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Franziska Zimmert (IAB)

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Abstract

This paper examines the effect of an expansion of subsidized early child care on maternal labor market outcomes. It contributes to the literature by analyzing, apart from the employment rate and agreed working hours, preferred working hours. Using the legal claim for subsidized child care introduced in Germany in August 2013 for children aged one to three years, I apply a semi-parametric difference-in-differences estimator to examine maternal labor market outcomes. Findings based on survey data from the German Micro Census show a positive effect on the employment rate, as well as on agreed and preferred working hours in districts where the child care coverage rate increases intensely in contrast to districts with a lower expansion of subsidized child care. As agreed and preferred working hours adjust in line with each other, expansion of early child care can tap labour market potentials beyond those of currently underemployed mothers.

Zusammenfassung

Das vorliegende Papier untersucht nicht nur den Effekt der Verfügbarkeit von öffentlicher Kinderbetreuung auf die mütterliche Erwerbsquote und den vereinbarten Stundenumfang, sondern auch auf die gewünschte Stundenzahl. Dabei wird der im August 2013 eingeführte Rechtsanspruch auf einen Betreuungsplatz für Kinder ab dem ersten Lebensjahr genutzt, um einen semi-parametrischen Differenzen-von-Differenzen-Ansatz anzuwenden. Die Ergebnisse auf Basis des Mikrozensus deuten auf einen positiven Effekt auf die Erwerbsbeteiligung und auf vereinbarte und gewünschte Arbeitsstunden in Landkreisen, in denen die Kinderbetreuungsquote intensiv anstieg, im Vergleich zu Landkreisen mit einem geringeren Anstieg dieser Quote, hin. Da sich gewünschte und vereinbarte Arbeitszeit gleichermaßen erhöhen, hat die Ausweitung von frühkindlicher Betreuung Arbeitsmarktpotentiale über die Gruppe der unterbeschäftigen Mütter hinaus.

JEL classification: J21, J22

Keywords: maternal labor supply, working hour preferences, subsidized child care, early child care, semi-parametric difference-in-differences

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1 Introduction

Employment rates and working hours in industrialized countries vary strongly across gender for which the family background is often considered to be a main driving force (OECD, 2017). While male careers are less life-course dependent, women more often withdraw from the labor market or reduce their working hours after giving birth to a child. Hence, policymakers advocate an expansion of publicly subsidized child care in order to strengthen the employment potential in aging societies. Indeed, the female employment rate turns out to be higher in countries such as the Scandinavian states where child care is sufficiently provided. However, empirical studies cannot unanimously support a positive causal relationship between subsidized child care and female employment outcomes.
I further inform these debates by evaluating the effect of low-cost subsidized child care on the employment share and agreed weekly working hours. The article especially contributes to the existing literature by also analyzing working hour preferences and the mismatch between agreed and preferred working hours. Working hour discrepancies are quite common in industrialized countries (Fagan, 2001; Merz, 2002; Reynolds, 2003, 2004; Drago/Tseng/Wooden, 2005; Pollmann-Schult, 2009; Ehing, 2014; Weber/Zimmert, 2017). Hence, evaluating if the availability of subsidized child care can affect working hour discrepancies is important in ageing societies as fulfilling a preference for more or less hours has positive effects on the employment potential and on individual life, health or work measures (Grözinger/Matiaske/Tobsch, 2008; Ehing, 2014; Matiaske et al., 2017). Furthermore, the focus is on early child care (children less than three years old) where there is less empirical evidence on compared to pre-school child care. I use a rich data set from the German Micro Census which is a one percent representative sample of German households. The repeated cross-sections contain information on the household composition and its economic and social background and the data allows to examine over- and underemployment as well as individual working hour preferences. Instead of applying a linear OLS estimator, a two-stage semi-parametric difference-in-differences estimation procedure proposed by Abadie (2005) is used such that the linear form assumption in the second stage does not need to hold and common support between treated and control group is given.

There is a growing literature on evaluating the effectiveness of subsidized child care not only on parental, mainly maternal, outcomes (e.g. Gelbach, 2002; Cascio, 2009; Havnes/Mogstad,2011; Bauernschuster/Schlotter, 2015) but also on the child's development (e.g. Duflo, 2001; Felfe/Nollenberger/Rodríguez-Planas, 2015; Felfe/Lalive, 2018) and fertility (Bauernschuster/Hener/Rainer, 2016). Many of these empirical studies rely on identification strategies that exploit exogenous variation resulting from quasi-experiments. In line with those authors I use the expansion of subsidized child care in Germany induced by the introduction of a legal claim for a child care slot to examine maternal employment. The claim was introduced for children aged one to three years old in August 2013.
The German labor market serves as an interesting example for the persistence of traditional employment patterns. Although the female employment rate converges to the male employment rate, there is strong variation when further conditioning on motherhood for both the extensive and the intensive margin. In 2011, almost one half of the childless couples both worked full-time while this was only the case for 22 percent of the couples with children (Wanger, 2015). In almost 20 percent of families, the mother is not employed

and the father works full-time (14 percent for childless couples). The majority of parents is characterized by a full-time working father while the mother holds a part-time position. About one quarter of part-time working women states the care for children or for people in need of care to be the reason for the employment status. Hence, the reform implemented in 2013 had a high potential to increase female employment both in terms of the extensive and intensive margin. Especially involuntarily underemployed mothers might have raised agreed hours.

In 2008, the German government formulated a law for the expansion of subsidized child care for children aged one to three (*Kinderförderungsgesetz, KiföG*) culminating in a legal claim for a child care slot from August 2013 onwards. While the supply of child care is organized on the community level, the federation was involved by one third of the costs (four billion Euros). The allocation of child care on the community level results in strong regional variation that is strengthened by huge disparities between West and East German federal states. In the former German Democratic Republic the education of children was considered to be a public issue translating in a high share of children institutionally cared for until today. In 2011, the coverage rate of children aged up to three years old in subsidized care amounted to 49 percent in East Germany compared to only 20 percent in the rest of the country (Federal Statistical Office, 2011a). The reform changed the availability of child care slots dramatically. In 2015, 28.2 percent of children living in West-Germany and 51.9 percent in East Germany were in subsidized care (Federal Statistical Office, 2015a). I use the exogenous variation of the expansion of subsidized child care induced by the reform to compare districts in which the coverage rate increased significantly (the treated or high-intensity group) with those for which the coverage rate changed only by a small amount (the control or low-intensity group). To be more concrete, I follow the approach of Havnes/Mogstad (2011), Felfe/Nollenberger/Rodríguez-Planas (2015) and Bauernschuster/Hener/Rainer (2016) who exploit spatial variation of Norwegian districts, Spanish states and German districts respectively for which the child care coverage expanded differently after the legal framework had changed. The authors define control and treatment group by dividing the observational units at the median of the percentage point change in the coverage rate. Thus, the difference-in-differences strategy compares labor market outcomes of mothers with children aged up to three years in treated districts with those where child care increases to a lesser extent before and after the legal claim came into force.

The examined reform mainly focussed on the availability instead of the affordability of child care (Kreyenfeld/Hank, 2000) which in turn can be considered as an implicit subsidy (Berlinski/Galiani, 2007). The legal claim introduced in August 2013 guaranteed parents at least part-time care (four hours per day). Neoclassical economic and sociological theory predict an increase for female labor force participation whenever child care costs decrease. However, the effect on the intensive margin remains ambiguous. It depends on the supplied hours before the reform came into force and the length of the child care slot such that it represents a weighted average of the substitution and income effect (Gelbach, 2002). Moreover, the overall effect is determined by the degree to which public care crowds out other care arrangements. Previous studies show that the impact on the extensive margin is negligible if women substitute informal or private with institutional, subsidized arrangements (Havnes/Mogstad, 2011) or the female labor market participation is already high

(Lundin/Mörk/Öckert, 2008).
Furthermore, sociological theory predicts that family policies encouraging female employment shape social norms (Gangl/Ziefle, 2015; Zoch/Hondralis, 2017). Hence, working mothers feel more accepted if they use institutional care resulting in an increase of female employment. The availability of subsidized child care can have a different effect on preferred and actual working hours. Neoclassical theory assumes perfect labor markets on which the absence of frictions equalizes working hour preferences and actual hours. However, a mismatch can occur whenever social or occupational constraints prevent employees from supplying the preferred hours (Fagan, 2001; Merz, 2002; Reynolds, 2003, 2004; Drago/Tseng/Wooden, 2005; Pollmann-Schult, 2009; Ehing, 2014; Weber/Zimmert, 2017). The availability of child care has the potential to decrease this discrepancy while the adjustment of preferred and/or agreed hours depends on the state of being under-, overemployed or unconstrained before the reform came into effect. Underemployed women are expected to adjust their agreed hours to their preferred amount as the availability of subsidized child care lowers time and monetary constraints. Furthermore, institutional child care can attenuate interrole conflicts between family and occupational requirements (Greenhaus/Beutell, 1985). Hence, overemployed mothers are expected to be more likely to adjust a working hour mismatch by an increase in preferred hours. Finally, if unconstrained women adjust their agreed hours due to the availability of subsidized child care, the change should go in line with an adjustment of their preferences. Also for theoretical considerations deviating from the neoclassical approach the total effect on preferred and agreed working hours remains ambiguous.

