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Early child care and the employment potential of mothers: evidence from semi-parametric difference-in-differences estimation

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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, the adjustment of agreed working hours and especially of preferred working hours. Semi-parametric difference-in-differences estimation based on survey data from the German Microcensus results in positive effects on the employment rate, as well as on agreed and preferred working hours by up to 20% of the pre-reform mean. As agreed and preferred working hours adjust in line with each other, the expansion of early child care can tap labor market potentials beyond those of currently underemployed mothers. Moreover, conditional effects show that especially better educated and non-single mothers respond to the reform.

Keywords Early child care, Maternal labor supply, Semi-parametric difference-in-differences, Subsidized child care, Working hour preferences

JEL Classification J21, J22, I38

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 (Lundborg et al. 2017). Hence, policymakers advocate an expansion of publicly subsidized child care in order to strengthen the employment potential in ageing societies. Indeed, the female employment rate turns out to be higher in countries such as the Scandinavian states where child care is sufficiently provided (Morrissey 2017). However, empirical studies cannot unanimously support a positive causal relationship between subsidized child care and female employment outcomes. I address this issue by evaluating not only the effect of low-cost subsidized child care on the employment share and agreed weekly working hours, but I further inform these debates by also examining underlying working hour preferences.

Working hour discrepancies are quite common in industrialized countries as previous studies suggest (Drago et al. 2005; Ehing 2014; Fagan 2001; Merz 2002; Pollmann-Schult 2009; Reynolds 2003, 2004). Hence, evaluating if the availability of subsidized child care can affect working hour discrepancies is important in ageing

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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 (Ehing 2014; Matiaske et al. 2017). Examining working hour preferences in addition to agreed hours might be more informative as they tell more about the underlying reasons of labour supply. While short working hours are considered to be one of the main reasons for the gender wage gap (Goldin 2014), they can also be the voluntary wish of a mother who is more satisfied with reduced working 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. I use the exogenous variation of the expansion of subsidized child care induced by the reform in 2013 to compare districts in which the coverage rate increased significantly (the treated or high-expansion group) with those for which the coverage rate changed only by a small amount (the control or low-expansion group). To be more concrete, I follow the approach of Bauernschuster et al. (2016), Felfe et al. (2015) and Havnes and Mogstad (2011) who exploit spatial variation of German districts, Spanish states and Norwegian 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 (DiD) strategy compares labor market outcomes of mothers with children aged up to 3 years in treated districts with those where child care increases to a lesser extent before and after the legal claim came into force. In comparison to related studies in which the child care coverage rate is on similar levels close to zero in the treatment and control group before the reform (Bauernschuster et al. 2016; Felfe et al. 2015; Havnes and Mogstad 2011), child care is already established in both groups in this setting, but on a lower level in districts with a higher expansion up to the year 2015 ("catch-up effect"). While other articles evaluate earlier periods of the German child care expansion (Bauernschuster et al. 2016; Müller and Wrohlich 2020), this article sets 2013 as the target year when every child is entitled to a childcare slot.[1]

I analyze the German labor market as an interesting example for the persistence of traditional employment patterns. 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 (Wanger 2015). 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.

Instead of applying a linear OLS estimator, a two-stage semi-parametric DiD estimation procedure proposed by Abadie (2005) is used such that the linear form assumption in the outcome equation does not need to hold and observations without overlap of the control variables can be dropped. Moreover, the approach allows to infer heterogeneous treatment effects. I use a rich data set from the German Microcensus which is a 1% representative sample of German households (Research Data Centre 2011, 2015). 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.

The resulting intention-to-treat estimates give a positive impact both on the extensive and intensive margin. Mothers of up to 3-year-olds in districts with a large increase of the child care coverage rate have a 5.7 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 5 h per week higher and change similarly such that their mismatch is not affected. I furthermore show that the estimates are higher for better educated mothers and that the adjustment mechanism of agreed and preferred working hours differs for cohabiting mothers.

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 Sect. 4. The last section concludes.

2 Child care availability and maternal employment

Estimating the causal effect of publicly financed child care on employment outcomes suffers from several difficulties. One is that its price and the availability of informal child care provided by the family are often insufficiently observed (Havnes and 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 an instrumental variable (for a review see Morrissey 2017). However,

[1] As I will discuss in detail later, the main identifying might furthermore be violated for earlier periods. Besides, relevant data is only available up from 2008.

Table 1 Main findings of related evaluation studies on child care concerning maternal employment

Article	Country/region	Method	Main findings
Andresen and Havnes (2019)	Norway	DiD	Positive ffects for cohabiting mothers with children younger than 3 years characterized by a shift to full-time employment
Baker et al. (2008)	Quebec	DiD	Increase of female employment by 7.7 percentage points
Bauernschuster and Schlotter (2015)	Germany	DiD	Transition to kindergarten is related to an increase in labor force participation by 36.6 percentage points and in average weekly hours by 14.3 h
Berlinski et al. (2011)	Argentina	RDD	Higher employment probability, also in full-time, and weekly hours rise on average by 7.8 if youngest child attends kindergarten
Berlinski and Galiani (2007)	Argentina	DiD	Positive employment effects for mothers of children aged three to five
Fendel and Jochimsen (2017)	Germany	DiD	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
Fitzpatrick (2012)	US	RDD	Positive effect of kindergarten attendance for single mothers
Gelbach (2002)	US	IV	Positive effect of public school enrollment on employment rate and on weekly hours for single mothers, slightly smaller effects for married women
Givord and Marbot (2015)	France	DiD	Effects close to zero for mothers of preschool children, higher effects for larger families
Havnes and Mogstad (2011)	Norway	DiD	Effects close to zero for mothers of 3–6 years old children
Lefebvre and Merrigan (2008)	Quebec	DiD	Positive effects on employment and working hours
Lundin et al. (2008)	Sweden	DiD	Effects close to zero, no effect variation across subgroups (age of children, educational level)
Müller and Wrohlich (2020)	West Germany	DiD	Increase in childcare slots by one percentage point goes along with a by 0.2 percentage points higher labor market participation, mainly explained by part-time employment and mothers with medium-level qualifications
Nollenberger and Rodríguez-Planas (2011)	Spain	DiD	Positive effects on maternal employment
Schlosser (2005)	Israel	DiD	Free public preschool increases employment of Arab mothers with children aged three to four by 8.1 percentage points and average weekly hours by 2.8 h

DiD, difference-in-differences; RDD, regression discontinuity design; IV, instrumental variable

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 range of the effect of more generous child care varies from positive to negligibly small or insignificant coefficients.

Table 1 gives an overview on related articles evaluating the effectiveness of available child care or a reduction in its price regarding maternal employment. It lists the countries or region under study as well as the used methodology and summarizes the main findings.

Referring to this table, ambiguous findings from preliminary empirical work might also stem from the ignorance of underlying preferences. Lundin et al. (2008) and Givord and Marbot (2015) might have find no effects in the context of an already high share of working mothers whose preferred and agreed working hours potentially match. Countries with lower maternal employment which show positive responses to the availability of subsidized child care could be those with a higher share of underemployed women adjusting agreed to preferred working hours. In line with these considerations several authors emphasize the role of adjusting preferences in case of occurring life events like the birth of a child (Campbell and van Wanrooy 2013; Drago et al. 2005; Reynolds and Johnson 2012). Reynolds and Johnson (2012) evaluate how the number of children living in the household affects preferred and actual working hours for the US and find 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 et al. (2005) who evaluate working hour preferences for Australian employees and conclude that women are more sensitive to changing life conditions than men. Zimmert and Weber (2021) examine the mismatch dynamics considering household and job characteristics and find suggestive evidence that the lack of institutional care arrangements may foster the creation of working hour discrepancies. However, the mentioned 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).

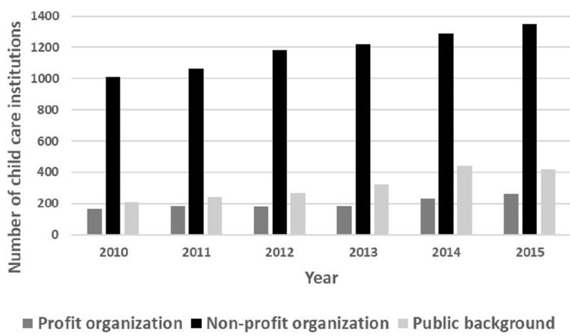

Fig. 1 Child care institutions by providers in Germany. *Notes*: Cut-off date is March 1st. *Source*: Federal Statistical Office (2010c, 2011c, 2012c, 2013c, 2014c, 2015b)

3 Institutional background, methodological approach, data and descriptive findings

3.1 Institutional background

The German system of child care has several particularities ranging from strong regional variation to the different providers of child care (Kreyenfeld and 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% in East Germany in comparison with 28.1% 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. Only 164 of 1386 child care institutions (about 12%) are profit organizations in 2010 (compare Fig. 1). In 2015, the number is higher (261), but the share remains stable by about 13%. Moreover, 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. One can also see in Fig. 1 that institutions with a public background experience the highest growth (in relative numbers) between the years.

