of family policy in the following. Nevertheless, in Figure 2 female employment and unemployment rate do not evolve in a different way by treatment status after the Hartz reforms were implemented (2003–2005).

⁸ Note that the description of how slots are assigned is simplified in order to explain the general system in a comprehensible way. The assignment of slots is not homogeneously handled by counties and states (see Boll & Lagemann, 2019; Müller & Wrohlich, 2020). There are differences by counties, districts, and by childcare providers. However, such differences are unobservable in the data we use. Formal childcare by nannies and informal private childcare (e.g., by relatives, acquaintances, and friends) were used by parents in Germany even before the reforms starting in 2005. For instance, theory by Bar et al. (2018) predicts that women increase their birth probability when wages relative to the price of time a mother spends with her child increase. It is thus expected that in particular, women in top income deciles demand private childcare even if there is no subsidized provider of childcare. Although the SOEP provides information about the utilization of such private childcare, this is information that is realized after the measurement of our outcome fertility. To avoid this new issue of reversed causality, we decide not to use this information.

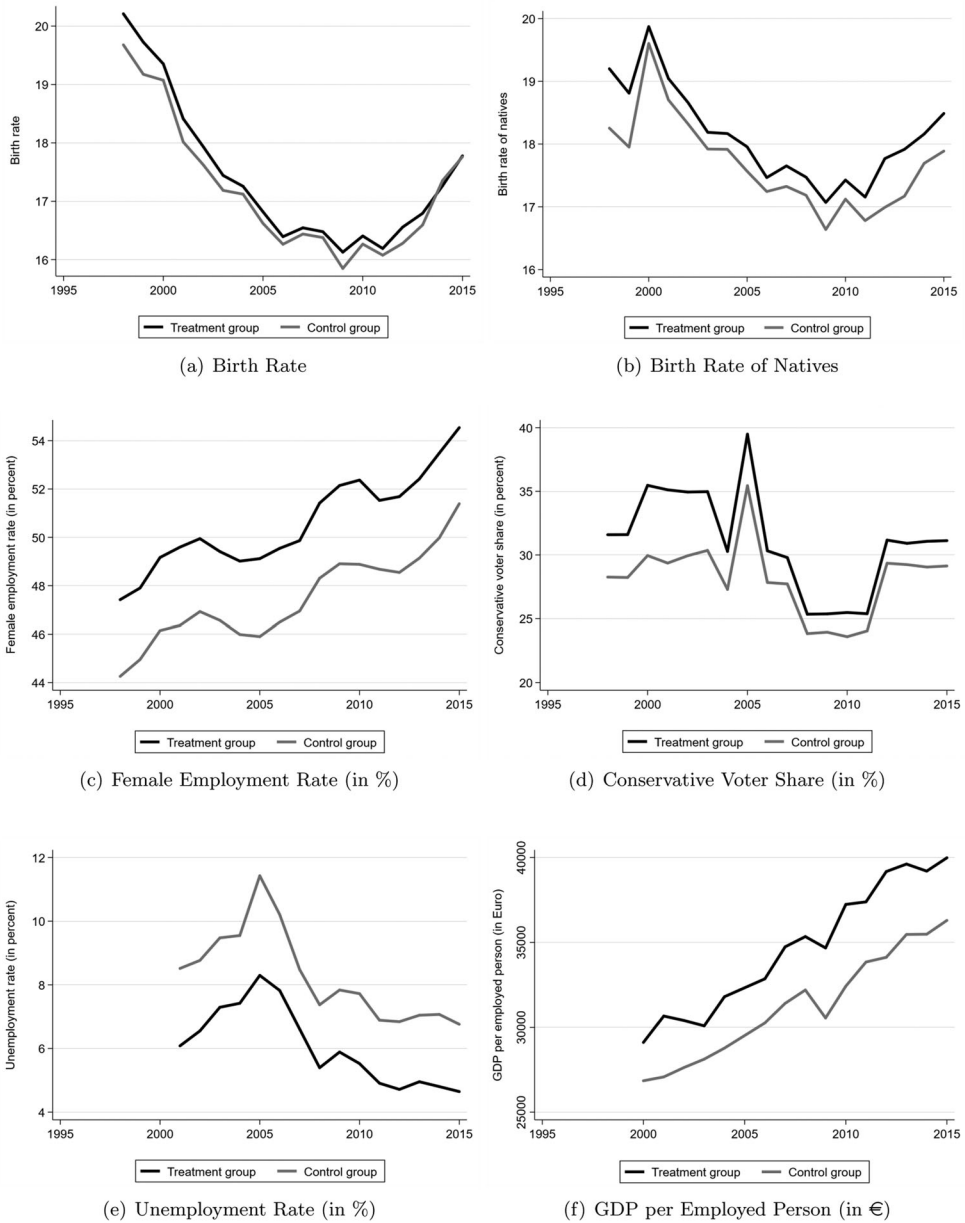


FIGURE 2 Common trends in fertility and spatial characteristics by expansion speed

Note: This figure distinguishes between treatment counties with above average expansion of childcare coverage (of at least 11.3pp) and control counties with below-average growth. The two upper graphs display fertility measured at the level of counties as birth rates and birth rates of natives. Birth rates are, respectively, approximated by the number of births per 1000 women. The second row displays trends in female employment and conservatism over time, while the latter is approximated by the conservative vote share at the last parliamentary election. These two variables are included as control variables in our analyses. The two lower graphs show two indicators of the general economic development in counties, namely, the unemployment rate and GDP per employed person. Because those variables are captured only since 2002 and 2000, they are omitted from duration analyses. However, robustness checks, not presented in this paper, demonstrate that including those two spatial variables does not affect our results.

Source: Statistical Offices of the German Federal States; Statistical Office of the Federal Employment Agency.

Furthermore, we cannot think of any economic reasons why the Hartz reforms should have affected the treatment and control group in a different way.⁹

The expansion of subsidized childcare for children under the age of three in Germany provides several advantages compared to previous research. First, in contrast to the smooth expansions over long time horizons in Scandinavia (e.g., Kravdal, 1996; Andersson et al., 2004; Ronsen, 2004; Rindfuss et al., 2007, 2010), this expansion in Germany was a quasi-experimental expansion of childcare over a very short period. This setting left little time for endogenous residency choices and selective migration reactions that may distort the causal effect of childcare on birth transitions. Moreover, the expansion starting in 2005 is huge and increased coverage from virtually nonexistent before 2005 (Bauernschuster et al., 2016) to 18.8 slots per 100 children in 2012. Second, by having a focus on childcare for children under the age of three, a shorter time distance between the decision to bear a child and the decision to demand external childcare is more likely to affect birth transitions. Third, most quality standards are officially specified by the federal states, such as minimum caring time, group sizes, and staff–child ratios (Felfe and Lalive, 2012; BMFSFJ, 2016; Busse & Gathmann, 2018; Sander et al., 2021). This creates heterogeneities between federal states. For instance, Nentwig-Gesemann and Hurmaci (2020) and Sander et al. (2021) underline that regional heterogeneities exist regarding activities during minding, hours in childcare, and fees. Research by Stahl et al. (2018) show that accounting for the quality dimension in childcare is important regarding children’s development. In our paper, we purely focus on the effect of the supplied quantity of publicly funded childcare.

Fourth, the institutional environment and other instruments of family policy have been relatively constant during our observation period. Two exceptions are the parental leave reform in 2007 and the abolition of childcare fees in some federal states. These reforms are described and tackled in our robustness checks in greater detail.

The literature overview and the detailed examination of the institutional setting emphasize the need to conduct an analysis at the individual level to examine internal migration patterns and heterogeneous effects. For instance, it is expected that highly educated women benefit the most from subsidized childcare because their opportunity costs are the highest (Haan & Wrohlich, 2011). While the institutional setting clarified why the effects on the transition to the first birth (extensive margin) are of particular political interest, it also highlights that parents who were in the labor force before childbearing are mainly prioritized. Moreover, evidence by Jessen et al. (2020) demonstrates that natives are more likely to use external childcare than migrants. Although parents who were employed before having a child and have German citizenship are only a subgroup of the eligibility group defined by the Law on Support for Children, we focus our research in the Section 5.2 on this group (so-called target group in the following).

