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The effects of after-school programs on maternal employment

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Fabian Dehos and Marie Paul

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The Effects of After-School Programs on Maternal Employment

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Abstract: This paper evaluates the impact of a massive expansion of after-school programs (ASPs) on the labor market participation of mothers with primary school children in the West German context of relatively low full-time employment rates. Using an instrumental variables approach we exploit regional and temporal variation in the provision of federal ASP starting grants by a nationwide investment program. Results suggest that additional ASP places had no effect on working hours or the employment probability of mothers with primary school children.

JEL Codes: J13, J21, J22

Keywords: maternal employment, after-school programs, instrumental variables

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1 Introduction

Publicly sponsored child care is a policy issue in many countries, and it may be one effective way for increasing maternal working hours. In countries with half-day schools, a lack of child care availability could be one of the reasons for relatively low (full-time) employment rates of mothers with school-age children.¹

In West Germany, the care restriction for primary school children has been relaxed during the last decade by an extensive expansion of after-school programs (ASPs). In 2002, 1% of primary school children in West Germany attended an ASP and in 2012 the amount had risen to 22%. Within primary schools, ASP slots for 483 000 additional children were created with the support of a federal investment program of overall four billion euros and additional financial assistance from states and municipalities.²

In this paper we investigate the impact of additional ASP places on maternal employment. In particular, we estimate the effect of a child's attendance in a newly created ASP place on the mother's working hours and employment probability. Using an instrumental variables (IV) approach, we exploit the successive expansion of ASPs in West Germany through the nationwide investment program. The amount of financial support and when it began vary across counties, and conditional on county fixed effects and time-varying covariates, we argue that the allocation is exogenous to unobserved maternal work preferences. Our first-stage estimates of the IV-approach are based on the German Socio-Economic Panel (SOEP) using a sample of mothers with primary school children living in West Germany. To obtain precise estimates of the reduced form, we require a larger data set. Drawing on the German Microcensus, we obtain an equivalent

¹See OECD (2011), section 4, and Plantenga and Remery (2013) for some information on coverage rates and excess demand for after-school child care in several countries. In Germany, the rate of full-time dual earner couples among couple households with the youngest child aged 6 to 14 years was 18% in 2013 compared with an OECD average of 44% (OECD Family Database, Table LMF 2.2.A). Some (but not all) countries that have a strong excess demand for after-school child care, such as Germany, also have low rates of maternal full-time employment (see Plantenga and Remery, 2013).

²Own calculations based on tables in KMK (2008) and KMK (2015) and the number of primary school students in a state provided by the Statistical Office.

sample that is about 27 times larger than the SOEP sample. However, we cannot use the Microcensus for the first stage because individual information on ASP attendance is not included in the data. Thus, we implement a two-sample two-stage least squares (TS2SLS) approach.

By this means, we provide an ex post evaluation of the extensive and costly expansion of ASP slots in Germany with regard to maternal labor market outcomes. Several recent discussion papers also focus on this reform, but they rely on more restrictive identification assumptions, draw exclusively on SOEP data and find different results than our analysis. Gambaro et al. (2016) focus on mothers of first-graders and control for a large set of observables including maternal employment status before school entry. They find a strong effect (11.4 p.p.) of afternoon child care on the probability of taking employment for mothers who did not work in the year before their child entered school and an effect of 2.6 additional hours of work for mothers who were already employed. However, the selection on observables strategy is likely to lead to upward biased estimates. Mothers with an employment offer in hand are, for instance, more likely to take a job and an ASP place for their child compared to mothers who have the same observable characteristics but less luck with work opportunities.³

Nemitz (2015) exploits – similar to our paper – the allocation of ASP grants through the federal investment program as conditional exogenous variation. In contrast to our analysis, she solely uses SOEP data and focuses on bivariate probit models because the relatively small SOEP sample does not allow obtaining precise 2SLS estimates. She finds a very strong effect (26 p.p.) of ASP on the employment probability and no significant effect on the probability of working full time for working mothers.

Furthermore, we aim to add to the literature on the causal employment effects of child care availability. Most papers focus on child care for preschool children, for example, Baker and Milligan (2008), Bauernschuster and Schlotter (2015),

³See also Rainer et al. (2013) and Tobsch (2013) for effects of ASP on maternal labor market outcomes estimated based on selection on observables assumptions. In addition, Shure (2016) uses a sample of mothers from the SOEP and estimates the effect of whether the closest primary school runs an ASP on mothers' employment outcomes. An ex ante simulation by Beblo et al. (2005) investigates the potential impact of smaller and larger ASP expansions.

Cascio (2009), Fitzpatrick (2010), Gathmann and Sass (2012), Gelbach (2002), Havnes and Mogstad (2011), Lefebvre and Merrigan (2008), and Nollenberger and Rodríguez-Planas (2015). But the need for reliable care is not restricted to early childhood. Most employed parents of school-aged children have to make additional care arrangements around the usual school day, which is a particular issue in countries that follow a mornings-only half-day system (Paull, 2008; OECD, 2011). The employment effects of the child care policies for school children, however, are not necessarily the same as for pre-school children. The existing literature is scarce and does not provide unambiguous evidence.

Felfe et al. (2016) estimate the causal effect of after-school care availability in Switzerland, and they find a positive effect on the maternal full-time rate but no effect on mothers' employment probability. The setting they investigate is characterized by low after-school child care coverage and high fees (about USD 40 per day without special subsidy). Berthelon et al. (2015) analyze a reform in Chile that lengthened the obligatory school day, and they find a strong positive effect on mothers' labor force participation. From a labor supply perspective, the reform involves additional hours of supervision, but no additional fees and no parental choice because participation is binding for all students. The German setting in contrast is characterized by a program with low fees, voluntary participation, and an initially low coverage rate.⁴

The results of our paper call into question the recent empirical evidence on the expansion of ASPs in Germany and suggest that the reform had no impact on maternal employment. We find that sending a child to an ASP has no causal effect on the employment probability for those mothers who are offered one of the additional places and accept it. There is also no evidence for any effect on hours. Results are stable across different subsample specifications and robustness checks. Thus, the massive expansion of school-based low-cost child care in the West German context of half-day schools did not induce stay-at-home mothers to start working or part-time mothers to increase working hours.

⁴Evidence also exists on the effects of a price reduction in a setting with very high child care coverage by Lundin et al. (2008) for Sweden. For children aged 5 to 9 years, they find no considerable employment effects.

The remainder of the paper proceeds as follows. The next section presents the institutional background. Section 3 discusses selection in ASP, the instrument, and its identifying assumptions. In section 4 we present the different data sets used. In section 5, we discuss some descriptive analysis on maternal employment, our main results, effect heterogeneity, and robustness checks. Section 6 concludes.

2 Institutional Framework

2.1 Primary Schools and Child Care in West Germany

In West Germany, primary school ranges from grade one to four, and children attending a primary school are usually 6 to 10 years old.⁵ The teaching curricula and the number of lessons differ slightly across federal states because education policies are each state's own responsibility to a large degree. Traditionally, primary schools are organized as mornings-only half-day schools that finish by lunchtime. Part of these schools provide a reliable framework of fixed start and end times; that is, supervision is guaranteed, for example, from 8 a.m. to 1 p.m. All remaining half-day schools send children home as soon as lessons end, even if this is at 11 a.m. on a particular weekday, or if lessons are canceled. Thus, differences exist in how many hours primary students at a half-day school are supervised; our best guess based on school curricula of the individual states is that supervision in a half-day school lasts about 4.5 hours on average in West Germany (KMK, 2011).

Before the start of the program in 2003, the vast majority of primary school children in West Germany were supervised by their mothers (or sometimes fathers) at lunchtime and in the afternoon. Supplementary care by grandparents has played a role, although according to Aassve et al. (2012) fewer parents receive child care support by grandparents in Germany than in most other European countries they compare with. Following Spieß et al. (2002), 23% of all school-aged

⁵In this paper, we focus on primary school students in West Germany. Bavaria and Berlin are excluded because of missing regional information (see subsection 4.2). East German states are also excluded because child care was less restricted in these states even before the investment program and full-time employment rates were considerably higher in East Germany.

children in Germany up to the age of twelve are supervised on a regular basis by relatives living outside the child's household and another 3.3% by paid persons, such as au pairs or nannies. Note, however, that these numbers include care arrangements that cover only a few hours per month.

The coverage rate of publicly sponsored external child care centers (*Horte*) has been traditionally low in West Germany. In 2002, about 4.2% of all primary school children in West Germany attended such publicly sponsored external care centers (Statistisches Bundesamt, 2004). As opposed to ASPs, these centers are run by the communities or by organizations such as churches, and they often share the building with child care facilities for preschool children. To our knowledge, privately run institutional care for school-aged children that is not publicly sponsored does not exist.