3.1.1 The expansion of early child care

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 children aged 1–3 years from August 2013 onwards embedded in a law supporting the child's development (*KiFöG*).[2] In line with the legal claim for a kindergarten slot introduced in 1996 (children older than 2 years), the law focuses firstly on the child's education and not on parental employment. The supply of child care is organized on the community level and subsidized by the federal states. Moreover, the federal government supports the child care expansion. Until 2014, it has spent 5.4 billion Euro for improving child care supply and engaged for annual 845 million Euro beginning in 2015 (BMFSFJ 2015). 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 as 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 3 years old in subsidized care amounted to 49% in East Germany compared to only 20% in the rest of the country (Federal Statistical Office 2011b). The reform significantly changed the availability of child care slots. In 2015, 28.2% of children living in West-Germany and 51.9% in East Germany were in subsidized care (Federal Statistical Office 2015a).

Although the legal claim was announced 5 years before it came into force, a shortage of 80,000–100,000 slots was predicted in July 2013 for the next month which suggests an almost full take up ratio. In general, the provision of early child care orients on the existing supply of child care slots and not on the actual needs of the parents (Kreyenfeld and Hank 2000; BMFSFJ 2015). While communities take population growth for the planning process into account, authorities mainly neglect any other factors determining the demand for child care. Table 2 shows the take up ratio of child care 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.

[2] The legal claim guaranteed child care provided by a facility or childminder for children aged one to three (Sect. 24 SGB VIII). Children younger than 1 year are also eligible if their parents are employed. The reforms of August 2013 included also the introduction of home care allowances (HCA) that were available for children between 15 and 36 months old born after August 2012 and who are not using subsidized child care. Gathmann and Sass (2018) show that the introduction of similar HCA in the German federal state Thuringia in 2006 has small and insignificant effects on maternal labor supply.

Table 2 Take up ratio of child care. *Source*: Own calculations based on the Statistical reports of the Statistical Offices of the Federal States (Statistical Office of Baden-Wuerttemberg 2013, 2014; Statistical Office of Bavaria 2013, 2014; Statistical Office of Hamburg and Schleswig-Holstein 2013, 2014; Statistical Office of Hesse 2013, 2014; Statistical Office of Mecklenburg-Vorpommern 2013, 2014; Statistical Office of Lower Saxony 2013, 2014; Statistical Office of North Rhine-Westphalia 2013, 2014; Statistical Office of Saarland 2013, 2014; Statistical Office of Saxony-Anhalt 2013, 2014). Cut-off date is March 1st

	Institution for children aged ... years	2013	2014
Baden-Wuerttemberg	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

3.2 Methodological approach

The child care reform of 2013 serves as a quasi-experiment I exploit for DiD 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 Bauernschuster et al. (2016), Felfe et al. (2015) and Havnes and Mogstad (2011), districts are split at the fourth and sixth decile of the increase in the child care coverage rate for up to 3-year-olds. Hence, treatment definition includes not a change from having no to having child care, but a change from a lower to a higher 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 the attendance of a child care institution, it is not possible to relate the resulting estimates to actual child care take up. Moreover, De Chaisemartin and D'Haultfoeuille (2017) show that such Wald-DiD estimator is only identified under restrictive assumptions. In any case, the resulting estimates clearly state the sign of the reform's impact.

Alternatively, the implementation of the reform using a cut-off date would allow for a regression discontinuity design. A major advantage of DiD estimation, however, is the possibility to take seasonal effects into account which is especially relevant in the given application. Early child care and kindergarten attendance often cannot start at any point in time, but follows the beginning of the school year in August or September. As older children have better chances for a child care slot, mothers with children born shortly before the cut-off date are more likely to take up a job when the school year starts. Empirical studies evaluating German family policies like parental benefit reforms prefer DiD estimation for the same reason albeit the presence of a cut-off date such that cohort effects can be ruled out (Cygan-Rehm 2016; Cygan-Rehm et al. 2018; Schönberg and Ludsteck 2014; Zimmert and Zimmert 2020).

As the reform took place in August 2013, the pre-reform period is measured in 2011. Although the expansion of subsidized child care has started earlier, the largest increase in child care slots can be observed in the year the legal claim came into force (BMFSFJ 2015) which additionally supports the use of the chosen survey years in contrast to previous years. 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 sensitivity analysis will provide similar results for the year 2014 as post-reform period. The treatment group comprises mothers whose youngest child is up to 3 years old and who live in a district in which the coverage rate increased by more than the sixth decile (8.0 percentage points) between 2011 and 2015.[3] Mothers of children up to 3 years old living in districts with a lower increase of the coverage rate than the fourth decile (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 317 districts.[4] Furthermore, it might be interesting to learn about middle- to long-term effects of the reform, i.e., evaluating mothers of 3-year-olds and older in 2015. These children might have already attended child care in 2013 or 2014. However, in this article the focus is on short-term effects of mothers with younger children.

Figure 2 shows how the child care coverage rates evolve in control and treated districts. Although the share of institutionally cared for children is higher in low-expansion districts, the lines are almost parallel until the

[3] Hence, the pre-(post-)reform period includes mothers with children born between February 2008 (2012) and December 2011 (2015).

[4] Fig. 6 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. The approach of Abadie (2005), used for the empirical analysis, also suggests multilevel treatments. Only for seven out of the overall 396 districts, the growth is close, but not exactly zero. Only for three districts, it is even negative. Given these findings and for the sake of simplicity, I focus on the binary treatment case.

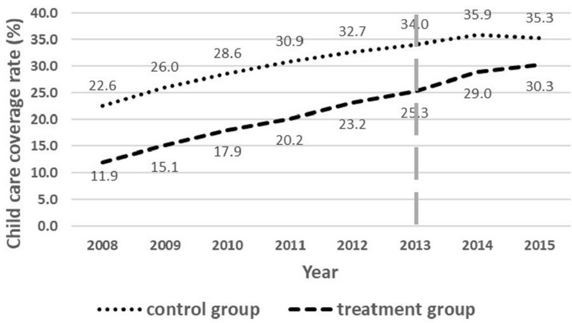

Fig. 2 Child care coverage rates (%) in control and treated districts. *Notes*: The child care coverage rate measures the number of children up to 3 years old in subsidized care in relation to all children in the respective birth cohort. The vertical line represents the reform year in 2013. *Source*: Own calculations based on numbers from the Federal Statistical Office (2008, 2009, 2010b, 2011b, 2012b, 2013b); Federal Statistical (2014b); Federal Statistical Office (2015a)

reform has become effective in August 2013. From 2014 onwards the difference gets smaller for the first time. This development is likely related to a catch-up effect in high-expansion districts while in the low-expansion districts it might be less necessary to expand child care. In related studies (Bauernschuster et al. 2016; Felfe et al. 2015; Havnes and Mogstad 2011), the child care coverage rate is on similar levels close to zero in the treatment and control group before the reform. In this setting, child care is already established in both groups, but on a lower level in districts with a higher expansion up to the year 2015.

The regional differences, which I use to define the treatment and control group, can be seen in Fig. 3. It depicts descriptive statistics of the child care coverage rate on the district level in 2011 and 2015 as well as its growth between these years. It 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. When comparing the development of these districts between 2011 and 2015, one can observe the largest changes in the western part of the country, especially in the federal states North Rhine-Westphalia and Lower Saxony as well as in Baden-Wuerttemberg close to the French border.

Moreover, Table 3 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.

3.2.1 *Average effects*

The idea of the DiD estimator is to compare average outcomes of a group affected by a reform with unaffected individuals before and after the treatment becomes effective. Under the assumptions of parallel trends of control and treated group in the absence of the reform, no anticipation effects, the stable unit treatment value assumption (SUTVA) and common support, the average treatment effect on the treated (ATET) can be identified. The assumptions are discussed in the following.

Assumption 1 Parallel trends

$$\mathbb{E}\left[Y^0(1)|D=1,X\right]-\mathbb{E}\left[Y^0(0)|D=1,X\right]$$
$$=\mathbb{E}\left[Y^0(1)|D=0,X\right]-\mathbb{E}\left[Y^0(0)|D=0,X\right]$$

where $Y^0(t)$ denotes the potential outcome in the absence of the treatment at time $T=t$ where $T=0$ is the pre-reform period (2011) and $T=1$ the post-reform period (2015). $Y^1(t)$ is its counterpart under the reform. D is the binary treatment status and X depicts some covariates. Controlling for a large set of covariates makes the assumption of parallel trends more likely. I include covariates concerning the mother herself, the household she lives in and also regional dummies to control for the economic background (compare Sect. 3.3 where a full list is given). Beyond that, I will run a placebo test by setting the timing of the reform to 2011. While this kind of sensitivity analysis cannot directly test the common trend assumption, it may give suggestive evidence that it is not violated. Related to that, one can see in Fig. 4 how the unconditional means of the outcome variables evolve for the treatment and control group between 2010 and 2015. If there were diverging trends for these groups, the common trend assumption might be hard to defend. However, the means of the employment share and of the agreed and preferred working hours develop very similar from 2010 onwards until the reform year 2013. For full- and part-time employment the movement is very similar for the years shortly before the reform (2011 and 2012).

