The Irrelevance of Subsidized Child Care for Maternal Employment: The Norwegian Experience

Tarjei Havnes* and Magne Mogstad†

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PRELIMINARY VERSION

Abstract

The strong correlation between child care and maternal employment have led researchers and policymakers alike to conclude that subsidized and readily available child care is a driving force both of cross-country differences in maternal employment and of its dramatic growth over the last decades. However, child care and maternal employment are simultaneously determined; instead of child care inducing mothers to work, it may be that increased maternal employment causes political pressure towards good access to cheap child care. Exploiting the unprecedented expansion in child care coverage in Norway after the passage of the Kindergarten Act in 1975, we find that there is little, if any, causal effect of subsidized child care on maternal employment, despite a strong correlation. The new subsidized child care crowds out informal child care arrangements, with almost no net increase in total child care use or labor supply. To identify the causal effect of subsidized child care on maternal employment, we use a difference-in-difference approach. Extensive robustness analyses support our precisely estimated results.

*ESOP and Dept. of Economics, University of Oslo. E-mail: tarjei.havnes@econ.uio.no

†Research Dept. of Statistics Norway. E-mail: magne.mogstad@ssb.no

1 Introduction

The massive increase in female labor force participation in developed countries since the 1960s, was led prominently by married mothers of young children (Cohany and Sok, 2007). Alongside the rapid increase in maternal employment, child care coverage has often grown dramatically. Today, mothers of young children are much more likely to work in countries with high child care coverage(see e.g. Jaumotte 2003). For example, Norway has practiced one of the most expansive child care policies, and achieved one of the highest maternal employment rates.[1] Figures 1 and 2 show labor force participation by gender, marital status, and age of the youngest child as well as child care coverage rate in Norway from 1960 to 1996.[2]

[Figure 1 about here.]

[Figure 2 about here.]

That good access to affordable child care would facilitate labor force participation of mothers, seems intuitive, and squares well with standard economic theories. It is therefore not surprising that generous child care policies are claimed as a key determinant both of cross country differences in maternal employment (e.g. Rosen 1996, Jaumotte 2003, Del Boca 2002, Aaberge et.al. 2005), and of the strong growth among mothers with young children (Attanasio, Low and Sanchez-Marcos 2008). A standard economics argument is that mothers must replace hours of market work with hours of child care almost one-for-one. Therefore, the price of child care is effectively a tax on the mothers wage, and any policies driving down this price is expected to have a strong and direct impact on mothers' labor supply decision. However, mothers with better access to affordable child care may be inherently different, and could be more inclined to work in any case. This raises the question: Does the correlation between child care and maternal labor force participation reflect a causal relationship?

[1] In the early 70s, Norwegian mothers were about as likely to work as their sisters in other western countries, like the US, the UK, and Germany, but pulled ahead during the late 70s and 80s. Today, Norwegian mothers are at the top of international rankings with an about ten percentage points higher employment rate than in these other countries. See e.g. Dolado et al. (2001).

[2] Throughout this paper, child care coverage rates refer to formal care, including publicly and privately provided child care institutions as well as licensed care givers, all eligible to subsidies from the government.

In this paper, we use the passing of the Kindergarten Act of June 1975 in Norway as a testing ground for the causal relationship between child care and maternal employment. The Kindergarten Act was passed in response to an increase in the demand for child care, and assigned the responsibility for higher child care coverage to local municipalities. In the succeeding years, access to subsidized child care surged for children aged 3 to 6 years. To identify the effect of subsidized child care on maternal employment, we exploit that the implementation of the child care reform occurred in different municipalities at different times, creating considerable regional as well as time variation in the rate of child care expansion. Having access to panel data from administrative registers covering the entire Norwegian population and all licensed care givers from 1972, we are able to pay close attention to the fact that the implementation of the reform could be endogenous. We address the endogeneity problem by applying a difference-in-difference (DD) approach, comparing the growth rate in employment of mothers with children aged 3 to 6 years living in municipalities where child care coverage expanded a lot (i.e. the treatment group), with the growth rate for mothers with children in the same age group who live in municipalities with little or no increase in child care (i.e. the comparison group).

The main results of our analysis can be seen in figure 3, showing child care coverage and maternal employment rates in Norway before and after the 1975 reform. Whereas the graph of the child care coverage rate in the treatment group kinks heavily after the reform, the graphs of employment rates in the treatment and comparison groups move almost in parallel. Consistent with this evidence, our estimation results provide strong and robust evidence that the large expansion in subsidized child care had little, if any, effect on maternal employment.

