Christian Richter - Public Childcare and mothers's labor supply

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The Public childcare and mothers' labor supply - Evidence from a quasi-experiment

written by Stefan Bauernschuster and Martin Schlotter

Introduction

Increasing the labor market participation of females to 75 percent is part of the EU 2020 strategy. Multiple strategies have been used to achieve this goal but one strategy is certainly the increased availability of public childcare. But for policy makers it is relevant to know how large the effects of such an increase in public childcare attendence would be. The topic has gained further relevance in Germany since their are just are a few studies investigating the effects of an expansion of public childcare yet although it was a major topic in several state election campaigns and included in the talks between CDU/CSU and the SPD to form a new coalition for Germany. The agreement that half-time childcare should be free of charge for everyone was labeled as a lighthouse-project by SPD chairman Martin Schulz.

Whereas the effects of an expansion of public daycare was not investigated in detail for Germany, for other industrialized countries more evidence already exists. Del Boca (2015) shows that the estimates obtained for different countries vary a lot, ranging from very small effects estimated by Wrohlich (2005) for Germany going up to enormous effects observed by Viitanen (2005) for the United Kingdom. While the studies named here use some structural estimation techniques, more recent paper focus on quasi-experimental evidence. Cascio (2009) produces a paper which is representative for the existing literature using experimental or quasi-experimental methods. Using the introduction of free childcare for pre-school children as unexpected event in the US, Cascio (2009) measures strong responses of maternal labor supply on the introduction of free childcare in the early 1960ies. Other papers such as Fitzpatrick (2010) measuring the effect in the US using more recent data on the other hand observe much smaller and even insignificant effects of comparable measures. For Canada, Lefebvre & Merrigan (2008) exploit the introduction of universal childcare with increased subsidies in Quebec and use the remainder of Canada as a control group for their difference-in-difference approach. Also the Scandinavian countries with their highly developed welfare state have been subjected to some research measuring the effects of increased public daycare availability. Havenes & Mogstad (2011) observe that a reform in Norway in the 1970ies almost yields no effects on maternal labor supply. Although the research conducted so far has yielded very different results and do not point in any direction at first glance, Lundin et al. (2008) however observes some regularities: Most studies which report quantitatively high effects are conducted in a setting where female labor market participation is rather low (i.e. Cascio using data from the 1960ies) and especially countries with already high labor market participation of show almost no effects even in cases where full *and* free public childcare would be introduced (Norway, East Germany). Very few papers furthermore discuss whether such a reforms a financing themselves. A

recent paper by Bettendorf et al. (2015) estimate that the costs of the introduction of a large childcare subsidy in the Netherlands are 2.6 bn Euros, which increased labor market participation by 30000 full-time equvalents and therefore Bettendorf et al.(2015) state that the reform was rather expensive. In this context the authors also report that the Dutch government in 2015 (VVD (liberal-conservative) & PvdA (Social Democrats)) at that time planned to reduce the subsidy introduced before in order to comply withthe Maastricht criteria. Furthermore the Dutch government believed that the program was ineffective which looks reasonable recognizing the results by Bettendorf et al. (2015). It must be stated that the magnitude of the results obtained for the Netherlands are not particularly high and rank between the results of Havnes & Mogstad (2011) and Lefebvre & Merrigan (2008).

Before we go into the details of the paper at hand, we will briefly discuss the results obtained for Germany. As already mentioned above Wrohlich (2005) using a structural model reports rather small effects even if public childcare would be available for everyone. Gathmann & Sass (2012) exploit a unique setting in the federal state of Thuringia where the (conservative) state government introduced a subsidy for caring children at home and not bringing them to a childcare institution. They observe, using the other Eastern German States as control group, small but significant (negative) effects on childcare attendance by this reform. Further evidence for Germany comes from Geyer et al. (2015) which use a structural model and a quasi-expermental methods for mothers with children aged three years and below. Together with new generous parental leave regualtions the authors conclude thchildcare especially in the first year after birth maternal employment increases by almost five percentage points which the authors interpret as considerably.

