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## Does Expanding Public Child Care Encourage Fertility? County-Level Evidence from Germany

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# Working Papers

## Does the Expansion of Public Child Care Increase Birth Rates? Evidence from a Low-Fertility Country

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## Does the Expansion of Public Child Care Increase Birth Rates? Evidence from a Low-Fertility Country

### Abstract

Germany has the lowest birth rate among all OECD countries. To encourage fertility, the federal government has recently introduced a set of reforms that led to a substantial expansion of public child care for under three year old children. Using administrative county-level data, we exploit within-county variation in this expansion and find evidence that the provision of public child care causes an increase in birth rates. Extended empirical specifications suggest that our results are neither confounded by selective migration nor driven by tempo effects. Our analysis therefore provides some first evidence that low fertility may be reversed through changes in public policy that allow women to combine employment and motherhood.

JEL Code: J13.

Keywords: Public child care, fertility.

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## 1. Introduction

Over the past few decades, many industrialized countries have entered uncharted demographic territory characterized by very low fertility rates.<sup>1</sup> The economic consequences of very low fertility are manifold, ranging from a decline in the working-age share of the population to a slowdown of economic growth to financial difficulties in health care and pensions systems (Bloom *et al.*, 2010). With the growing awareness that sustained low fertility has serious ramifications, governments in affected countries have introduced policy measures intended to support the private decision to have children. Chief among these have been efforts to expand public child care. However, there is almost no previous experience of the effects of such policies in the context of very low fertility rates (McDonald, 2007). As a result, it is still open for debate whether public child care policies are an effective way to increase fertility rates where these rates are considered to be too low.

In this paper we evaluate the impact on fertility of a German policy initiative from the mid-2000s, which led to a large scale expansion of public child care for children under the age of three. One of the explicitly stated goals of the initiative was to increase fertility rates by making children less costly in terms of income and career opportunities (Rindfuss *et al.*, 2010). Ever since the 1970s, Germany has been among the twenty countries with the lowest fertility rates worldwide (Population Reference Bureau, 2007). Its population reached a maximum shortly after the turn of the millennium and has started to decline thereafter as a result of sustained very low fertility rates (Dorbritz, 2008). According to data from the World Bank (2009), no other OECD country worldwide has fewer births per 1,000 inhabitants than Germany. In both political and academic debates, these demographics have been associated with a lack of progressive family policies. Especially the availability of public child care for young children was severely rationed in West Germany until very recently. For example, in a survey conducted in 2005, 35 percent of West German mothers with under three year olds stated a demand for a child care slot (Bien *et al.*, 2006), while only roughly 5 child care slots per 100 children in this age group were available.<sup>2</sup> Prompted by this rationing of public child care, the German government implemented a set of comprehensive public child care reforms during the period 2005-2008, with the explicit intention to increase fertility levels. Specific objectives were to: (i) increase the child care coverage rate for under three year olds to 35 percent by 2013, and (ii) establish a legal right to a public child care slot for all preschool children aged one and above by 2013. On aggregate, the policy initiative led to a substantial expansion of public child care provision for young children across West Germany's 326 counties. However, since the responsibility to create additional child care slots was decentralized, the counties differ distinctly in the extent to which child care coverage increased. Apart from certain socio-demographic county characteristics, idiosyncratic shocks to the local

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<sup>1</sup>“Very low” fertility countries are sometimes defined as having a total fertility rate of 1.6 or lower at some point from 1996 onward. Currently, there are 31 countries worldwide with total fertility rates below 1.6 (Population Reference Bureau, 2011), and the government of each of these countries considers this level of fertility too low (United Nations, 2003).

<sup>2</sup>Wrohlich (2008) estimates that more than 50 percent of West German mothers with children aged 0-3 were queuing for a child care slot in the mid-2000s, suggesting that the excess demand for child care was even more severe. In East Germany, by contrast, child care coverage for children aged 0-3 were around 40 percent already in 2006.

administrative process play a major role in explaining the variation in the pace of child care expansion.

The county-level differences that have emerged during the expansion of public child care in West Germany offer a natural way to evaluate its impact on fertility. In a first step, we estimate a difference-in-differences model which compares the county-level birth rate (i.e., births per 1,000 women) in West Germany before and after the child care reforms, between counties where child care coverage expanded a lot (i.e., the treatment group) and counties with little increase in child care coverage (i.e., the control group). In order to verify that the key identifying assumption of this approach—i.e., the “common trend” assumption—is plausible, we run placebo treatment difference-in-differences estimations in pre-treatment years. In a second step, we analyze a more flexible county-fixed effects model of the birth rate which allows for time-invariant heterogeneity between counties. In order to test the validity of the key identifying assumptions of our fixed-effects model, we perform a battery of sensitivity checks. For example, we provide evidence that our results are not confounded by selective migration of potential mothers to counties with high child care availability.

We find consistent and robust evidence of a positive effect of public child care expansion on fertility. To be concrete, our estimates suggest that a 10 percentage point increase in public child care coverage increases the number of births per 1,000 women by 1.4, or roughly 3.2 percent of the baseline birth rate. Under the strict assumption of linearity, this result has a striking implication: moving from having child care slots available for 5 percent of children under three years old to having slots available for 35 percent will lead the average woman to have roughly 0.13 more children. This, in turn, suggests that the policy initiative may be an important contributor to the reversal of very low fertility rates.<sup>3</sup> Further regressions provide evidence that the estimated increase in fertility is not just a relict of mere tempo effects. Thus, our paper contributes to the public debate on family policies and low fertility in Western Europe. In particular, our analysis provides some first causal evidence suggesting that low fertility may indeed be reversed as policy makers start to facilitate the combination of motherhood and labor force participation.

The remainder of the paper is organized as follows. Section 2 provides a review of the related literature. Section 3 gives detailed background information on fertility in Germany and on the policy initiatives that led to considerable expansion of public child care for under three year olds in West Germany. Section 4 outlines our empirical strategy while Section 5 describes the data. Section 6 presents the results on the impact of expanding public child care on fertility. In Section 7, we present robustness checks dealing with issues of selective migration and the relevance of tempo effects. Section 8 discusses our findings and concludes.

## 2. Related Literature

We build on an impressive amount of research that examines the consequences of policy efforts to improve the availability and affordability of public child care. These studies

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<sup>3</sup>The total fertility rate in Germany was around 1.4 in 2010. Our estimates suggest the following counterfactual: if Germany’s child care coverage rate for under three year olds had reached the 2013-target of 35 percent already in 2009 (instead of an actual 14.3 percent), the fertility level would have been 1.49 instead of 1.4.

can be divided into two broad types. The first type examines the effects of child care on maternal employment. The picture that emerges from this literature in general is that of positive effects from the introduction or expansion of affordable child care on the labor supply of mothers—particularly sole mothers (e.g., Gelbach, 2002; Blau and Tekin, 2007; Baker *et al.*, 2008; Lefebvre and Merrigan, 2008; Cascio, 2009). One notable exception is the study by Havnes and Mogstad (2011), who exploit the introduction of universal child care in Norway in the late 1970s and find no positive causal relationship between public child care and maternal employment.<sup>4</sup> Despite this, the total available evidence supports the widely held perception that the availability of public child care assists families in reconciling work with family life.

The second type of study examines the link between child care and child development. Several lines of evidence emerge from these studies. First, there exist studies offering both negative (e.g., NICHD – Early Child Care Research Network, 2003a; Baker *et al.*, 2008), neutral (e.g., Lefebvre and Merrigan, 2002) and positive (e.g., NICHD – Early Child Care Research Network, 2004; Havnes and Mogstad, 2012) effects of the amount of time spent in child care on the developmental outcomes of young children. Second, there appears to be a positive relationship between child care quality and child development (e.g., Field, 1991; NICHD – Early Child Care Research Network, 2003b). Finally, while the amount of time spent in child care contributes independently to child development, the impact of family background characteristics is generally stronger.

Much less can be said for our knowledge of the impact of public child care on fertility. From a theoretical perspective, the link between child care and fertility is clear. As affordable child care becomes available, mothers may return to work sooner after childbirth. This, in turn, reduces the opportunity cost of having children—e.g., forgone wages or loss of skill development while out of the labor force—and so increases fertility (e.g., Becker and Lewis, 1973; Willis, 1973; Ermisch, 2003). However, this theoretical prediction receives weak empirical support and the evidence is ambiguous at best – see, for example, Mason and Kuhlthau (1992); Kravdal (1996); Del Boca (2002); Hank and Kreyenfeld (2003). What distinguishes our work from these studies is that we exploit a set of policy reforms which provides us with exogenous variation in the expansion of public child care for under three year old children. This helps us to identify plausibly causal effects of public child care on fertility.

To the best of our knowledge, only two other studies exist that use policy reforms to assess the impact of public child care on fertility. Both studies are conducted within the Scandinavian context, where fertility rates are close to replacement levels. Rindfuss *et al.* (2010) examine a policy reform from the mid-1970s in Norway, which led to a substantial expansion of public child care. Their results suggest that moving from having no child care slots available for preschool children to having slots available for 60 percent of preschool children leads the average woman to have 0.5 more children. Mörk *et al.* (2013) exploit the exogenous variation in user fees caused by a Swedish child care reform to identify the causal effect of child care costs on fertility. The results suggest that the reduction in child care costs increased the number of first and higher order births, but only seemed to affect the timing of second births. Differently from the aforementioned studies, the context of

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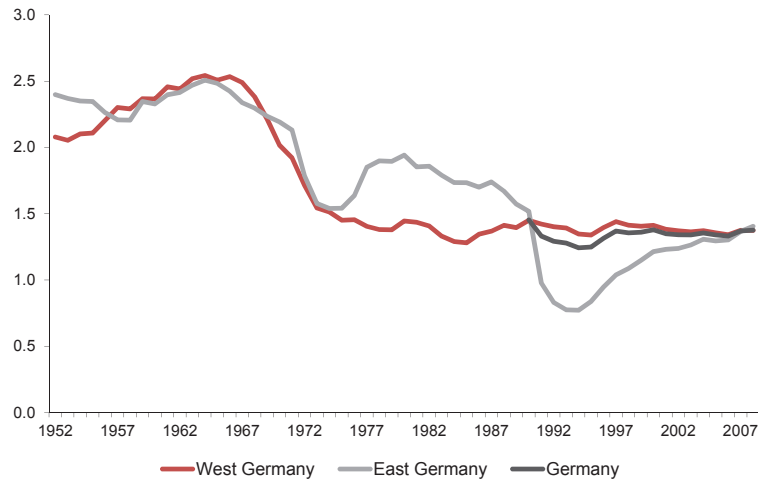
<sup>4</sup>Relatedly, Lundin *et al.* (2008) evaluate the impact of reductions in child care costs on female labor supply in Sweden and find no evidence of positive effects.

our study is Germany – a country severely struggling with the ramifications of remarkably low fertility. Thus, our results speak more directly to the policy debate as to whether or not low fertility can be reversed through changes in public policy that allow women to combine employment and motherhood.