The resulting intention-to-treat estimates give a positive impact both on the extensive and intensive margin. However, the latter is less precisely measured and the share of full-time working mothers does not significantly increase. Mothers of up to three-year-olds in districts with a large increase of the child care coverage rate have a ten percentage points higher employment rate after the reform than their counterparts in districts with a lower expansion of subsidized child care. Agreed and preferred working hours are on average about four hours per week higher and change similarly such that their mismatch is not affected. The results are robust to several sensitivity checks. Especially the common trend for treated and control group in the absence of the reform seems to hold. I furthermore show that the estimates differ for subgroups considering education and income.

The paper proceeds as follows: The next section gives an overview on previous empirical studies. Section 3 explains the institutional background of the German child care system including its reform and how it is exploited for the estimation strategy. Furthermore, the data is presented. The estimation results can be found in Section 4. The last section concludes.

2 Related empirical findings

Estimating the causal effect of publicly financed child care on employment outcomes suffers from several difficulties. One is that the price and availability of informal child care provided by the family are often insufficiently observed (Havnes/Mogstad, 2011). Another problem is the endogeneity of child care availability and costs to employment measures. Hence, most studies apply quasi-experimental designs that benefit from exogenous variation induced by a policy reform or instrumental variable (for a review see Morrissey, 2017). However, the empirical results strongly differ between countries depending on the economic conditions before the reform was implemented, the population under consideration and the organization of child care including private, public and informal arrangements. The bandwidth of the effect of more generous child care varies from positive (Gelbach, 2002; Schlosser, 2005; Berlinski/Galiani, 2007; Baker/Gruber/Milligan, 2008; Lefebvre/Merrigan, 2008; Berlinski/Galiani/Mc Ewan, 2011; Nollenberger/Rodríguez-Planas, 2011; Fitzpatrick, 2012; Bauernschuster/Schlotter, 2015; Geyer/Haan/Wrohlich, 2015; Fendel/Jochimsen, 2017) to negligibly small (Havnes/Mogstad, 2011) and also insignificant coefficients (Lundin/Mörk/Öckert, 2008; Cascio, 2009).

Gelbach (2002) uses an instrumental variable approach to estimate the effect of public school enrollment by exploiting quarter of birth regulations for the US. He estimates a positive effect on the probability for working and weekly hours for single mothers while the coefficient is slightly smaller for married women. Fitzpatrick (2012) finds only a positive effect for single mothers in the US with a regression discontinuity (RD) design that is as well characterized by a child's eligibility to kindergarten. Berlinski/Galiani/Mc Ewan (2011) apply a RD design for Argentina where kindergarten enrollment is defined by a cut-off date. Women whose youngest child attends kindergarten have a higher employment probability, also in full-time, and weekly hours rise on average by 7.8.

The majority of empirical studies uses quasi-experiments for a difference-in-differences (DD) design. Schlosser (2005) evaluates a reform that affected Arab mothers of children aged three to four in Israel. She finds that free public preschool increased maternal employment by 8.1 percentage points and average weekly hours by 2.8. Berlinski/Galiani (2007) estimate positive employment effects for Argentinean mothers of children aged three to five. The authors exploit a preschool construction program taking place in the mid 1990s. The staggered introduction of subsidized child care in the Canadian province Quebec was found to increase female employment by 7.7 percentage points (Baker/Gruber/Milligan, 2008) which is in line with Lefebvre/Merrigan (2008) who evaluate the same reform and also find a positive effect on working hours. Positive effects can also be found for Spain (Nollenberger/Rodríguez-Planas, 2011) and Germany (Bauernschuster/Schlotter, 2015; Geyer/Haan/Wrohlich, 2015; Fendel/Jochimsen, 2017). Bauernschuster/Schlotter (2015) show that the transition to kindergarten defined by cut-off rules is related to an increase in labor force participation by 36.6 percentage points and in average weekly hours by 14.3, i.e. by 23.2 percent. Fendel/Jochimsen (2017) find positive short-term effects on the maternal labor force participation for the child care reform of August 2013 including the legal claim for child care and the introduction of home care allowances. With a microsimulation study Geyer/Haan/Wrohlich (2015) demonstrate that universal child care has large, positive effects for children older than one year. In contrast, Lundin/Mörk/Öckert (2008) and Havnes/Mogstad (2011) find estimates for maternal employment in Sweden and Norway that are close to zero. The latter suggest that public child care mainly crowded out informal arrangements.

All these studies evaluate the effect on maternal employment or agreed/actual working hours while the impact on working hour preferences is neglected. Several authors emphasize the role of adjusting preferences in case of occuring life events like the birth of a child (Drago/Tseng/Wooden, 2005; Reynolds/Johnson, 2012; Campbell/van Wanrooy, 2013). Reynolds/Johnson (2012) evaluate how the number of children living in the household affects preferred and actual working hours for the US and finds that the birth of the first child is related to a larger drop of female working hour preferences compared to actual working hours. The impact on male working hours does not statistically significantly differ from zero. This finding is in line with Drago/Tseng/Wooden (2005) who evaluate working hour preferences for Australian employees and conclude that women are more sensitive to changing life conditions than men. Weber/Zimmert (2017) examine the mismatch dynamics considering household and job characteristics. They find that although the lack of institutional care arrangements does not foster the creation of working hour discrepancies, it impedes the mismatch resolution. However, those studies do not examine the direct effect of subsidizing child care on maternal working hours or neglect the adjustment mechanism (agreed versus preferred working hours). Hence, in order to further inform the debate on the effectiveness of subsidized child care, the analysis estimates the effect not only on agreed but also on preferred hours as well as on their mismatch.

3 Institutional background, estimation strategy, data and descriptive findings

3.1 Institutional background and estimation strategy

Institutional background The German system of child care has several particularities ranging from strong regional variation to the different providers of child care (Kreyenfeld/Hank, 2000). Spatial differences are not only defined between urban and rural areas, but also between the former GDR and the West German states. Still in 2016, child care coverage amounts to 51.8 percent in East Germany in comparison with 28.1 percent in West Germany (Federal Statistical Office, 2016). Child care is usually provided by the communities of which there are more than 11,000 resulting in huge differences not only considering the price but also the availability of child care. A private market is not well-developed as quality regulations and hence market entry are related to high costs. The share of private institutions with a pure profit background amounts to about 11 to 13 percent over the last years (compare Table 1). However, there is a variety of non-profit organizations, often with a religious background, that receive public subsidies. About two thirds of all institutions belong to this category.

The expansion of early child care The provision of child care has long time oriented on the existing supply of child care slots and not on the actual needs (Kreyenfeld/Hank, 2000). The expansion of early child care started in 2005 when the German government decided on supplying 230,000 additional child care slots by 2010 (*Tagesbetreuungsausbaugesetz*). Two years later the objective was reinforced by targeting a coverage rate of 35% by 2013 (*Krippengipfel*). In 2008, the government decided on a legal claim for a child care slot for

Table 1: Child care institutions by providers in Germany

	Total	*of which* Profit organization	%	Non-profit organization	%
2010	1,386	164	11.83	1,013	73.09
2011	1,486	184	12.38	1,061	71.40
2012	1,631	181	11.10	1,185	72.65
2013	1,725	185	10.72	1,219	70.67
2014	1,962	230	11.72	1,289	65.70
2015	2,029	261	12.86	1,348	66.44

The numbers relate to children up to three years old. Cut-off date: March, 1st.

Remaining institutions have a public background.