[Figure 3 about here.]

Our baseline result indicates that the expansion caused an average of less than 0.04 percentage points increase in maternal labor force participation per percentage point increase in the child care coverage rate. The 95 percent upper bound on the effect is a mere 0.08 increase per percentage point increase in child care coverage. Applying this to the historic trend, child care expansion can explain about 2 of the 55 percentage point rise in maternal labor force participation in Norway from 1972 to 1996 (the upper bound at about 4 points). By the same token, cross-country differences in child care are only able to account for a small fraction of the gap in maternal employment between Norway and other western countries.

To explain the lack of causality from child care to maternal employment—considering the strong correlation between the two—we propose a parsimonious model of the formal and informal child care market as we perceive it from the mid 1970s. Heavily subsidized supply contingent on a maximum price, caused substantial rationing of formal child care. This demand surplus was serviced in part by an informal market for child care, including both paid and unpaid forms of care (e.g. relatives, friends and neighbors), considered inferior by many households. The dissatisfaction of these rationed households caused political pressure that may ultimately have triggered the passage of the Kindergarten Act. We postulate that the implementation in specific municipalities depended on the local political pressure from rationed households. Since such pressure should have been highest where women were more likely to work in the first place, strong local expansion came as a response to an already high local labor force participation among mothers, rather than facilitating its rise. Interpreted in this way, our baseline estimate suggests that the new subsidized formal child care was associated with a 96 percent crowding out of informal care.

There exists a considerable literature on child care and maternal participation. As emphasized in the survey by Blau and Currie (2006), most previous empirical studies have suffered from two main problems: Child care coverage and prices are endogenous to the work decision of mothers, and the availability and cost of informal care is inherently unobserved. Some recent studies, to which our paper relates, have employed experimental methods to address these problems, including DD estimation and instrumental variables.[3]

Our contribution is threefold. First, and foremost, our results are germane for ongoing policy debates in many European countries as well as in the United States and Canada about a move towards subsidized, universally accessible child care (or preschool). For example, the European Union's Presidency recently formulated the need to remove *"barriers and disincentives for female labour force participation by, inter alia, improving the provision of childcare facilities"* (EU 2002, p. 42). Interestingly, the rates of

[3]See Gelbach (2002), Schlosser (2005), Berlinski and Galiani (2007), Baker, Gruber and Milligan (2008), Lefebvre and Merrigan (2008), Lundin, Mörk and Öckert (2008), and Cascio (2009).

maternal employment and child care coverage in 1970s Norway mirror quite well the situation in many other western countries today. As Norway has since practiced one of the most expansive child care policies, and achieved one of the highest maternal employment rates, it is therefore not surprising that researchers and policymakers alike have their eyes on the Norwegian experience and its apparent success. Our findings suggest, however, that the introduction of publicly subsidized child care mainly lead to a substitution from other forms of out-of-home care, with almost no impact on labor supply. This in turn implies a significant net cost of the policy, since subsidies are only marginally offset by an expansion in the tax base.

Second, because of the often slow and uniform expansion of child care over time and space, most previous studies have been confined to rely on limited variation in the data to identify the effect of child care on maternal employment. By contrast, our study compares municipalities that differ distinctly in terms of changes in child care coverage within a relatively narrow time frame. The shear strength of the variation in our data implies that any actual effect should be present in our precise estimates. That we find little, if any, effect therefore serves as strong evidence of a weak causal link from child care to maternal employment. Importantly, this conclusion holds true also when we estimate the model separately by age and education of the mother as well as family size.

Finally, our exceptionally rich data allows extensive control over both observed and unobserved heterogeneity. In previous studies applying a DD approach, a possible limitation of the identification strategy is that any shocks specific to the treatment areas that coincide with the policy changes will bias the estimate of the reform effect. We attempt to address this concern by applying a difference-in-difference-in-difference (DDD) approach, adding mothers with the youngest child just above child care age (7 to 10 years old) as a second comparison group to pick up time-varying effects specific to the treatment area. Another advantage of our data is the low level of aggregation, spanning more than 400 municipalities covering about 4 million people, which allows us to appropriately account for differences in treatment and comparison areas by including municipal-specific fixed effects as well as controls for changes in the local labor market conditions. This is also the first study of child care and maternal employment that applies a DD approach to panel data, instead of repeated cross-sections. By restricting the sample to the same mothers before and after the reform, we remove biases from comparison over time within the treatment group due to potentially unobserved compositional changes. Further, we include individual-specific fixed effects to eliminate biases in the comparison between mothers residing in treatment and comparison areas owing to permanent unobserved differences.

