KYKLOS, Vol. 72 – February 2019 – No. 1, 118–151

School Hours and Maternal Labor Supply

Nikki Shure*

I. INTRODUCTION

For many families, and mothers in particular, the fact that the length of the primary school day does not correspond to the length of the working day makes combining work and family challenging. While few countries have primary school days as long as the working day, policymakers continue to discuss extensions as a way to subsidize working families and increase female labor supply.[1]

This paper examines such an extension of the primary school day to see how it affects maternal labor supply. I exploit a recent reform in Germany, which extended the length of the primary school day by approximately two hours per day, an average of 10 hours per week, to examine whether mothers who gain access to a or "full day school" are more likely to enter the labor market if not working or extend their hours worked if already working.[2]

I use a self-collected school level data set, with information on all primary schools in four German states, and link it to the German Socio-economic Panel (GSOEP) using geographic information software (GIS). I observe women before and after they gain access to a full day school, which allows me to estimate a difference-in-difference model and causally estimate the effect of increasing the length of the primary school day on maternal labor supply. Because of the

*Nikki Shure Lecturer (Assistant Professor) in Economics Department of Social Science University College London, Institute of Education. Email: nikki.shure@ucl.ac.uk or nikki.shure@gmail.com. I would like to thank the two reviewers, seminar participants at the University of Oxford, UCL Institute of Education, 2014 International Workshop on Applied Education Economics, and IZA. Brian Bell, Margaret Stevens, Patricia Rice, Sandra McNally, Arik Levinson, Nick Papageorge, Dan Hamermesh, Jan van Ours, Alex Bryson, Damian Clarke, and Federico Torracchi provided helpful comments and advice. Thank you to the German Institute of Economic Research for hosting me and especially Jan Goebel for the help in linking my data to the GSOEP. This research was undertaken with the help of a 2014 Santander Travel Grant.

[1]In Germany, Gerhard Schröder campaigned on the extension of the primary school day as a way to help working families and Chicago Mayor Rahm Emanuel unsuccessfully proposed extending Chicago's primary school day to 7.5 hours per day (Neufeld, 2014).

[2]Before the reform came into effect in 2002, a typical German school day began around 8 am and ended at approximately 1 pm. After the reform, the school day now ends at approximately 3 pm (Stecher et al. 2008). The reform is still on-going. See the Appendix for a full overview of the reform.

slow and staggered nature of this reform, treatment – access to a full day school – is assigned to mothers at different points in time and I estimate an intention to treat (ITT) effect. Since there is no primary school choice in Germany and most children attend their closest school, I am able to precisely identify treatment in a way not previously done to examine this type of implicit childcare subsidy.

I find that women who were not working before they gain access to a full day school are nearly five percentage points more likely to enter the labor market once they gain access, but that for women already working, there is no statistically significant effect on the hours they work. This is an important result for a policy partially aimed at helping working families and increasing maternal labor supply along the extensive and intensive margins, especially since this is a costly reform. The results in this paper are robust to a series of checks on the identification strategy. Importantly, the evidence from this paper can inform policy debates on proposals to extend the primary school day by smaller increments, which do not coincide with the length of the working day.

The contribution of this paper is twofold. First, it is one of the first papers to causally identify the impact of the extension of the primary school day on maternal labor supply, and second, it examines the impact of extending the school day at the intensive and extensive margins of maternal labor supply. This is an improvement on existing papers on this type of reform, which have not been able to causally estimate the effect of extending the length of the school day. The results have broad policy implications for Germany and beyond as they consider policies to extend the length of the school day, which I return to in the conclusion.

The rest of the paper is structured as follows: in Section II, I situate this paper within the existing literature and in Section III, I discuss a theoretical framework of labor supply. In Section IV, I present the data used in this paper and some descriptive statistics. In Section V, I lay out the empirical strategy and in Section VI, I present the results. In Section VII, I include several robustness checks, followed by a conclusion in Section VIII.

II. RELATED LITERATURE

Much of the existing evidence on childcare and maternal labor supply comes from studies that focus on the early years of a child's life. A range of studies from different countries have shown that when mothers exogenously gain access to pre-school childcare, they enter the labor market or extend their hours if already working. Gelbach (2002) examines public kindergarten enrollment in the United States and finds that married women who gain access to kindergarten are 6-15 percentage points more likely to enter the labor market, regardless of whether or not they have an additional child under five. Lefebvre and Merrigan (2008) and Baker et al. (2008) look at the expansion of universal, highly subsidized

preschool childcare in Quebec, Canada. Lefebvre and Merrigan (2008) find a positive effect of the expansion at both the extensive and intensive margins for mothers with at least one child under the age of five and Baker et al. (2008) find a positive effect at the extensive margin for married mothers with pre-school aged children. For Germany, Geyer et al. (2015) use quasi-experimental and structural methods to show that combining parental leave and subsidized pre-school childcare policies for mothers has had a large, positive effect on their employment.

Women with primary school aged children are less likely to work than women without children, yet there have been fewer studies looking at extending childcare availability to mothers with primary aged children. The papers that exist often look at after-school childcare, not the length of the primary school day. For example, Felfe et al. (2016) find a positive effect of after-school childcare on mothers' probability of working full time in Switzerland while Lundin et al. (2008) find no effect of a cap on the cost of childcare on maternal labor supply in Sweden.

Previous research on changes in the length of the primary school day has been limited due to the small number of countries that have changed the length of the school day. One notable exception to this is Chile. Studies on the Chilean extension find that increasing the length of the school day enables mothers not previously working to enter the labor market and those who were previously working to decrease their hours (Contreras et al. 2010) and that mothers who gain access to a full day school are more likely to enter the labor market and exhibit longer term labor market attachment (Berthelon et al. 2015).

There have been a couple of studies on the German reform studied in this paper, which have produced contradictory results. Rainer et al. (2010) conducted the first large scale study of the German reform and collected data on a sample of families affected by the reform. They find that access to a full day school led mothers to increase their hours, but had no effect on entering into employment. Their sample only includes mothers who gained access to a full day school, so they are only able to use a before and after design and are therefore unable to estimate a causal effect. Additionally, they do not differentiate between mothers with primary and secondary school aged children, which is an issue since they have different labor supply concerns.

Nemitz (2015) also uses the GSOEP and identifies treatment uses spending on the reform at the county level as well as the share of pupils in a given state who attend a full day school as instruments for access to full day schools. She finds that the German reform caused mothers to enter the labor market, but had no effect on hours. The magnitude of her effect size on employment is much larger than what is found in this paper, potentially due to the IV identification strategy. Using money spent at the county level or the proportion of pupils attending a full day school is a much noisier measure of treatment than closest primary school

actually becoming a full day school. Nevertheless, our differing identification strategies produce complementary results.

This paper contributes to the existing literature by exploiting the staggered nature of the reform at the school level, yielding a more precise identification strategy. Since I observe mothers before and after gaining access and some mothers who never gain access, I am able to use a difference-in-difference approach. The advantage of using the GSOEP is that I can also look at women without children and fathers as robustness checks. I measure treatment at the level of the closest primary school unlike previous research, which uses broader regional and time variation in the introduction of implicit childcare subsidies, e.g. funding at the county level. Focusing on the school level reduces some of the concern surrounding the common time trend assumption and the comparability between states or regions across time. I return to this point in Section V when I verify the common time trend assumption for my sample of mothers. This paper also contributes to a small evidence base on the extension of the primary school day, which will continue to prove relevant for other countries as they debate this policy measure.

III. LABOR SUPPLY FRAMEWORK

It is important to consider how extending the length of the primary school day might affect maternal labor supply. There are four different cases to consider: mothers who do not work, mothers who work very few hours per week (less than the length of the primary school day), mothers who set their hours to match the length of the primary school day, and mothers who work more hours than the length of the primary school day.[3]

For mothers previously not working, the implicit childcare subsidy provided by the full day school makes a new portion of their budget constraint available and preferable to not working under the standard assumptions of a static labor supply model (Pencavel 1986). These may have been mothers who could not rely on informal childcare options (e.g. grandparents) or did not allow their children to be "latch-key children", two important phenomena in Germany (Dowideit 2011; Statista 2018), and did not have access to formal childcare options. In this case, the mothers who enter the labor market as a result of the implicit childcare subsidy end up with higher utility and are therefore better off than when they were not working.

In the case of mothers working a very small number of hours, it is unlikely that they will be affected by the reform. They have already set their optimal hours and

[3]For the sake of simplicity, I ignore the fact that the mother is actually a member of a household and therefore maximizing a joint household utility function. In reality her labor supply decisions will be made in the context of her partner's labor supply. Empirically I examine single mothers separately since their labor supply framework may be more similar to the one discussed here.

are utility maximizing; the implicit childcare subsidy does not cause them to substitute away from leisure towards labor.

In the case of mothers who set their working hours to correspond to the length of the school day before the reform, the effect on their hours is unclear. They may not change their hours at all after the school day is extended. These may be mothers who have already set their optimal number of working hours or women who are unable to flexibly change their hours worked because of contractual rigidities in the labor market. Since they already have childcare to cover their working hours via the length of the school day, the extension of the primary school day does not induce them to change their hours worked. It is also possible that mothers working exactly the length of the primary school day extend their hours since a new portion of the budget constraint becomes available to them.