The Setting of the legal claim on childcare in the 1990ies

Until the early 1990ies the Western German labor market was rather traditional with low employment rates for women, especially those with children. Same was true in West Germany for childcare attendance. Children younger than four almost never attended a kindergarden at that time. To counter the demographic developments that Germany aldready faced in the early 1990ies, one of the main goals of the federal government was to increase female labor market particiaption. In this setting the conservative-liberal government of Helmut Kohl introduced a legal claim for a subsidized public childcare spot for children aged three to six (Rechtsanspruch auf einen Kindergartenplatz) in 1995. On the 1st of January 1996 the law entered into force but the German Bundesrat (the legislative chamber where the federal states are represented) demanded some changes in order to relieve the municipalities which in the end are responsible for the creation of the respective spots. This lead to a compromise which allowed the municipalities to set cut-off rules, granting only children spots which were three years old at the beginning of every kindergarten year (which is more or less equivalent to the start of the school year). Municipalities which used the rule could do so until 1998. However this rule was not manatory and not all municipalities employed it. We will later elaborate on the details and the problems of this cut-off rule.

80 percent of the costs of public childcare are subsized in Germany therefore Bauernschuster & Schlotter argue the fee parents would have to pay for childcare can be neglected in their empirical analysis. We will later come back to this assumption and will strongly argue against the claim Bauernschuster & Schlotter are expressing here.

The identification strategy

The setting described can be easily characterized as a quasiexperimental setup created by an unanticipated policy shift. With that the paper is in line with most of the national and international literature which

already exists. The emloyed methods are both rather simple and applied straight forward.

The Instrumental Variable Approach

The cut-off described above is the key mechanism that Bauernschuster & Schlotter exploit for their Instrumental Variable (IV) approach. The authors argue that a child being above and below the cut-off is a sort of exogenous variation which influences the probabilty of attending a kindergarten significantly and influences maternal labor supply through no other channel than the attendance of the kindergarten. With this setting they follow Angrist & Krueger (1991) on their famous example to measure the effects of schooling. This means we will have a variable Z_i indicating whether the youngest child of a mother was 36 or older ($Z_i = 1$) or younger ($Z_i = 0$) when the school year started. This leads to the First-Stage-Regression

$$D_i = \alpha + \delta Z_i + \beta X_i + \epsilon_i \qquad (1)$$

In this regression D_i indicates whether the youngest child attends a kindergarten or not , Z is our cut-off dummy and X is a selection of observable characteristics which are included to control for characteristics such as maternal eduaction. ϵ is - usually - the error term. The paper now follows the standard two-stages-least squares (2-SLS) procedure to obtain the causal estimate, the authors are interested in.

$$Y_i = \eta + \tau \hat{D}_i + \varphi X_i + v_i \qquad (2)$$

Y_i is a variable expressing the labor market status of the mother (could be extensive margin, meaning a dummy indicating whether the mother works or intensive margin reporting the number of hours the mother has worked), $\hat{D}_i$ are the predicted values of D_i from the first stage regression , X is again a set of control variables and v_i is the error term. If Y_i is a dummy than (2) can be interpreted as Linear Probability Model (LPM).

Now it is necessary to shed light on what this IV approach actually would measure: It should be clear from the description of the cut-off mechanism that only a very particular group is targeted by this instrument and therefore Bauernschuster & Schlotter obtain a very specific Local Average Treatment Effect (LATE). So only complier households (meaning those who would without the claim don't bring their child to the kindergarten but do so after the introduction of the claim) are captured by the estimate of the authors. So neither households which already have their child in the childcare institution even without the claim (which are more than 55%!) nor the parents that would leave their kids at home are captured by the IV approach of this paper. Furthermore the cut-off mechanism is imprecise in various regards. The authors have no idea which municipalities actually applied the cut-off rule until when and furthermore other municipalities were not forced to apply it, so technically the authors can only observe the effects for municipalities they cannot name.

