BGPE Discussion Paper

No. 185

The role of the career costs of children for the effect of public child care on fertility and maternal employment

Katrin Huber

April 2019

ISSN 1863-5733

Editor: Prof. Regina T. Riphahn, Ph.D.
Friedrich-Alexander-Universität Erlangen-Nürnberg

BGPE Discussion Paper Series

The role of the career costs of children for the effect of public child care on fertility and maternal employment

Katrin Huber

26th April 2019

Abstract This paper investigates whether the effects of affordable and easily available public child care on fertility and maternal employment depend on the career costs of children a woman faces. It builds on the idea that these costs vary by occupation and education. In a generalized Diff-in-Diff, I exploit the substantial variation between West German counties concerning intensity and speed of the provision of new child care slots for under-three-year-olds. The combination of county-level data on child care coverage with detailed individual-level information from the German social security records allows me to analyze so far unexplored effect heterogeneities by occupational groups. The results indicate that the average positive effects on fertility and maternal employment are driven by women who face relatively higher career costs of children: women in occupations with a steeper age-earnings profile, women who cannot be easily substituted at work and women with medium and high education level. The findings reveal that policies which reconcile family and work life are indeed more beneficial for women facing higher career costs of having children.

Keywords: Child Care, Fertility, Maternal Employment, Career Costs of Children

JEL Classification: J13, J22

Acknowledgements

This paper benefited greatly from the valuable advice provided by Stefan Bauernschuster. I also thank Dana Mueller (IAB) & Aderonke Osikominu for providing their do-files to me and the researchers at the Research Data Centre (FDZ) of the Federal Employment Agency at the Institute for Employment Research (IAB) for helping me with questions on the data and for running my do-files via remote. I am also grateful for the insightful comments by Sebastian Krautheim, Hannah Lachenmaier, Susanna Grundmann and by the participants of the PhD seminar in Passau. Eva-Marie Schaupp, Georg Klein and Jan Reichert provided helpful research assistance.

K. Huber
Department of Economics, University of Passau, Innstr. 27, 94032 Passau, Germany
Tel.: +49-851-5092544
E-mail: katrin.huber@uni-passau.de

1 Introduction

Already the first seminal studies modeling intra-household time allocation characterize child rearing as a time intensive activity. Becker (1965), for example, points out that caring for children consumes many hours that could otherwise be used for market work, but at the same time does not produce income. It has also been recognized that the gender wage gap starts to emerge after the onset of parenthood and thus mainly originates from women with children (e.g. Waldfogel, 1998; Bertrand et al., 2010; Goldin, 2014; Kleven et al., 2018). Based on these findings the economic literature highlights that women face “career costs of children” (Adda et al., 2017) or said differently a “child penalty” in earnings (Kleven et al., 2018) which persists for at least 10 years after the first birth. These costs result from changes in female labor force participation, from reductions of working hours and from changes in the wage rates after the first child is born (Kleven et al., 2018). To reduce these career costs of children by improving the compatibility of family and career, many countries have increased their public spending on family benefits, among them the provision of subsidized, universal public child care. It is already well known that an improved access to high-quality and affordable public child care on average enhances maternal labor supply and increases birth rates substantially.

The costs of having children and providing care to them are however not of equal extent for every family, but vary according to differences in the cost of own time and in the household production functions (Becker, 1991). Both Becker (1965) and Mincer (1963) point out that the *price* of having children is relatively higher for families with higher income and if the wife has a higher potential wage rate. Moreover, Adda et al. (2017) highlight that the career costs of children depend on women’s occupation, due to different penalties on amenities related to workplace flexibility and due to heterogeneous pay structures (see also Goldin, 2014). To the best of my knowledge, the potential role of these differences in the career costs of children has been largely neglected in the empirical literature investigating the effects of public child care on fertility and maternal employment.[1]

To close this research gap, I use the staggered expansion of highly subsidized public child care for under-three-year-olds in Germany as a natural experiment to detect effect heterogeneities concerning fertility and maternal employment depending on the career costs of children a woman faces. These costs are higher for women employed in occupations with a steep age-earnings profile and for women in occupations in which the degree of substitutability between co-workers is low (Adda et al., 2017; Goldin, 2014). I combine county-level information on child care coverage and county-level background variables with the BIBB/BAuA Employment Survey 2006 and with the social security data from the Institute of Employment Research in Nuremberg (SIAB 7514), which contains detailed individual-level information including

[1] Few papers on maternal employment (e.g. Baker et al., 2008; Schlosser, 2011; Müller and Wrohlich, 2018) differentiate by educational degree. However, nothing is known about occupational differences and about any effect heterogeneities concerning fertility.

education and occupation. I follow Bauernschuster et al. (2016a) and estimate a generalized Difference-in-Differences model which exploits county and time variation in child care coverage in West Germany after several policy initiatives starting in 2005.

My results indicate that the expansion of child care slots had positive average effects on both fertility and maternal employment. A 10 percentage point increase in child care coverage is associated with a 7.3 - 10.4% increase in the probability of giving birth within two years and with a 5.6 - 6.3% increase in maternal employment. The heterogeneity analysis reveals that these average positive effects are indeed driven by women who tend to have higher career costs of having children: women who work in occupations with a steeper age-earnings profile, women employed in occupations in which they cannot be easily substituted by a co-worker and, as a robustness check indicates, women with medium and high education. These results are robust to including time-varying county-specific controls, taking account of selective migration and controlling for the potentially confounding effects of the parental leave reform in 2007. A placebo estimate and the inclusion of county-specific linear time trends corroborate the common trend assumption.

With this assessment of effect heterogeneities by career costs of children, my research is linked to the literature that investigates the effects of public child care on women's labor supply and childbearing decisions from a theoretical point of view. In a nutshell, this literature predicts positive average effects on maternal labor supply because formal child care serves as a substitute for maternal care and enables mothers to return to the labor market earlier after birth and to work more hours (Blau and Currie, 2006; Bick, 2016; Apps and Rees, 2004). This should also enhance the demand for children, as the opportunity cost of having children decreases and it becomes more likely that, in case of an increase of maternal wages, the positive income effect dominates the negative substitution effect (Ermisch, 2003; Apps and Rees, 2004). These positive average effects of an improved access to high-quality and affordable public child care are confirmed empirically for both maternal labor supply[2] and fertility[3]. My study overcomes the data limitations of this average-effects literature and can thus provide complementary evidence on so far unexplored effect heterogeneities by the career costs of children.

The remainder of the paper is organized as follows. In Section 2, I describe the institutional setting, Section 3 and 4 explain the data and identification strategy. Results and robustness checks are provided in Section 5. Section 6 discusses the findings and concludes.

[2]For example Cascio (2009); Lefebvre and Merrigan (2008); Baker et al. (2008); Schlosser (2011); Berlinski and Galiani (2007); Busse and Gathmann (2015); Bauernschuster and Schlotter (2015); Müller and Wrohlich (2018). In institutional settings with an already high female labor supply or in which public care crowds out private care, the availability of public care does not have an effect on maternal employment (Lundin et al., 2008; Havnes and Mogstad, 2011).

[3]For example Rindfuss et al. (2010); Mörk et al. (2013); Björklund (2006); Bauernschuster et al. (2016a).

2 Institutional setting

2.1 Maternal employment, fertility and child care in Germany until the mid-2000s

Maternal employment. In 2005, the maternal employment rate in Germany amounted to 64.3% and was thus above the average of the Euro area (60.6%) and similar to maternal employment in for example France (65%), UK (68.9%) or Austria (65.2%). Considering only mothers of children aged between 6 and 11 years or aged 12 years and older, maternal employment was even at a higher level[4]. Thus, labor supply of mothers with school-aged children was relatively high compared to other EU countries. The employment rate for mothers with children below 6, however, was strikingly low: in 2005, only 46.7% of German mothers with children in this age group were employed, a number which is almost 10 percentage points below the Euro area average of 55.8% (Eurostat, 2018). Looking in more detail at mothers with children in these younger age groups, Kreyenfeld and Geisler (2006) find that in 2002 only 19.2% of mothers with children between 3 and 6 were full-time employed and that the rate of full-time employment varies substantially between East (50.5%) and West Germany (14.5%). Among mothers of under-three-year-olds, only 11.8 % had a full-time job, the percentages for East and West Germany amounted to 31% and 8.9% respectively. These numbers demonstrate that - although employment rates of mothers of school-aged children were relatively high - mothers of younger children, especially below the age of three, were seldom employed and if they were, they worked only few hours.

Fertility. Total fertility rates[5] in Germany decreased from 2.4 children per women in 1960 to 1.34 in 2005. This number was below the average fertility rate in the Euro area and the European Union (1.6) and below the average number of children born to a woman among all OECD member states (1.7). In 2005, only very few countries had a lower fertility rate than Germany[6], most other high-income countries like for instance the US (2.1), France (1.9), Norway, Finland, Denmark or Australia (all 1.8) were well above the German fertility rate at that time (World Bank, 2019). Consequently, just before the child care reforms of interest came into effect, Germany was characterized by extremely low birth rates and in combination with the rapidly aging population, this posed an enormous risk to the social security systems (Dorbritz, 2008).

Child care for under-three-year-olds before the reforms. While the provision of public care for children between one and six years was already quite high in East Germany in times of the former German Democratic Republic, West Germany lacked behind substantially until the mid-2000s. In 1996, all parents were granted a legal claim to a place in kindergarten for 3-to-6-

[4] 68.1% and 75.2% respectively (Euro area: 66.4% and 67.6% respectively)

[5] The total fertility rate "[...] represents the number of children that would be born to a woman if she were to live to the end of her childbearing years and bear children in accordance with age-specific fertility rates of the specified year." (World Bank, 2019)

[6] Namely Poland (1.2), Moldova (1.2), Ukraine (1.2), Republique of Korea (1.1), Hong Kong SAR, China (1.0) and Macao SAR, China (0.8).

year-olds which resulted in full provision of half-day care for this age group by the mid-2000s in the whole of Germany (Bauernschuster and Schlotter, 2015). For children under the age of three however the supply of formal care was still limited in West German regions: in 2002, public child care for under-three-year-olds was available for only 2% of the children in the relevant age group in West Germany, whereas 35% of East German children had a slot in public child care institutions (Geyer et al., 2015). Due to the quite high regulations (e.g. concerning qualification of staff, group sizes) for private providers (see Felfe and Lalive, 2014) and the resulting high costs, this mode of care was no real substitute for public care. This lead to a huge excess demand for child care slots for 0-3-year-olds in the mid-2000s (Wrohlich, 2008).

2.2 The public child care reforms of the mid-2000s

In response to the large excess demand for child care slots for under-three-year-olds and due to the demographical challenges induced by the extremely low birth rates, the German government passed several reforms in the years 2005 to 2008 to increase child care coverage especially in West Germany. In 2005, the *Tagesbetreuungsausbaugesetz* (federal law) came into effect, regulating the expansion of daycare slots for under-three-year-olds with the aim to create 230.000 additional child care slots. Moreover, quality requirements were made more concrete to stress that early child care should also contribute to early childhood education (BMFSFJ, 2004). Two years later, in a summit of the federal state, the *Länder* and the municipalities, the expansion of child care coverage was brought to a higher level: in this so called *Krippengipfel* the involved authorities decided to reach a child care coverage rate for under-three-year-olds of 35% by 2013, which was equivalent to tripling the existing slots (Tiedemann, 2014). Lastly, in 2008, a federal law (*Kinderförderungsgesetz*) introduced a legal claim to a child care slot for all pre-school children older than one by 2013. Especially this last reform also led to a paradigm shift concerning the financing of additional child care slots. Until then, the local authorities had to cover all costs for expanding public child care alone, after 2008, the cost was shared between the state, the *Länder* and the municipalities (Tiedemann, 2014). Consequently, the heavy subsidies on child care slots led to a higher provision of low-cost[7] child care for 0-3-year-olds both in East and in West Germany. Although the state did not impose any penalties on local authorities that did not meet the goals by 1st of August 2013, municipalities had an incentive to expand the supply of child care. Parents had the right to claim additional costs for private child care from them (e.g. decision of the Federal Administrative Court (BVerwG), September 2013[8]) or to claim remuneration for forgone earnings (e.g. Federal Court, October 2016, press release 172/2016) if they did not get a child care slot for eligible children. Therefore, especially in West Germany, where child care for under-three-year-olds was technically not-existent, ad-

[7]The parental fees depend on family size and family income and range from 0 to 600 EUR per month (Bauernschuster et al., 2016a).
[8]BVerwG 5 C 35.12

ditional slots were created even before the claim to a child care slot became law (Tiedemann, 2014). The second part of Table A1 in the Appendix shows that child care coverage increased from 1.6% to 14.2% on average across all West German counties between 1998 and 2009. The variation of child care coverage after the reforms in 2009 is however even more striking: it ranges from 3.7% to 35.9% and demonstrates huge differences in the speed and intensity of the expansion of child care slots between German counties.

These differences in the magnitude of expansion across counties can be explained by both demand and supply side factors. As was pointed out by Bauernschuster et al. (2016a), on the one hand, local authorities had to make projections concerning the future demand for child care slots depending on demographic and economic factors. On the other hand, the state was responsible for approving the construction of new child care centers and this approval was necessary for receiving the state subsidies (Felfe and Lalive, 2012). The sharing of responsibilities between the three federal levels made the administrative process complex and lengthy and according to Hüsken (2011) also dependent on county-specific factors (like differences in expansion strategies, varying rules for financing etc.) that could not be influenced by the local authorities. Consequently, regional variation does not only stem from differences in demand, which are likely to be endogenous to expected changes in fertility levels and maternal employment. The regional differences primarily result from supply side shocks that should be exogenous to my outcomes of interest and lead to a deviation from the demand projected by the municipalities. Possible reasons for such supply side shocks named in the previous literature are shortages in construction ground, a lack of qualified staff, delays in approval or rejections due to non-compliance with specific regulations or differences in the routines and rules concerning the funding system (e.g. Bauernschuster et al., 2016a; Felfe and Lalive, 2012, 2014). I will exploit this exogenous part of the variation in child care coverage in my identification strategy in Section 4 and provide more evidence on the randomness of the expansion in Section 5.3.

3 Data

3.1 Individual-level data

My individual-level data stems from the the weakly anonymous Sample of Integrated Labor Market Biographies (years 1975-2014) (SIAB 7514). Data access was provided via on-site use at the Research Data Center (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) and subsequently remote data access.[9] This data covers a 2% subsample of all individuals who are employed subject to social security, receive benefits according to German Social Code III or (since 2005) II, are officially registered as job seeking or participate in active labor market policies. The key variables in this dataset contain

[9] For more information on the dataset see Antoni et al. (2016).

detailed information on the individuals' employment status, occupation, education level, daily wages and benefits. Moreover, it is possible to identify the event of a childbirth using a strategy provided by Müller and Strauch (2017).[10] In short, following Müller and Strauch (2017), I make use of a variable indicating the reason of cancellation/notification/termination of an employment or unemployment spell to derive the expected date of childbirth. To reduce the risk that maternity allowance is confounded with leave periods due to sickness, Müller and Strauch (2017) point out that the sample should be restricted to women up to age 40. Moreover, I restrict the sample to West German counties because they were most affected by the expansion of child care slots triggered by the policy initiatives. East German counties already started from a much higher level of child care availability as about one third of East German children had access to a child care slot before the reforms (Geyer et al., 2015). My period of analysis covers the years 1998-2010.

