School Hours and Female Labor Supply: Quasi-experimental Evidence from Germany

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Abstract

This paper examines the recent German reform (*Ganztagsschulreform*) to increase primary school hours and the effect this has had on maternal labor supply. This reform has been one of the largest and most expensive reforms in the German education landscape over the past 15 years, but with little evaluation. While the impetus for the reform came from improving pupils' learning outcomes, it was also motivated by a desire to increase maternal labor supply, which had been traditionally low in Germany as compared to other countries. I exploit the quasi-experimental nature of reform to assess whether or not increasing school hours increases the likelihood that mothers enter into employment, or extend their working hours if already employed. I use the German Socio-Economic Panel data set (GSOEP) and link it to a school-level data set with geographical information software (GIS). Using a flexible difference-in-difference approach in my estimation of linear probability and logit models, I find that the policy has an effect of approximately seven percent at the extensive margin, drawing more women into the labor market. I find no significant impact of the policy at the intensive margin, women who were already working do not extend their hours and in some cases even shorten them. These results are robust to a variety of checks and comparable to previous findings in the literature on childcare and maternal labor supply. This is one of the few papers, however, to look at the relationship between primary school and maternal labor supply.

Keywords: Time Allocation and Labor Supply, Education: Government Policy, Economics of Gender.

JEL Classification Codes: J22, I28, J16

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1 Introduction

The linkage between childcare and female labor supply has received much attention in labor economics and public policy. It has been shown that the costs of childcare affect how women determine their labor force participation and hours. As the primary carers of children, women often exit the labor market after having children and only re-enter once childcare becomes available and affordable.[1] When preschool childcare is expensive or limited in supply, primary school may be the first opportunity for families to utilize "free" childcare.

In some countries, the length of the primary school day makes combining work and family difficult. In Germany, this was the case until approximately ten years ago: a typical German school day, at both the primary and secondary level, began around 8am and would end at approximately 1pm. A recent and on-going reform in Germany to increase school hours, known in German as the *Ganztagsschulreform* or "full school day reform" was undertaken in an effort to extend the school day by a few hours until approximately 3pm (Stecher et al. 2008).[2]

In Germany, the debate to increase school hours centered on improving education outcomes, based on the idea that learning is a function of the number of hours spent in the classroom. Impetus to speed up this reform came from the 2001 "PISA Shock," the public reverberation of Germany's rather poor performance on the first Programme for International Student Assessment (PISA) tests administered in 2000. The other major reason for tackling the issue of school hours was to increase the labor market participation of mothers. It was argued that not having a full day of schooling was creating a structural barrier to women entering into employment after having children. Of course when we talk about Germany today, we refer to a reunified Germany, which brought together two different traditions and attitudes towards female participating in the labor market. Wenzel (2010) points out that these differences, primarily the result of East Germany having had a stronger tradition of women working and as a result 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). The data used in this paper comes from four West German states.

These low participation rate of mothers in West Germany does not come as a surprise. Apart from the cultural tradition of the nuclear family model with male breadwinner, limited and expensive

[1] This simplifies the decision to re-enter the labor market, ignoring the implicit costs associated with employment such as the penalty many women may pay for having taken time off for maternity decisions.

[2] The details of the reform will be further discussed with the data and as part of the identification strategy.

childcare options have made combining work and family life difficult.[3] As a result of these and other factors, female employment in Germany does not compare well to other European Union and partner countries at the beginning of the period of reform. Figure 1 shows how Germany's female

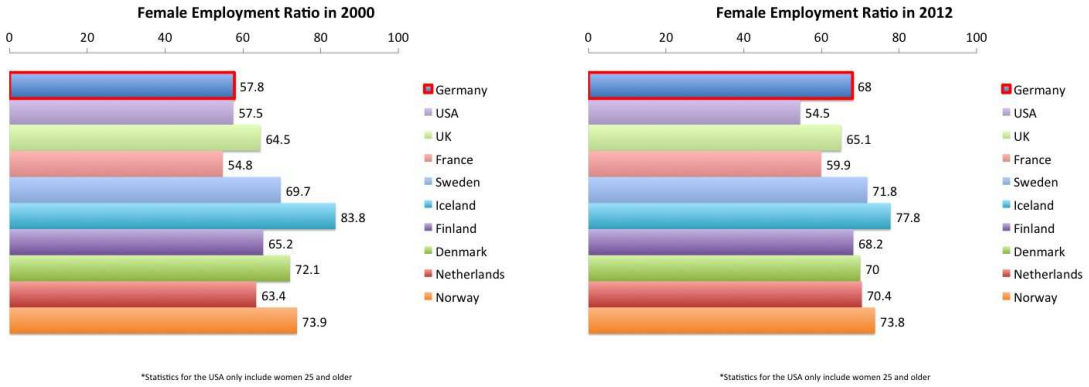


Figure 1: Female Employment Ratio in 2000 and 2012, Source: ILOStat

employment ratio for women aged 15-64 compares to the Nordic countries, the Netherlands, France, the United Kingdom, and the United States in 2000 and in 2012.[4] In 2000, only 57.8 percent of German women 15-64 were employed, either in full time or part time employment (International Labour Organisation Statistics). By 2012, this had increased to 68 percent (ILOStat). The data from both years shows that Germany has a lower female employment ratio than most of the Nordic countries, but that the gap has decreased over time.

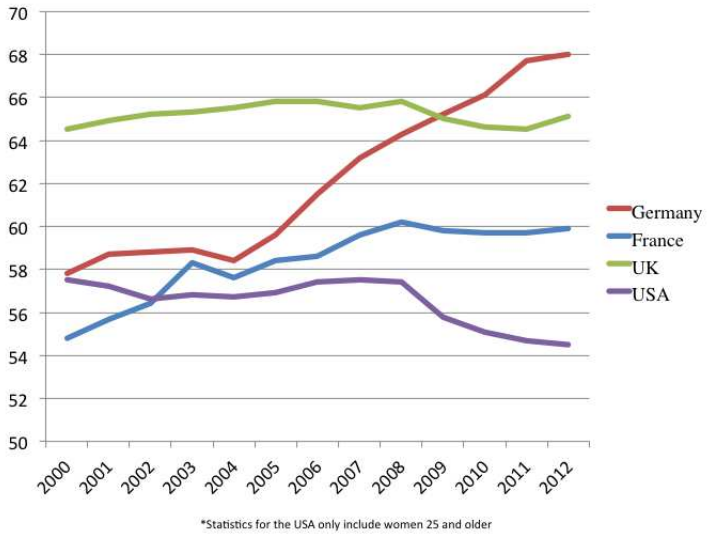


Figure 2: Female Employment Ratio, Source: ILOStat

The evolution of Germany's female employment ratio also differs from France, the United Kingdom,

[3] See the Appendix for statistics on after school childcare availability in the four states studied in this paper.

[4] It should be noted that the data for the United States only contains information on women aged 25 and older due to differences in labor force surveys.

and the United States. Although Germany, France, and the United States start off in similar positions in 2000, Figure 2 shows that Germany's female employment ratio has increased much faster and at a rate that allowed it to overtake the United Kingdom by 2012. This graph shows that Germany's female employment ratio has increased significantly over the period of the reform.

In this paper, I ask whether extending the primary school day caused mothers to enter the labor market if they were not working before or extend their working hours if they were already working. Using the slow and staggered nature of the reform, I am able to exogenously assign treatment, access to a full day school, to women and explore how this impacted their labor market status. High costs associated with building new cafeterias and hiring more teachers have delayed switch-over of schools even within the same city or town. I find that women who were not working were able to enter the labor market, but that for women already participating, there is either no effect or a very small negative effect on the hours they work. This result may be explained by the income and substitution effects underpinning a basic model of labor supply.

The rest of the paper is structured as follows. In Section 2 I discuss the relevant literature. In Section 3 I present the data used in this paper and present some descriptive statistics. In Sections 4 I lay out the empirical strategy and in Section 5 I present the results obtained. In Section 6 I include several robustness checks. Finally, in Section 7, I conclude.