The Difference-in-Differnece Approach

To further validatethe results from the IV approach, Bauernschuster & Schlotter also make use of a Difference in Difference (DiD) appoach. Now they compare the development of those mothers targeted by the reform to three different control groups with the following equation:

$$Y_i = \alpha + \beta T_i + \gamma D_i + \delta (T_i * D_i) + \epsilon_i \qquad (3)$$

where Y_i is again the labor market outcome of the mother, T_i a dummy whether the observation is from 2001 ($T_i = 1$) of from 1996 ($T_i = 0$), D_i a dummy whether the person belonged to the treatment group ($D_i = 1$) or the control group ($D_i = 0$), and ϵ the error term. The parameter of interest in this case is the δ because the interaction term shows the effect over time that can't be attributed to a general time trend but to the particular change the treatment group experienced. This approach relies on the common-trends asssumptions which states that the evolvment of the outcome variables over time in treatment and control group is similar (controling for differnces in observables).

Compared with the LATE from the IV approach Bauernschuster & Schlotter obtain now an Average Treatment Effect of the Treated (ATT), which is much more general than the LATE from before because now the authors have broader definition of treatment group namely all those mothers with their children which became elgible through the reform. However some questions arise when we take a closer look to the control groups the two reseachers take for their DiD approach: Since the claim was introduced on the federal level, other regions could not serve as control group. Therefore they take mothers where the youngest child is 11 or 12, females without children aged 29 to 36 and to all females between 18 and 60. This idea is not completely new in case a reform is implemented on a nation wide basis (check i.e. Bettendorf et al. (2015)) but other studies with a control group within the same country like Lefebvre & Merrigan (2008) or Gathmann & Sass (2012) have a control group which is more likely to fulfill the common-trends assumption.

Data Sources and Descriptive Statistics

Data Sources

Both approaches use micro data from mothers which youngest child is between three and four when the reform was implemented in the late 1990ies. The best potential data sources for this purpose and period in Germany is the Sozioökonomisches Panel (SOEP) which is an annual penal data survey with a rich set of observables to control. Furthermore it is possible to infer in which month a child was born, which is necessary to calculate whether the child was three years old when the school year started and therefore determine the value of the instrument Z_i. Problems the SOEP could have are that it potentially has not enough observations but to deal with that, Bauernschuster & Schlotter use the German Microcensus, one of the largest regular surveys in the world, which also include multiple covariates but - and this is crucial for the IV approach - not the birth month of the youngest child. Therefore the instrument can not be calculated properly and the microcensus data set can only used for the DiD approach. Given the fact that SOEP data set at least potentially contains information about the county of residence (if you have access to SOEP remote that should be no problem at all, and even the humbled author of this paper review has access to SOEP remote) it is questionable why they were not able to identify the municipalities applied the cut-off rule. The authors themselves remain silent on the matter but could be criticized for that.

Descriptive Statistics

It might be interesting before one sees the estimation results to have a look at the descriptive statistics for some of the variables which are important for the discussion of the results. Of most interest may be the development of the kindergarten attendance rates for children of various age groups (namely 3,4,5 and 6 years old) during the 1990ies and the early 2000nds displayed in Figure 1.
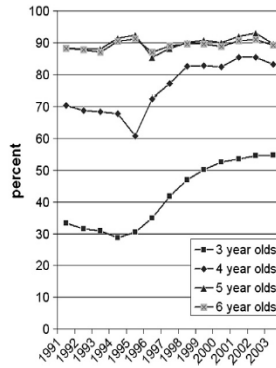
Figure 1: Childcare Attendance rates in West Germany

It is clearly visible that the attendedance rate for five and and six year old children remained rather constant around 90 percent. On the other hand there is a sharp increase in the attendance rates for three and four year old children observed between 1996 and 2001. For the children aged three years attendance increases from slightly above 30 percent to slightly above 50 percent and for the children aged four years the attendance rate increases from 60 percent to over 80 percent. So it seems reasonable that the introduction of the claim indeed raised childcare attendance especially among the children of age three and four.