4 | EMPIRICAL STRATEGY AND DATA

4.1 | Identification strategy and econometric methodology

After the description of the institutional background has revealed important facts that should be considered in the empirical analysis, this section describes the method applied. In particular,

⁹ The Hartz reforms were implemented between 2003 and 2005 and display a bulk of different reforms, for example, the system of the unemployment assistance was reformed and active labor market programs were reorganized. For analyses of those reforms, see Gianneli et al. (2016) and Woodcock (2020).

this section explains why and to what extent the expansion of early childcare can be considered exogenous in terms of individual birth choices.

The semiparametric Cox hazard model (Cox, 1972) is a reliable method to evaluate programs of social policy regarding the transition to births, birth spacing (Ronsen, 2004; Gutierrez-Domenech, 2008; Todd et al., 2012), and return to work after having a child (Kunze & Troske, 2012; Bergemann & Riphahn, 2017). It also tackles econometric issues pronounced by prior research and is highly suitable for the underlying institutional setting. Left censoring describes the problem that before the first survey of persons, the decision of bearing a child has begun or even a first child has been born. This may be unobservable in our data. Right censoring means that women have not reached the end of their reproductive age at the end of the observation period (Todd et al., 2012), so that childbearing can still occur after the last wave the women participated in the survey. The Cox hazard model considers the problem of right censoring by estimating partial likelihoods. Next to individuals that experience an event, observations without a birth are nevertheless part of the calculation of the risk set and of the underlying partial likelihood. The issue of left censoring is handled by considering women who have been part of the SOEP at the age of 21, which is our definition of the beginning of childbearing years (see Section 4.2)

The second challenge is emphasized by an important strand of research that is dedicated to the intrinsic link between women's age and reproductivity. The Cox model allows ordering observations concerning the age of potential mothers or the space of time between births instead of ordering information merely regarding survey years. Note that there is disagreement on the question regarding which cumulative distribution function correctly models the relationship between waiting time and birth risk (see, e.g., Heckman & Walker, 1990; Hoem et al., 2001; Vikat, 2004). The Cox hazard model, however, does not assume a specific form of time dependence, captured by the baseline hazard $h_0(t)$ and does not impose any assumption on the relationship between women's age as the waiting time t and birth probability.¹⁰

The third feature emphasizes the requirement of a parity-specific analysis and illustrates that the decision process of bearing a child differs heavily according to the order of the birth. Heckman and Walker (1990), Kravdal (1996), and Santos Silva and Covas (2000) demonstrate that couples without children and couples with children are differently decomposed regarding socioeconomic features and experience in combining employment and parenthood.

$$h_i(t) = h_0(t) \exp(\beta_1 Treated_{it} + \beta_2 Post_{it} + \beta_3 Treated_{it} \cdot Post_{it} + X'_{it} \beta_4 + R'_{jt} \beta_5). \quad (1)$$

To identify the effect of childcare expansion on transitions to first and second birth, a first approach is to implement a DiD estimator into the setting of the Cox hazard model in Equation (1). The hazard ratio $h_i(t)$ gives the probability that woman i bears a child at age t , if no birth has occurred until this point in time. Because childbearing is understood as a rational decision, and in order to leave time space for pregnancy and adaption to the reform, our preferred specification is to model births in $t + 1$. The binary treatment status $Treated_{it}$ equals one if woman i lives in a county with an above-average increase in early childcare coverage during the central expansion period from 2002 till 2010.¹¹ Following Havnes and Mogstad (2011) and Felfe and Lalive (2018), we

¹⁰ The Cox hazard model assumes proportionality. This proportionality involves the effect strength of one covariate having to be constant over the entire process (e.g., Friedman, 1982). However, the Cox model allows for modeling nonproportional effects by interacting the respective variable with the waiting time.

¹¹ The definition of the treatment is chosen in consistence to Bauernschuster et al. (2016). The period 2002–2010 covers the central expansion period, in which the majority of childcare slots is planned and opened. Following the identification

measure growth in childcare coverage in percentage points, whereas the median increase of 11.3 percentage points separates treatment and control individuals. The binary variable $Post_{it}$ refers to periods after the enactment of the Day Care Expansion Law in 2005 (see Bauernschuster et al., 2016). Thus, the parameter of interest β_3 estimates the causal effect of the childcare expansion on birth probabilities, while it is controlled on a large set of socioeconomic characteristics X'_{it} and regional characteristics of counties R'_{it} presented in the subsequent section.¹² This effect is an intend-to-treat (ITT) effect because the desire of childbearing before actual childbearing is not surveyed and because actual utilization of childcare is only observable after childbearing and thus after the measurement of our outcome.

$$h_i(t) = h_0(t) \exp(\delta_1 Coverage_{it} + \delta_2 Post_{it} + \delta_3 Coverage_{it} \cdot Post_{it} + X'_{it}\delta_4 + R'_{it}\delta_5). \quad (2)$$

In an alternative formulation, we replace the binary treatment status indicator $Treated_{it}$ with continuous childcare coverage $Coverage_{it}$ in Equation (2). Although Equation (1) seems to be characterized by information loss and an arbitrarily defined cutoff that separates the treatment group from the control group (see also Bauernschuster et al., 2016), we prefer this model because it is robust to missing values in coverage between 2002 and 2010 and allows the examination of how specific fertility differentials between treatment regions and control regions evolve over time.

To establish a causal interpretation of β_3 and δ_3 , we assume that the expansion of childcare coverage is unrelated to birth preferences and further individual characteristics, and exploit temporal and spatial heterogeneities in expanding childcare coverage across counties. To justify this assumption, we rely on prior expositions made by Felfe and Lalive (2012, 2018) and Bauernschuster et al. (2016). One fact that justifies this assumption is the process complexity of opening new facilities that requires cooperation between federal states, counties, and childcare facilities. While local administrations predict the demand for childcare concerning local characteristics, such as birth rate and female employment rate, nonprofit organizations propose opening new facilities. However, in the end, the federal state and the local administration are both responsible for evaluating these proposals and allocating subsidies so that it remains uncertain until the end whether a childcare provider receives funds and permission to open a new facility. Next to this precarious situation for the provider, spatial heterogeneities in the supply of childcare slots arise from non-predictable supply shocks and factors that are exogenously distributed, such as knowledge about the funding system, scarcity of qualified staff, and constraints in the caring space.

These facts establish the exogeneity of childcare expansion and explain the large variation in expansion within West German federal states displayed by the summary statistics in Table A.1. Simultaneously, it is important to capture differences at the level of federal states in the empirical analysis. Evidence provided by Felfe and Lalive (2012), Riedel et al. (2005), and Huesken (2010) illustrates that having the mandate of setting most of the quality standards in federal states' responsibility results in a large variation between federal states in opening hours, child-staff ratio, and other indicators of quality and in childcare coverage.

strategy, the future coverage in childcare could not been forecast in the first few years after 2005, so that the childcare supply is most likely to be exogenous regarding individual residency choices in this period. However, we extend the entire analysis period to 1998–2012 to extend our sample size and allow the examination of heterogeneous effects. Note that if we restrict our entire analysis to the period 2002–2010 the results do not change significantly. This result is available from the authors upon request.

¹² Note that the estimations do not change the magnitude and the significance of our results if we additionally use dummies for counties. Because we control for a large set of regional characteristics of counties (see Section 4.2), we already take account of heterogeneities between counties.

One issue for the empirical analysis may be if the childcare expansion has crowded out public funds of other instruments of family policy. Following remarks by Bauernschuster et al. (2016) and Cornelissen et al. (2018), this presumption can be excluded. Income taxes and social and unemployment benefits are regulated at the federal level, so that they do not depend on local government finances. In addition, funds allocated to child benefits and child allowances were unaffected by the reform.