2 Existing literature

The literature on childcare costs and female labor supply may be divided into structural and quasi-experimental strands. As this paper uses the latter approach, I limit my discussion of the literature to papers that use a policy change, which alters the price or availability across women and time periods to identify the relationship between maternal labor supply and childcare costs.[5]

Most of the literature in this strand looks at pre-primary childcare, which makes my paper a more unusual contribution to the field since I look at the extension of the primary school day. Lefebvre and Merrigan (2008) look at the expansion of universal, highly subsidized preschool childcare in Quebec, using the rest of Canada as a control. They find that the policy to heavily subsidize the cost of childcare increased the participation of mothers who have at least one child under the age of five by eight percentage points. Their findings also show that mothers in Quebec increased their annual hours worked by 231 hours and annual weeks worked by 5.17 weeks. Both sets of results indicate changes on the extensive and intensive margins.

Baker et al. (2008) look at the same reform in Quebec and also estimate the impact of the subsidized childcare on maternal labor supply, in addition to looking at child level outcomes. Their findings

[5]For a thorough overview of the structural literature in this field see Blau and Currie (2006).

show that maternal employment rates rose by 7.7 percentage points, approximately 14.5 percent of the baseline employment rate, as a result of the subsidy in Quebec. These studies differ from this paper because they exploit an explicit price change across regions within a country.

Gelbach (2002) is more similar to this paper since he exploits an implicit childcare subsidy as opposed to an observed change in price. He looks at the impact of public kindergarten enrollment in the United States, which varies based on birth month, on maternal labor supply. Enrollment in a public kindergarten the year before primary school begins is an implicit childcare subsidy to the family since there is no formal cost. The estimates obtained from his instrumental variable, whether or not the child qualifies for kindergarten enrollment based on birth month, show large results of the implicit subsidy. Gelbach finds that for single mothers, whose youngest child is five, their child's eligibility for free kindergarten increases their labor supply by between 6-24 percent. He notes that there is no significant impact of the policy on single mothers who have both a five year old child and a younger child, but that for married women the effect size is 6-15 percent regardless of whether or not they have an additional child under five.

Gelbach also makes the point that for any families consuming fewer or the same number of hours of childcare as the primary school day, this subsidy is a "100 percent marginal price subsidy for childcare of fixed quality" and that if the family consumes more hours of childcare than provided by the length of the school day, then "the subsidy is entirely inframarginal with respect to childcare costs," and leads to a kink in the budget constraint (Gelbach, 2002: 308).

Contreras et al. (2010) is one of the only other papers to my knowledge to also look at the extension of the primary school day. They use survey and administrative data from Chile, which also increased school hours to a full length school day beginning in 1996. This reform was undertaken on the municipal level, which allowed the authors to exploit the quasi-experimental nature of the reform across municipalities and time (Contreras et al., 2010). They acknowledge, however, that the reform in Chile was not undertaken in a random way. Municipalities that were classified as "higher risk" were also the first to receive full day schools. Similar to this work, their findings showed that the reform acted as an implicit childcare subsidy, which had a positive effect on female labor supply. They find that a one percent increase in full day schools causes a three percent increase in the likelihood that a woman works. When they look at women by age group, they find the largest impact of the policy for the oldest group of women (50-65). They argue this is because these women tend to have older children on average, who are affected by the reform, and that younger women tend to have younger children with different childcare needs. It is still surprising, however, that the oldest group of working age women is affected the most by the reform. Their findings also show that women already employed actually decrease their working hours.

Methodologically all of these quasi-experimental papers are similar to this paper in the identification strategy they use; however, I will exploit school level variation, which reduces some of the

concern surrounding the common time trend assumption and the comparability between states or regions. This differentiates my work from much of the field as many previous studies linking childcare and female labor supply compared regions within a country.

This work is also one of the first to assess the *Ganztagsschulreform* in Germany. There has been one major longitudinal study of the reform process, which collected data on a representative sample of children in full day primary and secondary schools in 14 German states (Holtappels et al. 2008). They also surveyed the parents of these pupils as part of the study. Their statistics show that 26 percent of mothers in the sample, whose children attend a full day school, reported being able to extend their working hours and 21.2 percent reported being able to re-enter the labor market (Holtappels et al. 2008). Their study does not differentiate between mothers of children in primary school and secondary school, and since there is school choice at the secondary level, it is not clear whether or not the mothers in this sample might be choosing a full day school and as a result, be able to change their labor supply. Holtappels et al. (2008) also examines the impact the reform has had on child level outcomes, something I am not able to do in this paper due to data limitations.

Marcus et al. (2013) provide a descriptive overview of the reform using the German Socio-Economic Panel (GSOEP) to show what types of families attend full day schools, but do not identify the causal impact of the reform on female employment. They find that primary school aged children whose mothers work full time and children in single mother households are more likely to attend a full day school, and that children with immigrant parents and children from families that receive social benefits are also more likely to attend a full day school. Marcus et al. also find that over time more children from low socio-economic households are attending full day schools. They use a question in the GSOEP that asks whether or not the child attends a full day school. This is not the same as when a mother would have gained access to a full day school because the full day school might still be opt-in once a school switches over. Marcus et al. provide evidence that either some sort of selection into full day schooling is taking place once it is available at a school or that the switch-over of schools might not be random since certain groups are more likely to take up the opportunity.

Rainer et al. (2013) also use the GSOEP and propensity score matching to look at the impact of various types of childcare in Germany on female labor supply. They create a matched sample of women who report their child attends a full day school with very similar women whose children do not. They find that the full day school reform caused women who were already working to extend their hours, but did not draw women into the labor market. Rainer et al. find that most of the impact they find on hours and wages comes from women who were already working in the year before their child started attending a full day school. Their analysis includes all mothers of children aged 6-18 in the GSOEP, which again mixes primary and secondary schooling, thereby allowing for school choice. Combining these two groups does not account for selection into a full day school.

Including women whose children are so much older also changes the importance of childcare within the sample. A woman whose youngest child is eight will have very different childcare needs from a woman whose youngest child is 17. This paper builds on the previous analysis of the German reform by exploiting a natural experiment in order to identify the causal impact of the reform on the labor supply of women with primary school aged children.

3 Descriptive statistics and data

In this paper, I combine state-level 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. 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. In this paper I use a self-collected data set to look at the process of the full day school reform in four West German[6] states: Hesse, Rhineland Palatinate, Schleswig-Holstein, and Bavaria. These four states were chosen because they were the only West German states to have the necessary data centrally collected over the period of interest. This data had never been collected across states in this manner because some states do not collect this information centrally, but rather allow school districts to record this information. Although the data collection process proved arduous, I now have a unique data set never before used for this type of analysis.

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 more than sufficient information about the children in the family and allows me to access to the household's address. While it would be possible to gain information about the labor market participation status of German women from a labor force survey (e.g. the Mikrozensus) and have a larger sample size, this data would have the disadvantage of not letting me know the age of any children in the household and not providing the geocoded home addresses of respondents.

Table 1 presents summary statistics for the variables used in the analysis as well as some additional demographic information.[7] My sample includes 6,965 women who are observed at some point during the period 2000-2012 in the four states of interest. I look at these years because this is the period in which the reform is taking place in the states for which I have data. I only include

[6] 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.

[7] 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.