Other descriptive statistics show that indeed a majority - by far not all - children started the kindergarten year when the school year started. For federal states where the schoolyear usually starts in August/September more than 50 percent of the children started their childcare attendance in the same month as the new school year. This magnitude is on the one hand probably relevant but as we will later discuss it also influences the LATE the IV approach in a unfavorable manner.
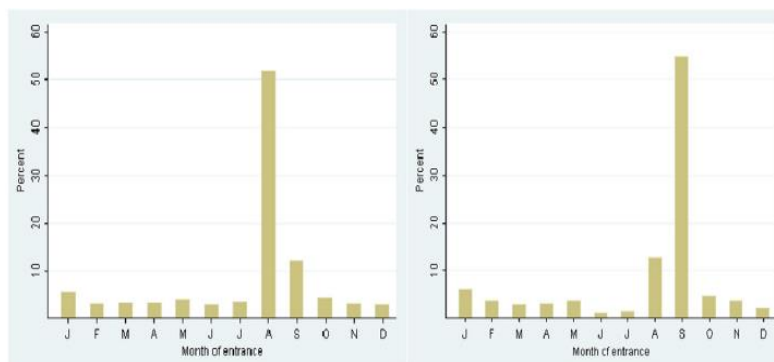

Figure 2: First month of attendance depending on start of the schoolyear

Results

In the following section we will report the estimates Bauernschuster & Schlotter and give an interpretation of the effect and its comparision to the other results which were reported in the introductory paragraph.

Results of the IV approach

First of all it is necessary to check the results of the first stage. According to the authors' calculations the first stage is for both specifications (with and without controls) highly significant (F-Test has a value of 30 and 35 respectively) and thereby the authors conclude that the releveance criterion is met in their approach. Using the predicted values for the childcare attendance rate they get estimates which are barely changed by the inclusion of control variables. Assuming linearity of the effects Bauernschuster & Schlotter estimate that a 10 percentage points increase in childcare availability increases the labor market participation of the mothers targeted by the reform by 3.6 to 3.7 percentage points. The amount is considerable but not spectacular and comparable to the other studies which were mentioned above. On the other hand this means that almost two-thirds of the mothers making use of the reform use it for different purposes that taking up a new job.

Given the imprecision of the IV estimates yielding standard errors six times larger than the OLS estimate the authors only can reject the null-hypothesis that at the 10%-level.

Bauernschuster & Schlotter also provide reduced form evidence which they label as Intention-to-treat effect which is significant at the 10%-level and states that the reform increased childcare attendance by slightly more than six percentage points over all mothers which were target of the reform.

Dependent Variable: Maternal Employment (yes/no)

	reduced form	First Stage	Second Stage	reduced form	First Stage	Second Stage
child above cut-off	0.06* (0.04)	0.18***(0.03)		0.07* (0.04)	0.18***(0.03)	
Childcare			0.36* (0.2)			0.37*(0.2)
Year, states and individual controls	No	No	No	Yes	Yes	Yes
F-statistc		30.91			35.63	
N	1936	1936		1936	1936	
R^2	0.01	0.05		0.12	0.13	

Table 1: IV estimates

Results of the DiD approach

The coefficient of interest δ can easily be taken from Table 2 as the coefficient related to the interaction term in equation (3). The three specifications yield quantitatively significant effects and vary only slightly. Bauernschuster & Schlotter estimate that through the reform the employment of the respective

mothers was increased by 5 percentage points according the the specification in which the authors use the mothers of 10 and 11 year old children as control group and up to 8 percentage points when all 29 to 36 year old women without children. To make this estimates comparable to those obtained in the IV estimation, it is crucial to know that between 1996 and 2001 the childcare attendance rate of three and four year old children in the sample increased by 17 percentage points from 53 to 70 percent. This means that an increase calulated for an 10 percentage point increase in the attendance rate results in a 3 percentage point increase in maternal employment (if we again assume linearity here). This estimate is nearly the same point estimate as the coefficient for the iV estimate. This result is a first indication that both approaches identify the true effect of public childcare on maternal employment.