Related articles (Bauernschuster et al. 2016; Müller and Wrohlich 2020) evaluate earlier periods of the child care expansion. While this might be a valid approach in their applications, a major parental leave reform in 2007 (Kluve and Schmitz 2018) could be a potential threat to the common trend assumption if both groups are affected differently.

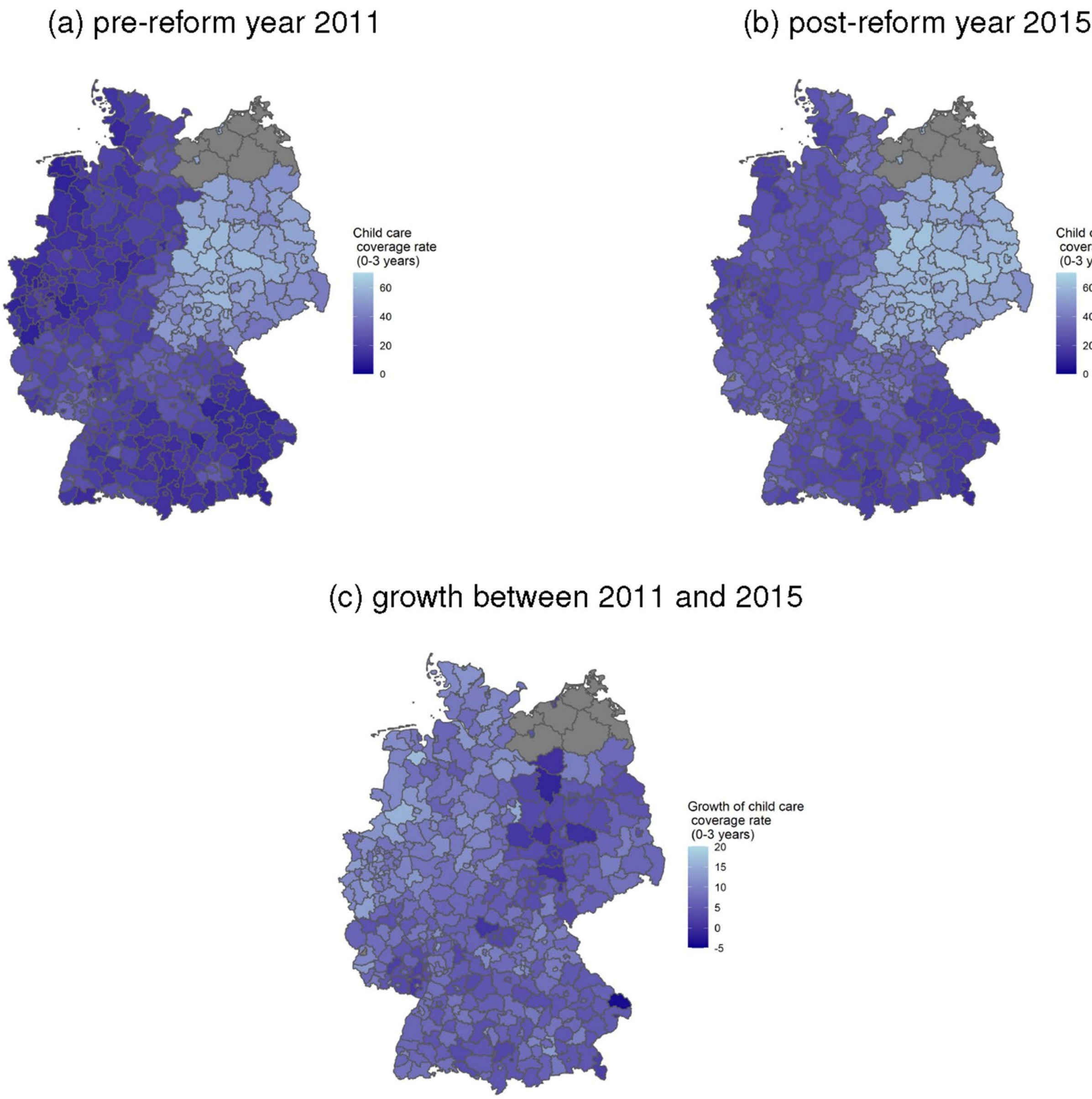

Fig. 3 Child care coverage rates for under 3-year-olds across districts. *Notes*: The child care coverage rate measures the number of children up to 3 years old in subsidized care in relation to all children in the respective birth cohort. Gray colored districts underwent a territorial reform and cannot be included in these maps. *Source*: Author's illustrations with data from the Federal Statistical Office (2011b, 2015a). Cut-off date is March 1st

Assumption 2 Absence of anticipation

$$\mathbb{E}\left[Y^1(0) - Y^0(0)|D = 1, X\right] = 0$$

As the reform was already announced in 2008, anticipation might be relevant in two different forms. Mothers might have tried to postpone firstly, the date of conception or secondly, the date of birth to be eligible for the new regulations (births from August 2012 onwards). Figure 5 depicts official birth numbers from the relevant cohort 2012 in comparison with the cohorts 2010, 2011, 2013 and 2014 and it does not show an irregular rise from August 2012 onwards. The figure rather suggests that the development in the second half of 2012 is part of a general upward trend of births numbers. Hence, selection into treatment in the form of anticipation should play a minor role. Conditioning on X further weakens the assumption, as one can control e.g. for the educational background of the mother that might be a relevant indicator describing the potential selection. Additionally, I

Table 3 Number of districts by group membership and federal states. *Source*: Own calculations based on numbers of the Federal Statistical Office (2011b, 2015a) from 317 districts

Federal state	Control group	Treatment group
West Germany		
Baden-Wuerttemberg	20	11
Bavaria	50	25
Bremen	0	1
Hamburg	0	1
Hesse	7	10
Lower Saxony	6	31
North Rhine-Westphalia	1	47
Rhineland-Palatinate	21	8
Saarland	1	2
Schleswig-Holstein	0	12
East Germany		
Berlin	1	0
Brandenburg	13	3
Mecklenburg-Vorpommern	2	0
Saxony	6	3
Saxony-Anhalt	14	0
Thuringia	17	4

only use pre-reform observations from 2011 (potential births between February 2008 and December 2011). This definition makes it less plausible that mothers desiring to have a child try to postpone conception longer than half a year such that the subsample of pre-reform mothers would have been selective.

Assumption 3 SUTVA

$$Y(t) = \begin{cases} Y^0(t) & \text{if} \quad D(t) = 0 \\ Y^1(t) & \text{if} \quad D(t) = 1. \end{cases}$$

As a further assumption, SUTVA rules out interactions between groups. 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 and Riphahn 2010, 2015; Kluve and Tamm 2013; Kluve and Schmitz 2018). However, the regulations were changed in July 2015 to make part-time work during benefit receipt more attractive leading to small employment effects (Zimmert and Zimmert 2020). A robustness check will suggest that dropping mothers of less than 1-year-olds, who are affected by the reform, will turn out to be robust compared to the baseline results.

Assumption 4 Common support

$$P(D = 1|X) < 1 \text{ where } P(D = 1|X) = \mathbb{E}[D|X]$$

Common support ensures that there is no covariate perfectly predicting the probability for belonging to the treatment group. In the empirical analysis, individuals with values of the propensity score close to the minimal or maximal value are dropped following the trimming rule of Imbens and Wooldridge (2009).[5]

Under Assumptions 1–4, the average treatment effect on the treated (ATET) is identified as

$$\begin{aligned} ATET &= \mathbb{E}\left[Y^1(1) - Y^0(1)|D = 1\right] \\ &= \mathbb{E}\left[\mathbb{E}[Y^1(1) - Y^0(1)|D = 1, X]|D = 1\right] \\ &= \mathbb{E}[\mathbb{E}[Y(1) - Y(0)|D = 1, X] \\ &\quad -\mathbb{E}[Y(1) - Y(0)|D = 0, X]|D = 1] \end{aligned}$$

which implies an outcome model that is usually estimated using OLS. Alternatively, Abadie (2005) shows that the ATET is also identified as

$$ATET = \mathbb{E}\left[\frac{P(D = 1|X)}{P(D = 1)}\rho_0 Y\right] \tag{1}$$

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 implies a two-step estimation procedure for the sample analogue of the estimand $\mathbb{E}\left[\frac{P(D=1|X)}{P(D=1)}\rho_0 Y\right]$, i.e.