Source: Federal Statistical Office (2010b, 2011b, 2012b, 2013b, 2014b, 2015b).

children aged one to three years from August 2013 onwards[1]. Although the legal claim was announced five years before it came into force and the federal government provided four billion Euro, a shortage of 80,000 to 100,000 slots was predicted in July 2013 for the next month which suggests an almost full take up ratio. Table 2 shows the take up ratio for several federal states for which official statistics are available. By March 1st, 2013, take-up ratios are close to unity in most states. After the introduction of the legal claim in 2014, the ratio gets less tight indicating that the scarcity of child care slots is less severe. Note however, that regional variation on the community level is still high and that in many agglomerated areas child care slots continue being undersupplied. One might furthermore question the exogeneity of the reform as policymakers might have pushed the expansion of child care in districts according to maternal labor supply adjustment. However, I argue that the possibility to sue communities for not providing a child care slot supports the exogenous nature of the reform imposing pressure on the communities to fulfill the demand of child care.

Home care allowances The reforms of August 2013 included also the introduction of home care allowances that were available for children between 15 and 36 months old born after August 2012 and who are not using subsidized child care. Younger children were also eligible if parental leave benefits had exhausted. The subsidy amounted to 100 Euro (150 Euro from August 2014) onwards irrespective of the parents' employment status or income. Opponents of the allowances feared that they would reinforce traditional employment patterns among couples. In July 2015, the home care allowances were declared unconstitutional while they normally expired for children already receiving the subsidy. Although the receipt of theses allowances is connected to not using subsidized child care, eligibility criteria for the allowances and subisdized care are not opposed to each other. Hence, children could be eligible for child care but not for the allowances. Furthermore, there is also a small amount of families neither requiring subsidies in form of the allowances nor in form of child care (Alt et al., 2015).

[1] The KiFöG came in force in December 2008. Five years later, from August 2013 onwards, the legal claim guaranteed child care provided by a facility or childminder for children aged one to three (§24 SGB VIII). Children younger than one year are also eligible if their parents are employed.

Table 2: Take up ratio of child care

	Institution for children aged ... years	2013	2014
Baden-Württemberg	0-3	0.942	0.879
Bavaria	0-3	0.977	0.872
Hamburg	all age groups	0.849	0.802
Hesse	0-3	0.939	0.840
Mecklenburg-Vorpommern	0-3	0.968	0.983
Lower Saxony	0-3	0.895	0.864
North Rhine-Westphalia	0-3	0.946	0.876
Saarland	0-3	0.930	0.882
Saxony-Anhalt	all age groups	0.881	0.880

The take up rate is defined as actual take up divided by authorized slots. Cut-off date: March, 1st.

Source: Statistical reports of Statistical Offices of the Federal States. Own calculations.

The estimation strategy does not allow to disentangle the reform effect into the impact of the legal claim and the home care allowances. However, the applied difference-in-differences estimator allows to solely measure the increase in the availability of child care if the treated group who is strongly affected by the expansion of subsidized child care reacts in the same way to the introduction of home care allowances like the control group. In general, the resulting estimates will at least give a lower bound for the expansion of subsidized child care as the home care allowances counteract. Details of the approach are discussed in the next chapter.

Methodological approach The child care reform of 2013 serves as a quasi-experiment I exploit for difference-in-differences estimation. Besides the temporal variation, the expansion of subsidized child care has a spatial dimension that is used to define the treatment and control group. Following the approach of Havnes/Mogstad (2011), Felfe/Nollenberger/ Rodríguez-Planas (2015) and Bauernschuster/Hener/Rainer (2016), districts are split at the fourth and sixth percentile of the increase in the child care coverage rate for children aged up to three years old. Hence, treatment definition includes not a change in extensive terms, thus from having no to having child care, but a change with respect to the intensive margin, the coverage rate. Furthermore, the resulting effect is an intention-to-treat effect as treatment definition does not inform about actual take-up of a child care slot. As from 2005 onwards the Microcensus does not provide information on attendance of a child care institution, calculation of Wald estimates is not possible. It would give the differences in the expected outcome divided by the differences in the expected take-up of a child care slot. However, the resulting estimates clearly state the sign of the reform's impact.

As the reform took place in August 2013, the pre-reform period is measured in 2011 to rule out any anticipation effects. From 2015 onwards the increase of the child care coverage rate is significantly smaller. Hence, I set this year as the post-reform period. The observational unit is the mother. The treatment group comprises mothers whose youngest child is up to three years old and who live in a district in which the coverage rate increased by more than the sixth percentile (8.0 percentage points) between 2011 and 2015. Mothers of children up to three years old living in districts with a lower increase of the coverage rate than the fourth percentile (6.5 percentage points) within these years belong to the control group. Districts within this interval and those undergoing a territorial reform within the considered time span are dropped from the sample resulting in a sample size of 318 districts[2]. The regional differences can be seen in Figure 1 and Table 3 which depict descriptive

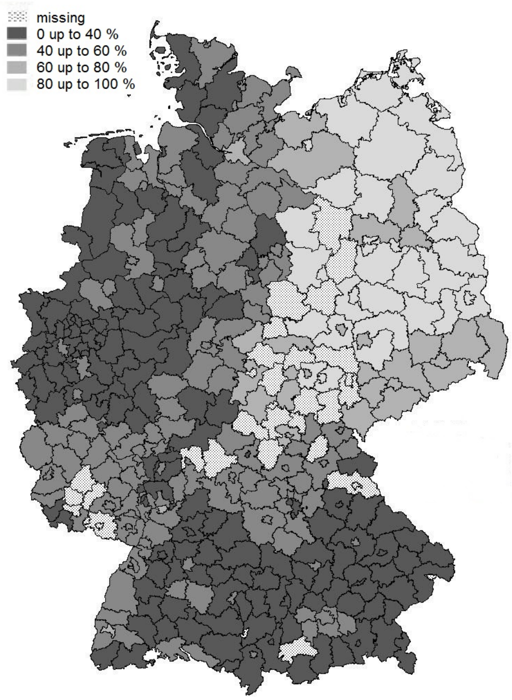

Figure 1: Share of 1- and 2-year-olds in subsidized care in 2015. Source: Alt et al. (2015)

statistics of the child care coverage rates on the district level. Figure 1 shows that child care coverage rates are the highest in East Germany while the lowest can be found in the Southern and West-Northern states. Table 3 confirms these findings for the included 318 districts. In 2011, the lowest share of children in subsidized care measured on the district level amounted to 9.2 percent while the maximum was at 61.0 percent. The maximal value remained stable until 2015, but the minimum almost increased by one half within 4 years. Furthermore, the mean increased from 2011 onwards by 7.2 percentage points to 32.8 percent. Table 4 indicates how the treated districts are spread over the federal states. The majority of Northern and Western districts belong to the treated group for which the coverage rate increased by more than 8.0 percentage points. In Southern states the distinction is less obvious while most districts in East Germany belong to the control group for whom the coverage rate increased to a lesser extent. One may be concerned that most districts of the former GDR belong to the control group. However, a robustness check that drops East German districts will provide similar results compared to the baseline estimates.

The idea of the difference-in-differences estimator is to compare average outcomes of a group affected by a reform with unaffected individuals before and after the treatment

[2] Figure 2 in the Appendix depicts the distribution of the growth of the child care coverage rate between 2011 and 2015. The identification of treatment and control group would be questionable in case of intense concentration around the separation. I find that the distribution is similar to the normal distribution and conclude that the identification strategy does not impose major problems.

Table 3: Coverage rates for children aged up to three years in %, district level

	Minimum	Maximum	Mean
2011	9.2	61.0	25.6
2012	10.5	63.3	28.0
2013	11.3	63.2	29.7
2014	13.9	63.0	32.5
2015	13.0	63.1	32.8

Unweighted. Own calculations based on 318 districts.

Source: Federal Statistical Office (2011a, 2012a, 2013a, 2014a, 2015a).

