DIW BERLIN

1622

Discussion Papers

Deutsches Institut für Wirtschaftsforschung 2016

School Entry, Afternoon Care and Mothers' Labour Supply

Ludovica Gambaro, Jan Marcus and Frauke Peter

IMPRESSUM

DIW Berlin
German Institute for Economic Research
Mohrenstr. 58
10117 Berlin

Tel. +49 (30) 897 89-0
Fax +49 (30) 897 89-200
http://www.diw.de

ISSN electronic edition 1619-4535

Papers can be downloaded free of charge from the DIW Berlin website:
http://www.diw.de/discussionpapers

Discussion Papers of DIW Berlin are indexed in RePEc and SSRN:
http://ideas.repec.org/s/diw/diwwpp.html
http://www.ssrn.com/link/DIW-Berlin-German-Inst-Econ-Res.html

School entry, afternoon care and mothers' labour supply

Ludovica Gambaro

DIW Berlin and UCL Institute of Education

Jan Marcus*

University of Hamburg and DIW Berlin

Frauke Peter

DIW Berlin

November 16, 2016

Abstract

Most literature on the relationship between childcare availability and maternal labour force participation examines childcare for preschool aged children. Yet families must continue to arrange childcare once their children enter primary school, particularly in countries where the school day ends at lunchtime. In this paper we examine the case of Germany, a country that has moved from an exclusively half-day school system to one where formal afternoon care is increasingly available. We estimate the effect of afternoon care on maternal labour supply. To do so, we use a novel matching technique, entropy balancing, and draw on the rich and longitudinal data of the German Socio-Economic Panel (SOEP). We show that children's afternoon care increases mothers' employment rate and their working hours. To confirm the robustness of our results we conduct a series of sensitivity analysis and apply a newly proposed method to assess possible bias from omitted variables. Our findings highlight how childcare availability shapes maternal employment patterns well after school entry.

Keywords: Afternoon care, Maternal labour supply, All-day schools, Entropy balancing
JEL: J13, J63, J65

*Corresponding author

Email addresses: lgambaro@diw.de (Ludovica Gambaro), Jan.Marcus@wiso.uni-hamburg.de (Jan Marcus), fpeter@diw.de (Frauke Peter)

1. Introduction

Women's labour force participation has changed dramatically in recent decades, narrowing the gender employment gap significantly. Yet women with children remain less likely to work in the labour market than other women or men, and when they do work, they tend to work fewer hours. Such lesser engagement has important consequences, from social and individual perspectives. In many advanced economies, mothers' weak attachment to the labour market leads to a systematic underutilisation of their human capital and often exacerbates an already unfavourable employment/population ratio. At the same time, mothers' economic position is affected both in the short- and long-term, as employment interruptions and lower working hours not only result in an immediate loss of earnings, but also tend to place them on a permanently lower earnings trajectory (e.g. Joshi et al., 1996; Sigle-Rushton and Waldfogel, 2007; Waldfogel, 1998).

Family policies appear to be successful in reducing the cost of motherhood. Indirect evidence comes from cross-country comparisons, with mothers maintaining stronger ties with the labour market where government support is more extensive (Gornick and Meyers, 2003; OECD, 2011). There is also persuasive evidence on the effectiveness of single family policies in specific countries, as a large number of studies examine the effect of an individual policy change on maternal employment. This body of research considers different policy reforms, including leave policies (e.g. Berger et al., 2005; Nollenberger and Rodríguez-Planas, 2015), childcare subsidies (e.g. Baker et al., 2008; Brilli et al., 2013; Lefevbre and Merrigan, 2008; Schober and Spiess, 2015), as well as public pre-school education (e.g. Berlinski and Galiani, 2007; Blau and Currie, 2006; Cascio, 2009; Fitzpatrick, 2012; Goux and Maurin, 2010). However, it focuses almost exclusively on children under compulsory school age.

Yet caring responsibilities for children do not end once they enter primary school. Although schools effectively provide what is free and universal childcare, maternal labour supply patterns continue to be influenced by the presence of children, even when they are of school age. Not only does school life impose a new set of demands on parental time, but, crucially, the problem of organising childcare outside the school day and, especially, in the afternoon remains, as a full-time working week tends not to be compatible with regular school hours (OECD, 2011; Paull, 2008). This problem is especially acute in countries where the school day has traditionally been limited to the morning only and where children typically go home at lunch time, including, for example, Austria, Chile, Germany, Mexico, and Switzerland (Allemann-Ghionda, 2009; OECD, 2011). Most of these countries are implementing reforms to increase the time school-age children spend in schools or in after-school programmes (OECD, 2015).

In this paper, we examine the case of Germany, where, since the early 2000s, policy makers have sought to support maternal employment by extending the time school children spend in formal afternoon care (Marcus et al., 2016). As a result, Germany has moved from an exclusively half-day school system to one where afternoon care is increasingly available, either because schools operate a full-day schedule or because after-school programmes, often based in school facilities, offer additional activities. The change amounts to an extension of the public school system, as schools remain the cornerstone of this expanded care provision, even when they do not provide the service themselves. Germany is an interesting case for a number of reasons. Unlike the Nordic countries or France, (West) Germany has long been characterised by low maternal employment: not only there is a large employment gap between mothers and childless women, but among working mothers short part-time work (less than 20 hours a week) is the dominant employment arrangement (Daly, 2000; Knittel et al., 2014; Lewis et al., 2008). Private or informal childcare arrangements are not common, as they are in Southern European countries such as Italy (Bettio and Plantenga, 2004). At the same time, Germany has witnessed a radical policy shift with the development of a number of family friendly policies aimed at easing the reconciliation between family responsibilities and employment (Bauernschuster and Schlotter, 2015; Schober and Spiess, 2015). These changes have occurred against the background of an expanding economy, which has not been hit by the great recession to the same extent as other countries. So, from a public policy perspective, Germany could be thought of as a "low hanging fruit": a context in which we would expect the availability of childcare to significantly influence maternal labour supply.

Of the considerable literature on childcare and maternal employment, studies evaluating the impact of pre-school provision one or two years before compulsory school age are most closely related to our paper. The roll-out of kindergarten programmes for five year olds in the US is a prominent example. Gelbach (2002) finds that enrolment increases labour market participation among both married and single mothers, albeit, for the latter group, only when they do not have an additional younger child. More recent results from Cascio (2009) and Fitzpatrick (2012) are mixed, as they show that kindergarten increases the probability of working among single mothers, with no effect on married mothers or mothers with an additional younger child. There are studies from other countries that also exploit reforms expanding preschool education throughout the 1990s. For example, Berlinski and Galiani (2007) show how the construction of pre-primary school facilities in Argentina helped raise enrolment among 3-5 year olds and, in turn, maternal employment. Nollenberger and Rodríguez-Planas (2015) estimate that, notwithstanding low labour demand, the fivefold expansion of universal preschool education for three year olds in Spain increased maternal employment by almost 10%. Bauernschuster and Schlotter (2015) examine the impact of the increase in kindergarten attendance by children over three in Germany. Their results indicate a large and positive effect on maternal labour force participation. In all these countries maternal employment at the time of the change in preschool availability was rather low, and, in the case of Germany, even among highly educated mothers living in strong labour markets. By contrast, in Scandinavian countries, public childcare is found to have no impact on the already high maternal labour force participation rate (e.g. Havnes and Mogstad, 2011; Kosonen, 2014; Lundin et al., 2008; Simonsen, 2010).

There are only a few studies that specifically examine the effect of reforms extending school opening hours or after-school care programmes. Evidence from Chile, where the school day was increased by two hours at the end of the 1990s indicates a positive effect on female employment (Berthelon et al., 2015). Felfe et al. (2016) examine the case of Switzerland, where the legal right to an after-school care place was introduced by different cantons in different years. They find a positive effect on full-time employment among mothers, but no effect on employment rate. Paternal employment, instead, did not appear to respond to the increase in after-school care. There is also an emerging literature on the German case, focusing on a specific federal policy programme launched in 2003 under which schools are expected to provide children with lunch and afternoon care after the regular morning instruction hours. One of the policy objectives of the programme is to increase maternal labour participation. Using a structural micro-simulation, Beblo et al. (2005) indeed show that such a programme would increase maternal labour force participation. It has however proven more challenging to estimate the impact of the policy using standard quasi-experimental evaluation methods. While there are two recent working papers (Nemitz, 2015; Shure, 2016) that attempt to exploit the staggered implementation of this programme as a source of exogenous variation, accounts of the actual roll out suggest that implementation neither was random (Wiezorek et al., 2011) nor occurred in isolation from childcare policy at local level (Autorengruppe Bildungsberichterstattung, 2016; Lange, 2016). As we explain more fully in section 2, this single federal programme contributes to the expansion of afternoon care, but in a way that reflects an intricate pattern of different local labour market conditions, political priorities and existing afternoon care services. Without data on all these other contextual factors, it is unlikely that the variations in the roll-out of this specific programme are unrelated to maternal employment.

In this paper, we take an approach that is more suitable to examine the kind of expansion in afternoon care that occurred in Germany. Drawing on the German Socio-Economic Panel (SOEP), we investigate the employment patterns of mothers the year that their child enters compulsory schooling and the year before. We examine two different outcomes: i) being in employment; and ii) actual hours worked per week, thus distinguishing between the extensive and intensive margin. We compare mothers whose child only attends school in the morning to similar mothers whose child also attends an afternoon care service. In order to make the two groups more similar, we apply a rather novel non-parametric matching estimator, entropy balancing (see Hainmueller, 2012). The matching procedure has several advantages compared to common propensity score methods and makes use of a rich set of information about the mothers, their children, and their partners to make our strategy robust against selection on observables. Further, by considering the lagged value of the outcome variable our empirical strategy takes into account selection on unobserved variables that do not change over a short period of time (such as attitudes toward work and family). We address concerns about reverse causality by exploiting a specific feature of enrolment procedures, whereby children receiving afternoon care organised under the auspices of the school can only register at the beginning of the school year. Despite the fact that we employ more than 100 conditioning variables and that these contain the mother's detailed labour market history as well as some usually unobserved variables like desired working hours and job search behaviours, omitted variables might still be present. Therefore, we apply the method proposed by Oster (2013, 2016) to assess the robustness of our results to omitted variable bias. This method exploits the fact that the bias from observed variables is informative of the bias from omitted variables, assuming that there is some kind of proportionality between the two biases.