### 3. Background and Context

West Germany has long been known for its low fertility rates. While fertility rates were increasing during the 1950s and early 1960s from just above 2.0 to 2.5, they dramatically decreased in the late 1960s and early 1970s to a level of 1.5 in 1974. In general, this drop in fertility can be observed across virtually all developed countries; yet, West Germany was hit quite early and particularly fiercely by international comparison (Hradil, 2012). During the last 40 years, fertility stayed roughly constant at a low level of 1.4 (Figure 1).

Figure 1: Fertility rate in Germany

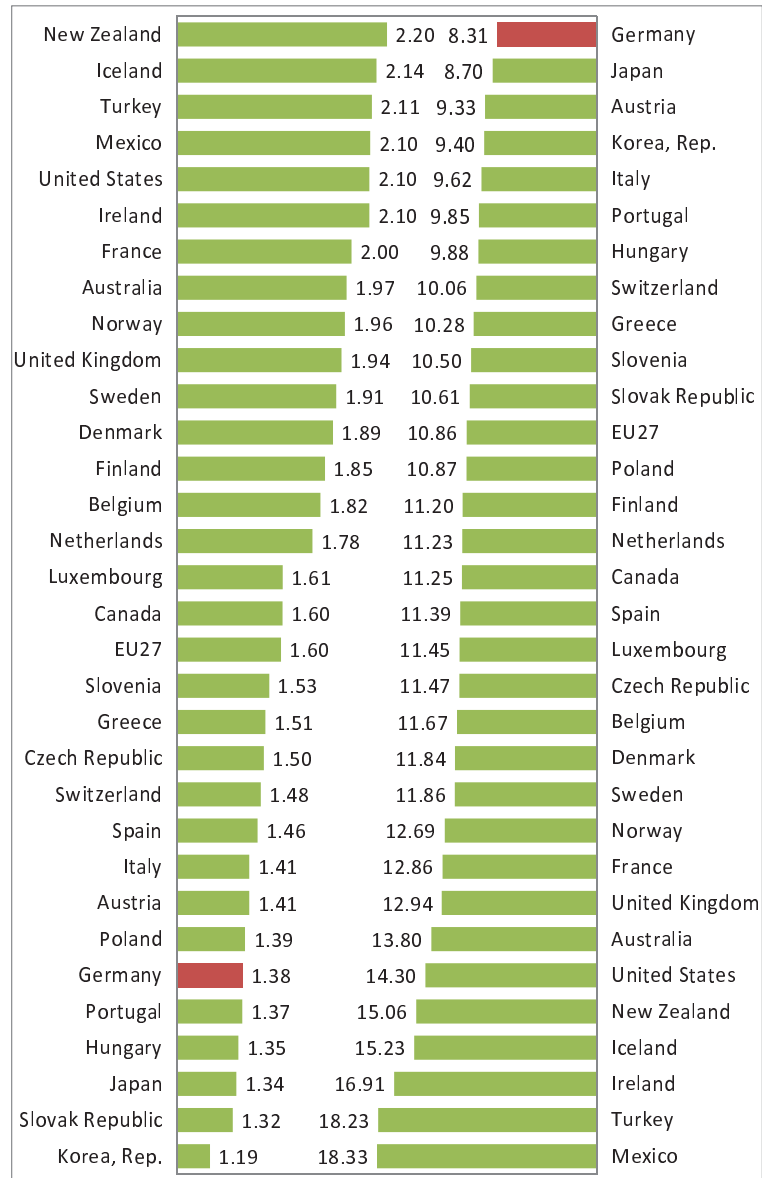


*Notes:* The figure shows total fertility rates in Germany from 1952 until 2008. Total fertility rates are defined as the average age-specific number of births per woman. It can be interpreted as the total number of children a woman would have, assuming that the age-specific fertility rates of that year stay unchanged throughout the life of a woman. From 2001, data from West Germany do not include West Berlin and data from East Germany do not include East Berlin (German Federal Statistical Office 2009).

Ever since the 1970s, West Germany has been among the twenty countries with the lowest fertility rates worldwide (Population Reference Bureau, 2007). Data from the World Bank (2009) depict a fertility rate of 1.38 for Germany in 2008. Thus, Germany lies clearly below the EU-27 average of 1.60 and close to Poland (1.39), Portugal (1.37), Hungary (1.35), or Japan (1.34). Fertility rates in the US (2.10), France (2.00), Norway (1.96), or Sweden (1.91) are substantially higher. The crude birth rate looks even grimmer. With 8.3 births per 1,000 inhabitants, Germany even falls behind Poland (10.9), Portugal (9.8), Hungary (9.9), or Japan (8.7). No other OECD country exhibits lower numbers of births per 1,000 inhabitants than Germany.

The low fertility in Germany does not go hand in hand with high female employment. To the contrary, Germany has also long been known for its low female and in particular maternal labor market participation. Indeed, data from the German Micro Census suggest that, in 2002, the employment gap between mothers of under three year olds and women of similar age without any children was more than 40 percentage points in West Germany.

Figure 2: Fertility and birth rates 2008



*Notes:* The figure shows total fertility rates (right) and birth rates (left). Total fertility rates are defined as the average age-specific number of births per woman. It can be interpreted as the total number of children a woman would have, assuming that the age-specific fertility rates of the year 2008 stay unchanged throughout the life of a woman. Birth rates are defined as the number of live births in 2008 per 1,000 inhabitants (World Bank, 2009).

According to Myrskylä *et al.* (2009), problems with reconciling work and family life could evoke both, low female employment and low fertility at the same time. In order to assess whether incompatibility of work and family life might be responsible for low fertility rates in Germany, we need more information on the institutional background faced by young families.

Germany has very generous family policies, even by Scandinavian standards. Mothers benefit from comparatively long durations of parental leave. In 2002, before the child care expansion, mothers could take 36 months of parental leave. This enables mothers to return to their employer three years after a child's birth because mothers must not be dismissed during the parental leave period. Parental leave benefits were paid for a maximum of 24 months with the amount varying from 300 to 0 Euros per month depending on household income. Furthermore, families receive unconditional child benefits in the order of more than 150 Euros per child per month at least until age of legal majority.

Child care availability, on the other hand, is underdeveloped. Although public subsidies are substantial—on average 80 percent of the child care costs are covered by public subsidies while only 20 percent are covered by parental fees—public child care provision is far from universal. Particularly in West Germany, public child care for under three year old children was virtually non-existent until recently while the situation was substantially different in East Germany.

Throughout the history of the former German Democratic Republic, the East German government strongly supported the use of public daycare for children of all ages. Apart from ideological purposes, the government's primary aim was to guarantee the compatibility of motherhood and employment. The East German child care system survived the German reunification, with more than one-half of all East German children under the age of three and almost all East German children between three and six attending a child care center in the mid-1990s. At the turn of the millennium, parents in East Germany demanded fewer child care slots for children than were available (Hank *et al.*, 2001). The picture today still is that there is no shortage of child care slots for children under six in East Germany.

The situation was much different in West Germany, where social policies supported private care by mothers over universal child care for preschool children until after reunification (Moeller, 1993). In 1996, the German government enacted legislation that grants children aged three to six the right to a place in a public kindergarten, which ultimately led to full provision of half-day public child care for that age group in West Germany. However, for children under the age of three, public child care coverage was less than 5 percent in West Germany. At the same time, virtually no private market for child care had emerged despite a massive excess demand. Indeed, according to a survey conducted in 2004/2005, 84 percent of all non-employed mothers of under three year olds state that they would like to enter the labor market; yet, more than half of them argue that they cannot because of non-existent or inadequate child care arrangements (Bien *et al.*, 2006). Altogether, in West Germany, 27 percent of parents would like to have a slot in public child care for their one year old child, and 55 percent for their two year old child (Bien *et al.*, 2006).

It was not until the mid-2000s that the German government undertook major efforts to increase public child care coverage for under three year olds. Interestingly, this latest wave of public child care policies in Germany is characterized by a marked change in

underlying political motivations. While previous family policies aiming at reconciling work and family life concentrated on improving mothers' labor market (re-)entry after birth, recently, the realization of births has been identified as an explicit political aim (Bien *et al.*, 2006). Three major policy initiatives were crucial for the expansion of public child care for under three year olds in the mid to late 2000s:

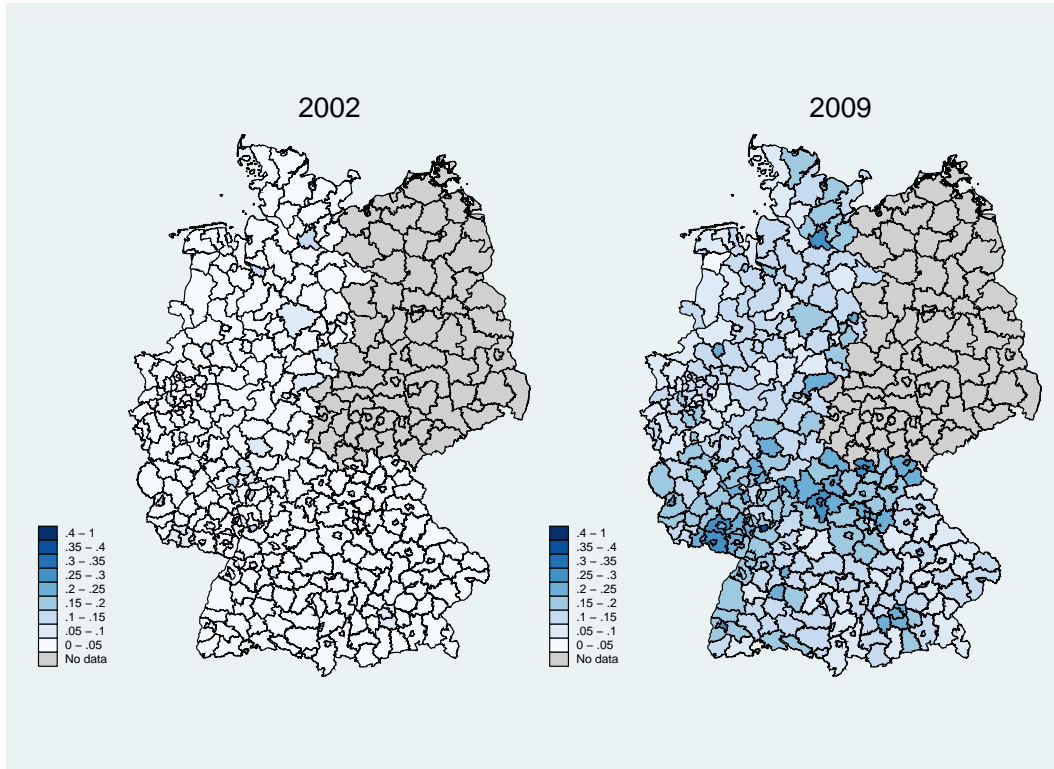
- In 2005, the government made the legal commitment ( "*Tagesbetreuungsbaugesetz*") to create 230,000 additional child care slots for under three year old children by 2010 in West Germany. The specific aim was a child care coverage rate of 17 percent by 2010 in West Germany.
- In 2007, a summit (called "*Krippengipfel*") of the three federal levels—i.e., federal state, "Länder", local authorities—agreed upon increasing the child care coverage rate for under three year olds to 35 percent by 2013.
- In 2008, the law to promote children ( "*Kinderförderungsgesetz*") established the legal claim to a child care slot for all preschool children age one and above by 2013.