Section 2 proceeds by discussing briefly previous research on child care and maternal labor force participation. Next, section 3 outlines the Norwegian market for child care in the mid 70s, and the passage of the 1975 Kindergarten Act. Section 4 discusses our empirical strategy, before section 5 describes the data and presents descriptive statistics. Section 6 presents our main empirical results, whereas Section 7 reports the robustness analysis. Section 8 concludes with a discussion of policy implications.

2 Previous research on child care and maternal employment

There exists a considerable literature on child care and maternal employment. A frequently used approach is to estimate the effect of child care prices on maternal labor supply, attempting to correct for selection into labor force participation and sometimes also for use of formal child care (e.g. Connelly (1992) Ribar (1992), Kimmel (1995, 1998)). Others have estimated structural models of female labor supply and child care choice (e.g. Michalopoulous et al. (1992), Ribar (1995), Thoresen and Kornstad (2007)) or exploited cross-sectional geographical variation in child care prices (e.g. Blau and Robins, 1988). In a survey of this literature, Blau and Currie (2006) report estimated elasticity of employment with respect to price of child care ranging from 0 to values greater than -1. Differences in data sources and sample composition do not appear to account for much of the variation in the estimates. Instead, Blau and Currie point out two fundamental problems for these papers: Child care access and prices are endogenous to the work decision of mothers, and the availability and cost of informal care is inherently unobserved. A large part of the discrepancy between the estimates is therefore likely to be explained by differences in biases owing to ignoring the substitution between formal and informal child care, misspecifications of functional forms for the employment and child care equations, and in particular violations of the exclusion restrictions (e.g. identification through variation in child care prices that are not exogenous to the employment decisions).

Some recent studies, to which our paper relates, alleviate these problems by exploiting time and regional variation in the access to or price of child care owing to reforms in the provision of child care or pre school. Baker, Gruber and Milligan (2008) and Lefebvre and Merrigan (2008) study the implementation of a child care subsidy in Quebec, whereas Lundin, Mörk and Öckert (2008) study the introduction of a maximum price in Sweden. Schlosser (2005), Cascio (2009), and Berlinski and Galiani (2007) evaluate the impact of a free public child care/pre school program in Argentina, the United States, and Israel, respectively. All of these studies apply a DD approach.

The results in our paper are consistent with Cascio (2009) and Lundin, Mörk and Öckert (2008), in finding hardly any effect on maternal labor supply for married mothers of increased access to (or lower prices on) child care. Meanwhile, Schlosser (2005), Berlinski and Galiani (2007), Baker, Gruber and Milligan (2008) and Lefebvre and Merrigan (2008) report significant positive effects, albeit at the lower range of estimates reported by Blau and Currie (2006). For instance, Baker, Gruber and Milligan (2008) find that the introduction of universal child care subsidies in Quebec in 1997 led to a 14 percentage point increase in child care use, which was associated with a 7.7 percentage point increase in employment; the difference between the rise in employment and the rise in child care utilization reflects a considerable crowding out of informal care arrangements by the newly subsidized (formal) child care.

It is likely that at least part of the discrepancy in the estimates can be explained by differences in the population studied (and the data sources used). For example, it can be hard to generalize the results from Lundin, Mörk and Öckert (2008) to other countries, as the maternal employment rate was about 70 percent and child care utilization more than 80 percent before the reform. By contrast, the Norwegian experience from the late 1970s may be more relevant from an international perspective, since many OECD countries today have similar maternal employment rates and child care coverage.[4] Moreover, the labor supply responses may depend on the age of the youngest child. As opposed to Cascio (2009), we are not confined to study mothers of 5-year olds, but are able to consider the impact for mothers of children aged 3 to 6.

[4]In 1976 Norway, female labor force participation was 50.4% (NOS Labour Market Statistics, table 9.7), compared to 2000-levels of around 40% in Mediterranean countries and around 55% in central european countries (Boeri, Del-Boca and Pissarides 2005, p. 13).