However it must be stated that this interpretation of Bauernschuster & Schlotter seems rather sloppy if one considers the following two imprecisions. Firstly the effect has almost the same magnitude if we take the 10 and 11 year old children but this is only one of three control groups and yields the lowest estimate of all three different specifications. If someone would for example calculate the effect obtained for the second control group the effect would be larger with 4.7 percentage points per 10 percentage points increase in availability. Even more staggering is the wrong computation of the effect in the preferred specification of Bauernschuster & Schlotter. To calculate the effect of a ten percentage points expansion of public childcare they randomly state that the increase in childcare attendance was from 55 to 70 percent instead of the 53 and 70 percent the authors present their readers one page before. Thereby the effect observed changes from 3.0 (true value) to 3.4 (what the authors report) percentage points per ten percentage points increase in kindergarten attendance. Although that might be not significant it is a mentionable inaccuracy of the paper.

Employment (yes/no) control group	10 and 11 year old children	women 29 to 36, no children	women 18 to 60, no children
Treatment	-0.21***(0.01)	-0.38***(0.01)	-0.29***(0.01)
Time	0.05***(0.01)	0.02***(0.01)	0.04***(0.0)
Interaction effect	0.05***(0.01)	0.08***(0.01)	0.07***(0.01)
Controls	Yes	Yes	Yes
N	19844	25796	108642
R^2	0.07	0.21	0.1

Table 2: DiD Results

Robustness Checks

Technically already the two other control groups of the served as a robustness check, however Bauernschuster & Schlotter use other techniques to argue in favour of both estimation techniques.

Robustness of the IV

To underline the robustness of the IV approach, the authors show that the parents below and above the cut-off do not vary significantly in most character traits and additionally they provide estimates for the predicted (by all characteristics except the cut-off rule) childcare attendance rate and try to show that based on observables the increase in predicted childcare attendance runs smoothly around the cut-off. The jump in the actual attendance seems to be driven by the cut-off rule shock. To further test the IV approach, the two

authors apply a placebo test by regressing the model on the employment status in period $t-1$ instead of period t as done in equation (2). The results of the IV estimate, displayed in Table 3 show that the coefficient is not statistically different from zero. This means that mothers whos youngest child turned three shortly after the cut-off date are not different from mothers whose youngest child turned three shortly before the cut-off date in terms of employment before the cut-off rule becomes relevant. The autors conclude that this further validates the use of the IV approach.

Dependent variable: Maternal employment in period t-1

	reduced form	First Stage	Second Stage
child above cut-off	0.01 (0.04)	0.18***(0.03)	
Childcare			0.06 (0.19)
Year, states and individual controls	Yes	Yes	Yes
F-statistc		37.61	
N	1911	1911	
R^2	0.13	0.13	

Table 3: Placebo test fot the IV estimate

Further robustness for the estimates comes from regressiounswherre Bauernschuster & Schlotter change the dependent variable to the number of hours worked. In this specification they use the same IV approach and come to results which have rougly the same magnitude as the estimates from LPM described the results section: For both specifications (with and without control variables) the IV yields 14 hours of additional working time per week, yielding 1.4 additional working hours if we assume that childcare attendance increases by 10 percentage points. This is mentionable and statistically significant but in no direction surprising. Therefore this regression adds further credibilty to the IV estimate (having in mind what LATE they identify).