We find that a child's participation in afternoon care during the first year of primary school increases her mother's employment. By taking into account the different employment patterns prior to school entry, we further show that a mother who did not work before is more likely to take up paid work (+11.4 percentage points), while among mothers who already worked prior their child's school entry, afternoon care leads to an increase in working hours by about 2.6 hours per week on average. There is little evidence that this increase in maternal employment crowds out paternal employment or other childcare arrangements. Our results are robust to various sets of control variables, different sample restrictions, and alternative estimations techniques. Further, the Oster (2013, 2016) method suggests that the impact of omitted variables must be substantively stronger than that of the included control variables in order to completely explain the effects of afternoon care.

The remainder of the paper is organised as follows. Section 2 briefly describes the institutional context. In Section 3 we present our empirical strategy and Section 4 provides details about the data used. Section 5 discusses our results and Section 6 comprises various robustness checks before Section 7 concludes.

2. Institutional context

Afternoon care

In Germany primary schools have traditionally been organised on a half-day basis, with lessons only taking place in the morning and children returning home for lunch. In the German Democratic Republic (GDR; East Germany) this system was progressively supplemented with after-school programmes that took place in the school building (*Schulhorte*) but organised under different auspices (Mattes, 2011). In West Germany, instead, the traditional half-day structure of the school system remained in place until the early 2000s and relied on the presence of mothers at home (Hagemann, 2006). The lack of afternoon care, with its consequences for maternal employment and possible contribution to educational inequalities, gained great political prominence only in the early 2000s and culminated in the launch of a federal government flagship programme, "The Future of Education and Care" (*Investitionsprogramm Zukunft Bildung und Betreuung or IZBB*). This policy initiative provides funds designated to support both primary and secondary schools in remaining open in the afternoon, offering after-school activities. Schools participating in the programme are now known as "all-day schools". The name is somewhat misleading, as among primary schools the large majority of "all-day schools" do not have instruction hours in the afternoon. Instead, they offer optional extra-curricular activities after normal lessons.[1] Despite IZBB being a single high profile initiative, each federal state administers the funds differently. This is in part due to German governance structure, whereby education policy is the exclusive responsibility of each individual federal state. For example, some states concentrate their funding on secondary schools, while others opt to support primary schools. Likewise, the programme is framed in

[1] Less than 5% of primary schools are all-day schools in the strict sense of operating an all-day schedule for all the children enrolled (Marcus et al., 2013).

a variety of ways, with some states targeting disadvantaged groups, while others give priority to working parents. In addition, lower level local government within each state is in charge of the actual service provision, which involves not only schools, but also youth and social services. Because of this complex implementation structure, IZBB-sponsored afternoon programmes have evolved differently across the country, reflecting each state's political priorities and district's resources. In particular, differential expectations or policy aims in relation to maternal employment may play a role in how federal funds are spent. As such, it is difficult to exploit these differences in development rates as a source of exogenous variation without data capturing how local level policies and practices mediate access to this specific federal initiative (Wiezorek et al., 2011).

Notwithstanding these variations, all-day schools across the whole of Germany have been increasing, with over 53% of all primary schools offering afternoon care in 2014 (KMK, 2016). In practice, this means that children attending these schools have the option of remaining at school, eating lunch and taking part in extracurricular activities. The activities are offered either directly by the school, albeit not by teaching staff, or by external providers operating on school premises. Afternoon programmes vary greatly in their content, with some closely linked to the morning school lessons, while others offer other types of activities, such as sport or music. Enrolment in the afternoon programme is voluntary in almost all primary schools (Marcus et al., 2013) and takes place at the beginning of the school year (Federal Ministry of Education and Research, 2016). This means that parents do not have the flexibility to adjust their children's attendance on the basis of job opportunities that may arise during the school year.

Afternoon services can also be provided independently from primary schools, resulting in a combination of primary schooling in the morning and an after-school programme in the afternoon (*Hort*). Within this type of programme, children are picked up from school, given lunch, and offered a variety of activities. This is most commonly offered in community centres, but sometimes on school premises. Like all-day schools, this type of programme can operate in very different ways, with looser or tighter links to primary schools. While the precise educational and pedagogical content of the different programmes are likely to matter to children's development, here we intentionally leave aside these aspects and focus instead on those organisational features that are most relevant to enabling mothers to work. From this perspective, there is considerable overlap between the two types of programmes. In both cases children are provided with lunch and spend their afternoon hours in a supervised environment with learning and enrichment opportunities. Afternoon activities take place four or five days a week and usually last until 3:00 or 4:00 pm, depending on the programme (Holtappels et al., 2008), providing a considerable extension of childcare coverage relative to morning only school attendance.

It is perhaps not surprising that policy makers see these two types of programmes as either substitute or essentially equal. Thus, with the role out of the IZBB programme some Western states (such as Berlin, Hamburg, North Rhine-Westphalia) have substituted *Hort* programmes with all-day schools, while other states have fostered the increase of both *Hort* programmes and all-day schools. In the remainder of this paper, we use the terms "afternoon care" to refer to the two formal services described above. When we need to distinguish between specific types of programme, we refer to afternoon care under the auspice of the school as all-day schools (*Ganztagsschule*) and to all other formal afternoon care as after-school programmes (*Hort*). We exclude other forms of care that may be provided by friends or relatives as well as other privately arranged out-of-school activities.

Maternal labour supply

In Germany, the employment rates of mothers differ greatly between West and East. In particular, in West Germany, where in 2012 82% of all mothers with dependent children lived, their labour force participation has historically been low. However, maternal labour supply is growing considerably, increasing from 59% in 2000 to 66% in 2012 among mothers with children younger than 18 (Knittel et al., 2014). Between 2006 and 2012 increases were particularly pronounced among mothers with children around school entry age: the employment rates of mothers with children aged 4-6 and 6-8 rose by 8.3 and 8.8 percentage points, respectively (Knittel et al., 2014). Although mothers are increasingly in employment, they mainly work part-time, with more than three-quarters of employed mothers working less than 32 hours a week in 2012. This pattern does not vary substantially with the age of the youngest child. In 2012, 74% of mothers with children aged between 6 and 8 worked less than 32 hours a week and the same percentage did so among mothers with children aged between 4 and 6 (Knittel et al., 2014). Mothers increase their hours as their children grow, but compulsory school entry does not mark an abrupt increase in maternal labour market engagement. An increase in full-time work among mothers appears once children are 10 or older and, thus, more capable of self-care. The start of compulsory schooling at age 6 may even lead to a reduction in maternal employment or working hours in areas where the increasing availability of full time early childhood education and care places has not been matched by an extension of childcare coverage for primary school children. For example, in West Germany while early childhood education and care centres have traditionally only been open in the morning, in 2015 39% of children aged three to six had a full day place (Autorengruppe Bildungsberichterstattung, 2016). Overall working part-time appears to be the way German mothers reconcile paid work with caring responsibilities for young children, yet Wunder and Heineck (2013) show that a substantial share of mothers (26%) would like to work longer hours.

Within such a context, afternoon care can be a powerful policy lever to support maternal employment. First, it can help mothers enter the labour market, giving them greater flexibility to opt for jobs that do not closely match half-day schools' opening hours. Second, afternoon care can help mothers already in employment extend their working hours, either within a part-time working arrangement or by moving to full-time. We test these hypotheses in what follows.

3. Empirical strategy

Our empirical strategy seeks to identify the impact of afternoon care for primary school children on maternal labour force participation. We investigate the role of both all-day schools (*Ganztagsschule*) and after-school programmes organised by other providers (*Hort*). Both types provide subsidized public afternoon care and, in practice, cannot always be distinguished from one another (Lange, 2015). Our identification combines value-added modelling and matching. The longitudinal nature of our data allows the value-added approach, while matching is performed by a rather novel matching procedure, entropy balancing. The general idea of the estimation strategy is straightforward. We examine mothers' employment patterns before and after their child enters school and compare those whose child receives afternoon care (treatment group) to those whose child only attends school in the morning (control group). In order to make the control group children similar to the treatment group, we apply the non-parametric entropy balancing (EB).[2] This technique reweights the observations in the control group in such a way that they have the same mean and variance for all included variables as the treatment group.[3] We opt for entropy balancing over the more conventional propensity methods for a number of reasons. First, EB is more effective at reducing the imbalance between treatment and control group characteristics and, unlike propensity score methods, never

[2] We use the user-written programme "ebalance" (Hainmueller and Xu, 2013) in Stata to implement entropy balancing.

[3] Out of the many possible weighting schemes that fulfil these balancing conditions with only non-negative weights, entropy balancing selects the weighting scheme in which the weights deviate as little as possible from uniform weights - where distance is measured by the eponymous entropy divergence (Kullback, 1959).

produces a worse balance. Second, while the covariate balance is only checked for the means in most propensity score applications, EB allows for balancing both the mean and variance of each individual variable, thus further enhancing the balance between the two groups. Third, EB is fully non-parametric and does not rely on functional form assumptions necessary for the propensity score equation. Fourth, entropy balancing spares the burdensome iterations of propensity score methods between estimating the propensity score, checking for covariate balance and readjusting the propensity score model to achieve a better balance.

However, as in all propensity score methods, entropy balancing requires the inclusion of all variables that simultaneously affect the probability of children's participation in afternoon care and maternal employment (conditional independence assumption, CIA). Because we work with longitudinal data and observe mothers both the year their child starts school and the year before, we can take several steps to make it more likely for the CIA to hold. We start by including the mothers' employment status when the child is below compulsory school age; this is the "value-added" component of our model. In particular, we specify whether mothers work at all, whether they are employed full-time, and the number of actual hours they work. We include the same information on their labour force participation from *two* years before the child's school entry. This way we compare mothers with similar employment trajectories. We also pay particular attention to work plans and motivation, including information that is often unobserved in other datasets, such as the number of desired working hours, job search behaviour, and working intentions for those not employed. In addition, we include information on children's childcare attendance the year before school entry. We distinguish between institutional care and informal care, thus capturing preferences for centre-based care and whether relatives are available. Family socio-economic and demographic characteristics, such as education levels, income, and family composition, are also accounted for by a large set of variables. Finally, we add regional indicators to capture disparities in economic conditions. In short, our matching strategy relies on an extensive set of observables that go a long way to capture work and childcare preferences to the fullest extent possible.