In summary, the exposition of our identification strategy demonstrates that spatial heterogeneities in the expansion of subsidized childcare for children under the age of three result from randomly distributed factors and that the treatment status can therefore be seen as exogenous to individual birth decisions. Nevertheless, the institutional background and our remarks on the identification strategy expose several items that need to be regarded in the empirical analysis. First, the expansion of childcare slots is expected to be larger in circumstances where birth rates and labor market attachment of women are also increased. This endogeneity is harmless as long as those differences between treatment and control regions are time-invariant, and providing the common trend assumption holds. Second, we need to examine whether the Day Care Expansion Law and the Law on Support for Children triggered internal migration behavior and stimulated parents with a high desire to bear a child to move to counties with increased supply in early childcare.

4.2 | Data and descriptive statistics

In view of the empirical results presented in the following sections, this section describes the data set and provides first indications of the internal validity of our empirical strategy.

To study the impact of childcare expansion since 2005 on transitions to first and second births in West Germany, representative survey data from the SOEP is linked with spatial data from the Statistical Offices of the German Federal States. The SOEP has surveyed households on a large set of socioeconomic characteristics every year since 1984 (Wagner et al., 2008). Because home care subsidy was introduced in 2013 in Germany, something which presumably also affects women's birth patterns and labor market decisions, we use waves from 1998 until 2012. Choosing the correct time frame is demanding. A sufficient long-time horizon is important for treatment couples to adjust their complex birth decisions to the policy reform (Todd et al., 2012), while too long a treatment period leaves space for endogenous migration reactions. The choice of studying transitions to births during 1998–2012 accounts for this trade-off.¹³

The most important advantage of using this data set is the precise identification of biological mother–child couples with information about the year and the month of birth. We only consider women under the age of 45 who are in an ongoing relationship (marriage or cohabitation) and with full socioeconomic information about their partners living in the same household.¹⁴ To account for left censoring, we only consider women who have been part of the SOEP at the age of

¹³ Note, furthermore, that the transition year 2005 is excluded from the empirical analysis.

¹⁴ The age of 45 is a broad definition of the end of childbearing years. Although some papers define this age as 36 (e.g., see Rindfuss et al., 2010), we choose this broad definition in order to construct a sample with sufficient observations. By considering women in an ongoing relationship, we follow previous papers that examine the impact of subsidized childcare on birth transitions (see Haan & Wrohlich, 2011; Bick, 2016). Although the choice of relationship is endogenous and fertility patterns of solitary persons are a relevant research gap, we take this restriction to facilitate the comparison of our estimates with previous studies.

TABLE 1 Descriptive statistics by parity

	First birth		Second birth	
	Mean	SD	Mean	SD
Panel A: Births				
Births	396		219	
Average waiting time at birth	27.6		29.3	
Panel B: Women's socioeconomic characteristics				
Age	28.2	5.6	30.1	5.9
German citizenship (in %)	87.8	32.8	81.3	39.0
Foreign-born (in %)	11.7	32.2	19.9	39.9
Non-EU (in %)	8.9	28.5	14.9	35.6
Low education, ISCED 1-2 (in %)	17.4	37.9	22.1	41.5
Middle education, ISCED 3-4 (in %)	57.6	49.4	57.2	49.5
High education, ISCED 5-6 (in %)	20.0	40.0	17.0	37.5
Schooling years	12.3	2.6	11.7	2.5
Equivalent household labor income (in €)	29,368.5	16,800.7	25,515.6	16,288.5
Panel C: Partners' socioeconomic characteristics				
Age	31.8	6.6	33.7	6.5
Migration background (in %)	15.3	36.0	23.5	42.4
Low education, ISCED 1-2 (in %)	9.4	29.1	11.7	32.1
Middle education, ISCED 3-4 (in %)	52.5	49.9	57.1	49.5
High education, ISCED 5-6 (in %)	27.7	44.8	24.2	42.8
Schooling years	12.2	2.5	11.8	2.3
Observations	2895		3619	
Individuals	833		651	
Childless women (in %)	52.5			

Note: This table reports the number of first and second births and respective average waiting time. This is the age of mothers at childbearing. Moreover, the sample composition concerning socioeconomic characteristics of women and their partners is presented. Note that the sample of first births exclusively consists of couples who stayed childless until the beginning of the observation period, whereas couples in the sample of second births have already born a first child.

Source: SOEP; own illustration.

21, which is our definition of the beginning of childbearing years. Because this definition is quite permissive, robustness checks in Section 6.2 reduce this age definition and show that estimates are unchanged.

Table 1 provides an overview of the outcome and socioeconomic variables by parity. While in the sample of first birth we consider women who have been childless from the beginning of the observation period in 1998, the sample of second births exclusively includes women who are at risk of bearing a second child. In all, we identify 396 first births and 219 second births, which result in a share of childlessness of 52.5 in the entire sample in 2012. Furthermore, Table 1 provides information about socioeconomic and educational variables of potential mothers and their partners. A comparison of the two samples hints that women in the sample of first births are more likely to be highly educated. Furthermore, the proportion of migrants is larger in the sample of second births. This result confirms the expected higher fertility level of migrants at the aggregated level (e.g., see Stichnoth & Yeter, 2016). Alongside the influence of cultural attitudes on birth decisions

(Mayer & Riphahn, 2000; Borck, 2014), this observation legitimates a separate analysis for natives and migrants. A separate analysis accounts for the large heterogeneity in the group of migrants concerning birth preferences and the labor market attachment of women (Blau et al., 2011; Stichnoth & Yeter, 2016) and the information channel, whereby language skills are an important factor affecting migrant utilization of childcare (Jessen et al., 2020). Since obtaining German citizenship reflects the assimilation of values of the country of residence, we use citizenship instead of country of birth in order to define the group of migrants.

Next to household labor income, we control for prebirth employment status in the empirical analysis and differentiate between nonemployment, part-time, and full-time jobs. Figure A.1 illustrates that while a share of about 58.6% is employed part-time 1 year before bearing the first child, and only approximately 14.4% is employed full time, this labor market participation is reduced largely due to birth. Simultaneously, data illustrate that partners attempt to compensate for the reduced labor market participation by significantly increasing their full-time employment share.

With detailed information about the individual residency at the level of counties, the final sample is merged with administrative data on childcare coverage from the Statistical Offices of the German Federal States. Note that the merging of individual level data and county level data generates a final sample that includes 84% of all counties in West Germany. This final sample is not selective. Instead, the distribution of childcare coverage is relatively similar between the raw county level data and the final sample. Relevant distribution parameters such as the mean and the respective quartiles thus do not differ by more than 0.5 percentage points between the county data set and the final sample.

Panel (a) of Figure 1 displays statistics on childcare coverage available for the years 1998, 2002, and 2006–2015.¹⁵ Lessons from the section describing the institutional background emphasize the importance of capturing further spatial variables that characterize the demand for childcare and the economic structure of counties. This procedure also enables the examination of the validity of the common trend assumption. We therefore add spatial information regarding birth rates (number of births per 1000 women), female employment rates, female share of obtaining the university entrance qualification, unemployment rate, GDP per employed person, the share of the foreign population, and the population density as an indicator of urbanity (Felfe and Lalive, 2012) provided by the Regional Database Germany of the Federal Statistical Office and the German Federal Employment Agency. Furthermore, the conservative vote share at the last parliamentary election is included to capture difference in the resistance to early childcare that is larger in more traditional regions (Felfe and Lalive, 2018; Bauernschuster et al., 2016).