[5] In principle, any trimming rule dropping very high values of the propensity may be sufficient. Based on the Monte Carlo simulation of Frölich (2004), other articles show better performance of weighting estimators with good overlap in finite samples (Busso et al. 2014; Lechner and Strittmatter 2019). Hence, I rely on this more restrictive trimming rule.

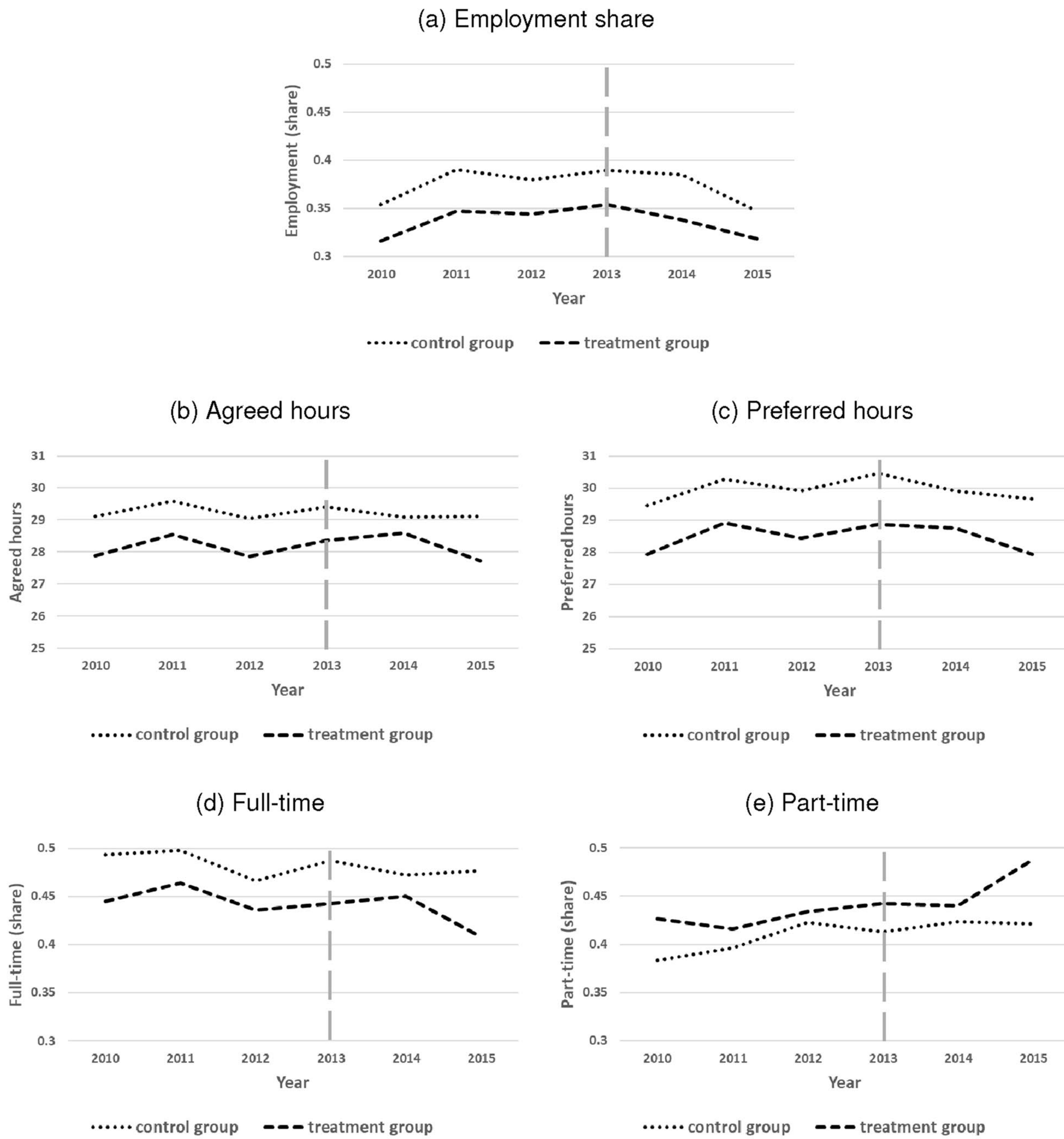

Fig. 4 Development of outcome variables. *Notes*: These means are based on the larger sample before trimming. The vertical line represents the reform year in 2013. *Source*: Own calculations based on data from the Federal Statistical Office (2011b, 2015a) and its Research Data Centre (2010, 2011, 2012, 2013, 2014, 2015)

$$\frac{1}{N}\sum_{i=1}^{N}\left[\frac{\widehat{P(D_i=1|X_i)}}{\widehat{P(D_i=1)}}\widehat{\rho_{0,i}}Y_i\right]=\frac{1}{N}\sum_{i=1}^{N}\left[\frac{\widehat{P(D_i=1|X_i)}}{\widehat{P(D_i=1)}}\frac{T_i-\hat{\lambda}}{\hat{\lambda}(1-\hat{\lambda})}\frac{D_i-\widehat{P(D_i=1|X_i)}}{\widehat{P(D_i=1|X_i)}\widehat{P(D_i=0|X_i)}}Y_i\right] \tag{2}$$

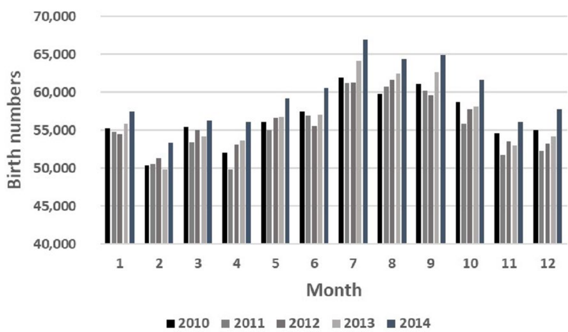

Fig. 5 Monthly birth numbers. *Source*: Own representation with numbers from the Official birth registers of the Federal Statistical Office (2010a, 2011a, 2012a, 2013a, 2014a)

for the whole sample $i = 1, \ldots, N$. In a first step, the propensity score $P(D = 1|X)$ is estimated. Abadie (2005) proposes either non-parametric or parametric estimation methods. For simplicity, this article uses logistic regression. The second step then gives the weighted non-parametric mean differences as a plug-in version of (2).

This approach has three main advantages. Firstly, it does not require a functional form assumption in the second stage and allows for flexibility which is especially useful for binary outcomes. Linear probability models usually used for parametric DiD estimation cannot satisfy the scale of such outcomes while nonlinear models based on the standard common trend assumption lead to inconsistent estimates (Lechner 2011). 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 is usually neglected in outcome based models. Finally, the specific form of the estimator allows to infer heterogeneous effects (to be discussed in the next section).

3.2.2 Heterogeneous effects

To target particular groups, policymakers are often not only interested in average effects for the whole population, but also in an policy's impact for these groups. Hence, previous studies estimate effects for specific subgroups (e.g., Cascio 2009; Havnes and Mogstad 2011)—a procedure suffering from the multiple testing problem. The issue aggravates the more hypotheses, in this case heterogeneities, are investigated. Abadie (2005) proposes a least squares approximation for the conditional effect

$$\mathbb{E}\left[Y^1(1) - Y^0(1)|D = 1, Z\right]$$

given by $g(Z;\gamma)$ where $Z \subseteq X$, i.e., Z is a subset for the heterogeneity variables of interest:

$$\gamma_0 = arg\ min_{\gamma \epsilon \Gamma} \mathbb{E}\left[P(D = 1|X)\{\rho_0 Y - g(Z;\gamma)\}^2\right]. \tag{3}$$

$\hat{\gamma}_0$ solves the weighted least squares problem for the sample analogue

$$arg\ min_{\gamma \epsilon \Gamma} \frac{1}{N} \sum_{i=1}^{N} P(\widehat{D_i = 1}|X_i)\left[\widehat{\rho_{0,i}} Y_i - Z_i'\gamma\right]^2 \tag{4}$$

and it directly indicates how the average effect varies over Z such that *joint* ordinary least squares inference is given without the necessity to correct for the number of tested hypotheses.

3.3 Data and descriptive findings

The data is from the German Microcensus,[6] a one percent representative sample of German households. The repeated cross-sections conducted by the Federal Statistical Office contain annual information on the family background, employment and other individual-specific characteristics. A main advantage of the Microcensus 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 3 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 Microcensus is the availability of individual working hour preferences on top of agreed working hours.[7] In contrast to other surveys like the German Socio-economic Panel (GSOEP) the question on working hours in the Microcensus is filtered. This 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 conditional on an earnings adjustment[8] (for

[6] For the baseline specification the analysis uses the on-site access of the Microcensus (Research Data Centre 2011, 2015).