Our set of control variables consists of factors that might affect the treatment and factors that might affect the outcome. Note that the conditional independence assumption states that the estimator is biased only if a variable that is related to both treatment and outcome is not included. Therefore, a failure to control for any variable in the two sets of factors only results in a biased estimator if the omitted variable should be included in both sets of factors. The matching procedure makes our strategy robust against selection on observables. Further, by considering the lagged dependent variable, our empirical strategy also takes into account selection on unobservable characteristics that are likely to remain stable over time, such as work-family attitudes. We include all control variables, not only in the entropy balancing step, but also in the regression equation. This makes the estimator double-robust (Bang and Robins, 2005) and also increases the precision of the estimates as the control variables reduce the unexplained variance in the outcome. Hence, we estimate the average treatment effect on the treated (ATT) using the following equation:

$$ATT = \sum_{k \in T} \left[(Y_{1k} - X_k' \hat{\beta}) - \sum_{l \in C} W_{k,l}(X)(Y_{0l} - X_l' \hat{\beta}) \right], \tag{1}$$

where Y_{1k} and Y_{0l} denote the observed outcomes of individuals in the treatment (T) and control group (C), respectively. X depicts the vector of control variables including the lagged outcome, while $W_{k,l}(X)$ refers to the weights from entropy balancing and, hence, depends on X. $\hat{\beta}$ denotes the vector of estimated coefficients from the weighted regression of Y on all control variables. Eq. (1) shows that the control variables are used both for entropy balancing and regression-adjustment.[4] All reported standard errors are robust to heteroscedasticity and clustered at the mother level.

Two main threats to our identification strategy however remain: omitted variables and reverse causality. While our identification strategy includes several features against these two threats, in the robustness section we apply the method developed by Oster (2016) to assess robustness to bias from those unobserved variables that may have a time-varying impact on maternal employment and for which we cannot control. We further address the issue of reverse causality by exploiting a specific feature related to the timing of maternal employment choices and all-day schools enrolment.

4. Data

The analyses in this study are based on the German Socio-Economic Panel (SOEP). The SOEP is an annual nationwide random panel survey of German households, carried out since 1984 (see Wagner et al., 2007). Currently, it covers more than 30,000 individuals in approximately 17,000 households. SOEP has several advantages for the present analysis. First, it is among the few nationally representative datasets in Germany that

[4] As the treatment indicator is orthogonal to the control variables after entropy balancing (treatment and weighted control group have the same means in all control variables), the inclusion of control variables in the regression step does not change the estimated treatment effect but rather its precision.

include information on participation in afternoon care services. Second, the longitudinal design allows comparing maternal labour force participation before and after their child's school start. Third, the dataset is especially rich, with detailed information on children and their childcare usage, on parents and their employment histories, as well as on the entire household. Fourth, SOEP comprises information that is usually unobserved in other data, such as job search behaviour and the intention to work for non-working individuals. Finally, the data include the date of the interview and detailed calendar information on individual labour force statuses, which helps to mitigate concerns that mothers' decisions to work precede the choice of enrolling their children in afternoon care services.

4.1. Sample selection

Our focus in on change in mothers' employment patterns between two time points: when their child is in her first year of primary school (t_1) and the year before (t_0). We use information on mothers whose child entered primary school between 1999 and 2013. Mothers can appear more than once in our sample if they have several children who enter primary school during those years. All presented standard errors are clustered at the level of the mother and, hence, take multiple observations into account. Moreover, the robustness section shows that our results are robust to only using one observation per mother. The 1999-2013 time window is chosen because the 1999 school cohort is the first one to have fully benefited from the 1997 legal right to a subsidised kindergarten place since the age of three (see Bauernschuster and Schlotter, 2015).[5] There are also data reasons: some of our control variables regarding preschool are only regularly surveyed from 1999 onwards. Similarly, we can only include mothers whose child enters school in 2013 or before, because information on more recent cohorts is not yet available. In the robustness section, we experiment with a shorter time period.

In order to get closer to an ideal experimental situation, we only consider respondents who were interviewed between January and July in both years (t_0 and t_1). This ensures that the employment patterns observed at t_0 and t_1, respectively, precedes and follow decisions around enrolment in an afternoon care service, which takes place at the start of the school year in August/September. As the vast majority of interviews in SOEP takes place in the first half of the year, these two sample restrictions reduce the sample size by

[5]Although starting to observe children from 1999 onward precedes the expansion of afternoon care, we still observe children in both treatment and control groups between 1999 and 2003. Between 1999 and 2003 few children receive afternoon care; this increases from 2003 onwards. Therefore, we also conduct a sensitivity analysis and only look at mothers and children observed from 2003 onward. The results remain very similar.

only 6.2%.[6] We only examine school starters for two reasons. First, there is a high degree of persistence in the decision to participate in all-day schooling (Steiner, 2011). Hence, there is little variation in a child's treatment status over time. Second, as the school entry date is rather exogenous to the individual family, we argue that reverse causality issues are much more a concern for children who change their treatment status while they are already in school.

We omit individuals with missing information on the key variables (maternal labour force status in t_0 and/or t_1, child's afternoon care status in t_1). This reduces the sample size by 4%. However, we include individuals with missing values in control variables by using for each variable with missing values a separate missing-value dummy. In a robustness test, we show that disregarding these observations does not change our conclusions. As a final sample restriction, we only look at children who turn 5, 6, or 7 in the year of their school entry. This restriction reduces the sample by 1.2% and is imposed in order to reduce measurement error in school entry. Again, the results are robust to including these cases as well.

4.2. Treatment and control group

Our control group includes mothers with children who are only in school in the morning and who do not receive formal afternoon care. The treatment group includes mothers whose child is (i) in an all-day school until mid or late afternoon, or (ii) attends primary school and a separate after-school programme (*Hort*). As mentioned earlier, the two forms of provision cannot be easily distinguished, as all-day schools typically offer afternoon activities run by staff external to the school, thus closely resembling afternoon programmes provided by social services and non-profit organisations, either on school premises or in other care centres. In addition, before 2009 the SOEP questionnaire does not allow for differentiating between the two types of afternoon programmes.[7] While this is problematic for studies trying to separate all-day schools and after-school programmes, it does not hamper our strategy. From the point of view of maternal employment, the precise administrative structure of the afternoon care is not relevant; what matters is whether the child attends a formal afternoon service, which is what we capture. In 2009 the relevant SOEP questions changed to unambiguously classify students according to

[6] The results are also robust to using cases with an interview between August and December; see section 6.

[7] Before 2009 the question reads "Which of the following institutions do [your] children currently attend?" and lists both primary school and after-school programme as category leaving parents the possibility to give multiple answers.

the type of afternoon care they participate. We use this information and, as a robustness check, we exclude children attending *Hort* from the analysis.

In summary, our sample consists of mothers whose child attends day care in t_0 and primary school in t_1. Mothers are assigned to the treatment group if in t_1 their child usually receives afternoon care, either defined as attendance at primary school the whole day or attending primary school and a separate after-school programme; the remaining mothers in the sample constitute the control group, whose children only attend primary school in the morning. Our sample consists of 4,254 mother-child pairs: 1,278 in the treatment group and 2,976 in the control group.

4.3. Outcome variables

Two indicators describe our maternal employment outcomes, one relates to the extensive margin and one to the intensive margin. The first outcome variable is binary and indicates whether a mother works in t_1, while the second outcome indicates the number of actual hours a mother works per week in t_1.

4.4. Conditioning variables

We make use of a broad range of control variables in our analyses. All originate from the interview in t_0 and, hence, describe the situation before their child enters primary school. We include control variables that are likely to be related to both maternal labour force participation in t_1 and child's attendance in afternoon services (see also the discussion in Section 3). These conditioning variables describe mothers' labour market history, their education, and their demographic characteristics. For their labour market history, we do not just rely on information about their work status or weekly hours of work in t_0, but also on the same variables from two years before the child's school entry and retrospective information on the number of years in full-time and part-time employment. In addition, the SOEP, unlike many other general surveys, elicits rich information on work preferences and plans. For mothers who are in employment at t_0, we include the number of desired working hours, while, for mothers who are not in work at t_0, we include variables on job search behaviour and working intentions.[8]

[8]We include binary variables for each answer (see Table A.1 in the appendix) to the questions "Do you intend to engage in paid employment (again) in the future?", "When, approximately, would you like to start with paid employment?", "Are you interested in full-time or part-time employment, or would both suit you?", "Is it or would it be easy, difficult or almost impossible to find an appropriate position?", "Could you start working within the next two weeks?", and "Have you actively looked for work within the last four weeks?".

In addition to information about mothers, we also use information regarding their child, their partner, and the household. Child characteristics included are age, the presence of younger siblings, and enrolment in childcare in t_0. The characteristics of maternal partners include labour market attachment and education as well as some demographics. Household information relates to income, rural/urban classification of the place of residence, federal state, as well as unemployment and GDP rates at the state level. Additionally, we include indicators for each survey year as well as for the subsamples of SOEP.

Table 1 shows the means of selected conditioning variables in t_0 for mothers in the treatment and control groups, respectively (Table A.1 in the appendix provides a full list of control variables). This table compares the means of the treatment group (column 1) with the means of the unmatched control group (column 2). The similarity between treatment and control groups is shown by the mean differences (column 3) - the difference in the mean between treatment and control groups. In addition, Table 1 comprises information of the means of the matched control group, i.e. re-weighted by entropy balancing (column 4), and the similarity between treatment and control groups is shown by the standardized bias (columns 5 and 6).

[Table 1 about here]

The descriptive comparison between treatment and control groups shown in Table 1 suggests that maternal labour supply differs already in t_0 between treatment and control group. Mothers whose children receives afternoon care are 18 percentage points more likely to have worked in t_0 and work about 11 hours more a week in t_0, i.e. prior school entry of their child. Table 1 also points to substantial differences with respect to other characteristics between mothers whose child receives afternoon care and those whose child does not. For instance, mothers with children in afternoon care services are much more likely to live without a partner, to have a highest secondary school degree, and to have more full-time work experience. Moreover, their children are more likely to have attended day care for longer hours.