Table 1: Summary Statistics By Observations

Variable Name	**N**	**Mean**	**Standard Deviation**	**Minimum**	**Maximum**
Treatment	36,158	0.012	0.146	0	1
Access to a FDS	36,158	0.110	0.314	0	1
Employed	36,158	0.602	0.489	0	1
Full time	22,710	0.408	0.492	0	1
Part time	22,710	0.592	0.492	0	1
Weekly hours	20,993	30.993	13.495	1	80
Log weekly hours	20,993	3.296	0.596	0	4.382
Labor income	21,663	1,776	1,652	0	99,999
Log labor income	21,542	7.160	0.874	0	11.513
Age	36,158	40.2	13.8	15	64
Has primary school children	36,158	0.133	0.339	0	1
Has pre-school children	36,158	0.104	0.305	0	1
Has seconday school children	36,158	0.207	0.405	0	1
Years of education	31,141	12.065	2.627	7	18
Married	36,158	0.554	0.497	0	1
Husband works	26,713	0.796	0.403	0	1
Single mother	36,158	0.165	0.371	0	1

NB: N are person-year observations

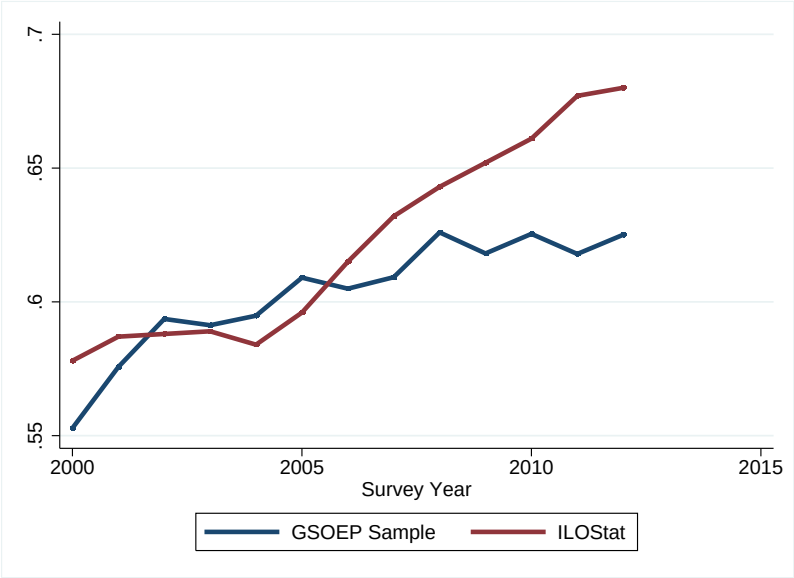


Figure 3: Employment Ratio Within Sample and in ILOStat Data By Year

women who are aged 15-64, as this is the working age population as defined by the German Federal Employment Agency.

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. This means that I have at most 36,158 person-year observations for some variables. There are fewer observations for some variables due to missing values.

The GSOEP is a representative sample of people living in Germany and the descriptive statistics in my sample confirm this. Over half of the women I observe in this sample are married and on average 40 years old. Twenty percent of the sample have children in secondary school, thirteen percent have primary school aged children, and only ten percent have pre-school aged children, potentially due to declining fertility across time. On average, the women in my sample have completed approximately twelve years of education, which is more than the legally required minimum of nine years of education. A university educated woman will have completed 18 years of education, the maximum reported in the data.

Across all time and person observations, 60 percent are employed. The dynamics of their employment proves similar to the ILO statistics presented earlier; over the period of interest, employment has also increased for the women in my sample (Figure 3). This indicates a change at the extensive margin of the labor market over the period of interest not only in the ILO data, but also in my sample.

For the women that work, their average weekly hours in Table 1, is approximately 31. This is

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 4, 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 found in this paper.

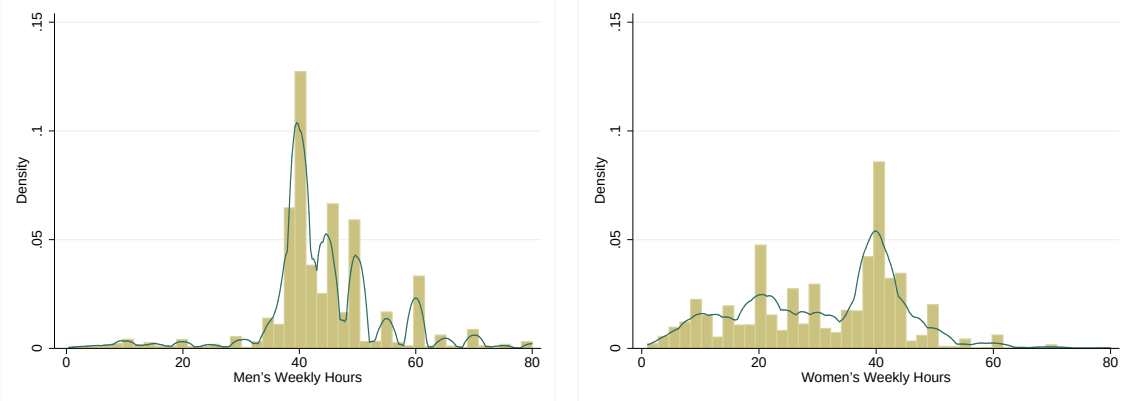


Figure 4: Distribution of Hours Worked For Men and Women

For the women that work, their monthly labor income is reported in levels and logarithms. Income in the GSOEP is top coded with 99,999, which affects the mean value reported in Table 1. The median income for the women working in my sample is 1,500 Euros per month, which is less than the reported mean.

If we believe that mothers have different labor force participation patterns as a result of having children, then we would expect to observe a difference in the employment and hours data for mothers and non-mothers. As Table 2 shows, there is a difference in the employment rate of women who have primary school aged children versus those who do not. Furthermore, the employment rate decreases as the number of primary aged school children in the house increases. Table 2 shows a similar trend for weekly hours of work: they decrease as the number of primary school aged children in the household increases.

As previously mentioned, during this period, 13.3 percent of my person-year observations have primary school aged children. This limits the number of women in the sample affected by the policy significantly since having access to a full day school only matters if you have primary school aged children. Because of this, my "treatment" is the interaction of the binary variable for having access to a full day school and the binary variable for having primary school aged children. This means that for a woman to be treated, or affected by the reform, she must not only have access

[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: Employment Information by Number of Primary School Children

	Employed		
	Mean	N	Standard Deviation
Number of Primary School Children			
0 children	0.625	27,721	0.484
1 child	0.574	5,158	0.495
2 children	0.472	2,701	0.499
3 children	0.367	528	0.483
4 children	0.319	47	0.471
5 children	0.333	3	0.577

	Weekly Hours of Work		
	Mean	N	Standard Deviation
Number of Primary School Children			
0 children	32.903	16,720	13.043
1 child	24.899	2,857	12.688
2 children	21.117	1,219	12.001
3 children	18.616	183	11.876
4 children	16.538	13	7.957
5 children	10	1	

NB: N are person-year observations

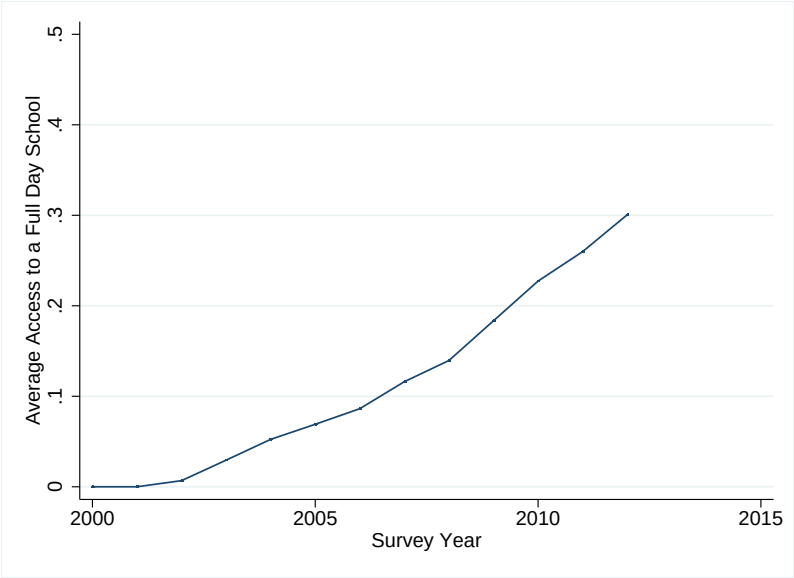


Figure 5: Proportion of Sample with Access to Full Day Schools By Year

to a full day facility, but also have primary school aged children. When interpreting my results, the coefficient of interest on the variable treatment, will tell me the additional impact of gaining access to a full day school when a woman has primary school aged children.