Comparison of level differences in spatial characteristics is displayed in Figure 2, and this confirms the validity of the common trend assumption, which is required in the DiD approach (see Section 4.2). In the treatment group with an above-average growth of childcare coverage, birth rates and female employment rates are significantly larger and economic conditions approximated by unemployment rate and GDP per employed person are better compared to the control group with below-average growth in coverage. Birth rates and economic conditions follow very similar

¹⁵ The number of observations is larger by using Equation (1) instead of by modeling continuous childcare coverage. Moreover, in 1998 and 2002, only the number of slots for children under the age of three is reported. By assuming an excess demand for childcare for this age group, a supposition which is also supported by empirical evidence by Wrohlich (2008) and is consistent with Bauernschuster et al. (2016), we construct childcare coverage and calculate the ratio of available slots under the age of three to the number of children of the same age group for 1998 and 2002. Wrohlich (2008) uses SOEP data from 2002 to illustrate that more than 50% of children younger than four are queuing for childcare slots, whereas only 10% of children older than three are queuing.

trends, whereas the gap in the birth rate of natives seems to widen after 2010. However, this merely reflects the treatment effect on fertility. In the period 2000–2005, the German economy experienced a recession characterized by high unemployment of 11.7% in 2005 (Federal Employment Agency, 2014). If this recession met the treatment group and individuals in the control group in a different way, β_3 would be biased. However, the gap in employment rate (Panel c) and unemployment (Panel e) in Figure 2 seems to be fairly constant. One exception with regard to the common trend assumption is the share of conservative voters as the gap between treatment and control regions is reduced after 2005. However, it is more reasonable to relate this cut to the regime switch in 2005 from the left-wing government to a conservative regime consisting of the Christian Democratic Union (CDU) and the Free Democratic Party (FDP) than to childcare policy. Thus, Figure 2 supports our empirical strategy and confirms the validity of the common trend assumption.

Alongside the testing of the common trend assumption, Figure 2 illustrates that fertility is also driven by the economic situation of regions (see Schmitt, 2012; Pollmann-Schult et al., 2015; Bauernschuster et al., 2016). It appears that aggregated fertility is correlated with education, employment, and economic conditions in the region. This underlines the importance of controlling for regional characteristics in our estimations and identifying residency choices using individual-level data.

5 | EMPIRICAL RESULTS

5.1 | Baseline results

In this section, we provide evidence regarding the effects of childcare expansion on individual transitions to births based on several versions of Equation (1), whereas the women's age is chosen as the waiting time concept. Throughout the paper, we apply the Breslow's method to correct for ties (for further details, see Cleves et al., 2016). Both link and Schoenfeld tests do not provide any indication that the assumption of proportionality would be violated. To estimate the effect of the expansion of childcare on the probability of first birth in t , we gradually add covariates regarding women and their partners and spatial controls.

With a focus on the full sample in Columns (1)–(3) of Table 2, estimated hazard ratios do not significantly differ from zero. To capture distortions caused by EU enlargement in 2005 and to take into account different birth preferences between persons with German (natives) and foreign citizenship (migrants), Columns (4)–(6) show a separate analysis by nationality. Column (4) displays a significant effect on transition probability to first birth for native women, whereas an above-average increase in childcare coverage significantly increases the probability of first birth by 67.0%. However, this effect is not robust. If Column (5) requires the exclusive consideration of native women with a native partner exclusively, the effectiveness of the childcare reforms found appears to be not robust. In Columns (7)–(12), modeling first birth in $t + 1$ confirms this result, which displays the more appropriate specification because it leaves space for pregnancy.

Following Figure 2, the estimated hazard ratios for the variable $Post_{it}$ confirm general lower fertility after 2005 in West Germany. Interestingly, women living in treatment regions display a significantly lower level of fertility. On the one hand, this result contradicts Panels (a) and (b) of Figure 2 and the expectation that policy aims to increase the availability of subsidized childcare in regions with high birth rates. On the other hand, the result is reasonable because counties with comfortable economic conditions can expand childcare sizably. Moreover, the counties with comfortable economic conditions and financial potential are those that can attract women with

TABLE 2 Effects for first births with Cox hazard estimations

	Birth in t				Birth in $t + 1$				Migrants			
	Full sample		Natives		Full sample		Natives		Natives		Migrants	
	(1)	(2)	(3)	(4) Native women	(5) Native couples	(6)	(7)	(8)	(9)	(10) Native women	(11) Native couples	(12) Migrants
Treated*Post	1.171 (0.240)	1.260 (0.266)	1.303 (0.279)	1.670** (0.399)	1.500 (0.396)	0.721 (0.401)	1.037 (0.228)	1.093 (0.245)	1.126 (0.222)	1.377 (0.313)	1.186 (0.306)	1.105 (0.668)
Treated	0.847 (0.115)	0.875 (0.124)	0.820 (0.131)	0.657** (0.109)	0.692** (0.119)	2.068** (0.740)	0.999 (0.151)	0.906 (0.142)	0.880 (0.141)	0.737* (0.131)	0.815 (0.149)	1.503 (0.475)
Post	1.067 (0.144)	1.156 (0.167)	0.666** (0.115)	0.542*** (0.101)	0.589** (0.124)	0.720 (0.350)	0.787 (0.115)	0.917 (0.141)	0.746* (0.124)	0.594*** (0.112)	0.607** (0.132)	0.746 (0.414)
Additional controls:												
Socioeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Labor market controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupational sector	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County clustered SE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Births	396	396	396	319	267	77	339	339	339	273	229	66
Observations	2895	2895	2895	2541	2285	354	2558	2558	2558	2267	2044	291
Individuals	833	833	833	719	636	129	756	756	756	658	586	111

Note: Stars denote significance of displayed hazard ratios: * $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$; standard errors in parentheses. This table gives results from Cox hazard estimations of Equation (1). We model the probability to bear a first child at age t or $t + 1$ with respect to covariates that are gradually added and given in Table 1. The binary treatment status $Treated_{it}$ identifies women who live in counties with an overaverage growth in early childcare of at least 11.3 percentage points during the period 2002–2010. The dummy $Post_{it}$ refers to survey years after 2005. In Columns (4)–(6) and in Columns (10)–(12), we separate analysis by ethnic origin with respect to surveyed citizenship, while separation by birthplace does not affect the results.

Source: SOEP; Statistical Offices of the German Federal States; Statistical Office of the Federal Employment Agency; own illustration.

a general tendency to work full time and that can provide qualified childcare employees.¹⁶ The same econometric procedure is applied to the sample of second births. Results in Table A.2 do not provide any indication regarding a positive and significant effect on transition probability to second birth from childcare expansion.

Despite some evidence of an increase in the probability of first birth for native women in Table 2, the overall evidence for a positive effect of childcare provision on the transition to first and second birth is weak. Two explanations for this may shed light on this puzzling finding. First, we do not observe the actual desire for childbearing. Rather, we only model an ITT effect. Thus, a large share in treatment regions without the preference to demand childcare would result in a high downward bias of estimation.

This would be true if preference to demand external childcare in the early years of a child is low and frowned upon by society. However, because evidence by Wrohlich (2008) suggests an enormous scarcity of slots and excessive demand for early childcare, this presumption is thus unlikely and may only be true in a few very traditional regions. A second explanation is more suitable in the context of revealed preferences and ITT estimation. Waiting lists prioritize couples where both parents are employed, and single parents, parents who are on job search, in education and children who need childcare due to developmental reasons—eligibility rules that are strictly followed due to the severe scarcity of slots. Statistics on the sample of first births illustrate that only a share of 58.6% are couples where both persons are employed. The absence of a significant treatment effect is thus not particularly surprising. A closer examination of the target group and exploiting the scarcity of slots and of the strict eligibility rules should shed light on the real effectiveness of the underlying policy reforms. However, if we include all those persons mentioned in the above prioritization list in our new sample, our sample is barely reduced and estimation does not differ significantly to the baseline results. Based on previous evidence on the family–work conflict (Haan & Wrohlich, 2011; Bick, 2016; Jessen et al., 2020), we presume that effects can be particularly expected for native, employed parents. This is the group, which we define as the target group in the following.

5.2 | Effects on the transitions to birth for the target group

In this section, we reestimate the effect of childcare expansion applying the Cox hazard model. In contrast to the previous subsection, we now focus on couples where both persons with German citizenship are employed before having children. We do this in order to assess whether the absence of significant effects in the previous subsection can be explained by the fact that employed couples are prioritized when assigning childcare slots. In addition, we compare our results with earlier findings from Germany and show where our results are similar and different, and why.