Robustness of the DiD

Besides the control groups used which actually reduce the trust in the estimate of Bauernschuster & Schlotter (see Critical Discussion of the Results), the authors also employ a placebo test to validate that actually identify a causal effect and not a time trend or something which varies over the whole population. Indeed the estimated coefficients are all not distinguishable from zero and therefore support the common trends assumption and the relevance of the introduction of the legal claim.

Decomposing the effect on a year basis shows clearly that before the reform no effects were observable. In contrast to that after the reform the effect gradually increases over time, demonstrating that the reform needed some time to affect womens labor supply decision but clearly affected maternal labor supply five years later considerably. This decomposition first shows the existence of an effect and secondly its evolvement over time. Furthermore it increases the confidence in the common trend assumption.
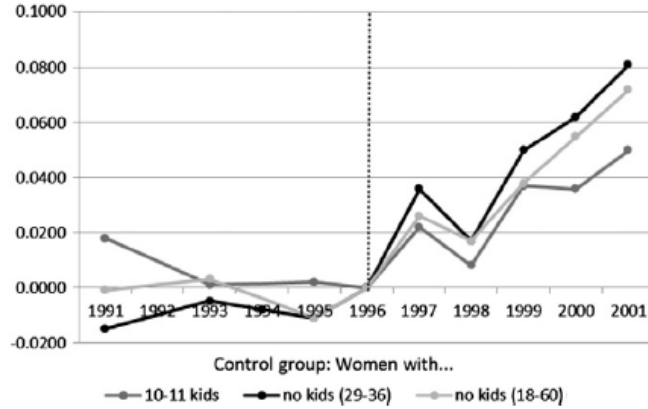
Figure 3: Estimates for the DiD effect for each year

Critical Discussion of the Results

Overall this paper certainly contributes to the existing literature in the field and adds evidence to the relationship between maternal labor supply and public childcare. Especially for Germany where not much evidence exists yet, the paper delivers a valuable contribution. The paper gives a concrete estimation of an important policy change in Germany. In fact in appears reasonable that the introduction of the legal claim for a kindergarten was an rather unexpected event and could therefore be described as exogenous variation. So indeed the paper uses an appropriate policy change to apply estimation strategies which are widely used in the context of quasi-experimental methods.

Indeed all robustness checks validiate the assumptions which have to be made to justify the IV approach. The relevance criterion is clearly fulfilled and also the Placebo tests further strenghtens the arguments made in favor of the IV approach. Most difficult in the IV case is to assess the validity of the exclusion restriction. The authors argue here that just around the cut-off age (36 months) the existence of the cut-off rule is the only factor pushing mothers back to work. Indeed in the close range around the cut-off it appears rather unlikely that the age matters through a different channel than the elgibility for the kindergarten. Potential threats to the identification approach such as other caring opportunities (i.e. grandparents, private daycare) driven by age as well can rejected by the fact that private daycare is so small in size in Germany (less than 1 (!) percent) and that more care through grandparents will not hang on a two month age differential, so the exclusion restriction seems reasonable in this context.

More criticism deserves the cut-off rule itself. It is by definition very fuzzy (remember municipalities apply it as they want) and the subgroup to which the it applies is very small. In fact Bauernschuster & Schlotter only identify effects for municipalities which applied the cut-off rule and inside these municipalities mothers

9

which complied with the treatment. This excludes all mothers which sent their children to kindergarten at a younger age or sent them to kindergarten not in August/September when the cut-off rule was potentially applied. According to their own data Bauernschuster & Schlotter report that only 70 percent of the children in the sample started with childcare in August or September. All this reduces the group for which the authors estimate their LATE. Since the authors can't identify the municipalities they don't know for which group they finally identify the treatment effect. This is slightly unfortunate since generally it should be possible for Bauernschuster & Schlotter to obtain data for the municipalities and the application of the cut-off rule. This would have increased the knowledge which population is actually targeted by the instrumental variable.