[7] Information on the preference for an hour increase (decrease) is included since 2006 (2008). Bauernschuster et al. (2016) and Müller and Wrohlich (2020) examine earlier periods of the expansion, which is not possible for this article given the data availability.

[8] In general, the formulation of the survey question on preferred working hours differentiates between two concepts of hours constraints. Although most surveys on working hour preferences consider earnings adjustments, one has to distinguish if respondents are free to indicate their preferences or if they take other constraints like the care for children into account. Campbell and van Wanrooy (2013) suggest for further clarification that closed-ended questions on working hour preferences can be followed up by questions on the feasibility of preferences or on the constraints preventing from adjusting to the respondent's preferences. These are exactly the kind of questions the Microcensus adds to the indication on working hour

a methodological comparison of survey data on working hour preferences see Holst and 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). Apart from an earnings adjustment, respondents are not supposed to internalize any circumstances preventing them from increasing agreed hours, as follow-up questions explicitly ask for the main reason for not being able to work more hours within the next 2 weeks. In contrast to the compulsory question on the wish for an hour increase, respondents are free to answer their wish for an hour decrease. Holst and Bringmann (2016) point out that the voluntary indication might imply the underrepresentation of overemployed. The analysis includes only respondents answering the related questions, but I generally expect it to be a minor problem for the subsample of young mothers.[9]

I link the Microcensus data with statistics on the regional child care coverage rate for children aged up to 3 years old from the German Federal Statistical Office on the district level (Federal Statistical Office 2010b, 2011b, 2012b, 2013b; Federal Statistical 2014b; Federal Statistical Office 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. The final sample includes 11,640 mothers (of which 3505 are currently employed) of children not older than 3 years.

The variables used for estimating the propensity score described in the previous section and their descriptive statistics are listed in Table 4: family and individual characteristics, but also information on the interview.[10] 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 and Wooldridge 2009). Trimming makes it more likely that the common support assumption holds such that 5192 observations in the whole sample ($N = 348$ in the control group, $N = 4844$ in the treated group) and 1710 individuals of the employed sample ($N = 230$ in the control group, $N = 1480$ in the treated group) are excluded.

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 over time. Additional to mean values and standard deviations, Table 4 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 does not exceed the critical value of 0.25 defined as large suggesting that selection over time depicts a minor problem. The remaining columns show that differences between mothers in high- and low-expansion districts are not large. Not surprisingly, only regional characteristics diverge as treatment is defined upon German districts.[11]

Table 5 shows the means of the child care coverage rate and of the examined outcome variables, their standard deviations and mean differences between treated and control group before and after the reform. The average coverage rate shows that less than one quarter of children in high-expansion districts are in subsidized care before the reform came into force. More mothers in low-expansion districts use subsidized care before the reform (negative, statistically significant difference), but high-expansion districts catch up.[12]

As outcomes I examine the extensive and intensive margin, i.e., a dummy for employment, agreed and preferred working hours as well as their mismatch and a binary indicator for working full-time (more than 30 h per week) or part-time (between 12 and up to 30 h 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 code these individuals as not employed. About one third of all mothers in high-expansion districts are currently employed with an average of 25.5 h per week. They prefer to slightly work more, on average 1 h per week. However, for the majority working hour preferences and agreed hours match (13.8% of treated mothers are underemployed and only 2% overemployed[13]). About 35% of them hold a full-time position and almost one half works in part-time. The last two columns of Table 5 give the differences in means between treated and control group before and after the reform.

Footnote 8 (continued)

preferences. Thus, respondents indicating the wish for a change of working hour preferences are likely to freely choose the amount of preferred working hours.

9 In the group of high-expansion districts only two percent indicate overemployment before the reform.

10 To estimate the propensity score I use logistic regression including a constant and the here presented variables. Instead of a dummy for East Germany, I include dummies for each federal state. Detailed regression results are presented in Table 8 and the distribution of the predicted propensity score is given by treatment status in Fig. 7.

11 The analysis includes federal states instead of a dummy for East Germany to better take regional differences into account.

12 Note that these are aggregated numbers that cannot give information on actual take up of a child care slot on the individual level.

13 These are additional statistics not shown in Table 5.

Table 4 Descriptive statistics of control variables by group membership. *Source*: Own calculations based on data from the Federal Statistical Office (2011b, 2015a) and its Research Data Centre (2011, 2015)

Variable	Pre		Post		Post–Pre	Control group		Treated group		Treated-control group
	Mean	SD	Mean	SD	SD mean diff.	Mean	SD	Mean	SD	SD mean diff.
Individual age	32.297	5.638	32.395	5.048	0.018	32.270	5.283	32.427	5.426	0.029
Age of youngest child	0.986	0.812	0.969	0.810	− 0.020	0.980	0.810	0.975	0.811	− 0.007
Number of children	1.943	1.028	1.857	0.993	− 0.084	1.877	0.969	1.925	1.056	0.047
Migration background										
None	0.851	0.357	0.835	0.371	− 0.042	0.868	0.339	0.816	0.387	− 0.142
From EU country	0.041	0.198	0.057	0.231	0.074	0.041	0.199	0.057	0.231	0.072
Not from EU country	0.109	0.311	0.108	0.310	− 0.002	0.091	0.288	0.127	0.333	0.116
Quarter of interview										
1	0.250	0.433	0.240	0.427	− 0.022	0.251	0.434	0.238	0.426	− 0.032
2	0.247	0.432	0.239	0.426	− 0.020	0.243	0.429	0.244	0.429	0.003
3	0.247	0.431	0.246	0.431	− 0.002	0.244	0.429	0.250	0.433	0.015
4	0.256	0.436	0.275	0.446	0.043	0.262	0.440	0.268	0.443	0.014
Interview part										
Head of household	0.726	0.446	0.683	0.465	− 0.093	0.712	0.453	0.696	0.460	− 0.035
Self-reported	0.189	0.392	0.202	0.402	0.033	0.185	0.388	0.208	0.406	0.058
No information	0.085	0.279	0.114	0.318	0.098	0.141	0.103	0.096	0.295	− 0.023
Educational degree										
Lower secondary school	0.254	0.436	0.225	0.418	− 0.069	0.247	0.431	0.232	0.422	− 0.033
Middle secondary school	0.353	0.478	0.356	0.479	0.007	0.373	0.484	0.335	0.472	− 0.079
High school	0.393	0.488	0.419	0.493	0.053	0.381	0.486	0.433	0.495	0.106
Partner										
No partner living in household	0.171	0.377	0.124	0.330	− 0.132	0.153	0.360	0.142	0.349	− 0.031
Activity										
Inactive	0.047	0.212	0.047	0.211	− 0.002	0.042	0.201	0.052	0.221	0.045
Active	0.782	0.413	0.829	0.377	0.119	0.805	0.397	0.806	0.395	0.004
Educational degree										
Lower secondary school	0.260	0.439	0.243	0.429	− 0.040	0.263	0.440	0.239	0.426	− 0.056
Middle secondary school	0.225	0.417	0.240	0.427	0.036	0.239	0.426	0.225	0.418	− 0.031
High school	0.344	0.475	0.393	0.488	0.101	0.345	0.476	0.394	0.489	0.100
Degree of urbanization										
Urban	0.373	0.484	0.345	0.475	− 0.059	0.266	0.442	0.460	0.498	0.412
Middle	0.459	0.498	0.406	0.491	− 0.108	0.476	0.499	0.386	0.487	− 0.182
Rural	0.168	0.374	0.249	0.433	0.202	0.259	0.438	0.154	0.361	− 0.261
East Germany	0.145	0.352	0.121	0.326	− 0.070	0.185	0.389	0.076	0.265	− 0.329
N	5847		5793			6052		5588		

The sample includes 18–45 years old mothers of up to 3-year-olds. Instead of a dummy for East Germany, the analysis includes federal states. The standardized mean difference (SD mean diff.) gives the mean difference divided by the square root of the average variance (Rubin 2001)

Before the reform employment rates in high- and low-expansion 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. Only part-time jobs seem to have increased in high-expansion districts. Hence, descriptive findings suggest a positive link between the expansion of subsidized child care and the employment rate, but no or only a weak relation to the intensive margin.