Table 3 applies the Cox hazard model and reports parameter estimates for first birth probabilities. This time, we stratify regressions by the employment status of women and their partners, while we allow for different intensities of labor market participation and exclusively consider native couples. To ensure that estimates do not simply give the effect of proceeding time, we model

¹⁶ A second presumption is that counties with a large initial childcare coverage (in 1998) are assigned to the control group because the need to expand is low there. However, statistics reflect that initial childcare coverage is not significantly different between treatment and control regions. In addition, Table A.1 provides a maximum of coverage of approximately one-tenth in 2002 and, therefore, the supply of early childcare seemingly does not meet demand in each county (see also Wrohlich, 2008).

TABLE 3 The effects on the transition to first birth stratified by parents' prebirth employment

Woman: Partner:	Birth in t		Birth in $t + 1$	
	(1) Employed	(2) Full-time employed	(3) Employed	(4) Full-time employed
A: Employed				
Cox model: HR of β_3	2.319*** (0.659)	2.165** (0.748)	2.380*** (0.663)	2.121** (0.792)
OLS: Coeff. of β_3	0.070** (0.029)	0.072** (0.032)	0.084*** (0.028)	0.089*** (0.032)
Births	160	141	138	121
Observations	1,283	1,104	1,148	993
Individuals	373	337	341	312
B: Full-time employed				
Cox model: HR of β_3	2.511*** (0.802)	1.741 (0.618)	1.957** (0.620)	1.452 (0.544)
OLS: Coeff. of β_3	0.056* (0.032)	0.026 (0.031)	0.071** (0.034)	0.042 (0.034)
Births	114	104	98	89
Observations	862	765	758	677
Individuals	281	269	253	244
Additional controls	Yes	Yes	Yes	Yes
Period dummies	Yes	Yes	Yes	Yes

Note: * $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$; robust standard errors clustered at the county level in parentheses. This table gives the estimated hazard ratio (HR) from Cox hazard estimations of Equation (1) and exclusively considers native couples. We model the probability to bear a first child at age t or $t + 1$ concerning additional controls given in Tables 1 and 2 and stratify by women's and partners' employment status before the first birth. In addition, dummies for survey years are added to capture aggregated trends in fertility. Each cell provides the estimated hazard ratio of β_3 . This is the effect of the interaction between treatment status $Treated_{it}$ and time dummy $Post_{it}$ on birth transition. The binary treatment status $Treated_{it}$ identifies women who live in counties with an overaverage growth in early childcare of at least 11.3 percentage points during 2002–2010. The dummy $Post_{it}$ refers to years after 2005. Below the HR from estimating the Cox model, the table also provides the estimated coefficient β_3 from estimating linear regression.

Source: SOEP; Statistical Offices of the German Federal States; Statistical Office of the Federal Employment Agency; own illustration.

survey year dummies instead of the plain variable $Post$. With a focus on modeling childbearing in t , the results illustrate that the expansion of subsidized childcare for children under the age of three significantly increases the transition probability to first birth. This finding is robust if one parent works full time. If both parents work full time, the effect is still positive, but no longer significant. If we leave some space for pregnancy and model first birth probability in $t + 1$, the results are quite similar. Interestingly, the hazard ratio does not significantly vary depending on whether the woman or the partner is employed full time.

When duration models are applied, one challenge in evaluating policy reforms is to assess the quantity of the estimated effects. So far, presented estimations give a treatment effect of growth in childcare coverage of at least 11.3 percentage points from 2002 to 2010. However, no county can suddenly achieve such expansion within 1 year. To obtain a feeling for the effect size regarding the transition to the first birth, we use the alternative specification in Equation (2) and replace the binary treatment status $Treated_{it}$ with continuous childcare coverage $Coverage_{it}$. This gives

a marginal effect of 11.9% on the transition to first birth in $t + 1$ in Table A.3. First, this alternative specification demonstrates that the main findings do not depend on the binary version of the treatment indicator in Equation (1). Second, it shows that an annual increase of 1 percentage point in childcare coverage increases the transition probability to first birth by 11.9%. As the sole paper that conducts an analysis at the individual level and takes heterogeneities into account, a comparison with the effects found by Haan and Wrohlich (2011) is sensible. With a focus on childless couples with both parents in the labor force, their point estimate of 9% is similar to our estimate of 11.9%.

Ai and Norton (2003) underline that the estimation of the magnitude and standard errors of an interaction effect in nonlinear models can be biased. To tackle this issue, we reestimate Equation (1) by applying linear regression (OLS) and display the OLS coefficient of β_3 in Tables 3 and 4. Regarding first birth in $t + 1$, Table 3 displays an effect of being assigned to the treatment group of 8.4 percentage points. Compared to the mean share of women without a first birth of 52.5% in our sample, this is an effect of 16.0%. Our main findings are thus not biased by the choice of the nonlinear Cox model.

A closer examination of the sample of second births indicates no significant effect for employed couples from childcare expansion. Descriptive statistics show that 24.7% of the sample of second births conceives a second child within 24 months after first birth, while the median time space between first and second birth is about 33.5 months in our sample. Hence, a broader definition of prebirth employment should be applied in this case. Thus, in Table 4, we include women who were employed before first birth or before second birth.¹⁷ If age of women is used as the waiting time, the positive but very small hazard ratios are far from being significant. The empirical analysis also tackles the question regarding whether the absence of a significant effect results from the definition of the waiting time. In Columns (2) and (4), time after first birth measured in months is applied as the waiting time, something which does not improve insights into our research question (for further applications of this procedure, see Heckman and Walker, 1990; Hoem, 1993; Hoem et al., 2001).

In overall terms, results for transition probability to first birth are robust for the target group, while at the intensive margin no hints on transition probability to second births are found. These findings are in accordance with Haan and Wrohlich (2011) and reflect initial expectations taken from theoretical groundwork. As Ermisch (1989) and Apps and Rees (2004) suggest that a better availability in childcare increases opportunity costs and reentry earnings of women who already have one or more children. Larger effects at extensive margins, as compared to intensive margins, are thus consistent with theory. However, the inconsistency of our results with the findings of the sole paper that uses quasi-experimental data (Bauernschuster et al., 2016) is puzzling, but reasons for this apparent inconsistency exist. First, the approach of the latter analyzes the level of counties and applies spatial rather than individual data so that the method used and the nature of their outcome variable is quite different. Second, sample restrictions are one main challenge in the framework of individual data and duration models. The consideration of econometric issues, such as the problem of right censoring, also determine the composition of the sample. This consideration holds in particular for the sample of second births.

Third, the effect size of 11.9% seems quite large. It should be noted that childcare has increased enormously within a few years. Childcare coverage rose from 2.2% in 2002 to 18.8% in 2012, while growth thereafter stagnated. We are therefore estimating the effect of the supply of childcare on

¹⁷ Note that results do not indicate noteworthy changes if only women are considered who were employed before their first birth or who were employed before their second birth.

TABLE 4 The effects on the transition to second birth stratified on parents' prebirth employment with alternative waiting time concepts

	Waiting time concept			
	Birth in t		Birth in $t + 1$	
	(1) Women's age	(2) Time after first birth	(3) Women's age	(4) Time after first birth
A: Woman & Partner employed				
Cox model: HR of β_3	1.228 (0.479)	0.889 (0.384)	1.040 (0.438)	0.843 (0.400)
OLS: Coeff. of β_3	-0.004 (0.020)	-0.025 (0.030)	-0.008 (0.022)	-0.027 (0.033)
Births	107	102	101	96
Observations	2015	1267	1822	1163
Individuals	351	287	337	272
B: Woman & Partner employed with one person being full-time employed				
Cox model: HR of β_3	1.324 (0.514)	0.923 (0.398)	1.152 (0.482)	0.902 (0.433)
OLS: Coeff. of β_3	-0.001 (0.020)	-0.024 (0.030)	-0.005 (0.022)	-0.025 (0.034)
Births	107	102	101	96
Observations	1948	1222	1762	1122
Individuals	340	276	326	261
Additional controls	Yes	Yes	Yes	Yes
Period dummies	Yes	Yes	Yes	Yes

Note: * $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$; robust standard errors clustered at the county level in parentheses. This table gives results from Cox hazard estimations of Equation (1) and exclusively considers native couples. We model the probability to bear a second child at age t or $t + 1$ concerning additional controls given in Tables 1 and A.2 and stratify by parents' employment status before the first birth. In addition, dummies for survey years are added to capture trends in fertility. In Columns (1) and (3), the age of women is used as the waiting time, while in Models (2) and (4), time after first birth measured in months is applied as the waiting time. Each cell provides the estimated hazard ratio of β_3 . This is the effect of the interaction between treatment status $Treated_{it}$ and time dummy $Post_{it}$ on transitions to births. The binary treatment status $Treated_{it}$ identifies women who live in counties with an overaverage growth in childcare coverage of at least 11.3 percentage points during the period 2002–2010. The dummy $Post_{it}$ refers to years after 2005. Below the hazard ratio from estimating the Cox model, the table also provides the estimated coefficient β_3 from estimating linear regression.