The legal prerequisites for the public child care expansion were set on the federal level. Regulating child care lies within the responsibility of states whereas counties are responsible for the implementation and organization. In the run-up to the law to promote public child care, the three federal levels agreed that every level bears a share of the expansion costs. Since public child care for under three year olds was virtually non-existent in West Germany before 2005, all counties in each state had to substantially expand public child care in order to be able to fulfill legal claims to a child care slot for all preschool children age one and above by 2013.

In order to assess the expansion brought about by these initiatives, Figure 3 provides two maps which illustrate the child care coverage rate for West Germany's 326 counties in 2002 and 2009, respectively. In 2002, we observe that the child care coverage rate was consistently below 5 percent across virtually all West German counties. In succeeding years, the child care coverage rate more than quintupled to reach an average of 15 percent in 2009. However, it is also evident from the map that the counties differ distinctly in the magnitude of public child care coverage expansion. In 2009, the public child care coverage rates vary from 3.7 percent to 41.6 percent. The percentage point increases in child care coverage from 2002 to 2009 range from a minimum of 3 percentage points to a maximum of 27 percentage points. We observe considerable variation in the pace of this expansion across counties even within the same state. Therefore, Huesken (2011) argues that this variation can be traced back to very local decisions and conditions at the county level. Thus, even if federal and state policies laid out the ground for public child care expansion, it is local decision makers who actually determine the pace and the extent of expansion. Thinking about local factors which could be relevant for public child care expansion, we might argue that, although predicted child care demand exceeds current supply in virtually all counties, the political pressure to quickly increase child care supply might be higher in counties where predicted fertility (or closely related, predicted child care demand) is exceptionally high. Socio-demographic factors which could be relevant predictors of future fertility are foremost county characteristics such as population age structure or population density but also economic wealth, or labor market conditions. However, apart

from these socio-demographic variables, the variation in the mid to late 2000s child care expansion can mainly be ascribed to idiosyncratic shocks to the administrative process. Amongst them are varying routines and knowledge about the complicated funding system (with subsidies coming from the federal state, the state and the municipality), shortages in construction ground, various regulations for building child care centers, shortages in qualified child care workers, serious delays in approval or final rejections of applications due to non-compliance with regulations (see, e.g., Huesken (2011), or Felfe and Lalive (2012)).

Figure 3: Public child care coverage in West German counties in 2002 and 2009



Notes: The left panel shows child care coverage in West German counties in 2002, the right panel shows child care coverage in West German counties in 2009. East German counties are shaded in gray.

#### 4. Empirical Strategy

In order to identify the effect of public child care on fertility, we start with a simple difference-in-differences approach which exploits the expansion of public child care during the last decade. In particular, we order the German counties by the absolute size of the increase in the public child care coverage rate from 2002 to 2009. Then, we define those counties whose increase in public child care coverage was above the median as the treatment group, while those counties whose increase was below the median constitute the control group. Using this setting, we follow Havnes and Mogstad (2011) who use a very similar identification strategy for analyzing the effects of a public child care expansion on maternal employment in Norway. Further, we use the year 2002 as the pre-treatment

year and the year 2009 as the post-treatment year in our basic two-period difference-in-differences model. There are two reasons for choosing the year 2002 as the pre-treatment year: First, since public child care was not a major political issue until the year 2005, the year 2002 is certainly a year which is unaffected by any political decisions fostering the expansion of public child care. Second, the year 2002 is the last year where administrative data on public child care coverage on the county level is available before public child care actually became a political issue in 2005.

We can express our simple two-period difference-in-differences strategy that identifies the treatment effect  $\delta$  in the following way:

$$\begin{aligned} & \{E(y_{ct+1} \mid D_c = 1, T_t = 1) - E(y_{ct+1} \mid D_c = 1, T_t = 0)\} \\ & - \{E(y_{ct+1} \mid D_c = 0, T_t = 1) - E(y_{ct+1} \mid D_c = 0, T_t = 0)\} = \delta \end{aligned} \quad (1)$$

where  $y_{ct+1}$  is the number of births by 1,000 women aged 15 to 44 living in county  $c$  in  $t+1$ . The outcome variable is measured in  $t+1$  because there are at least 10 months from the decision to have a child to the actual birth.  $D_c$  is the treatment group indicator for county  $c$ , which is unity for counties of the treatment group, i.e., for those counties with above median increase in public child care coverage, and zero for counties of the control group.  $T_t$  is the post-treatment year indicator, which is unity for all observations in the post-treatment year 2009, and zero for all observations in the pre-treatment year 2002.

This difference-in-differences model can be rewritten in regression form:

$$y_{ct+1} = \alpha + \beta D_c + \gamma T_t + \delta D_c T_t + \epsilon_{ct+1} \quad (2)$$

In this regression, the treatment effect  $\delta$ , which captures the Average Treatment Effect on the Treated (ATT), is identified by the coefficient on the interaction of the treatment group indicator  $D$  and the post-treatment indicator  $T$ , while  $\alpha$  captures the number of births per 1,000 women aged 15 to 44 in the control group in the pre-treatment year 2002,  $\beta$  captures the difference of the treatment group from the control group in the pre-treatment year 2002, and  $\gamma$  gives the difference between the post- and the pre-treatment year for the control group. A vector of covariates  $X$  can be introduced into the model in a straightforward way.

The key identifying assumption for this difference-in-differences model is that treatment and control group follow the same time trend in absence of the treatment. To test whether this assumption is plausible, we investigate the trends of treatment and control group in the pre-treatment period. Yet, even if the trends are very similar prior to the treatment, the common trend assumption would be violated if any other policy change occurred in the post-treatment period which affected treatment and control group systematically different. In 2007, the federal government introduced a parental leave reform, which essentially decreased the duration of parental leave benefits from 24 to 12 months while increasing the amount of the benefit. Since this federal reform applied to all German counties at the same time, it should not confound our estimates as long as the effects of the reform do not systematically vary between counties of the treatment group and counties of the control group. Apart from the parental leave reform, we are not aware of any other family policy which might potentially affect fertility. Note further that we do not necessarily need the assumption that the treatment was randomly assigned to the treat-

ment group counties. However, the counterfactual post-treatment trends must also be equal, which is not testable. In this respect, a violation of this assumption would be likely if the treatment and the control group were very different on observable characteristics. We show later that this is not the case.

Even if the counties with above median child care expansion were different from the control group counties, we would identify an Average Treatment Effect on the Treated (ATT).

The main specification in this paper, however, is a more generalized fixed-effects framework that allows for unobserved time-invariant differences between counties. The respective regression equation can be written in the following way:

$$y_{ct+1} = \eta_c + \mu_t + X'_{ct}\lambda + \rho d_{ct} + \zeta_{ct+1} \quad (3)$$

where  $\eta_c$  is a county fixed effect for county  $c$  and thus captures all time-invariant heterogeneity between counties,  $\mu_t$  is a year fixed effect for year  $t$ , and  $X'_{ct}$  is a vector of covariates of county  $c$  that vary over time  $t$ . The variable  $d_{ct}$  represents the public child care coverage rate of county  $c$  in year  $t$ . Accordingly,  $\rho$  captures the effect of public child care coverage on fertility. As before,  $y_{ct+1}$  is our outcome variable measuring the number of births per 1,000 women aged 15 to 44 living in county  $c$  in period  $t + 1$ .

Note that in this fixed-effects specification, identification comes from within-county variation in public child care coverage over time. Consequently, the key identifying assumption in this framework is that there are no unobserved characteristics of a county that vary over time and are correlated with the variable of interest  $d$  and the outcome variable. One potential confounding factor could be changes in predicted fertility at the local level. As we have outlined above, virtually all West German counties had to massively expand public child care provision following the federal child care policy initiatives. Therefore, it does not seem reasonable to argue that changes in predicted fertility over time should affect local governments' decisions whether to increase public child care coverage or not. Even if counties which have always had high levels and predictions of fertility expand public child care more quickly, our county-fixed effects exclude this as a potential confounding factor. However, it might well be that the pace of expansion is affected by changes in predicted fertility within a county over time. In other words, although predicted child care demand exceeds current supply in virtually all counties, the political pressure to quickly increase child care supply might be higher in counties where predicted fertility (or closely related, child care demand) is exceptionally high. Therefore, it seems essential to control for time-varying local socio-demographic factors which might predict fertility and which might therefore also be relevant for local politicians. To this end, we particularly include detailed information on a county's population age structure, population density, economic wealth, and labor market conditions. However, apart from these socio-demographic variables, we argue that the variation in the mid to late 2000s child care expansion can mainly be ascribed to idiosyncratic shocks to the administrative process. Given the range of county specific administrative difficulties mentioned in section 3, it seems not implausible that the public child care expansion in the mid to late 2000s was far from being a systematic reply to local demand. Rather, we assume that conditional on county fixed effects and our time-varying socio-demographic variables, public child care expansion is orthogonal to future fertility rates. And indeed, very similar arguments have

Table 1: Child care coverage over time

Year	N	Child care coverage				
		Mean	S.D.	Median	Min	Max
1998	326	0.017	0.026	0.009	0.000	0.322
2002	326	0.023	0.029	0.015	0.000	0.358
2006	326	0.074	0.042	0.068	0.010	0.378
2007	326	0.095	0.047	0.085	0.022	0.398
2008	326	0.118	0.050	0.109	0.033	0.405
2009	326	0.143	0.052	0.135	0.037	0.416
Total	1956	0.078	0.063	0.071	0.000	0.416

*Notes:* The figures show mean child care coverage rates across West German counties as well as standard deviations, median, minimum, and maximum values. All information is provided for the years 1998, 2002, 2006, 2007, 2008, and 2009.

already been laid out by Felfe and Lalive (2012), who exploit this exact public child care expansion to identify causal effects on child outcomes.

## 5. Data on Child Care Coverage and Fertility

For our empirical analysis, we draw on administrative data from the Statistical Offices of the German Länder (*Statistische Landesämter*) and the Federal Employment Agency (*Bundesagentur für Arbeit*) aggregated on the county level (*Landkreise*). Using these data, we create a balanced panel of all 326 West-German counties covering the period from 1998 to 2010. A detailed description of the data and variable definitions can be found in the data appendix.

Public child care data for children less than three years old is available for 1998, 2002, 2006, 2007, 2008, and 2009.<sup>5</sup> We define our variable of interest, public child care coverage, as public child care slots over the population of children less than three years old. Public child care is reported in the first half of March. Table 1 shows that child care coverage averages 7.8 percent over the whole period of observation. It varies widely from 0 to 41.6 percent. Public child care coverage was very low in 1998 and 2002. The average over the 326 counties is only 1.7 in 1998 and 2.3 percent in 2002. The modest increase is mainly explained by a decrease in births rather than an increase in public child care slots (DJI, 2005). The median county only has 1.5 percent public child care coverage. Yet, there is already large variation across counties with some reporting no child care for under three year olds at all and some counties with quotas up to 32.2 percent. Thus, counties start off with different levels of child care coverage before the reforms were implemented. After 2005, the reform takes effect and the rise in public child care coverage accelerates. In 2006, child care coverage reaches 7.4 percent on average; the median county shows a quota of 6.8 percent. The minimum value is lifted above zero and the maximum value up to 37.8 percent. Until 2009, the average child care coverage is doubled to a value of

<sup>5</sup>Data for 2010 is not used, as our dependent variable, which is forwarded by one year, is not available for 2011.