For mothers already working more hours than the length of the school day, the implicit childcare subsidy could have an ambiguous effect on their hours worked (Pencavel 1986). This is determined by their preferences and how many hours they were working before the reform. It is unclear how they will respond to the implicit childcare subsidy since they experience both the income and substitution effects. If the income effect dominates the substitution effect, the woman will decrease her hours worked. If the opposite is true, the woman will increase her hours worked.

This framework predicts that as a result of the extension of the primary school day, mothers who were not working before will be drawn into the labor market. The effect of the reform on women already working is unclear. They will either increase, decrease, or remain at the same number of hours. This change in hours is unclear due to the different responses of individuals depending on the number of hours they work, their preferences, and the availability of informal childcare. In almost all cases, however, the policy change allows women to increase their utility, making them better off than they were before.

IV. DATA AND DESCRIPTIVE STATISTICS

In this paper, I combine self-collected school data with individual level data from the GSOEP. The school-level data used in this paper is the year in which a given primary school started offering a full school day. This data has been collected for all primary schools in four German states.[4]

Education policy is devolved in Germany, which means that each state has a large amount of autonomy over its education system. The devolution of education policy to the states makes data collection in Germany difficult, but also creates ample regional variation in policy implementation.

[4]This paper focuses only on West Germany because of the underlying differences between the West and East German education systems. East Germany already had many schools that offered full school days because women were expected to participate in the labor market; childcare was much more developed in East Germany as a result.

Table 1

Summary Statistics by Observation

Variables	N	Mean	SD	Minimum	Maximum
Access to a FDS	11,013	0.103	0.304	0	1
Employed or seeking work	11,013	0.636	0.481	0	1
Employed	11,013	0.619	0.485	0	1
Full time	7,152	0.230	0.491	0	1
Part time	7,152	0.770	0.420	0	1
Weekly hours	6,554	25.635	12.997	1	80
Log weekly hours	6,554	3.086	0.616	0	4
Labor income	6,797	1,451	1,784	0	99,999
Age	11,013	37.200	9.500	15	64
Number of children 0-2	11,013	0.059	0.240	0	2
Number of children 2-4	11,013	0.206	0.441	0	3
Number of children 5-7	11,013	0.304	0.513	0	3
Number of children 8-10	11,013	0.249	0.554	0	3
Number of children 11-12	11,013	0.249	0.455	0	3
Number of children 13-15	11,013	0.331	0.541	0	3
Number of children 16-18	11,013	0.263	0.504	0	3
Years of education	9,979	12.064	2.591	7	18
Married	11,013	0.711	0.453	0	1
Single mother	10,606	0.114	0.318	0	1

NB: N are person-year observations.

The four German states analyzed in this paper are: Hesse, Rhineland Palatinate, Schleswig-Holstein, and Bavaria. Initially, I approached the state ministries of education in all West German states; however, these four states were the only ones to have the necessary data collected over the period of interest. The other West German states did not have centralized records of when primary schools began offering the full day option. The data I have collected is a census of all primary schools in these four states. It is a panel data set from the period 2000-2012 that indicates when a school started (or did not start) operating as a full day school.

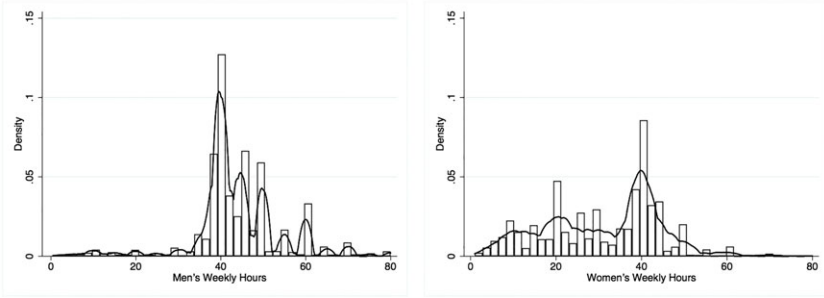
I link the school-level data to the GSOEP, a longitudinal study of families and individuals in Germany, which includes questions on work and family and was started in 1984 (SOEP 2013). It includes data on over 11,000 households across Germany. I use the GSOEP because it is a longitudinal study with information about the children in the family and allows access to the household's address via a secure on-site server. Since parents do not report the name of the school their child attends, I use their address to link them to the closest primary school (see Section V for further discussion).

Table 1 presents summary statistics for the variables used in the analysis as well as some additional demographic information.[5] My sample includes 1,496 women who are observed at some point during the period 2000-2012 in the four

[5]Some of the variables I do not actually include in the analysis, due to the inclusion of individual fixed effects, but they prove interesting for descriptive purposes.

Figure 1

Distribution of Hours Worked For Men and Women



states of interest. I only include women who have primary school aged children during this period and who are aged 15-64, as this is the working age population as defined by the German Federal Employment Agency. These women are linked to 1,084 primary schools in the four states.

Because of attrition and sample refreshment, the GSOEP is not a balanced panel, which should be kept in mind when analyzing the descriptive statistics presented in Table 1. Since I only look at women up to age 64, once a woman turns 65, she also drops out of my sample. I only look at women who had primary school aged children during the period 2000-2012, so this type of attrition does not pose a large problem.[6] This means that I have at most 11,013 person-year observations for some variables.[7]

Across all time and person observations, approximately 60 percent are employed. The dynamics of their employment prove similar to International Labor Organization statistics (see Figure A1 for further detail). Over the period of interest, employment has also increased for the women in the four states in my sample. Weekly hours worked are approximately 26 (Table 1). This is well below the threshold of 35 hours[8] for a full time job, indicating that many women in the sample are engaged in part time work.

Figure 1, a histogram of hours worked by men and women from the data used in this paper shows this clearly. This figure shows that most men in the sample work full time, but that much of the density of hours for women is found towards the left of the distribution. This prevalence of part time work is characteristic of female labor supply in Germany and will help explain the results in this paper.

[6]A total of 11 women exit the sample for this reason, which is 0.7 percent of the sample.

[7]There are fewer observations for some variables due to missing values.

[8]Here I use the OECD definition of part time work to be anything less than 35 hours per week (OECD Glossary of Statistical Terms).

Table 2

Labor Market Participation by Age of Youngest Child

	Employed or seeking work			Weekly hours		
	N	Mean	SD	N	Mean	SD
Age category of youngest child						
0-1	624	0.212	0.409	116	20.138	14.339
2-4	1,728	0.481	0.500	758	21.100	12.422
5-7	1,929	0.650	0.477	1,181	23.585	12.485
8-10	2,186	0.659	0.474	1,360	24.575	12.306
11-12	1,305	0.701	0.454	852	26.153	12.760
13-15	1,205	0.740	0.439	839	27.821	12.871
16-18	643	0.757	0.429	462	28.502	13.151

NB: N are person-year observations. Means are statistically different from each other at the five percent significance level.

For the women who work, their monthly labor income is reported in Euros. Income in the GSOEP is top coded with 99,999 Euros, which affects the mean value reported in Table 1. The median income for the women working in my sample is 1,200 Euros per month, which is less than the reported mean of approximately 1,450 Euros per month.

As the literature has shown, mothers have different labor force participation patterns as a result of how old their children are. Table 2 shows there is a difference in the participation rate of women in this sample whose youngest child is primary school aged (approximately 65 percent) compared to women whose youngest child is still preschool aged (under 50 percent). Furthermore, the participation rate increases as the age of the youngest child increases. Table 2 shows a similar trend for weekly hours of work: they increase as the age of the youngest child increases, but still never reach an average value above 35 hours per week. This is why I control for the age of other children in the household in all models.

Table 3 shows the exact number of women in the sample in a given year and state that have access. As more schools convert to full day facilities over the period, the number of women gaining access increases; however, the relatively modest rate of gaining access is due to the slow nature of the reform across the four states. Bavaria, the most populous state in the data set, has the slowest switch-over rate (see Figure 2 in Section V for more information), limiting the absolute number of women who gain access to a full day school. Nevertheless, 333 women will still allow me to estimate the impact of the extension of the school day on female labor supply.