4 Estimation results

4.1 Main results

Table 6 shows the baseline estimation results of the estimand in Eq. (1) for the whole sample and different sensitivity checks. Bootstrapped standard errors (in

Table 5 Mean outcomes and coverage rate by group membership. *Source*: Own calculations based on data from the Federal Statistical Office (2011b, 2015a) and its Research Data Centre (2011, 2015)

Variable	Treated group before refom			Difference in means	
	Mean	SD	*N*	Treated-control group	
				Before	After
Coverage rate %	20.16	(8.24)	158	− 10.74***	− 5.02***
Employed of which	0.348	(0.476)	2721	− 0.042***	− 0.009
Agreed hours	25.50	(13.66)	862	0.81	1.16**
Preferred hours	26.96	(13.71)	862	0.95	1.00*
Mismatch	1.46	(6.34)	862	0.14	− 0.16
Full-time	0.348	(0.477)	862	0.006	0.011
Part-time	0.470	(0.499)	862	0.021	0.042*

The sample includes 18–45 years old mothers of up to 3-year-olds. Agreed and preferred hours are measured on the weekly basis

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

parentheses) take the two-step nature of the procedure and 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 5.7 percentage points corresponding to an increase of about 16% compared to the pre-reform mean. Agreed and preferred weekly hours increase by 5.1 and 5.3 (20% of the pre-reform mean) respectively. 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. Further estimation results shown in Table 9 in the appendix demonstrate that the share of under- and overemployed mothers is not significantly affected. These findings imply that the effects on hours are not only driven by involuntarily underemployed mothers who adjust agreed to preferred working hours, but that both distributions change. They suggest (see Figs. 8 and 9 of agreed and preferred working hours in the appendix) a shift from marginal employment (categorized as up to 12 h per week) to part-time work (between 12 and up to 30 h 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 a similar movement in the group of low-expansion districts. Hence, the overall positive effect on working hours is driven by a shift from marginal to part-time employment which also shows up in an unaffected share of full-time employed.

The main findings are in general in line with previous results for Germany. Bauernschuster and 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 and 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. Müller and Wrohlich (2020) evaluate earlier periods of the child care expansion in West Germany and conclude on positive effects on maternal employment that can be explained by an increase in part-time work. Hence, these findings for Germany turn out to be robust compared to other countries with low maternal labor market participation (Baker et al. 2008; Berlinski and Galiani 2007; Berlinski et al. 2011; Lefebvre and Merrigan 2008; Nollenberger and Rodríguez-Planas 2011; Schlosser 2005). Another crucial finding concerns the adjustment of agreed and preferred working hours. Both measures change, but in contrast to Reynolds and Johnson (2012) this article finds that agreed and preferred working hours adjust on average in line with each other. Furthermore, the average effect on the share of under- and overemployed mothers is not significant. These results imply that also the size of the mismatch remains close to zero and that the results are not only 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.[14]

The remaining panels of Table 6 contain different robustness checks. Firstly, to investigate the common trend assumption 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 are close to zero (Panel B). Hence, shortly before the reform treated and control group show a similar time trend.

The next specification (Panel C) uses the median of the increase of the coverage rate for redefining the treatment and control group. The effects are similar to the results in the main specification with the clearer cut. The same holds for changing the post reform year to 2014. While similar in size, the effects for the intensive margin are

[14] One might have expected that the share of full-time employment is positively affected. However, also other analyses observe an increase of female labor market participation that is mainly driven by part-time employment (Wanger 2015).

Table 6 Results of main estimation and sensitivity analysis—*ATET*. *Source*: Own calculations based on data from the Federal Statistical Office (2011b, 2015a) and its Research Data Centre (2010, 2011, 2014, 2015)

	Employment	Agreed hours	Preferred hours	Mismatch (hours)	Full time	Part time
Panel A: Baseline						
	0.057**	5.089**	5.303**	0.213	0.063	0.126**
	(0.028)	(2.382)	(2.580)	(0.790)	(0.048)	(0.063)
N	11,640	3505	3505	3505	3505	3505
Relative effect size (compared to pre-reform mean)	0.164	0.200	0.197	0.146	0.182	0.268
Panel B: Common trend						
Placebo reform	− 0.007	− 0.687	0.127	0.814	− 0.031	0.008
	(0.032)	(2.417)	(2.006)	(0.605)	(0.048)	(0.067)
N	11,307	3638	3638	3638	3638	3638
Panel C: Sample composition						
Median division	0.069***	3.823**	4.130**	0.307	0.027	0.119**
	(0.023)	(1.929)	(2.006)	(0.403)	(0.037)	(0.050)
N	16,203	5113	5113	5113	5113	5113
post = 2014	0.057**	3.263	3.360	0.097	0.037	0.090
	(0.025)	(2.179)	(2.247)	(0.380)	(0.044)	(0.055)
N	15,919	5142	5142	5142	5142	5142
Panel D: Sample composition						
West Germany	0.066*	5.316*	6.562**	1.246	0.025	0.184**
	(0.038)	(3.027)	(3.268)	(0.865)	(0.058)	(0.080)
N	10,618	3196	3196	3196	3196	3196
Without under 1-year-olds	0.110***	6.621***	6.087**	− 0.534	0.097*	0.142**
	(0.039)	(2.460)	(2.564)	(0.647)	(0.051)	(0.068)
N	7695	3001	3001	3001	3001	3001
Without childminders	0.052*	4.503*	4.687*	0.184	0.058	0.111*
	(0.029)	(2.491)	(2.700)	(0.767)	(0.051)	(0.065)
N	11,438	3441	3441	3441	3441	3441
Without families having moved	0.066**	5.401**	6.186**	0.785	0.054	0.163**
	(0.030)	(2.521)	(2.743)	(0.791)	(0.053)	(0.068)
N	10,330	3177	3177	3177	3177	3177

The effects represent estimations of the estimand in Eq. (1). Standard errors (in columns) are bootstrapped with 1000 replications considering clusters on the district level. The sample includes 18–45 years old mothers of up to 3-year-olds. Agreed and preferred hours are measured on the weekly basis. The control variables for estimating the propensity score are listed in Table 4

*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

smaller compared to the baseline and only close to significance on conventional levels. This finding may also hint at effect dynamics, in the sense that the adjustment of working hours is time-shifted.

Other checks deviate from the baseline by changing the sample composition (Panel D). The reform demonstrates to have a similar, but stronger effect when using only West German districts.[15] Employment of mothers living in high-expansion West German districts rises by 6.6 percentage points which is mainly driven by part-time employment. Interestingly, their preferred working hours increase slightly more compared to agreed working hours. As the parameter of interest is an *ATET* and the composition of the treatment group might only slightly change, the general findings can be similar for the full and the West German sample. Thus, including East Germany in the baseline analysis should not be problematic and results mainly in a different control group.

Dropping mothers of children younger than 1 year old leads to a slightly larger effect for all outcomes. In particular, full-time employment is positively affected for mothers whose children are older than 1 year. Moreover,

[15] To estimate the effect for the West German subsample the analysis defines treatment solely based on the quantile increase of the coverage rate in these districts and does not include East Germany.

Table 7 Results of heterogeneity analysis—effect variation. *Source*: Own calculations based on data from the Federal Statistical Office (2011b, 2015a) and its Research Data Centre (2011, 2015)

	Employment	Agreed hours	Preferred hours	Mismatch (hours)	Full time	Part time
Heterogeneities						
Education (reference: lower secondary school)						
Middle secondary school	0.044	6.216	5.600	− 0.616	0.161	− 0.113
	(0.063)	(6.250)	(6.881)	(1.904)	(0.139)	(0.154)
High school	0.122*	10.457*	9.773	− 0.684	0.182	0.116
	(0.069)	(6.051)	(6.673)	(1.979)	(0.130)	(0.159)
Number of children	− 0.004	1.462	2.550	1.087	0.014	0.055
	(0.025)	(2.629)	(2.912)	(1.059)	(0.053)	(0.074)
Partner (reference: no partner living in household)						
Partner living in household	0.052	2.641	− 0.454	− 3.095*	0.027	0.104
	(0.073)	(6.840)	(7.343)	(1.834)	(0.140)	(0.160)
N	11,640	3505	3505	3505	3505	3505

The results represent estimations of γ_0 given in Eq. (3). Hence, they give the difference to the reference group for categorical variables or to a one-unit increase in case of continuous variables. Standard errors (in columns) are bootstrapped with 1000 replications considering clusters on the district level. The sample includes 18–45 years old mothers of up to 3-year-olds. Agreed and preferred hours are measured on the weekly basis. The control variable for estimating the propensity score are presented in Table 4

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

the results show that the parental leave reform of 2015 that affected mothers of less than 1-year-olds 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. The same holds when checking for selective migration by excluding those having changed their place of residence within the last 12 months.

4.2 Heterogeneous effects

Table 7 indicates how the effects vary over different subgroups as estimated in Abadie (2005). Note that the estimates give the difference to the reference group for categorical variables or to a one-unit increase in case of continuous variables. E.g., mothers with high school degree show a by twelve percentage points higher employment effect compared to mothers with a degree from lower secondary school. The impact on the intensive margin is as well higher for better educated women, but the estimates are characterized by a high variance. These findings are in line with Müller and Wrohlich (2020) and Havnes and Mogstad (2011) who also find larger effects for better educated mothers. However, this difference is weaker pronounced in the latter article, as the general reform effect also turns out to be smaller. One explanation of this result could be that external child care costs continue to be too high for mothers with lower educational degree. Besides, the opportunity costs of reducing the working hours or not working at all are in general higher for better educated mothers which may prevent them to withdraw from the labour market. This result is in line with another evaluation of a German family policy (Zimmert and Zimmert 2020).