14.3 percent, the median reaches 13.5 percent and even the least successful county shows a quota of 3.7 percent. Note that while the whole distribution of child care coverage shifted to the right, we do not observe a convergence process between counties; rather, the standard deviation of public child care coverage increased from 1998 to 2009. As we argued earlier, differences in the pace of public child care expansion are affected by idiosyncratic shocks to the local administrative process. Thus, on the county level, public child care expansion is not as smooth as the aggregate statistics might suggest. There are counties that constantly increase the coverage over time, but there are all kinds of patterns as shown in Figures A.1 and A.2 in the Appendix. The expansion pattern can be linear over time, spiked in the first year after the reform or any year thereafter, it can start off strong but come to a halt or be delayed by a couple of years and then increase steeply or it can simply be erratic. Some counties expand very slowly and some very fast.

Our dependent variable births per 1,000 women is calculated as the sum of births over 1,000 women in reproductive age, i.e., between 15 and 44 years. This formulation makes sure that our results are not confounded by changes in the size of the relevant female population. We measure births in year  $t$ , i.e., in the same year as our independent variables, and in year  $t + 1$ . The reason is that our main variable of interest, public child care coverage, is measured in the first half of March each year, while births (as the sum of births in a year) are measured on 31st December. By allowing fertility to respond in  $t$  and in  $t + 1$ , we allow for a conception and gestation lag of maximum 22 months. We empirically validate the suitability of this specification with respect to timing by running regressions with different lags and leads in the outcome (see Figure 4).<sup>6</sup> As can be seen from Table 2, births per 1,000 women in  $t$  average at 44.196, with a standard deviation of 4.402. Figures range from 28.000 in the lowest fertility county-year observation to 64.072 in the highest fertility county-year observation. For births per 1,000 women in  $t + 1$ , figures are very similar with an average of 44.163 births per 1,000 women.

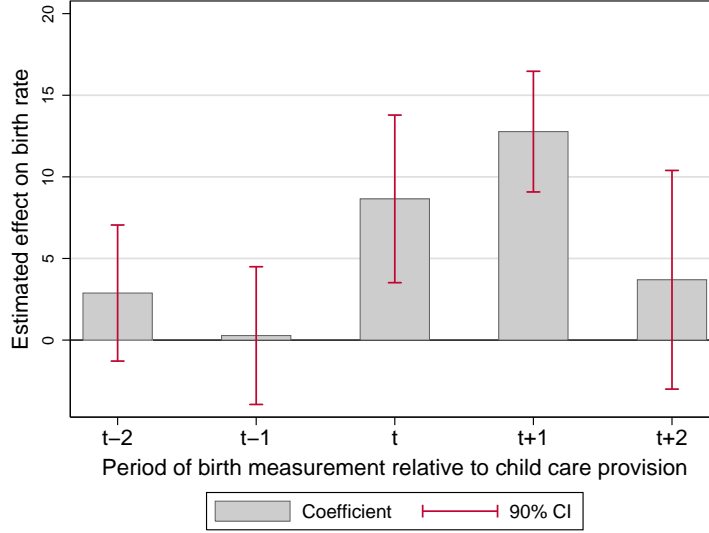
Age-specific fertility rates differ substantially—fertility of 25- to 29-year-olds is much higher than that of women over 40—such that changes in the composition within the population of 15 to 44 year old women affect our outcome variable, births over 1,000 women in reproductive age. Therefore, in all regressions, we include the share of women aged 15-19, 20-24, 25-29, 30-34, 35-39 and 40-44 in all women aged 15-44 within a county. As Table 2 shows, younger cohorts are typically smaller than older cohorts, which reflects the declining fertility rates since the mid-1960s.

Further control variables in our regressions are the county's population density, GDP per capita, male employment rate, and shares of older cohort in the population. Population density is defined as total population divided by surface area in square kilometers. Counties differ much in population density ranging from 40 to over 4,000 people per square kilometer; on average, the population of one square kilometer amounts to 576 people. Male employment rate is defined as the total number of male employees subject to social insurance contribution at place of residence divided by male working age population, with working age being 20 to 64 years. Male employment rate averages at 60.4 percent with a standard deviation of 0.06. The lowest observed male employment rate is 40.6 percent, while the highest one is 73.7 percent. GDP per capita is defined as GDP divided by a county's total population. It averages at 28,030 Euro per capita, ranging

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<sup>6</sup>A detailed description of the methods can be found in the Appendix.

Figure 4: Timing of the dependent variable



*Notes:* The bars indicate the effect of child care coverage in period  $t$  on births per 1,000 women in the period according to the x-axis. Red lines indicate 90-percent confidence intervals. All five regressions are independently estimated using the fixed-effects approach. Control variables including child care coverage are lagged by one year and are included in period  $x-1$ . Only for  $x-1=t$  the control variable child care coverage is identical to the variable of interest child care coverage in period  $t$ .

from 11,000 to 86,000. Further, we also control for population shares of groups aged 45-49, 50-54, 55-59, 60-64, 65-74, and 75 and older. The shares are defined as the population in the specific age group over the total population in that county. These population age group shares are supposed to control for possible changes in the suitability and tailoring of counties' policies for senior citizens.

In later robustness checks, we use gross migration flows in  $t + 1$  divided by a county's total population in  $t$  as dependent variables. This variable is not available for the year 1998, which reduces the sample size by 326 observations. The numbers indicate that women migrating across county borders represent about 0.8 respectively 1.4 percent of the population. We also use the male and female commuters as a share of the total population in further robustness checks. On average, commuting females account for 5.3 percent and male commuters for 8.4 percent of a county's population.

## 6. Results

### 6.1. Difference-in-differences results

Before we analyze the development of public child care and fertility in treatment and control group over time, we elaborate on some descriptive statistics. In particular, we compare West German counties with above median increase in the period from 2002 until 2009 (treatment group) with West German counties with below median increase in the period from 2002 until 2009 (control group) along several dimensions measured prior to the federal policy initiatives that triggered public child care expansion. Table 3 depicts means of dependent and independent variables for the two groups in the pre-treatment year 2002, as well as t-tests for differences in means. As for the basic variables, child care coverage is

Table 2: Descriptive statistics

Variable	N	Mean	S.D.	Min	Max
<i>Dependent variables</i>					
Births per 1,000 women (t)	1956	44.196	4.402	28.000	64.072
Births per 1,000 women (t+1)	1956	44.163	3.979	29.890	65.440
<i>Control variables</i>					
Share of women 15-19 <sup>1</sup>	1956	0.145	0.020	0.084	0.186
Share of women 20-24 <sup>1</sup>	1956	0.144	0.020	0.107	0.272
Share of women 25-29 <sup>1</sup>	1956	0.149	0.019	0.114	0.223
Share of women 30-34 <sup>1</sup>	1956	0.161	0.025	0.121	0.232
Share of women 35-39 <sup>1</sup>	1956	0.192	0.017	0.109	0.250
Share of women 40-44 <sup>1</sup>	1956	0.211	0.024	0.137	0.276
Population density	1956	575.626	712.438	40.720	4286.211
Employment rate (m)	1956	0.604	0.060	0.406	0.737
GDP per capita (in 1,000)	1956	28.030	10.797	11.238	86.079
Population fraction 45-49	1956	0.078	0.008	0.057	0.095
Population fraction 50-54	1956	0.067	0.007	0.041	0.085
Population fraction 55-60	1956	0.062	0.007	0.038	0.090
Population fraction 60-64	1956	0.056	0.008	0.036	0.081
Population fraction 65-74	1956	0.107	0.014	0.067	0.148
Population fraction 75+	1956	0.083	0.013	0.046	0.127
Gov income	1610	383.213	452.072	56.630	5775.025
Gov debt	1932	0.214	0.279	0.000	3.335
New premises	1956	0.597	0.735	0.008	15.520
<i>Robustness dependent variables</i>					
In-migrants 18-29, fem., (t+1)	1630	0.009	0.005	0.003	0.049
In-migrants 18-49, fem., (t+1)	1630	0.014	0.007	0.006	0.096
Out-migrants 18-29, fem., (t+1)	1630	0.008	0.003	0.004	0.047
Out-migrants 18-49, fem., (t+1)	1630	0.014	0.005	0.006	0.095
Commuter (out), fem., (t+1)	1753	0.053	0.023	0.011	0.126
Commuter (out), mal., (t+1)	1753	0.084	0.031	0.021	0.175

*Notes:* The table shows descriptive statistics (number of observations, mean, standard deviation, minimum, and maximum) on the county level aggregated over all waves used in the estimations. The figures show aggregated values over the years 1998, 2002, 2006, 2007, 2008, and 2009. Accordingly, all variables measured in (t+1) are aggregated over the years 1999, 2003, 2007, 2008, 2009, and 2010. Debt and income of municipalities are not reported for the federal city states Berlin, Hamburg and Bremen (including Bremerhaven). Income information is missing in 2001 from all 15 Schleswig-Holstein counties. Income information in 2009 is not included due to fragmentary raw data. <sup>1</sup> Denotes women in the particular age group as the share of all women aged 15 to 44.

statistically indifferent between the groups prior to the treatment, whereas the birth rate is significantly lower in the treatment group. Interestingly, treatment and control group are statistically indistinguishable along dimensions such as population density, male employment rate, GDP per capita, government income, government debt, and newly build premises. Out of twelve variables that measure the age structure of the population and particularly of women in reproductive age, four are statistically different between treatment and control group. However, also note that the absolute and relative size of these differences are tiny. Thus, even if we should include all these observable characteristics as covariates in our regressions, the general picture we get from Table 3 is compatible with the expressed view that child care expansion did not happen in a predictable, systematic way but was subject to various idiosyncratic shocks to the administrative process.