2.2 The Recent Expansion of After-School Programs

During the last decade, many schools in Germany have started to run ASPs. These programs extend the supervised time at school to the afternoon and provide lunch and complementary courses such as sports and arts, recreational and leisure activities, as well as homework assistance. Schools offering such a program are called all-day schools (*Ganztagschule*) in Germany, but we prefer ASP because this term is a better description of what is provided by most of these schools.

In West Germany, the large majority of primary schools offering an ASP – 86% in 2011 (KMK, 2015) – are organized in a non-integrated way; that is, all children attend regular lessons in the morning, and only ASP participants stay in school for lunch and activities. At the beginning of each term, parents apply for an ASP place. If accepted, they have to cover the expenses of the child's lunch and often a small participation fee.⁶

The whole organization of the ASPs is the school's responsibility, and it is supposed to be conceptually related to the syllabus (KMK, 2015). However,

⁶Fischer et al. (2013) present a survey conducted among head teachers. In 98% of all primary ASP schools, parents have to cover the child's expenses for lunch, and in 59% of all primary ASP schools, they have to pay a general participation fee.

most ASPs offer supervised leisure and homework time rather than a structured program directly supporting the learning conditions (STEG, 2010). Only a few primary schools follow an integrated approach that makes school attendance in the afternoon compulsory for all children.

As a minimum requirement, ASPs have to provide a program that lasts at least seven hours per weekday, including regular lessons, three times a week.⁷ Most schools provide a far more extended program, and the median time of supervision amounts to 8.5 hours per weekday (Fischer et al., 2013). From 2002 to 2012, the share of primary school students attending an ASP increased from 1% to 22% in West Germany.⁸ The extensive expansion of ASPs has been a top priority of educational politics in recent years, and it was fostered by the federal government with a nationwide investment program. Section 3.2 describes in more detail how we exploit the underlying variation of the program to detect the causal impact of ASP attendance on maternal employment.

2.3 Demand for ASP Places

According to Klemm (2012), 70% of parents report that they would like to send their child to an ASP. This proportion indicates a strong excess demand, which is also reflected by almost all new places being taken immediately. In general, ASP attendance is voluntary and participants and non-participants differ in terms of demographic and socioeconomic characteristics (Marcus et al., 2016).

There are several potential reasons why parents want to register their child at an ASP when the child enters school or when an ASP becomes available while the child is in primary school. First, mothers may consider their child old enough that increasing maternal working time from part to full time is feasible. Second, ASPs may attract the labor participation of mothers who stayed at home while their child was below school age. For those mothers, finding a part-time job with working time in the morning only may be difficult, because they have been out of the labor market for a longer time. ASPs, however, expand the choice set of these

⁷See KMK-definition: KMK (2015).

⁸Own calculation based on KMK (2008) and KMK (2015).

mothers to jobs with less convenient working hours such that they are more likely to take up employment again. Similarly, ASPs may be particularly helpful for single mothers who cannot share child care duties with a partner. Third, ASPs could be crucial to maintain high maternal labor market participation during the child's progression to primary school. This possibility exists because many preschool kindergartens offer more reliable and longer hours of supervision than primary schools. Fourth, parents may substitute other forms of informal child care by an ASP, and fifth, choosing an ASP may be unrelated to employment issues. Parents may believe, for instance, that their child can benefit from ASP attendance. In a survey commissioned by the federal ministry, 77% of parents with a child in an ASP agree with the statement that children enjoy attending it, and 85% support the statement that an advantage of ASPs is children spending time together with peers.⁹ Finally, ASPs may increase maternal leisure time as well as the quality of family life. With a child attending an ASP, parents may organize their working schedule to coincide with the child's school day such that the whole family can have breakfast and dinner together.

3 Empirical Approach

3.1 Selection into ASP

We aim to analyze the causal impact of ASP attendance on maternal employment, exploiting regional and temporal variation in the provision of federal ASP starting grants. This identification strategy follows an approach comparable to those used in other recent studies on the effects of child care, e.g. Bauernschuster et al. (2016), Cornelissen et al. (2016), and Felfe and Lalive (2014).

For a causal interpretation, we have to consider selection into ASP at the following three stages. The first hurdle is the availability of ASPs in the residential area. We do not know if the few pre-existing ASPs are primarily located in counties where parents tend to work more as opposed to other counties. Within

⁹See Heimar et al. (2011, chap. 2, fig. 12).

a county, it could further be the case that traditional as well as new ASPs are clustered around neighborhoods with high maternal employment rates.

Second, selection into schools with ASPs is likely to occur at an individual level. Theoretically, children are assigned to a primary school in most regions according to their residential address. In reality, however, several ways exist that permit circumventing the assignment rule. For instance, parents may choose a private school, or they can request an exception that justifies attending a school with an ASP. Such strategic behavior is most likely correlated with work preferences and other unobservable characteristics that might bias the analysis.

Third, and probably most importantly, selection takes place within schools that offer ASPs. Since ASP participation is mostly voluntary, parents have to decide whether to apply for a place and they may do so based on their work preferences. Within this context, schools may also favor single mothers or give priority to working parents (see Börner et al., 2012), which induces a further selection mechanism.

To illustrate these concerns, consider the following regression model:

$$(1) \quad y_{it} = \beta_1 ASP_{it} + X'_{it}\beta_2 + \epsilon_{it}$$

where $i = 1, \dots, N$ indicates the respective mother, t the year, and y_{it} the employment outcome (e.g., hours worked or an employment dummy). X_{it} is a vector of control variables capturing mother-, child-, and household-specific characteristics, aggregated covariates of the county of residence, and time dummies. The inclusion of X_{it} may reduce the bias that potentially occurs when comparing y_{it} between ASP participants and nonparticipants. However, ASP_{it} likely remains endogenous because of unobserved factors that are correlated with ASP attendance as already described. To overcome this issue, we apply an IV estimator.

3.2 The Instrument

The recent increase of ASPs in Germany was fostered through a nationwide investment program, which was initiated in response to the PISA report in 2001. The poor performance of German students – scoring substantially below OECD average – set off a vigorous public debate about the quality of the educational system and compelled policy makers to take action.¹⁰ As a consequence, the German Federal Ministry of Education and Research (BMBF) provided four billion euros for the expansion of ASPs through the investment program “Future of Education and Care”. A pedagogically valuable program following regular lessons was expected to improve the educational standards and students’ test scores all over Germany. As a second objective, a better reconciliation of work and family life was pursued in order to stimulate maternal employment.¹¹

In an IV approach, we exploit the successive expansion of ASPs using the provision of grants provided through the investment program.¹² The instrument z is defined as *the ratio of schools in a county funded by ASP grants*. Further information on the construction of the instrument as well as the underlying data sources is outlined in the subsequent section (subsection 4.2). In the present subsection, the institutional background is described in more detail.

ASP grants provided by the investment program were issued to schools from 2003 to 2009 and more than 50% of the financial support was directed to primary schools (BMBF, 2009). The use of these grants was strictly reserved for infrastructural measures (BMBF, 2003), such as the expansion and restructuring of school buildings or schoolyards, the establishment of refectories, or the construction of recreational areas and other facilities necessary to start or expand ASPs. States and communities usually provided additional financial support to cover the costs of running an ASP, such as salaries and subsidized meals.¹³ Across states, federal

¹⁰In response to that shock, the former Federal Chancellor Schröder proposed a reform agenda that included the expansion of ASPs (Schröder, 2002).

¹¹For a comprehensive overview on the background and the objectives of the investment program see Augustin-Dittmann (2010).

¹²Nemitz (2015) also uses the provision of ASP grants in her identification strategy.

¹³According to Blossfeld et al. (2013), federal states invested a further 400 million in the creation and expansion of ASPs.