Table 4: Number of districts by group membership and federal states

Federal state	Control group	Treatment group
Schleswig-Holstein	0	13
Hamburg	0	1
Lower Saxony	6	31
Bremen	0	1
North Rhine-Westphalia	1	46
Hesse	7	10
Rhineland-Palatinate	22	8
Baden-Württemberg	20	11
Bavaria	50	25
Saarland	1	2
Berlin	1	0
Brandenburg	13	3
Mecklenburg-Vorpommern	2	0
Saxony	6	3
Saxony-Anhalt	14	0
Thuringia	17	4

Source: Own calculations based on numbers of the
Federal Statistical Office (2011a, 2015a) from 318 districts.

comes into effect. Under the assumptions of i) parallel trends of control and treated group in the absence of the reform, ii) the absence of anticipation effects and iii) the stable unit treatment value assumption (SUTVA), the double difference depicts the treatment effect (Lechner, 2011). Assumption i) can be expressed as

$$E(Y^0(i,1)|X(i),D(i,1)=1) - E(Y^0(i,0)|X(i),D(i,1)=1)$$
$$= E(Y^0(i,1)|X(i),D(i,1)=0) - E(Y^0(i,0)|X(i),D(i,1)=0)$$

where $E(Y^0(i,t)$ denotes the potential outcome in the absence of the treatment at time $t=0,1$ for individual i, some covariates $X(i)$ and the treatment status $D(i,t)=0,1$ which is always 0 in $t=0$. $E(Y^1(i,t))$ is its counterpart under the reform. Assumption i) is crucial for DD-estimation, as deviating from the common trend gives a biased estimate for

$$E(Y^1(i,1)|X(i),D(i,1)=1) - E(Y^0(i,1)|X(i),D(i,1)=1)$$

$$= E(Y^1(i,1)|X(i), D(i,1) = 1) - E(Y^0(i,0)|X(i), D(i,1) = 1)$$
$$- [E(Y^0(i,1)|X(i), D(i,1) = 0) - E(Y^0(i,0)|X(i), D(i,1) = 0)]$$
$$= E(Y(i,1) - Y(i,0)|X(i), D(i,1) = 1)$$
$$- E(Y(i,1) - Y(i,0)|X(i), D(i,1) = 0)$$

the average reform effect of interest. For the following, I simplify notation and write $D(i,1) = D(i)$ and drop the individual index i. I follow the approach of Abadie (2005) by combining the DD-estimator with inverse probability weighting (IPW) to take non-parallel outcome dynamics caused by differences in observable characteristics into account. A two step-procedure firstly estimates weights based on the propensity $P(D = 1|X)$ for weighting temporal differences in the outcome. The second step gives a non-parametric mean comparison weighted by ρ_0 which is determined parametrically:

$$E[\frac{P(D = 1|X)}{P(D = 1)}\rho_0 Y] = E(Y^1(1)|D = 1 - Y^0(1)|D = 1)$$

where

$$\rho_0 = \frac{T - \lambda}{\lambda(1 - \lambda)} \frac{D - P(D = 1|X)}{P(D = 1|X)P(D = 0|X)}$$

and λ being the share of post-treatment observations (see Abadie (2005) for details). This approach has two main advantages. Firstly, it does not met a functional form assumption in the second stage and allows for flexibility. The second advantage concerns the common support between control and treatment group. If an observational unit does not have common support within the other group, it can be dropped leading to higher comparability between treated and control group - a feature that the outcome-based linear model cannot consider.

Weighting temporal differences in the outcome is in particular relevant, as the reform not only included the expansion of subsidized child care, but also the introduction of home care allowances. Both treated and control group can apply for these benefits and thus, if their outcome dynamics are the same in presence of the home care allowances , the estimated effect only measures the effect of the child care expansion. Therefore, I furthermore rely on defining treatment status based on the median increase in child care coverage. Using mothers of older children as control group (e.g. Bauernschuster/Schlotter, 2015) would not allow for minimizing the effect of the home care allowances. One might argue that control districts for which child care increased by a lower amount are characterized by a larger increase in receipt of home care allowances. However, Alt et al. (2015) show that benefit receipt is the smallest in East Germany where most of the control districts are placed (compare Table 4). Furthermore, I will run robustness checks with the number of public subsidies received as outcome variable and find no systematic differences in the take-up of public subsidies between high- and low-intensity districts.

Anticipation effects can be ruled out by not using observations directly before the intervention came into force. Like previously mentioned, I only use pre-reform observations from 2011. Besides, SUTVA rules interactions between groups out. The assumption implies that individuals should not change between groups which might in particular be relevant for families moving from a control district to a treated district or vice versa. Due to the repeated cross sections I cannot completely exclude these individuals, but I can control for families having moved within the last 12 months. The estimates would also be biased in case of other reforms taking place during the observational period. A major reform on parental leave already came into force in January 2007 incentivizing mothers to return to work at expiration of parental benefits (Bergemann/Riphahn, 2010, 2015; Kluve/Tamm, 2013; Kluve/Schmitz, 2018). However, the regulations were changed in July 2015 to make part-time work during benefit receipt more attractive (*Elterngeld Plus*). Findings suggest that dropping mothers of less than one-year-olds, who are affected by the reform, will turn out to be robust compared to the baseline results.

3.2 Data and descriptive findings

The data is from the German Micro Census, a one percent representative sample of German households. It contains annual information on the family background, employment and other individual-specific characteristics. The survey conducted by the Federal Statistical Office partially rotates between chosen districts such that a district of households stays in the sample for four years. A main advantage of the Micro Census is the detailed information on the family composition. Hence, a child's and partner's characteristics can be connected with the observational unit of interest, mothers whose youngest child is aged up to three years old. I restrict the sample to mothers who are between 18 and 45 years old and who live in a private household which corresponds to the main place of residence. A further particularity of the Micro Census is the availability of not only agreed weekly working hours, but also the individuals working hour preferences. In contrast to other surveys like the German Socio-econmic Panel (GSOEP) the question on working hours in the Micro Census is filtered which means that before stating the amount of preferred working hours the individual is asked if he/she wants to increase or decrease the agreed weekly working hours conditioned on an earnings adjustment[3] (for a methodological comparison of survey data on working hour preferences see Holst/Bringmann, 2016). Thus, there is also a measure for under- (the wish for an increase of agreed hours) and overemployment (the preference for less weekly hours).
I link the Micro Census data with statistics on the regional child care coverage rate for children aged up to three years old from the German Federal Statistical Office on the district level (Federal Statistical Office, 2010a, 2011a, 2012a, 2013a, 2014a, 2015a). The child care coverage rate is measured on the cut-off date March, 1st and includes children in subsidized care not additionally attending another care arrangement and children in other care arrangements apart from subsidized care. Furthermore, I include measures for the regional number of unemployed and vacancies from the German Federal Employment Agency to calculate the labor market tightness in each district.
The final sample includes 9,668 mothers (of which 3,038 are currently employed) of children aged up to three years old. I restrict the sample to married mothers sharing the place of residence with their partner as the share of single mothers is too small (less than five percent in the final sample) to account for this group.

[3] Information on the preference for an hour increase (decrease) is included since 2006 (2008).

The variables used for estimating the propensity score described in the previous section and their descriptive statistics are listed in Table 5: family and individual caracteristics, but also information on the interview and the macroeconomic situation. These numbers result after trimming observations, i.e. dropping individuals with a propensity score close (< 0.05) to the minimum and maximum value (compare Imbens/Wooldridge (2009)). Trimming excludes 3,840 observations in the whole sample ($N = 146$ in the control group, $N = 3,694$ in the treated group) and 1,188 individuals of the employed sample ($N = 76$ in the control group, $N = 1,112$ in the treated group)[4]. A major threat to identification might stem from using repeated cross-sections instead of panel data as individuals could have selected into employment after the reform came effective. Hence, a balancing check looks at the covariate distribution of control and treated group over time. Additional to mean values and standard deviations, Table 5 gives the standardized mean difference defined as the mean difference over time divided by the square root of the average variance (see Rubin (2001)). It rarely exceeds the critical value of 0.25 suggesting that selection over time depicts a minor problem. One can only observe substantial differences for the labor market tightness reflecting economic growth over these last years. However, this development is similar for low- and high-intensity districts.

Table 6 shows the mean of the child care coverage rate and of the examined outcome variables, its standard deviation and mean differences between treated and control group before and after the reform. The average coverage rate is weighted by the number of included individuals living in each district and shows that less than one quarter of children in high-intensity districts are in subsidized care before the reform came into force. More mothers in low-intensity districts use subsidized care before the reform (negative, statistically significant difference at $t = 0$), but high-intensity districts catch up resulting in a positive spread at $t = 1$[5].
As outcomes I examine the extensive and intensive margin, i.e., a dummy for employment, agreed and preferred working hours and a binary indicator for working full-time (more than 30 hours per week). The federal Statistical Office measures employment according to the concept of the International Labour Organization (employment for at least one paid hour or self-employment in the week before the interview) which includes employees in maternity protection and parental leave. Hence, I rely on the concept of realized employment and exclude them. About one third of all mothers in high-intensity districts are currently employed with an average of 24 hours per week. They prefer to slightly work more, on average one hour per week. About 28 percent of them hold a full-time position. The last two columns of Table 6 give the differences in means between treated and control group before and after the reform. At $t = 0$ employment rates in high- and low-intensity districts differ significantly, but the difference vanishes after the reform. Concerning the intensive margin, one cannot detect any strong variation across groups and time for all measures. Hence, descriptive findings suggest a positive link between the expansion of subsidized child care and the employment status, but no relation to the intensive margin.