Table 3: Pre-treatment descriptives for treatment and control group

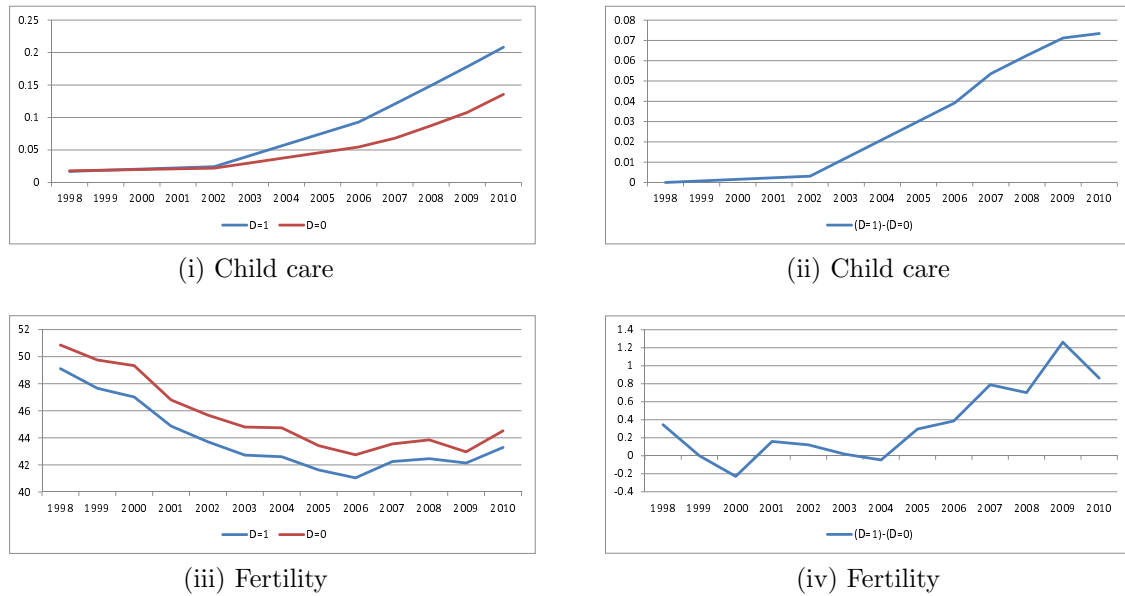
Variable	Mean Control	Mean Treatment	Mean-Diff. (T-C)	T-test t-stat      p-value	
<i>Child care</i>					
Child care coverage	0.022	0.024	0.002	-0.685	0.494
<i>Dependent variables</i>					
Births per 1,000 women (t)	45.676	43.706	-1.970	5.142	0.000
Births per 1,000 women (t+1)	44.797	42.724	-2.073	5.661	0.000
<i>Control variables</i>					
Share of women 15-19 <sup>1</sup>	0.138	0.132	-0.006	3.456	0.001
Share of women 20-24 <sup>1</sup>	0.139	0.140	0.001	-0.671	0.503
Share of women 25-29 <sup>1</sup>	0.136	0.136	0.000	-0.273	0.785
Share of women 30-34 <sup>1</sup>	0.179	0.179	0.000	0.184	0.854
Share of women 35-39 <sup>1</sup>	0.210	0.213	0.003	-2.245	0.025
Share of women 40-44 <sup>1</sup>	0.198	0.200	0.001	-1.053	0.293
Population density	610.878	540.916	-69.963	0.889	0.375
Employment rate (m)	0.603	0.600	-0.003	0.493	0.622
GDP per capita (in 1,000)	25.363	26.956	1.593	-1.449	0.148
Population fraction 45-49	0.070	0.071	0.001	-2.794	0.006
Population fraction 50-54	0.064	0.065	0.001	-1.962	0.051
Population fraction 55-60	0.053	0.052	0.000	0.492	0.623
Population fraction 60-64	0.067	0.066	0.000	0.707	0.480
Population fraction 65-74	0.099	0.097	-0.002	1.997	0.047
Population fraction 75+	0.078	0.078	0.000	0.343	0.732
Gov income	358.914	358.051	-0.863	0.019	0.985
Gov debt	0.220	0.214	-0.006	0.201	0.841
New premises	0.691	0.635	-0.055	0.945	0.345

*Notes:* The table shows means, differences in means and differences-in-means-tests for the control and the treatment group in 2002, the pre-treatment period. The last two columns depict results of T-tests for equality in means for each variable as t-statistics and p-values. Debt and income of municipalities are not reported for the federal city states Berlin, Hamburg and Bremen (including Bremerhaven). Income information is missing in 2001 from all 15 Schleswig-Holstein counties. Income information in 2009 is not included due to fragmentary raw data. <sup>1</sup> Denotes women in the particular age group as the share of all women aged 15 to 44.

We now turn to Figure 5 which shows the increase in public child care coverage and the development of fertility for our treatment and control group counties. As we can see from the upper left panel (i) of Figure 5, public child care coverage was at a low level of not even 2 percent for both groups of counties in 1998. From 1998 to 2002, we observe hardly any dynamics; the slight rise in coverage rates is rather due to a decreasing number of births than due to an increasing number of public child care slots. As outlined in Section 3, the federal public child care initiatives started in 2005. However, unfortunately, there are no data available for public child care in the period from 2003 to 2005. Yet, by 2006, child care coverage had increased from a mere 2 percent to 5 percent in the control group and

9 percent in the treatment group. By 2010, public child care coverage was 21 percent in treatment counties and 14 percent in control counties. Thus, while treatment and control counties started out from the same low level of child care coverage in 2002, trends have diverged since then and the difference in the coverage rate has increased to more than 7 percentage points in 2010. The upper right panel (ii) of Figure 5 graphically depicts this increase in the difference between treatment and control counties. Again, note that we do not have data on public child care coverage for the years 2003 to 2005. Thus, the graph should not be mistaken as evidence for a starting divergence in public child care trends already before 2006.

Figure 5: Child care and fertility in treatment and control group



*Notes:* The figures show averages of the treatment group ( $D=1$ ) and the control group ( $D=0$ ) and the difference between the treatment and the control group ( $(D=1)-(D=0)$ ) over time. The treatment group consists of all counties with above median increase in child care coverage rates from 2002 until 2009, whereas the control group consists of counties with below median increase in child care coverage rates from 2002 until 2009. The figure in the upper left panel (i) depicts child care coverage over time, separately for treatment and control group. The figure in the upper right panel (ii) depicts the difference between the treatment and the control group in child care coverage over time. The figure in the lower left panel (iii) depicts births per 1,000 women over time, separately for treatment and control group. The figure in the lower right panel (iv) depicts the difference between the treatment and the control group in births per 1,000 women over time.

Turning to the lower left panel (iii) of Figure 5, we see that, for both groups of counties, the number of births per 1,000 women aged 15 to 44 generally decreased from 1998 (49.1; 50.9) until 2006 (41.0; 42.7) whereas there is a slight upward movement from 2006 until 2010 (43.3; 44.5). Over the whole period of observation, the treatment group counties show lower numbers of births than the control group counties. However, the difference between treatment and control group becomes smaller over time as the treatment group slowly approaches the control group level. Normalizing the difference in births per 1,000 women in 1999 to zero, the lower right panel (iv) of Figure 5 depicts the dynamics of this difference over time. As can be seen, the difference in fertility between treatment and control group does not systematically change from 1998 until 2004. However, the number of births in the treatment group gradually increases as compared to the number of births in the control group starting in 2005, which is exactly the year of the first federal

policy initiative to increase public child care provision. By 2010, the number of births has increased by 0.86 births more in the treatment group than the number of births in the control group. Thus, taken together, these graphs are compatible with the hypothesis that public child care coverage increases fertility.

Table 4: Difference-in-differences estimates

	Child care coverage		Birth rate		Birth rate	
	$t$		$t$		$t+1$	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment group x Post-treatment	0.068*** (0.003)	0.064*** (0.003)	1.141*** (0.309)	1.293*** (0.308)	0.845*** (0.310)	0.895*** (0.312)
Treatment group	0.002 (0.003)	-0.002 (0.002)	-1.970*** (0.383)	-0.917*** (0.266)	-2.073*** (0.366)	-1.127*** (0.267)
Post-treatment	0.086*** (0.001)	0.019 (0.013)	-2.709*** (0.215)	5.819*** (1.100)	-0.280 (0.221)	6.498*** (1.149)
$R^2$	0.781	0.852	0.140	0.594	0.066	0.508
Treatment group x Post-placebo (2002)	0.003** (0.001)	0.003 (0.002)	-0.223 (0.280)	0.195 (0.313)	0.018 (0.296)	0.361 (0.330)
Treatment group	-0.001 (0.003)	-0.004* (0.002)	-1.746*** (0.482)	-0.603* (0.323)	-2.091*** (0.453)	-1.173*** (0.300)
Post-placebo (2002)	0.004*** (0.001)	-0.011 (0.008)	-5.184*** (0.198)	4.877*** (1.102)	-4.955*** (0.183)	4.872*** (1.135)
$R^2$	0.011	0.448	0.339	0.725	0.344	0.700
Regional controls	No	Yes	No	Yes	No	Yes
N	652	652	652	652	652	652

*Notes:* The table shows results of difference-in-differences estimations where the treatment group consists of all counties with above median increase in child care coverage rates from 2002 until 2009. The control group consists of counties with below median increase in child care coverage rates from 2002 until 2009. In columns (1) and (2), first stage results for the treatment size are depicted with child care coverage as the dependent variable. Columns (3) to (6) show the treatment effects on births over 1,000 women in period  $t$  and  $t+1$ . In the upper panel, the baseline period is the year 2002 whereas the post-treatment period is the year 2009. In the lower panel, the baseline period is the year 1998 whereas the placebo post-treatment period is the year 2002. Regional control variables include the county's population density, GDP per capita, the male employment rate as well as an extensive set of age structure controls. Age structure control variables include the age composition of the 15 to 44 year old women as 5-year interval shares (15-19, 20-24, 25-29, 30-34, 35-39, 40-44 over all women aged 15-44 with 25-29 as reference category) as well as the shares of the population over 44 (45-49, 50-54, 55-59, 60-64, 65-74, 75 and older, over the whole population). Robust standard errors are clustered at the county level and given in parentheses. \*\*\* 1 percent significance level; \*\* 5 percent significance level; \* 10 percent significance level.

In a next step, we bring these graphs to a multivariate difference-in-differences framework using the year 2002 as the pre-treatment year and the year 2009 as the post-treatment year, while measuring the outcome variable in the year  $t$  and  $t+1$ . In column 1 of Table 4, we see that counties of the treatment group had on average 1.970 fewer births per 1,000 women than counties of the control group in 2002. Further, the coefficient on the post-treatment dummy shows that the number of births per 1,000 women in the control group decreased by 2.709 from 2002 to 2009. Most importantly, the coefficient on the interaction term depicts the positive treatment effect observed in Figure 5. Compared to the control group, the number of births per 1,000 women in the treatment group increased by 1.141 more from 2002 to 2009. This effect turns out to be statistically significant at the 1 percent level. Including regional control variables such as population density, male employment rate, and GDP per capita as well as controls for the age composition of women in reproductive age and of the total population even slightly increases the treatment effect (see column 2 of Table 4). To interpret the treatment effect, we have to take into consideration that the treatment size, i.e., the difference in the increase of child care

coverage between treatment and control group between 2002 and 2009, corresponds to 6.8 percentage points without controls and 6.4 percentage points with controls, as can be seen in columns 5 and 6. Thus, the difference-in-differences estimate of 1.141 without controls suggests that a 10 percentage point increase in public child care coverage increases the number of births per 1,000 women by 1.7 births or by 4.1 percent.<sup>7</sup>

Remember that public child care coverage is measured in the first half of March of the respective year while the number of births per 1,000 women covers the period from 1st of January until 31st of December of the respective year. Yet, individuals might not be immediately aware of increases in public child care coverage. Further, even if individuals are aware, they might not react immediately to changes in public child care provision. Moreover, even if they do, women might not be successful in getting pregnant within a short period. For these reasons, we forward our outcome variable by one year, which means that we allow for a conception and gestation lag of up to 22 months after public child care coverage is observed, which is in line with the literature (see, e.g., Rindfuss *et al.* (2010)). In columns 3 and 4 of Table 4, we estimate the effect of child care coverage in year  $t$  on birth rate in year  $t + 1$ . The estimated effect turns out to be slightly smaller in year  $t + 1$  than in year  $t$  but it is still significantly different from zero at the one percent level. Further, the coefficient is virtually not affected by including our regional control variables. In regressions which are available from the authors upon request, we additionally control for municipalities' debts and the number of newly build premises in a county.<sup>8</sup> Our results remain virtually unaffected by this specification.