While the average effect does not vary for the number of children, further interesting findings concern the presence of a partner. Although the estimates in general do not support deviating adjustment mechanisms for agreed and preferred working hours, cohabiting mothers show a significant higher adjustment of agreed hours compared to preferred hours. As the rate of underemployment also decreases for this subgroup, the reform was especially successful for families with a more traditional employment pattern by adapting agreed hours to the desired level. These results for cohabiting mothers are supported by a related study for Norway. Andresen and Havnes (2019) find that especially cohabiting mothers respond to child care attendance of 2-year-olds by increasing full-time employment in the context of the majority (63%) holding a part-time contract before the reform.

5 Discussion and 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 5.7 percentage points on the maternal employment rate and of 5 h on agreed and preferred weekly working hours. Besides, the share of full-time employed women does not significantly 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 7 h per day) almost doubled from 2011 to 2015 in high-expansion districts, only one out of ten children attends full-time care in post-reform years (Federal Statistical Office 2011b, 2015a). 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. Since the analysis reveals some effect dynamics, i.e., the effect on working hours is larger in 2015 compared to 2014, one might interpret this pattern as changing attitudes. With an increasing number of mothers benefiting from subsidized child care and raising their working hours, others possibly get encouraged to follow. This aspect can be part of future research and would need more detailed data on the provision of subsidized child care and/or parental attitudes.

This article also provides conditional average effects with two interesting findings. Firstly, mothers with high school degree show large positive responses in contrast to women with lower educational degree which may be explained by too high external child care costs for the latter group. Hence, a possible implication is to organize parental contributions for child care slots income-related as many communities already have implemented. Secondly, cohabiting mothers who might have previously provided additional earnings to a partner's main income show a higher adjustment of agreed than of preferred working hours which is reflected in a lower share of underemployed. This finding extends the declarations for ambiguous results in different countries and underlines the possibility for deviating adjustments of preferred and agreed working hours. The effect size can also depend on the degree to which mothers are not satisfied with their actual or agreed working hours. Hence, underlying working hour preferences are relevant to consider when assessing the potential success of a reform targeting female labor supply.

Although the reform's overall effect seem to be positive, questions remain. Especially the group of mothers with lower educational degree and singles show small responses. Hence, further research might focus on the channels that drive these results. Additionally, it might be interesting to learn about long-term effects of the child care provision. Since it turns out that mothers are more likely to return in part-time, a follow-up question could be how their working time pattern changes in the long-run.

Appendix 1: Additional results on the distribution of the child care coverage growth, propensity score and agreed and preferred working hours

See Figs. 6, 7, 8 and 9.

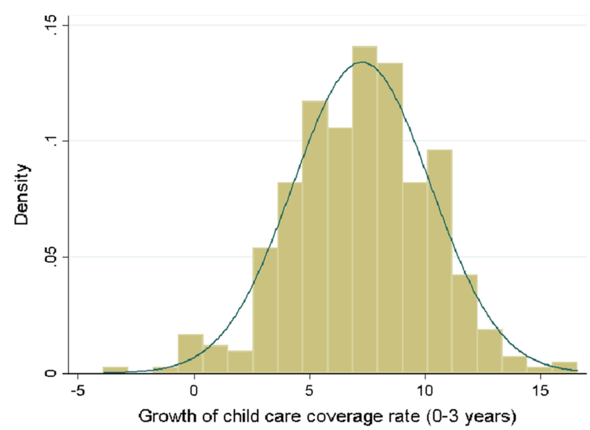

Fig. 6 Histogram of child care coverage growth from 2011 to 2015. *Notes*: Unweighted calculations based on 317 districts. *Source*: Own calculations based on data from the Federal Statistical Office (2011b, 2015a)

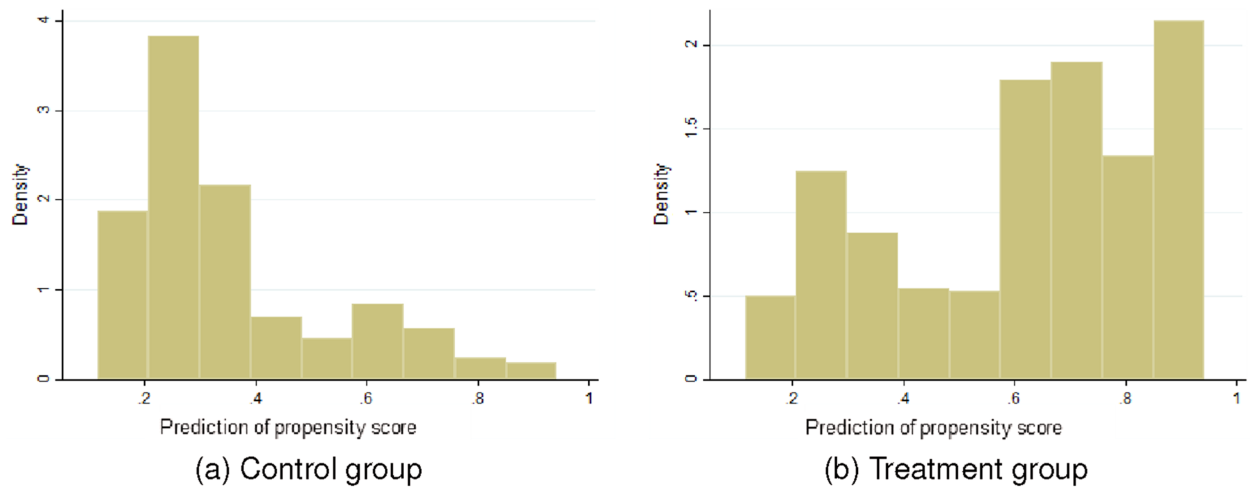

Fig. 7 Distribution of propensity score. *Notes*: The propensity score is estimated with logistic regression based on the variables given in Table 4. Detailed regression results are provided in Table 8. *Source*: Own calculations based on data from the Federal Statistical Office (2011b, 2015a) and its Research Data Centre (2011, 2015)