With regards to the standardized difference for the unmatched control group (see column 5 in Table 1), for many variables it exceeds the value of 20 in absolute terms, which is considered to be a large difference (Rosenbaum and Rubin, 1985). But Table 1 also shows that after re-weighting with the weights from entropy balancing, the matched control group has the same mean as the treatment group in all variables (see column 4) and a standardized bias of zero (column 6).[9]

5. Results

Table 2 provides an initial snapshot of the differential changes in employment patterns between mothers whose child is enrolled in afternoon services and those whose child is in school only in the morning. Such differential change is most visible when mothers are grouped according to their employment pattern the year before their child enters school (t_0). We start by looking at mothers who are not in paid work in t_0. We then divide this group according to their children's afternoon services attendance in t_1 and notice that only 65% of those whose child is cared for in the afternoon are not in work, as opposed to 79% of those whose child is not. And while both groups of mothers are more likely to work part-time than full-time, 7% of those in the treatment group work full-time, while only 1.6% do so among those whose child attends no afternoon care.

[Table 2 about here]

When looking at mothers who are already in paid work the year before school starts, differences between those using afternoon care and those who do not are less stark and relate to working hours rather than employment status. For example, among those who work part-time before their child enters primary school 8% shift to full-time work if their child receives afternoon care, as opposed to only 3.8% in the control group. It is also notable that among mothers who are working full-time before their child is in school, 17% scale back to part-time if their child is in half-day school, as opposed to only 10% of those whose child attends an afternoon care service. Overall, mothers, whose child attends school for a full day or combined with an after-school programme, are more likely to start working and to work longer hours than mothers whose child is only attending school for half a day. This difference is visible for all groups of mothers irrespective of the employment status before the child's entry in school. In the following we will continue to differentiate the results according to mothers' t_0 working status.

[9] In Table A.1 we show that propensity score matching also works well in reducing the differences between treatment and control groups. None of the standardized biases is larger than 20 % after propensity score matching; although several values are greater than 5%, which is considered to be a threshold for low values (see Caliendo and Kopeinig, 2008). However, the standardized bias is clearly smaller for the entropy balancing specification than for the propensity score specification. For some variables, such as child's gender, the standardized bias in the propensity score specification is even larger than in the unweighted control group.

The regression results in the first column of Table 3 confirm these conditional correlations for all mothers (Panel A), as well as for mothers who did not work in t_0 (Panel B) and those who worked in t_0 (Panel C). This column is based on our baseline regression, which controls only for the value of the outcome variable when the child was in kindergarten (i.e. the t_0 working status and actual working hours, respectively), alongside time and state fixed effects (capturing general differences over time and between states). This specification can be seen as a "value-added" model. These conditional correlations do not imply causality, as mothers in treatment and control group have different socio-economic and demographic characteristics that might underlie the differential changes in the outcome. For instance, mothers in the treatment group are, on average, better educated (see Table 1) and this labour market advantage might make them more likely to start working or to increase their working hours (even conditional on their previous labour market status). Hence, the conditional correlations presented would be (upward-) biased estimates of the true causal effects.

We take into account differences in the observed characteristics in the second column of Table 3. This specification is based on the regression-adjusted matching procedure with entropy balancing outlined in Section 3. Column (2) suggests that the child's participation in afternoon care increases maternal employment and maternal working hours, irrespective of the mother's t_0 working status. The coefficients in this specification are of similar magnitude as in the regression without control variables in column (1). Column (3) further exploits the richness of our data by including a set of variables on working preferences and intentions, elicited at t_0. This way we are comparing mothers who not only have similar characteristics and behave similarly, but who also express similar preferences in relation to work. For mothers who did not work prior child's entry to school, child's afternoon care increases the probability of taking up paid work by 11.4 percentage points. Among mothers who are already in work during the preschool year, the effect of the child being in afternoon care is less pronounced on the mother's decision to work or not, but still significant (+5.4 percentage points). Further, these mothers take advantage of afternoon care to increase their weekly working time by 2.6 hours on average.

[Table 3 about here]

The main message from these results is that a child's participation in afternoon care services seems to affect maternal labour supply, both on the extensive and intensive margin. Among women who are not employed during their child's preschool years, the effect appears to be on the extensive margin – these mothers are more likely to take up paid work if their child attends afternoon care. On the other hand, mothers who are already in employment before their child starts school appear able to extend their working hours and maintain employed.

However, it remains a question of whether afternoon care is replacing other forms of non-formal childcare, thus truly affecting maternal employment but crowding out unpaid childcare by relatives and friends or paid childcare by childminders. We explore this hypothesis using information about other types of childcare used. We use the same specifications as for maternal employment shown in Table 3 . Only this time our outcomes are a series of binary indicators measuring whether other forms of non-formal childcare are used in the first year of school (t_1). We find very little evidence of any substitution between afternoon care services and other forms of non-formal childcare (Table 4). Interestingly, Table 4 shows an increase in the reliance on friends among mothers following the child's participation in afternoon care services. This might reflect that opening hours of afternoon care services do not completely account for a full working day.

[Table 4 about here]

As a last step, we examine the effect of afternoon care on paternal employment. As fathers have increased their involvement in childcare over time it is possible that the effects of extended school days ripple into their working pattern. We find no evidence of this, thus confirming the in-elasticity of paternal labour supply to childcare (Table 5).[10]

[Table 5 about here]

6. Robustness Checks

In this section, we provide additional evidence for the robustness of our main results. We start by investigating the two main sources of bias that could potentially undermine our identification strategy: reverse causality (section 6.1) and omitted variable bias (section 6.2). At the end of this section we test the robustness of our results to applying different sample restrictions and estimation techniques (section 6.3).

[10]We also looked for treatment effect heterogeneity, but did not find evidence that the treatment effect differs significantly according to specific characteristics of the mother or the child. However, as the sample sizes are rather small, heterogeneity analysis and power issues might arise. Results are available upon request.

6.1. Reverse causality

Reverse causality is an issue if the child is receiving afternoon care *because* the mother increases her labour force participation. We focus on school starters in our main specification in order to mitigate concerns of reverse causality as the school entry date is rather exogenous to the individual family. To further address this threat to our identification strategy, this section makes use of a specific feature of all-day schools: Parents must decide whether or not their child participates in the afternoon programme at the start of a school year.[11] Hence, reverse causality is less of an issue in the case of mothers who find a job after the beginning of the school year because they can only enrol their child to all-day schooling in the next school year.

Columns (2) and (3) of Table 6 present the results relating to these reverse causality considerations. In a first step, we demonstrate that the estimated effect sizes are rather similar if the treatment group consists exclusively of children who attend all-day schools (model 2). This model omits children who attend an after-school programme, *Hort*, as the aforementioned institutional particularity mainly relates to all-day schools. In a second step, we exclude all mothers from the sample of model (2) who started, quit, or changed their job after the interview in t_0 and before the school start in September of that year. This way we drop those mothers for whom the temporal ordering of events is clearly employment change first and all day school enrolment after.[12] The estimated effects in model (3) are very similar to the effects in our main specification and in model (2), suggesting that reverse causality does not drive our results.

[Table 6 about here]

6.2. Omitted variable bias

To identify the effect of afternoon care on maternal labour supply the models must include all variables affecting both afternoon care attendance and changes in mothers' labour market participation. While, so far, all the control variables originate from t_0, specific events might occur between t_0 and t_1, which might affect both mothers' working patterns and changes in children's participation in afternoon care services and, therefore,

[11] This rule applies to "open all-day schools" only. These are those where participation to afternoon activities is voluntary and they constitute the large majority of all-day schools among primary schools (Marcus et al., 2013). In the 5% of schools, where participation in all-day schooling is compulsory for all the pupils, parents do not have the choice option at the beginning of the school year.

[12] This specification is not our preferred one as the restriction might be overly conservative: mothers know their child's treatment status before school start and can, hence, adapt their employment pattern in anticipation of the treatment.

constitute omitted variables. Hence, in model (4) of Table 6 we try to control for such events, which include the birth of a child between t_0 and t_1, the absence of a partner in t_1, and the partner's involuntary job loss between t_0 and t_1. We also consider the existence of various alternative childcare arrangements in t_1. We do not include the t_1 variables in our main specification as they could also be affected by the treatment, and, hence, be bad control variables. It is reassuring to see that the inclusion of these additional controls does not alter the results.

Nevertheless, we cannot be sure that we included all relevant control variables. To assess how big the influence of potentially omitted variables must be in order to completely explain the obtained effects of afternoon care, we apply the method proposed by Oster (2013, 2016). This method builds on the idea that the bias from observed variables is informative regarding the bias from omitted variables (under the assumption that there is some kind of proportionality between the forms of bias). Oster (2016) elaborates on the approach suggested by Altonji et al. (2005) and the often applied procedure of looking at coefficient stability after the inclusion of control variables. The main contribution of her method is to take into account the explanatory power (and thereby the relevance) of the included control variables: The method relates the change in the estimated treatment effect (due to the inclusion of control variables) to the associated change in the R^2. More formally, Oster (2016) approximates a bias-adjusted treatment effect, β^*, by

$$\beta^* \approx \tilde{\beta} - \tilde{\delta}\left[\dot{\beta} - \tilde{\beta}\right] \frac{(R^2_{max} - \tilde{R}^2)}{(\tilde{R}^2 - \dot{R}^2)}, \tag{2}$$

where $\dot{\beta}$ and $\dot{R}^2$ are the estimated treatment effect and coefficient of determination from a baseline regression without additional control variables and $\tilde{\beta}$ and $\tilde{R}$ are their equivalents from the full regression with additional control variables. While all these four quantities can be estimated from the data, we need to make some assumptions regarding $\tilde{\delta}$ and R^2_{max}. $\tilde{\delta}$ is assumed to be positive and denotes the degree of proportionality. It indicates how much of the variation in the outcome is explained by the observed controls versus unobserved. $\tilde{\delta} = 1$ means that we assume an equal importance of observed and unobserved factors ("equal selection assumption"), while $\tilde{\delta} > 1$ [$\tilde{\delta} < 1$] indicates that the degree of selection on unobserved variables necessary to explain away the effects is stronger [weaker] than selection on the observables. R^2_{max} denotes the share of variation in the outcome variable that is explained by observed and unobserved variables together. It is less than one if there is measurement error in the dependent variable. Oster (2013) derived a value for R^2_{max} based on empirical reasoning and suggests $R^2_{max} = min\{2.2 \cdot \tilde{R}^2, 1\}$.[13]

Based on Eq. (2), Oster (2016) suggests two closely related approaches to evaluate the robustness to omitted variable bias. We follow the suggestions and report in Table 7 (i) a lower bound of the treatment effect assuming equal selection on observed and unobserved variables (i.e. we set $\tilde{\delta} = 1$); and (ii) the degree of proportionality for which our treatment effect would equal zero (i.e. we set $\beta^* = 0$). While the first approach checks whether the lower bound is still larger than 0 and included in the 95% confidence interval of the previously estimated treatment effect, the second approach examines whether $\tilde{\delta} > 1$, i.e. if selection on unobserved variables has to be more important than selection on observables in order to pull the estimated effect of afternoon care to zero. This would be the case if the unobserved variables are more important than the whole set of included control variables, which we selected drawing on the literature on maternal employment and childcare.