The key identifying assumption of our difference-in-differences approach is that the trends in the outcome variable are the same for treatment and control group in absence of the treatment. In order to provide evidence for the plausibility of this assumption, we run placebo treatment difference-in-differences estimations in the pre-treatment years. In particular, we use the year 1998 as the pre-placebo treatment year and the year 2002 as the post-placebo treatment year. The lower panel of Table 4 presents the results of this placebo treatment test. In columns 1 and 2, we see that there is hardly any difference in child care expansion between treatment and control group in the pre-treatment period. Although the coefficient is marginally significant in the specification without any further covariates (column 1), its size is tiny. Once we introduce our regional control, the coefficient loses significance (column 2). This result confirms our idea that the placebo is in fact no treatment in the sense of child care expansion. Columns 3 and 5 of Table 4 provide evidence that treatment and control group indeed follow the same trend in fertility in the pre-treatment period. Including our regional control variables in columns 4 and 6 does not affect this result. Taken together, this placebo treatment exercise corroborates the key identifying assumption of our difference-in-differences approach, namely that treatment and control group follow the same fertility trend in absence of the treatment.

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<sup>7</sup>The percentage increase is computed by taking the counterfactual post-treatment average in the treatment group in case of no treatment as the baseline ( $43.706 - 2.709 = 40.997$ ).

<sup>8</sup>Data on municipalities' income for 2009 are very fragmentary; therefore, we do not include this variable in this robustness check.

Table 5: Fixed-effects estimates

	Birth rate			
	t (1)	t+1 (2)	t (3)	t+1 (4)
Child care coverage	13.213*** (2.738)	12.235*** (2.439)	14.179*** (3.160)	14.264*** (2.808)
Population density	-0.018*** (0.004)	-0.018*** (0.003)	-0.021*** (0.004)	-0.021*** (0.004)
Employment rate (m)	-1.998 (6.433)	-2.212 (5.781)	0.752 (6.969)	-1.764 (6.235)
GDP per capita	0.063 (0.039)	0.054 (0.035)	0.042 (0.046)	0.051 (0.035)
Income, debt, premises	No	No	Yes	Yes
Age controls	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
N	1,956	1,956	1610	1610
Number of counties	326	326	322	322
F-statistic	181.6	154.7	168.7	142.1

*Notes:* The table shows the results of county fixed-effects estimations. The outcome variable births per 1,000 women aged 15 to 44 is measured in period t columns (1) and (3), and forwarded by one period in columns (2) and (4). Age control variables include the age composition of the 15 to 44 year old women as 5-year interval shares (15-19, 20-24, 25-29, 30-34, 35-39, 40-44 over all women aged 15-44 with 25-29 as reference category) as well as the shares of the population over 44 (45-49, 50-54, 55-59, 60-64, 65-74, 75 and older, over the whole population). Debt and income of municipalities are not reported for the federal city states Berlin, Hamburg and Bremen (including Bremerhaven). Income information is missing in 2001 from all 15 Schleswig-Holstein counties. Income information in 2009 is not included due to fragmentary raw data. Robust standard errors are clustered at the county level and given in parentheses. \*\*\* 1 percent significance level; \*\* 5 percent significance level; \* 10 percent significance level.

## 6.2. Fixed-effects results

Now, we turn to our more generalized county fixed-effects model which allows for time-invariant unobserved heterogeneity between counties. Apart from county and year fixed effects, all regressions include as control variables a county's population density, male employment rate and GDP per capita, as well as a county's age composition of the population and in particular among the reproductive women. Again, we report estimates for the effect of public child care coverage on births in year  $t$  as well as in year  $t+1$ . In column 1 of Table 5, we observe that increases in population density are negatively associated with the birth rate, while the coefficients on male employment rate and GDP per capita turn out to be insignificant. Most importantly, also the fixed effects specification confirms our result that public child care coverage positively affects births per 1,000 women in year  $t$ . The coefficient suggests that introducing full provision of public child care increases the number of births per 1,000 women by 13.213. Assuming linearity, this means that a 10 percentage point increase in public child care coverage leads to an increase in births per 1,000 women of 1.32. Thus, the fixed effects model yields an effect of public child care on fertility which is slightly smaller than the effect identified in the difference-in-differences model; however, the effect again turns out to be highly significant and very precisely estimated. In column 2 of Table 5, the effect of child care coverage in year  $t$  on the birth rate in year  $t+1$  is estimated to be 12.235; this precisely estimated coefficient suggests that a 10 percentage point rise in public child care coverage yields an increase in the birth rate of 1.22.

Unobserved time-varying factors that correlate with both child care expansion and fertility are potential threats to identification. Apart from the regional characteristics included as covariates, further natural candidates seem to be potential determinants of child care expansion such as municipalities' debts and income as well as the number of newly build premises in a county. Unfortunately, the debt and income variables are not available for the city states of Berlin, Hamburg, and Bremen; moreover, information on municipalities' income is missing for all counties in the state of Schleswig-Holstein in 2001 and we do not have income information from the year 2009. Still, we introduce these additional variables in columns 3 and 4 of Table 5. The effect of public child care coverage on the birth rate stays highly significant and positive. Indeed, the point estimates of 14.179 in  $t$  and 14.264 in  $t+1$  are very similar to the earlier estimates.

We investigate the heterogeneity of our results across a number of regional characteristics. Note that in the absence of individual-level data, we should be cautious with the interpretation of these heterogeneity analyzes. This is because we cannot infer from regional level characteristics that the heterogeneity is actually driven by individuals that carry the respective characteristics (*ecological fallacy*). However, comparing the effects across different samples of counties might still yield interesting insights. In the first row of Table 6, we report the baseline fixed-effects estimate on the full sample for comparison. The heterogeneity results in the following rows are derived from separate fixed-effects regressions for county subgroups and depict the public child care coefficient on births per 1,000 women in period  $t+1$ . Counties are divided at the median of the respective county characteristics, with the median counties being included in the above median group.<sup>9</sup>

Table 6 shows that we do not find any effect heterogeneity with respect to population

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<sup>9</sup>Thresholds are calculated as the medians of county-specific means over time.

Table 6: Effect heterogeneity in fixed-effects estimates

	Effects on birth rate in t+1					F-stat
	Child care coverage (1)	Regional controls (2)	Year dummies (3)	N (4)	Counties (5)	
<i>Baseline</i>	12.235*** (2.439)	Yes	Yes	1,956	326	154.7
<i>Population density</i>						
Above median	10.256*** (3.437)	Yes	Yes	978	163	47.2
Below median	10.939*** (3.569)	Yes	Yes	978	163	143.8
<i>Employment rate (m)</i>						
Above median	6.273* (3.245)	Yes	Yes	978	163	112.6
Below median	10.496*** (3.551)	Yes	Yes	978	163	67.3
<i>GDP per capita</i>						
Above median	16.787*** (3.709)	Yes	Yes	978	163	50.3
Below median	6.416** (2.804)	Yes	Yes	978	163	141.6
<i>Catholic ratio</i>						
Above median	10.577*** (3.597)	Yes	Yes	972	162	91.6
Below median	11.426*** (3.567)	Yes	Yes	972	162	87.3
<i>Conservative vote share</i>						
Above median	8.096** (3.483)	Yes	Yes	978	163	93.8
Below median	13.001*** (3.730)	Yes	Yes	978	163	93.4
<i>Higher secondary grad share</i>						
Above median	12.437*** (3.728)	Yes	Yes	978	163	55.0
Below median	10.018*** (3.314)	Yes	Yes	978	163	139.2

*Notes:* The table shows the results of county fixed-effects estimations. The outcome variable births per 1,000 women aged 15 to 44 is measured in period t+1. Regional control variables include the county's population density, GDP per capita, the male employment rate as well as an extensive set of age structure controls. Age structure control variables include the age composition of the 15 to 44 year old women as 5-year interval shares (15-19, 20-24, 25-29, 30-34, 35-39, 40-44 over all women aged 15-44 with 25-29 as reference category) as well as the shares of the population over 44 (45-49, 50-54, 55-59, 60-64, 65-74, 75 and older, over the whole population). Medians are defined as median observation of county-means over time. The above median group includes the median county. Estimates in rows are from independent regressions. Data on the Catholics share in 1987 is missing for Hannover and Berlin. Robust standard errors are clustered at the county level and given in parentheses. \*\*\* 1 percent significance level; \*\* 5 percent significance level; \* 10 percent significance level.

density. However, the effect seems to be stronger for counties with below median male employment rates, which seems to contradict the next finding that the effect is also stronger for counties with above median GDP per capita. One explanation for this pattern could be non-linear effects of public child care with respect to a county’s economic wealth. However, we prefer the interpretation that the effect is strongest in wealthy counties with a large fraction of high-skilled individuals (such as university students). Since employment rates are defined as the number of employed individuals over the total population of 20 to 64 year olds, counties with a large number of university students have lower employment rates *ceteris paribus*. This interpretation is also supported by the fact that the effect is large in counties with a larger share of higher secondary school graduates.<sup>10</sup> Thus, it seems that the public child care is of higher concern for fertility decisions in wealthier counties with a high-skilled population. One reason could be that labor market attachment and career concerns are stronger for high skilled individuals, which makes child care coverage more important for fertility decisions.

Attitudes of a county’s population do not change rapidly over our short period of observation and should therefore be picked up by our county fixed effects. However, they could be a strong mediating factor for the effect of public child care coverage on fertility. Attitudes shape the acceptance of maternal employment and formal child care for young children. If maternal employment and formal child care are less accepted in a society, public child care may play a minor role for fertility decisions. We use a county’s share of Catholics in 1987 and a county’s conservative vote share in the previous federal state elections as proxies for these cultural attitudes. Indeed, our results suggests that the effect of public child care on fertility tends to be larger in counties with a low share of Catholics and a smaller conservative vote share.

In sum, our fixed effects results suggest that public child care coverage can indeed positively affect fertility. While we do not find any heterogeneity with respect to population density, the effect tends to be larger in wealthy counties with a large share of highly educated individuals and less conservative cultural attitudes (measured by conservative vote shares and the ratio of Catholics).