Figure 5 shows what percent of the sample of 6,965 women has access to a full day facility in any given year and Table 3 shows the exact number of women in the sample in a given year and state that have access and of those women, which ones are treated, i.e. also have primary school aged children.

Table 3: Number of Individuals with Access and Treatment By Survey Year

	2000	**2001**	**2002**	**2003**	**2004**	**2005**	**2006**	**2007**	**2008**	**2009**	**2010**	**2011**	**2012**	**Overall**
Number of women with access	0	0	21	85	144	183	245	310	344	501	551	746	865	1,413
...as a proportion of sample	0	0	0.007	0.030	0.052	0.069	0.086	0.116	0.141	0.186	0.228	0.260	0.299	0.203
...in Bavaria	0	0	0	0	0	0	15	34	34	136	151	191	281	407
...in Hesse	0	0	0	39	47	50	74	84	109	131	183	223	248	405
...in Rhineland-Palatinate	0	0	21	45	61	75	94	100	103	119	108	157	164	287
...in Schleswig-Holstein	0	0	0	1	36	58	62	92	98	115	109	175	172	322
Number of treated women	0	0	1	12	21	20	29	39	35	49	61	86	89	211
...as a proportion of sample	0	0	0	0.008	0.012	0.015	0.017	0.025	0.031	0.038	0.042	0.048	0.054	0.045
...in Bavaria	0	0	0	0	0	0	3	8	11	14	17	21	26	55
...in Hesse	0	0	0	5	8	12	13	13	11	9	20	25	25	62
...in Rhineland-Palatinate	0	0	1	6	9	4	9	11	8	12	8	14	14	43
...in Schleswig-Holstein	0	0	0	1	4	4	4	7	5	14	16	26	24	52
Number of women in panel	3,124	2,769	3,103	2,887	2,747	2,650	2,840	2,669	2,465	2,731	2,424	2,871	2,878	6,965

This table shows that access and treatment are both increasing over time as more schools switch-over to become full day facilities, but that treatment has been limited. I only observe a total of 211 women who have gained access to a full day school whilst they had primary school aged children. This is the result of only having school data on four states, the slow switch-over rate of schools within those states, and the limited number of women that have primary school aged children at the same time they gain access. Bavaria, the most populous state in my data set, has had the slowest switch-over rate, limiting the absolute number of treated women. Nevertheless, 211 women will still allow me to estimate the impact of the extension of the school day on female labor supply.

4 Methodology

In this paper, I attempt to identify the impact of the extended school day on female labor supply. Usually, the challenge to identification in this type of research lies in disentangling the endogenous work and childcare decisions. The advantage of the German reform is that access to a full day school comes exogenously to different women at different times. I exploit this variation in my identification strategy and verify its validity using two different approaches.

4.1 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, county, 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 variation arises because of the costs associated with the reform. One of the main costs of extending the school day arises in the necessity of building cafeterias to provide lunch on site. Very few primary schools in Germany would have had a cafeteria or any kitchen facilities on site since all children went home for lunch when school ended. The costs and time lags associated with constructing cafeterias should not be underestimated when assessing the speed of the reform. 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). This means that the reform process has been more standardized across states since much of the money comes from a federal source.

Another 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 working hours. 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 government

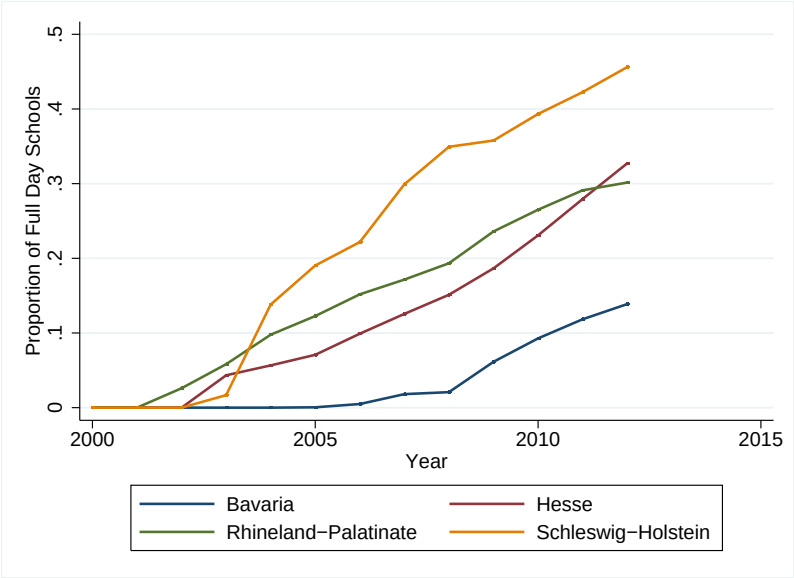


Figure 6: Proportion of Primary Schools Operating as Full Day Facilities By Year

and state and not by the individual municipality or county, 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. Potentially school switch-over year could be correlated with some school specific characteristic, e.g. the seniority of the principal, however, since I do not look at education outcomes of the pupils, this seems less relevant for mothers' labor supply.

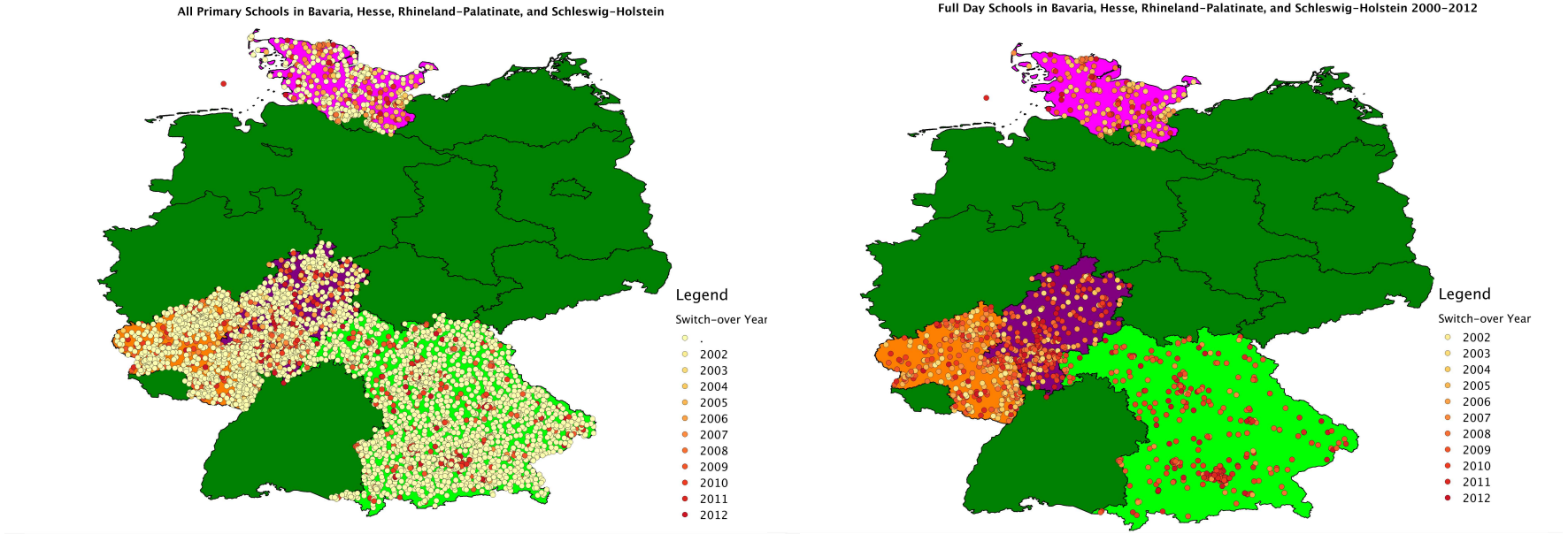
Primary school[9] attendance in Germany is decided solely on proximity to school. There are few private primary schools in Germany and homeschooling is prohibited by law. This allows me to use geographic proximity to a full day school to evaluate the effect of the reform on maternal labor supply. After 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.