[4] Histograms of the propensity score before and after trimming by group membership can be found in the appendix.
[5] Note that these are unconditional numbers that cannot give information on actual take-up of a child care slot on the individual level.

Table 5: Descriptive statistics by group membership

	Whole sample						Currently employed sample					
	$d=0,t=0$		$d=0,post-pre$	$d=1,t=0$		$d=1,post-pre$	$d=0,t=0$		$d=0,post-pre$	$d=1,t=0$		$d=1,post-pre$
Variable	Mean	sd	Std. mean diff.	Mean	sd	Std. mean diff.	Mean	sd	Std. mean diff.	Mean	sd	Std. mean diff.
Age of youngest child	0.985	0.805	-0.024	0.989	0.818	-0.035	1.341	0.682	0.102	1.354	0.702	0.090
Number of children	1.951	0.999	-0.111	1.971	1.042	-0.112	1.878	0.942	-0.125	1.922	1.068	-0.122
Age	32.79	5.027	-0.015	32.96	5.237	-0.015	33.81	4.955	-0.068	34.03	5.194	-0.090
Migration background:												
none	0.881	0.323	-0.039	0.821	0.384	0.025	0.925	0.264	0.042	0.891	0.312	0.057
from EU country	0.033	0.178	0.088	0.051	0.219	0.046	0.030	0.172	0.011	0.047	0.211	-0.032
no EU country	0.086	0.280	-0.016	0.129	0.335	-0.062	0.045	0.207	-0.066	0.062	0.242	-0.046
Job position:												
Self-employed	-	-	-	-	-	-	0.113	0.317	-0.126	0.120	0.326	-0.149
Civil servant	-	-	-	-	-	-	0.066	0.249	0.074	0.082	0.275	0.009
White-collar worker	-	-	-	-	-	-	0.707	0.455	0.067	0.690	0.463	0.141
Blue-collar worker	-	-	-	-	-	-	0.100	0.300	-0.053	0.096	0.295	-0.134
Others	-	-	-	-	-	-	0.013	0.115	0.022	0.011	0.106	0.108
Tenure	-	-	-	-	-	-	6.837	5.955	0.037	6.996	6.032	-0.124
Net individual income	762.1	793.0	0.193	784.0	864.8	0.196	1110.0	948.2	0.240	1257.1	1009.9	0.112
Educational degree:												
Lower secondary school	0.219	0.414	-0.059	0.203	0.403	-0.150	0.173	0.378	-0.046	0.136	0.343	-0.096
Middle secondary school	0.376	0.484	0.004	0.333	0.471	-0.029	0.363	0.481	0.047	0.353	0.478	-0.065
High school	0.405	0.491	0.045	0.464	0.499	0.141	0.465	0.499	-0.011	0.511	0.500	0.124
Inactive partner	0.051	0.220	-0.046	0.062	0.242	-0.039	0.049	0.217	-0.143	0.057	0.231	-0.138
Job position:												
Self-employed	0.124	0.330	-0.093	0.113	0.316	-0.063	0.164	0.370	-0.170	0.147	0.355	-0.147
Civil servant	0.043	0.203	0.071	0.045	0.208	0.053	0.051	0.219	0.097	0.061	0.239	0.015
White-collar worker	0.488	0.500	0.118	0.516	0.500	0.165	0.477	0.500	0.147	0.492	0.500	0.289
Blue-collar worker	0.279	0.449	-0.073	0.248	0.432	-0.150	0.245	0.430	-0.029	0.224	0.417	-0.156
Others	0.015	0.120	-0.030	0.016	0.126	-0.030	0.015	0.120	0.001	0.020	0.140	-0.067
Employment status:												
Full-time	0.906	0.292	-0.020	0.864	0.343	0.073	0.897	0.304	0.040	0.865	0.341	0.091
Part-time	0.032	0.177	0.058	0.051	0.220	-0.031	0.044	0.205	0.064	0.054	0.226	0.028
Marginal employment	0.011	0.103	0.040	0.022	0.148	-0.061	0.010	0.100	0.006	0.024	0.153	-0.059
Labor market tightness (district)	0.022	0.011	0.449	0.019	0.009	0.511	0.021	0.011	0.466	0.019	0.009	0.507
East Germany	0.171	0.376	-0.009	0.070	0.255	0.004	0.224	0.417	0.009	0.096	0.295	0.019
Quarter of interview												
1	0.261	0.439	-0.039	0.232	0.422	-0.005	0.275	0.447	-0.015	0.229	0.421	0.055
2	0.249	0.433	-0.027	0.263	0.440	-0.064	0.249	0.433	0.004	0.258	0.438	-0.055
3	0.238	0.426	0.036	0.256	0.437	-0.041	0.224	0.417	0.012	0.254	0.435	-0.079
4	0.251	0.434	0.030	0.249	0.432	0.107	0.251	0.434	-0.001	0.259	0.439	0.076
Interview part												
Head of household	0.193	0.394	0.011	0.217	0.412	0.013	0.205	0.404	<-0.001	0.248	0.432	-0.032
Self-reported	0.716	0.451	-0.060	0.710	0.454	-0.100	0.746	0.435	-0.006	0.717	0.451	0.007
No information	0.092	0.289	0.076	0.073	0.260	0.141	0.048	0.214	0.013	0.035	0.185	0.053
N	2,539			2,233			891			706		

The sample includes 18 to 45 years old, married mothers of up to three-year-olds. Instead of a dummy for East Germany, the analysis includes federal states.
The standardized mean difference gives the mean difference of post- and pre-reform years divided by the square root of the average variance (Rubin, 2001).
Source: Own calculations based on data from the German Micro Census, the Federal Statistical Office (2011a, 2015a) and the Federal Employment Agency.

Table 6: Mean outcomes and coverage rate by group membership

	$d = 1, t = 0$			Difference in means *Treated-control group*	
Variable	Mean	sd	N	$t = 0$	$t = 1$
All: Coverage rate (weighted) $\%$	22.04	(8.06)	2,233	-2.76***	2.8***
Employed: Coverage rate (weighted) $\%$	22.95	(9.10)	706	-3.33***	1.87***
Employed *of which:*	0.3515	0.4776	2,233	-0.0364***	0.0013
Agreed hours	24.00	13.13	706	0.48	0.49
Preferred hours	25.13	13.11	706	0.53	0.26
Full-time	0.2847	0.4516	706	-0.0138	-0.0115

$*p < 0.1, **p < 0.05, ***p < 0.01$. The sample includes 18 to 45 years old, married mothers of up to three-year-olds.
Source: Own calculations based on data from the German Micro Census, the Federal Statistical Office (2011a, 2015a) and the Federal Employment Agency.

4 Estimation results

4.1 Main results

Table 7 shows the baseline estimation results for the whole sample and different subgroups according to Abadie (2005). Standard errors consider the two-step estimation procedure by bootstrapping taking clusters on the district level into account.
In general, districts with a large increase of the coverage rate experience a rise of both the employment rate and working hours compared to districts with a lower expansion of child care. The reform effect amounts to an increase of the employment rate of ten percentage points. Agreed and preferred weekly hours increase by 4.1 and 4.0 (17 percent of the pre-reform mean) respectively. Although the estimates lack statistical significance on conventional levels, they are close to it. Interestingly, these findings suggest an almost equal adjustment of agreed and preferred hours such that the effect on the mismatch size is close to zero. The similar adjustment also implies that the effect is not only driven by involuntarily underemployed mothers who adjust agreed to preferred working hours, but that both distributions change. They suggest (see histograms of agreed and preferred working hours in the Appendix) a shift from marginal employment (categorized as up to 15 hours per week) to part-time work (between 16 and up to 30 hours per week). One can also observe a decrease at the upper part of the hour distribution. However, it contributes less to the average effect due to the lower share of full-time working mothers. Further estimation results not shown in Table 7 demonstrate that the share of under- and overemployed mothers is not significantly affected. Hence, the overall positive effect on working hours is driven by a shift from marginal to part-time employment which also shows up in a not affected share of full-time employed.
The estimates vary strongly for other subgroups. Mothers with high school degree show a strong response to the reform by a rise in the employment rate of about 14 percentage points. Mothers with a lower degree are characterized by small or even negative effects that are not statistically significant, but sample size is considerably smaller for those subgroups. Havnes/Mogstad (2011) also find larger effects for better educated women. However, this difference is weaker pronounced as the general reform effect also turns out to be smaller.