V. METHODOLOGY

In this paper, I identify the causal impact of the extended school day on female labor supply. Usually, the challenge to identification in this type of research lies

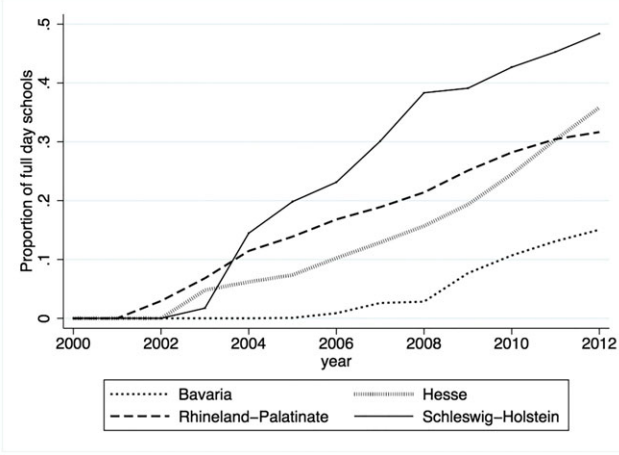
Table 3

Number of Women with Access By Survey Year

	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	Overall
Number of women with access	0	0	2	27	43	54	66	96	114	149	165	206	217	333
… in Bavaria	0	0	0	0	0	0	5	11	14	28	38	56	66	95
… in Hesse	0	0	0	16	20	21	23	27	34	38	56	63	70	103
… in Rhineland-Palatinate	0	0	2	10	17	21	25	31	33	43	37	44	41	66
… in Schleswig-Holstein	0	0	0	1	6	12	13	7	33	40	34	43	40	72
… as a proportion of sample	0.000	0.000	0.002	0.030	0.050	0.063	0.086	0.110	0.136	0.180	0.220	0.252	0.284	0.223
Number of women in panel	863	832	896	886	861	855	921	882	838	844	751	819	765	1,496

Figure 2

Proportion of Primary Schools Operating as Full Day Facilities By Year



in disentangling the endogenous work and childcare decisions. In this context, where attending the full day school option is not mandatory, parents have to make this simultaneous labor supply and childcare decision. The advantage of my data on the German reform is that access to a full day school comes exogenously to different women at different times, which means I can estimate an intention to treat (ITT) effect of gaining access to a full day school. I exploit this variation in my identification strategy and verify its validity using several different approaches.

V.1. Identification strategy

The reform to extend the school day has been slow and staggered, which proves useful for identification. Schools did not switch over all at once; in fact, even within a state, district, or city there is substantial variation over a period of almost 10 years as to when schools switched over from half day institutions to full day ones. This means that two mothers who live in the same city may have gained access at different points in time because of the difference in when the primary school closest to their home began operating as a full day school.

As previously mentioned, attending the full day option at these schools is not necessarily mandatory. Because of the constraints in extending the day, some schools offer an extended day option for which parents must enroll their children. This means there is still an element of choice in whether or not a child attends a full day school. This is generally free since the schools are public, but parents

may be asked to sign their children up to a lunch option in order to keep the cafeteria running.[9]

In this paper I treat all schools offering the full day option the same and estimate an intention to treat effect since I am interested in how gaining access affects labor market outcomes.

Primary school[10] attendance in Germany is decided solely on proximity to school.[11] There are few private primary schools in Germany and homeschooling is prohibited by law. This allows me to use geographic proximity to a full day school to evaluate the effect of the reform on maternal labor supply.[12]

The variation in when a school switches over arises because of the costs associated with the reform. One major cost of the reform is the hiring of additional teachers. Teachers in Germany have a special civil servant status, which means the government cannot simply extend their current hours worked. Teachers must be converted from part to full time or additional teachers must be hired. The costs associated with hiring new teachers and building new cafeterias are paid by the federal[13] and state governments and not by the individual municipality or district, so it seems reasonable to assume that when a school switches is not correlated with other characteristics of the local area in which the school is located, especially since two schools within a relatively homogenous region (e.g. a small city) may differ in their switch-over years. Another costs of extending the school day arises in the necessity of building cafeterias to provide lunch on site. The costs and time lags associated with constructing cafeterias and hiring new teachers should not be underestimated when assessing the speed of the reform. Nevertheless, this is something I will explicitly test. Potentially school switch-over year could be correlated with some school specific characteristic, e.g. the seniority

[9]Families who cannot afford this can get the lunch cost fully subsidized by the state.

[10]Here primary school aged children are six to 10 years old, as secondary school begins in grade five in the four states I analyze.

[11]These catchment areas are binding and known as *Schulsprengel* or *Schulbezirke*. Each state has its own law, which defines the catchment areas within its borders. For Bavaria the relevant law is §42 of the *Bayerisches Gesetz über das Erziehungs- und Unterrichtswesen* (BayEUG); in Hesse the relevant law is §60(4) of the *Hessisches Schulgesetz* (HschG); in Rhineland-Palatinate the relevant law is §62 *Rheinland-Pfälzisches Schulgesetz* (SchulG); and in Schleswig-Holstein, the relevant law was §44 of the *Schleswig-Holsteinisches Schulgesetz 1990* (SchulG 1990) and is now §24 of the *Schleswig- Holsteinisches Schulgesetz 2007* (SchulG 2007).

[12]After speaking to people from the Ministries of Education in these four states, it seems that based on their anecdotal evidence, on average less than one percent of families request that their child attend a primary school that is not the school to which they were assigned, i.e. the school closest to their home. They unfortunately do not collect official statistics on this, but if their estimates are accurate, then using closest school as a measure of access seems valid.

[13]Much of the funding for the reform has come out of the *Investitionsprogramms "Zukunft Bildung und Betreuung"* (Investment Program: The Future of Education and Childcare), which committed 4 billion Euros of federal money to the reform during the period 2003-2009 (Rainer et al., 2010).

of the principal, however, since I do not look at education outcomes of the pupils, this seems less relevant for mothers' labor supply.

For every primary school from the four states in my sample, I have the year in which the reform took effect and the school began operating as a full day facility. I observe the first schools operating as full day schools for the 2002-2003 school year; my data continues until the 2012-2013 school year. The switch-over process is still on-going in Germany, and in Bavaria, for example, there are still many primary schools that have not switched over, while in the other states almost half of all primary schools have since transitioned. Figure 2 shows the percentage of total primary schools in each of the four states of interest that have started operating as full day facilities in each year. As may be seen in this figure, less than 50 percent of total primary schools in each state have switched over as of 2012.

In order to analyze the geographic distribution of full day schools and link school-level data to individual-level data, I use geographic information system (GIS) software to link a woman to her closest primary school using her geocoded address and geocoded addresses of all primary schools in her state. Unfortunately, the GSOEP does not identify the name of the school a child attends, only the type of school (i.e. "primary school"). However, because children attend their closest primary school, I can determine in which year a woman gained access to a full day school based on the status of her closest primary school. Panel (a) of Figure 3 shows all the primary schools in the four states and panel (b) of Figure shows the geographical distribution of full day primary schools in these states. This allows me to observe a woman before and after gaining access and estimate a difference-in-difference model (see Section V.3 for a complete discussion of the empirical strategy).

This identification strategy uses a woman's geographically closest primary school, which due to the fact that school catchment areas are not convex sets with the school in the exact center, may lead to some mismatch. This type of mismatch could lead to an attenuation bias, which should be kept in mind when interpreting the results.

V.2. Verification

This identification strategy relies on the same pre-treatment labor market trends for women who gain access to a full day school and those who do not. This is commonly referred to as the "common time trend assumption" underpinning difference-in-difference. The control group and the treated group should have been following the same trends in labor market participation before the treated group gained access to a full day primary school. Since I only look at women who have primary school aged children during the period 2000-2012, it is reasonable to assume that these women follow a parallel trend; however, I am able

Figure 3

Geographical Distribution of Primary Schools

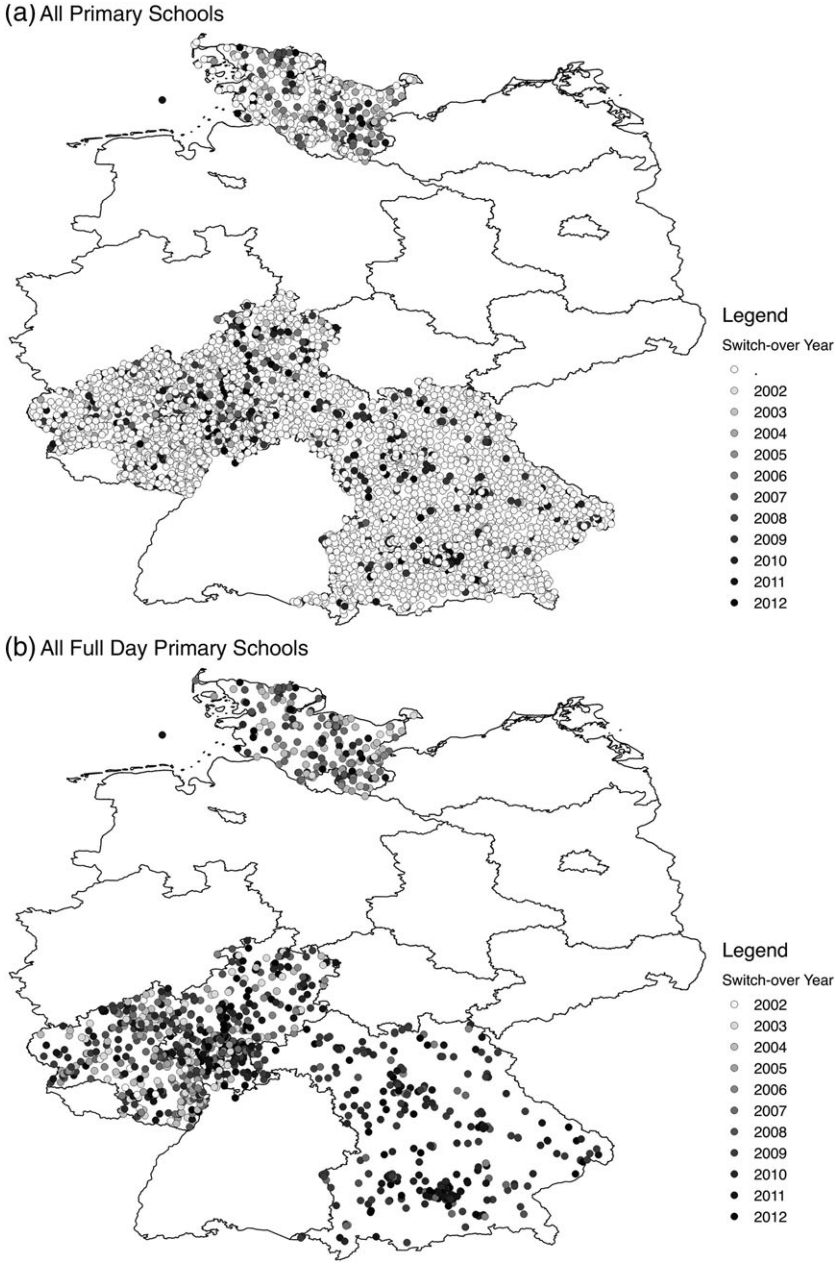
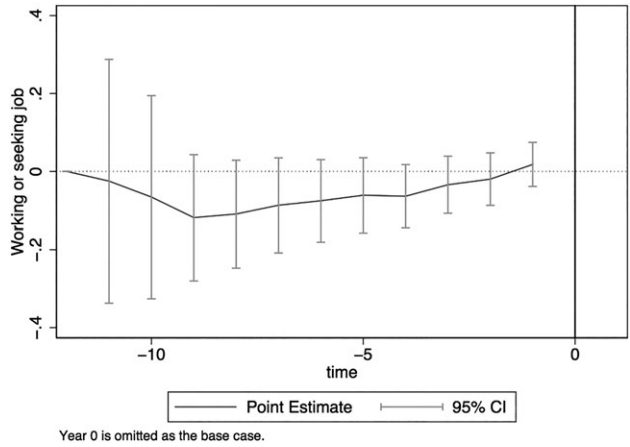