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 or a missing value if the school has not yet extended its hours. 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 6 shows the percentage of total primary schools in each of the four states of

[9]Here primary school aged children are 6 to 10 years old, as secondary school begins in grade 5 in the four states I analyze.

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 (obviously for women without children it would not do so either), 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 7 shows all the primary schools in the four states and panel (b) of Figure 7 shows the geographical distribution of full day primary schools in these states.



(a) All Primary Schools

(b) All Full Day Schools

Figure 7: Geographical Distribution of Primary Schools

4.2 Verification

This identification strategy relies on the switch-over year of a given school not being correlated with location specific factors that would affect female employment. Because I am using difference-in-difference for my estimation, this does not pose a problem, but it is still prudent to check whether the rate of reform might be correlated with location specific economic factors. In order to verify this, I look at the correlation between district-level[10] unemployment and land prices 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 land per 100 square meters. The results of this analysis are presented in Table 5. To check the correlation, I run three simple linear regressions of a district level economic factor on the district level switch-over rate, including year dummies and district fixed effects. I cluster the standard errors at the district level.

Table 5: Economic Factors Affecting Switch-over

	(1)	(2)	(3)
	ols	ols	ols
VARIABLES	switchover_rate	switchover_rate	switchover_rate
unemployment	0.008		0.008
	(0.005)		(0.005)
landprice		0.015*	0.015**
		(0.008)	(0.007)
Constant	-0.013	-0.018	-0.031
	(0.046)	(0.012)	(0.048)
District FE	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes
Observations	2,076	2,206	2,036
Districts	173	173	173
R-squared	0.747	0.721	0.745

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

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) 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 5. Column (2) shows that the correlation

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

between land prices and switch-over intensity is statistically significant at the 10 percent level, but the coefficient is very close to zero. Column (3) shows that including both unemployment at the district level and land prices does not change the correlations. As previously mentioned, these land prices are not the same as residential or commercial property prices, so they may not actually reflect the economic prosperity of a community. Because of the lack of association between unemployment and switch-over and the weak relationship between land prices and switch-over, 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.

The exogeneity of the reform means that women cannot affect 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 137 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; however, only seven of these women moved whilst having primary school aged children, thereby changing their treatment status. This is a small percentage of my treated sample, but something I will address in the Robustness section.

The GSOEP also 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.[11]

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:

[11] 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 6.

Table 6: Estimates on Child's School Hours

VARIABLES	hrsschool
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

$$SHours_{it} = \alpha_0 + \beta\, FDS_{it} + \gamma_i + \theta_t + e_{it} \quad (1)$$

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 6 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.

4.3 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 employment status and one looking at changes in hours. This allows me to explore the impact of the policy on the extensive and intensive margins. My variable of interest is "treatment," which is the interaction of whether or not a woman in the sample has access to a full day school and primary school children. For this reason, I always include the binary variables for access and primary school children in all regressions.

The model for employment status, whether or not the woman is employed, takes the following form, where E_{it} is a binary variable that takes the value "1" when the woman is employed and "0" otherwise:

$$E_{it} = \alpha_0 + D_{it}\delta + \eta_i + \phi_{st} + X_{it}\beta + \varepsilon_{it} \quad (2)$$

All of the employment models are estimated either as linear probability or conditional logit models because of the binary outcome measure. This model does not take into account whether or not the woman is working part or full time, but rather pure, binary employment status. In all models, "i" signifies "individual" and "t" signifies "year." This specification allows for the inclusion of the treatment variable, D_{it}, which can switch back and forth between 0 and 1 depending on the woman's treatment status. These regressions also include an individual fixed effect, η_i, state-year dummies, ϕ_{st}, 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 employment status. Similarly, the state-year dummies should explain any variance in employment status caused by events occurring in a specific year in the state of residence, i.e. larger macroeconomic events. Since individual decisions to supply labor could be correlated over time, it makes sense to cluster at the individual level even though the treatment is occurring at the school level.

The variables included in the vector X_{it} include a binary variable for whether or not the woman has primary school aged children, a binary variable for whether or not she has access to a full day school, a binary variable for whether or not the woman has pre-school aged children, and a binary variable for whether or not she has children in secondary school. These variables are included in order to disentangle the general effects of being a mother on labor supply that are not affected by this policy. These covariates are summarized in Table 1.

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

$$H_{it} = \alpha_0 + D_{it}\delta + \eta_i + \phi_{st} + X_{it}\beta + \epsilon_{it} \tag{3}$$

Here H_{it} is a continuous variable representing either level hours of work or the logarithm of hours worked. The vector X_{it} includes the same covariates as in the employment regressions and again, the standard errors, ϵ_{it}, are clustered at the individual level.

I estimate the employment and hours 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, 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 employment status. By separating the two, however, I am able to disentangle the extensive from the intensive margin for more nuanced analysis.

This empirical strategy, however, does not take potential spillover effects into account. There are 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

getting squeezed out or having their hours reduced. The workers who exit the labor market could be women who do not have primary school aged children or men. 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. These are all things to keep in mind when thinking about the policy implications of this type of reform.

5 Results

All regressions presented in this section include the full sample of all women aged 15-64, who live in the four states of interest during the period 2000-2012. Because the treatment variable is defined as the interaction of the variable for access and the variable for having primary school aged children, the control group is actually composed of two different sub-groups: women who have primary school aged children, but do not have access to a full day school, and women who do not have primary school aged children, but have access to a full day school. In order to disentangle the control group, I also run the same models on a sub-sample of only women with primary school aged children. These results follow a discussion of the main employment and hours regressions on the entire sample.

5.1 Full sample

In Table 7 I present the results from the regressions on employment using the full sample. 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 being treated, having access to a full day school when you have primary school aged children, increased the probability of being employed by 6.9 percent.[12] This effect is statistically significant at the five percent significance level. The sign and significance of this 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 and state-year dummies with standard errors clustered at the individual level. The marginal effect associated with the logit coefficient on the treatment variable reported in Column (2) is 0.075, which is comparable to the estimate obtained through OLS.

As might be expected, having primary school aged children, pre-school aged children, or secondary school aged children all decrease the probability of being employed. There is clearly a negative effect of being a mother on employment, which matches the previously discussed statistics of West

[12]This marginal effect on labor force participation is (0.071-0.002), the difference between the coefficient on the treatment variable and the coefficient on the access variable. The coefficient on the treatment variable, 0.071, measures the reduction in the negative impact of having primary school aged children on labor market participation from gaining access to a full day school.

German mothers' employment. Once women have children they exit the labor market and often do not return.

Using a back of the envelope calculation, I estimate that this 6.9 percent increase in the probability of being employed translates into a less than one percentage point increase in overall female employment over this period. This is rather small in terms of a change at the extensive margin in the macro picture since over the period 2000-2012, female employment in Germany increased by approximately 10 percent (ILOStat).

The potential of this 6.9 percent 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, scaling up the size of the treatment by switching over all primary schools could increase overall female employment by seven percentage points, which would be very significant in the German context.

All of these results are being estimated on the "switchers," women who changed their labor market status as a result of the treatment. This is because of the nature of the difference-in-difference estimation; we are interested in how the treated individuals' employment status changes across periods compared to the non-treated sample's changes. In the case of the conditional logit model, women whose employment status does not change across periods are actually dropped in the estimation. This is why both the number of observations and the number of individuals are lower than the numbers reported in the Descriptive Statistics in Table 1. This reinforces the point that women are being drawn into the labor market and there is actually a change at the extensive margin as a result of this reform.

Turning to the intensive margin, I find no impact of the reform on hours worked. These regressions are only being estimated on women who report a positive number of hours worked with zero hours being treated as missing. This means that any changes in hours will reflect changes at the intensive margin. As the coefficients on the treatment variable in Table 8 show, the effect on both level and log hours is small and not statistically different from zero. For women who were already working before getting treated, their treatment did not cause them to change their working hours. 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. As the stylized facts from Germany and the data showed, many women are working part time. An increase of the school day by a few hours per day might not provide enough childcare to allow women already working to transition from part time to full time work.