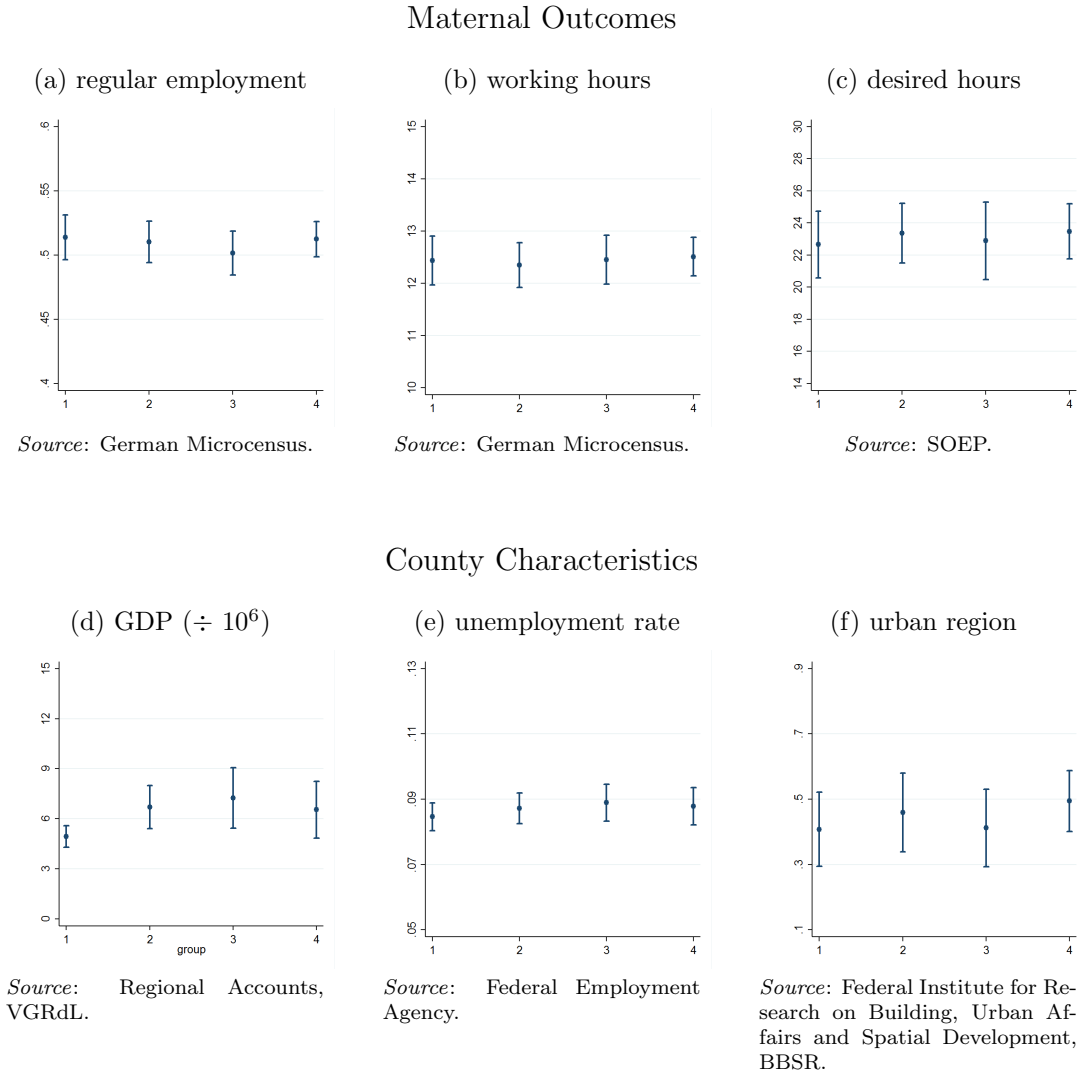
ASP grants were distributed proportionately to the share of students. Within a state, school boards applied for ASP grants by sending a formal application to the respective state authority. A short proposal on the plans for setting up or expanding an ASP had to be included to the application.¹⁴ Furthermore, the possibility of implementing infrastructural measures at or within the school building had to be given. Once an application was received by a state authority, fulfillment of certain minimum requirements was examined. Thereupon, ASP grants were typically issued on a first-come, first-served basis. Schools that did not apply or did not receive grants could re-apply one year later (see Schiersmann et al., 2011).

Whether a school board submits an application and when it does so strongly depend on the head teacher's as well as the staff's preferences with regard to ASP. On the one hand, setting up an ASP involves additional effort and extra work for the head teacher and his or her team; on the other hand, it increases the attractiveness of the school. While the school board has to decide on the submission of an application in accordance with the school committee, local authorities might have influenced the decision. Similarly, a few states may have potentially favored schools with many disadvantaged children. Additionally, states differ in the amount of funds that they issue to primary schools as compared to other school types. This allocation process certainly involves factors unrelated to individual work preferences and job opportunities (e.g., the motivation or indifference of a particular head teacher; whether the school building has space for expansion), but it most likely also includes factors related to county-specific maternal work preferences that we need to control for.

Figure 1 provides an initial impression of how much the provision of ASP grants is related to various labor market as well as county-specific characteristics. Outcomes are classified by the average funding intensity in the years 2003 to 2012, and they are shown for a point in time right before the actual program started. As graphically illustrated, we distinguish between four different groups of counties that received a small, medium, large, or very large funding rate. Since

¹⁴For a comprehensive overview of the state-specific regulations see the summary reports issued by Schiersmann et al. (2011).

Figure 1: Sample Means Before the Provision of Grants Grouped by the Average Funding Ratio.



Note: The figure presents for maternal labor market outcomes (a-c) as well county specific characteristics (d-f) the sample means following the county specific funding intensity. Four different groups are defined using the quartiles of the average funding ratio (instrument z) in a county as thresholds (x-axis). Group means as well as 95% confidence intervals (y-axis) are shown for a point in time right before the actual start of the provision of funds (first and second quarter of 2013 if possible). To account for systematic differences across states, variables are adjusted by state specific means.

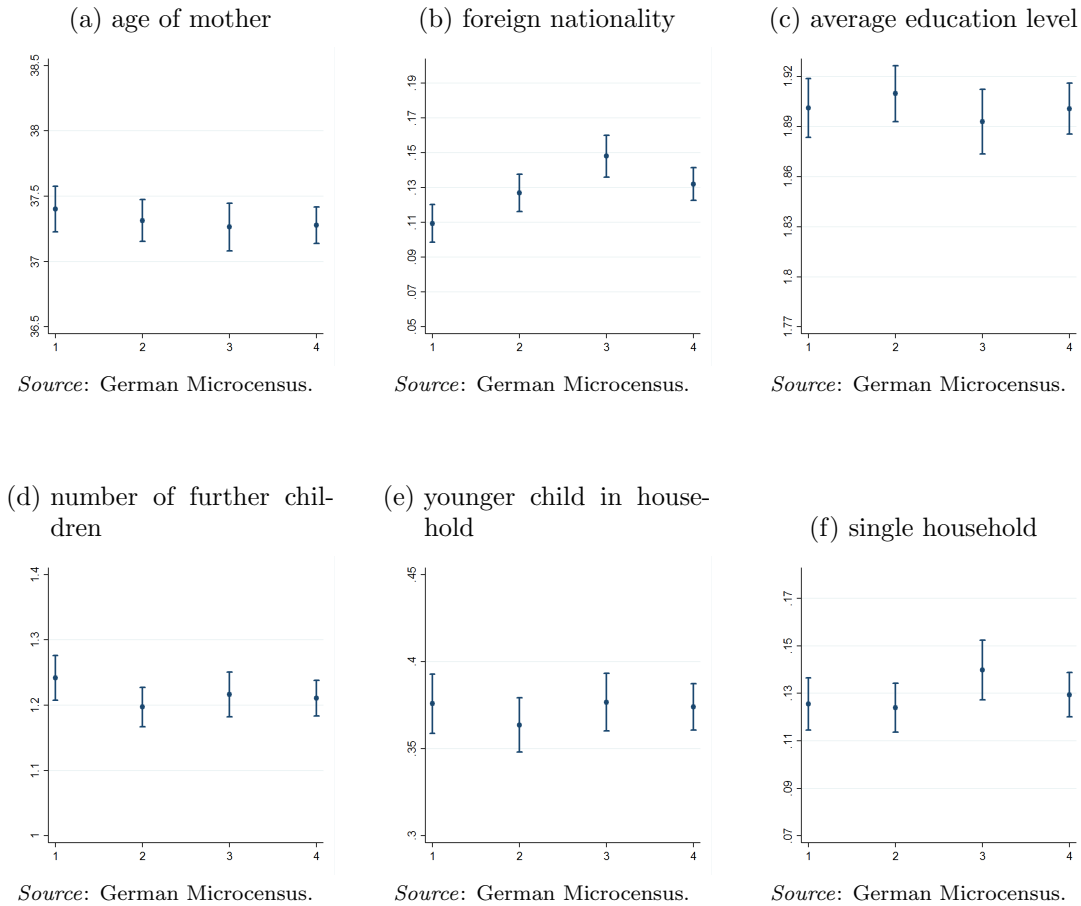
the distribution of grants within a state was conducted by the respective state authority, there was potentially some scope to pursue the state's own funding priorities as reported above. To account for such systematic differences, we adjust the depicted variables by the respective state means.

The first row (a - c) of Figure 1 shows outcomes that refer to the maternal attachment to the labor market. The second row (d - f) of Figure 1 captures

county-specific characteristics such as GDP, unemployment rate, and a classification of the spatial structure. Interestingly, none of the graphs suggests a significant difference in the respective variables with regard to the average funding intensities.

Similar graphs are shown in Figure 2 for further specific individual control variables that are included to the analysis. Except for foreign nationality, none of

Figure 2: Sample Means of Specific Individual Control Variables Before the Provision of Grants Grouped by the Average Funding Ratio.