## 7. Robustness checks

### 7.1. *Selective migration*

Although we control for unobserved time-invariant heterogeneity and include several time-varying control variables in our county-fixed effects framework, selective migration of to-be-mothers might confound our estimates. In particular, if couples or women who are pregnant or plan to have a child systematically move to counties that increase public child care coverage, our fixed effects regressions would yield upward biased estimates of the causal effect of public child care coverage on fertility. However, also note that movements to counties that have always been known for family friendly infrastructure do not confound our estimates as long as this kind of selective migration is constant over time. Similarly, our results are not confounded by the movement of couples or single

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<sup>10</sup>Graduating from a higher secondary school (A-level equivalent) provides admission to universities. The share is computed by taking the higher secondary school graduates over all graduates in that year.

mothers with a baby since our outcome variable does not measure the number of under three year olds but the actual number of births in a county.

If an increase in public child care coverage can indeed redirect migration flows, this should show up in higher gross in-migration flows or lower gross out-migration flows in counties that increase public child care coverage. In order to test whether this is the case, we run our fixed-effects regressions using the ratio of female in-migrants aged 18 to 29 in  $t + 1$  over the total population in  $t$  as the outcome variable (row 1 of Table 7). As an alternative outcome variable, we use the ratio of female in-migrants aged 18 to 49 in  $t + 1$  over the total population in  $t$  as the outcome variable (row 2 of Table 7). We do not find any effects of public child care coverage on gross in-migration of women of reproductive age. Similarly, we do not find any effect of child care coverage on gross out-migration in rows 3 and 4 of Table 7. Thus, these fixed-effects regressions do not provide any evidence for the kind of selective migration which would confound our estimates.<sup>11</sup>

Table 7: Fixed-effects estimates on gross migration flows and commuting

	Effects on birth rate in t+1					
	Child care coverage (1)	Regional controls (2)	Year dummies (3)	N (4)	Counties (5)	F-stat (6)
<i>Dependent var</i>						
In-migrants 18-29 per pop, female (t+1)	0.002 (0.001)	Yes	Yes	1,630	326	10.7
In-migrants 18-49 per pop, female (t+1)	0.002 (0.002)	Yes	Yes	1,630	326	10.8
Out-migrants 18-29 per pop, female (t+1)	-0.000 (0.001)	Yes	Yes	1,630	326	41.8
Out-migrants 18-49 per pop, female (t+1)	-0.000 (0.001)	Yes	Yes	1,630	326	19.9
Commuter (out) per pop, female (t+1)	0.011*** (0.004)	Yes	Yes	1,630	326	250.1
Commuter (out) per pop, male (t+1)	-0.006 (0.006)	Yes	Yes	1,735	326	97.7

*Notes:* The table shows the results of county fixed-effects estimations. The outcome variables are denoted in rows. Regional control variables include the county's population density, GDP per capita as well as the male employment rate. Age control variables include the age composition of the 15 to 44 year old women as 5-year interval shares (15-19, 20-24, 25-29, 30-34, 35-39, 40-44 over all women aged 15-44 with 25-29 as reference category) as well as the shares of the population over 44 (45-49, 50-54, 55-59, 60-64, 65-74, 75 and older, over the whole population). Estimates in rows are from independent regressions. Robust standard errors are clustered at the county level and given in parentheses. \*\*\* 1 percent significance level; \*\* 5 percent significance level; \* 10 percent significance level.

We further investigate the issue of selective migration by using administrative data on commuting incidences. If the motivation for choosing the place of residence was driven by increasing child care coverage rather than job opportunities, we would expect more people to commute out of the county with high public child care expansion. This is because the place of residence determines the eligibility for public child care slots but

<sup>11</sup>We also estimated our basic fixed-effects models on the level of 75 more aggregated regions (*Raumordnungsregionen*) instead of counties using the number of births per 1,000 women of reproductive age as the outcome variable. The intuition is that selective migration due to increases in public child care coverage is less likely between these larger regions than between counties. These fixed-effects models yield effects very similar to the county fixed-effects models. Detailed results are available from the authors upon request.

not the workplace. We run fixed effects regressions using the share of female and male commuters over the total population as the outcome variable. The results depicted in rows 5 and 6 of Table 7 show that public child care coverage indeed increases the share of female commuters, yet not the share of male commuters. If there was selective choice of families' residence with respect to public child care expansion, we would expect an equal effect on commuting patterns between the sexes as it affects the couple as a whole. This is not the case; rather, the commuting pattern is compatible with a story where public child care increases maternal employment, which results in a higher number of female commuters but does not confound our analysis.

## 7.2. *Tempo effects*

Our measure of fertility, births per 1,000 women, is a period fertility measure. Defined as the sum of all births per women between 15 and 44 multiplied by 1,000, it measures how many children 1,000 women would have assuming age-specific fertility to be constant during the women's reproductive age. Our measure therefore shares most features with the total fertility rate. As such, it is suitable to analyze the effects of policy changes but, in contrast to cohort fertility, which gives the actual number of births per woman measured after the reproductive age, it can be distorted by so-called tempo effects, i.e., by changes in the timing of births.

So far we have argued that the public child care coefficients from our multivariate regressions can be interpreted as providing evidence for positive effects of public child care coverage on the number of births. However, this neglects the fact that the very same coefficients could emerge if couples just decide to have children earlier as a response to public child care expansion but end up with the same total number of children. In other words, such a temporary shift in age at birth would yield a short-term increase in period fertility but no long-term effect on completed fertility.

Therefore, we now look for empirical evidence which allows us to distinguish actual quantum effects from mere tempo effects. To this end, we return to our standard fixed effects regressions and adjust the outcome variable to allow for heterogeneous effects along the age distribution of mothers. In particular, we split the dependent variable in six separate variables where each of them measures the birth rate for a 5-year-interval age group, starting with 15 to 19 year old women up to 40 to 44 year old women. If our results are just a relict of tempo effects, i.e., induced by women getting children earlier in life, we should observe larger effects of public child care on fertility for younger cohorts than for older cohorts of women. Yet, also note that an increase in the total number of children in a cohort is often induced by higher order births, which makes the average age at birth increase as well. Still, this kind of age effect would not be considered as a mere tempo effect. Thus, all results on age at birth should be interpreted with caution.

Table 8 presents county fixed-effects results for the six age-group specific birth rates. Although we find some heterogeneity in the effect of public child care provision on the birth rates over different age groups, there is no indication that the effect is mainly driven by younger cohorts. Remarkably, for women from 25-29 years, the point estimate is positive but insignificant and the lowest of all groups in terms of percent increase. Thus, at prime fertility age, child care coverage seems to play no major role for the fertility decision. On the other hand, women above and below this age group react strongly. Women over

30 years are presumably more attached to the labor market, which might make public child care more important to reconcile work and family life. Another explanation could be that higher order births occurring at higher ages are more responsive to public child care coverage. Unfortunately, our administrative data set does not include information on birth order, which does not allow us to disentangle these two explanations. For the youngest (15-20 years) and oldest (40-44 years) groups, we find small and less precisely estimated coefficients, although in terms of percent change they are larger than in the other groups. However, due to the low incidence of births in these groups the results should not be over-interpreted.

Table 8: Fixed-effects estimates: Effect heterogeneity across age groups

	Birth rate $t+1$					
	aged 15-19 (1)	aged 20-24 (2)	aged 25-29 (3)	aged 30-34 (4)	aged 35-39 (5)	aged 40-44 (6)
Child care coverage	6.051** (2.431)	15.668*** (5.945)	7.886 (7.191)	31.523*** (7.296)	12.094*** (4.566)	3.858** (1.663)
Regional controls	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Percent effect of 10pp care increase	0.060	0.032	0.009	0.036	0.032	0.058
N	1,956	1,956	1,956	1,956	1,956	1,956
Number of counties	326	326	326	326	326	326
F-statistic	37.0	142.1	39.1	40.5	158.2	63.2

*Notes:* The table shows the results of county fixed-effects estimations. The outcome variables are age group-specific fertility rates, i.e., the number of births per 1,000 women aged 15-19 (column 1), 20-24 (column 2), 25-29 (column 3), 30-34 (column 4), 35-39 (column 5), and 40-44 respectively (column 6). All outcome variable are forwarded by one period. Regional control variables include the county's population density, GDP per capita, the male employment rate as well as an extensive set of age structure controls. Age structure control variables include the age composition of the 15 to 44 year old women as 5-year interval shares (15-19, 20-24, 25-29, 30-34, 35-39, 40-44 over all women aged 15-44 with 25-29 as reference category) as well as the shares of the population over 44 (45-49, 50-54, 55-59, 60-64, 65-74, 75 and older, over the whole population). Robust standard errors are clustered at the county level and given in parentheses. \*\*\* 1 percent significance level; \*\* 5 percent significance level; \* 10 percent significance level.

In a second step, we now estimate the effect of child care coverage on mean age at birth, and using this result, we present a tempo-adjusted fertility measure. County fixed-effects regressions of age at birth on public child care coverage yield no indication of women getting their children earlier in life due to child care provision (Table 9). Quite to the contrary, a 10 percentage point increase in public child care coverage even increases the average age at birth by 0.050 in  $t$  and 0.065 in  $t + 1$ , where the latter coefficient is statistically significant at the five percent level. Thus, instead of accelerating individual fertility, public child care expansion rather seems to have inspired older women to have children. It could either be the case that families have an additional higher order child, which makes the mothers older on average, or that older women have a first child they would not have had without the availability of public child care. Either way, the observed increase in fertility does not seem to be the relict of a short-term tempo effect. Rather, we expect public child care to have positive effects on long-term completed fertility. Still, in the absence of more detailed data on fertility, we should not completely exclude the

possibility of tempo effects within age groups.

Table 9: Tempo effects

	Age at birth		Tempo-adjusted birth rate	
	t (1)	t+1 (2)	t (3)	t+1 (4)
Child care coverage	0.501 (0.327)	0.652** (0.297)	17.929 (11.513)	20.646** (9.097)
Regional controls	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
N	1,956	1,956	1,630	1,630
Number of counties	326	326	326	326
F-statistic	68.4	84.6	6.2	10.0

*Notes:* The table shows the results of county fixed-effects estimations. Age at birth is calculated as the mean age of all births at county-year level, where we assume the lower end age as the actual age at birth for each 5-year interval 15-19, 20-24, 25-29, 30-34, 35-39 and 40-44. Tempo-adjusted birth rates are births per 1,000 women divided by one minus the change in age at birth. The latter is defined as the mean of the deviations in mean age at birth from the previous and the following year as in  $\frac{(Age_{it} - Age_{it-1}) + (Age_{it+1} - Age_{it})}{2}$ . Regional control variables include the county's population density, GDP per capita, the male employment rate as well as an extensive set of age structure controls. Age structure control variables include the age composition of the 15 to 44 year old women as 5-year interval shares (15-19, 20-24, 25-29, 30-34, 35-39, 40-44 over all women aged 15-44 with 25-29 as reference category) as well as the shares of the population over 44 (45-49, 50-54, 55-59, 60-64, 65-74, 75 and older, over the whole population). Robust standard errors are clustered at the county level and given in parentheses. \*\*\* 1 percent significance level; \*\* 5 percent significance level; \* 10 percent significance level.