I also condition on the individual net income that includes public allowances and capital income. Interestingly, mothers with low- to middle income (up to 1,000 Euro) seem to have benefitted more from the reform as these mothers show a large rise of employment (11 percentage points) and especially of hours (11 and 12 hours per week for agreed and preferred hours). For mothers with higher income the effects are as well positive but lower and not statistically significant. The latter finding provides evidence for a stronger income or weaker substitution effect for this group, i.e. mothers with higher income have a higher preference for leisure. The variation of the estimates for income- and education-based subgroups do not necessarily contradict as the net income also depicts other income sources, tax incentives and the employment status (marginal employment, full- or part-time work). Hence, mothers with lower secondary school degree can also belong to the group with higher net income. Besides, the small group of employed women with lower secondary school degree contributes less to the average effect on working hours.

As Weber/Zimmert (2017) show socio-economic characteristics are correlated with the type of an working hour discrepancy. Therefore, heterogenous effects can indicate how unconstrained, under- or overemployed mothers adjust preferred and agreed working hours. The authors find that overemployment is linked to better education, jobs of higher occupational autonomy and higher wages while the opposite holds for underemployment. The estimates do not suggest deviating adjustment mechanisms for preferred and agreed working hours for different subgroups which supports the consideration of an equal shift along the distribution.

Finally, the last rows show that the effects do not strongly vary for different age groups.

4.2 Robustness

Table 8 contains different robustness checks. Firstly, for investigating the common trend assumption which is crucial for DD-estimation I check whether the time trend before the reform is the same for districts with a high and smaller increase of the coverage rate. I test a specification by introducing a placebo reform with the pre-reform period being 2010 ($t = 0$) and the post-reform period 2011 ($t = 1$). The estimates prove not to statistically significantly differ from zero (Panel D, first row). Hence, close to the reform treated and control group show a similar time trend. Since the reform also included the introduction of home care allowances, a second test uses the number of received public subsidies as outcome variable. Unfortunately, an explicit information on the receipt of home care allowances is not available. The last rows of Panel D demonstrate that treated and control group do not show a statistically different pattern concerning the receipt of public subsidies suggesting that the baseline estimate in table 7 is likely to solely measure the effect of the expansion of subsidized child care.

The next specification (Panel A) uses the median of the increase of the coverage rate for redefining the treatment and control group. As expected, the effect is less pronounced compared to using a clearer cut as in the main specification in Table 7.

Other checks deviate form the baseline by changing the sample composition (Panel B). The reform demonstrates to have a similar, but stronger effect when using only West German districts. Employment of mothers living in high-intensity West-German districts increases by 12 percentage points, their working hours by about seven hours per week. Again, the

Table 7: Main estimation results

	Employed (binary)	Agreed hours	Preferred hours	Full-time
Panel A: Whole sample				
Whole sample	0.1006***	4.1228	3.973	0.0010
	(0.0317)	(2.6386)	(2.6727)	(0.05583)
N	9,668	3,038	3,038	3,038
Panel B: Heterogeneities				
Education:				
Lower secondary school	0.0780	-1.2869	-0.4762	-0.1103
	(0.0624)	(5.7127)	(6.2163)	(0.0992)
N	1,810	399	399	399
Middle secondary school	0.0128	3.4284	3.5950	0.0322
	(0.0645)	(4.5154)	(4.8162)	(0.0877)
N	3,391	1,066	1,066	1,066
High school	0.1354***	4.9319	5.4396	-0.0106
	(0.0467)	(4.0373)	(4.0415)	(0.0872)
N	4,355	1,436	1,436	1,436
Individual net income:				
< 1000 Euro/month	0.1149***	10.9015***	12.2056***	0.0436
	(0.0340)	(2.7107)	(3.1427)	(0.0376)
N	6,375	1,377	1,377	1,377
≥ 1000 Euro/month	0.0946	1.2796	1.3972	-0.0090
	(0.0696)	(4.1941)	(4.2007)	(0.0943)
N	3,188	1,620	1,620	1,620
Age:				
< 35 years	0.1008**	3.4583	3.7562	-0.0570
	(0.0401)	(3.4481)	(3.6123)	(0.0737)
N	6,110	1,734	1,734	1,734
≥ 35 years	0.0951	3.5326	3.1989	0.0172
	(0.0600)	(3.9087)	(4.0019)	(0.0845)
N	3,497	1,253	1,253	1,253

$*p < 0.1, **p < 0.05, ***p < 0.01$. Standard errors (in columns) are bootstrapped with 1,000 replications considering clusters on district level. The sample includes 18 to 45 years old, married mothers of up to three-year-olds. Agreed and preferred hours are measured on weekly basis.
Source: Own calculations based on data from the German Micro Census, the Federal Statistical Office (2011a, 2015a) and the Federal Employment Agency.

share of full-time employed is not significantly affected. These findings show that the overall effect for both East and West Germany turns out to be robust considering any systematic differences between districts of the former GDR and West German districts implying no major problem for including East Germany in the baseline analysis. Dropping mothers of children younger than one year old leads to a slightly larger effect for all outcomes. Hence, the parental leave reform of 2015 that affected mothers with children in this age group is not supposed to drive the results. As mothers working in a child care facility might be differently affected by the reform, they are excluded in another specification which only slightly changes the estimates. Secondly, I test for selective migration by excluding those having changed their place of residence within the last twelve months. These estimates are also similar to the baseline results of Table 7.

A last specification in Panel C varies by running parametric regressions with OLS of the form:

$$y_{it} = \gamma_1 post_{it} + \gamma_2 treat_{it} + \gamma_3 post_{it} * treat_{it} + \delta X_{it} + \mu_{it}$$

with $post$ being one in $t = 1$ and zero otherwise, $treat$ depicting treatment status and their interaction giving the estimate of interest, γ_3. X includes the same covariates as for semi-parametric estimation and the standard errors are clustered on the district level. Although the estimates are clearly smaller compared to the baseline according to Abadie (2005), their signs and statistical relevance are the same.

Table 8: Robustness

	Employed (binary)	Agreed hours	Preferred hours	Full-time
Panel A: Treatment definition				
Median division	0.0631**	3.1862	2.8345	0.0176
	(0.0252)	(2.0715)	(2.0884)	(0.0408)
N	13,246	4,224	4,224	4,224
Panel B: Sample composition				
West Germany	0.1209***	7.2871**	7.0683**	0.0800
	(0.0399)	(3.2207)	(3.2726)	(0.0639)
N	8,838	2,685	2,685	2,685
Without 1-year-olds	0.1440***	5.6205*	5.279*	0.0586
	(0.0478)	(2.8811)	(2.9714)	(0.0599)
N	6,338	2,725	2,725	2,725
Without childminders	0.0863***	3.7500	3.4963	0.0023
	(0.0314)	(2.6609)	(2.7514)	(0.0574)
N	9,692	3,044	3,044	3,044
Without families having moved	0.1064***	3.5563	4.0309	-0.0284
	(0.0340)	(2.7094)	(2.8439)	(0.0586)
N	8,660	2,784	2,784	2,784
Panel C: Parametric form				
Parametric form	0.0304*	1.0072	0.5826	0.0359
	(0.0171)	(0.7353)	(0.7792)	(0.0282)
N	9,668	3,038	3,038	3,038
Panel D: Common trend				
Placebo reform	0.0088	-0.6104	0.3298	-0.0387
	(0.0322)	(2.2377)	(2.4367)	(0.0477)
N	9,574	3,175	3,175	3,175

Number of public subsidies	*Whole sample:* Treated, $t = 0$: Mean	ATT	*Employed:* Treated, $t = 0$: Mean	ATT
	0.8433	-0.0102	0.6275	0.0791
	(0.7582)	(0.0625)	(0.5675)	(0.0880)
		9,665		3,037

$*p < 0.1, **p < 0.05, ***p < 0.01$. Standard errors (in columns) are bootstrapped with 1,000 replications considering clusters on district level. The sample includes 18 to 45 years old, married mothers of up to three-year-olds. Agreed and preferred hours are measured on weekly basis.
Source: Own calculations based on data from the German Micro Census, the Federal Statistical Office (2011a, 2015a) and the Federal Employment Agency.