Figure 4

Event study: verifying the common time trend assumption



to explicitly test this assumption using an event study design similar to Autor (2003). The results from the event study specification are presented in Figure 4.

Figure 4 plots the coefficients obtained from a regression of the outcome of interest, labor market participation, on pre- and post-treatment dummies, state-year fixed effects, state trends, and individual fixed effects. The vertical line indicates the year a woman gains access to a full day school. If women who gain access are no different from women who do not gain access in terms of pre-treatment participation, we would expect all of the pre-access parameters to be equal to zero, which is what we observe in Figure 4. The 95 percent confidence intervals are such that we cannot claim these parameters are different from zero. Mothers who gain access to a full day school do not exhibit different pre-access trends in labor market participation than mothers who do not gain access, which validates my usage of difference-in-difference.

Because I am using difference-in-difference for my estimation, we do not need to worry about the switch-over year of a given school being correlated with location specific factors that would affect female employment. Nevertheless, I look at the correlation between district-level[14] unemployment, land prices, and GDP per capita with switch-over intensity, the percentage of schools in a given district in a given year that have already converted to full day schools. Here these land prices are collected by the *Statistisches Bundesamt* and reflect the actual sale price of undeveloped land that may be developed for commercial or private use in a given year averaged at the district level. These prices are measured in Euro value of

[14]Here district refers to *Kreis*, of which there are 173 in the four states of interest.

Table 4

Economic Factors Affecting Switch-over

VARIABLES	(1) ols switch-over rate	(2) ols switch-over rate	(3) ols switch-over rate
Unemployment	-0.004 (0.004)		-0.004 (0.004)
Undeveloped land price		0.000 (0.000)	0.000 (0.000)
Log GDP per capita	-0.061 (0.077)	-0.059 (0.073)	-0.074 (0.075)
Constant	-0.013 (0.046)	-0.018 (0.012)	-0.031 (0.048)
District FE	Yes	Yes	Yes
State-year dummies	Yes	Yes	Yes
State trend terms	Yes	Yes	Yes
Observations	2,076	2,206	2,036
Districts	173	173	173
R-squared	0.829	0.820	0.829

Clustered standard errors in parentheses.
*** $p<0.01$, ** $p<0.05$, *$p<0.1$
NB: Undeveloped land price is the average per square meter sale price of land for construction purposes in each year in each district. Unemployment is average for each year in each district. All data is from the Statistical Offices of the German Laender and Federal Statistical Office.

land per square meter. The results of this analysis are presented in Table 4. To check the correlation, I run three simple linear regressions of the district level switch-over rate on district level economic factors, including state-year dummies, linear trends for each state, and district fixed effects.[15] I cluster the standard errors at the district level.

We might be concerned that districts with low unemployment would be more likely to have a faster switch-over rate, since they require more childcare and are potentially more affluent; however, this is not observed. Column (1) in Table 4 shows that there is no observable correlation between the unemployment at the district level and the rate of switch-over. The coefficient on unemployment is not statistically different from zero and very small. At the same time, we might think that districts with high land prices might be economically booming and again require more childcare or have faster switch-over since they are more affluent, which is observed in Table 4. Column (2) shows no correlation between land prices and switch-over intensity; the estimated coefficient is not different from zero. Column (3) shows that including both unemployment at the district level and land prices does not change the correlations. In all three specifications, there is no statistically significant relationship between the switch-over rate of primary schools and GDP per capita, which also indicates that more affluent areas are not more likely to have a higher proportion of full day schools. Given this analysis, it seems plausible that the switch-over rate of primary schools in these four states is not being driven by economic factors at the district level.

[15]It is possible that the political orientation of the party in power at the local level also plays a role in how quickly schools offer the full day option, which is something I have not accounted for in these models and should be explored in future work.

The exogeneity of the reform means that women cannot influence when they gain access to a full day school because their children must attend the closest primary school. There is still the possibility that some families may send their children to a private school that offers extended hours or potentially move house to live closer to a full day primary school. Unfortunately, I am not able to identify reasons for moving house, however, it does not appear to pose a serious problem to identification. I observe 40 women in the data set who have moved house from a home where the closest primary school was not a full day school to a home where the closest primary school is a full day school. This is something I will address in the Robustness section.

V.3. Empirical strategy

In this section, I describe the models used to estimate the effect of the policy on maternal labor supply. I estimate two main models: one looking at changes in labor market participation status (employment or actively seeking a job versus not working) and one looking at changes in hours worked. This allows me to explore the impact of the policy on the extensive and intensive margins. My variable of interest is access to a full day school.

All of the participation models are estimated as linear probability and conditional logit models because of the binary outcome measure.[16] This model does not consider whether or not the woman is working part or full time, but rather pure, binary labor market participation status. The model for labor market participation status, whether or not the woman is employed or actively seeking employment, takes the following form, where E_{it} is a binary variable that takes the value "1" when the woman is employed or seeking employment and "0" otherwise:

$$E_{it} = \alpha_0 + D_{it}\delta + \eta_i + \phi_{st} + t_s + X_{it}\beta + \epsilon_{it}$$

In all models, "i" signifies "individual," "s" signifies "state," and "t" signifies "year." This specification allows for the inclusion of the treatment variable, D_{it}, which switches to one once a woman's closest school becomes a full day school. These regressions also include an individual fixed effect, η_i, state-year dummies, ϕ_{st}, state trend terms, t_s as well as standard errors, ϵ_{it}, clustered at the individual level. The individual fixed effects pick up any individual specific, time invariant characteristics that could explain participation. Similarly, the state-

[16]I estimate the linear probability model because of its ease in interpretation (Angrist 2001), but also because in a fixed effects framework it does not require variability in the outcome variable. The conditional logit requires variability in the outcome variables and drops all individuals, whose labor market outcomes do not vary over the period they are observed, because of complete separation. This means any estimation done using a conditional logit model is done on the "switchers," mothers who change labor market status as opposed to the entire sample of mothers.

year dummies should explain any variance in participation status caused by events occurring in a specific year in the state of residence, i.e. larger macroeconomic events or state specific labor market policies, and the state trend terms should account for any linear trends in a given state's macroeconomic situation. Since individual decisions to supply labor could be correlated over time, I cluster the standard errors at the individual level even though the treatment occurs at the school level.

The vector X_{it} includes a set of variables that account for whether or not the woman has children in a set of age categories. These include the number of children under the age of two, children aged two to four, children aged five to seven, children aged eight to ten, children aged 11-12, children aged 13-15, and children aged 16-18. These variables are included in order to disentangle the general effects of being a mother on labor supply and are summarized in Table 1.

Similarly, the regressions exploring weekly hours worked, take the following general form:

$$H_{it} = \alpha_0 + D_{it}\delta + \eta_i + \phi_{st} + t_s + X_{it}\beta + \varepsilon_{it}$$

Here H_{it} is a continuous variable representing either level hours worked or the logarithm of hours worked. As before, this model also includes an individual fixed effect, η_i, state-year dummies, ϕ_{st}, state trend terms, t_s, as well as standard errors, ε_{it}, clustered at the individual level. The vector X_{it} includes the same covariates as in the participation regressions.