Another explanation could be a positive income effect as a result of this implicit childcare subsidy. As the women in the sample become wealthier as a result of the free childcare, they could be substituting away from labor to leisure. If women have heterogeneous preferences, then some women could increase their hours as a result of the policy change and some women could decrease

Table 7: Estimates on Employment

	(1) ols	(2) logit
VARIABLES	employed	employed
Treatment	0.071**	0.756**
	(0.031)	(0.299)
Has primary school children	-0.053***	-0.467***
	(0.012)	(0.096)
Has access to FDS	-0.002	-0.012
	(0.014)	(0.133)
Has pre-school children	-0.324***	-2.334***
	(0.016)	(0.118)
Has secondary school children	-0.041***	-0.360***
	(0.009)	(0.081)
Individual FE	Yes	Yes
State-year dummies	Yes	Yes
Observations	17,234	17,234
Individuals	2,161	2,161
(Pseudo) R-squared	0.656	0.083

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

them. On average this effect will appear as zero, which could explain the coefficients in these regressions as they estimate the average effect on hours.

In Tables 7 and 8, the coefficient on the access variable is small, negative, and not statistically significant. The access variable is a binary variable for whether or not a woman's closest primary school is a full day school. This essentially means that the access variable is picking up the effect of living in a certain geographic region at a certain time. Since it is small and not statistically significant, this means that we do not observe any region-time specific effect associated with the school reform that is influencing labor supply.

Another point to consider when assessing these results are rigidities in the labor market. If there are rigidities that prevent varying hours or extending contracts right away, we might expect delays in how the treatment affects outcomes. It is possible that although women might be made aware of their closest primary school switching over to become a full day facility, they are unable to alter their employment status or current hours worked right away due to the rigidity of contracts. At the same time, schools might not announce their switch-over until shortly before the school year begins, which would not allow many women the chance to change their labor supply right away in response to the treatment. I check for a delay in the treatment effect by regressing the aforementioned outcome variables on a lagged treatment variable. Here the lagged treatment variable is the interaction of a lagged access variable and the non-lagged variable for having primary school aged children. This is because the lag should arise as a result of delayed access, not delays in becoming a parent.

Table 8: Estimates on Hours

VARIABLES	(1) ln(weekly hours)	(2) weekly hours
Treatment	-0.069	-1.339
	(0.050)	(0.853)
Has primary school children	-0.104***	-2.689***
	(0.017)	(0.343)
Has access to FDS	-0.012	-0.506
	(0.020)	(0.461)
Has pre-school children	-0.407***	-8.830***
	(0.031)	(0.612)
Has secondary school children	-0.050***	-1.299***
	(0.015)	(0.305)
Individual FE	Yes	Yes
State-year dummies	Yes	Yes
Observations	20,985	20,985
Individuals	4,614	4,614
R-squared	0.756	0.784

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

Table 9: Estimates using Lagged Treatment

	(1) ols	(2) logit	(3) ols	(4) ols
VARIABLES	employed	employed	ln(weekly hours)	weekly hours
Treatment	0.053	0.695*	-0.074	-1.627*
	(0.038)	(0.422)	(0.069)	(0.959)
Lagged treatment	0.005	0.085	-0.010	-0.080
	(0.043)	(0.530)	(0.064)	(1.060)
Has primary school children	-0.044***	-0.411***	-0.092***	-2.306***
	(0.013)	(0.111)	(0.019)	(0.371)
Lagged access to FDS	0.007	0.085	0.012	-0.055
	(0.016)	(0.157)	(0.022)	(0.508)
Has pre-school children	-0.327***	-2.422***	-0.403***	-8.604***
	(0.018)	(0.136)	(0.035)	(0.679)
Has secondary school children	-0.010	-0.050	-0.045***	-1.176***
	(0.010)	(0.097)	(0.016)	(0.332)
Individual FE	Yes	Yes	Yes	Yes
State-year dummies	Yes	Yes	Yes	Yes
Observations	28,637	13,282	17,248	17,248
Individuals	5,343	1,713	3,686	3,686
(Pseudo) R-squared	0.660	0.084	0.761	0.792

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

The results from these regressions are presented in Table 9. Here the results show that the lagged treatment variable has a small coefficient in all of the regressions. In the hours regressions, it is small and negative, and in the employment regressions, it is small and positive. The inclusion of the lagged treatment variable does not change the treatment variable from having a statistically significant effect of increasing the probability of being employed, as estimated by the logit regression. The marginal effect associated with this logit coefficient is 0.072, which is similar to the treatment coefficient calculated in the non-lagged treatment regressions. The introduction of the lagged treatment variable does, however, induce a small, negative, and statistically significant impact on weekly hours. In these results we observe negative coefficients on the lagged treatment variable in the regressions on level and log hours, perhaps indicating an income effect or labor market rigidities that do not allow women to simply extend their working hours by a small, incremental amount at a current job. These coefficients, however, are not statistically different from zero and very small.

The inclusion of the lagged treatment variable in Table 9 does not change the coefficients on the treatment variable significantly from those results presented in Tables 7 and Table 8. The lagged access variable in Table 9 is never statistically significant, indicating that there is no region-time specific effect of the school reform impacting labor supply. The lack of any observable difference here indicates that there is no increased effect on labor supply as a result of being treated one year earlier.[13]

5.2 Mothers only

All of the regressions presented in Tables 7, 8, and 9 include both women with and without primary school aged children. As previously mentioned, this essentially means that there are two control groups for the treated women: those who had access to a full day school, but do not have primary school aged children, and those who have primary school aged children, but did not have access. I have controlled for whether or not a woman has primary school aged children in every regression, as well as whether or not she has access to a full day facility, but the mixture of this control group is something to keep in mind when analyzing the results. If we believe that women who have children are fundamentally different than women who do not, then the common time trends assumption may be violated for this portion of the control group and the treated women.

In order to account for this potential issue, I run the same models using the sub-sample of only women who have primary school children. Since all of these women have primary school aged children, I no longer need to interact the binary variable for having primary school aged children with the binary variable for having access to a full day school to get my treatment variable. Because

[13] I also estimate the same models presented in Table 9, but only include the lagged treatment variable. The coefficients on this variable are similar in sign and magnitude on the treatment variable in Tables 7 and 8, but are not statistically significant. This confirms there is no effect on labor supply from being treated one year earlier.

Table 10: Estimates on Mothers with Primary School Aged Children Only

	(1) ols	(2) logit	(3) ols	(4) ols
VARIABLES	employed	employed	ln(weekly hours)	weekly hours
Has access to FDS	0.029	0.178	0.020	0.119
	(0.060)	(0.630)	(0.112)	(1.601)
Has pre-school children	-0.148***	-1.308***	-0.089	-1.776
	(0.037)	(0.293)	(0.077)	(1.134)
Has secondary school children	0.016	0.135	-0.000	-0.011
	(0.023)	(0.209)	(0.033)	(0.529)
Individual FE	Yes	Yes	Yes	Yes
State-year dummies	Yes	Yes	Yes	No
Observations	1,216	1,216	2,179	2,179
Individuals	233	233	751	751
(Pseudo) R-squared	0.727	0.173	0.815	0.854

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

of this, the variable of interest is now whether or not they had access to a full day school. I also do not need to control for whether or not they have primary school aged children, since they all do.

The results of these regressions may be seen in Table 10.[14] The coefficients on the access variable in Table 10 exhibit similar signs and magnitudes as the previous regressions run on the full sample, but are not statistically significant. The coefficient on the variable for access to a full day school is small and positive in all regressions, including the hours regressions. The marginal effect associated with the reported logit coefficient in Table 10 is 0.020, which again is very similar to the effect size estimated using OLS. The similarity in signs and magnitudes on the employment regressions, but loss of significance may be caused by the much smaller sample size and the loss of power as a result. The sample size has decreased by a significant amount. In these regressions, I am estimating the employment regressions off of a sample of only 233 women as compared to 2,161 women in the full sample regressions. Here I estimate the hours regressions using 751 women and in the full sample I have 4,614 women. This is due in part to the fact that this is an unbalanced panel and I do not always observe women when they have primary school aged children and also to the fact that many women in the sample do not have any children. These decreases in sample size, however, cause me to lose significant power in these regressions.