Note: The figure presents for different control variables the sample means following the county specific funding intensity. Four different groups are defined using the quartiles of the average funding ratio (instrument z) in a county as thresholds (x-axis). Group means as well as 95% confidence intervals (y-axis) are shown for a point in time right before the actual start of the provision of funds (first and second quarter of 2013 if possible). To account for systematic differences across states, variables are adjusted by state specific means.

the covariates appears to differ systematically. Thus, perhaps surprisingly, there is hardly any indication that the funding intensity is systematically correlated with county as well as individual characteristics.

3.3 Identifying Assumptions

Our IV approach draws on two identifying assumptions that have to hold for the results to be interpreted causally. First, z must be correlated with the child's probability to attend an ASP. This assumption is testable, and we detect a strong and significant relationship. As a second assumption, z must be – conditional on the covariates included – unrelated to unobservable factors, ϵ . In our preferred specification we use county-fixed effects, dummies for years and calendar quarters, individual and household controls, as well as time-varying county variables. By including time dummies, we rule out the possibility that our instrument z takes up pre-existing trends or seasonal effects in maternal labor market participation that would have occurred even in the absence of funding, such as a general increase in maternal employment rates. Time-invariant characteristics of a county (e.g., an industry structure favorable to maternal employment or time-constant cultural norms) are absorbed by county fixed effects. This is potentially important, because the funding intensity could be lower in counties where mothers tend to have unobservables in favor of staying at home. It might also be the case that ASP grants go to poorer counties. If these confounding characteristics remain constant over time, county fixed effects will account for this aspect. Still, there might be county-specific factors related to maternal work preferences, job opportunities, and the allocation of ASP grants that change over time. Therefore, we also control for time-varying county characteristics such as the lagged employment rate and lagged GDP. This would be important if head teachers or local authorities anticipated an increasing labor demand by sending out (better) applications for ASP grants, or if the state authorities favored counties that suffered a recent economic shock such as a firm closure.

In section 5.4 we present several robustness checks that strengthen the (untestable) conditional exogeneity assumption. First, we estimate the reduced form for women without children and mothers with children below school age. Since neither groups should be affected by new ASPs, we do not expect to find significant effects. Second, we define the instrument with a one year lag to check if our

results might be biased by individual mothers who may have pushed an application of their school (e.g., through the parents' council). As an additional test, we add county-specific averages of individual employment patterns of mothers, add further county-specific controls, and vary the model specification as well as the outcome variables. In a last step, we look at mothers' moving behavior to investigate whether they select into counties with a higher funding rate.

Finally note that – as long as the conditional exogeneity of the instrument is fulfilled – our approach allows unobserved maternal work attitudes to be correlated with a) the individual preferences for ASP, b) the actual school choice as well as the respective application for an ASP slot, c) the final acceptance for a place offered by the school, or d) the spatial distribution of ASPs within a county.

4 Data

The empirical analysis combines two individual level data sets, the German Socio-Economic Panel (SOEP) and the German Microcensus, as well as different minor data sources that provide information on the county level. German counties correspond to the NUTS 3 definition and represent the smallest common territorial unit for which the following data are available.

4.1 SOEP and German Microcensus

The SOEP is an annually conducted representative longitudinal survey of households and their respective members. It provides rich information on individual, socioeconomic, and household characteristics and covers all West-German states since 1984 (see Wagner et al., 2007).

In order to add county-specific information to the analysis, we make use of the remote access, which includes the standard SOEP extended by a county identifier for each household.¹⁵ A major advantage of the SOEP is that we can identify primary school children who attend an ASP. The respective information

¹⁵Socio-Economic Panel (SOEP), data for years 1984-2012, version 29, SOEP, 2013, doi:10.5684/soep.v29.

is based on two annually stated questions in the household questionnaire about the school type and children’s attendance in the afternoon. A comparison with administrative data provided by the KMK anchors the adequacy of the obtained ASP measurement and its representative structure.¹⁶

Our sample of interest contains mothers who are 20 to 60 years old, live in West Germany, and have at least one child in primary school during the years 2003 to 2012.¹⁷ The age of primary school students ranges from 6 to 11 years. If a mother has several children in primary school, we restrict the child characteristics to the youngest child, such that a mother is just included once per year. However, a mother may appear for more than 4 years in the sample if she has several children attending primary school during the time period analyzed. This leaves us with 1 809 mothers and 5 103 mother-year observations in 230 counties and 10 years.

The SOEP sample delivers precise results for the first stage, but the reduced form estimates suffer from large standard errors. Therefore, we draw on a much larger data source, the German Microcensus, to estimate the reduced form. The Microcensus is a representative household survey conducted annually that covers 1% of the population in Germany and includes detailed information on the individual as well as household level.¹⁸ Our sample construction is equivalent to the SOEP and includes 140 071 mothers. Thus, the sample is about 27 times larger than the SOEP sample. We cannot use the Microcensus for the first stage because individual information on ASP attendance is not included in the data. Thus, we implement a TS2SLS estimator following Inoue and Solon (2010). The standard errors of the respective estimates are calculated using the delta method as proposed by Pischke and Von Wachter (2008) and Dustmann et al. (2017).¹⁹

¹⁶For a detailed comparison see Marcus et al. (2016).

¹⁷The federal states of Bavaria and Berlin are excluded because of missing regional information (see subsection 4.2).

¹⁸Since the scientific use files of the Microcensus do not provide county identifiers, we draw on an on-site version that can be accessed solely from the research centers of the statistical offices. The version created for our project is complemented with county-specific variables that we sent to the statistical office, most importantly our instrument.

¹⁹The standard error of the TS2SLS estimator is calculated as the square root of the respective variance which reads: $var(\hat{\beta}) = \frac{1}{\hat{\pi}^2} var(\hat{\delta}) + \frac{\hat{\delta}^2}{\hat{\pi}^4} var(\hat{\pi})$ with $\hat{\pi}$ being the first-stage coefficient and $\hat{\delta}$ the reduced form coefficient.

4.2 County-Level Data and Instrument Construction

The instrument and further county-level controls are merged to the Microcensus and the SOEP, respectively. For the construction of the instrument we extract school- and year-specific information on the nationwide ASP investment program from reports issued by the Federal Ministry of Education and Research (see BMBF, 2010). By these means, we obtain a list of all primary schools that received an ASP grant for every year of the program. Since Berlin and Bavaria did not report the year of support, we have to exclude both states, which reduces the analysis to the nine remaining West German states.

The school names and the respective identifiers from the federal reports are merged to school address registers provided by each of the nine states. Thus, we can identify the county where a school is located through geocoding of the addresses. Subsequently, we sum up for each year and county the number of initial ASP grants that have been issued to the primary schools up to the respective year. Thereby, we implicitly assume that a school continues running an ASP if it received a grant in any prior year. This reflects the idea of the federal program to permanently expand the available number of ASPs through investments into the infrastructure.

In the next step, we divide the annual sum of ASP grants in a county by the actual number of primary schools. The resulting ratio measures the county-specific strength of the federal program in a specific year and serves as our instrument. We directly merge this variable to an observation if the respective household interview was conducted in the second half of the calendar year. If the interview took place in the first half of the year, we use the respective information of the prior year. By doing so we aim to capture the time structure of the German school year, which usually ends in July. Besides the instrument, we include further county-level control variables that we obtained from the regional database of the federal statistical office, such as the unemployment rate and GDP. Summary statistics of the included control variables, the instrument, and the outcome variables are shown in Table 4 of the Appendix.

5 Results

5.1 Descriptives on Maternal Labor Market Participation in West Germany

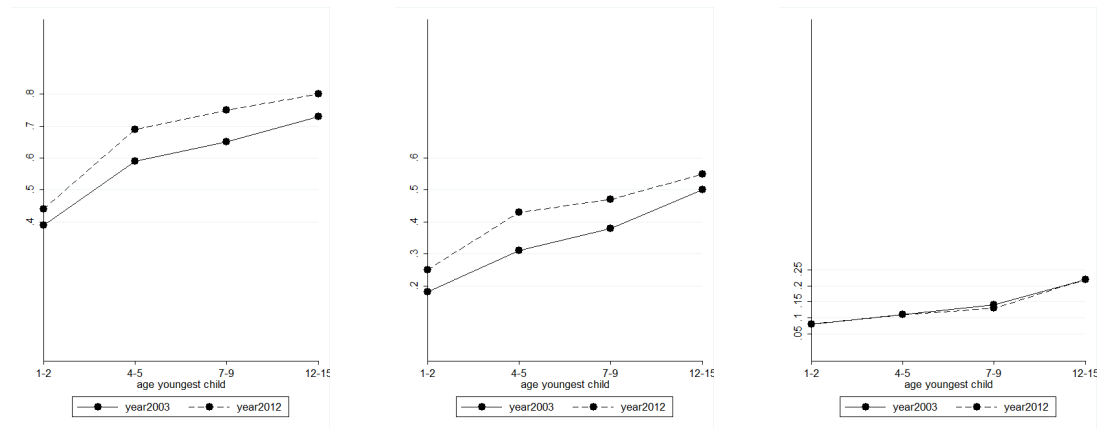
Mothers' labor market participation increases with the age of their youngest child. The solid line in Panel A of Figure 3 depicts the rate of mothers in our sample who work at least one hour in 2003 for different age groups of the youngest child. The rate increases from 39% when the youngest child is 1 or 2 years old to 59% when the child is 4 to 5 years old (i.e., the age at which almost all children go to kindergarten). Sixty-five percent of mothers whose youngest child is aged 7 to 9 years (primary school-age) work at least one hour, and the rate finally reaches 73% when the youngest child is 12 to 15 years old.