I estimate the participation and hours worked models separately as opposed to in a joint participation-hours framework because I am not working in the standard censored context. In my data set, all of the hours worked are positive values; any woman who does not work receives a missing value instead of a zero for her hours. This allows me to estimate the impact of the policy on hours conditional on employment before gaining access, which is the intensive margin. I still look at how the extension of the school day affects the extensive margin by looking at the dummy variable for being employed or seeking employment. By separating the two, however, I am able to disentangle the question of being in the labor market from the effect on hours worked.

This empirical strategy, however, does not take potential spillover effects into account. There is a limited number of jobs available in the labor market and in order for these mothers to enter the labor market, vacancies must be created at a fast enough rate or some other workers must be squeezed out or have their hours reduced. The workers who exit the labor market could be women who do not have children, or men. I include mothers who are actively seeking a job in the participation model to capture part of this, but other aspects of labor demand may also change as a result of the policy. Since this reform was widely discussed in

Germany, it is likely that endogenous job creation took place as firms created new jobs in response to the reform. One example of this is teaching jobs. I will exclude women who work as teachers from my estimation of these models as a robustness check. All of these general equilibrium concerns should be kept in mind when thinking about the policy implications of this type of reform.

VI. RESULTS

In this section, I present the regression results of the models discussed in Section V on the full sample of all women who live in the four states of interest during the period 2000-2012 and have primary school aged children. I examine the extensive margin first by looking at whether a woman is working or actively seeking a job or not. I then turn my attention to the intensive margin by looking at hours worked conditional on working. I conclude by exploring some heterogeneous treatment effects.

In Table 5, I present the results from the regressions on employment using the full sample of women with primary school aged children. I first run a linear probability model on the binary outcome variable in Column (1), followed by a conditional logit in Column (2). Column (1) shows that gaining access to a full day school increases the probability of being employed or actively seeking a job by 4.4 percentage points. This effect is nearly statistically significant at the five percent significance level (p-value is 0.053). The sign and significance of this

Table 5

Estimates on Participation and Hours Worked

VARIABLES	(1) ols participation	(2) logit participation	(3) ols weekly hours	(4) ols ln (weekly hours)
Access to FDS	0.044* (0.023)	0.535** (0.234)	-1.004 (0.675)	-0.033 (0.036)
Children aged 0-1	-0.436*** (0.024)	-3.371*** (0.222)	-10.441*** (1.165)	-0.515*** (0.062)
Children aged 2-4	-0.219*** (0.018)	-1.749*** (0.142)	-8.541*** (0.665)	-0.391*** (0.032)
Children aged 5-7	-0.062*** (0.014)	-0.600*** (0.111)	-4.878*** (0.523)	-0.199*** (0.025)
Children aged 8-10	-0.060*** (0.013)	-0.550*** (0.108)	-4.169*** (0.434)	-0.168*** (0.021)
Children aged 11-12	-0.031** (0.013)	-0.295** (0.118)	-2.958*** (0.415)	-0.114*** (0.019)
Children aged 13-15	-0.030** (0.012)	-0.309*** (0.116)	-2.019*** (0.402)	-0.075*** (0.018)
Children aged 16-18	-0.019 (0.012)	-0.116 (0.110)	-1.091*** (0.408)	-0.030* (0.017)
Individual FE	Yes	Yes	Yes	Yes
State-year dummies	Yes	Yes	Yes	Yes
State trend terms	Yes	Yes	Yes	Yes
Observations	11,013	6,584	6,554	6,554
Individuals	1,496	702	1,135	1,135
(Pseudo) R-squared	0.596	0.220	0.754	0.724

Clustered standard errors in parentheses.
***$p<0.01$, **$p<0.05$, *$p<0.1$
NB: Outcome "participation" includes those employed and actively seeking employment.

coefficient do not change when I move from the linear probability model to the logit regression. In Column (2) we observe a small, yet statistically significant effect of providing women with an implicit childcare subsidy on their labor supply. Both of these regressions include individual fixed effects, state-year dummies, and linear state trend terms with standard errors clustered at the individual level.[17] The marginal effect associated with the odds ratio reported in Column (2) is 0.045, which is comparable to the estimate obtained through OLS.

As might be expected, having children of any age decreases participation and as children get older, this gets smaller. There is a negative effect of being a mother on employment, which matches the previously discussed statistics of German mothers' employment.

I estimate that this 4.4 percentage point increase in the probability of being employed or seeking employment translates into a two percentage point increase in overall participation over this period.[18] This is rather small in terms of a change at the extensive margin in the macro picture since over the period 2000-2012, the female employment ratio in Germany increased by approximately 10 percentage points (ILOStat). Nevertheless, the extension of the primary school day could account for approximately twenty percent of this total increase.

The potential of this 4.4 percentage point effect, however, should be considered. As my data on primary schools shows, only 50 percent of primary schools in these four states have switched over to full day schools. If this treatment effect remains constant, switching over all primary schools could increase overall maternal participation by nearly five percentage points, which would be very significant in the German context. This should of course be weighed with the cost of converting primary schools to full day schools, which is something I will return to in the conclusion.

The results of the linear probability model and the conditional logit show accordance even though they are estimated on slightly different samples. In the case of the conditional logit estimates, the results are being estimated on the "switchers," women who changed their labor market status. This is why both the number of observations and the number of individuals are lower than the numbers reported for the linear probability model in Column (1) of Table 5. In the linear probability model framework, mothers who gain access to a full day school are compared against all mothers who did not gain access. These findings reinforce the point that women are being drawn into the labor market and there is actually movement into the labor market as a result of this reform.

Turning to the intensive margin, I find no effect of the reform on hours worked. These regressions are only being estimated on women who report

[17]All results remain robust when regressions are clustered at the school level.

[18]I calculate this using the total population of working aged mothers in these four states in 2012 (ILOStat).

a positive number of hours worked with zero hours being treated as missing. This means that any changes in hours worked will reflect changes at the intensive margin. As the coefficients on the treatment variable in Columns (3) and (4) of Table 5 show, the effect on both, level and log hours, is small and not statistically different from zero due to large standard errors. For women who were already working before getting treated, their treatment did not cause them to change their hours worked. This could be driven by rigidities in the labor market that do not allow workers to easily increase their hours of work by small increments or the possibility that these mothers have already set up informal childcare via grandparents and have set their working hours as they desire.

I explore the possibility of heterogeneous treatment effects by interacting the access variable with different demographic control variables since the effects of the treatment may differ between women. I look at the effect on hours worked by the number of hours the woman worked before gaining access, I separately estimate the hours regressions for the subsample of mothers working part time, and then I look at single mothers since they could have fundamentally different responses to being treated. I do not conduct any heterogeneous treatment analysis along other interesting dimensions (e.g. state of residence) because dividing the sample into so many sub-groups significantly decreases the number of women in any group, decreasing the reliability of the estimates. These results, presented in the Appendix, do not reveal any statistically significant heterogeneous treatment effects.

VII. ROBUSTNESS

VII.1. Estimation without teachers

In the previous analysis, I have ignored any possible spillover effects the extension of the school day may have had on the labor market. This might not be reasonable given how large the reform is and its effect on the labor market for teachers. Since schools needed to hire many new teachers as a result of extending the school day, there has been increased demand for teachers across Germany.

Statistics from the Federal Statistical Office show that in the 2012-2013 school year, 88 percent of all primary school teachers were women (Statistisches Bundesamt). Since teaching is a traditionally female dominated career in Germany, the large increase in demand for teachers could affect the mothers in my sample. Teaching is also a career that allows women to combine work with childcare in a relatively straightforward fashion since their hours worked do not extend beyond school hours. Additionally, many teachers in Germany also

Table 6

Estimates Without Teachers

VARIABLES	(1) ols participation	(2) logit participation	(3) ols weekly hours	(4) ols ln (weekly hours)
Access to FDS	0.048** (0.023)	0.539** (0.234)	-0.055 (0.035)	-1.163* (0.699)
Controls for children	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
State-year dummies	Yes	Yes	Yes	Yes
State trend terms	Yes	Yes	Yes	Yes
Observations	10,452	6,216	6,176	6,176
Individuals	1,432	666	1,071	1,071
(Pseudo) R-squared	0.599	0.214	0.726	0.761

Clustered standard errors in parentheses.
***p<0.01, **p<0.05, *p<0.1
NB: Outcome "participation" includes those employed and actively seeking employment.

work part time. Of all teachers working in primary schools in the 2012-2013 school year, 39 percent of them were employed on a part time basis (Statistisches Bundesamt).

In order to disentangle the increased demand for teachers from the implicit childcare subsidy the mothers receive as a result of the extended school day, I drop all women from my sample who ever worked as teachers.[19] I observe 64 women in my sample who worked as teachers during the period 2000-2012, who I exclude and then run the same regressions on employment status and hours on this sub-sample. These results may be seen in Table 6.

These results are similar to those on the full sample in terms of the employment regressions. Columns (1) and (2) in this table show that women who gain access to a full day school and have never worked as teachers are still 4.8 percentage points more likely to be employed. The marginal effect associated with the logit coefficient in Column (2) is 0.046. These results indicate that the increased demand for teachers is not driving the change at the extensive margin.