5 Conclusion

This paper provides empirical evidence for the causal effect of subsidizing early child care on maternal labor market outcomes. It exploits the staggered expansion of early child care provision in Germany culminating in a legal claim for a child care slot introduced in 2013. The presented semi-parametrically estimated intention-to-treat effects suggest a strong impact of ten percentage points on the maternal employment rate and less significantly of four on agreed and preferred weekly working hours. Besides, the share of full-time employed women does not change in response to the reform which might result from limited provision of full-time child care slots or the parental preference for part-time care. Although the share of realized full-time slots (defined as more than seven hours per day) almost doubled from 2011 to 2015 in high-intensity districts, only one out of ten children attends full-time care in post-reform years. However, these numbers cannot definitely answer which of the two channels, lack of provision or parental preferences, prevails as they do not give information on the supply of full-time slots.

The main findings are in general in line with previous results for Germany. Bauernschuster/Schlotter (2015) estimate intention-to-treat effects for the eligibility to kindergarten in the range of five to eight percentage points for employment and of 2.5 for weekly hours. Fendel/Jochimsen (2017) find an increase of maternal employment of eight percentage points for the overall reform, i.e. the legal claim for a child care slot and the introduction of the home care allowances. Hence, these findings for Germany turn out to be robust compared to other countries with low maternal labor market participation (Schlosser, 2005; Berlinski/Galiani, 2007; Baker/Gruber/Milligan, 2008; Lefebvre/Merrigan, 2008; Berlinski/Galiani/Mc Ewan, 2011; Nollenberger/Rodríguez-Planas, 2011). Another crucial finding concerns the adjustment of agreed and preferred working hours. Both measures change, but in contrast to Reynolds/Johnson (2012) this article finds that agreed and preferred working hours adjust in line with each other. This result implies that also the size of the mismatch remains close to zero and that the results are not driven by involuntarily underemployed mothers adjusting agreed to preferred working hours. On the contrary, the availability of low cost child care has the potential to increase working hour preferences also for other groups represented in an overall shift of the distributions of agreed and preferred working hours. Mothers changing from marginal to part-time work characterize this shift. However, the impact on the share of under- and overemployed mothers is not significant.

Heterogenous effects furthermore suggest that families with low and middle income show the largest response. As the German government recently decided on further investment in early child care, especially on supporting low-income earners, the findings turn out to be relevant for current discussions.

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A Graphs

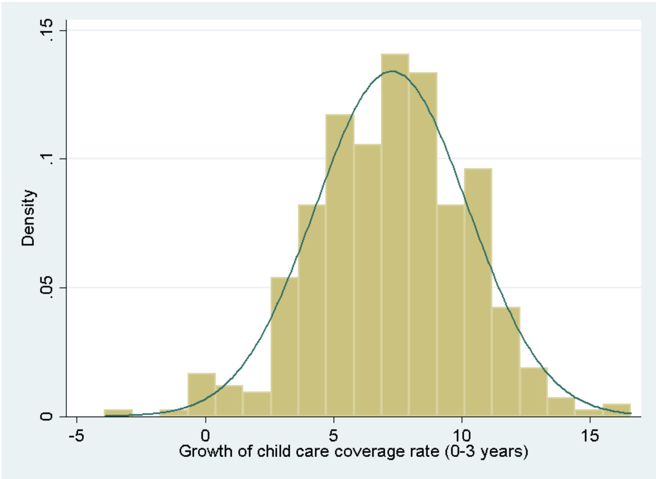

Figure 2: Histogram of child care coverage growth from 2011 to 2015.
Unweighted calculations based on 318 districts. Source: Federal Statistical Office (2011a, 2015a).

Figures 3-6: Propensity scores of whole sample

Source: Own calculations based on data from the German Micro Census, the Federal Statistical Office (2011a, 2015a) and the Federal Employment Agency.

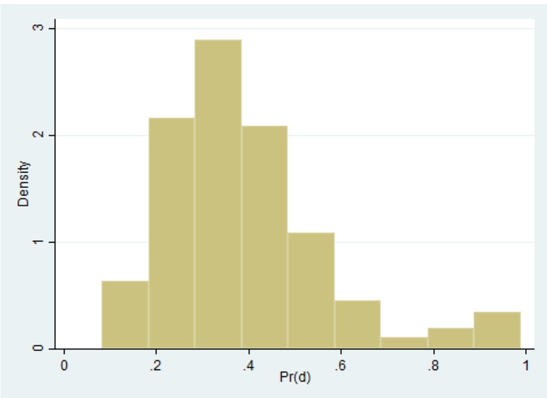

Figure 3: Control group before trimming

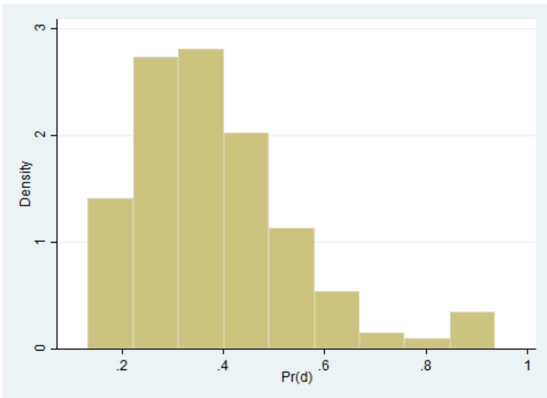

Figure 4: Control group after trimming

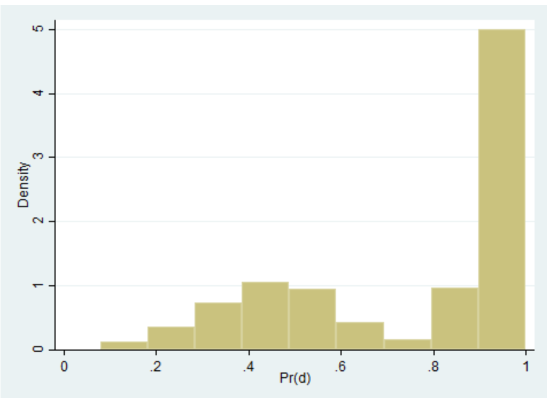

Figure 5: Treated group before trimming

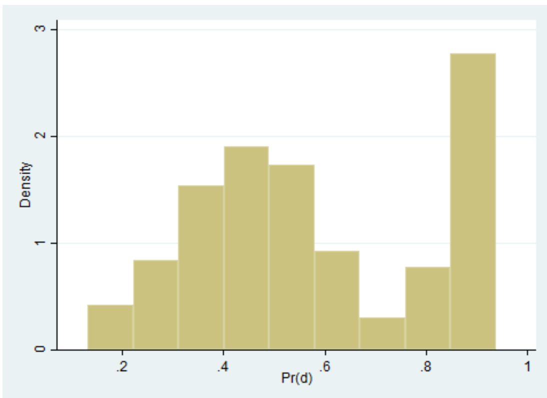

Figure 6: Treated group after trimming

Figures 7-10: Propensity scores of employed sample

Source: Own calculations based on data from the German Micro Census, the Federal Statistical Office (2011a, 2015a) and the Federal Employment Agency.