Figure 3: Labor Market Participation by Age of Youngest Child

Panel A: At least 1 hour

Panel B: At least 20 hours

Panel C: At least 35 hours



Note: Figures are based on the Microcensus and show the labor market participation of mothers by the age of the youngest child for the years 2003 and 2012, respectively. Following the sample definition, all West German states are included, except Bavaria and Berlin.

The solid line in Panel B (Panel C) shows the rate of mothers working at least 20 hours (35 hours) in 2003; again separately for different age groups of the youngest child. Thirty-eight percent of mothers whose youngest child is aged 7 to 9 work at least 20 hours and 13% at least 35 hours. Interestingly, the propensity to work at least 35 hours (full-time employment rate) increases only

marginally from kindergarten to primary school. The increase is more pronounced for mothers with children aged 7 to 9 years and 12 to 15 years. This denotes the age when parents consider it acceptable to leave a child unattended at home for some hours. When the care restriction in the afternoon is relaxed by the massive ASP expansion, these mothers could consider it worthwhile to take up a full-time job again, even though the child is still in primary school. Overall, there seems to be a large potential for additional hours in West Germany in 2003, but child care is of course not the only determining factor.

The dashed lines in Figure 3 depict the same rates for the year 2012; thus, at a point in time when the ASPs created through the funding program are already in place. In Panels A and B we see that the share of women working at least one hour (or 20 hours, respectively) has increased. Compared to the reference year 2003, the employment rate of mothers whose youngest child is primary school age is 10 percentage points higher in 2012. Yet, this increase is a bit larger for mothers of 4- to 5-year-old children, and it is also existent for mothers with children below 3 years. Thus, the increase may be unrelated to the expansion of ASPs, reflecting solely a cohort effect that occurs because more women start employment when the child is young (possibly due to increased child care options for children below school age) and then keep working regardless of ASP.

This reasoning is supported by a closer look at panel data from the SOEP. While some stay-at-home mothers start employment when their child enters school, we observe very few mothers who stop working when their child progresses from kindergarten to primary school – a pattern that also occurs before the expansion of ASPs. Even though the child care situation deteriorates in primary school, it does not seem that mothers who worked during the kindergarten time give up work once their child is in school.

Panel C contrasts maternal full-time employment rates of the year 2003 (solid line) with the year 2012 (dashed line). Within each age group, almost no additional mothers increased their working time to at least 35 hours. Even though it is a relatively naïve before-and-after comparison, it may suggest that there is po-

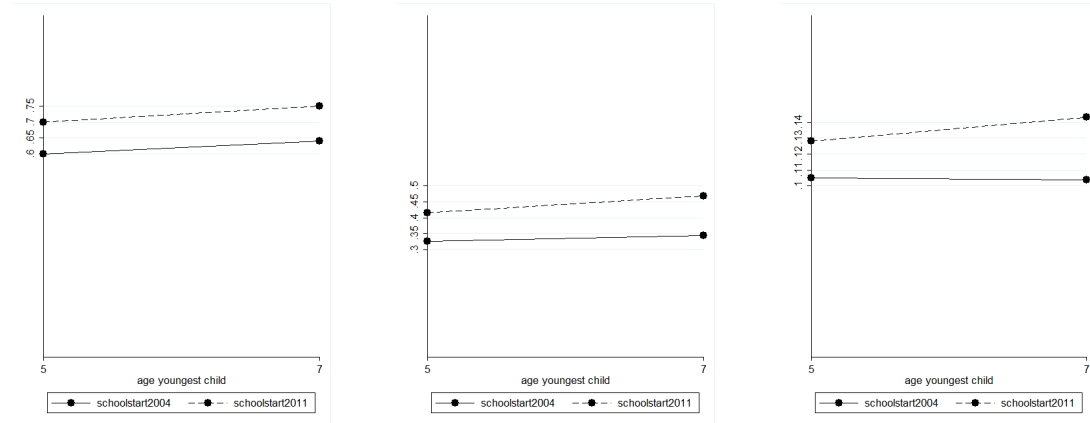
tentially no impact of additional ASP places on full-time employment of mothers with a child in primary school.

Figure 4: Employment Rate by Cohort

Panel A: At least 1 hour

Panel B: At least 20 hours

Panel C: At least 35 hours



Note: Figures are based on the Microcensus and show the labor market participation for two different cohorts. Following the sample definition, all West German states are included (except Bavaria and Berlin). The 2004 (2011) cohort includes all mothers whose youngest child has the age for starting school (i.e., 6 years old) in 2004 (2011).

The solid line in Panel A of Figure 4 depicts the employment rate (at least one hour) for a cohort of mothers whose youngest child has the age for starting school (6 years old) in 2004, that is, in a year at the very beginning of the ASP expansion. Only 4 out of 100 mothers in this cohort begin employment when their youngest child is between 5 and 7 years old. This finding is compared with mothers whose youngest child started school most likely in the year 2011 (dashed line), that is, when many ASPs are already in place. The employment rate for these mothers is higher for both age categories of the youngest child. In addition, the dashed line being slightly steeper than the solid line could also indicate that a few additional mothers start employment due to the new ASPs when their child enters school. A similar pattern is observed for working at least 20 hours (Panel B) and working at least 35 hours (Panel C).

5.2 Main Results

OLS Results

Table 1 summarizes the main results. A detailed overview of all the coefficients and standard errors is provided by Table 5 in the Appendix for our preferred specification.

The first line in Table 1 (Panel A) depicts the OLS results for the effect of ASP on hours worked estimated with the SOEP sample.^{20,21} Hours are set to zero if the individual does not work. The first regression (first column) contains only time dummies (i.e., dummies for years and calendar quarters) as well as state dummies as control variables and suggests that ASP participation is related to approximately 7 additional hours worked per week. The coefficient decreases slightly to 5.5 hours when individual- and county-level covariates are added, and it basically stays the same when county fixed effects are finally included. The second line of Table 1 suggests that ASP participation is related to a 19 p.p. increase in the probability of regular employment (to work at least 10 hours). As soon as additional covariates (and county fixed effects) are controlled for, the estimate is 16 p.p.. All OLS results are highly significant and suggest that, after observable characteristics are controlled for, mothers of children attending an ASP work more than mothers whose children do not attend an ASP.

First-Stage Results

The first-stage estimates of our IV approach indicate that the ratio of schools in a county receiving ASP grants is strongly related to ASP participation in the following years.

²⁰Working hours refer to the usual amount of hours worked in a normal week.

²¹Notice that we cannot estimate OLS regressions of the effect of ASP on maternal outcomes with the Microcensus because individual information on ASP attendance is not included in this data set (see section 4.1).