When we turn our attention to Columns (3) and (4), the results differ from those using the full sample. Now the negative effect of being treated on hours worked is statistically significant at the 10 percent level. The size of the coefficients is larger than those obtained from the full sample. This would reinforce the idea that extending the school day has made mothers who were already working decrease their hours due to the income effect of the implicit childcare subsidy.

[19]I drop any woman who has worked as a teacher at any type of school, not just primary, because the reform to extend the school day has also occurred at the secondary schooling level.

VII.2. Estimation without women who move

One way that women may be able to change their treatment status is by moving house so that their new closest primary school is a full day school. These women would undermine my identification strategy because their assignment to treatment is no longer random, which would make me unable to disentangle the childcare and labor supply decisions. These women might have a strong preference to work, which would overstate the importance of access to a full day school. This is why I run the same participation and hours worked models on a sub-sample of women that excludes the 40 women who have moved house from a home where the closest school was not a full day school to a home where the closest school is a full day school. Although I do not know whether or not this is the reason these women have moved (this is not explicitly asked in the GSOEP), I still exclude these women as an additional robustness check.

The results of this analysis may be seen in Table 7. As this table shows, excluding the women who moved house and thereby changed their access status does not greatly change the results. I still find a positive and statistically significant effect of the policy on participation, although this effect is now only statistically significant at the 10 percent significance level and only in the conditional logit model, and no effect on hours worked. The marginal effect associated with the odds ratio estimated in Column (2) is 0.042, which is similar in magnitude to the estimate from the full sample. These robustness checks indicate that the results obtained in this paper are not being driven by changes in the demand for teachers or by women selecting into treatment.

Table 7

Estimates Without Women Who Move

VARIABLES	(1) ols participation	(2) logit participation	(3) ols weekly hours	(4) ols ln (weekly hours)
Access to FDS	0.035 (0.025)	0.497* (0.259)	-0.035 (0.039)	-1.066 (0.705)
Controls for children	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
State-year dummies	Yes	Yes	Yes	Yes
State trend terms	Yes	Yes	Yes	Yes
Observations	10,610	6,295	6,332	6,332
Individuals	1,456	673	1,100	1,100
(Pseudo) R-squared	0.599	0.083	0.726	0.759

Clustered standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1.
NB: Outcome "participation" includes those employed and actively seeking employment.

Tables 8–12 in the Appendix provide additional robustness checks verifying the impact of the treatment and additional heterogeneous treatment effects based on hours worked and single mother status.

VII.3. Estimation with fathers and women without children

As a final robustness check, I run the same employment and hours models on the fathers in the four states affected by this reform and the same models on the women without children in these four states. The descriptive statistics on fathers' hours presented in Figure 1 showed that most fathers in the four states work full time already. Unsurprisingly, I find no effect of getting access to a full day school on fathers' probability of working or working hours if already employed. While the coefficients on the employment regressions in Table 13 in the Appendix are positive, they are not statistically significant. The coefficients on the access variable in the hours regressions are small and also insignificant. This indicates that the reform to extend the primary school day did not have an effect on fathers' labor market participation. Similarly, I find no effect of the reform on women who do not have children (see Table 14 in the Appendix for further details). This placebo test confirms that the effect found for mothers is not simply capturing a regional effect, but rather the effect of gaining access to a full day school.

VIII. CONCLUSION

Previous research has shown that mothers respond to changes in the price and availability of childcare by changing their labor supply. This can mean either entering the labor market if they were not working before or extending their hours worked if they were already working when childcare becomes available or more affordable. Most of this evidence comes from studies that look at pre-school childcare, which means we still do not fully understand how mothers with primary school aged children respond to childcare subsidies. This is an important gap since policymakers continue to promote longer school days as a way to help working parents and increase female labor supply. This paper provided some of the first causal evidence on the effect of extending the length of the primary school day on maternal labor supply.

The reform to extend the primary school day in Germany has been one of the largest reforms ever undertaken in their school system. As shown in the descriptive statistics of this paper, the reform is far from complete as many primary schools still have to switch-over to an extended school day. This entails hiring new teachers and building cafeterias. This lag in the reform has staggered access to treatment, which allows me to look at how extending school hours causally affects maternal labor supply in a way few other studies have previously been able to do. Because there is still an element of choice in having your child attend a full

day school in Germany, this identification strategy allows me to separate the endogenous childcare and labor supply decisions and estimate an ITT. I am also able to use a more precise measure of treatment than other studies, which exploit regional variation, by focusing on the school instead of a larger geographical area and obtain a causal estimate.

I find robust effects of the extension of the primary school day on maternal labor supply. Mothers of primary school aged children are 4.4 percentage points more likely to enter the labor market once they gain access to a full day primary school. The effect for mothers is large and shows this policy has been successful at drawing mothers into the labor market, potentially accounting for two percentage points of the increase in the female employment ratio over this period. This effect is economically important since previous studies from other countries (e.g. Canada and the USA) have found effect sizes of between six and 25 percentage points for implicit preschool childcare subsidies. For Germany, Geyer et al. (2015) find an effect size of over seven percentage points of a childcare reform coupled with a parental leave reform for mothers with very young children. The fact that I still find an effect size of approximately five percentage points for a much smaller implicit childcare subsidy (only two hours per day) for mothers of older children means that this policy has had a large effect on mothers' probability of entering the labor market.

At the intensive margin, the results show overall no effect on hours worked. In most specifications, the effect of the reform on hours worked is small, negative, and statistically insignificant. I find no heterogeneous effect for mothers working part time since these mothers may have already set their working hours optimally. The finding that hours remain unchanged is something of which policymakers should be aware, especially as they aim to increase female labor supply. This reform may not be the most effective way to increase maternal labor supply in regions or countries where many mothers already work part time.

The results are robust to a series of checks on the identification strategy. I am able to verify the common time trend assumption underlying difference-in-difference and show that mothers who gain access to a full day school do not exhibit different pre-treatment employment patterns to those who do not. The results are robust to excluding teachers and women who may have moved house in order to live near a full day school. I do not observe any effect of the policy on fathers or on women without children. I also examine the relationship between district level economic factors and the rate of schools switching over in that county and find no evidence that the two are related.

These findings have important policy implications for Germany and other countries. The European Commission has set a target of at least 75 percent employment for both genders aged 20-64 in the European Union by 2020, which they plan to achieve through "the greater involvement of women, older workers and the better integration of migrants in the work force" (European Commission 2010). While Germany is ahead of many of its EU partners in reaching this goal

(female employment in 2012 was 68 percent), it still stands out as an industrialized country with low labor market participation of mothers.[20]

EU Labor Force Survey (LFS) statistics show mothers with children under the age of 12 in Germany are nearly 20 percentage points less likely to be employed than their counterparts without children in this age category as compared to an average across EU member states of 10 percentage points (Miani and Hoorens 2014). EU LFS statistics also reveal that nearly two thirds of mothers who work in Germany work part time (Miani and Hoorens 2014). Taken together this shows that mothers in Germany are less likely to participate in the labor market than their EU counterparts and that if they do participate, it is likely to be on a part time basis.[21]

In order for policymakers to increase maternal labor supply, we need to better understand how outsourcing childcare responsibilities affects the labor force status of mothers. This paper has shed light on using childcare policies as a lever to move maternal labor supply. Childcare policies may be used to draw mothers into the labor market after having children or extend their hours worked if already working; however, when policymakers advocate extending the length of the school day, they must consider that an extension shorter than the length of the working day may not have the consequences they intend. This does not mean that a short extension of the primary school day is not beneficial to working families. In this paper, I do not explore whether working mothers and families experience welfare gains as a result of gaining access to a full day school. It is certainly possible, especially for those families who no longer have to pay for as much childcare because of the extension of the school day.

Nevertheless, for policymakers concerned with increasing hours worked, an extension of this length does not prove promising. They may need to consider longer extensions of the primary school day in order to see changes in the hours worked by mothers. For policymakers interested in getting more mothers into the labor market, this policy shows some promise in helping to reach the European Commission's female employment ratio targets. In Germany, where female labor supply is dominated by part time work and stay-at-home mothers, this type of policy can enact fundamental change to the labor market, but

[20]Apart from the cultural tradition of the nuclear family model with male breadwinner, limited and expensive child- care options have made combining work and family life difficult (see the Appendix for statistics on after-school childcare availability in the states studied in this paper). In German there is even a pejorative term used to describe mothers who "neglect" their children, even as a result of working: *Rabenmutter*. The literal translation is "raven mother", but its actual meaning is an "uncaring mother" who neglects her children.
[21]When talking about Germany today, we refer to a reunified Germany, which brought together two different traditions and attitudes towards women participating in the labor market. Wenzel (2010) points out that these differences, a result of East Germany's stronger tradition of women working and more developed childcare options, still persist today. Even in 2002, more than ten years after German reunification, 51.7 percent of mothers in former East Germany were in full time employment; this is contrasted with only 16.8 percent of mothers in West Germany (Wenzel 2010).

may need to be combined with other measures in order to see working mothers extend their hours.

REFERENCES

Angrist, J. D. (2001). Estimation of limited dependent variable models with dummy endogenous regressors: simple strategies for empirical practice. *Journal of Business & Economic Statistics*, 19(1), 2-28.

Autor, D. H. (2003). Outsourcing at will: The contribution of unjust dismissal doctrine to the growth of employment outsourcing. *Journal of Labor Economics*, 21(1), 1-42.