Overall, these similar results in terms of signs and magnitudes, indicate that the inclusion of women who do not have primary school children in the sample is not driving the results.

[14] I also ran the same employment and hours regressions on a different sub-sample of mothers. This time, I included any woman who has ever been a mother, not just the women I observe at the time they have primary school children. In this specification, the magnitudes and signs of coefficients were similar to the mothers-only regressions presented in this section and for this reason have not been included in this paper.

Table 11: Heterogeneous Treatment Effects: Single Mothers

	(1) ols	(2) ols	(3) ols
VARIABLES	employed	ln(weekly hours)	weekly hours
Treatment	0.061*	-0.090	-1.755*
	(0.036)	(0.056)	(0.937)
Single mother	-0.151***	0.024	0.355
	(0.016)	(0.029)	(0.619)
Treatment*Single mother	0.049	0.116	2.298
	(0.062)	(0.104)	(1.827)
Has primary school children	-0.052***	-0.104***	-2.696***
	(0.013)	(0.017)	(0.343)
Has access to FDS	-0.003	-0.011	-0.491
	(0.014)	(0.020)	(0.462)
Has pre-school children	-0.319***	-0.408***	-8.840***
	(0.016)	(0.031)	(0.611)
Has secondary school children	-0.019**	-0.052***	-1.330***
	(0.009)	(0.015)	(0.307)
Individual FE	Yes	Yes	Yes
State-year dummies	Yes	Yes	Yes
Observations	36,149	20,985	20,985
Individuals	6,964	4,614	4,614
R-squared	0.659	0.756	0.784

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

5.3 Heterogeneous Treatment Effects

I also explore the possibility of heterogeneous treatment effects by interacting the treatment variable with demographic control variables since the effects of the treatment may differ for different women. First, I look at single mothers and then at women whose husband is employed since these two types of women could have fundamentally different responses to being treated.

In Table 11, I present the results from the interaction of the treatment variable with a binary variable for whether or not the woman is a single mother. 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.

These results do not show a statistically significant heterogeneous treatment effect for single mothers versus the rest of the sample. Even though the coefficient on the interaction term of treatment and single mothers is positive in all three regressions, it is not statistically different from zero. This indicates that the treatment has not been different for single mothers in this linear framework. Interestingly, in Column (1) the coefficient on the binary variable for single mothers is statistically significant at the one percent level and large. This implies that single mothers are much less likely to be employed than the rest of the women in this sample, which is a surprising result.

Turning to look at heterogeneous treatment effects for women whose husband is employed versus those whose husband is not working, I find some statistically significant effects. Whether or not the

Table 12: Heterogeneous Treatment Effects: Working Husband

	(1) ols	(2) ols	(3) ols
VARIABLES	employed	ln(weekly hours)	weekly hours
Treatment	-0.023	0.099	3.826
	(0.053)	(0.080)	(2.945)
Husband works	0.029**	0.000	0.154
	(0.013)	(0.018)	(0.385)
Treatment*Husband works	0.088	-0.180**	-5.623*
	(0.056)	(0.086)	(2.919)
Has primary school children	-0.043***	-0.094***	-2.404***
	(0.013)	(0.019)	(0.377)
Has access to FDS	0.002	0.009	-0.175
	(0.017)	(0.024)	(0.489)
Has pre-school children	-0.304***	-0.388***	-8.328***
	(0.018)	(0.033)	(0.644)
Has secondary school children	-0.031***	-0.050***	-1.364***
	(0.010)	(0.017)	(0.336)
Individual FE	Yes	Yes	Yes
State-year dummies	Yes	Yes	Yes
Observations	26,712	15,526	15,526
Individuals	5,483	3,634	3,634
R-squared	0.666	0.772	0.806

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

husband is employed is an indicator of socio-economic status and economic security of the family. At the same time, it might serve as a predictor of whether or not the woman decides to work.

Table 12 shows the results from interacting the treatment variable with the binary variable for whether or not the woman's husband is employed. In the hours regressions in Columns (2) and (3), I find that the women who are treated whilst their husband is employed decrease their hours. This would indicate that the negative coefficient on the treatment variable in many of the hours regressions is driven by an income effect. These women are potentially more economically secure due to their working partner and can substitute away from labor towards leisure as a result of the implicit childcare subsidy. I do not find any significant heterogeneous effect for these women in the employment regression in Column (1). Overall the evidence for heterogeneous treatment effects along the dimensions explored in this section is weak.

6 Robustness

6.1 Alternative clustering

As a robustness check, I run the same models presented in the Results section, but cluster the standard errors differently. The results in Table 13 show that clustering on the school variable did not change the standard errors enough to change the significance test results of any of the

Table 13: Results with SE's Clustered at the School Level

	(1) ols	(2) logit	(3) ols	(4) ols
VARIABLES	employed	employed	ln(weekly hours)	weekly hours
Treatment	0.071**	0.756**	-0.069	-1.339
	(0.030)	(0.297)	(0.050)	(0.816)
Has primary school children	-0.053***	-0.467***	-0.104***	-2.689***
	(0.012)	(0.102)	(0.016)	(0.324)
Has access to FDS	-0.002	-0.012	-0.012	-0.506
	(0.014)	(0.143)	(0.019)	(0.438)
Has pre-school children	-0.324***	-2.334***	-0.407***	-8.830***
	(0.016)	(0.133)	(0.028)	(0.556)
Has secondary school children	-0.041***	-0.360***	-0.050***	-1.299***
	(0.008)	(0.082)	(0.014)	(0.303)
Observations	17,223	17,223	20,985	20,985
(Pseudo) R-squared	0.656	0.083	0.756	0.784

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

coefficients, even though there are fewer schools than individuals in the data set. It seems logical in this context to cluster at the individual level since labor market decisions are ultimately being made by the individual and this is where correlation in the error term could arise.

Table 14 shows the results of clustering the standard errors along both the person and the time dimension.[15] Cameron et al. (2011) outline the advantages of two-way or multiway clustering, especially in panel contexts. In this case, the two-way clustering changes the statistical significance of the coefficient on log weekly hours. The coefficient is still negative and this decrease amounts to approximately one hour and twenty minutes per week. This result is similar to Contreras et al. (2010), who also found that the extension of the school day in Chile had a negative impact on mothers' hours worked. Contreras et al. (2010) pointed to an income effect as the explanation for this negative effect: as the women in the sample become wealthier as a result of the implicit childcare subsidy, they substitute away from labor towards leisure.

6.2 Dropping teachers

In my previous analysis, I have ignored any possible impacts 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 the impact it may have had 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.

[15]It should be noted that the coefficients in this specification differ slightly than previous specifications, which is due to the fact that this specification only includes year dummies and not state-year dummies. Because this analysis must be undertaken on the servers at the German Institute for Economic Research (DIW), I have not yet had the chance to run the new specification with state-year dummies.

Table 14: Two-way Clustered Results

	(1) ols	(2) ols	(3) ols
VARIABLES	employed	ln(weekly hours)	weekly hours
Treatment	0.072**	-0.070	-1.335*
	(0.032)	(0.043)	(0.749)
Has primary school children	-0.057***	-0.109***	-2.747***
	(0.013)	(0.018)	(0.417)
Has access to FDS	0.009	0.002	-0.314
	(0.013)	(0.018)	(0.402)
Has pre-school children	-0.329***	-0.411***	-8.853***
	(0.017)	(0.024)	(0.473)
Has secondary school children	-0.043***	-0.053***	-1.323***
	(0.008)	(0.014)	(0.326)
Individual FE	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes
Observations	34,578	19,768	19,768
R-squared	0.049	0.054	0.057
Individuals	5,385	3,391	3,391

Two-way cluster-robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

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 working hours do not extend beyond school hours. Additionally, many teachers in Germany also work part time. Of all teachers working in primary schools in the 2012-2013 school year, 38.65 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.[16] This leaves me with 6,695 women. I then run the same regressions on employment status and hours on this sub-sample. These results may be seen in Table 15.