Table 1: Main Results

| | (1) | (2) | (3) | (4) | (5) |
|--|------------------|------------------|------------------|------------------|------------------|
| Panel A: OLS (<i>SOEP</i>) | | | | | |
| <i>Hours worked</i> | | | | | |
| ASP attendance | 6.905 (0.928)*** | 5.477 (0.834)*** | 5.408 (0.833)*** | 5.626 (0.830)*** | 5.612 (0.835)*** |
| <i>Employment</i> | | | | | |
| ASP attendance | 0.191 (0.030)*** | 0.154 (0.027)*** | 0.154 (0.027)*** | 0.160 (0.027)*** | 0.160 (0.027)*** |
| Observations | 5 103 | 5 103 | 5 103 | 5 103 | 5 103 |
| Panel B: 1 st stage (<i>SOEP</i>) | | | | | |
| <i>ASP attendance</i> | | | | | |
| Ratio of schools funded by ASP grants | 0.269 (0.042)*** | 0.254 (0.039)*** | 0.235 (0.034)*** | 0.224 (0.038)*** | 0.229 (0.037)*** |
| 1 st stage F-statistic | 41.216 | 43.165 | 47.334 | 34.457 | 37.577 |
| Observations | 5 103 | 5 103 | 5 103 | 5 103 | 5 103 |
| Panel C: Reduced form (<i>Microcensus</i>) | | | | | |
| <i>Hours worked</i> | | | | | |
| Ratio of schools funded by ASP grants | 0.232 (0.354) | 0.184 (0.276) | 0.179 (0.254) | 0.118 (0.268) | 0.118 (0.270) |
| <i>Employment</i> | | | | | |
| Ratio of schools funded by ASP grants | -0.009 (0.013) | -0.005 (0.010) | 0.002 (0.010) | -0.001 (0.010) | -0.000 (0.010) |
| Observations | 140 071 | 140 071 | 140 071 | 140 071 | 140 071 |
| Panel D: TS2SLS (<i>Microcensus</i> / <i>SOEP</i>) | | | | | |
| <i>Hours worked</i> | | | | | |
| ASP attendance | 0.863 (1.322) | 0.727 (1.093) | 0.763 (1.088) | 0.529 (1.200) | 0.514 (1.180) |
| <i>Employment</i> | | | | | |
| ASP attendance | -0.035 (0.048) | -0.019 (0.041) | 0.010 (0.042) | -0.003 (0.044) | -0.000 (0.043) |
| Time dummies | yes | yes | yes | yes | yes |
| State dummies | yes | yes | yes | | |
| Individual covariates | | yes | yes | yes | yes |
| County covariates | | | yes | | yes |
| County fixed effects | | | | yes | yes |

Note: Regressions are run over the period from 2003-2012. The sample consists of mothers aged 20 to 60 years who have at least one child in primary school. OLS regressions as well as first-stage, reduced form, and TS2SLS regressions are shown separately. Individual covariates include mother-, child- and household-specific controls. County covariates refer to control variables aggregated at the county level and time dummies to yearly as well as quarterly dummies. Standard errors are shown in parentheses: For OLS, first stage and reduced form they are clustered at the county level; for the TS2SLS estimates they are calculated following the delta method. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

According to Panel B of Table 1, the effect of the instrument z on the probability of participating in an ASP is estimated to be 27 p.p. if only state and time dummies are controlled for. The effect is highly significant and the F-statistic is sufficiently large, indicating that our estimates do not suffer from the problem of weak instruments. The coefficient proves robust and reduces only slightly to 25 p.p. and finally to 23.5 p.p. if individual- and county-specific covariates are included successively. The last specification (fifth column) with covariates on the individual and county level as well as county fixed effects is our preferred specification, and it can be interpreted as follows. If the county-specific ratio of schools funded by ASP grants rose from 0 to 1, the child’s probability to attend an ASP would increase by 23 p.p..²² The first-stage estimates of our preferred specification is significant at a 1% level, and the F-statistic is 38. Throughout all first-stage as well as reduced form estimates, standard errors are clustered at the county level.

IV-Results

Panel C in Table 1 presents the reduced form estimates based on the Microcensus. This data set is much larger than the SOEP; the respective sample of mothers with children in primary school includes 140 071 observations compared to 5 103. For the first line of Panel C, we regress hours worked on the instrument z and for the second line of Panel C the regular employment dummy on z . All results are very small and insignificant. To evaluate the effect size, it is helpful to look at the TS2SLS results in Panel D. Column 1 suggests that ASP participation increases hours worked on average by 0.86. The identified effect represents a LATE for the subgroup of compliers, that is, the employment effect for mothers who take an offered ASP place that was induced by grants directed to the county where they live. Since the magnitude of the coefficient is very small, it is not surprising that it proves insignificant despite the large sample size for the reduced form and the strong first stage. The inclusion of additional covariates (columns 2 and 3) does not change the results, and in our preferred specification with county fixed-effects

²²Note, however, that the largest annual increase observed in the data is much smaller.

(column 5), the coefficient amounts to 0.51 hours. The 95% confidence interval ranges from -1.80 to 2.83 hours.

The effect is thus always close to zero, independent of the specification, such that the null hypothesis of a zero effect cannot be rejected. Our estimates do not suffer from imprecise estimation or a weak instrument; rather, the estimated effect on hours worked is very close to zero. Thus, we conclude that ASP has on average no effect or at most a very small effect on hours worked. The second line of Panel D shows the TS2SLS results on the employment probability. Depending on the specification, we identify a slightly negative or slightly positive effect, which is always insignificant. Our preferred specification suggests that the employment probability of a mother who accepts an offered ASP place is approximately -0.01 p.p.. We take these estimates as evidence that stay-at-home mothers are not induced to start working if their child attends an ASP.

In sum, the statistically insignificant and economically negligible IV estimates indicate that our OLS results suffer from a strong positive upward bias because parents decide whether to apply for an ASP place based on their unobserved work preferences. We believe that this conclusion can be made even though it requires comparing the LATE to the ATE estimated by our OLS regression. If the effect for those mothers who accept an offered ASP place is zero, the causal effect for the overall population of mothers is unlikely to be positive.

At first glance this outcome seems to contradict Gambaro et al. (2016) since they find positive effects on the employment probability and on hours worked. Despite a large set of observable characteristics, the inclusion of prior employment patterns, and several robustness checks, they cannot rule out that their estimates are upward biased due to selection on unobservables or reverse causality. Consider two stay-at-home mothers with the same observable characteristics who want to start working once the child is in school age. However, one mother has a greater preference for work or is luckier with job opportunities (e.g., her former employer has a vacancy to fill). This mother is thus more likely to start working once her

child is in school, and at the same time she is possibly also more likely to accept an offered ASP slot because she needs to organize after-school supervision.²³

Nemitz (2015) finds a strong positive effect of ASP on the employment probability using SOEP data and applying bivariate probit models.²⁴ These results contradict ours. One potential reason might be that the SOEP sample is too small to disentangle the effect of the instrument from the effect of time on the outcome variable.

5.3 Effect Heterogeneity

In this subsection, we investigate the effect of ASP on maternal labor supply along different dimensions. We check whether employed mothers or mothers without a young child behave differently and whether the results vary by nationality, family status, or education level. The precise regression outputs are shown for each subgroup separately in Table 6 of the Appendix and are briefly explained in the following.

Mothers without a child younger than the one in primary school might react more notably to an offered ASP place because they do not have further care obligations. We also condition our sample on employed mothers only and investigate the impact of ASP on normal working hours that are greater than 0. The respective coefficients are shown for both subsamples in columns 1 and 2 of Table 6. They remain insignificant and close to zero such that the null hypothesis of a zero effect cannot be rejected. For low and medium education (columns 5 and 6), we do not find significant effects either. The results with respect to the remaining dimensions (i.e., nationality, family status, and high education) are also listed in Table 6 (columns 3, 4, and 7) but we refrain from interpreting them because of weak first stages.

²³An upward bias in cases like this is not ruled out by Oster bounds or the robustness check on a specific case of reverse causality that Gambaro et al. (2016) present.

²⁴Nemitz (2015) additionally presents a large positive 2SLS estimate obtained with the SOEP data as some additional evidence for the positive effect. But, as she acknowledges, this estimate suffers from huge standard errors. In fact, the 95% confidence interval goes from -39 p.p. to +97 p.p. around the point estimate of the employment probability.

5.4 Robustness Checks

As shown by Table 1, the results prove robust across all specifications. Even the simplest version with time and state dummies delivers IV estimates around zero. In the present subsection, we provide several robustness checks to challenge our findings and the underlying conditional exogeneity assumption. First, we estimate the reduced form for women without children to check that our results are not due to other trends or interventions enhancing the female working conditions in treated counties. In a second and similar step, we estimate the reduced form for mothers who have a child younger than 5 but no older child that could potentially attend a primary school.

Table 2: Reduced Form Robustness Checks

| | (1) | (2) | (3) |
|---------------------------------------|----------------------------------|--------------------------------|------------------|
| | women without child <16 years | mothers with child <5 years | baseline |
| | OLS | OLS | Probit |
| <i>Hours worked</i> | | | |
| Ratio of schools funded by ASP grants | 0.083 (0.129) | 0.026 (0.367) | |
| <i>Employment</i> | | | |
| Ratio of schools funded by ASP grants | 0.004 (0.003) | -0.003 (0.011) | -0.0001 (0.0226) |
| Observations | 660 978 | 103 152 | 140 071 |
| Time dummies | yes | yes | yes |
| Individual covariates | yes | yes | yes |
| County covariates | yes | yes | yes |
| County fixed effects | yes | yes | yes |

Note: Regressions are run over the period from 2003-2012 using the Microcensus. The underlying sample is indicated for each regression separately. Individual covariates include woman-, (or mother-, child-,) and household-specific controls. County covariates refer to control variables aggregated at the county level and time dummies to yearly as well as quarterly dummies. Standard errors are shown in parentheses: For OLS they are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The reduced form estimates are shown for both robustness checks in columns 1 and 2 of Table 2. The respective coefficients turn out to be insignificant and close to zero, which strengthens the untestable exogeneity assumption. In a third step, we construct the instrument with a one-year lag to exclude any immediate influence by third parties (e.g., parents who pushed an application through the school council). In a fourth test, we aggregate the county-specific employment

patterns of mothers with a child in primary school and include lagged average maternal working hours as well as the lagged regular maternal employment probability. By these means we rule out any strategic expansion patterns that consider potential changes in the maternal demand for ASPs. In a fifth and similar test, we include the share of people of foreign nationalities residing in a county because it might be correlated with the funding intensity as stated earlier (see subsection 3.2). The precise coefficients of these robustness checks are shown in columns 1 to 3 of Table 3. They provide similar results as our preferred specification, which strengthens our findings.