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

Berthelon, M., Kruger, D. I., & Oyarzun, M. A. (2015). The Effects of Longer School Days on Mothers' Labor Force Participation. IZA Discussion Papers No. 9212.

Booth, A. L., & van Ours, J. C. (2013). Part-time jobs: What women want?. *Journal of Population Economics*, 26(1), 263-283.

Buddelmeyer, H., Mourre, G., & Ward-Warmedinger, M. E. (2005). Part-time work in EU countries: labour market mobility, entry and exit. IZA Discussion Papers No. 1550.

Contreras, D., Sepúlveda, P., & Cabrera, S. (2010). *The effects of lengthening the school day on female labor supply: Evidence from a quasi-experiment in Chile*. Centro de Microdatos, Departamento de Economía. Universidad de Chile.

Dowideit, A. (2011, January 30). Land der Schlüsselkinder. Welt. Retrieved from https://www.welt.de/

Dustmann, C. (2004). Parental background, secondary school track choice, and wages. *Oxford Economic Papers*, 56(2), 209-230.

European Commission. (2010). *Europe 2020, A European strategy for smart, sustainable and inclusive growth*. Brussels: European Commission.

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

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

Germany. Statistisches Bundesamt. Statistik Der Kindertagesbetreuung. N.p., 2014a. Web. 13 Nov. 2014.

Germany. Statistisches Bundesamt. Allgemeinbildende und berufliche Schulen: Lehrkräfte insgesamt sowie Anteil der weiblichen Lehrkräfte nach Schularten und Beschäftigungsumfang. N.p., 2014b. Web. 3 Dec. 2014.

Geyer, J., Haan, P., & Wrohlich, K. (2015). The effects of family policy on maternal labor supply: Combining evidence from a structural model and a quasi-experimental approach. *Labour Economics*, 36, 84-98.

Holtappels, Heinz Günter. (2008). In. *Ganztagsschule in Deutschland: Ergebnisse Der Ausgangserhebung Der "Studie Zur Entwicklung Von Ganztagsschulen" (StEG)*. Ed. Eckhard Klieme, Thomas Rauschenbach, and Ludwig Stecher. Weinheim: Juventa Verlag.

International Labor Organization Statistics (ILOStat). "Employment-to-population ratio by sex and age (%)." Retrieved from http://www.ilo.org/ilostat.

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

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

Miani, C. & Hoorens, S. (2014). Parents at work: men and women participating in the labour force. European Commission Short Statistical Report No. 2.

Nemitz, J. (2015). The Effect of All-Day Primary School Programs on Maternal Labor Supply. Department of Economics Working Paper No. 213, University of Zurich.

Neufeld, S. (2016). Extending the School Day Is a Lot Harder Than It Seems. The Atlantic. Atlantic Media Company, 21 Jan. 2014. Web. 29 Sept. 2016.

Pencavel, J. (1986). Labor supply of men: a survey. *Handbook of Labor Economics*, 1, 3-102.

Rainer, H., Bauernschuster, S., Auer, W., Danzer, N., Hancioglu, M., Hartmann, B., Hener, T., Holzner, C., Ott, N., Reinkowski, J. & Werding, M. (2013). Kinderbetreuung. ifo Forschungsberichte 59.

Riedel, B. (2005). *Das institutionelle Angebot für Kinder ab 6 Jahren (Grundschulalter)*. Deutsches Jugendinstitut eV/Dortmunder Arbeitsstelle Kinder-und Jugendhilfestatistik: Zahlenspiegel: 143-155.

Schmiedekampf, K. (2007, October 30). Verzwickte Reform Rheinland-Pfalz schafft die Hauptschule ein bisschen ab. Der Spiegel. *Retrieved from* http://www.spiegel.de/.

Socio-economic Panel (SOEP). Data for years 1984-2012, Version 29, 2013.

Statista (2018). "Wie Großeltern die Familie ihrer Kinder bei der Haus- und Familienarbeit unterstützen". Retrieved from https://de.statista.com/statistik/daten/studie/163493/umfrage/art-der-unterstuetzung-bei-der-familienarbeit-durch-grosseltern/.

Wenzel, S. (2010) Konvergenz oder Divergenz? Einstellungen zur Erwerbstätigkeit von Müttern in Ost-und Westdeutschland. GENDER–Zeitschrift für Geschlecht, Kultur und Gesellschaft 2.3.

APPENDICES

A. After-school Childcare in Germany

Before the school day was extended in Germany, there was the option of after-school care, known as a *Hort*, most often provided by non-profit organizations, but often physically located at the primary school (Riedel 2005). Parents had to sign their children up for a place at the *Hort* and pay for this service, which would often end at 4pm unless they also signed them up for an extended program (Riedel 2005). *Hort* still operate at many primary schools in Germany and even at full day schools since working parents may require additional childcare. Table 8 shows that places at such facilities were extremely limited in the states studied during the period of the reform. Note that "PA" in Table 8 refers to place availability, which is the ratio of children to places.

Table 8

After-school Childcare Place Availability Age 6-10

	2006			2009			2012		
State	**Places**	**Children**	**PA**	**Places**	**Children**	**PA**	**Places**	**Children**	**PA**
Bavaria	103,613	639,815	16.2	108,121	592,139	18.3	117,255	556,147	21.1
Hesse	56,004	301,950	18.5	58,927	280,988	21.0	59,138	268,690	22.0
Rhineland-Palatinate	29,302	205,163	14.3	24,803	185,738	13.4	23,544	171,342	13.7
Schleswig-Holstein	21,330	148,701	14.3	23,736	136,583	17.4	21,809	125,084	17.4

Source: *Statistisches Bundesamt, Statistik der Kindertagesbetreuung.*

B. The Ganztagsschulreform (Full Day School Reform)

The reform to extend the primary school day in Germany has been an on-going process over the last 10-15 years, born out of the motivation to not only improve educational outcomes, but also to make work and family more manageable for women. The *Ganztagsschulreform* or full day school reform is the reform process to extend the length of the school day at both the primary and secondary schooling levels. In 2006, the *Kultusministerkonferenz*, a regular assembly of all Ministers of Education from the federal states, defined a *Ganztagsschule* as a school that offers at least seven hours of instruction per day for a minimum of three days out of the school week and offers lunch to its pupils (Holtappels 2008). Since education is a devolved issue, states agree to have their schools extend the length of the school day according to a timeline they develop. This timeline is based on discussions with the Ministry of Education in each state and the feasibility of transitioning to a full day school. This feasibility is determined in part by the speed at which new teachers may be hired and cafeterias may be built since lunch must now be available, which was not the case under the old system. States also have the flexibility to determine the model of full day schools they wish to implement.

Because Germany has a federal system, the education system and the reform process in the four states analyzed in this paper are not identical. The *Kultusministerkonferenz* ensures, however, that many elements of the education systems are standardized. These four states all have a similar structure to their education system, where children attend primary school from age six until the end of fourth grade, when they are ten year old. At this point, the children are then placed into one of three tracks: the university track secondary school (*Gymnasium*), a higher vocational track secondary school (*Realschule*), and a lower vocational track secondary school (*Hauptschule*) (Dustmann 2004).

There is one key difference in the reform between the four states, which has to do with whether or not every class at a given school switches to a full day or just a certain percentage of classes switch (in German this is the difference between an *offene Ganztagsschule*, open full day school, and a *gebundene Ganztagsschule*, complete full day school). An *offene Ganztagsschule* might only have one or two classes per grade level that offer the extended school day option and parents would have to choose to sign their child up for this option whereas at a *gebundene Ganztagsschule*, all children automatically receive the longer school day. Regardless of the type of full day school, they still may offer only three days of extended instruction per week.

In Bavaria, for example, all primary schools that have switched to the full day are *gebundene Ganztagsschule*, while in the other states, this has not been the case. Some schools in some of the other states may have switched all classes while others may only have switched one class. The main difference between these two models of switch-over is the cost: switching all classes at the same time

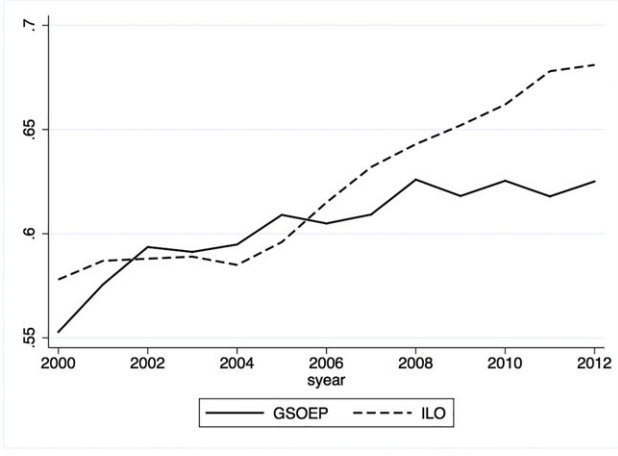
means even more teachers must be hired. This accounts for the slower rate of reform in Bavaria as opposed to the other states. For the purposes of the analysis in this paper, I treat all full day primary schools in the same manner because I assume that having the availability of a full school day is enough for the mother to be treated and allows me to estimate an intention to treat (ITT) effect.