It is important to highlight the differences between this dataset and the birth registry data used by for example Bauernschuster et al. (2016a). The birth register allows to exactly determine the universe of all births in Germany during the relevant time period. However, it does not offer a lot of additional information on maternal characteristics and makes it impossible to estimate heterogeneous effects by education or occupation. The SIAB contains exactly this information, the sample composition however markedly differs and in comparison to the official statistics only about 50% of all births in Germany are identified. As the data only covers individuals who have a record in the administrative data sources, I cannot identify births and labor supply behavior of self-employed, civil servants or females before they enter the labor market. Moreover, it is not possible to identify twin births. If a mother does not return to an employment subject to social security between two successive births, the second birth is almost impossible to detect (Müller and Strauch, 2017) and the data therefore covers primarily first-order births. Consequently, I cannot clearly differentiate between the intensive and the extensive margin in my fertility analysis and I estimate the effects of expanding child care for below-three-year-olds only for women who have a record in the social security data.[11]

I collapse the SIAB data to have one observation per individual, per year.[12] As employers are not obliged to report all variables, some missing values for the education level have to be imputed following the imputation described in Fitzenberger et al. (2005).

[10] The identification of births developed by Schönberg (2009) and for example applied in Schönberg and Ludsteck (2014) is not applicable with the new version of the SIAB as it does no longer contain the leave variable *btype* used to determine childbirth in these papers.

[11] In 2017, about 50% of all women between 15 and 40 were employed subject to social security (DESTATIS, 2019), about 2.76% were officially registered in the unemployment statistics (Statistisches Bundesamt, 2018).

[12] In some cases, the SIAB data contains several data entries per individual per year due to the splitting of episodes. In cases where the education information changes within a year, the highest educational degree is assumed. In case of changing occupations, the most frequent one is assumed to be relevant. Labor supply variables are taken from the cutoff information June, 30 of every year. If the June information is not available, it is replaced by the cutoff values of March, 31 or September, 30, conditional on an overall observation period of at least 6 months.

I then construct my two outcome variables: first, a dummy variable indicating whether mother i, having a child of age 0-3 and living in county c is employed in year t[13] and second an indicator for woman i living in county c giving birth to a child in period t or $t+1$[14]. In comparison to Bauernschuster et al. (2016a), who analyze births in t and $t+1$ separately, I define a single birth indicator which is equal to one if a woman gives birth in either t or $t+1$. The main reason for this deviation is the considerably lower number of births per county and per year I can identify in my data and the fact that the date of birth cannot be as precisely estimated as in the birth registry data. Summary statistics on the outcome variables, for the whole sample and for the subgroups I will use in the heterogeneity analysis and in the robustness checks, can be found in Table A1 in the Appendix. Surprisingly, in my data, the probability of giving birth for women with low education is lower than the birth rate of women with medium and high education. This is in contrast to the generally higher incidence of childlessness in Germany among highly educated women (Pötzsch, 2012). A possible explanation stems from the nature of my individual-level data: as highlighted above, I cannot capture births that take place during periods in which women do not have a record in the administrative data sources (for example if they are non-employed but not officially registered as unemployed). Investigating the length of gaps for which I cannot observe a woman in my data, I detect that the share of women for whom I have gaps of one to six years is higher among women with low education compared to women with medium or high education. The T-test for the hypothesis that the mean gap is equal for women with low education and for women with medium or high education can be rejected at a 1% significance level.[15] Consequently, I might underestimate the number of births for women with low education if they get pregnant and give birth in exactly these gap periods.[16] Moreover, the imputation procedure for education by Fitzenberger et al. (2005) I apply, tends to slightly overestimate educational degree. Some individuals classified as having medium or high education might thus in fact have lower education.

Furthermore, I exploit the individual-level information to construct control variables for a woman's age, (pre-birth) education, German nationality, the number of children below 18 and two indicators for whether a mother already has older children up to three years or between four and six years old at the time of the birth I identify. In addition, the information on (pre-

[13]In cases where a mother does not appear in my data in the year t for which the expected birth was computed, I code her as being non-employed. Evidence form the German Socio-Economic Panel (SOEP v28, 2012) affirms that this is a reasonable assumption, because the percentage of women who switch from being employed subject to social security to being self-employed or civil servants after birth is very low (0.3 and 2.5 % respectively).

[14]Bauernschuster et al. (2016a) show that births should be evaluated in both t and $t+1$ due to differences in the timing of measurement of child care coverage and births and due to the fact that conception might not happen immediately.

[15]As gaps do not differ systematically between weak (increase in public child care coverage below the median) and strong expansion (increase in public child care coverage above the median) counties this should not bias my estimates. The T-test for equality of the pre-reform mean of gaps in both types of counties cannot be rejected for any education level with p-values above 0.6.

[16]The same argument applies to the birth rates by wage profile. Gaps by wage profile do also not differ systematically between weak and strong expansion counties and my identification strategy should thus not be threatened.

birth) occupation (measured at the 2-digit and 3-digit level) and the information on daily wages, which is used to calculate age-earnings profiles (see Section 3.3), is drawn from this data.

3.2 County-level data

As the main explanatory variable *child care coverage* varies at the county level, I also need detailed information on county characteristics. I use the dataset provided by Bauernschuster et al. (2016b) which contains the information on the child care coverage rates and all relevant county-specific time-varying control variables: population age structure, population density, male employment rate, GDP per capita, the conservative vote share, the share of highly educated women in fertile age and municipalities' gross revenue, debt and newly built dwellings. This data was collected from the Statistical Offices of the German *Länder*, the Federal Employment Agency and the German Microcensus (see Bauernschuster et al., 2016a). For summary statistics on these control variables see Table A1 in the Appendix.

3.3 Occupational classification

In the following, I describe how individuals are assigned to occupational groups by wage profile and substitutability.[17] This classification will later on be used in the heterogeneity analysis to investigate whether the effects of child care on fertility and maternal employment vary for these subgroups. To avoid that the occupation is endogenous if mothers switch to a different occupation in response to the child birth (see Kleven et al., 2018), I rely on the pre-birth information on occupation for this assignment.

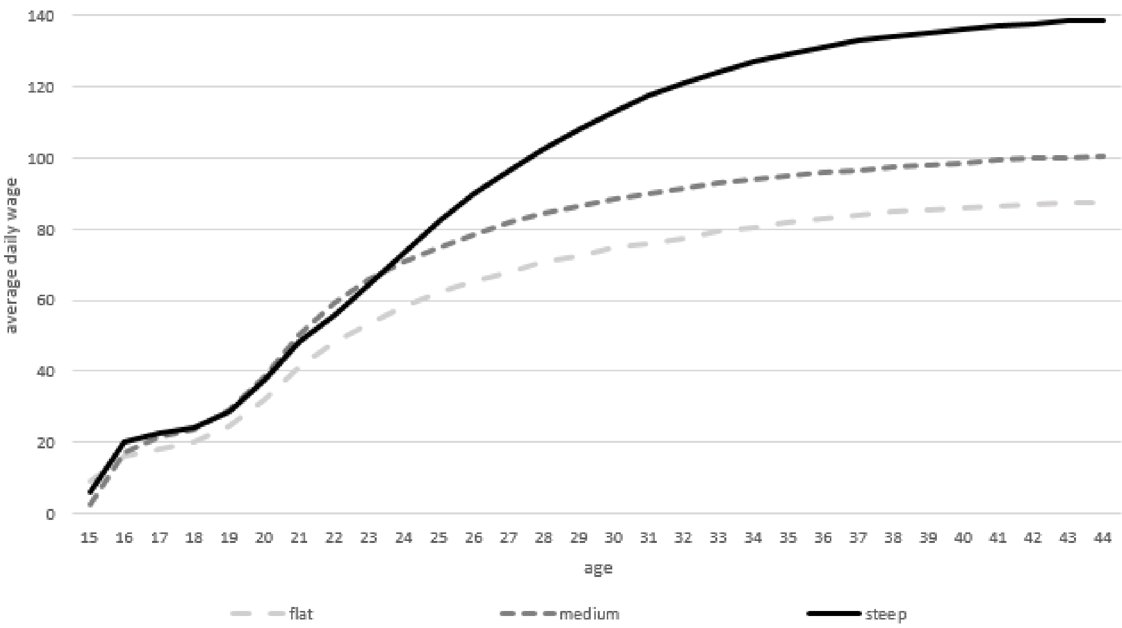
Occupation - by wage profile. The first occupational classification builds on Adda et al. (2017) who highlight that different occupations have a different wage growth over time. Adda et al. (2017) for example find that abstract occupations are characterized not only by a higher wage at the beginning of the career but also by a higher wage growth at each level of experience. A similar argument is made by Goldin (2014), who points out that some occupational positions are characterized by highly non-linear (convex) pay structures with respect to hours worked and impose heavy wage penalties on time out of the labor force. Others pay almost completely linearly to work hours without wage penalties on part-time work or on interruptions.[18] If wage profiles are less concave and continue to grow faster at higher levels of experience, this implies that work interruptions in the middle of the career are more costly, because women have higher forgone earnings and therefore higher opportunity cost in these occupations (Adda et al., 2017; Weiss and Gronau, 1981; Arntz and Gathmann, 2014). In other words, "missing" a steeper part of the wage profile hurts more than missing a flatter part. To capture this finding in my

[17] The 2% subsample of the SIAB does not provide enough observations to directly run the analysis at the 2-digit occupational level.
[18] See also Goldin and Katz (2008, 2011).

occupational grouping, I again rely on the SIAB individual-level data that contains detailed information on daily wages. Wages are deflated (base year 2010) according to the Consumer Price Index provided by the Federal Statistical Office. I follow Mincer (1974) and construct age-earnings profiles for 2-digit occupations. To get counterfactual age-earnings profiles without pregnancies and work interruptions, I construct these profiles for full-time employed males aged between 15 and 44. Occupations are then grouped into having a flat, medium-slope or steep wage profile and the first part of the heterogeneity analysis in Section 5.1 and 5.2 will be based on these three groups. The following paragraph gives a more detailed description of the three wage profiles and provides a graphical overview in Figure 1.

Occupations characterized by a flat age-earnings profile are for example occupations in agriculture, forestry, and horticulture, occupations in the food industry or cleaning services. A medium sloped age-earnings profile is for example found for most of the manufacturing occupations, occupations in building and interior construction and for most service occupations in the social sector and in cultural work. Occupations concerned with production technology, medical and health care occupations and occupations in business management, for instance, are characterized by a steep development of earnings over time. For a detailed list of 2-digit-occupations, sectors and the respective wage-profiles, see Table A2 in the Appendix.

Fig. 1: Age-earnings profiles



Notes: Figures depict average daily wages of male workers in respective age group. Wages are deflated according to the consumer price index provided by the Federal Statistical Office, base year 2010. Sample: West Germany, 1997-2013, full-time employed males, 15-44 years old. *flat* contains 2-digit occupations 11, 12, 29, 52-54, 63, 93; *medium* includes 21-24, 28, 32, 33, 43, 42, 51, 62, 82-84, 94 and *steep* is decomposed of 2-digit occupations 01, 25-27, 31, 41, 43, 61, 71-73, 81, 91, 92 (German Classification of Occupations 2010).

Figure 1 depicts the average age-earnings profiles of the three groups and shows that they all start at a quite similar level. The flat profile group however is already clearly below the medium-sloped and steep profile at the age of 17. Between the age of 17 and 25, average daily wages of the flat profile increase from 18 EUR to 62 EUR and are characterized by an average growth rate of about 16% per year. Afterwards the increase in wage earnings with age is very modest (2.5% per year) and workers reach an average daily wage of 86 EUR at the age of 40. The medium-sloped profile grows faster in the beginning of a career, the average growth rate per year amounts to 18% between 17 and 25, afterwards, the medium and flat profile evolve quite similarly in terms of growth rates. The difference of the steep profile however is striking. While it evolves very similarly to the medium slope profile in the beginning, it surpasses the medium profile at the age of 25 and grows at a very fast rate. Compared to the flat and medium sloped profile, the growth rate of the steep profile is almost twice as large between the age of 25 and 40.

Occupation - by substitutability. While the first occupational classification by wage profiles mainly relies on differences in the development of earnings with respect to age, a second possibility to group occupations is in the spirit of Goldin (2014), who puts more emphasis on the reasons behind the differences in pay structures across occupations. She finds that the main determinants of occupational pay structure are workplace flexibility (e.g. hours worked, precise times at which the worker has to be present and the flexibility of the time schedule) but also the degree to which a worker can be substituted by a co-worker (Goldin, 2014; Goldin and Katz, 2016). Hotz et al. (2018) call the possibility to easily transfer a task to a co-worker "flexibility in production". They highlight that providing workplace flexibility is costly for firms. This cost however decreases if workers are good substitutes for each other and can thus "hand off clients [...] with little loss in fidelity" (Goldin and Katz, 2016, p.736). A higher degree of substitutability is thus important to reduce the work penalties to motherhood and the gender gap. To construct a measure reflecting how easily a worker can be substituted by another worker in each occupation, I use the BiBB/BAuA Employment Survey of the Working Population on Qualification and Working Conditions in Germany 2006 (Hall and Tiemann, 2009), which is a representative survey of 20,000 employees in Germany and contains a lot of information on the kind of job the employee performs, on tasks, qualifications needed and the work environment.[19] I consider the following workplace characteristics to determine substitutability and thereby largely follow Goldin (2014):

1. *Time flexibility*, including variables on for example flexible work schedules, time pressure or the possibility to work in home office. Higher time flexibility should increase substitutability of a worker as the particular time a worker is present at the workplace is less important with higher flexibility.
2. *Degree of regular contact to clients*, for instance indicating how much contact to clients is needed in general or whether the job regularly requires persuasive efforts vis-à-vis clients.

[19] I make use of the wave 2006 as this is the first one fitting to my observation period and containing the variables relevant to classify occupations according to the categories described below. For more information on the dataset, see Rohrbach-Schmidt (2009).

The lower the degree of personal contact to clients, the easier a worker can be substituted by a co-worker.

3. *Room for own decision making*, showing whether employees have to follow a clear and prescribed operating process, whether they are engaged in problem solving and can in general make own decisions. A higher degree of freedom to make own decisions implies that decisions depend more on the individual worker and make workers poorer substitutes for each other.
4. *Transferability of skills*, indicating whether work processes are repeated, whether a long training period is necessary for the tasks performed or whether special seminars and courses are required. The less prior training and the more standard procedures, the higher should be the transferability of skills and tasks and the higher the substitutability between co-workers.

I recode all relevant variables such that a higher value implies a higher degree of substitutability and standardize them to a mean of 0 and a standard deviation of 1. These variables are then grouped into a composite index for substitutability (also with a mean of 0 and a SD of 1). To get an average measure for how easily workers can be substituted by co-workers in each 2-digit occupation, I take the average over all women in the same 2-digit occupation.[20] Occupations are classified as having a *rather low* or *rather high degree of substitutability* depending on which half of the distribution they belong to[21] and these two groups will be used for the heterogeneity analysis in Section 5.1 and 5.2.

Table 1, column (1) shows the five occupations with the lowest and highest degree of overall substitutability respectively. Exemplary, column (2) and (3) list the occupations with the lowest and highest index values for contact to clients and skill transferability. Contact to clients is coded such that a higher index indicates less contact to clients. In general, most of the occupations concerned with farming and foresting, many manufacturing occupations and some service occupations like accountants, office specialists, warehouse managers and transport occupations are characterized by a rather high degree of substitutability.[22] Table 1 shows that almost all occupations listed among those having the highest index values in terms of overall substitutability, little contact to clients and skill transferability are manufacturing occupations. Technical occupations, many service occupations (e.g. occupations in health service[23], social care, education

[20]As the number of observations is very low within 3-digit-occupations, I take averages over 2-digit levels. The BiBB/BAuA 2006 contains the classification of occupations KldB1992 and KldB1988. To be able to merge the information to the SIAB 7514, I rely on the KldB1988.