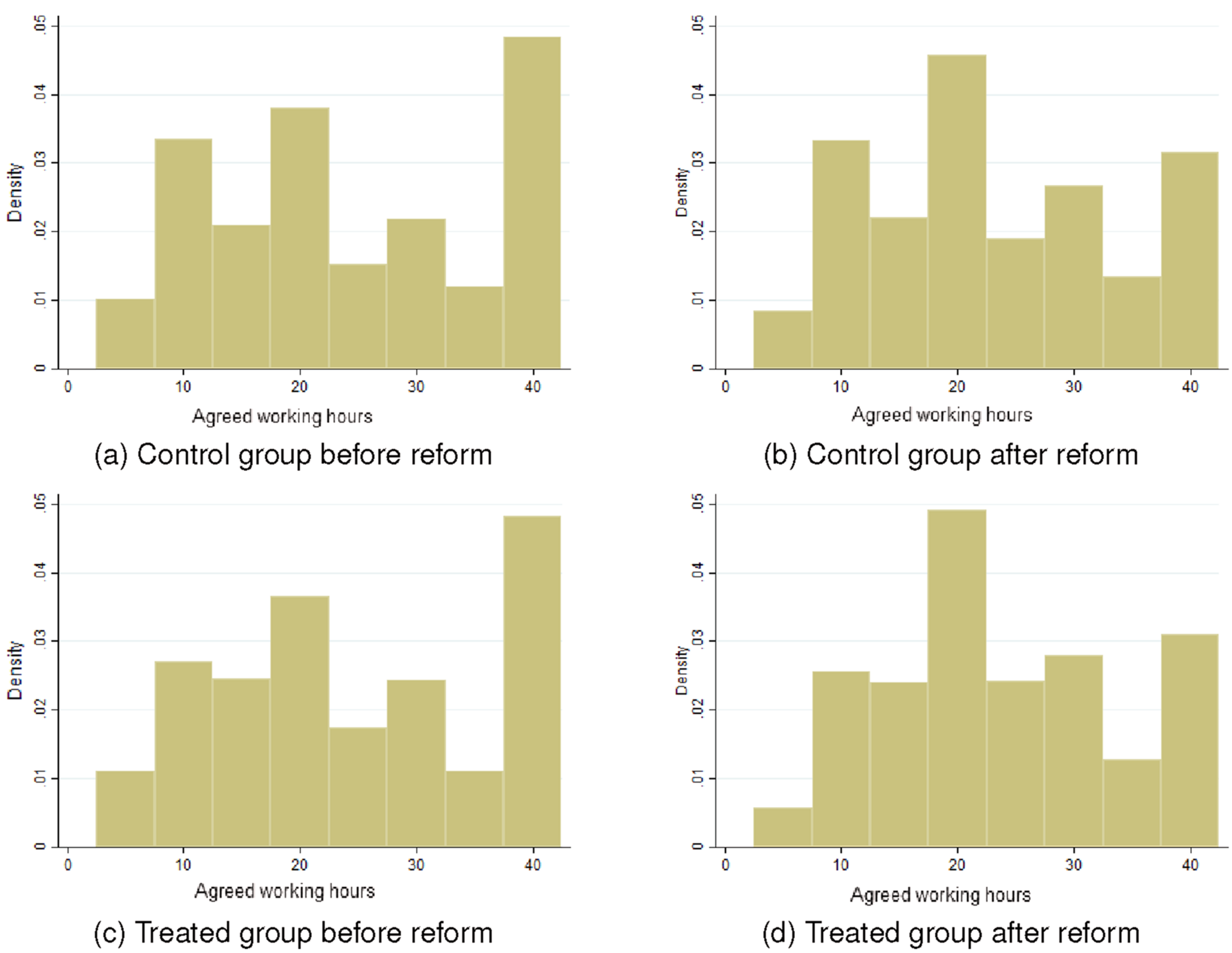

Fig. 8 Distribution of agreed working hours by group status. *Source*: Own calculations based on data from the Federal Statistical Office (2011b, 2015a) and its Research Data Centre (2011, 2015)

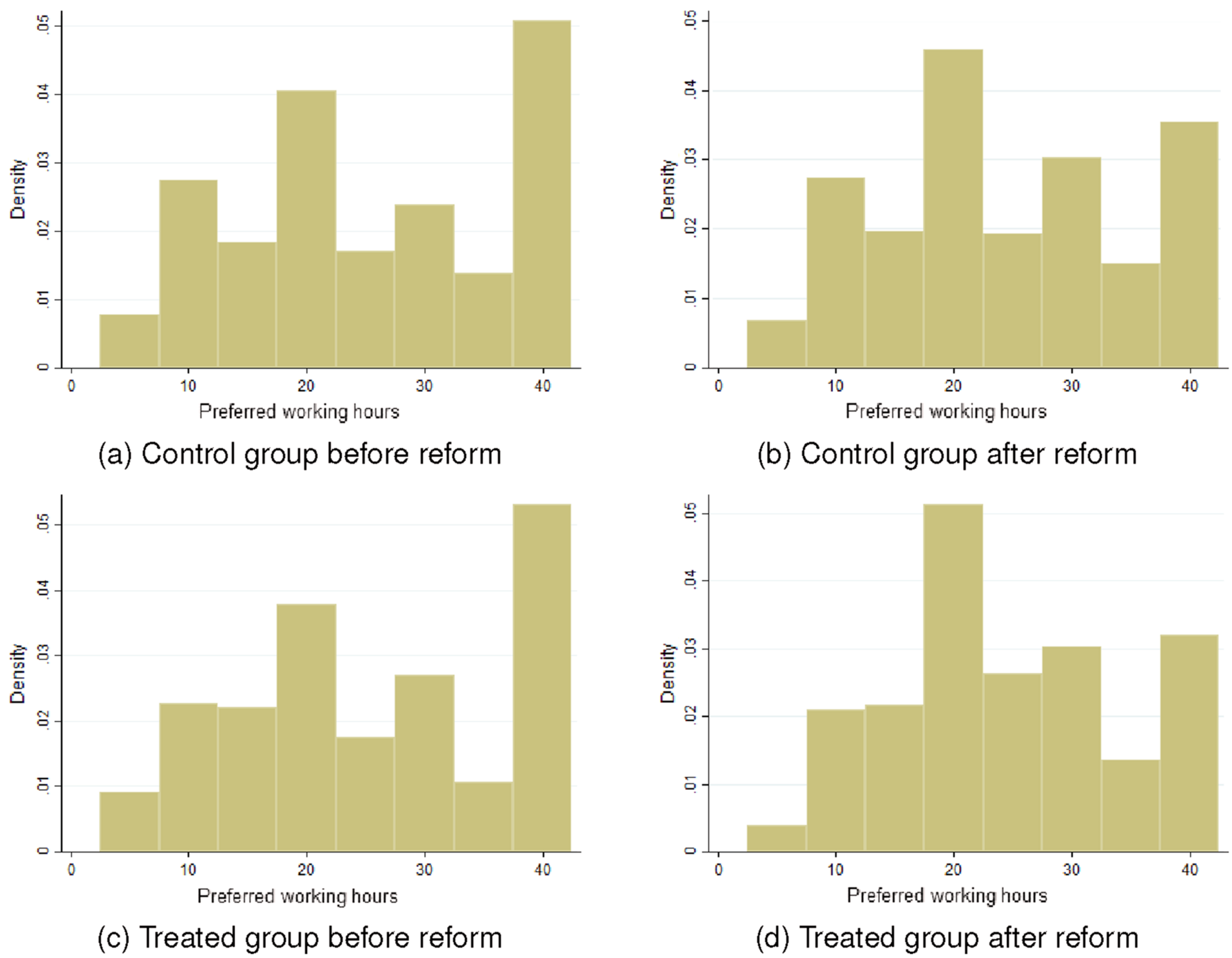

Fig. 9 Distribution of preferred working hours by group status. *Source*: Own calculations based on data from the Federal Statistical Office (2011b, 2015a) and its Research Data Centre (2011, 2015)

Appendix 2: Estimation results for the propensity score and additional outcomes

See Tables 8 and 9.

Table 8 Logistic regression results for the propensity score. *Source*: Own calculations based on data from the Federal Statistical Office (2011b, 2015a) and its Research Data Centre (2011, 2015)

Variable	Coefficient	Standard error
Individual age	0.002	0.004
Age of youngest child	0.009	0.026
Number of children	− 0.004	0.022
Migration background (ref. none)		
From EU country	0.402	0.100
Not from EU country	0.352	0.071
Quarter of interview (Ref. 1)		
2	0.045	0.060
3	0.068	0.059
4	0.083	0.059
Interview part (Ref. Self-reported)		
Head of household	0.008	0.055
No information	− 0.056	0.070
Educational degree (Ref. lower secondary school)		
Middle secondary school	− 0.157	0.144
High school	0.118	0.145
Partner (Ref. no partner living in household)		
Yes	− 0.099	0.192
Activity (Ref. inactive partner)		
Active	− 0.241	0.100
Educational degree (Ref. lower secondary school)		
Middle secondary school	0.139	0.176
High school	0.309	0.176
Degree of urbanization (Ref. urban)		
Middle	− 1.432	0.051
Rural	− 1.341	0.066
Federal states (Ref. Thuringia)		
Baden-Wuerttemberg	0.753	0.133
Bavaria	0.599	0.130
Berlin	–	–
Brandenburg	− 0.781	0.173
Bremen	–	–
Hamburg	–	–
Hesse	0.696	0.139
Lower Saxony	3.340	0.141
Mecklenburg-Vorpommern	–	–
North Rhine-Westphalia	4.423	0.162
Rhineland-Palatinate	− 0.009	0.148
Saarland	1.265	0.238
Saxony	− 0.145	0.149
Saxony-Anhalt	–	–
Schleswig-Holstein	–	–
N	16,832	
Pseudo R^2	0.368	

The sample includes 18–45 years old mothers of up to 3-year-olds. Not all federal states are included, because they perfectly predict $P(D = 1|X)$

Table 9 Estimation results of the *ATETs* for additional outcomes. *Source*: Own calculations based on data from the Federal Statistical Office (2011b, 2015a) and its Research Data Centre (2011, 2015)

	Baseline	West Germany	Without under 1-year-olds	Without childminders	Without families having moved
Underemployment (binary)					
ATET	0.024	0.097*	− 0.002	0.020	0.050
se	(0.042)	(0.051)	(0.041)	(0.041)	(0.042)
Overemployment (binary)					
ATET	0.001	0.010	0.002	2.7E−05	0.010
se	(0.012)	(0.012)	(0.013)	(0.013)	(0.011)
N	3492	3183	2991	3428	3177

The effects represent estimations of the estimand in Eq. (1). Standard errors (in columns) are bootstrapped with 1000 replications considering clusters on the district level. The sample includes 18–45 years old mothers of up to 3-year-olds. The control variable for estimating the propensity score are presented in Table 4. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

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Author contributions
FZ defined the research question and prepared and analyzed the data set. She also wrote and approved the final manuscript.

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Availability of data and materials
The article uses confidential data from the German Microcensus maintained by the Federal Statistical Office. It can be obtained after application at https://www.forschungsdatenzentrum.de/en/request.

Declarations

Competing interests
No potential competing interests was reported by the author.

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