During the period of this reform, there have been other reforms ongoing in the German school system. One of these has been the rise of comprehensive schools, *Gesamtschulen*, and other alternatives, which have taken a variety of forms depending on the state. For example, in 2013, Rhineland-Palatinate abolished all *Hauptschulen* (Schmiedekampf 2007). Since many of these other policy changes primarily affects secondary school (from grade five onwards), I do not provide further detail in this paper.

C. GSOEP sample and ILO yearly employment ratios

Figure A1

Employment Ratio Within GSOEP and in ILOStat Data by Year



Source: Author's calculation using data from GSOEP and ILOStat.

Note: The female employment ration from the GSOEP includes all women aged 15–64 in the four states of interest and the female employment ratio from the ILO includes all women aged 15–64 from all Germany. *Source*: Author's calculations using data from GSOEP and ILOStat.

D. Additional verification of identification strategy

The GSOEP includes a limited number of variables related to child level outcomes, which may have been affected by this reform. It does not include enough

information on grades or other academic outcomes to assess the impact of the reform on learning outcomes; however, there are some limited time use variables, which will allow me to verify the validity of my identification strategy. My goal is to show that the children of the woman I am assigning treatment to have actually experienced a change due to the treatment and therefore, assigning treatment to their mothers is a valid approach.

The GSOEP collects information on how many hours primary school aged children spent in various types of childcare, including hours spent at school, when the children start primary school (age six) and again shortly before they transition to secondary school (at age ten). These questions are answered by parents, and the data is only available for a rather small sample of children, 152, who live in the four states of interest. It should be noted, however, that these results are being estimated on a very small sub-sample of the 152 children, as many of them have missing values on the outcome variable in one year of being surveyed and therefore drop out of the fixed effects estimation. This may be seen in Table 9.

Table 9

Estimates on Child's School Hours

VARIABLES	School Hours
FDS	16.268*** (2.095)
Constant	14.665*** (0.532)
Individual FE	Yes
Year dummies	Yes
Observations	181
R-squared	0.189
Number of children	152

Clustered standard errors in parentheses.
***$p<0.01$, **$p<0.05$, *$p<0.1$.

If the identification strategy is working, we would expect that having access to a full day primary school would increase the number of hours a child spends at primary school. I use the same variable, access to a full day school, as determined by proximity to closest primary school, to estimate the following model:

$$SHours_{it} = \alpha_0 + \beta FDS_{it} + \gamma_i + \theta_t + e_{it}$$

Here the subscript "i" denotes the child and the subscript "t" denotes the year. The variable $SHours_{it}$ is the number of hours the child spends in school and

FDS_{it} is the binary indicator for whether the child's closest primary school is a full day school.

Indeed, as the results in Table 9 show, having access to a full day school increased the number of hours a child spent per week at primary school by approximately 16 hours. Given the confidence interval on this coefficient, this result is in line with an extension of the school day by 2.5 hours per day. Based on this analysis, it seems as though the strategy of using the closest primary school to determine access to a full day facility is a valid method for determining treatment status of mothers.

E. Heterogeneous Treatment Effects

In order to test some of the predictions provided in this paper, I categorize mothers based on the number of hours they were working before the reform. These groups are: one to 20 hours per week, 20-35 hours per week, and more than 35 hours per week. I then interact these groups with the access variable. The results presented in Table 10 show no heterogeneous treatment effects based on number of hours worked before gaining access to a full day school. The coefficients on each of the interaction terms are not statistically different from zero due to large standard errors. The same is true for the coefficients on the access variable, which again show no heterogeneous effect of gaining access for women who were working between one and 20 hours per week before they gained access. Previous research has shown that there is a strong preference for working part time, especially amongst mothers with partners (Booth and van Ours 2013) and that once people start working part time, they develop a preference

Table 10

Heterogeneous Treatment Effects: Hours Worked Before Gaining Access

VARIABLES	(1) ols weekly hours	(2) ols ln (weekly hours)
Access to FDS	-0.790 (1.333)	0.052 (0.085)
Access*Pre-Hours 20-35	0.859 (1.571)	-0.069 (0.091)
Access*Pre-Hours 35+	-0.672 (1.902)	-0.130 (0.097)
Controls for children	Yes	Yes
Individual FE	Yes	Yes
State-year dummies	Yes	Yes
State trend terms	Yes	Yes
Observations	6,554	6,554
Individuals	1,135	1,135
(Pseudo) R-squared	0.755	0.725

Clustered standard errors in parentheses.
***$p<0.01$, **$p<0.05$, *$p<0.1$.
NB: Women have been divided into three groups based on the number of hours they worked in the year before gaining access to a full day primary school. These groups are: l-20 hours per week, 20-35 hours per week, and more than 35 hours per week.

for it (Buddelmeyer et al. 2005), which might explain the lack of heterogeneous effects on hours worked.

Additionally, I look at whether or not the zero hours effect holds on a subsample of mothers who work part time. Mothers who work full time will be unable to adjust their hours, so this robustness check allows me to see if the mothers working fewer than 35 hours per week change their hours in response to gaining access to a full day school. The results of this robustness check in Table 11 show no statistically significant effect of gaining access to a full day school on hours worked for mothers working part time. The implicit childcare subsidy provided by the extended school day is not enough for these

Table 11

Estimates on Hours Worked for Part Time Mothers

VARIABLES	(1) ols weekly hours	(2) ols ln (weekly hours)
Access to FDS	-0.311 (0.722)	-0.007 (0.050)
Controls for children	Yes	Yes
Individual FE	Yes	Yes
State-year dummies	Yes	Yes
State trend terms	Yes	Yes
Observations	4,634	4,634
Individuals	1,006	1,006
(Pseudo) R-squared	0.720	0.695

Clustered standard errors in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.
NB: Part time is defined as working fewer than 35 hours per week.

mothers to change their working hours, indicating a preference for part time work given the constraints they face.

To further explore these potential heterogeneous treatment effects, I look at the effect of the reform on single mothers. If we think that single mothers are more likely to work due to economic concerns, then their response to the treatment may differ in a key way. Again, I interact the access variable with a binary variable for whether or not the woman is a single mother in Table 12. The results do not show a statistically significant heterogeneous treatment effect for single mothers versus the rest of the sample. The coefficient on the interaction term of access and single mother is small and negative in all three regressions, but it is not statistically different from zero.

Table 12

Heterogeneous Treatment Effects: Single Mothers

VARIABLES	(1) ols participation	(2) ols weekly hours	(3) ols ln (weekly hours)
Access to FDS	0.039 (0.024)	-0.896 (0.684)	-0.024 (0.042)
Single mother	0.033 (0.029)	2.955*** (0.962)	0.115** (0.047)
Access*Single mother	-0.000 (0.068)	-0.335 (1.844)	-0.038 (0.078)
Controls for children	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
State-year dummies	Yes	Yes	Yes
State trend terms	Yes	Yes	Yes
Observations	10,606	6,222	6,222
Individuals	1,496	1,126	1,126
R-squared	0.604	0.769	0.730

Clustered standard errors in parentheses.
***p<0.01, **p<0.05, *p<0.1.
NB: Outcome "participation" includes those employed and actively seeking employment.

F. Additional robustness checks

Table 13

Estimates on Participation and Hours Worked for Fathers

VARIABLES	(1) ols participation	(2) logit participation	(3) ols weekly hours	(4) ols ln (weekly hours)
Access to FDS	0.022 (0.018)	0.359 (0.267)	-0.359 (0.469)	-0.008 (0.013)
Controls for children	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
State-year dummies	Yes	Yes	Yes	Yes
State trend terms	Yes	Yes	Yes	Yes
Observations	10,116	3,233	7,956	7,956
Individuals	1,380	375	1,171	1,171
(Pseudo) R-squared	0.657	0.059	0.680	0.634

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.
NB: Outcome "participation" includes those employed and actively seeking employment.

Table 14

Estimates on Participation and Hours Worked for Women without Children

VARIABLES	(1) ols participation	(2) ols weekly hours	(3) ols ln (weekly hours)
Access to FDS	0.005 (0.021)	-0.713 (0.694)	-0.018 (0.027)
Individual FE	Yes	Yes	Yes
State-year dummies	Yes	Yes	Yes
State trend terms	Yes	Yes	Yes
Observations	13,636	8,832	8,832
Individuals	3,257	2,295	2,295
R-squared	0.746	0.803	0.797

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.
NB: Outcome "participation" includes those employed and actively seeking employment.

SUMMARY

This paper examines the effect of extending the primary school day on maternal labor supply. I exploit the staggered nature of the recent German reform to extend school hours and assess whether or not gaining access to a full day school increases the likelihood that mothers enter into the labor market or extend their hours worked if already employed. I use the German Socio-Economic Panel data set (GSOEP) and link it to a self-collected school-level data set with geographical information software (GIS). Using a flexible difference-in-difference approach in the estimation of linear probability and logit models, I find that the policy has a statistically significant effect of approximately five percentage points at the extensive margin, drawing more women into the labor market. I find no significant effect of the policy at the intensive margin; women who were already working do not extend their hours worked. This has implications for policies to extend the school day that do not correspond to the working day.