[21]In a robustness check in Section 5.4, I will drop occupations that cannot be clearly assigned to one of the two groups.

[22]Concretely, 2-digit occupations 1, 2, 4-6, 9, 11, 13-27, 31-35, 39, 40-45, 47, 49-54, 63, 71, 73, 74, 77-79, 92, 93, 98, 99 (German Classification of Occupations 1988).

[23]Goldin and Katz (2016) point out the high degree of substitutability of the profession of pharmacists in the US. Conflicting with this result, pharmacists are listed among the occupations with a low degree of substitutability in my classification (see Table 1). A potential reason for this is that, due to the low number of observations in the BIBB/BAuA, I have to rely on 2-digit occupations, implying that physicians, dentists, veterinarians and pharmacists are all summarized as one single 2-digit-number (84).

and science) and some manufacturing occupations like mechanics or tool makers are classified as having a rather low degree of substitutability on average.[24]

Table 1: Degree of substitutability

	(1)	(2)	(3)
Kind of index:	Overall substitutability	Contact to clients	Skill transferability
Lowest index	72 - Water and air transport 84 - Physician, Pharmacists 89 - Pastoral workers 87 - Teachers 86 - Social care	84 - Physician, Pharmacists 86 - Social care 72 - Water and air transport 85 - Other health occupations 89 - Pastoral workers	89 - Pastoral workers 61 - Chemists, Physicists, Mathem. 76 - Administrative decision makers 87 - Teachers 88 - Humanities specialists, Scientists
Highest index	45 - Carpenters, Roofers, Scaffolders 18 - Wood preparers, -products makers 19 - Metal producers, Rollers 93 - Cleaning occupations 42 - Beverage and luxury food makers	43 - Other nutrition occupations 32 - Assemblers and metal workers 23 - Metal surface & couting 21 - Metal moulders (non-cutting) 45 - Carpenters, Roofers, Scaffolders	43 - Other nutrition occupations 23 - Metal surface & couting 18 - Wood preparers, -products makers 19 - Metal producers, Rollers 45 - Carpenters, Roofers, Scaffolders

Notes: All indexes standardized to a mean of zero and a standard deviation of 1. Overall substitutability index: higher value indicates higher degree of substitutability. Variables on time flexibility, freedom of decision making, degree of contact to clients and of skill transferability are included in the overall substitutability index. Contact to clients: higher value indicates less contact to clients required. Skill transferability: higher value indicates higher degree of transferability. German Classification of Occupations 1988, 2-digit-level, females. Data Source: BIBB/BAuA (2006)

4 Identification

To identify the causal effects of the availability of public child care on fertility and maternal employment, I stick very closely to the identification strategy applied by Bauernschuster et al. (2016a), who use the regional and temporal variation in the expansion of public child care coverage in a generalized Difference-in-Differences setting. However, as I have individual-level data, I do not aggregate information on fertility and employment at the county level but run individual-level regressions. This comes along with the advantage that I can condition on individual-level covariates and investigate heterogeneous effects by individual occupational group and education level.

4.1 Average effects

In the average effects framework, I estimate the following regression:

$$y_{ict} = \alpha + \rho d_{ct} + Z_i'\beta + X_{ct}'\lambda + \eta_c + \mu_t + \xi_{ict} \tag{1}$$

ρ is the coefficient of main interest as d_{ct} represents a continuous variable measuring the local child care coverage rate in county c in year t. Local child care coverage is defined by the number of child care slots relative to the number of children aged 0 to 3 and is available for the years 1998, 2002, 2006, 2007, 2008, 2009 and 2010. The analysis is thus restricted to these

[24]Concretely, 2-digit occupations 3, 7, 10, 12, 28-30, 36, 37, 46, 48, 60-62, 68-70, 72, 75, 76, 80-91 (German Classification of Occupations 1988).

years.[25] The outcome y_{ict} is either a dummy variable for having a birth in year t or $t+1$ or a dummy-variable for being employed in year t. To investigate the effect of early child care for the group of women for whom the access to child care slots for under-three-year-olds is indeed relevant, I restrict my sample to mothers of 0-3-year old children when analyzing employment. I include county fixed effects (η_c) to capture time constant general differences between German regions and year fixed effects (μ_t) to control for general time-trends in fertility or maternal employment that are similar across all regions.

Z_i' contains individual-level control variables. Both the fertility and the maternal employment regressions include the woman's age and pre-birth education level as standard controls. When the outcome of interest is maternal employment, I follow for example Müller and Wrohlich (2018) and add age squared and a dummy for being German in Z_i'.[26] In a further specification for maternal employment, I add the number of children younger than 18 in the family[27] and two dummies for whether the newborn already has a sibling aged 0-3 years or aged between 4-6 years. Information on marital status is only available for unemployed women and can thus not be included in the analysis.

X_{ct}' is a vector of county-specific time-varying covariates. Controlling for such factors reduces the risk that they evolve differently in regions with fast and slow expansion of child care slots and have at the same time an impact on fertility or maternal employment, thereby biasing the estimates. Counties expecting more future births might have an incentive to expand public child care coverage faster, which would lead to an upward bias for the estimated effect of child care availability on fertility. Therefore, I include several region-specific socio-demographic factors that also serve as proxies for measuring future fertility. Analogous to Bauernschuster et al. (2016a), I include the following control variables in extended specifications of the fertility regressions: age-specific population shares of females between 15 and 40, age-specific population shares of the whole population older than 41, population density, the male employment rate, GDP per capita, the conservative vote share, the share of highly educated women in the fertile age, municipalities' gross revenue and debt to account for local public finance and the number of newly built dwellings, which might be indicative for attracting young families. Concerning maternal employment, a similar arguments applies: counties which, due to economic or socio-demographic factors, are facing a potentially higher maternal employment might put more effort in a fast and extensive expansion strategy and this might overestimate the true effect. Consequently, the regressions on maternal employment contain the same control variables except for the information on revenue, debt and newly built dwellings.[28]

[25] As the information on GDP per capita, the male employment rate, municipalities' gross revenue, debt and newly built dwellings is missing for the year 2010, most regressions include only the years 1998-2009.

[26] The results depicted in specification (2) and (3) of Table 2 are qualitatively unaffected if age squared and a dummy for being German are included in Z_i' analogously for the fertility regressions.

[27] Measured before the respective birth in year t.

[28] Including controls for the municipalities' gross revenue, debt and newly built dwellings would reduce the already low sample size for the analysis of maternal employment by further 20%. If included, the results depicted in Table 3 however remain qualitatively unaffected for all three specifications.

As the treatment variable d_{ct} varies at the county level, whereas the outcome y_{ict} varies at the individual level, I introduce weights to account for the different number of individuals per county. Standard errors are clustered at the individual level as the same individuals are observed multiple times.[29]

4.2 Heterogeneous effects

To estimate the child care expansion's heterogeneous effects on fertility and maternal employment, I interact the treatment variable d_{ct} with woman i's respective occupational group (defined by wage profile or degree of substitutability). Equation 2 below exemplifies the regression estimated to detect heterogeneities by wage profile in the spirit of Adda et al. (2017) and Goldin (2014). *Steep wage profile* serves as the omitted category. In addition to including an interaction term of d_{ct} with the dummies for *flat* and *medium slope wage profile*, the main effects of the wage profile dummies are also included in the individual-level vector of controls Z_i'. All other covariates are equivalent to equation 1.

$$\begin{aligned} y_{ict} = & \alpha + \rho_1 d_{ct} + \rho_2 (d_{ct} \times \text{medium slope profile}_i) + \rho_3 (d_{ct} \times \text{flat profile}_i) \\ & + Z_i' \beta + X_{ct}' \lambda + \eta_c + \mu_t + \xi_{ict} \end{aligned} \tag{2}$$

ρ_1 then measures the effect of a change in child care coverage for women in occupations with steep age-earnings profile, ρ_2 and ρ_3 indicate whether the effect is significantly different for women in occupations with medium slope or flat wage profile respectively. The overall effect of a change in child care coverage on women with medium slope (flat) age-earnings profile is defined by $\rho_1 + \rho_2$ ($\rho_1 + \rho_3$). To estimate the effect heterogeneity by substitutability, which refers to Goldin (2014) and Hotz et al. (2018), I run an additional regression in which I interact child care coverage d_{ct} with a dummy variable *high substitutability* indicating that workers in the respective occupation have on average a rather high degree to which they can be substituted by a co-worker. *High substitutability* is equal to zero for occupations which are characterized by a rather low degree of substitutability between workers (omitted category).

4.3 Key identifying assumptions

As for every Difference-in-Differences approach, two key identifying assumptions have to be satisfied. Fertility and maternal labor supply must follow the same time trend in absence of the treatment in counties with different expansion patterns (common trend assumption) and individuals must not have selected into (or out of) the treatment. The common trend assumption requires that after controlling for county fixed effects and the time-varying county-specific

[29] The results remain virtually unchanged if standard errors are instead clustered at the county level.

controls in vector X'_{ct}, there are no other county-specific time varying unobserved variables which are correlated with (future) fertility or maternal employment of woman i and at the same time affect the child care coverage d_{ct} in county c at time t. In addition to including a bunch of relevant time-varying county-specific controls, I provide a specification including county-specific time trends and a placebo estimate for maternal employment in Section 5.3.2 and 5.3.3 to corroborate this assumption. In Section 5.3.5, I can moreover show that my estimates are not confounded by the parental leave reform in 2007, which did also affect fertility (Raute, 2019) and maternal employment (Kluve and Tamm, 2013).

Selection into treatment in my setting, could emerge via selective migration. If women who would have given birth to a child or would have entered the labor market anyway move to a strong expansion county, this might overestimate the true treatment effect. In a robustness check in Section 5.3.4, I assign the treatment based on the woman's pre-reform county of residence and can thereby show that selective migration is not of a major concern.

5 Results

5.1 The impact of child care on fertility

To analyze the effect of child care coverage on fertility, I estimate equation 1 and 2. While column (1) of Table 2 only includes county and year fixed effects, further controls are added in column (2) and (3). Due to some missing values for these covariates, the number of observations differs between specifications. Panel A of Table 2 shows the average effects estimates, in Panel B I differentiate by slope of the wage profile and Panel C shows effect heterogeneities by degree of substitutability.

Panel A of Table 2 reveals that the reforms on average had a significant and positive effect on the probability of giving birth in either t or $t+1$. A 10 percentage point increase in child care coverage is associated with a 0.55 to 0.78 percentage point increase in the probability of giving birth. This amounts to a percentage change of 7.3 - 10.4% compared to the baseline mean over 2 years (7.5%, see Table A4 Appendix). These effects are slightly larger than the effect found by Bauernschuster et al. (2016a), who estimate a 2.7% increase for t and a 2.8% increase for $t+1$. Due to the different composition of women in my data, it is however not surprising that the point estimates slightly deviate. When estimating the effects of child care coverage on births in t and births in $t+1$ separately, the results indicate a 0.2 to 0.4 percentage point increase in the probability of having a child in period t (for a 10 percentage point increase in child care coverage) and a 0.3 percentage point increase for the probability of giving birth in $t+1$. The respective estimates can be found in Table A3 in the Appendix. As a birth is still a quite rare event in my data, the number of births I observe per county per year (and especially per occupational group) is limited. In the following, I therefore rely on the specification in which I look at the joint probability of a birth in t or $t+1$.

Table 2: Effects on fertility

Dep. Var.: Birth in t or $t+1$	(1)	(2)	(3)
Panel A: Average effects			
Child care coverage	0.0550***	0.0641**	0.0775***
	(0.0212)	(0.0249)	(0.0292)
N	845,327	828,668	662,909
Panel B: Heterogeneity by wage profile (omitted: steep profile)			
Child care coverage	0.0950***	0.1059***	0.1273***
	(0.0231)	(0.0265)	(0.0311)
Child care coverage × medium slope	-0.0901***	-0.0795***	-0.0915***
	(0.0165)	(0.0166)	(0.0202)
Child care coverage × flat profile	-0.0956***	-0.0954***	-0.1131***
	(0.0181)	(0.0185)	(0.0222)
N	831,372	816,604	654,062
Panel C: Heterogeneity by substitutability (omitted: low substitutability)			
Child care coverage	0.0632***	0.0801***	0.0999***
	(0.0229)	(0.0264)	(0.0310)
Child care coverage × high substitutability	-0.0233	-0.0326**	-0.0419**
	(0.0143)	(0.0144)	(0.0175)
N	829,234	814,529	652,648
Year & county FE	√	√	√
Regional controls		√	√
Individual controls		√	√
Revenue, debt, dwellings			√
Number of counties	325	325	322

Notes: Results of the generalized Diff-in-Diff, equation 1 for Panel A, equation 2 for Panel B and C. Regional controls include controls for age-specific population shares of females between 15 and 40, age-specific population shares of the whole population older than 41, population density, the male employment rate, GDP per capita, the conservative vote share and the share of highly educated women in the fertile age. Individual controls include the pre-birth education level and age. Revenue and debt information is not available or incomplete for Hamburg, Bremen and Schleswig-Holstein, revenue information from 2009 is too fragmentary to be included. Occupational grouping based on mother's pre-birth occupation. Regressions in Panel B and C include the interactions and all main effects. Sample restrictions: women aged 15-40, West Germany, 1998-2009. Standard errors in parentheses are clustered at the individual (mother) level. Regression is weighted with the number of observations per county. * significant at 10% level, ** at 5 % level, *** at 1% level. Data source: SIAB 7514, BIBB/BAuA (2006), dataset provided by Bauernschuster et al. (2016b).

Panel B reveals that the expansion of child care slots had a significantly positive effect on women who were employed in occupations with on average steep age-earnings profiles. The increase in the probability of giving birth due to the reforms is significantly lower for women employed in occupations with medium sloped profiles, and this difference is even more pronounced for women in occupations with a flat age-earnings profile. The estimates in column (3) indicate that a 10 percentage point increase in child care coverage increases the probability of giving birth by 1.2 percentage points for women who prior to birth were employed in occupations with steep age-earnings profiles. This increase in birth rates due to the expansion of child care slots is 0.9 and 1.1 percentage points lower for women in occupations with medium sloped or flat wage profiles. The F-test of joint significance of the main effect of child care coverage and the respective interaction term is insignificant in all specifications. This indicates that the overall effect of child care coverage on women employed in occupations with on average medium sloped or flat age-earnings profiles is not statistically different from zero. The overall point estimate amounts to 0.04 for the medium sloped profile and to 0.01 for the flat profile and is thus also economically close to zero.

In Panel C, I detect a significant positive effect on the probability of giving birth for women whose pre-birth occupation is on average characterized by a low degree of substitutability between workers. As the F-test on joint significance indicates, the overall effect of child care for the high substitutability group is still significant and positive. It is however evident that the reform effect of higher child care coverage is significantly lower for women in occupations in which individual tasks are easy to transfer to co-workers. The percentage increase in fertility for women in jobs with a low degree of substitutability amounts to 7.6 - 12.0% (baseline mean of 8.3%), for women who are more easily substituted fertility increases by 6 - 8.6% (baseline mean of 6.7%).

To sum up, the positive effects on fertility are driven by women whose pre-birth occupation is characterized by a steep age-earnings profile and by women in occupations in which workers are poorer substitutes for each other.

5.2 The impact of child care on maternal employment

To investigate whether the positive effects of child care coverage on fertility are mirrored in maternal employment patterns, I estimate equation 1 and 2 with y_{ict} being 1 if a mother i of a 0-3-year old child is employed in period t. As the sample is restricted to mothers with children in the age group that was affected by the expansion of child care slots (0-3-year-olds), the sample size is considerably smaller than for the fertility analysis.