Another reason for the differences in the estimates is that the identification strategies differ in a number of ways. As pointed out by Cascio (2009), one potential problem with several of the studies reporting significant labor supply effects is that the pre-reform trends of the treatment and comparison areas (when reported) often differ significantly. For example, as is evident from Lefebvre and Merrigan (2008), the trend in maternal employment is significantly different in Quebec compared to the rest of Canada prior to the reform. The positive labor supply response to this child care reform may therefore reflect differential time trends, rather than a true policy impact. Our paper addresses this issue by showing graphically that the pre-reform trends in maternal employment are similar in treatment and comparison areas. Moreover, we conduct a placebo-reform pretending that the policy changes took place in the pre-reform period; no effect of the placebo-reform increases our confidence in the empirical strategy.

Another concern with several of the previous studies is the high level of geographical aggregation. For example, Cascio (2009) identifies the policy effects by comparing changes in maternal employment rates between 50 US states, covering a population of about 200 million people. A concern is that the included state-specific effects are not sufficient to control for differences in treatment and comparison areas. The low level of aggregation in our data, spanning more than 400 municipalities covering about 4 million people, allows us to account for geographical heterogeneity by including municipality-specific fixed effects as well as controls for changes in the local labor market conditions.[5]

A possible limitation of the identification strategy in the previous studies is that any shocks specific to the treatment areas that coincide with the policy changes will bias their estimates. We attempt to address this concern by applying a DDD approach, adding mothers with the youngest child just above child care age (7 to 10 years old) as a second comparison group, to pick up time-varying effects specific to the treatment area.

Finally, previous studies of child care and maternal employment applying a DD approach have used repeated cross-sectional data. As we have access to panel data, we can improve on this by restricting the sample to the same mothers before and after the reform; this removes biases from compar-

[5]Heckman et al. (1998) demonstrate the importance in policy evaluations of controlling for variation in the local labour market conditions of those treated by the policy change and the comparison group.

ison over time within the treatment group due to unobserved compositional changes. Further, we can include individual-specific fixed effects to eliminate biases in the comparison between mothers residing in treatment and comparison areas owing to permanent unobserved differences.

3 Formal and informal child care and the 1975 reform

In this section, we aim to describe the market for child care in Norway before the reform, and how we interpret its implementation, by outlining a simple model of formal and informal child care supply.

In the late 60s, and early 70s, the federal government implemented large subsidies for formal child care. However, the subsidies were contingent on a maximum price to be paid by the parents,[6] causing a kinked supply curve for formal care, illustrated in panel (a) of figure 4. The demand and supply of formal care is represented by the curves D and $S.1$. Formal care is provided at marginal cost net of subsidies up to the maximum price, yielding a normal upward-sloping supply curve. Above the maximum price, formal care is provided at marginal cost excluding subsidies, in the figure taken to be above the scale of the y-axis, yielding a vertical supply curve after this point.

[Figure 4 about here.]

As female wages increased and the household value of mothers labor participation grew, demand for child care became stronger. Over time this caused a demand surplus, where formal care was rationed at the maximum price. In panel (a), formal care supplied is b^{s1} at the maximum price. At this price, however, there is a larger demand for formal child care at the intersection of the demand curve and the prevailing price.

This demand surplus was in turn serviced, in part, by a generic informal market for child care, where mothers paid a higher (quality-adjusted) price than in the formal market. This is illustrated in panel (b) of figure 4, where we integrate the formal and informal market for child care. In this figure, formal care is preferred to informal care below the maximum price, such that

[6]The maximum price was about 250 NOK per week for full day care (NOU, 1972). In 2006, this is equivalent to about 1000 NOK, or about 150 USD.

the informal market (curve I) only services the demand surplus. The use of formal and informal care are then b^{s1} and b^{I}, respectively.

Both working mothers using informal arrangements but preferring formal care, and mothers unwilling to use informal care but willing to use formal care at the maximum price, are dissatisfied, and form local political pressure towards expansions of formal care. In panel (b), the political pressure from the first group of mothers using informal care is indicated by the shaded rectangle a, while the pressure from the second group of mothers is indicated by the shaded triangle b.

As female wages increased, demand for child care grew. The political pressure for public policies to expand child care therefore increased across the country. Since the federal government reasonably had imperfect information about local demand, and centrally governed supply therefore would imply a massive rent-seeking opportunity for the municipalities, the response was instead to pass the 1975 Kindergarten Act, in which local municipalities were assigned the responsibility for facilitating child care supply.