The first two columns in Table 7 basically repeat the estimated treatment effects from the baseline and the main specification (see also Table 3), while the other columns display the results of the two approaches outlined above. Panel A shows that in the baseline model, afternoon care increases the probability of working by 15.4 percentage points for mothers who are not working in t_0. The inclusion of the full set of control variables in our main model leads to a slight decrease in the treatment effect estimated to 11.4 percentage points but to an increase in R^2 from 0.044 to 0.38. Based on these estimates and Eq. (2), we calculate that $\tilde{\delta}$ would have to be as large as 1.91 in order to completely explain the estimated effect.[14] This means that the influence of omitted variables needs to be almost twice as important as of the observed factors included in the model to bring the effect of afternoon care to zero. The estimated lower bound of the treatment effect is 0.058. It is larger than 0 and included in the 95% confidence interval around the estimated treatment effect. For mothers who worked in t_0 (Panel B), our estimates of $|\delta|$ are also both greater than 1 (and the values for $|\delta|$ even exceed that of Panel A). Further, the lower bounds are clearly larger than 0 and included in the respective confidence bands, suggesting that it is very unlikely that our findings are explained by omitted variable bias.[15]

[13]Note that in the published article, Oster (2016) suggests a value of $R^2_{max} = min\{1.3 \cdot \tilde{R}^2, 1\}$. Nevertheless, we rely on the working paper version, which is more conservative as it generally generates higher values of R^2_{max}.

[14]We use the Stata command *psacalc* provided by Oster (2013) to calculate the estimates of δ and the lower bound.

[15]Note that the $\tilde{\delta}$ found for the outcome "working" in Panel B is negative as the treatment effect moves away from zero rather than toward zero when including control variables. The negative value of $\tilde{\delta}$ implies

Thus, the method developed by Oster (2016) corroborates our findings.

[Table 7 about here]

6.3. Alternative sample restrictions and estimation methods

The next set of specifications addresses different sample restrictions (see Table 8). Model (1) disregards all observations with missing information on the control variables due to item non-response (see section 4.4). Specification (2) extends the sample by lifting the restrictions on the month of the interview and the age of the child (see section 4.1), while model (3) restricts the sample to a shorter observation period, the years 2003-2013. 2003 is the year in which a federal investment programme was launched to foster the expansion of all-day schools. In model (4) we only include one observation for each mother, namely the observation that refers to the mother's first child to enter primary school in our observation period. Table 8 shows that our findings are robust to the different sample restrictions.

In the last set of sensitivity checks, we assess a number of issues regarding our estimation method. Model (5) presents the results obtained from Ordinary Least Squares estimation with the same set of control variables, while column (6) displays the estimates from propensity score matching.[16] Specification (7) performs entropy balancing separately according to the mother's working status in t_0. Finally, as there is an ongoing discussion about whether one should apply survey weights in matching applications or not (Solon et al., 2015), we re-estimate our main specification using survey weights in both the entropy balancing and the regression step (see column 8). Our findings are robust to all these sensitivity checks.

[Table 8 about here]

7. Conclusion

In this paper we examine how maternal labour supply changes due to her child's participation in afternoon care. A vast literature in economics provides evidence on the effect of early childhood care services provision on maternal employment, yet little

that unobserved factors would have to be 34 times stronger correlated than observed factors but with reversed sign. In this case, the value for the lower bound is calculated based on $\tilde{\delta} = -1$. Also in this case the lower bound is clearly within the 95% confidence band around $\tilde{\beta}$.

[16] For the propensity score matching, we rely on kernel matching with a Gaussian kernel and a bandwidth of 0.06 (see Heckman et al., 1997; Marcus, 2014).

is known about effects on maternal employment of subsidized childcare for school-aged children. One might argue that the problem of arranging for suitable childcare clearly becomes less pressing once children reach school age. Yet the problem does not disappear, as primary school children are not capable of self-care and full-time working hours rarely match the school day hours. Such mismatch is especially visible in countries with half-day schools, whereby children are home at lunch time. We extend the existing literature by focusing on primary school children and examine how hours spend in formal afternoon care affect maternal labour supply patterns. We consider not only labour market participation, but also the actual hours worked, thus covering variations both at the extensive and intensive margin.

Our empirical strategy combines value-added modelling and matching, in which the treatment group consists of mothers whose children participate in formal afternoon care (i.e. in all-day schooling and/or after-school programmes). We use a non-parametric matching technique, entropy balancing, to generate a control group of mothers with similar characteristics whose children do not participate in afternoon care. The matching procedure considers a wide range of control variables, including the mother's detailed labour market history, the child's attendance in preschool, several characteristics of the partner, as well as household, regional labour market characteristics and some often unobserved information like job search intentions and desired working hours. Our identification strategy is robust against selection on observables as well as against selection on unobserved variables with time-constant effects. We assume that conditional on the mother's labour force status before the child's school entry, there are no other variables other than the included control variables that simultaneously affect the child's participation in afternoon care and the mother's labour force status when the child is in first grade. We evaluate the robustness of our results to bias resulting from potentially omitted variables applying the method developed by Oster (2013, 2016).

Across the whole sample, we find that the child's being at school in the afternoon increases the mother's probability to start working, to remain working, and to increase the number of hours they work as their child enters school. Splitting the sample according to mothers' work status prior to their child's school entry shows that the child's participation in afternoon care increases the likelihood of mothers who did not work before to take up paid work by 11.4 percentage points. Furthermore, mothers who already worked during the year prior to their child's school enrolment increase their working hours by an average of 2.6 hours per week due to their child's participation in afternoon care services. This is in line with studies on childcare availability for children below compulsory school age in Germany (Bauernschuster and Schlotter, 2013) and other countries (e.g. Berlinski and Galiani, 2007) as well as with the few studies on school-aged children (Berthelon et al., 2015; Felfe et al., 2016). We do not find any effects on paternal labour force participation, underlining that mothers' labour force participation differs from fathers', as mothers' greater responsibility for children lead them to interrupt or reduce their labour force participation.

Our findings highlight that childcare availability continues to shape maternal employment patterns well after school entry. While so far the focus of researchers and policy-makers alike has mainly been on pre-school children, our analysis highlights that the need for childcare does not end when the child enters school. Policy-makers intending to foster maternal labour force participation should improve childcare opportunities not only for pre-school children but also for young school-aged ones.

Acknowledgements

We gratefully acknowledge funding and support by the College for Interdisciplinary Educational Research (CIDER). Moreover, we thank C. Katharina Spieß, Janina Nemitz, seminar participants at the University of Chicago and DIW Berlin as well as participants of the GEBF 2016, and the annual conference of the ESPE 2016 for valuable comments.

References

Allemann-Ghionda, C. (2009). Ganztagsschule im europäischen Vergleich. Zeitpolitiken modernisieren - Durch Vergleich Standards setzen? *Zeitschrift für Pädagogik*, Beiheft 54:190–208.

Altonji, J. G., Elder, T. E., and Taber, C. R. (2005). Selection on observed and unobserved variables: Assessing the effectiveness of catholic schools. *Journal of Political Economy*, 113:151–184.

Autorengruppe Bildungsberichterstattung (2016). Bildung in Deutschland 2016. Ein indikatorengestützter Bericht mit einer Analyse zu Bildung und Migration. W. Bertelsmann Verlag, Bielefeld.

Baker, M., Gruber, J., and Milligan, K. (2008). Universal child care, maternal labour supply, and family well-being. *Journal of Political Economy*, 116(4):709–745.

Bang, H. and Robins, J. (2005). Doubly robust estimation in missing data and causal inference models. *Biometrics*, 61(4):962–973.

Bauernschuster, S. and Schlotter, M. (2013). Public child care and mothers' labor supply - evidence from two quasi-experiments. *CESifo Working Paper Series 4191*.

Bauernschuster, S. and Schlotter, M. (2015). Public child care and mothers' labor supply - evidence from two quasi-experiments. *Journal of Public Economics*, 123(C):1–16.

Beblo, M., Lauer, C., and Wrohlich, K. (2005). Ganztagsschulen und Erwerbsbeteiligung von Müttern: Eine Mikrosimulationsstudie für Deutschland. *Zeitschrift für ArbeitsmarktForschung - Journal for Labour Market Research*, 38(2):357–372.

Berger, L. M., Hill, J., and Waldfolgel, J. (2005). Maternity leave, early maternal employment and child health and development in the US. *The Economic Journal*, 115(501):F29–F47.

Berlinski, S. and Galiani, S. (2007). The effect of a large expansion of pre-primary school facilities on preschool attendance and maternal employment. *Labour Economics*, 14(3):665–680.

Berthelon, M., Kruger, D., and Oyarzun, M. (2015). The effects of longer school days on mothers' labor force participation. *IZA Discussion Paper*, 9212.

Bettio, F. and Plantenga, J. (2004). Comparing care regimes in Europe. *Feminist Economics*, 10(1):85–113.