Table 3 shows the average effect estimates in Panel A and the heterogeneous effects by wage profile and substitutability in Panel B and C. The average effect estimates indicate a significant positive effect of higher child care coverage on the probability of being employed and the effect is quite stable across all three specifications. The estimate in column (3) can be interpreted as a 2.9 percentage point increase in the employment probability for a 10 percentage point increase in child care coverage.[30] Compared to the baseline mean of employment of mothers of 0-3-year-olds (46%, see Table A4), this is equivalent to a 6.3% increase in employment probability.

Panel B shows that the probability of being employed increases significantly for women working in jobs with an age-earnings profile of steep or medium slope. Referring to specification (3), a 10 percentage point increase in child care coverage leads to a 3.54 and to a 2.97 percentage points higher probability of being employed for mothers in steep and in medium

[30]This result is comparable with Müller and Wrohlich (2018), who estimate a 0.2 percentage point increase in employment probability for a 1 percentage point increase in child care coverage in Germany. They apply a similar strategy, however focus on a later time period (2007-2014).

profile occupations respectively. This is equivalent to a 7.2% (baseline mean of 49% and 41% respectively) increase in the employment probability. The F-test of joint significance is highly significant for mothers in occupations with medium-sloped profile. Mothers who prior to birth were employed in occupations with on average flat wage profiles react significantly less in two of the three specifications. The F-test of joint significance shows that the overall effect of child care is not significantly different from zero for the flat wage profile group.

Table 3: Effects on maternal employment

Dep. Var.: Employment in t	(1)	(2)	(3)
Panel A: Average effects			
Child care coverage	0.2936**	0.2648**	0.2939**
	(0.1289)	(0.1274)	(0.1321)
N	96,963	96,165	92,354
Panel B: Heterogeneity by wage profile (omitted: steep profile)			
Child care coverage	0.3679***	0.3242**	0.3543***
	(0.1316)	(0.1307)	(0.1354)
Child care coverage × medium slope	-0.0804	-0.0592	-0.0573
	(0.0811)	(0.0807)	(0.0826)
Child care coverage × flat profile	-0.1846*	-0.1484	-0.2016*
	(0.1104)	(0.1081)	(0.1113)
N	96,657	95,885	92,151
Panel C: Heterogeneity by substitutability (omitted: low substitutability)			
Child care coverage	0.3279**	0.3190**	0.3486**
	(0.1329)	(0.1314)	(0.1362)
Child care coverage × high substitutability	-0.0824	-0.1303*	-0.1400*
	(0.0729)	(0.0717)	(0.0736)
N	96,554	95,784	92,045
Year & county FE	√	√	√
Regional controls	√	√	√
Individual controls		√	√
Sibling controls			√
Number of counties	325	325	325

Notes: Results of the generalized Diff-in-Diff, equation 1 for Panel A, equation 2 for Panel B and C. Regional controls include controls for age-specific population shares of females between 15 and 40, age-specific population shares of the whole population older than 41, population density, the male employment rate, GDP per capita, the conservative vote share and the share of highly educated women in the fertile age. Individual controls include the pre-birth education level, age, age^2 and a dummy for being German. Sibling controls comprise the number of further children younger than 18 and two dummies indicating whether the newborn has a sibling aged 0-3 years or aged between 4-6 years. Occupational grouping based on mother's pre-birth occupation. Regressions in Panel B and C include the interactions and all main effects. Sample restrictions: women aged 15-40, West Germany, 1998-2009, youngest child 0-3 years old. Standard errors in parentheses are clustered at the individual (mother) level. Regression is weighted with the number of observations per county. * significant at 10% level, ** at 5 % level, *** at 1% level. Data source: SIAB 7514, BIBB/BAuA (2006), dataset provided by Bauernschuster et al. (2016b).

Panel C reveals an interesting pattern that mirrors the effects on fertility: when differentiating the employment effect of child care by the occupation-level degree to which women can be substituted at workplace, I find a significantly positive effect on those whose occupations are characterized by a low degree of substitutability, i.e. on mothers in jobs in which one worker cannot be easily replaced by a co-worker. The effect of child care on mothers in occupations where workers are better substitutes for each other is significantly lower and the overall effect on this group is not significant in two of the three specifications. Thus, a 10 percentage point increase in child care coverage increases the employment probability of mothers in occupations with a low degree of substitutability by roughly 7.6% (baseline mean of 46%) for specification (3) and does not have an effect on women in jobs in which tasks are easy to transfer to co-workers.

To summarize the results, the positive effects of child care on fertility are largely mirrored in the effects on maternal employment. The average positive effects are driven by mothers in occupations with on average medium sloped and steep age-earnings profiles and by mothers who before birth worked in a job in which the average degree of substitutability is low. The child care expansion did not have an effect on mothers whose occupations are characterized by high substitutability between workers and on mothers who prior to birth worked in occupations with flat age-earnings curves.

5.3 Validity checks

5.3.1 Was the expansion of child care slots random?

The key identifying assumption of my identification strategy is that there are no time-varying systematic differences between counties with different expansion patterns, which also have an effect on the outcome variables. Therefore, I will in the following provide evidence for the randomness of the expansion of public child care slots.

Control variables on the right-hand side of the regression. The results I presented in Section 5.1 and 5.2 have already shown that my effects are robust to including a bunch of county-level time varying controls and several individual-level covariates. This confirmed that county-level differences that evolve differently over time should not be a large concern for my identification strategy.

Control variables as outcome. Pei et al. (2018) point out that controlling for potential confounders on the right-hand side of the regression and checking whether the coefficient of interest changes due to the additional controls is just one possibility to test whether these controls would really induce omitted variable bias. This method is particularly doubtful if the included control variables are subject to measurement error or only a noisy proxy for the true confounder. Pei et al. (2018) show that the resulting attenuation bias for the noisily measured controls results in a virtually unchanged estimate of the coefficient of interest but does not help a lot to avoid omitted variable bias. The authors instead propose to put the potential confounders on the left-hand side of the regression because measurement error is less of a concern for dependent variables. If the candidate variable is no confounder, the coefficient of interest should be insignificant and close to zero. As described in Section 3, individual level background characteristics in the SIAB 7514 might be less reliable or measured with error because they are not related to the purpose of the administrative reporting process (see Fitzenberger et al., 2005). Fitzenberger et al. (2005) showed that is especially relevant for the measurement of educational degree. Moreover, information on the number of children below 18 and the age of older siblings is not directly given in the data but is derived from combing several other variables of the SIAB. Consequently these variables might be measured with noise. If this measurement error biases the coefficients of these control variables towards zero, including them in the regressions will not help to weaken omitted variable bias. I thus follow Pei et al. (2018) and - in addition to including the individual-level variables as controls on the right-hand side of the regression - investigate whether they give rise to selection bias, by putting them on the left-hand side of the regression.

As in my setting, the variation of child care coverage comes from the years after the first reform in 2005, I estimate the first difference between child care coverage in 2009 and in 2006[31] and use this difference as the main explanatory variable. For all individual-level variables, I construct averages at the county-year-level and compute the same first difference.

Table 4: Individual-level controls on LHS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var. in $\Delta_{2009-2006}$	Age	Education level			German	N^o kids	Siblings	
		low	medium	high			0-3	4-6
Δ Child care coverage	0.1637	-0.0203	0.0125	0.0079	0.1086***	0.1046	0.0262	0.0241
	(1.2652)	(0.0724)	(0.0714)	(0.0381)	(0.0373)	(0.1063)	(0.1038)	(0.0565)
N	322	322	322	322	322	322	322	322
LHS joint balancing test (p-value):								
all variables					0.0778			
w/o “German”					0.9232			

Notes: Heteroscedasticity robust standard errors in parentheses. Control variables are included in first differences between 2006 and 2009 (2008 for revenue information); all regressions include county-level controls: controls for age-specific population shares of females between 15 and 40, age-specific population shares of the whole population older than 41, population density, the male employment rate, GDP per capita, the conservative vote share and the share of highly educated women in the fertile age, revenue, debt and number of newly built dwellings. Revenue and debt information is not available or incomplete for Hamburg, Bremen and Schleswig-Holstein, revenue information from 2009 is too fragmentary to be included. LHS joint balancing test is calculated using the Stata *suest*-command, analogous to Pei et al. (2018). * significant at 10% level, ** at 5 % level, *** at 1% level. Data source: SIAB 7514, dataset provided by Bauernschuster et al. (2016b).

Table 4 shows that seven out of eight of my individual-level controls are insignificant and small in magnitude if considered as outcome variable on the left-hand side of the regression. The change in child care coverage between 2006 and 2009 seems to be unrelated to a change

[31] Information on child care coverage for 2005 is not available.

in maternal age, a change in the share of low, medium and high educated women, a change in the number of further children below 18 and the share of newborns having older siblings. The only variable which turns out be be significant is the dummy variable for being German. As nationality is reported very reliably in the SIAB 7514 (Antoni et al., 2016), it is unlikely to suffer from attenuation bias and including it as a control on the right-hand side of the regression should control for this seemingly systematically higher share of Germans in counties with strong expansion between 2006 and 2009. Analogous to Pei et al. (2018), I conduct a left-hand-side joint balancing test, which tests the hypothesis that all eight variables are balanced across counties with different expansion patterns. As long as the variable *German* is included in this test, the hypothesis is still rejected at a 10 percent significance level. Excluding *German* from the test, the hypothesis cannot be rejected, the p-value amounts to 0.932 and confirms that the counties should be balanced in all individual-level control variables but the share of Germans.

Table 5: County-level controls on LHS

	(1)	(2)	(3)	(4)	(5)
Dep. Var.: (in $\Delta_{2009-2006}$)	Population density	Conservative vote share	Female high education share	GDP per capita	Male employment rate
Δ Child care coverage	61.8078*	-0.0002	0.1579	-0.6188	-0.0106
	(31.6193)	(0.0415)	(0.1147)	(3.9057)	(0.0188)
N	322	322	322	322	322
LHS joint balancing test (p-value):					
all variables			0.1823		
w/o “pop. density”			0.5950		

Notes: Heteroscedasticity robust standard errors in parentheses. Control variables are included in first differences between 2006 and 2009 (2008 for revenue information); all regressions include the following county-level controls: controls for age-specific population shares of females between 15 and 40, age-specific population shares of the whole population older than 41, revenue, debt and number of newly built dwellings. Revenue and debt information is not available or incomplete for Hamburg, Bremen and Schleswig-Holstein, revenue information from 2009 is too fragmentary to be included. Individual-level controls include controls for age, education and German nationality. LHS joint balancing test is calculated using the Stata *suest*-command, analogous to Pei et al. (2018). * significant at 10% level, ** at 5 % level, *** at 1% level. Data source: SIAB 7514, dataset provided by Bauernschuster et al. (2016b).

The majority of county-level controls was collected from the Statistical Offices of the German *Länder*, is precisely measured and should not be subject to measurement error. The county-level time-varying controls should thus reduce potential omitted variable bias if included as controls on the regression's right-hand side. Nevertheless, I repeat the procedure of Pei et al. (2018) for the five variables I believe most relevant for child care expansion: population density, the conservative vote share as a proxy for a cultural attitude more towards the classical male-breadwinner model (see Bauernschuster et al., 2016a), the share of highly educated females reflecting the potential gains of sending children to public care (Felfe and Lalive, 2018), the GDP per capita for overall prosperity in the county and the male employment rate as a proxy for general labor market conditions in the region. The results in Table 5 show that the coefficient of the change of child care coverage between 2006 and 2009 only gets significant when population density is taken as an outcome. The left-hand-side joint balancing test for the hypothesis that these five variables are balanced across counties with a different change in child care coverage cannot be rejected at any conventional significance level.

Pre-treatment differences. Despite the fact that the county fixed effects capture time constant differences between counties, it is worthwhile to compare counties in terms of observable time-varying characteristics before the reforms. To this end, I follow Bauernschuster et al. (2016a); Havnes and Mogstad (2011) and split the counties into treatment and control regions instead of comparing pre-reform characteristics of all German counties separately. I define treatment regions as those in which the increase in public child care coverage was above the median, those with an increase below the median belong to the control group. To identify the median, I sort all West German counties by the absolute size of the increase in public child care coverage from 2002 to 2009. Table A4 in the Appendix shows pre-reform differences in outcomes between weak (C) and strong (T) expansion counties, as well as the overall pre-reform means. There is no difference in the probability of giving birth in $t = 2002$ and a very small significant difference in the probability of having a birth over two years $t = 2002$ and $t+1 = 2003$. If anything, the counties with a slightly lower birth rate (0.2 percentage points) expanded more quickly (see also Bauernschuster et al., 2016a). Concerning the probability of giving birth by subgroups, the only significant differences in the pre-treatment year 2002 can be found for the birth incidence of medium-educated women and of women with steep age-earnings profiles. Again, the difference is tiny (0.4 and 0.5 percentage points) and would suggest that regions with fewer births expanded more quickly. Looking at maternal employment, there were no significant pre-treatment differences. Table A5 depicts pre-reform differences for the independent variables. This pre-treatment comparison reveals that the strong expansion counties already had a slightly higher child care coverage in 2002, the difference however amounts to only 0.5 percentage points. The weak and strong expansion counties are quite similar in terms of county-level control variables, the only significant difference which is large enough to give rise to concerns is the share of highly educated females which is 3.5 percentage points higher in the control regions.[32] This difference in education is also visible when relying on the education level measured at the individual level and I also detect a significant difference for German nationality. As the left-hand-side test presented in the previous paragraph has shown that the change in child care coverage is unrelated to a change in education level, this pre-treatment difference in education should not threaten my identification strategy. The differences in nationality can be controlled for by including nationality as a right-hand side covariate, as long as it does not suffer from measurement error. The county fixed effects should account for all remaining county-specific time constant differences between counties.

[32] As Bauernschuster et al. (2016a) highlight, the few significant differences in population age structure are very tiny (0.06-0.01 percentage points) and do not concern the most relevant age group of 20-34 year old women.

5.3.2 Placebo regressions

One possibility to verify whether the common trend assumption holds or whether the estimated effect emerges due to reasons other than the child care expansion, is to run placebo tests. This is especially relevant for the maternal employment outcome, since the German labor market was subject to several reforms in the early 2000s.[33]

Table 6: Placebo - effect on maternal employment in $t-3$

Dep. Var.: Employed in $t-3$	(1)	(2)	(3)
Panel A: Average effects			
Child care coverage	-0.1343	-0.0487	0.0181
	(0.2530)	(0.2521)	(0.2542)
N	29,396	29,224	28,994
Panel B: Heterogeneity by wage profile (omitted: steep profile)			
Child care coverage	-0.1668	-0.1090	-0.0587
	(0.2596)	(0.2594)	(0.2615)
Child care coverage × medium slope	-0.0725	-0.0095	0.0081
	(0.1472)	(0.1467)	(0.1460)
Child care coverage × flat profile	0.2952	0.3395*	0.4153**
	(0.1981)	(0.1967)	(0.1997)
N	29,367	29,198	28,971
Panel C: Heterogeneity by substitutability (omitted: low substitutability)			
Child care coverage	-0.1114	-0.0131	0.0355
	(0.2608)	(0.2601)	(0.2627)
Child care coverage × high substitutability	-0.0664	-0.0930	-0.0307
	(0.1351)	(0.1342)	(0.1337)
N	29,313	29,145	28,917
Year & county FE	√	√	√
Regional controls	√	√	√
Individual controls		√	√
Sibling controls			√
Number of counties	325	325	325

Notes: Results of the generalized Diff-in-Diff, equation 1 for Panel A, equation 2 for Panel B and C. Regional controls include controls for age-specific population shares of females between 15 and 40, age-specific population shares of the whole population older than 41, population density, the male employment rate, GDP per capita, the conservative vote share and the share of highly educated women in the fertile age. Individual controls include the pre-birth education level, age, age^2 and a dummy for being German. Sibling controls comprise the number of further children younger than 18 and two dummies indicating whether the newborn has a sibling aged 0-3 years or aged between 4-6 years. Outcome lagged by three periods. Sample restrictions: women aged 15-40, West Germany, 1998-2009, youngest child 0-3 years old. Standard errors in parentheses are clustered at the individual (mother) level. Regression is weighted with the number of observations per county. * significant at 10% level, ** at 5 % level, *** at 1% level. Data source: SIAB 7514, BIBB/BAuA (2006), dataset provided by Bauernschuster et al. (2016b).