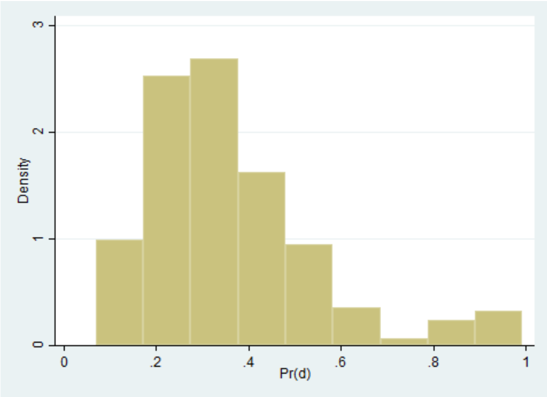

Figure 7: Control group before trimming

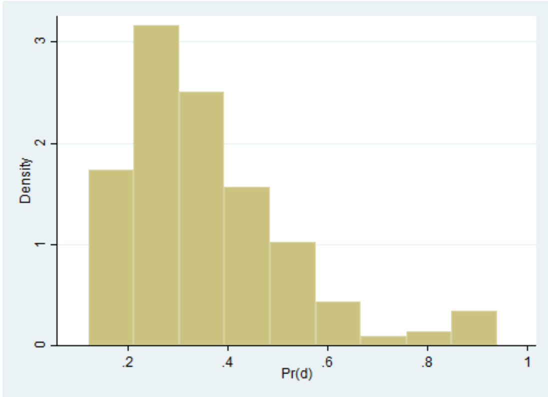

Figure 8: Control group after trimming

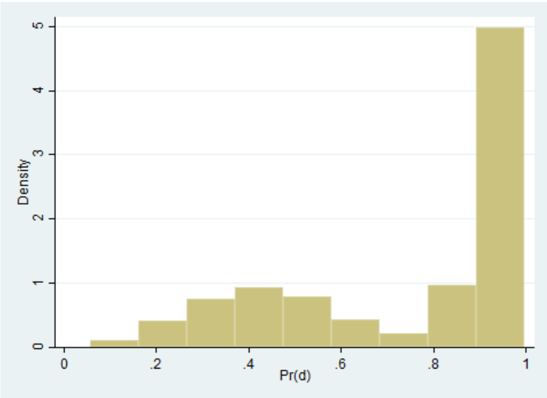

Figure 9: Treated group before trimming

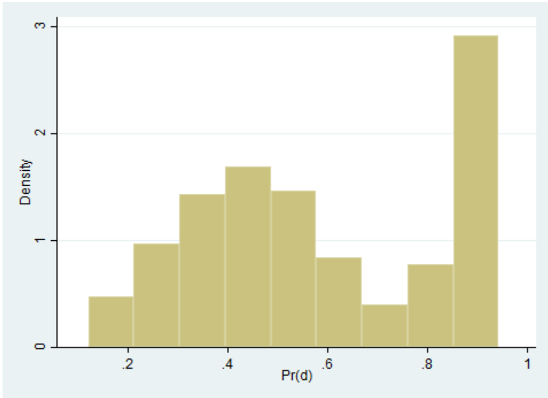

Figure 10: Treated group after trimming

Figures 11-14: Distribution of agreed working hours across group membership

Source: Own calculations based on data from the German Micro Census, the Federal Statistical Office (2011a, 2015a) and the Federal Employment Agency.

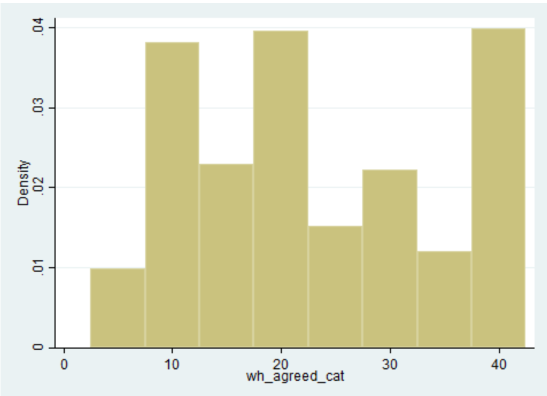

Figure 11: Control group before reform

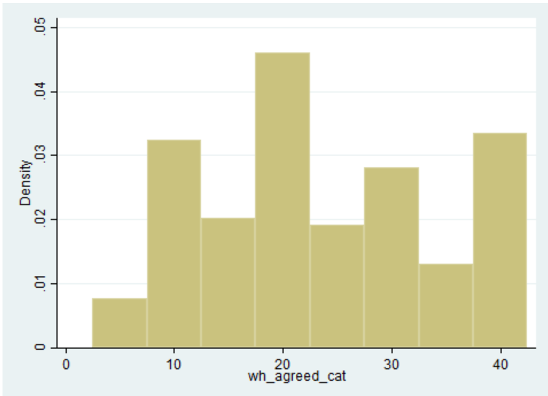

Figure 12: Control group after reform

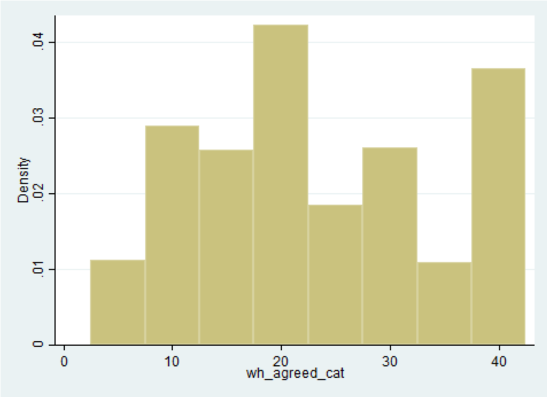

Figure 13: Treated group before reform

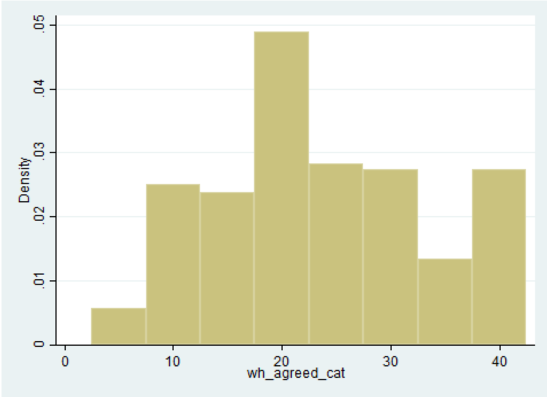

Figure 14: Treated group after reform

Figures 15-18: Distribution of preferred working hours across group membership

Source: Own calculations based on data from the German Micro Census, the Federal Statistical Office (2011a, 2015a) and the Federal Employment Agency.

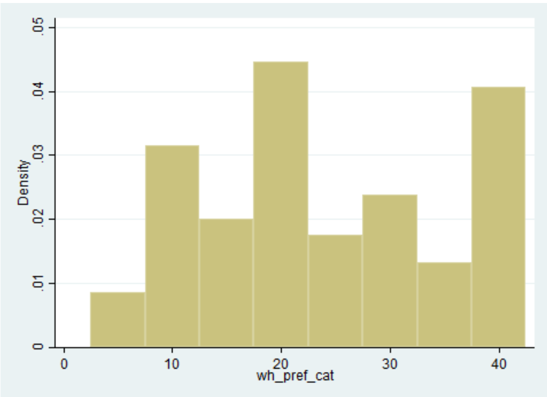

Figure 15: Control group before reform

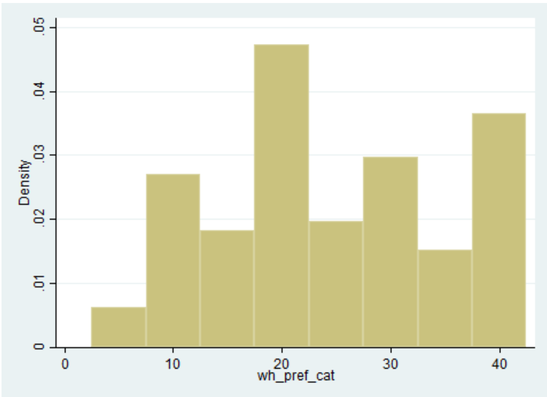

Figure 16: Control group after reform

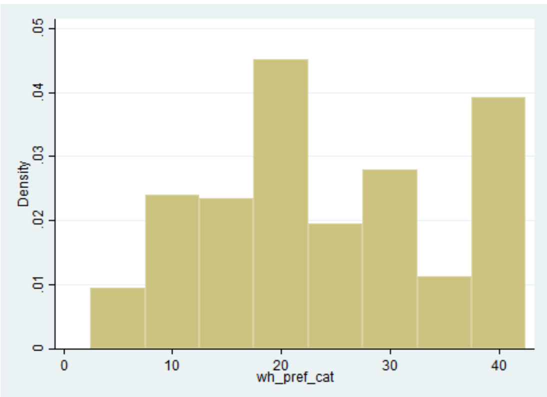

Figure 17: Treated group before reform

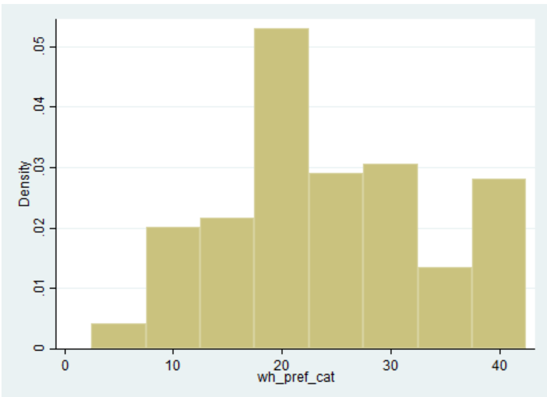

Figure 18: Treated group after reform

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