Source: SOEP; Statistical Offices of the German Federal States; Statistical Office of the Federal Employment Agency; own illustration.

birth transitions during a period of strong growth in childcare coverage. An interesting question for future research should therefore be how the long-term increase in birth transitions through childcare differs from the transitory effect. Furthermore, the sample under study in Subsection 5.2 is a selective one in terms of employment and education. We restrict analysis to employed couples to exclude women who are not eligible for childcare and to make our sample more homogeneous. Hazan and Zoabi (2015) and Bar et al. (2018) predict that women increase their fertility when wages relative to the price of maternal time spent on raising children increase. The focus on employed women could thus distort our estimates upward. However, as a consequence of the conclusion by Hazan and Zoabi (2015) and Bar et al. (2018), mothers in top income deciles could substitute private minders with subsidized childcare. Such a substitution mechanism would bias

our estimates downward. It is therefore not clear whether this restriction increases or decreases our estimates.

In the following, we examine transmission channels of our results for the sample of first births for native couples who were employed before having children. We begin with a detailed examination of whether internal migration patterns affect our estimates.

6 | FURTHER EXAMINATIONS WITH A FOCUS ON TRANSITIONS TO FIRST BIRTH

6.1 | Internal migration patterns

For parents with a desire to bear a child, an improvement in local childcare arrangements is likely to be included in the individual decision function of choosing a place of residency. The question of whether and to what extent the childcare reforms stimulate internal migration patterns and suffer from selection is therefore of high relevance in rating the reliability of effects. This selection would upward bias our estimation results, so that the examination of this question is very important. Our panel data allow for following individuals throughout time and identifying their moving patterns and for observing whether and when they switched their residence at the level of counties.

We apply three approaches. First, we follow Havnes and Mogstad (2011) and fix the first observed residency. Columns (1) and (2) of Table 5 present Cox estimations and model first birth in $t + 1$ while keeping the first observed county constant. Results for employed women reveal that the treatment effect remains unchanged.

A second more reliable approach is to conduct a separate analysis for couples who changed counties at least once during the observation period (*movers*) and couples who stayed at one place constantly (*nonmovers*). The latter group represents a share of about 71.8% in the sample. Columns (3) and (4) of Table 5 demonstrate that the expansion of childcare also significantly increases the probability of first birth if nonmovers are considered exclusively. If both parents are somehow employed, a significant effect on the transition probability to first birth is observed, while the effect increases if one parent works full time. For childless couples, these findings highlight the relevance of the childcare reforms for the decision to bear a child.

Does this finding demonstrate that the childcare reform has triggered internal migration? Probit estimations provided in Table 6 display a negative association between moving status and treatment assignment or local supply childcare, which, however, is not significant.¹⁸ Empirical evidence does not therefore support the presumption that the expansion of childcare provides an incentive for parents to move. However, coefficients of socioeconomic variables indicate that observable characteristics, such as education, determine the decision regarding whether to move or to stay. Highly educated women are more likely to move compared to women with a low level of education, while a higher age marginally decreases the probability of native women to move at least once during the observation period by 12.3 percentage points. So, if we consider internal migration patterns and the treatment effect decreases, this is not due to internal migration patterns, but to heterogeneous effects regarding education. The findings of Tables 5 and 6 thus indicate a violation of the homogeneity assumption and emphasize the need to examine heterogeneities.

¹⁸ The results do not change if we replace the time-invariant dummy that identifies whether a couple ever moves during the observation period with an indicator variable of whether a couple changed their residency within the last year.

TABLE 5 Transition to first birth: Estimations to account for internal migration patterns

Woman:/Partner:	Fixed residency		Nonmovers	
	(1) Employed	(2) Full-time employed	(3) Employed	(4) Full-time employed
A: Employed	2.305*** (0.687)	2.043** (0.710)	1.712** (0.518)	1.848* (0.694)
Births	138	121	102	91
Observations	1148	993	838	721
Individuals	341	312	245	225
B: Full-time employed	3.026*** (1.020)	1.701 (0.609)	1.945** (0.607)	1.795 (0.650)
Births	98	89	73	67
Observations	758	677	557	489
Individuals	253	244	184	176
Additional controls	Yes	Yes	Yes	Yes
Period dummies	Yes	Yes	Yes	Yes

Note: * $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$; robust standard errors clustered at the county level in parentheses. The table gives results from the Cox hazard estimations of Equation (1) and exclusively considers native couples. We model the probability to bear a first child at age $t + 1$ concerning additional controls given in Tables 1 and 2 and stratify by women's and partners' employment status before the first birth. Furthermore, dummies for survey years are added to capture trends in fertility. In Columns (1) and (2), the first observed county of individuals is held constant over time. In Columns (3) and (4), Cox estimations are exclusively conducted for women who never moved to another county during the observation period. Each cell provides the estimated hazard ratio of β_3 . This is the effect of the interaction between treatment status $Treated_{it}$ and time dummy $Post_{it}$ on the transition probability to births. The binary treatment status $Treated_{it}$ identifies women who live in counties with an overaverage growth in early childcare of at least 11.3 percentage points during 2002–2010. The dummy $Post_{it}$ refers to years after 2005.

Source: SOEP; Statistical Offices of the German Federal States; Statistical Office of the Federal Employment Agency; own illustration.

6.2 | Heterogeneities

This section again benefits from our analysis at the individual level and examines heterogeneities concerning education and earnings. This procedure enables us to test a hypothesis outlined in the theoretical section. Ermisch (1989) and Kravdal (1996) presume that the larger a woman's wage is, the stronger the effect of subsidized childcare on birth transition will be. On the contrary, it may be that low-income households benefit the most from subsidizing childcare because before the expansion of early childcare, requesting external childcare from private sources (for instance, private daycare, nannies, and babysitters) was limited to financial resources.

To test the empirical impact of education and income on the reform impact, Table 7 displays the effect of expanding childcare on first birth probability with respect to education and income. In order to base our estimations on a reliable number of observations, we use the broad definition of high education given by Riphahn and Wynnck (2017) and consider couples where at least one partner achieves level four on the International Standard Classification of Education (ISCED) scale of 1997. Level four is achieved if the person obtained an upper secondary school diploma and a vocational qualification (OECD, 1999; Riphahn & Wynnck, 2017). Regarding income, we consider

TABLE 6 The impact of childcare availability on internal moving behavior of the sample of first births