Although I am not aware of any reform which differed between counties in a way similar to the expansion of child care slots, I re-estimate equation 1 and 2 on maternal employment

[33]For example the introduction of *Hartz IV* in 2005 which among others changed regulations concerning unemployment benefits.

but lag the outcome by three periods. I should not find an effect of child care coverage in t on employment in $t-3$ of mothers whose children are 0-3 years old in t because these children are newborns or have not yet been born in $t-3$. Table 6 shows that the effects indeed turn insignificant and much closer to zero compared to the effects I detected in Table 3. The F-test of joint significance of the main effect of child care plus the respective interaction term is not significant for any of the subgroups.

5.3.3 County-specific linear time trends

As the placebo regressions in Section 5.3.2 showed, the trends in maternal employment do not seem to diverge before the treatment between counties with different speed and intensity of child care expansion. To eliminate the minor remaining deviations in trends both for maternal employment and for fertility, I include county-specific linear time trends in the regressions. Wolfers (2006) shows that including state-specific time trends in the form of a $state_s \times time_t$ interaction term in a Difference-in-Differences approach might lead to an intermingling of the pre-existing trends and the dynamic effects from the policy reform. To avoid that my trends confound pre-treatment trends and the effects of child care expansion, I proceed as follows: I aggregate my data at the county-year-level and calculate the pre-treatment trend (1998-2004) in fertility and maternal employment for each county separately. The estimated β-coefficients for the pre-treatment trends in the outcomes are then used to predict the trends for the years 2005 to 2010. These county-specific trends are subsequently matched to the individual-level data and included in the regressions as a control variable. Results for including this county-specific time trend in a linear way are depicted in Tables 7 and 8 for the average effects and the effect heterogeneities by wage profile and substitutability. The results remain qualitatively the same if trends are included quadratically.[34]

Both the effects for fertility and the effects for maternal employment remain virtually unaffected by the inclusion of county-specific linear time trends. This corroborates the assumption that counties with a different speed and intensity of child care expansion do not follow different fertility and employment trends in absence of the reforms.

5.3.4 Selective migration

One potential issue for my identification strategy is that women might move from counties with low to counties with high child care coverage in response to the reforms. This selective migration could be considered as part of the effect of child care expansion if a women moves to a county with high child care coverage and decides to have a child (to enter the labor market) because of this higher child care coverage. I would however overestimate the treatment effect, if women who would have had a child (would have entered the labor market) anyway move to a county with a more intense expansion pattern. To avoid that my effects are driven by selective

[34] Results can be obtained upon request.

migration, I use the woman's county of residency in 2004 (or if this information is missing in the years before 2004) to assign the intensity of the treatment, i.e. I keep the woman's county constant at the pre-reform county. Table 9 shows the coefficients using the pre-reform county to assign child care coverage for fertility, Table 10 for maternal employment.

The point estimates of the effect of child care on fertility stay both qualitatively and quantitatively very similar to the baseline. The differences between women employed in occupations with on average low versus high substitutability even get a bit more pronounced. The point estimates on maternal employment decrease and partly lose significance, but are still qualitatively similar to the baseline.

5.3.5 Parental leave reform in 2007

Between 2005 and 2008, when the child care reforms were implemented, the German government passed a further family policy reform in 2007, resulting in substantial modifications of the parental leave system. The change from a means-tested to an earnings related system, the reduction in benefit duration from 24 to 12 months and a change in eligibility criteria altogether made high-income parents benefit and low-income parents lose. This triggered significant effects on women's labor supply[35] (e.g. Kluve and Tamm, 2013) and increased fertility (e.g. Raute, 2019), especially for women with higher education. The inclusion of year fixed effects should protect my estimates from being confounded by the parental leave reform, as the reform was implemented in all German counties and there is no good reason why its impact should vary systematically between regions. Nevertheless, I follow Bauernschuster et al. (2016a) and include an interaction term of a post-2007 dummy and the woman's education level to capture potential changes in the relationship between education and fertility or maternal employment induced by the parental leave reform. Table 11 shows the results for fertility, Table 12 the effects on maternal employment.

The coefficients on the probability of giving birth slightly decrease, they are however still large and significant in almost all specifications. In line with the results by Raute (2019) and the robustness check of Bauernschuster et al. (2016a), the coefficients for the interaction of the post-2007 dummy with the dummies for medium and high education are significantly positive and larger in magnitude for high education women. The estimates on employment are neither qualitatively nor quantitatively very much affected and the interactions of the post-2007 dummy with medium and high education remain insignificant.

[35]Negative effects in the first year after birth, positive effects thereafter.

Table 7: County-specific linear time trends - effects on fertility

Dep. Var.: Birth in t or $t+1$	(1)	(2)	(3)
Panel A: Average effects			
Child care coverage	0.0283	0.0433*	0.0536*
	(0.0215)	(0.0250)	(0.0292)
N	845,327	828,668	662,909
Panel B: Heterogeneity by wage profile (omitted: steep profile)			
Child care coverage	0.0679***	0.0848***	0.1029***
	(0.0233)	(0.0266)	(0.0312)
Child care coverage × medium slope	-0.0897***	-0.0791***	-0.0911***
	(0.0165)	(0.0166)	(0.0202)
Child care coverage × flat profile	-0.0959***	-0.0955***	-0.1129***
	(0.0181)	(0.0185)	(0.0222)
N	831,372	816,604	654,062
Panel C: Heterogeneity by substitutability (omitted: low substitutability)			
Child care coverage	0.0359	0.0588**	0.0754**
	(0.0232)	(0.0265)	(0.0311)
Child care coverage × high substitutability	-0.0227	-0.0323**	-0.0414**
	(0.0143)	(0.0144)	(0.0175)
N	829,234	814,529	652,648
County-specific linear trend	√	√	√
Year & county FE	√	√	√
Regional controls		√	√
Individual controls		√	√
Revenue, debt, dwellings			√
Number of counties	325	325	322

Notes: Results of the generalized Diff-in-Diff, equation 1 for Panel A, equation 2 for Panel B and C. Regional controls include controls for age-specific population shares of females between 15 and 40, age-specific population shares of the whole population older than 41, population density, the male employment rate, GDP per capita, the conservative vote share and the share of highly educated women in the fertile age. Individual controls include the pre-birth education level and age. Revenue and debt information is not available or incomplete for Hamburg, Bremen and Schleswig-Holstein, revenue information from 2009 is too fragmentary to be included. Additionally a linear county-specific time trend is included. Sample restrictions: women aged 15-40, West Germany, 1998-2009. Standard errors in parentheses are clustered at the individual (mother) level. Regression is weighted with the number of observations per county. * significant at 10% level, ** at 5 % level, *** at 1% level. Data source: SIAB 7514, BIBB/BAuA (2006), dataset provided by Bauernschuster et al. (2016b).

Table 8: County-specific linear time trends - effects on maternal employment

Dep. Var.: Employed in *t*	(1)	(2)	(3)
Panel A: Average effects			
Child care coverage	0.3729***	0.3387***	0.3616***
	(0.1287)	(0.1273)	(0.1321)
N	96,963	96,165	92,354
Panel B: Heterogeneity by wage profile (omitted: steep profile)			
Child care coverage	0.4456***	0.3973***	0.4219***
	(0.1315)	(0.1307)	(0.1355)
Child care coverage × medium slope	-0.0812	-0.0600	-0.0585
	(0.0810)	(0.0806)	(0.0824)
Child care coverage × flat profile	-0.1936*	-0.1563	-0.2057*
	(0.1100)	(0.1079)	(0.1111)
N	96,657	95,885	92,151
Panel C: Heterogeneity by substitutability (omitted: low substitutability)			
Child care coverage	0.4065***	0.3916***	0.4157***
	(0.1328)	(0.1314)	(0.1362)
Child care coverage × high substitutability	-0.0803	-0.1218*	-0.1379*
	(0.0728)	(0.0716)	(0.0735)
N	96,554	95,784	92,045
County-specific linear trend	√	√	√
Year & county FE	√	√	√
Regional controls	√	√	√
Individual controls		√	√
Sibling controls			√
Number of counties	325	325	325

Notes: Results of the generalized Diff-in-Diff, equation 1 for Panel A, equation 2 for Panel B and C. Regional controls include controls for age-specific population shares of females between 15 and 40, age-specific population shares of the whole population older than 41, population density, the male employment rate, GDP per capita, the conservative vote share and the share of highly educated women in the fertile age. Individual controls include the pre-birth education level, age, age^2 and a dummy for being German. Sibling controls comprise the number of further children younger than 18 and two dummies indicating whether the newborn has a sibling aged 0-3 years or aged between 4-6 years. Additionally a linear county-specific time trend is included. Sample restrictions: women aged 15-40, West Germany, 1998-2009, youngest child 0-3 years old. Standard errors in parentheses are clustered at the individual (mother) level. Regression is weighted with the number of observations per county. * significant at 10% level, ** at 5 % level, *** at 1% level. Data source: SIAB 7514, BIBB/BAuA (2006), dataset provided by Bauernschuster et al. (2016b).

Table 9: Accounting for selective migration - effects on fertility

Dep. Var.: Birth in t or $t+1$	(1)	(2)	(3)
Panel A: Average effects			
Child care coverage	0.0567**	0.0472*	0.0615*
	(0.0243)	(0.0282)	(0.0323)
N	725,643	715,946	591,911
Panel B: Heterogeneity by wage profile (omitted: steep profile)			
Child care coverage	0.1049***	0.0999***	0.1164***
	(0.0263)	(0.0300)	(0.0343)
Child care coverage × medium slope	-0.0842***	-0.0853***	-0.0883***
	(0.0197)	(0.0197)	(0.0233)
Child care coverage × flat profile	-0.1507***	-0.1575***	-0.1636***
	(0.0214)	(0.0217)	(0.0255)
N	720,724	711,262	587,808
Panel C: Heterogeneity by substitutability (omitted: low substitutability)			
Child care coverage	0.0858***	0.0799***	0.0982***
	(0.0263)	(0.0300)	(0.0342)
Child care coverage × high substitutability	-0.0614***	-0.0675***	-0.0725***
	(0.0171)	(0.0171)	(0.0201)
N	718,908	709,504	586,556
Year & county FE	√	√	√
Regional controls		√	√
Individual controls		√	√
Revenue, debt, dwellings			√
Number of counties	325	325	322

Notes: Results of the generalized Diff-in-Diff, equation 1 for Panel A, equation 2 for Panel B and C. Regional controls include controls for age-specific population shares of females between 15 and 40, age-specific population shares of the whole population older than 41, population density, the male employment rate, GDP per capita, the conservative vote share and the share of highly educated women in the fertile age. Individual controls include the pre-birth education level and age. Revenue and debt information is not available or incomplete for Hamburg, Bremen and Schleswig-Holstein, revenue information from 2009 is too fragmentary to be included. Contains county information from $t \leq 2004$. Sample restrictions: women aged 15-40, West Germany, 1998-2009. Standard errors in parentheses are clustered at the individual (mother) level. Regression is weighted with the number of observations per county. * significant at 10% level, ** at 5 % level, *** at 1% level. Data source: SIAB 7514, BIBB/BAuA (2006), dataset provided by Bauernschuster et al. (2016b).

Table 10: Accounting for selective migration - effects on maternal employment

Dep. Var.: Employed in t	(1)	(2)	(3)
Panel A: Average effects			
Child care coverage	0.2286*	0.1820	0.2118
	(0.1304)	(0.1283)	(0.1330)
N	94,979	94,302	90,554
Panel B: Heterogeneity by wage profile (omitted: steep profile)			
Child care coverage	0.2987**	0.2474*	0.2768**
	(0.1327)	(0.1315)	(0.1363)
Child care coverage × medium slope	-0.0999	-0.0980	-0.0933
	(0.0819)	(0.0808)	(0.0828)
Child care coverage × flat profile	-0.1117	-0.1134	-0.1642
	(0.1137)	(0.1113)	(0.1145)
N	94,718	94,050	90,378
Panel C: Heterogeneity by substitutability (omitted: low substitutability)			
Child care coverage	0.2647**	0.2389*	0.2694**
	(0.1341)	(0.1321)	(0.1369)
Child care coverage × high substitutability	-0.0805	-0.1281*	-0.1375*
	(0.0744)	(0.0730)	(0.0750)
N	94,609	93,944	90,267
Year & county FE	√	√	√
Regional controls	√	√	√
Individual controls		√	√
Sibling controls			√
Number of counties	325	325	325

Notes: Results of the generalized Diff-in-Diff, equation 1 for Panel A, equation 2 for Panel B and C. Regional controls include controls for age-specific population shares of females between 15 and 40, age-specific population shares of the whole population older than 41, population density, the male employment rate, GDP per capita, the conservative vote share and the share of highly educated women in the fertile age. Individual controls include the pre-birth education level, age, age^2 and a dummy for being German. Sibling controls comprise the number of further children younger than 18 and two dummies indicating whether the newborn has a sibling aged 0-3 years or aged between 4-6 years. Contains county information from $t \leq 2004$. Sample restrictions: women aged 15-40, West Germany, 1998-2009, and youngest child 0-3 years old. Standard errors in parentheses are clustered at the individual (mother) level. Regression is weighted with the number of observations per county. * significant at 10% level, ** at 5 % level, *** at 1% level. Data source: SIAB 7514, BIBB/BAuA (2006), dataset provided by Bauernschuster et al. (2016b).

Table 11: Parental leave reform in 2007 - effects on fertility

Dep. Var.: Birth in t or $t+1$	(1)	(2)	(3)
Panel A: Average effects			
Child care coverage	0.0401*	0.0596**	0.0731**
	(0.0214)	(0.0249)	(0.0292)
Post2007 × medium education	0.0146***	0.0153***	0.0143***
	(0.0015)	(0.0015)	(0.0017)
Post2007 × high education	0.0297***	0.0298***	0.0264***
	(0.0032)	(0.0033)	(0.0036)
N	828,668	828,668	662,909
Panel B: Heterogeneity by wage profile (omitted: steep profile)			
Child care coverage	0.0715***	0.0933***	0.1151***
	(0.0233)	(0.0265)	(0.0312)
Child care coverage × medium slope	-0.0651***	-0.0666***	-0.0792***
	(0.0166)	(0.0166)	(0.0202)
Child care coverage × flat profile	-0.0652***	-0.0677***	-0.0868***
	(0.0186)	(0.0186)	(0.0224)
Post2007 × medium education	0.0133***	0.0139***	0.0129***
	(0.0016)	(0.0016)	(0.0017)
Post2007 × high education	0.0268***	0.0269***	0.0235***
	(0.0033)	(0.0033)	(0.0037)
N	816,604	816,604	654,062
Panel C: Heterogeneity by substitutability (omitted: low substitutability)			
Child care coverage	0.0497**	0.0705***	0.0908***
	(0.0231)	(0.0264)	(0.0310)
Child care coverage × high substitutability	-0.0206	-0.0211	-0.0311*
	(0.0144)	(0.0144)	(0.0175)
Post2007 × medium education	0.0146***	0.0151***	0.0141***
	(0.0016)	(0.0016)	(0.0017)
Post2007 × high education	0.0297***	0.0297***	0.0262***
	(0.0033)	(0.0033)	(0.0036)
N	814,529	814,529	652,648
Year & county FE	√	√	√
Regional controls		√	√
Individual controls		√	√
Revenue, debt, dwellings			√
Number of counties	325	325	322

Notes: Results of the generalized Diff-in-Diff, equation 1 for Panel A, equation 2 for Panel B and C. Regional controls include controls for age-specific population shares of females between 15 and 40, age-specific population shares of the whole population older than 41, population density, the male employment rate, GDP per capita, the conservative vote share and the share of highly educated women in the fertile age. Individual controls include the pre-birth education level and age. Revenue and debt information is not available or incomplete for Hamburg, Bremen and Schleswig-Holstein, revenue information from 2009 is too fragmentary to be included. Both interaction term and main effects of *post2007* and education level included. Sample restrictions: women aged 15-40, West Germany, 1998-2009. Standard errors in parentheses are clustered at the individual (mother) level. Regression is weighted with the number of observations per county. * significant at 10% level, ** at 5 % level, *** at 1% level. Data source: SIAB 7514, BIBB/BAuA (2006), dataset provided by Bauernschuster et al. (2016b).