Given that, if at all, we find an increase in the average age at birth induced by public child care, we might wish to adjust our fertility effects. Bongaarts and Feeney (1998) propose an adjustment of the total fertility rate that can easily be applied to our period fertility measure. The adjusted fertility rate is defined as  $\sum(TFR_i/(1 - r_i))$ , where  $TFR_i$  is the total fertility rate of birth order  $i$  and  $r_i$  is the change in age at birth for birth order  $i$  within one year. As we do not observe birth order specific fertility in our data, we assume that the change in age at birth is uniformly distributed over all birth orders and get the expression  $Births_{per1,000women}/(1 - r)$  as our tempo-adjusted fertility measure.<sup>12</sup> This measure captures quantum effects (free of tempo effects) if tempo effects are at work equally for all age groups and the quantum effect does not affect age at birth. We measure  $r$  as the change in average age at birth<sup>13</sup> within each county. As suggested by the increases in age at birth, the effect of public child care on fertility should be underestimated in our baseline specifications.

Columns 3 and 4 of Table 9 show our fixed effects estimation results for the effects of public child care coverage on the tempo-adjusted birth rate. Using the adjusted birth rate, we find that an increase in public child care coverage by 10 percentage points increases births per 1,000 women by 1.8 in  $t$  and by 2.1 in  $t + 1$ . The large standard errors are

<sup>12</sup>Ideally, we would estimate separate birth rates for each birth order, but our data does not allow a distinction. The German Micro Census could in principle be used to identify birth order specific fertility; however, the number of births per county-year cell is too low to credibly execute the respective analysis.

<sup>13</sup>We use the mean of the deviation from the previous and the following year to approximate for the change during the year of observation.

most likely due to measurement error in the tempo-adjusted outcome variable. They do not allow us to claim that effects on the adjusted rate are statistically larger than in the baseline case. Nevertheless, it becomes evident that due to the positive age at birth effect, we rather underestimate the quantum effect of public child care on fertility in our baseline specification.

## 8. Concluding Remarks

The question of whether the presence of policies and arrangements, such as affordable child care, can positively affect the private choice to have children has gained importance over the past decades. Although the literature on this subject has grown recently, most of what we know about the effects of child care policy on fertility comes from countries where birth rates are regarded as “satisfactory”. An important remaining challenge for policy-oriented social science research is to understand if child care policy can positively influence childbearing decisions in countries where birth rates are considered to be “too low”. In this paper, we have made a first step towards such an understanding by evaluating the impact on fertility of a major German child care reform. The reform we study led to a significant expansion of child care slots for young children, and our empirical strategy exploits the temporal and spatial variation in child care coverage induced by this expansion. First, we apply a difference-in-differences method that compares a treatment group of counties with above-median child care expansion to a control group of counties with below-median child care expansion. Second, we use a fixed-effects estimator to control for unobserved heterogeneity at the county level. Results from both specifications show that child care expansion has a significant positive effect on fertility. In particular, our results suggest that an increase in child care coverage for under three year olds from 5 percent to 35 percent can increase fertility rates by roughly 0.13 children per woman. In other words, a ten percentage point increase in child care coverage leads to an increase in births per 1,000 women of 3.2 percent. Taken together, the results presented in this article suggest that there is nothing inevitable about very low fertility rates: policies that facilitate the combination of parenthood and employment hold the promise of being an effective way to boost birth rates where these rates are considered to be too low.

This analysis is subject to a number of limitations and should only be the first step in a more in-depth examination of the implications of child care policy in the context of very low fertility rates. Due to the lack of comprehensive micro data, the unit of our analysis is annual county-level birth rates. As a result, we were neither able to examine whether the fertility effect is realized at the intensive or extensive margin, nor could we address issues related to the spacing of births. Future work should also attempt to analyze whether there is effect heterogeneity with respect to women’s educational attainment. Finally, clarifying whether there is a level of child care provision above which the positive effect on fertility disappears also merits further attention.

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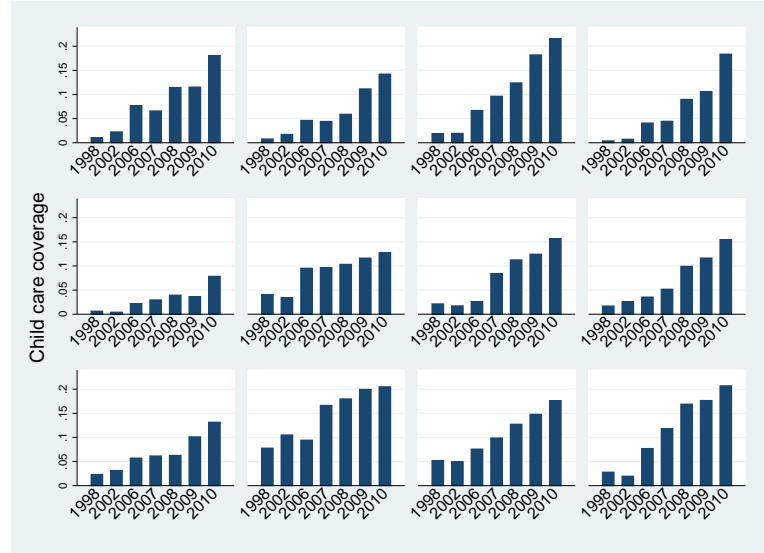
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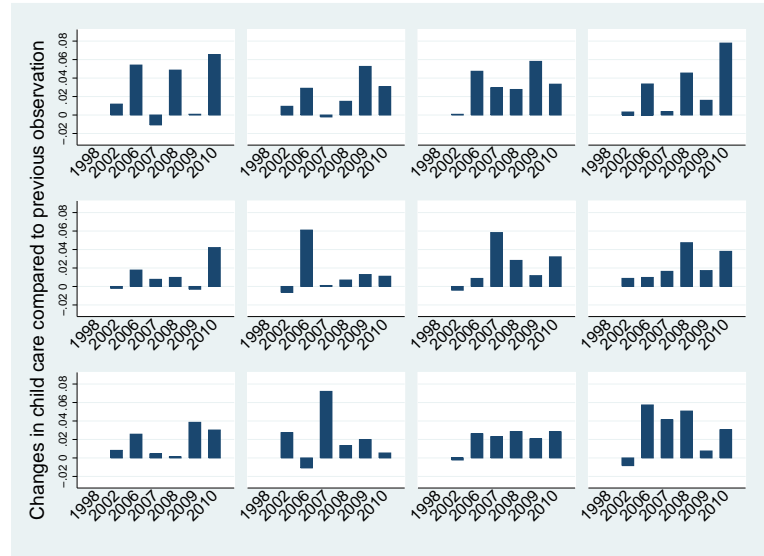
## A. Additional Graphs

Figure A.1: Child care coverage in selected counties over time



*Notes:* The figure shows child care coverage over time for individual, selected counties. Each panel depicts a single county.

Figure A.2: Changes in child care coverage in selected counties over time



*Notes:* The figure shows changes in child care coverage over time relative to the previous observation for individual, selected counties. Each panel depicts a single county.

## B. Data Appendix

County level data from the Statistical Offices of the German Länder (*Statistische Landesämter*) is provided in per year per item files. We identify counties by official id numbers and conduct a county level panel from 1997 to 2010. During this investigation period, reforms in geographic local government competency and in data aggregation rules altered county identifiers in some instances. In order to get a fully balanced panel, we made necessary changes that produced time-stable units of observations. Hannover, Aachen and Saarbruecken are merged to city-urban regions, whereas in earlier waves each consists of two separate counties. We use the merged definition throughout all waves and add up values if necessary. Our basic data is from the Statistical Offices of the German Länder (*Statistische Landesämter*), whereas the employment related data is from the Federal Employment Agency and matched using county-year identifiers.

Our variables have different underlying measurement concepts. Child care slots, population and employment are record date measures, reported in the first half of March (child care from 2006 onwards), 31st December (population, child care until 2002) resp. 30th June (employment). Births, GDP and in- and out-migrants are yearly sums. Births per 1,000 women are defined as the sum of births within the year divided by population as of 31st December. As child care from 2006 onwards is evaluated in March and population on 31st December, we divide it by one year lagged population. Child care in 1998 and 2002 is measured on 31st December and is therefore divided by population in the same year.

Public child care figures are conducted from two different data collections by the Statistical Offices of the German Länder (*Statistische Landesämter*). Public child care is defined as all publicly funded or subsidized child care, including child care centers and childminders. From 2006 onwards we observe occupied child care slots, whereas in 1998 and 2002 we observe available child care slots. As there was excess demand for child care, we can credibly assume both measures to be comparable. However, there is reason to believe that in 1998 and 2002 the number of public child care slots is underestimated. In these years, children under the age of three that were enrolled in child care centers for over three year olds are not included in the supply measures and the number of childminders had be estimated. The undervaluation of public child care coverage is believed to be of the magnitude of about one percentage point (DJI, 2008).

## C. Timing of dependent and independent variables

We exploit the variation of child care coverage over time to identify effects on an outcome (births) that can only react with a certain lag. Therefore, it is essential for our analysis to get the timing of cause and effect right. In our baseline specification, we estimate both the effect of child care coverage on contemporaneous births and on births in period  $t+1$ . The reason is that child care coverage is measured in the beginning of March each year. As child care centers sometimes align their service to school years (August or September to June or July), the March rate may partly reflect coverage from August or September in the year before. Furthermore, utilized child care slots in March must have been established some time in the preceding 12 months. Then, fertility may already react to changes in child care in period  $t$ . Our more conservative alternative is using births in period  $t+1$  as

the outcome, which allows some more time for conception and pregnancy—in sum 10 to 22 months after public child care coverage is observed in March. The latter measure is our preferred specification. In Figure 4, we show effects of child care coverage measured in period  $t$  on births in period  $x$ , while controlling for child care and all other covariates in period  $x-1$ . Accordingly, in the third column ( $x=t+1$ ) we observe the effect of child care coverage in  $t$  on births in  $t+1$ , which is exactly our baseline effect from the fixed-effects estimation on births in  $t+1$  (12.235). To check whether the timing is reasonable, we shift the outcome and control variables on the timeline. The middle bar reveals that even if we control for the relevant child care coverage in  $t-1$ , child care coverage in  $t$  has a positive and significant effect on births in  $t$ . This suggests that our conservative forwarding of one period in the outcome measure is not enough to pick up all variation that can be linked to child care coverage. It therefore seems reasonable to estimate both the effect of child care on births in  $t$  and in  $t+1$ .

The expansion of child care within counties might not be independent over periods. In that case, our fixed-effects regressions could be spurious. To be sure that we do not only pick up pure child care expansion trajectories, we estimate the effect of child care in  $t$  on births in  $t+2$  while controlling for child care in  $t+1$  in the far right bar. There is no significant effect of child care coverage in  $t$ . We apply the same procedure for the other direction on the timeline. The estimates of child care in  $t$  on births in  $t-1$  resp.  $t-2$ , while controlling for child care coverage in  $t-2$  resp.  $t-3$ , reveal coefficients that are close to zero and far away from conventional significance levels. Thus, taken together, child care coverage in  $t$  is relevant for births in  $t$  and  $t+1$ . As we move further away on the timeline, we do not find any effect of contemporaneous child care coverage. This indicates that our results are not driven by time-dependencies of child care expansions within counties.

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