	Indicator of migration behaviour			
	Native couples		Employed natives	
	(1)	(2)	(3)	(4)
Panel A: Childcare coverage				
Treated*Post	−0.254 (0.830)		−0.193 (0.834)	
Treated	−0.151 (0.718)		−0.876 (0.633)	
Childcare Coverage*Post		−0.120 (0.109)		−0.072 (0.137)
Childcare Coverage		0.139 (0.103)		0.057 (0.135)
Panel B: Women's socioeconomic characteristics				
Age	−0.123** (0.062)	−0.128** (0.054)	−0.208*** (0.063)	−0.229*** (0.065)
Middle education, ISCED 3-4	0.343 (0.589)	0.412 (0.567)	1.013 (0.909)	0.476 (0.692)
High education, ISCED 5-6	1.342** (0.528)	1.439** (0.684)	2.223** (0.974)	1.694** (0.745)
(Log.) Equivalent household	0.282 (0.225)	0.302 (0.246)	0.174 (0.442)	0.106 (0.425)
Labor income				
Full-time employed	0.668 (0.531)	0.651 (0.489)	0.088 (0.394)	0.142 (0.378)
Part-time employed	0.760 (0.542)	0.850* (0.484)		
Additional controls:				
Occupational sector	Yes	Yes	Yes	Yes
Partner's socioeconomic controls	Yes	Yes	Yes	Yes
Spatial controls	Yes	Yes	Yes	Yes
Period dummies	Yes	Yes	Yes	Yes
Observations	2044	2044	1148	1148
Individuals	586	586	341	341
χ^2	50.3***	57.3***	61.8***	61.3***

Note: * $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$; robust standard errors clustered at the county level in parentheses. This table shows marginal effects from random-effects probit estimation where binary moving status is regressed on the first lag of treatment status and covariates given in Table 1. Regressions are run for the sample of first births on native couples, while Column (3) and (4) only consider native couples where both partners are employed before having children. Moving status as the dependent variable refers to individuals that switched the county at least one time during the observation period. In Columns (1) and (3), $Treated_{it}$ equals one for women who live in counties with above-average growth in early childcare of at least 11.3 percentage points during the period 2002–2010. In Models (2) and (4), this dummy is replaced by continuous childcare coverage. $Post_{it}$ refers to years after 2005.

Source: SOEP; Statistical Offices of the German Federal States; Statistical Office of the Federal Employment Agency; own illustration.

TABLE 7 Heterogeneities regarding income and education

	High education (Broad definition)								Labor market income ≥ Median								Labor market income ≥ 1. Quartile							
	Drop of the Upper ...								Drop of the upper ...								Drop of the upper ...							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
Treated*Post	3.248*** (1.417)	3.424*** (1.529)	3.024** (1.438)	2.682** (1.255)	2.539* (1.418)	2.927*** (1.018)	3.076*** (1.034)	2.950*** (0.997)	2.612*** (0.905)	2.426** (1.028)	2.455*** (0.734)	2.472*** (0.735)	2.324*** (0.706)	2.169** (0.671)	2.038*** (0.687)									
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Births	76	74	70	69	54	97	94	90	88	71	126	123	119	117	100									
Observations	644	526	447	386	269	977	855	752	660	469	1098	976	873	781	590									
Individuals	187	175	162	149	121	290	278	261	246	210	324	312	295	280	246									

Note: Stars denote significance of displayed hazard ratios: * $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$; robust standard errors clustered at the county level in parentheses. The table gives results from Cox hazard estimations of Equation (1) and exclusively considers native couples where both partners were employed before childbearing. We model the probability to bear a first child at age $t + 1$ regarding additional controls given in Tables 1 and 2. In addition, dummies for survey years are added to capture trends in birth transitions. In Column (1), Cox estimation is stratified on highly educated couples, whereas high education refers to couples where at least one partner achieves ISCED-level four (based on Riphahn & Wijnck, 2017). In Columns (2)–(5), the upper tail of the distribution of labor income is excluded from analysis. For example, in Column (2) the 5% with the highest income of income distribution are excluded and so forth. Analogously, Cox estimation is stratified on high-income women in Columns (6)–(15) with gradually exclusion of the upper tail of income distribution. The binary treatment indicator $Treated_{it}$ identifies women who live in counties with an above-average growth in early childcare of at least 11.3 percentage points during the period 2002–2010. The dummy $Post_{it}$ refers to years after 2005.

Source: SOEP; Statistical Offices of the German Federal States; Statistical Office of the Federal Employment Agency; own illustration.

equivalent labor market income of mothers, interpreted as opportunity costs in the theoretical framework.¹⁹

The first estimation for the group of highly educated mothers shows that the effect of the childcare expansion increases. If we drop those women from the upper 5% of the income distribution, the effect is slightly increased. However, the effect begins to decrease if we drop the upper 10%, 15%, and 25% of the distribution. This procedure provides two messages. First, the theoretical hypothesis whereby a stronger relationship between the availability of childcare and the transition to first birth is expected with higher income can be confirmed. The treatment effect for highly educated women is above the average effect for the sample conditional on employment. Second, effects are not attributable to the top income decile, which mainly represents couples who are not dependent on subsidized childcare and who have been able to demand private childcare from other sources before the childcare reforms.

Both findings are confirmed if we focus our Cox estimations on women with high income instead of for education. Here, we apply two definitions of high income. First, we define high income as labor market income above the median of the income distribution of the full sample of first births (Columns (6)–(10)). Second, high income refers to labor market incomes that exceed the first quartile of the income distribution (Columns (11)–(15)). Again, stratified estimations result in an effect above average, which decreases if the upper 10%, 15%, and 25% are excluded from analysis.

6.3 | Robustness checks

After analyzing the effects of internal migration behavior on the internal validity of our empirical approach and considering the heterogeneity in the estimates, further robustness tests are performed in this subsection. In the following, we demonstrate that our estimates of the sample of first births with a focus on couples who were employed before having a child are not distorted by simultaneous family policy reforms and that the expansion of early childcare has affected the transition probability to first birth and not just the age at first birth.

The parental leave reform of 2007

As shown in Section 1, some papers studying the link between the local supply of childcare and birth transitions suffer because the remaining institutional environment is exposed to changes during the observation period. In our setting, there is one single reform in family policy that could distort our estimation results upward, namely, the reform of the parental leave system in 2007 (for more details about this reform, see Cygan-Rehm, 2016; Gobbi et al., 2018; Raute, 2019). The reform replaced a means-tested system that paid a maximum of 300 euros monthly for up to 24 months or 450 euros for up to 12 months with a new system in which the exact amount depends on a leave taker's average net income in the last 12 months before childbirth, while the regular eligibility duration is 12 months. One advantage when studying first births is that the sibling premium—a bonus payment for further births if the time between births is short—does not affect our analysis. Research by Raute (2019) shows that women with earnings above the median are those whose

¹⁹ Although the theoretical models by Ermisch (1989) and Apps and Rees (2004) may indicate that earnings should be captured as an indicator of opportunity costs, we use equivalent household labor market earnings of mothers. This approach accounts for the labor market performance of partners and for the household composition, which are important components of the individual decision function of childbearing.

claims are raised due to the reform, while Geyer et al. (2015) confirm this result for the first year after birth. We therefore follow Bauernschuster et al. (2016) and add an interaction term between being highly educated and the time dummy that refers to survey years from 2007 to the Cox estimation. This time, we attempt two definitions of high education. In Column (1) of Table 8, we again use the broad definition by Riphahn and Wijnck (2017). In Column (2), high education refers to women with a minimum ISCED level of five. In both estimations, the treatment effect for the target group from Subsection 5.2 is barely affected.

A third approach used to cancel out potential biases due to the parental leave reform of 2007 is to truncate the observation period to 1998–2006. As Kluve and Tamm (2013) illustrate, an impact on birth decisions from reforming parental leave before 2007 is unlikely because the time between the first draft of the law in September 2006 and its enactment was quite short. This time, the effect is only weakly significant in Column (3), which is reasonable because the growth of childcare coverage until 2006 was still moderate in West Germany.

Abolition of childcare fees

Following recent research by Huebener et al. (2020), some federal states abolished childcare fees for children in the last year of childcare before primary school, affecting children born between 2001 and 2006. Huebener et al. (2020) find that those reforms affect the labor supply of mothers. Although the last year of childcare before primary school takes place several years after the decision as to whether to bear a child or not, it may be that those reforms also affect fertility decisions. Because three federal states of West Germany (Baden-Wuerttemberg, Bavaria, and Bremen) did not abolish fees during our observation period, the exclusive consideration of those three states provides a reliable robustness check. By applying Cox estimation for the sample of those three federal states, the hazard ratio remains significant. Thus, while childcare fees do not change the fact that we find a significant positive relationship between childcare and fertility, the small sample size of 453 observations makes it difficult to evaluate whether childcare fees affect the magnitude of the effect.