Blau, D. M. and Currie, J. (2006). Preschool, day care, and after-school care: Who's minding the kids? In Welch, F. and Hanushek, E. A., editors, *The Handbook of Economics of Education*, pages 1163–1267. North-Holland, New York.

Brilli, Y., Del Boca, D., and Pronzato, C. (2013). Does child care availability play a role in maternal employment and children's development? Evidence from Italy. *Review of Economics of the Household*, pages 1–25.

Caliendo, M. and Kopeinig, S. (2008). Some practical guidance for the implementation of propensity score matching. *Journal of Economic Surveys*, 22(1):31–72.

Cascio, E. (2009). Maternal labor supply and the introduction of kindergartens into American public schools. *Journal of Human Resources*, 44(1):140–170.

Daly, M. (2000). *The gender division of welfare: The impact of the British and German welfare states*. Cambridge University Press, Cambridge.

Federal Ministry of Education and Research (2016). Was ist der Unterschied zwischen offenen und gebundenen Ganztagsschulen? http://www.ganztagsschulen.org/archiv/188_306.php. Accessed on 10/07/16.

Felfe, C., Lechner, M., and Thiemann, P. (2016). After-school care and parents' labour supply. *Labour Economics*, 42(3):64–75.

Fitzpatrick, M. (2012). Revising our thinking about the relationship between maternal labor supply and preschool. *Journal of Human Resources*, 47(3):583–612.

Gelbach, J. (2002). Public schooling for young children and maternal labor supply. *American Economic Review*, 92(1):307–322.

Gornick, J. C. and Meyers, M. K. (2003). *Families that work: Policies for reconciling parenthood and employment.* Russell Sage Foundation.

Goux, D. and Maurin, E. (2010). Public school availability for two-year olds and mothers' labour supply. *Labour Economics*, 17(6):951–962.

Hagemann, K. (2006). Between ideology and economy: The time politics of child care and public education in the two Germanys. *Social Politics: International Studies in Gender, State & Society*, 13(2):217–260.

Hainmueller, J. (2012). Entropy balancing: A multivariate reweighting method to produce balanced samples in observational studies. *Political Analysis*, 20(1):25–46.

Hainmueller, J. and Xu, Y. (2013). Ebalance: A stata package for entropy balancing. *Journal of Statistical Software*, 54(7):1–18.

Havnes, T. and Mogstad, M. (2011). No child left behind: Subsidized child care and children's long-run outcomes. *American Economic Journal: Economic Policy*, 3(May 2011):97–129.

Heckman, J., Ichimura, H., and Todd, P. (1997). Matching as an econometric evaluation estimator: Evidence from evaluating a job training programme. *Review of Economic Studies*, 64(4):605–654.

Holtappels, H. G., Klieme, E., Rauschenbach, T., and Stecher, L., editors (2008). *Ganztagsschule in Deutschland: Ergebnisse der Ausgangserhebung der "Studie zur Entwicklung von Ganztagsschulen (StEG)"*. Weinheim: Juventa Verlag.

Joshi, H., Macran, S., and Dex, S. (1996). Employment after childbearing and women's subsequent labour force participation: Evidence from the British 1958 birth cohort. *Journal of Population Economics*, 9(3):325–348.

KMK (2016). Allgemeinbildende Schulen in Ganztagsform in den Ländern in der Bundesrepublik Deutschland - Statistik 2010 bis 2014. *Sekretariat der Ständigen Konferenz der Kultusminister der Länder in der Bundesrepublik Deutschland. https://www.kmk.org/dokumentation-und-statistik/statistik/schulstatistik/allgemeinbildende-schulen-in-ganztagsform.html.*

Knittel, T., Henkel, M., Krämer, L., Lopp, R., and Schein, C. (2014). *Dossier Müttererwerbsstätigkeit: Erwerbstätigkeit, Erwerbsumfang und Erwerbsvolumen 2012*. Prognos AG, Berlin.

Kosonen, T. (2014). To work or not to work? The effect of childcare subsidies on the labour supply of parents. *The BE Journal of Economic Analysis & Policy*, 14(3):817–848.

Kullback, S. (1959). *Information theory and statistics*. New York: Dover Publications.

Lange, J. (2015). "Da war doch noch was?" Der Hort als wenig beachtete Betreuungsalternative zur Ganztagsschule im Grundschulalter. *Kommentierte Daten der Kinder- und Jugendhilfe*, 18:9–11.

Lange, J. (2016). Der Hort: viel genutzt, wenig beachtet! *DJI Impulse*, 2:21–23.

Lefevbre, P. and Merrigan, P. (2008). Child-care policy and the labor supply of mothers with young children: A natural experiment from Canada. *Journal of Labor Economics*, 26(3):519–548.

Lewis, J., Campbell, M., and Huerta, C. (2008). Patterns of paid and unpaid work in Western Europe: Gender, commodification, preferences and the implications for policy. *Journal of European Social Policy*, 18(1):21–37.

Lundin, D., Mörk, E., and Öckert, B. (2008). How far can reduced childcare prices push female labour supply? *Labour Economics*, 15(4):647–659.

Marcus, J. (2014). Does job loss make you smoke and gain weight? *Economica*, 81(324):626–648.

Marcus, J., Nemitz, J., and Spiess, C. K. (2013). Ausbau der Ganztagsschule: Kinder aus einkommensschwachen Haushalten im Westen nutzen Angebote verstärkt. *DIW Wochenbericht*, 27.

Marcus, J., Nemitz, J., and Spieß, C. K. (2016). Veränderungen in der gruppenspezifischen Nutzung von ganztägigen Schulangeboten - Längsschnittanalysen für den Primarbereich. *Zeitschrift für Erziehungswissenschaft*, 19(2):415–442.

Mattes, M. (2011). Children, families and states. Time policies of childcare, preschool and primary education in Europe. chapter Economy and politics: The time policy of the East German childcare and primary school system, pages 344–363. Oxford: Berghahn Books.

Nemitz, J. (2015). The effect of all-day primary school programs on maternal labor supply. ECON – Working Papers 213, Department of Economics, University of Zurich.

Nollenberger, N. and Rodríguez-Planas (2015). Full-time universal childcare in a context of low maternal employment: Quasi-experimental evidence from Spain. *Labour Economics*, 36:124–136.

OECD (2011). *Doing Better for Families.* OECD, Paris.

OECD (2015). *Education Policy Outlook 2015: Making Reforms Happen.* OECD, Paris.

Oster, E. (2013). Unobservable selection and coefficient stability: Theory and validation. NBER Working Paper No. 19054, NBER, Cambridge Massachusetts.

Oster, E. (2016). Unobservable selection and coefficient stability: Theory and validation. *Journal of Business Economics and Statistics*, *forthcoming.*

Paull, G. (2008). Children and women's hours of work. *The Economic Journal*, 118(526):F8–F27.

Rosenbaum, P. and Rubin, D. (1985). Constructing a control group using multivariate matched sampling methods that incorporate the propensity score. *American Statistician*, 39:33–38.

Schober, P. S. and Spiess, C. K. (2015). Local day care quality and maternal employment: Evidence from East and West Germany. *Journal of Marriage and Family*, 77(3):712–729.

Shure, N. (2016). School hours and maternal labour supply: A natural experiment from Germany. Department of Quantitative Social Science Working Paper 16-13, Institute of Education, London.

Sigle-Rushton, W. and Waldfogel, J. (2007). Motherhood and women's earnings in Anglo-American, Continental European, and Nordic countries. *Feminist Economics*, 13:55–91.

Simonsen, M. (2010). Price of high-quality daycare and female employment. *The Scandinavian Journal of Economics*, 112(3):570–594.

Solon, G., Haider, S., and Wooldridge, J. (2015). What are we weighting for? *Journal of Human Resources*, 50(2):301–316.

Steiner, C. (2011). *Ganztagsschule: Entwicklung, Qualität, Wirkungen*, chapter Teilnahme am Ganztagsbetrieb - Zeitliche Entwicklung und mögliche Selektionseffekte, pages 57–75.

Wagner, G. G., Frick, J. R., and Schupp, J. (2007). The German Socio-Economic Panel Study (SOEP) - Scope, evolution, and enhancements. *Schmollers Jahrbuch*, 127(1):139–169.

Waldfogel, J. (1998). The family gap for young women in the united states and britain: Can maternity leave make a difference? *Journal of Labor Economics*, 16(3):505–545.

Wiezorek, C., Stark, S., and Dieminger, B. (2011). "Wissen Sie, die Infrastruktur ist einfach nicht so, dass ich aus dem Vollen schöpfen kann" – Ganztagsschulentwicklung in ländlichen Räumen. *Zeitschrift für Erziehungswissenschaft*, 15:109–124.

Wunder, C. and Heineck, G. (2013). Working time preferences, hours mismatch and well-being of couples: Are there spillovers? *Labour Economics*, 24:244–252.