With respect to the DiD approach it has to be stated that this approach relies on the common trends assumption. In order to check the validity Bauernschuster & Schlotter fortunately perform some placebo tests and yield insignificant results which support the common trends assumption. Some concerns could be raised about the choice of control groups as already mentioned earlier which in fact might differ in some aspects and therefore might not be optimal. The coefficient estimates vary considerably over the three different control groups which supports the notion that at least some of the control groups are far away from satisfying the common trends assumption.

Even worse than that is the blunt way in which Bauernschuster and Schlotter try to argue that both identification strategies yield rather similar point estimates. Whereas the IV point estimate is obtained in a transparent manner, the computation for the point estimate for the DiD (remember this is an ATT) is calculated intransparently and even worse arithmetically wrong. Whereas the authors first only consider the estimate which fits best for their purposes they then use the wrong values for the increase in kindergarten attendance in order to obtain a point estimate which is almost the same as for the IV. What was intented as argument to underline the robustness of both estimates, turns to be a serious flaw which reduces the credibility of the paper.

Furthermore it is questionable whether the assumption that the costs don't matter and can be neglected since 80 percent of the costs are subsidized by the government . First of all the fees for childcare vary dramatically over Germany since they are (usually) set by the municipality. Furthermore surveys like the SOEP show that prices actually matter when parents decide whether they should bring their child to kindergarten or not. So childcare fees appear to be crucial determinant of the final decision whether children attend a kindergarten and whether the resprective mother workds. This might overlap with the effect from the expansion and therefore the effects estimated by Bauernschuster & Schlotter could be biased.

Research Outlook

Based on that criticism issued before a study which examines the effect that childcare fees would have seems a valuable target. In order to achieve this, the IZA - Institute of Labor Economics has started to collect data on childcare fees for the largest municipalities in Germany and for municipalities in North Rhine-Westphalia. Although it is tedious to obtain the data since their is no central register for them. The most current data set on this so far has been collected by the Initiative Neue Soziale Marktwirtschaft (INSM) comparing childcare fees for almost a hundret cities in Germany for selected "prototype families" therefore the data set which is also rather old (2010) can not be used for attachment to the any other data sets. In contrast, by now the IZA has collected the largest data set containing the childcare fees for all 400 municipalities in North Rhine-Westphalia and for all urban districts in Germany. In municipalities where this was possible the IZA has obtained that data for multiple years reaching back until 2008. This data set linked with data from the SOEP or the Microcensus might be able to figure out what the effect of different childcare fees might be. Since then for each mother both the actual and the hypothetical costs for childcare could be calculated

causal effects of childcare fees could be identified.

Furthermore the particular setting in North Rhine-Westphalia allows for the application of quasi-experimental methods. In 2006 the state government transfered the legislative power from the state level to the level of the municipalities. Before the reform the childcare fees were therefore similar all municipalities of North Rhine-Westphalia. Afterwards the municipalities could set the fees individually. Whereas some (richer municipalities) reduced the fees afterwards others left the fees unchanged and some even increased them. This provides us with a set-up which could be exploited for further research (i.e. the Master thesis of the author of this report).

Conclusion

The paper contributes to the existing literature on the topics of childcare and its effect on maternal labor supply. It is part of an growing body of papers examining this topics also in German contexts. The methods applied are standard quasi-experimental methods and their application seems to be justified overall. The magnitude of the estimates appears to be reasonable and is in line with other estimates for Germany (esp. Geyer et al. (2015)). However the paper suffersfrom some considerable flaws. Firstly the use of the instrument narrows the LATE Bauernschuster & Schlotter measure to an undefined small subgroup although identifying at least the municipalities which applied the cut-off rule should have been possible. Secondly, the calculation of the effect in the DiD framework is arbitrary and simply wrong. This indeed reduces the credibility of the paper significantly. Thirdly the ignorance towards childcare fees is questionable. This criticism is the base for further research which is already planned. The strive for knowledge continues...

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