Completed fertility and the definition of childbearing years

Table 8 presents two further specification checks. First, Columns (5) and (6) explore alternative definitions of childbearing years. If the beginning of childbearing years is reduced from 21 to 18 years, this approach results in a larger marginal effect on the transition probability to first birth. This result may be interpreted as an indication that young women in particular react to improvements in subsidized childcare arrangements. Does the expansion of childcare coverage solely make women bear a first child at younger ages without increasing completed fertility over the entire life cycle?

A first step for examining this question is displayed in Model (6). Here, we only include women in our sample that reach their end of childbearing years (here defined as 36 years) while still being part of the SOEP surveys. The treatment effect is relatively unchanged compared to our preferred specification from Subsection 5.2. This alternative specification thus also illustrates that the Cox hazard model reliably considers right censoring. The question regarding whether the expansion of childcare increases completed fertility or whether our results merely reflect decreased ages at first birth is important in terms of uncovering the actual benefits of the childcare reforms. Model (7) thus shows coefficients from regressing the age at first birth of those who experience a first birth on regressors (see also Bauernschuster et al., 2016). Although the estimated coefficients indeed reflect a negative sign, this relationship is not significant. This result supports the finding that the

TABLE 8 Robustness checks for the sample of first births

	Parental leave			Definition of childbearing years			Completed fertility (7) Age at first birth as outcome
	(1) Broad	(2) Narrow	(3) 1998–2006	(4) Childcare Fees	(5) Start at 18 years	(6) End at 36 years	
Treated*Post	2.289*** (0.620)	2.325*** (0.649)	2.709* (1.573)	4.728** (3.006)	2.907*** (0.981)	2.317** (0.958)	−0.482 (0.811)
High Educ*Post 2007	1.509 (0.481)	0.854 (0.224)					
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Births	138	138	87	53	104	78	138
Observations	1148	1148	644	453	770	674	630
Individuals	341	341	244	135	224	155	203

Note: Except for Model (7) where coefficients are displayed, stars denote significance of displayed hazard ratios: * $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$; robust standard errors clustered at the county level in parentheses. The table tests the robustness of Cox hazard estimations of Equation (1) and exclusively considers native couples where both partners were employed before childbearing. We model the probability to bear a first child at age $t + 1$ regarding additional controls given in Tables 1 and 2. In addition, dummies for years are added to capture trends in birth transitions. The binary treatment status $Treated_{it}$ identifies women who live in counties with an above-average growth in early childcare of at least 11.3 percentage points during the period 2002–2010. The dummy $Post_{it}$ refers to years after 2005.

In Columns (1) and (2), an interaction term between being highly educated and the time dummy Post-2007 that refers to years from 2007 is added to the Cox estimation in order to capture birth transition effects from the parental leave reform in 2007. Two definitions of high education are tried. In Column (1), a highly educated couple refers to couples where at least one partner has ISCED-level four (based on Riphahn & Wiynek 2017). In Column (2), high education applies if women are at least of ISCED-level five. In Column (3), the analysis period ends before the reform. In Model (4), we exclusively consider the federal states Baden-Wuerttemberg, Bavaria, and Bremen who did not abolish childcare fees during the observation period. While in Model (5), the definition of the beginning of childbearing years is reduced to 18 years, in Model (6), we only include women in our sample that reach their end of childbearing years (here defined as 36) during being still part of the SOEP. Finally, in Model (7), we apply OLS and regress the age at first birth on regressors to indicate whether the expansion of childcare affects completed fertility or only makes women to bear the first child at younger ages.

Source: SOEP, Statistical Offices of the German Federal States; Statistical Office of the Federal Employment Agency; own illustration.

supply of childcare is associated with an increase in the transition to first birth and not solely with a decrease in the age of women at first birth.

Moreover, Cornelissen et al. (2018) underline that initial childcare coverage is a further explanatory factor of childcare expansion and that the expansion is smaller if the initial level of childcare supply is larger. Although the authors consider childcare for children up to 6 years, we test whether the results change if we additionally control for initial childcare coverage in 2002 and childcare coverage in 2010. However, this is not the case. Regarding the estimation for first birth in $t + 1$ when both parents were employed before the birth, the hazard ratio of β_3 of Equation (1) due to the Cox model even decreases from 2.38 (from Table 3) to 2.33.²⁰

In addition, Appendix 2 presents further minor robustness checks regarding the common trend assumption and the definition of the treatment status.

7 | CONCLUSION

In this paper, we dedicated our analysis to the complexity of fertility decisions and examined the causal relationship between the local supply of publicly funded childcare for children under the age of three and childbearing. Research indicates that the high level of childlessness in Germany is due to a plethora of reasons, such as cultural values, varying life attitudes, educational expansion after World War II, family policy, and the lack of support from fathers (e.g., Cygan-Rehm & Maeder, 2013; Kamhöfer & Westphal, 2017). From the perspective of policymakers, the hurdle of childlessness is therefore a challenge of special economic relevance.

To account for the endogeneity of fertility decisions, we exploit the quasi-experimental expansion of childcare slots for children under the age of three initiated in 2005 in Germany. The Day Care Expansion Law from 2005 and the Law on Support for Children from 2008 increased childcare coverage for children under the age of three in West Germany from almost zero to 18.8% in 2012. Furthermore, the laws created large regional variations in childcare coverage by counties. The application of the suitable Cox hazard model demonstrates robust and sizeable effects from childcare expansion concerning transitions to the first birth. Childless couples who are employed before having children are more likely to conceive a first child by approximately 12%, while at the intensive margin, no indications regarding reactions are found for second birth probabilities. The relationship between transition to births and the availability of childcare arrangements is thus fairly different by parity. While those results provide important implications, it should be noted that these findings are only attributable to couples with German citizenship and to those who are employed before having children, which is a selective sample. This limits the external validity of the results. Moreover, we did not consider effect heterogeneity with respect to daily duration of childcare or type of childcare facility.

To support the evidence found by Haan and Wrohlich (2011) and Bauernschuster et al. (2016), our particular focus is on heterogeneities and internal migration patterns. This research demonstrates some crucial findings. Interestingly, effects on the transition to first birth remain significant after accounting for internal migration behavior of couples at the level of counties. The focus on

²⁰ The estimation table for this robustness check can be obtained from the authors. Note that initial childcare coverage positively correlates with the first birth transition probability. However, this relationship is not of statistical significance. This can be explained by the fact that although some heterogeneities existed in childcare coverage in 2002 between counties, each county started from a rather low level in 2002. This is a significant difference to childcare for older children up to 6 years in Germany, which Cornelissen et al. (2018) consider.

internal migration also raises an interesting question for future research, from the perspective of local policymakers. If a large regional childcare coverage makes parents change their residency, the question is whether such local family instruments can be means of attracting new residents.

Further examinations highlight that highly educated women are more likely to bear a first child than the average, while the effects do not apply to the upper decile of the income distribution. After applying a large set of robustness checks, we conclude that the childcare reforms indeed increase transition probability of the first birth and do not merely lower a mother's age at first birth. This finding underlines the effective impact of the German childcare reforms and provides a further element of the benefits which the childcare reforms produce.

The focus needs to be expanded if a complete cost–benefit analysis of the reforms is to be drawn. Alongside the effects on fertility, the expansion of early childcare has also promoted maternal labor participation (Boll & Lagemann, 2019; Müller & Wrohlich, 2020), potentially changes the role of the father in the long run and weakens depreciation of human capital during a break from the labor market because the break can be shortened by using childcare. Furthermore, early childcare affects the development of children's cognitive and noncognitive skills (Felfe & Lalive, 2018). In order to get an expanded image about the effects of the German childcare reforms therefore several outcomes and papers need to be considered and our paper only looks at a partial aspect of the effects of those reforms.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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