Table 12: Parental leave reform in 2007 - effects on maternal employment

Dep. Var.: Employed in *t*	(1)	(2)	(3)
Panel A: Average effects			
Child care coverage	0.2863**	0.2658**	0.2946**
	(0.1282)	(0.1274)	(0.1322)
Post2007 × medium education	0.0118	0.0034	0.0119
	(0.0112)	(0.0111)	(0.0115)
Post2007 × high education	0.0028	-0.0025	0.0083
	(0.0170)	(0.0168)	(0.0172)
N	96,166	96,165	92,354
Panel B: Heterogeneity by wage profile (omitted: steep profile)			
Child care coverage	0.3582***	0.3267**	0.3529***
	(0.1316)	(0.1310)	(0.1357)
Child care coverage × medium slope	-0.0727	-0.0591	-0.0518
	(0.0815)	(0.0811)	(0.0830)
Child care coverage × flat profile	-0.1767	-0.1483	-0.1887*
	(0.1107)	(0.1092)	(0.1125)
Post2007 × medium education	0.0135	0.0053	0.0129
	(0.0113)	(0.0112)	(0.0116)
Post2007 × high education	-0.0044	-0.0092	0.0019
	(0.0171)	(0.0170)	(0.0174)
N	95,886	95,885	92,151
Panel C: Heterogeneity by substitutability (omitted: low substitutability)			
Child care coverage	0.3488***	0.3223**	0.3487**
	(0.1324)	(0.1316)	(0.1364)
Child care coverage × high substitutability	-0.1359*	-0.1331*	-0.1365*
	(0.0727)	(0.0720)	(0.0739)
Post2007 × medium education	0.0114	0.0027	0.0114
	(0.0113)	(0.0112)	(0.0115)
Post2007 × high education	-0.0048	-0.0098	0.0029
	(0.0171)	(0.0170)	(0.0173)
N	95,785	95,784	92,045
Year & county FE	√	√	√
Regional controls	√	√	√
Individual controls		√	√
Sibling controls			√
Number of counties	325	325	325

Notes: Results of the generalized Diff-in-Diff, equation 1 for Panel A, equation 2 for Panel B and C. Regional controls include controls for age-specific population shares of females between 15 and 40, age-specific population shares of the whole population older than 41, population density, the male employment rate, GDP per capita, the conservative vote share and the share of highly educated women in the fertile age. Individual controls include the pre-birth education level, age, age^2 and a dummy for being German. Sibling controls comprise the number of further children younger than 18 and two dummies indicating whether the newborn has a sibling aged 0-3 years or aged between 4-6 years. Both interaction term and main effects of *post2007* and education level included. Sample restrictions: women aged 15-40, West Germany, 1998-2009, and youngest child 0-3 years old. Standard errors in parentheses are clustered at the individual (mother) level. Regression is weighted with the number of observations per county. * significant at 10% level, ** at 5 % level, *** at 1% level. Data source: SIAB 7514, BIBB/BAuA (2006), dataset provided by Bauernschuster et al. (2016b).

5.4 Robustness checks

5.4.1 Alternative subgroup classifications

Besides providing evidence for the validity of my results, I further check their robustness by considering two alternative subgroup classifications for the heterogeneity analysis.

First, occupations can also be classified based on the tasks performed by a worker. Adda et al. (2017) show that *abstract* occupations, which are characterized by an ongoing need of skill updating, pay high wages but also punish job interruptions due to higher atrophy rates and are thus less family-friendly than *manual* or *routine* occupations. I use the dataset provided by Dengler et al. (2014) who apply the task-based approach developed by Autor et al. (2003) to occupations in the German labor market. Using expert knowledge about competencies and skills needed for the respective job, Dengler et al. (2014) compute the main task type and the composition of tasks of every 2-digit occupation. In this robustness check, I follow Adda et al. (2017) and summarize occupations to be mainly *abstract*, or *routine & manual task-intensive* occupations.

Second, Becker (1965) and Mincer (1963) highlight that the price of having children and of providing care is higher for high income families with a higher potential female wage rate. Therefore, besides differentiating the effects of child care by occupational group, a further heterogeneity to look at is by educational degree. I take educational information from the SIAB 7514 individual-level data and define education groups as follows. An individual is classified as having low education if she does not have any vocational degree. The category medium education includes individuals with primary, secondary or intermediate school leaving degree with vocational qualification and high education is assigned to those with technical college or university degree.

I rely on the pre-birth information on education and occupation for this assignment. Analogously to equation 2, I interact the treatment variable d_{ct} with either a dummy variable indicating that workers in woman i's respective occupation perform mainly routine and manual tasks at work (omitted category: abstract tasks) or with indicators for low and medium education (omitted category: high education).

The results for these further effect heterogeneities concerning fertility and maternal employment can be found in Table A6 and A7 in the Appendix respectively. Panel A of both tables reveals a significant and positive effect of an increase in child care coverage on fertility and maternal employment for the high education group. While the interaction terms in Table A6 indicate a significantly lower responsiveness of women with medium and low education in terms of fertility, Table A7 does not provide evidence for significant differences concerning employment between mothers with different education level. The F-test of joint significance however indicates that the overall effect on employment of mothers in the medium education group is still significant, whereas the effect on mothers with low education turns out to be insignificant. In Panel B of both tables, I detect a large and highly significant effect on women in abstract occupations. The effect on women in routine & manual occupations tends to be smaller, the difference is however not significant. The results for the educational classification are in line with the classical concept of opportunity cost in which the price of staying at home is higher for women with higher education level due to the positive correlation of education and - in this case - forgone wage. One possible reason for the lack of effect heterogeneities by task might be that I cannot directly replicate the occupational classification by Adda et al. (2017) with my data. The two categories of *abstract* and *routine & manual* tasks might contain different occupations and this could explain that my results differ in this respect.

5.4.2 Alternative definition of substitutability

In a further robustness check, I slightly change the definition of the two substitutability groups by dropping occupations that cannot be clearly assigned to having a rather low or a rather high degree of substitutability. To this end, I sort occupations into 11 percentiles of the substitutability index and drop percentile 6.[36] Table A8 and A9 in the Appendix show the results for fertility and maternal employment respectively. When excluding occupations that are on the margin between the two categories, the difference between women in occupations in which substitution between co-workers is difficult and those in occupations with many close substitutes gets even more pronounced. The overall effect for women in occupations with high substitutability is no more statistically different from zero for both fertility and maternal employment.

6 Discussion and conclusion

We already know from the previous literature that the provision of easily available and affordable public child care might encourage both fertility and maternal employment. None of the existing studies[37], however, have had a deeper look into possibly important effect heterogeneities which might emerge due to differences in the career costs of children women are facing. This is surprising in the light of the recent literature (e.g. Goldin, 2014; Adda et al., 2017) which highlights that these career costs vary between occupations and educational degree and given that reconciling family and career is on the top of the political agenda in many Western countries.

My paper makes a first step to close this research gap by investigating the effect heterogeneity of several policy initiatives in Germany which led to a large-scaled expansion of child care slots for under three-year-olds and by relying on data that enable me to differentiate individuals by occupation. In a generalized Diff-in-Diff, I exploit the regional and temporal variation in the

[36] These are 2-digit occupations 9, 11, 16, 17, 39, 41, 71 and 99 (KldB88).

[37] Except few studies which differentiate the effects on maternal employment by education (e.g. Baker et al., 2008; Schlosser, 2011; Müller and Wrohlich, 2018).

intensity of child care expansion to estimate heterogeneous effects on both fertility and maternal employment. In line with the existing literature, I detect that an increase in the child care coverage has on average positive and significant effects on both fertility and maternal employment. In comparison to the previous papers, I can however deliver additional evidence on so far unidentified heterogeneities: these average positive effects are indeed driven by the groups of women that tend to face higher career costs when having children. Both concerning fertility and concerning maternal employment the pattern is as follows.

First, the effects are heterogeneous across women who prior to birth were employed in occupations with different age-earnings profiles. The positive effects on fertility (maternal employment) are mainly driven by women in occupations with a steep (medium-sloped or steep) age-earnings profile, i.e. for whom earnings increase fast with increasing age. This result can be explained by the findings of Adda et al. (2017), who show that work interruptions in the middle of the career are more costly in occupations with steep age-earnings profiles because the forgone earnings are higher. As the average age at first birth in my data is 29 years, the age at which women have to interrupt work if they become a mother is indeed right in the middle of the career. This period of work life is characterized by an extremely strong increase in wages for occupations with a steep age-earnings profile (see Figure 1). Better and cheaper access to high quality child care enables women to reduce the high opportunity cost of staying at home during this period and is thus especially helpful for women employed in jobs with a steep wage profile.

Second, the effects differ between women whose pre-birth occupations vary in terms of average degree of substitutability. Providing higher child care coverage results in positive effects for women who work in occupations in which the main tasks cannot be easily transferred to a co-worker, i.e. whose jobs are characterized by an on average low degree of substitutability. Women working in occupations in which one worker can be easily substituted by another worker, react significantly less. This result is in line with the framework provided by Goldin (2014) who highlights that non-linearities in an occupation's pay structure arise whenever workers do not have perfect substitutes. This means that for example working fewer hours imposes higher wage penalties on women who do not have close substitutes at the workplace. These penalties on earnings arise because firms have higher costs for providing workplace flexibility, if handing off clients from one worker to another goes along with a deterioration in the customer relationships (Goldin and Katz, 2016). If mothers return to the job after a shorter period of time and are less dependent on flexible work schedules, they can more easily maintain contacts to clients and this should reduce the child penalty in earnings. Therefore, the provision of public child care as substitute for maternal care has a more pronounced effect on women in occupations in which substitution of workers is difficult.

What remains to be discussed is in how far my results can be generalized to the whole population. As pointed out in Section 3, my administrative dataset contains rich information on occupations, it however only covers a specific part of the German working population. We cannot per se assume that the results are also valid for self-employed women, women in civil service and women who have never entered an employment subject to social security before giving birth. Self-employment is usually characterized by a higher flexibility of the work schedule, more opportunities to provide maternal care and it is often argued that self-employment improves the compatibility of family and career (e.g. Joona, 2017; Connelly, 1992). Civil servants in Germany are generally subject to slightly different regulations concerning, among others, family benefits. They for example receive additional family allowances ("*Familienzuschlag*") depending on their marital status and number of children, which are not granted to employees subject to social security payments. Moreover, civil servants are usually entitled to a larger amount of parental leave benefits, as their income does not underlie reductions due to social security contributions which results in a higher net income. Therefore, I would expect that the effects for these groups might differ from those I detected for women employed subject to social security or officially registered as unemployed in the administrative data. My effects should thus be interpreted as effects on the group captured in my data only. Moreover, as pointed out in Section 3, women with low education (flat wage profiles) are more likely to have gaps in their work career, during which I cannot observe them in my data and can consequently also not identify births. As gaps for all education levels (wage profiles) do not differ systematically between weak and strong expansion counties, this should not bias my estimates. For the interpretation of the effects, it should however be kept in mind that the share of women with low education (flat wage profile), which is not captured in my data, is higher than the respective share of women with medium or high education (medium sloped or steep wage profile).

For policy-makers, my results are good news because they demonstrate that the provision of child care slots for under-three-year-olds is indeed most beneficial for women facing relatively higher career costs of children. This high responsiveness of the group that was originally targeted by this policy might serve as a motivation for policy-makers to implement further policies which aim to improve the compatibility of family and career and reduce the career costs of having children.

References

Adda, J., Dustmann, C., and Stevens, K. (2017). The Career Costs of Children. *Journal of Political Economy*, 125(2): 293–337.

Antoni, M., Ganzer, A., and vom Berge, P. (2016). Sample of Integrated Labour Market Biographies (SIAB) 1975 - 2014. *FDZ data report*, No. 04/2016 (en).

Apps, P. and Rees, R. (2004). Fertility, Taxation and Family Policy. *The Scandinavian Journal of Economics*, 106(4): 745–763.

Arntz, M. and Gathmann, C. (2014). Permanent Changes in the Wage Structure and the East German Fertility Crisis. *Beiträge zur Jahrestagung des Vereins für Socialpolitik 2014: Evidenzbasierte Wirtschaftspolitik - Session: Marriage and Children*, No. B02-V2.

Autor, D., Levy, F., and Murnane, R. (2003). The Skill Content of Recent Technological Change: An Empirical Exploration. *The Quarterly Journal of Economics*, 118(4): 1279–1333.

Baker, M., Gruber, J., and Milligan, K. (2008). Universal Child Care, Maternal Labor Supply, and Family Well-Being. *Journal of Political Economy*, 116(4): 709–745.

Bauernschuster, S., Hener, T., and Rainer, H. (2016a). Children of a (Policy) Revolution: The Introduction of Universal Child Care and its Effect on Fertility. *Journal of the European Economic Association*, 14(4): 975–1005.

Bauernschuster, S., Hener, T., and Rainer, H. (2016b). [Dataset] Supplementary Data, Online Version - Children of a (Policy) Revolution: The Introduction of Universal Child Care and Its Effect on Fertility. Journal of the European Economic Association. https://doi.org/10.1111/jeea.12158.

Bauernschuster, S. and Schlotter, M. (2015). Public Child Care and Mothers' Labor Supply - Evidence from Two Quasi-Experiments. *Journal of Public Economics*, 123: 1–16.

Becker, G. S. (1965). A Theory of the Allocation of Time. *The Economic Journal*, 75(299): 493–517.

Becker, G. S. (1991). *A Treatise on the Family Enlarged Edition*. Cambridge, London: Harvard University Press.

Berlinski, S. and Galiani, S. (2007). The Effect of a Large Expansion of Pre-Primary School Facilities on Preschool Attendance and Maternal Employment. *Labour Economics*, 14: 665–680.

Bertrand, M., Goldin, C., and Katz, L. F. (2010). Dynamics of the Gender Gap for Young Professionals in the Financial and Corporate Sectors. *American Economic Journal: Applied Economics*, 2: 228–255.

BIBB/BAuA (2006). [Dataset] BIBB/BAuA Employment Survey of the Working Population on Qualification and Working Conditions in Germany 2006.

Bick, A. (2016). The Quantitative Role of Child Care for Female Labor Force Participation and Fertility. *Journal of the European Economic Association*, 14(3): 639–668.