The next two robustness checks stress the impact of differential trends in a further way. In Germany, federal states can target their educational policies independently. Even within the nationwide investment program, federal states have the option to foster the ASP expansion of certain school types. These funding priorities could be correlated with other state-specific policies that aim to improve the care conditions for primary school children. By including additional state-by-year interactions, we rule out that our instrument takes up any of these confounding trends or shocks. In a similar test, we interact the degree of urbanization with year dummies to account for differences in trends that might have occurred within more rural or urban counties (Wiezorek et al., 2011). For both robustness checks we report the respective results in columns 4 and 5 of Table 3. The inclusion of year-by-state interactions reduces the effect slightly if working hours are considered. Overall, however, the coefficients remain insignificant and close to zero, which strengthens our previous conclusion. In an eighth step we check whether newly created ASPs crowd out publicly sponsored external child care (*Horte*). In West Germany, the number of available places in external care increased from 2002 by approximately 12 000 places to a total of 192 000 in 2009.²⁵ Thus, at the aggregate level no evidence exists that ASPs have crowded out external care.

²⁵See “Child and Youth Services” statistics provided by Statistisches Bundesamt (2005) and Statistisches Bundesamt (2009).

Table 3: Robustness Checks

| | (1) | (2) | (3) | (4) | (5) | (6) |
|--|------------------|------------------|------------------|------------------|------------------|--|
| Panel A: 1 st stage (<i>SOEP</i>) | | | | | | |
| <i>ASP attendance</i> | | | | | | |
| Ratio of schools funded by ASP grants | | 0.230 (0.037)*** | 0.229 (0.037)*** | 0.234 (0.073)*** | 0.242 (0.043)*** | 0.229 (0.037)*** |
| Lagged ratio of schools funded by ASP grants | 0.228 (0.036)*** | | | | | |
| 1 st stage F-statistic | 40.196 | 37.946 | 37.823 | 10.240 | 31.584 | 37.577 |
| Observations | 5 103 | 5 103 | 5 103 | 5 103 | 5 103 | 5 103 |
| Panel B: Reduced form (<i>Microcensus</i>) | | | | | | |
| <i>Hours worked</i> | | | | | | |
| Ratio of schools funded by ASP grants | | 0.033 (0.219) | 0.110 (0.269) | -0.305 (0.649) | 0.010 (0.283) | <i>Actual hours</i> 0.099 (0.262) |
| Lagged ratio of schools funded by ASP grants | 0.080 (0.259) | | | | | |
| <i>Employment</i> | | | | | | |
| Ratio of schools funded by ASP grants | -0.002 (0.008) | 0.000 (0.010) | -0.001 (0.021) | 0.005 (0.011) | 0.006 (0.010) | <i>Employment (0h thrsh.)</i> 0.006 (0.010) |
| Lagged ratio of schools funded by ASP grants | -0.003 (0.010) | | | | | |
| Observations | 140 071 | 140 071 | 140 071 | 140 071 | 140 071 | 140 071 |
| Panel C: TS2SLS (<i>Microcensus</i> / <i>SOEP</i>) | | | | | | |
| <i>Hours worked</i> | | | | | | |
| ASP attendance | 0.349 (1.135) | 0.145 (0.952) | 0.479 (1.175) | -1.304 (2.740) | 0.042 (1.166) | <i>Actual hours</i> 0.432 (1.147) |
| <i>Employment</i> | | | | | | |
| ASP attendance | -0.013 (0.043) | -0.011 (0.034) | 0.000 (0.043) | -0.006 (0.091) | 0.020 (0.045) | <i>Employment (0h thrsh.)</i> 0.026 (0.045) |
| covariates as in preferred specification | yes | yes | yes | yes | yes | yes |
| lagged aggregated maternal employment | | yes | | | | |
| aggregated rate of foreign nationalities | | | yes | | | |
| year-by-state inter. | | | | yes | | |
| region-type inter. | | | | | yes | |

Note: Regressions are run over the period from 2003-2012. The sample consists of mothers aged 20 to 60 years who have at least one child in primary school. First-stage, reduced form and TS2SLS regressions are shown separately. The included covariates differ across specifications. Standard errors are shown in parentheses: For the first stage and reduced form they are clustered at the county level; for the TS2SLS estimates they are calculated following the delta method. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Within federal states, however, the expansion of after-school child care could have been pursued either by ASPs or by publicly sponsored external child care. Counties that did not receive ASP funding could still have pursued the opening of new external child care centers for school-aged children. Within states, such an alternative treatment strategy would imply a strong correlation between the growth rates of external child care centers and ASPs, which we do not observe for the given years.²⁶

The subsequent robustness checks prove the stability of our results with regard to outcome and model specifications. As a ninth test, we estimate the reduced form for the regular employment dummy by a probit model (column 3 of Table 2). Tenth, we redefine the employment dummy. Instead of regular employment, which is defined to be greater than 10 working hours, we set the threshold at zero hours. In a further step, we adjust the definition of working hours and consider the actual number of hours that a mother worked during the reference week, instead of referring to the usual amount that she is supposed to work in a normal week. The coefficients of these tests are shown in column 5 of Table 3. They are in line with our preferred specification such that the null hypothesis of a zero effect cannot be rejected.

As a final check, we analyze the individual moving behavior in the SOEP data to check if mothers strategically relocate to counties with a higher funding ratio. Our first observation is that moving plays a minor role. In our underlying sample just 2% of all mother-child pairs ever change their county of residence. This amounts to 36 relocations in total, which are divided into 19 (13) moves to counties with a higher (lower) funding ratio and four moves within zero funded counties. Since the settlement decision is likely to occur at an earlier stage, we also look at mothers whose youngest child is under 6 years. The moving propensity is indeed slightly higher and amounts to 5.8% or 116 moves in total. As for the previous subgroup, we cannot identify a strategic behavior either, since 52

²⁶County-specific data on external child care centers are extracted from the regional database of the Federal Statistical Office. The limited number of available years restricts the comparison to the last 3 years of the federal investment program (2007 to 2009).

(49) mothers move to higher (lower) funded counties and 15 within zero funded counties.

6 Conclusion

In countries with half-day school systems, a lack of child care availability may be one reason for low full-time employment rates of mothers with school-age children. In West Germany, the care restriction for primary school children has been relaxed during the last decade by an extensive expansion of ASPs. In this paper, we investigate the impact of additional ASP places on maternal employment. We provide an ex post evaluation of the extensive and costly expansion of ASP slots in Germany with regard to maternal labor market outcomes and add to the scarce literature on the causal employment effects of the child care availability for school-age children. In particular, we estimate the effect of a child's attendance in a newly created ASP place on the mother's working hours and employment probability. Using a TS2SLS approach, we combine data from the SOEP as well as the much larger Microcensus and exploit the successive expansion of ASPs in West Germany through a nationwide investment program to identify a causal effect.

Our results suggest that the reform had no impact on maternal employment. We find that sending a child to an ASP has no causal effect on the employment probability for mothers who are offered one of the additional places and accept it. There is also no evidence for any effect on working hours. Results are stable across different subsample specifications and robustness checks. Thus, the massive expansion of school-based low-cost child care in the West German context of half-day schools did not induce stay-at-home mothers to start work or part-time mothers to increase working hours. This lack of evidence for an adaption of maternal employment outcomes due to afternoon care does not contradict an immediate occupancy of newly created places and a positive acceptance of ASPs. Based on our findings, we conclude that the lack of child care is not solely re-

sponsible for the relatively low number of hours worked by mothers of school-age children in West Germany.²⁷

The descriptive statistics show that maternal labor market participation increased from 2003 to 2012 for mothers with children of primary school age who worked at least one hour or at least 20 hours. Our main results indicate that this rise cannot be explained by the expansion of ASPs and must be due to other factors. One potential mechanism might be an increased availability of preschool child care, which induced mothers of young children to take up a part time-employment. These mothers may stay in the labor market (in part-time) once the child reaches school age regardless of ASP availability. Based on the descriptives we further observe that the share of mothers with children of primary school age who work at least 35 hours a week has (almost) stayed stable during the 10 years in focus. This finding is in line with our IV results, which suggest no impact of the availability of ASPs on maternal working hours. Thus, a lack of afternoon child care was not a critical hurdle preventing mothers from working full time. Other institutional characteristics, such as disincentives in taxation and the social security system (e.g., marital tax splitting), might be hurdles that are more important to an increased labor market engagement than the lack of afternoon child care. Further, a large majority of West German mothers may not want to work full time given an existent family income level.