These results are similar to the results on the full sample in terms of the employment regressions. Columns (1) and (2) in this table show that women who are treated and have never worked as teachers are still approximately 7 percent more likely to be employed. The marginal effect associated with the logit coefficient in Column (2) is 0.069. 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

[16] 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.

Table 15: Estimates Without Teachers

	(1) ols	(2) logit	(3) ols	(4) ols
VARIABLES	employed	employed	ln(weekly hours)	weekly hours
Treatment	0.069**	0.692**	-0.092*	-1.633*
	(0.032)	(0.301)	(0.048)	(0.861)
Has primary school children	-0.050***	-0.435***	-0.103***	-2.635***
	(0.013)	(0.100)	(0.018)	(0.354)
Has access to FDS	-0.002	-0.014	-0.017	-0.553
	(0.015)	(0.135)	(0.020)	(0.473)
Has pre-school children	-0.323***	-2.352***	-0.411***	-8.790***
	(0.017)	(0.123)	(0.032)	(0.630)
Has secondary school children	-0.041***	-0.359***	-0.046***	-1.189***
	(0.010)	(0.082)	(0.015)	(0.310)
Individual FE	Yes	Yes	Yes	Yes
State-year dummies	Yes	Yes	Yes	Yes
Observations	16,135	16,135	19,627	19,627
Individuals	2,040	2,040	4,345	4,345
(Pseudo) R-squared	0.659	0.083	0.761	0.792

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

sample. Now the negative effect of being treated on hours worked, both the log and level, is statistically significant at the 10 percent level; however, the size of the coefficients is similar to 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.

6.3 Dropping the 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. Additionally, if the women who want to move closer to a full day school are women who have a strong preference to work, this would overstate the importance of access to a full day school on mothers with primary school aged children. This is why I run the same participation and hours models on a sub-sample of women that excludes 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 actually know whether or not this is the reason these women have moved (this is not explicitly asked in the SOEP), I still drop these women as an additional robustness check.

I only observe seven women in the data who move house and thereby directly change their treatment status. These women all had primary school aged children at the time of their move and move from a school without a full day to a school with a full day. I first drop these seven women to ensure

Table 16: Estimates Without Women Who Move and Change Treatment Status

	(1) ols	(2) logit	(3) ols	(4) ols
VARIABLES	employed	employed	ln(weekly hours)	weekly hours
Treatment	0.075**	0.790**	-0.064	-1.251
	(0.032)	(0.312)	(0.051)	(0.869)
Has primary school children	-0.054***	-0.472***	-0.105***	-2.700***
	(0.012)	(0.097)	(0.017)	(0.345)
Has access to FDS	-0.003	-0.019	-0.012	-0.502
	(0.014)	(0.134)	(0.020)	(0.463)
Has pre-school children	-0.322***	-2.328***	-0.408***	-8.830***
	(0.016)	(0.119)	(0.031)	(0.617)
Has secondary school children	-0.041***	-0.369***	-0.051***	-1.302***
	(0.009)	(0.081)	(0.015)	(0.307)
Individual FE	Yes	Yes	Yes	Yes
State-year dummies	Yes	Yes	Yes	Yes
Observations	17,162	17,162	20,948	20,948
Individuals	2,154	2,154	4,608	4,608
(Pseudo) R-squared	0.656	0.083	0.756	0.784

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

that my treatment effects are not being driven by these "movers." The results of this analysis may be seen in Table 16. As this table shows, dropping the seven women who moved house and thereby changed their treatment status does not impact the results. I still find a positive and statistically significant impact of the policy at the extensive margin and no effect at the intensive margin.

It is possible that some families decide to move a few years before their children are ready to enter primary school and still choose to move closer to a full day school. I observe a total of 137 women who move house from a home where the closest primary school was not a full day school to a home where the closest school is a full day school. As previously mentioned, I do not know the reason for their move, however, in the interest of robust results, I also drop these women from the sample in case they were anticipating the implicit childcare subsidy in advance and thereby changing their treatment status. These results may be seen in Table 17.

These results are similar to the results obtained on the full sample, although the magnitude of the coefficients on the employment regressions increases when I drop all the women who have moved to be closer to a full day school. The marginal effect associated with the logit coefficient reported in Column (2) is 0.103, which is larger than in the standard regressions on the full sample. Statistically speaking, however, these differences are negligible and reinforce the point that women are not selecting into treatment.

These robustness checks on women moving house in order to select a full day primary school show that this is not driving the overall results I find. The extended school day has enabled mothers to enter the labor market if they were not working before and has not impacted the number of hours they were working.

Table 17: Estimates Without Women Who Move

	(1) ols	(2) logit	(3) ols	(4) ols
VARIABLES	employed	employed	ln(weekly hours)	weekly hours
Treatment	0.094***	1.006***	-0.034	-0.705
	(0.036)	(0.370)	(0.057)	(0.920)
Has primary school children	-0.055***	-0.483***	-0.101***	-2.601***
	(0.013)	(0.099)	(0.018)	(0.357)
Has access to FDS	-0.012	-0.095	-0.008	-0.426
	(0.017)	(0.165)	(0.023)	(0.511)
Has pre-school children	-0.322***	-2.356***	-0.389***	-8.211***
	(0.017)	(0.127)	(0.032)	(0.637)
Has secondary school children	-0.041***	-0.372***	-0.050***	-1.291***
	(0.010)	(0.084)	(0.015)	(0.315)
Individual FE	Yes	Yes	Yes	Yes
State-year dummies	Yes	Yes	Yes	Yes
Observations	15,793	15,793	19,531	19,531
Individuals	2,025	2,025	4,418	4,418
(Pseudo) R-squared	0.664	0.083	0.766	0.796

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

7 Conclusion

The reform to extend the 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 building cafeterias and hiring new teachers. This lag in the reform has created a unique natural experiment, which allows me to look at how extending school hours affects maternal labor supply in a way few other studies have previously been able to do.

I find robust effects of the extension of the primary school day on maternal labor supply. Mothers of primary school aged children are roughly seven percent more likely to enter the labor market once they are given access to a full day primary school. This result is robust to changing the sample to include only mothers of primary school aged children and to dropping women who may have moved house in order to live near a full day school. It is a large effect that shows this policy has been successful at drawing mothers into the labor market.

At the intensive margin, I find less robust evidence. In most specifications, the effect of the reform on hours worked is small, negative, and statistically insignificant. In the few specifications where the coefficient is statistically significant, its magnitude is still very small and negative. This result of mothers decreasing their working hours when given an implicit childcare subsidy can be explained by the income effect of the implicit childcare subsidy dominating the substitution effect when leisure is a normal good. The unintended consequence of mothers potentially reducing working hours is something of which policymakers should be aware.

Childcare policies may be used to draw mothers into the labor market after having children or extend their working hours if already working; however, childcare costs and the length of the school day must be taken into consideration by policymakers. In a country such as Germany, where female labor supply is dominated by part time work and stay-at-home mothers, such policies can enact fundamental change to the labor market if implemented appropriately.

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 programme (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 18 shows that places at such facilities were extremely limited in the states studied during the period of the reform.

Table 18: 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	18.6	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 et al. 2008). Since education is a federal 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, something I will discuss later on. I have collected a unique data set on when every primary school in four German states began operating as a full day school

(see Section 3 for further discussion of the data and these states) in order to evaluate the impact the reform has had on female labor supply.

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*) (Robelen, 2005). Because there are no national standardized tests in Germany, it is very difficult, and not encouraged, to compare the education systems across states.

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; however, in the data collection and contacting of Ministries of Education for this project, it seems that many schools offer the extended instruction every day of the week.

The type of full day school issue, however, may pose a challenge to my analysis. 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. In discussions with the Ministries of Education for this paper, there confirmed that there has not been massive over-subscription or issues of shortage surrounding full day school places, which makes me confident in combining these two types of full day schools in my analysis.

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