Björklund, A. (2006). Does Family Policy Affect Fertility? Lessons from Sweden. *Journal of Population Economics*, 19(1): 3–24.

Blau, D. M. and Currie, J. (2006). Pre-School, Day Care, and After-School Care: Who's Minding the Kids? In: Hanushek, E. A. and Welch, F. (editors), *Handbook of the Economics of Education 2*, pp. 1163–1278.

BMFSFJ (2004). Das Tagesbetreuungsausbaugesetz (TAG). Berlin: Bundesministerium für Familien, Senioren, Frauen und Jugend.

Busse, A. and Gathmann, C. (2015). The Effects of Free Childcare on Labor Supply and Children. *Beiträge zur Jahrestagung des Vereins für Socialpolitik 2015: Ökonomische Entwicklung - Theorie und Politik - Session: Children 2*, No. F07-V2.

Cascio, E. U. (2009). Maternal Labor Supply and the Introduction of Kindergartens into American Public Schools. *The Journal of Human Resources*, 44(1): 140–170.

Connelly, R. (1992). Self-Employment and Providing Child Care. *Demography*, 29(1): 17–29.

Dengler, K., Matthes, B., and Paulus, W. (2014). Occupational Tasks in the German Labour Market. *FDZ Methodenreport*, No. 12.

DESTATIS (2019). Bevölkerung (Zensus): Deutschland, Stichtag, Geschlecht, Altersgruppen. DESTATIS - Statistisches Bundesamt. https://www-genesis.destatis.de/genesis/online.

Dorbritz, J. (2008). Germany: Family Diversity With Low Actual and Desired Fertility. *Demographic Research*, 19(17): 557–598.

Ermisch, J. (2003). *An Economic Analysis of the Family*. Princeton University Press.

Eurostat (2018). Beschäftigungsquote von Erwachsenen nach Geschlecht, Altersgruppe, Bildungsabschluss, Zahl der Kinder und Alter des jüngsten Kindes. Europäische Kommission, Eurostat, Daten, Datenbank. https://ec.europa.eu/eurostat/de/data/database.

Felfe, C. and Lalive, R. (2012). Early Child Care and Child Development: For Whom it Works and Why. *IZA Discussion Paper*, No. 7100.

Felfe, C. and Lalive, R. (2014). Does Early Child Care Help or Hurt Children's Development? *IZA Discussion Paper*, No. 8484.

Felfe, C. and Lalive, R. (2018). Does Early Child Care Affect Children's Development? *Journal of Public Economics*, 159: 33–53.

Fitzenberger, B., Osikominu, A., and Völter, R. (2005). Imputation Rules to Improve the Education Variable in the IAB Employment Subsample. *FDZ Methodenreport*, No. 03.

Geyer, J., Haan, P., and Wrohlich, K. (2015). The Effects of Family Policy on Maternal Labor Supply: Combining Evidence from a Structural Model and a Quasi-Experimental Approach. *Labour Economics*, 36: 84–98.

Goldin, C. (2014). A Grand Gender Convergence: Its Last Chapter. *American Economic Review*, 104(4): 1091–1119.

Goldin, C. and Katz, L. F. (2008). Transitions: Career and Family Life Cycles of the Educational Elite. *American Economic Review: Papers & Proceedings*, 98(2): 363–369.

Goldin, C. and Katz, L. F. (2011). The Cost of Workplace Flexibility for High-Powered Professionals. *The ANNALS of the American Academy of Political and Social Science*, 638(45): 45–67.

Goldin, C. and Katz, L. F. (2016). A Most Egalitarian Profession: Pharmacy and the Evolution of a Family-Friendly Occupation. *Journal of Labor Economics*, 34(3): 705–746.

Hall, A. and Tiemann, M. (2009). BIBB/BAuA Employment Survey of the Working Population on Qualification and Working Conditions in Germany 2006. vt1.0. Bonn: Research Data Center at BIBB (ed., data access); Federal Institute for Vocational Education and Training. DOI: 10.7803/501.06.1.4.10.

Havnes, T. and Mogstad, M. (2011). Money for Nothing? Universal Child Care and Maternal Employment. *Journal of Public Economics*, 195: 1455–1465.

Hotz, V. J., Johansson, P., and Karimi, A. (2018). Parenthood, Family Friendly Firms, and the Gender Gaps in Early Work Careers. *NBER Working Paper*, No. 24173.

Hüsken, K. (2011). Kita vor Ort. Betreuungsatlas auf Ebene der Jugendamtsbezirke 2010. Deutsches Jugendinstitut e.V.

Joona, P. (2017). Are Mothers of Young Children More Likely to Be Self-Employed? The Case of Sweden. *Review of Economics of the Household*, 15(1): 307–333.

Kleven, H., Landais, C., and Sogaard, J. E. (2018). Children and Gender Inequality: Evidence from Denmark. *NBER Working Paper*, No. 24219.

Kluve, J. and Tamm, M. (2013). Parental Leave Regulations, Mothers' Labor Force Attachment and Fathers' Childcare Involvement: Evidence from a Natural Experiment. *Journal of Population Economics*, 26: 983–1005.

Kreyenfeld, M. and Geisler, E. (2006). Müttererwerbstätigkeit in Ost- und Westdeutschland. *Zeitschrift für Familienforschung*, 18(3): 333–360.

Lefebvre, P. and Merrigan, P. (2008). Child-Care Policy and the Labor Supply of Mothers with Young Children: A Natural Experiment from Canada. *Journal of Labor Economics*, 26(3): 519–548.

Lundin, D., Mörk, E., and Öckert, B. (2008). How Far Can Reduced Childcare Prices Push Female Labour Supply? *Labour Economics*, 15: 647–659.

Mincer, J. (1963). Market Prices, Opportunity Costs, and Income Effects. In: Christ, C. F. (editor), *Measurement in Economics - Studies in Mathematical Economics and Econometrics in Memory of Yehuda Grunfeld*, Stanford University Press, pp. 67–83.

Mincer, J. (1974). Age and Experience Profiles of Earnings. In: Mincer, J. (editor), *Schooling, Experience, and Earnings*, National Bureau of Economic Research, pp. 64–82.

Müller, D. and Strauch, K. (2017). Identifying Mothers in Administrative Data. *FDZ Methodenreport*, No. 13/2017.

Müller, K.-U. and Wrohlich, K. (2018). Does Subsidized Care for Toddlers Increase Maternal Labor Supply? Evidence from a Large-Scale Expansion of Early Childcare. *DIW Discussion Papers*, No. 1747.

Mörk, E., Sjörgen, A., and Svaleryd, H. (2013). Childcare Costs and the Demand for Children - Evidence from a Nationwide Reform. *Journal of Population Economics*, 26: 33–65.

Pei, Z., Pischke, J.-S., and Schwandt, H. (2018). Poorly Measured Confounders are More Useful on the Left than on the Right. *Journal of Business & Economic Statistics*, forthcoming.

Pötzsch, O. (2012). Geburten in Deutschland, Ausgabe 2012. Wiesbaden: DESTATIS - Statistisches Bundesamt.

Raute, A. (2019). Can Financial Incentives Reduce the Baby Gap? Evidence from a Reform in Maternity Leave Benefits. *Journal of Public Economics*, 169: 203–222.

Rindfuss, R. R., Guilkey, D. K., Morgan, S. P., and Kravdal, O. (2010). Child-Care Availability and Fertility in Norway. *Population and Development Review*, 36(4): 725–748.

Rohrbach-Schmidt, D. (2009). The BIBB/IAB- and BIBB/BAuA-Surveys of the Working Population on Qualification and Working Conditions in Germany, Version 1.1, Data and Methodological Reports. *BIBB-FDZ Daten- und Methodenberichte*, No. 1/ 2009.

Schlosser, A. (2011). Public Preschool and the Labor Supply of Arab Mothers: Evidence from a Natural Experiment. *mimeo*.

Schönberg, U. (2009). Does the IAB Employment Sample Reliably Identify Maternity Leave Taking? A Data Report. *Zeitschrift für ArbeitsmarktForschung*, 42(1): 49–70.

Schönberg, U. and Ludsteck, J. (2014). Expansions in Maternity Leave Coverage and Mothers' Labor Market Outcomes after Childbirth. *Journal of Labor Economics*, 32(3): 469–505.

SOEP v28 (2012). [Dataset] Socio-Economic Panel (SOEP), data for years 1984-2011, version 28. doi:10.5684/soep.v28.

Statistik der Bundesagentur für Arbeit (2015). German Classification of Occupations 2010. http://statistik.arbeitsagentur.de/Navigation/Statistik/Grundlagen/ Klassifikation-der-Berufe/KldB2010/KldB2010-Nav.html.

Statistisches Bundesamt (2018). Statistisches Jahrbuch 2018, Kapitel 13 Arbeitsmarkt.

Tiedemann, B. (2014). Vereinbarkeit von Familie und Beruf. Bundeszentrale für politische Bildung. http://www.bpb.de/politik/innenpolitik/arbeitsmarktpolitik/187837/vereinbarkeit-von-familie-und-beruf?p=all.

Waldfogel, J. (1998). Understanding the "Family Gap" in Pay for Women with Children. *The Journal of Economic Perspectives*, 12(1): 137–156.

Weiss, Y. and Gronau, R. (1981). Expected Interruptions in Labour Force Participation and Sex-Related Differences in Earnings Growth. *The Review of Economic Studies*, 48(4): 607–619.

Wolfers, J. (2006). Did Unilateral Divorce Laws Raise Divorce Rates? A Reconciliation and New Results. *American Economic Review*, 96(5): 1802–1820.

World Bank (2019). Fertility Rate, Total (Births per Woman). The World Bank. https://data.worldbank.org/indicator/SP.DYN.TFRT.IN.

Wrohlich, K. (2008). The Excess Demand for Subsidized Child Care in Germany. *Applied Economics*, 40(10): 1217–1228.

A Appendix

Table A1: Descriptive statistics

Variable	N	Mean	SD	Min	Max
Dependent variables - births					
Birth (0/1) in t	833,851	0.0249	0.1557	0	1
Birth (0/1) in t or $t+1$	833,851	0.0613	0.2400	0	1
Births by wage profile in t or $t+1$					
Flat	141,810	0.0398	0.1954	0	1
Medium slope	270,076	0.0589	0.2354	0	1
Steep	408,057	0.0724	0.2592	0	1
Births by substitutability in t or $t+1$					
Rather low	422,253	0.0701	0.2554	0	1
Rather high	395,553	0.0542	0. 2265	0	1
Births by education level in t or $t+1$					
Low education	237,163	0.0318	0.1755	0	1
Medium education	505,447	0.0726	0.2595	0	1
High education	74,727	0.0865	0.2812	0	1
Births by task in t or $t+1$					
Abstract	233,865	0.0631	0.2432	0	1
Routine & manual	586,060	0.0620	0.2411	0	1
Dependent variables - employment					
Employed (0/1) in t	96,963	0.4322	0.4954	0	1
Employment by wage profile in t					
Flat	12,795	0.3561	0.4789	0	1
Medium slope	30,842	0.3787	0.4851	0	1
Steep	53,020	0.4839	0.4997	0	1
Employment by substitutability in t					
Rather low	53,027	0.4414	0.4966	0	1
Rather high	43,527	0.4236	0.4941	0	1
Employment by education level in t					
Low education	14,270	0.2992	0.4579	0	1
Medium education	71,636	0.4468	0.4972	0	1
High education	10,260	0.5103	0.500	0	1
Employment by task in t					
Abstract	27,324	0.4052	0.4909	0	1
Routine & manual	69,331	0.4445	0.4969	0	1

Table A1 continued

Variable	N	Mean	SD	Min	Max
County-level controls					
Child care coverage 1998	325	0.016	0.02	0	0.118
Child care coverage 2009	325	0.142	0.05	0.037	0.359
Child care coverage 1998-2009	1,950	0.077	0.061	0	0.359
Population density	1,950	565.61	690.240	40.720	4286.211
Male employment rate	1,950	0.6041	0.0593	0.4056	0.7368
GDP per capita (in 1000)	1,950	28.040	10.811	11.238	86.079
Conservative vote share	1,950	0.3917	0.0929	0.1945	0.7496
Female high education share	1,950	0.1542	0.0617	0.0280	0.4069
Revenue	1,610	383.213	452.072	56.630	5775.025
Debt	1,932	0.2138	0.2790	0.000	3.335
New dwellings	1,950	0.5828	0.6391	0.008	14.536
Share of women 15-19	1,950	0.1450	0.0198	0.0842	0.1864
Share of women 20-24	1,950	0.1435	0.0198	0.1066	0.2716
Share of women 25-29	1,950	0.1484	0.0188	0.1137	0.2233
Share of women 30-34	1,950	0.1609	0.0248	0.1212	0.2315
Share of women 35-40	1,950	0.2335	0.0189	0.1333	0.2955
Population fraction 41-44	1,950	0.0666	0.0058	0.0492	0.0814
Population fraction 45-49	1,950	0.0777	0.0079	0.0567	0.0954
Population fraction 50-54	1,950	0.0670	0.0074	0.0407	0.0846
Population fraction 55-59	1,950	0.0623	0.0067	0.0379	0.0895
Population fraction 60-64	1,950	0.0559	0.0081	0.0361	0.0814
Population fraction 65-69	1,950	0.0581	0.0079	0.0359	0.0813
Population fraction 70-74	1,950	0.0494	0.0072	0.0308	0.0718
Population fraction 75+	1,950	0.0819	0.0143	0.0401	0.1272
Individual-level controls					
Women's age	833,851	29.2137	7.048	15	40
Low education	817,337	0.2902	0.4538	0	1
Medium education	817,337	0.6184	0.4858	0	1
High education	817,337	0.0914	0.2882	0	1
German	833,780	0.8889	0.3142	0	1
German mothers	96,962	0.9030	0.2959	0	1
N^o children < 18 years	96,963	1.3601	0.6062	1	9
More children ≤ 3 years	93,094	0.1518	0.3588	0	1
More children $4-6$ years	93,094	0.0590	0.2357	0	1

Notes: The figures for births are based on the whole sample of 15-40 years old women living in West Germany between 1998 and 2010. For employment, the sample is restricted to women whose youngest child is between 0 and 3 years old. The figures show aggregated values over all waves used in the regression (1998, 2002, 2006, 2007, 2008, and 2009). Accordingly, variables measured in (t and $t+1$) comprise the years 1998, 1999, 2002, 2003, 2006, 2007, 2008, 2009 and 2010. Education level and the number of older siblings is measured before birth. Occupational grouping is based on pre-birth occupation. Revenue and debt figures are divided by 1,000,000 Euros and the number of new dwellings is divided by 1,000. Number of observations slightly lower when differentiating by education or occupation due to missing information on educational degree or occupation. Data source: SIAB 7514, BIBB/BAuA (2006), dataset provided by Bauernschuster et al. (2016b).