Table 1: Summary of selected conditioning variables for treatment and control groups

	Mean			Mean	Standard. Bias (%)	
	Afternoon care (1)	No afternoon care (unmatched) (2)	Mean diff. (3)	No afternoon care (matched) (4)	unmatched (5)	matched (6)
Maternal labour supply in t_0						
Working	0.72	0.54	0.18***	0.72	38.3	0.0
Actual working hours	22.27	11.45	10.81***	22.27	70.3	0.0
Maternal characteristics t_0						
Migration background	0.19	0.28	−0.09***	0.19	−20.2	0.0
Age mother	36.23	36.13	0.10	36.23	1.9	0.2
No spouse	0.21	0.10	0.11***	0.21	32.0	0.0
School degree						
Basic school	0.10	0.22	−0.12***	0.10	−31.6	−0.0
Intermediate school	0.40	0.39	0.01	0.40	2.9	0.0
Technical college	0.06	0.06	−0.00	0.06	−0.9	0.0
Highest secondary	0.33	0.20	0.13***	0.33	28.9	0.0
Other school	0.07	0.09	−0.02**	0.07	−8.8	0.0
School drop-out	0.02	0.02	−0.01	0.02	−4.0	0.0
Work experience						
Years part time	3.10	2.92	0.18	3.10	5.4	0.0
Years full time	6.29	5.72	0.57***	6.29	11.1	0.0
Missing LFS-experience	0.00	0.00	0.00	0.00	4.3	0.0
Child characteristics t_0						
Attendance of ECEC centre full day	0.23	0.12	0.11***	0.23	30.1	0.0
Younger siblings	0.40	0.46	−0.05***	0.40	−11.0	0.0
Older siblings	0.42	0.55	−0.13***	0.42	−26.7	0.0
Only child	0.27	0.14	0.13***	0.27	32.0	0.0
Female child	0.48	0.50	−0.02	0.48	−4.1	0.0
Type of non-formal childcare (CC)						
CC none	0.61	0.61	−0.01	0.61	−1.1	0.0
CC relatives	0.28	0.25	0.03*	0.28	6.3	0.0
CC friends	0.07	0.04	0.03***	0.07	11.2	0.0
CC paid carer	0.05	0.03	0.02***	0.05	9.9	0.0
Household characteristics t_0						
Home owner	0.41	0.56	−0.15***	0.41	−30.1	0.0
HH income (in 1000)	48.12	48.64	−0.52	48.12	−1.3	0.0
Unemployment share	9.85	7.78	2.07***	9.85	56.5	0.1
N	1,278	2,976	4,254			

Note: This table displays descriptive statistics for selected conditioning variables for treatment and control groups. The first column presents the means for mothers whose children attend afternoon care (treatment group), the second column for unmatched mothers whose children do not participate in afternoon care, and the third column comprises the mean differences between the two groups. Column four shows the mean of matched mothers in the control group, while columns five and six depict the percentage standardized bias for unmatched and matched conditioning variables. Source: SOEP v31, significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. CC=childcare, ECEC=early childhood education and care

Table 2: Transition matrix

	Not working in t_1		Part-time in t_1		Full-time in t_1		
	Afternoon care	No afternoon care	Afternoon care	No afternoon care	Afternoon care	No afternoon care	N
Not working in t_0	64.77	78.96	27.84	19.43	7.39	1.62	1711
	*-14.18****		*8.41****		*5.77****		
Part-time in t_0	8.64	11.75	83.22	84.46	8.14	3.80	1998
	*-3.11**		*-1.23*		*4.34****		
Full-time in t_0	8.28	9.33	9.82	16.89	81.90	73.78	551
	-1.05		*-7.07**		*8.12**		

Note: This table presents a transition matrix for the employment status of mothers when their child enters primary school, differentiated by treatment status. The numbers in the upper left cell indicate that 64.77 % of mothers who did not work in t_0 continue to not work in t_1 if their child is in afternoon care. The numbers shown in italics represent the percentage point differences between mothers whose child attends afternoon care and those whose children do not participate. Source: SOEP v31, significance levels (based on robust standard errors clustered at the mothers' level): * p<0.10, ** p<0.05, *** p<0.01.

Table 3: The effect of afternoon care on maternal labour supply

	Value added model (Baseline model)	**"Normal" set of t_0 controls**	**Full set of t_0 controls** (Main model)
Panel A: All mothers			
Working	0.081***	0.078***	0.075***
	(0.014)	(0.024)	(0.024)
Hours	3.212***	2.808***	2.779***
	(0.468)	(0.858)	(0.836)
N	*4,254*	*4,254*	*4,254*
Panel B: Not working in t_0			
Working	0.154***	0.128***	0.114***
	(0.032)	(0.039)	(0.037)
N	*1,711*	*1,711*	*1,711*
Panel C: Working in t_0			
Working	0.053***	0.054**	0.054**
	(0.015)	(0.025)	(0.025)
Hours	2.636***	2.554***	2.590***
	(0.561)	(0.943)	(0.940)
N	*2,543*	*2,543*	*2,543*

Note: Each cell depicts the effect of afternoon care on maternal labour supply indicators for different groups of mothers as indicated by the panel name. All regressions include state and time fixed effects. The first column comprises the association between after-school care attendance and maternal labour supply controlling for mothers' employment status prior their child's school entry (in period t_0). The second column includes a large set of conditioning variables from t_0, while the third column comprises a full set of conditioning variables. Source: SOEP v31. Robust standard errors clustered at the mothers' level in parentheses, significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 4: The effect of afternoon care on other types of childcare

	Value added model (Baseline model)	**"Normal" set of t_0 controls**	**Full set of t_0 controls** (Main model)
No child care	-0.027	0.005	0.003
	(0.017)	(0.028)	(0.028)
Child care by relative	0.011	-0.009	-0.007
	(0.015)	(0.025)	(0.025)
Child care by friend	0.032***	0.048***	0.050***
	(0.009)	(0.010)	(0.009)
Paid child care	0.005	-0.020	-0.020
	(0.010)	(0.017)	(0.017)
Child care, m.a.	0.004	0.002	0.002
	(0.005)	(0.006)	(0.006)
N	4254	4254	4254

Note: Each cell depicts the effect of a child attending afternoon care on binary indicators of other types of childcare, as indicated by the row name (see Table 3 for a description of the models). Source: SOEP v31. Robust standard errors clustered at the mothers' level in parentheses, significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table 5: The effect of afternoon care on paternal labour supply

	Value added model (Baseline model)	**"Normal" set of t_0 controls**	**Full set of t_0 controls** (Main model)
Working	-0.006	0.016	0.015
	(0.008)	(0.015)	(0.012)
Hours	-0.700	-0.044	-0.460
	(0.509)	(1.106)	(0.936)
N	2919	2919	2919

Note: Each cell depicts the effect of a child attending afternoon care on paternal labour supply (see Table 3 for a description of the models). Source: SOEP v31. Robust standard errors clustered at the mothers' level in parentheses, significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table 6: Sensitivity checks

		Identification issues		
	Main effect (1)	Only children in all-day primary schools (2)	No job change *prior* September (3)	Including information from period t_1 (4)
Panel A: Not working in t_0				
Working	0.114***	0.087**	0.107***	0.115***
	(0.037)	(0.035)	(0.036)	(0.034)
N	*1,711*	*1,513*	*1,346*	*1,346*
Panel B: Working in t_0				
Working	0.054**	0.068**	0.058*	0.061**
	(0.025)	(0.032)	(0.030)	(0.030)
Hours	2.590***	2.876***	2.699***	2.859***
	(0.940)	(1.071)	(0.971)	(0.958)
N	*2,543*	*1,938*	*1,815*	*1,815*

Note: As before, each cell depicts the effect of afternoon care participation on maternal labour supply indicators. All models are based on the main specification (repeated in the first column). The second column shows the effect of afternoon care only for children participating in all-day primary schools, i.e. excluding children attending after-school programmes (*Hort*) from the analysis. The third column only considers mothers who find a job after the beginning of the school year and column four depicts the estimate of afternoon care on maternal labour supply controlling for selected t_1 variables. Source: SOEP v31. Robust standard errors clustered at the mothers' level in parentheses, significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table 7: Assessing the potential bias of omitted variables

			Bounds of β		Proportionality	
	Baseline model	**Main model**	Lower bound	In 95%-c.i. band	δ	$\lvert\delta\rvert > 1$
Panel A: Not working in t_0						
Working	0.154***	0.114***	0.058	✓	1.914	✓
	(0.032)	(0.037)				
R^2	0.044	0.38				
Panel B: Working in t_0						
Working	0.053***	0.054**	0.051	✓	-34.073	✓
	(0.015)	(0.025)				
R^2	0.018	0.25				
Hours	2.636***	2.590***	1.967	✓	3.950	✓
	(0.561)	(0.940)				
R^2	0.48	0.52				

Note: The first and second column comprise the baseline and main effect of afternoon care on maternal labour supply, respectively. All regressions include y_{t0} as well as state and time fixed effects. The second column additionally considers a full set of conditioning variables. Based on the approach outlined in Oster (2016), the third column shows the lower bound of β and the fourth column checks whether this value is within the 95% confidence interval of the treatment effect. The fifth column reports the value of proportionality δ and shows how strong the influence of unobserved factors has to be compared to the observed to pull the treatment effect to zero (main effect). The last column checks whether $|\delta| > 1$. Source: SOEP v31. Robust standard errors clustered at the mothers' level in parentheses, significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table 8: Sensitivity checks

	Sample restriction				Estimation issues			
	w/o missing information	Full sample	2003 – 2013	One child	Ordinary Least Squares	Propensity Score Matching	Separate	Survey weights
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Not working in t_0								
Working	0.106***	0.127***	0.146***	0.119**	0.126***	0.117***	0.161***	0.163***
	(0.039)	(0.033)	(0.040)	(0.050)	(0.032)	(0.033)	(0.036)	(0.042)
N	*1,573*	*1,956*	*1,278*	*1,329*	*1,711*	*1,711*	*1,711*	*1,711*
Panel B: Working in t_0								
Working	0.053**	0.081***	0.048*	0.055**	0.045***	0.059**	0.063**	0.062**
	(0.025)	(0.026)	(0.025)	(0.026)	(0.017)	(0.024)	(0.029)	(0.029)
Hours	2.378**	3.847***	2.184**	3.168***	2.423***	2.721***	3.179***	2.428**
	(0.946)	(0.976)	(0.956)	(0.869)	(0.620)	(0.919)	(1.120)	(1.025)
N	*2,417*	*2,850*	*2,087*	*1,929*	*2,543*	*2,543*	*2,543*	*2,543*

Note: Each cell depicts the effect of afternoon care participation on maternal labour supply indicators. As before, all models are based on the main specification. The first column shows the effect of afternoon care estimated only for those mothers who have non-missing information on all variables. The second column relaxes the sample restriction regarding interview dates, while the third column only considers mothers whose children enter school from 2003 onwards. The fourth column includes only the first observed child for each mother. The fifth column comprises estimates obtained from ordinary least squares, and the sixth column from propensity score matching. Column seven performs entropy balancing separately according to the t_0 working status of mothers, while column eight includes sample weights in both entropy balancing and regression step. Source: SOEP v31. Robust standard errors clustered at the mothers' level in parentheses, significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Appendix