At this point, it remains an open question why parents send their children to an ASP. As opposed to the afternoon child care institutions studied by Felfe et al. (2016), the German program requires parents to pay only a small fee. Furthermore, many parents seem to believe that ASP attendance is beneficial for their child (see Heimar et al., 2011). Thus, parents might accept an offered place due to pedagogical reasons or simply to reduce their care obligations.

But how does a reduction in child care change parental time use? Fathers usually work full time in Germany, thus there is little potential for an increase in hours worked. Parents probably make less use of informal care (e.g., by grandpar-

²⁷The majority of years included in our sample are characterized by a decreasing unemployment rate in Germany. Therefore, it is very unlikely that only a few mothers could achieve increased labor market engagement.

ents). However, it is not possible to quantify this potential effect because no data on the number of hours of informal care is available. Finally, parents may enjoy more leisure time by sending their child to an ASP. The SOEP includes some rough information on time use, which confirms (on a purely descriptive level) that mothers who send their child to an ASP spend less time on child care. However, the lack of available information and the small sample size make it impossible to find out how the additional time is used.

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Appendix

Table 4: Summary Statistics

| | MEAN | SD |
|---|--------|--------|
| <i>ASP funding rate</i> | 0.223 | 0.325 |
| <i>regular employment</i> | 0.543 | 0.498 |
| <i>hours worked</i> | 13.036 | 13.272 |
| <i>age of mother</i> | 37.940 | 5.360 |
| <i>foreign nationality (dummy)</i> | 0.148 | 0.355 |
| <i>low education (dummy)</i> | 0.182 | 0.386 |
| <i>medium education (dummy)</i> | 0.693 | 0.461 |
| <i>high education (dummy)</i> | 0.125 | 0.330 |
| <i>single household (dummy)</i> | 0.139 | 0.346 |
| <i>age of child</i> | 8.014 | 1.285 |
| <i>sex of child (male-dummy)</i> | 0.510 | 0.500 |
| <i>younger child in household (dummy)</i> | 0.353 | 0.478 |
| <i>number of further children</i> | 1.196 | 0.972 |
| <i>county covariates of the previous year:</i> | | |
| <i>gdp ($\div 10^6$)</i> | 11.926 | 15.925 |
| <i>unemployment rate</i> | 0.081 | 0.029 |
| <i>avg. hours worked by women without child</i> | 23.448 | 2.035 |
| <i>avg. employment ratio by women without child</i> | 0.693 | 0.050 |

Note: The presented summary statistics are based on the German Microcensus. The sample consists of mothers aged 20 to 60 years who have at least one child in primary school. The time period ranges from 2003 to 2012 and includes 140 071 observations.

Table 5: Preferred Specification Showing all Covariates

| | (1) | (2) | (3) |
|---|-----------------------------|---------------------|---------------------|
| | <i>1st stage</i> | <i>reduced form</i> | |
| | ASP attendance | regular employment | hours worked |
| <i>ASP funding rate</i> | 0.2291 (0.0374)*** | -0.0001 (0.0099) | 0.1179 (0.2696) |
| <i>age of mother</i> | -0.0033 (0.0014)** | 0.0014 (0.0004)*** | 0.0154 (0.0112) |
| <i>foreign nationality (dummy)</i> | 0.0253 (0.0208) | -0.1195 (0.0061)*** | -2.5607 (0.1680)*** |
| <i>medium education (dummy)</i> | -0.0092 (0.0164) | 0.1829 (0.0050)*** | 4.3297 (0.1298)*** |
| <i>high education (dummy)</i> | 0.0669 (0.0278)** | 0.2993 (0.0074)*** | 9.3909 (0.2193)*** |
| <i>single household (dummy)</i> | 0.0990 (0.0256)*** | 0.0186 (0.0062)*** | 2.3995 (0.1731)*** |
| <i>age of child</i> | -0.0055 (0.0030)* | 0.0080 (0.0011)*** | 0.2376 (0.0292)*** |
| <i>sex of child (dummy)</i> | 0.0073 (0.0099) | -0.0013 (0.0030) | -0.0619 (0.0830) |
| <i>younger child in household (dummy)</i> | -0.0027 (0.0126) | -0.1176 (0.0042)*** | -3.1974 (0.1144)*** |
| <i>number of further children</i> | -0.0276 (0.0079)*** | -0.0810 (0.0018)*** | -2.1099 (0.0569)*** |
| <i>county covariates of the previous year:</i> | | | |
| <i>gdp ($\div 10^6$)</i> | -0.0026 (0.0064) | -0.0021 (0.0013) | 0.0393 (0.0369) |
| <i>unemployment rate</i> | -1.0895 (0.8956) | -0.0298 (0.2673) | -3.5775 (6.7757) |
| <i>avg. hours worked by women without child</i> | 0.0040 (0.0083) | 0.0063 (0.0030)** | 0.1815 (0.0830)** |
| <i>avg. employment ratio by women without child</i> | 0.0622 (0.2802) | -0.1741 (0.1207) | -6.2765 (3.0542)** |
| <i>time dummies</i> | yes | yes | yes |
| <i>county fixed effects</i> | yes | yes | yes |
| <i>1st stage F-stat.</i> | 37.577 | | |
| <i>Observations</i> | 5 103 | 140 071 | 140 071 |
| <i>R²</i> | 0.0649 | 0.1215 | 0.1282 |

Note: Regressions are run over the period 2003-2012. The sample consists of mothers aged 20 to 60 years who have at least one child in primary school. First-stage and reduced form estimates are based on the SOEP and the Microcensus, respectively. Standard errors in parentheses are clustered at the county level: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Effekt Heterogeneity

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|--|-------------------|------------------|------------------|---------------------|------------------|------------------|----------------|
| | w/o younger child | employed | single household | foreign nationality | low education | medium education | high education |
| Panel A: 1 st stage (<i>SOEP</i>) | | | | | | | |
| <i>ASP attendance</i> | | | | | | | |
| Ratio of schools funded by ASP grants | 0.217 (0.048)*** | 0.310 (0.053)*** | 0.188 (0.190) | 0.143 (0.088) | 0.214 (0.081)*** | 0.211 (0.042)*** | 0.227 (0.122)* |
| 1 st stage F-statistic | 20.794 | 34.457 | 0.980 | 2.690 | 6.970 | 25.301 | 3.460 |
| Observations | 3313 | 2979 | 645 | 733 | 968 | 3519 | 616 |
| Panel B: Reduced form (<i>Microcensus</i>) | | | | | | | |
| <i>Hours worked</i> | | | | | | | |
| Ratio of schools funded by ASP grants | -0.138 (0.325) | 0.134 (0.301) | 0.562 (0.772) | -0.336 (0.652) | 0.229 (0.524) | 0.336 (0.302) | -0.367 (0.841) |
| <i>Employment</i> | | | | | | | |
| Ratio of schools funded by ASP grants | -0.008 (0.012) | 0.041 (0.025) | 0.041 (0.025) | -0.017 (0.024) | 0.009 (0.019) | 0.008 (0.012) | -0.035 (0.027) |
| Observations | 90651 | 80235 | 19410 | 20697 | 25571 | 97039 | 17461 |
| Panel C: TS2SLS (<i>Microcensus</i> / <i>SOEP</i>) | | | | | | | |
| <i>Hours worked</i> | | | | | | | |
| ASP attendance | -0.638 (1.491) | 0.433 (0.974) | 2.984 (5.087) | -2.343 (4.313) | 1.072 (2.483) | 1.596 (1.467) | -1.616 (3.600) |
| <i>Employment</i> | | | | | | | |
| ASP attendance | -0.035 (0.053) | 0.217 (0.257) | 0.217 (0.257) | -0.119 (0.149) | 0.044 (0.090) | 0.040 (0.056) | -0.152 (0.084) |
| Covariates as in preferred specification | yes | yes | yes | yes | yes | yes | yes |

Note: Regressions are run over the period from 2003-2012. The underlying sample is indicated for each regression separately. First-stage, reduced form and TS2SLS regressions are shown separately. The included covariates are the same as in our preferred specification. Standard errors are shown in parentheses: For the first stage and reduced form they are clustered at the county level; for the TS2SLS estimates they are calculated following the delta method. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.