Table A2: Occupations and wage profiles

Occupational segment	Occupational main group (KldB2010)	Age-earnings profile
Occupations in agriculture, forestry, horticulture	11 Occupations in agriculture, forestry, farming	flat
	12 Occupations in horticulture, floristry	flat
Manufacturing occupations	21 Occupations in production, processing of raw materials, glass- & ceramic-making and -processing	medium
	22 Occupations in plastic-making & -processing, wood-working & -processing	medium
	23 Occupations in paper-making & -processing, printing, technical media design	medium
	24 Occupations in metal-making & -working, metal construction	medium
	28 Occupations in textile- & leather-making & -processing	medium
	93 Occupations in product design, artisan craftwork, fine arts, making of musical instruments	flat
Occupations concerned with production technology	25 Technical occupations in machine-building & automotive industry	steep
	26 Occupations in mechatronics, energy electronics, electrical engineering	steep
	27 Occupations in technical research & development, construction, production planning & scheduling	steep
Occupations in building and interior construction	31 Occupations in construction scheduling, architecture & surveying	steep
	32 Occupations in building construction above & below ground	medium
	33 Occupations in interior construction	medium
	34 Occupations in building services engineering, technical building services	medium
Occupations in the food industry, gastronomy & tourism	29 Occupations in food-production & -processing	flat
	63 Occupations in tourism, hotels and restaurants	flat
Medical and non-medical health care occupations	81 Medical and health care occupations	steep
	82 Occupations in non-medical healthcare, body care, wellness, medical technicians	medium
Service occupations in social sector, cultural work	83 Occupations in education, social work, housekeeping, theology	medium
	84 Occupations in teaching and training	medium
	91 Occupations in philology, literature, humanities, social sciences, economics	steep
	94 Occupations in the performing arts & entertainment	medium
Occupations in commerce and trade	61 Occupations in purchasing, sales & trading	steep
	62 Sales occupations in retail trade	medium
Occupations in business management and organisation	71 Occupations in business management & organisation	steep
Business related service occupations	72 Occupations in financial services, accounting & tax consultancy	steep
	73 Occupations in law and public administration	steep
	92 Occupations in advertising & marketing, in commercial & editorial media design	steep
Service occupations in IT-sector and natural sciences	41 Occupations in mathematics, biology, chemistry, physics	steep
	42 Occupations in geology, geography, environmental protection	medium
	43 Occupations in computer science, information, communication technology	steep
Safety and security occupations	53 Occupations in safety & health protection, security & surveillance	flat
	1 Armed forces personnel	steep
Occupations in traffic and logistics	51 Occupations in traffic & logistics (without vehicle driving)	medium
	52 Drivers and operators of vehicles & transport equipment	flat
Occupations in cleaning services	54 Occupations in cleaning services	flat

Notes: German Classification of Occupations 2010, table slightly adapted from Statistik der Bundesagentur für Arbeit (2015), last column added. Information on wage profiles from SIAB 7514.

Table A3: Average effects on fertility - t and $t+1$ separately

Dep. Var.: Birth in	t	t+1	t	t+1	t	t+1
	(1)	(2)	(3)	(4)	(5)	(6)
Child care coverage	0.0198	0.0284*	0.0294*	0.0288*	0.0405**	0.0290
	(0.0133)	(0.0141)	(0.0160)	(0.0169)	(0.0189)	(0.0200)
Year & county FE	√	√	√	√	√	√
Regional controls			√	√	√	√
Individual controls			√	√	√	√
Revenue, debt, dwellings					√	√
N	909,709	833,851	886,964	817,337	710,857	653,367
Number of counties	325	325	325	325	322	322

Notes: Results of the generalized Diff-in-Diff, equation 1. Regional controls include controls for age-specific population shares of females between 15 and 40, age-specific population shares of the whole population older than 41, population density, the male employment rate, GDP per capita, the conservative vote share and the share of highly educated women in the fertile age. Individual controls include the pre-birth education level and age. Revenue and debt information is not available or incomplete for Hamburg, Bremen and Schleswig-Holstein, revenue information from 2009 is too fragmentary to be included. Sample restrictions: women aged 15-40, West Germany, 1998-2009. Standard errors in parentheses are clustered at the individual (mother) level. Regression is weighted with the number of observations per county. * significant at 10% level, ** at 5 % level, *** at 1% level. Data source: SIAB 7514, dataset provided by Bauernschuster et al. (2016b).

Table A4: Pre-treatment means and differences, dependent variables

	Mean			Mean-Diff.	
	All	Control	Treatment	(T-C)	p-value
Dependent variables - births					
Birth(0/1) in t	0.022	0.022	0.022	0.000	0.758
Birth(0/1) in t or $t+1$	0.075	0.075	0.073	-0.002	0.084
Births by wage profile in t or $t+1$					
Flat	0.040	0.038	0.041	0.003	0.210
Medium slope	0.058	0.058	0.058	0.000	0.910
Steep	0.065	0.068	0.063	-0.005	0.022
Births by substitutability in t or $t+1$					
Rather low	0.083	0.085	0.082	-0.003	0.199
Rather high	0.067	0.068	0.065	-0.003	0.150
Births by education level in t or $t+1$					
Low education	0.047	0.048	0.046	-0.002	0.293
Medium education	0.086	0.088	0.084	-0.004	0.047
High education	0.086	0.085	0.086	0.001	0.973
Births by task in t or $t+1$					
Abstract	0.077	0.078	0.075	-0.003	0.380
Routine-manual	0.075	0.076	0.073	-0.003	0.107
Dependent variables - employment					
Employed (0/1) in t	0.456	0.453	0.460	0.007	0.390
Employment by wage profile in t					
Flat	0.426	0.415	0.437	0.022	0.293
Medium slope	0.410	0.416	0.402	-0.014	0.303
Steep	0.392	0.487	0.497	0.010	0.342
Employment by substitutability in t					
Rather low	0.464	0.464	0.465	0.001	0.919
Rather high	0.450	0.443	0.457	0.014	0.218
Employment by education level in t					
Low education	0.346	0.333	0.359	0.026	0.165
Medium education	0.473	0.473	0.473	0.000	0.977
High education	0.503	0.501	0.505	0.004	0.878
Employment by task in t					
Abstract	0.422	0.422	0.421	-0.001	0.983
Routine-manual	0.470	0.466	0.475	0.009	0.285

Notes: The figures show means for all counties, the control (C) and treatment group (T), differences in means between T and C, and the p-value for the T-test of equality in means of T and C. Pre-treatment period, 2002. Treatment regions are defined as those whose increase in public child care coverage was above the median, those with an increase below the median belong to the control group. Median is identified by sorting all West German counties by the absolute size of the increase in public child care coverage from 2002 to 2009. The figures for births are based on the whole sample for 15-40 year old women living in West Germany in 2002. For employment, the sample is restricted to women whose youngest child is between 0 and 3 years old. Data source: SIAB 7514, BIBB/BAuA (2006), dataset provided by Bauernschuster et al. (2016b).

Table A5: Pre-treatment differences TG and CG, independent variables

	Mean		Mean-Diff.	
	Control	Treatment	(T-C)	p-value
Child care				
Child care coverage	0.019	0.024	0.005	0.067
County-level control variables				
Population density	590.764	541.310	-49.454	0.518
Male employment rate	0.604	0.599	-0.005	0.380
GDP per capita (in 1000)	25.465	26.867	1.402	0.205
Conservative vote share	0.438	0.448	0.010	0.484
Female high education share	0.130	0.165	0.035	0.000
Revenue	357.555	359.393	1.838	0.968
Debt	0.219	0.215	-0.005	0.881
New dwellings	0.668	0.635	-0.033	0.548
Share of women 15-19	0.138	0.132	-0.006	0.000
Share of women 20-24	0.139	0.140	0.001	0.484
Share of women 25-29	0.136	0.136	0.000	0.687
Share of women 30-34	0.179	0.179	0.000	0.919
Share of women 35-40	0.251	0.254	0.003	0.013
Population fraction 41-44	0.064	0.065	0.001	0.000
Population fraction 45-49	0.070	0.071	0.001	0.005
Population fraction 50-54	0.064	0.065	0.001	0.036
Population fraction 55-60	0.053	0.052	0.000	0.867
Population fraction 60-64	0.067	0.066	0.000	0.578
Population fraction 65-69	0.056	0.054	-0.001	0.054
Population fraction 70-74	0.044	0.043	-0.001	0.026
Population fraction 75+	0.073	0.069	-0.003	0.018
Individual-level controls				
Woman's age	29.493	29.536	0.043	0.221
Low education	0.285	0.272	-0.013	0.000
Medium education	0.648	0.628	0.020	0.000
High education	0.067	0.099	0.032	0.000
German	0.913	0.889	-0.023	0.000
German mother	0.921	0.909	0.012	0.006
N^o children < 18	1.367	1.363	0.004	0.664
More children ≤ 3	0.149	0.148	-0.001	0.799
More children $4-6$	0.067	0.068	0.001	0.722

Notes: The figures show means for the control (C) and treatment group (T), differences in means and the p-value for the T-test of equality in means of T and C. Pre-treatment period, 2002.Treatment regions are defined as those whose increase in public child care coverage was above the median, those with an increase below the median belong to the control group. Median is identified by sorting all West German counties by the absolute size of the increase in public child care coverage from 2002 to 2009. Revenue and debt figures are divided by 1,000,000 Euros and the number of new dwellings is divided by 1,000. Data source: SIAB 7514, dataset provided by Bauernschuster et al. (2016b).

Table A6: Effects on fertility - further heterogeneities

Dep. Var.: Birth in t or $t+1$	(1)	(2)	(3)
Panel A: Heterogeneity by education (omitted: high education)			
Child care coverage	0.1307***	0.1456***	0.1550***
	(0.0330)	(0.0356)	(0.0420)
Child care coverage × medium education	-0.0681**	-0.0629**	-0.0547
	(0.0281)	(0.0284)	(0.0342)
Child care coverage × low education	-0.1558***	-0.1557***	-0.1595***
	(0.0281)	(0.0282)	(0.0340)
N	828,668	828,668	662,909
Panel B: Heterogeneity by task (omitted: abstract tasks)			
Child care coverage	0.0500**	0.0711**	0.0820**
	(0.0244)	(0.0277)	(0.0325)
Child care coverage × routine-manual	0.0034	-0.0082	-0.0037
	(0.0160)	(0.0161)	(0.0195)
N	831,354	816,586	654,051
Year & county FE	√	√	√
Regional controls		√	√
Individual controls		√	√
Revenue, debt, dwellings			√
Number of counties	325	325	322

Notes: Results of the generalized Diff-in-Diff, equation 2. Regional controls include controls for age-specific population shares of females between 15 and 40, age-specific population shares of the whole population older than 41, population density, the male employment rate, GDP per capita, the conservative vote share and the share of highly educated women in the fertile age. Individual controls include the pre-birth education level and age. Revenue and debt information is not available or incomplete for Hamburg, Bremen and Schleswig-Holstein, revenue information from 2009 is too fragmentary to be included. Occupational and educational grouping based on pre-birth information. All regressions include the interactions and all main effects. Sample restrictions: women aged 15-40, West Germany, 1998-2009. Standard errors in parentheses are clustered at the individual (mother) level. Regression is weighted with the number of observations per county. * significant at 10% level, ** at 5 % level, *** at 1% level. Data source: SIAB 7514, dataset provided by Bauernschuster et al. (2016b).

Table A7: Effects on maternal employment - further heterogeneities

Dep. Var.: Employment in t	(1)	(2)	(3)
Panel A: Heterogeneity by education (omitted: high education)			
Child care coverage	0.2983*	0.2705*	0.3085*
	(0.1640)	(0.1632)	(0.1678)
Child care coverage × medium education	-0.0020	-0.0041	-0.0048
	(0.1212)	(0.1203)	(0.1221)
Child care coverage × low education	-0.0929	-0.0207	-0.0855
	(0.1495)	(0.1490)	(0.1519)
N	96,166	96,165	92,354
Panel B: Heterogeneity by task (omitted: abstract tasks)			
Child care coverage	0.3548**	0.3444**	0.3866***
	(0.1432)	(0.1418)	(0.1465)
Child care coverage × routine-manual	-0.0739	-0.0980	-0.1190
	(0.0822)	(0.0811)	(0.0828)
N	96,655	95,883	92,149
Year & county FE	√	√	√
Regional controls	√	√	√
Individual controls		√	√
Sibling controls			√
Number of counties	325	325	325

Notes: Results of the generalized Diff-in-Diff, equation 2. Regional controls include controls for age-specific population shares of females between 15 and 40, age-specific population shares of the whole population older than 41, population density, the male employment rate, GDP per capita, the conservative vote share and the share of highly educated women in the fertile age. Individual controls include the pre-birth education level, age, age^2 and a dummy for being German. Sibling controls comprise the number of further children younger than 18 and two dummies indicating whether the newborn has a sibling aged 0-3 years or aged between 4-6 years. Occupational and educational grouping based on pre-birth information. All regressions include the interactions and all main effects. Sample restrictions: women aged 15-40, West Germany, 1998-2009, youngest child 0-3 years old. Standard errors in parentheses are clustered at the individual (mother) level. Regression is weighted with the number of observations per county. * significant at 10% level, ** at 5 % level, *** at 1% level. Data source: SIAB 7514, dataset provided by Bauernschuster et al. (2016b).

Table A8: Effects on fertility - alternative definition substitutability

Dep. Var.: Birth in t or $t+1$	(1)	(2)	(3)
Heterogeneity by substitutability (omitted: low substitutability)			
Child care coverage	0.0689***	0.0900***	0.1123***
	(0.0225)	(0.0262)	(0.0307)
Child care coverage × high substitutability	-0.1031***	-0.1233***	-0.1517***
	(0.0167)	(0.0170)	(0.0207)
N	797,218	783,532	627,856
Year & county FE	√	√	√
Regional controls		√	√
Individual controls		√	√
Revenue, debt, dwellings			√
Number of counties	325	325	322

Notes: Results of the generalized Diff-in-Diff, equation 2. Occupations that cannot be clearly assigned to low or high degree of substitutability are excluded. Regional controls include controls for age-specific population shares of females between 15 and 40, age-specific population shares of the whole population older than 41, population density, the male employment rate, GDP per capita, the conservative vote share and the share of highly educated women in the fertile age. Individual controls include the pre-birth education level and age. Revenue and debt information is not available or incomplete for Hamburg, Bremen and Schleswig-Holstein, revenue information from 2009 is too fragmentary to be included. Occupational and educational grouping based on pre-birth information. Regression includes the interactions and all main effects. Sample restrictions: women aged 15-40, West Germany, 1998-2009. Standard errors in parentheses are clustered at the individual (mother) level. Regression is weighted with the number of observations per county. * significant at 10% level, ** at 5 % level, *** at 1% level. Data source: SIAB 7514, dataset provided by Bauernschuster et al. (2016b).

Table A9: Effects on maternal employment - alternative definition substitutability

Dep. Var.: Employment in t	(1)	(2)	(3)
Heterogeneity by substitutability (omitted: low substitutability)			
Child care coverage	0.2954**	0.2695**	0.3054**
	(0.1323)	(0.1311)	(0.1357)
Child care coverage × high substitutability	-0.2102**	-0.2589***	-0.2912***
	(0.1014)	(0.0998)	(0.1031)
N	93,358	92,617	89,008
Year & county FE	√	√	√
Regional controls	√	√	√
Individual controls		√	√
Sibling controls			√
Number of counties	325	325	325

Notes: Results of the generalized Diff-in-Diff, equation 2. Occupations that cannot be clearly assigned to low or high degree of substitutability are excluded. Regional controls include controls for age-specific population shares of females between 15 and 40, age-specific population shares of the whole population older than 41, population density, the male employment rate, GDP per capita, the conservative vote share and the share of highly educated women in the fertile age. Individual controls include the pre-birth education level, age, age^2 and a dummy for being German. Sibling controls comprise the number of further children younger than 18 and two dummies indicating whether the newborn has a sibling aged 0-3 years or aged between 4-6 years. Occupational and educational grouping based on pre-birth information. Regression includes the interactions and all main effects. Sample restrictions: women aged 15-40, West Germany, 1998-2009, youngest child 0-3 years old. Standard errors in parentheses are clustered at the individual (mother) level. Regression is weighted with the number of observations per county. * significant at 10% level, ** at 5 % level, *** at 1% level. Data source: SIAB 7514, dataset provided by Bauernschuster et al. (2016b).