Table A.1: Appendix: Descriptive statistics - before and after matching

Variable	Means treated	Means controls unmatched	matched w/ EB	matched w/ PSM	Standard. Bias (%) unmatched	matched w/ EB	matched w/ PSM
Maternal characteristics in t_0							
Vocational training^{+}	64.6	68.8	64.5	67.1	−8.9	0.0	−5.3
University^{+}	29.3	15.6	29.3	22.9	33.1	0.0	14.5
Missing uni^{+}	0.5	0.9	0.5	1.6	−3.9	0.0	−10.1
Basic school^{+}	10.4	21.9	10.4	13.7	−31.6	−0.0	−10.2
Intermediate school^{+}	39.9	38.5	39.9	45.6	2.9	0.0	−11.6
Technical college^{+}	6.1	6.3	6.1	4.8	−0.9	0.0	5.5
Highest secondary^{+}	33.1	20.4	33.1	24.1	28.9	0.0	20.0
Other school^{+}	7.0	9.4	7.0	7.0	−8.8	0.0	−0.2
School dropout^{+}	1.7	2.3	1.7	2.6	−4.0	0.0	−5.7
In school^{+}	0.3	0.0	0.0	0.0	7.9	7.9	7.9
School missing^{+}	1.5	1.2	1.8	2.1	2.7	−2.6	−4.4
Migration Background^{+}	19.0	27.5	19.0	20.9	−20.2	0.0	−4.7
Age mother	36.2	36.1	36.2	35.5	1.9	0.2	12.5
Maternal employment history							
Works full-time^{+}	25.4	7.6	25.3	25.6	49.4	0.0	−0.5
Working^{+}	72.5	54.3	72.4	70.2	38.3	0.0	5.1
Actual working-hours	22.3	11.5	22.3	21.9	70.3	0.0	2.2
Desired -actual hours	−6.1	−1.8	−6.1	−4.6	−37.4	−0.0	−11.7
Missing desired^{+}	1.3	1.3	1.3	1.7	0.5	0.0	−3.3
Years part-time	3.1	2.9	3.1	2.9	5.4	0.0	6.0
Years full-time	6.3	5.7	6.3	5.8	11.1	0.0	10.3
Missing LFS-experience^{+}	0.2	0.1	0.2	0.6	4.3	0.0	−6.1
Full-time t-1^{+}	0.2	0.1	0.2	0.2	40.3	0.0	2.8
Working t-1^{+}	0.6	0.4	0.6	0.6	29.8	0.0	6.8
Missing working t-1^{+}	0.2	0.1	0.2	0.1	5.8	0.0	2.2
Working-hours t-1	17.9	9.1	17.9	16.8	56.6	0.0	6.3
Missing working-hours t-1^{+}	0.4	0.6	0.4	0.4	−30.8	0.0	−6.9
Labour market "unobservables" in t_0							
SW definitely not^{+}	2.6	8.4	2.6	3.2	−25.7	0.0	−3.5
SW improbable^{+}	1.7	6.3	1.7	2.6	−23.3	−0.2	−5.8
SW probable^{+}	6.7	15.2	6.7	7.7	−27.6	0.0	−4.2
SW definitely^{+}	15.8	15.2	15.8	16.0	1.6	0.0	−0.5
SW missing^{+}	0.8	0.6	0.8	0.4	2.1	0.0	5.5
SW asap^{+}	8.5	6.3	8.5	9.4	8.7	0.0	−3.0
SW this year^{+}	7.7	8.2	7.7	7.8	−1.8	0.0	−0.3
SW 2-5 years^{+}	6.1	15.4	6.1	7.1	−30.2	0.0	−4.1
SW 5+ years^{+}	1.7	6.4	1.7	1.9	−23.9	0.0	−1.6
SW missing time^{+}	0.2	0.5	0.2	0.1	−5.6	−0.5	1.3
SW full-time^{+}	3.8	2.0	3.8	3.6	10.4	−0.1	0.6
SW part-time^{+}	14.9	29.7	14.9	16.5	−36.2	0.0	−4.6
SW both^{+}	5.0	2.7	5.0	5.5	12.1	0.0	−2.0
SW dont know^{+}	0.5	2.3	0.5	0.7	−14.5	0.0	−1.3
FJ not applicable^{+}	75.0	60.0	75.0	72.7	32.6	0.0	5.3
FJ easy^{+}	3.3	7.1	3.3	3.1	−17.3	−0.1	1.2
FJ difficult^{+}	14.9	23.8	14.9	17.2	−22.6	0.0	−6.3
FJ almost impossible^{+}	6.5	8.5	6.5	6.7	−7.5	0.0	−0.7
FJ missing^{+}	0.2	0.6	0.2	0.3	−5.7	0.0	−0.9
Job-search yes^{+}	7.7	6.5	7.7	8.6	4.6	0.0	−3.5
Job-search no^{+}	16.5	30.0	16.5	17.7	−32.4	0.0	−3.1
Job-search missing^{+}	0.0	0.1	0.0	0.0	−5.2	−2.1	−1.7
SI yes^{+}	11.5	13.1	11.5	13.3	−5.0	0.0	−5.6
SI no^{+}	12.5	23.2	12.5	12.9	−28.0	0.0	−1.0
SI missing^{+}	0.2	0.4	0.2	0.1	−4.2	−0.5	1.3
Child characteristics in t_0							
Age child	5.7	5.7	5.7	5.7	0.5	0.3	−3.4
Younger siblings^{+}	40.5	45.9	40.4	42.4	−11.0	0.0	−4.0
Older siblings^{+}	41.9	55.1	41.9	48.2	−26.7	0.0	−12.7

Variable	Means treated	Means controls unmatched	Means controls matched w/ EB	Means controls matched w/ PSM	Standard. Bias (%) unmatched	Standard. Bias (%) matched w/ EB	Standard. Bias (%) matched w/ PSM
Only-child^{+}	27.2	14.3	27.1	21.7	32.0	0.0	12.7
Female child^{+}	47.6	49.6	47.6	48.5	−4.1	0.0	−1.9
CC none^{+}	60.7	61.3	60.7	60.2	−1.1	0.0	1.0
CC relatives^{+}	28.2	25.4	28.2	30.0	6.3	0.0	−4.1
CC friends^{+}	7.0	4.4	7.0	6.0	11.2	0.0	3.9
CC paid carer^{+}	4.5	2.7	4.5	3.1	9.9	0.0	7.3
Missing CC^{+}	4.0	9.2	4.0	4.9	−21.0	0.0	−4.4
ECEC Hours	4.7	2.0	4.7	4.6	86.2	0.0	3.5
Missing ECEC-hours^{+}	0.8	1.1	0.8	0.8	−3.7	0.0	−0.2
ECEC full-time^{+}	22.9	11.7	22.9	22.7	30.1	0.0	0.4
Partner information in t_0							
Vocational training^{+}	0.5	0.6	0.5	0.5	−18.7	0.0	−3.3
University^{+}	0.2	0.2	0.2	0.2	1.3	0.0	8.6
Missing uni^{+}	0.0	0.0	0.0	0.0	−7.1	0.0	−4.2
Basic school^{+}	28.0	15.9	28.0	24.5	29.5	−0.0	7.9
Intermediate school^{+}	11.8	24.9	11.8	10.6	−34.3	0.0	3.7
Technical college^{+}	25.4	20.5	25.4	32.6	11.8	0.0	−15.9
Highest secondary^{+}	4.1	7.1	4.1	3.8	−13.1	0.0	1.3
Other school^{+}	22.6	19.5	22.6	18.7	7.8	0.0	9.7
School dropout^{+}	6.1	9.0	6.1	7.1	−10.9	0.0	−4.0
In school^{+}	1.3	2.2	1.3	2.0	−7.0	0.0	−6.0
School missing^{+}	0.7	1.1	0.7	0.6	−4.0	0.0	1.1
Migration Background^{+}	0.2	0.2	0.2	0.2	−19.8	0.0	0.8
Age	28.1	32.9	28.1	29.0	−28.2	0.0	−4.8
Working^{+}	0.7	0.8	0.7	0.7	−27.3	0.0	−7.3
Desired -actual hours	−9.9	−9.9	−9.9	−9.4	−0.0	−0.0	−2.5
Missing desired^{+}	0.0	0.0	0.0	0.0	−4.7	0.0	−3.4
Actual working-hours	29.2	34.4	29.2	30.3	−24.0	0.0	−5.1
Missing hours^{+}	0.0	0.0	0.0	0.0	−7.1	0.0	0.6
Household characteristics in t_0							
No spouse^{+}	21.4	9.9	21.4	19.2	32.0	0.0	5.3
Home owner^{+}	40.9	55.8	40.9	43.5	−30.1	0.0	−5.2
HH income (in 1000)	48.1	48.6	48.1	44.5	−1.3	0.0	8.5
Village^{+}	7.4	8.0	7.4	11.5	−2.2	0.0	−13.9
Small town^{+}	8.2	12.2	8.2	7.7	−13.2	0.0	2.1
Medium town^{+}	22.1	31.4	22.1	23.9	−21.1	0.0	−4.1
Large town^{+}	17.5	18.4	17.5	22.5	−2.2	0.0	−12.3
Small city^{+}	7.0	9.8	7.0	7.2	−10.3	0.0	−1.0
Medium city^{+}	17.8	12.6	17.8	12.8	14.4	0.0	13.8
Large city^{+}	20.0	7.6	20.0	14.5	36.6	−0.0	14.5
State GDP/1000	233.2	299.4	233.1	210.8	−35.6	0.0	11.0
Unemployment share	9.9	7.8	9.9	10.3	56.5	0.1	−9.5
N	1,278	2,976					

Note: EB=entropy balancing; PSM=propensity score matching; SW=searching for work; FJ=finding a job; SI=starting job immediately; CC=childcare. Summary statistics for treated, all controls and matched control (indicators for state, year and SOEP sample not shown). The first two columns present the variable means before matching for treated and controls. Third and fourth column show the means for the re-weighted control group according to entropy balancing (EB) and kernel matching, a propensity score method. The last three columns display a measure for the quality of the matching process. The standardized bias is defined for each conditioning variable s as $SB_s = 100 \cdot \frac{\overline{s_1}-\overline{s_0}}{\sqrt{\frac{1}{2}(\sigma^2_{s1}+\sigma^2_{s0})}}$, where $\overline{s_1}$ and $\overline{s_0}$ are the means of treated and controls, respectively, and σ^2_{s1} and σ^2_{s0} the corresponding variances. $^{+}$ indicates that the mean represents a percentage share.