The decentralization of child care policies meant that the local municipalities pursued separate child care policies. Child care policies should therefore have been particularly expansive where the political pressure was larger. From panel (b) of figure 4, this implies that policies should be expected to be more expansive in municipalities where many mothers were already working. This positive correlation between expansion of child care and the level of maternal labor force participation, suggests that the causal effect of the former on the latter might be smaller than a direct estimation would imply. In panel (c), we illustrate a municipality that significantly expands its supply of formal child care, to the new level b^{s2}. The actual impact on outsourcing of child care and maternal labor force participation is, however, much smaller, since much of the expansion simply replaces care that was previously provided in the informal market. The supply of informal care is now only b^{I2}, compared to b^{I1} earlier.

4 Empirical strategy

To estimate the effect of subsidized child care on maternal employment, we apply a DD approach. Our empirical strategy is the following: We compare the growth rate in employment from 1976 to 1979 of mothers with children aged 3 to 6 years living in municipalities where child care coverage expanded a lot (that is, the treatment group), with the growth rate for mothers (with children of the same age) who live in municipalities with little or no increase in child care (that is, the comparison group). The motivation for using 1979 as the last year of the comparison is to give the municipalities some time to plan and react to the policy change (the law was passed in June of 1975). In the robustness analysis, we make sure that our results are robust to changes in the exact choice of time intervals as well as the child care coverage threshold defining treatment and comparison areas.

The DD estimator of the child care expansion on maternal employment can be defined as

$$E\big[Y_{1979} - Y_{1976} | Young = 1, Treated = 1\big] - E\big[Y_{1979} - Y_{1976} | Young = 1, Treated = 0\big]$$

where E is the expectations operator (conditional on controls), Y is a dichotomous dependent variable equal to 1 if the mother works (and 0 otherwise), $Young$ is a dummy variable equal to 1 if the youngest child of the mother is between 3 and 6 year old (and 0 otherwise), and $Treated$ is a dummy variable equal to 1 if the mother lives in a treatment area (and 0 if she lives in a comparison area). The identifying assumption is that the growth rate of employment of mothers with 3 to 6 year olds would have been the same in the treatment area as in the comparison area, in the absence of the reform.

The corresponding DD regression, estimated over the sample of mothers with 3 to 6 year olds in treatment and comparison municipalities, can be expressed as

$$Y_{it} = \beta_0 + \beta_1 Treated_i + \beta_2 Post_t + \beta_3 Treated_i Post_t + \mathbf{X}_{it}\zeta' + \epsilon_{it} \qquad (1)$$

where i indexes mother, t indexes year (1976 or 1979), $Post$ is a dummy variable equal to 1 when $t = 1979$ and 0 when $t = 1976$, and X is a vector of controls including dummy variables for the mothers and her spouses age and education, immigrant status, number of children by age, and moving between municipalities within treatment/comparison areas, as well as municipal-specific fixed-effects, and local unemployment rate of prime age males to capture potentially differing labor market environment. The effect of the child care expansion on maternal employment is given by β_3.

A possible limitation of the identification strategy in the approach is that any shocks specific to the treatment areas that coincide with the policy changes will bias our estimates. If, for instance, there are economic fluctuations specific to the treatment group that are not accounted for by local unemployment rates, then the DD estimator will be biased. A similar problem would arise if the treatment municipalities also initiated other policies to stimulate female labor force participation.

We attempt to address this concern by adding mothers with the youngest child just above child care age (7 to 10 years old) as a second comparison group. This gives us a DDD estimator, exploiting that the child care reform creates variation along three dimensions: (a) between mothers with 3 to 6 year olds and 7 to 10 year olds; (b) between time periods before and after the reform; (c) between treatment areas and comparison areas. The additional comparison should pick up time-varying effects specific to the treatment area, and correct for the potential biases mentioned above, since mothers of 7 to 10 year olds are unaffected by the reform.

The DDD estimator of the child care expansion on maternal employment can be defined as

$$\begin{aligned}&\Big\{E\big[Y_{1979}-Y_{1976}|Young=1,Treated=1\big]-E\big[Y_{1979}-Y_{1976}|Young=1,Treated=0\big]\Big\}\\-&\Big\{E\big[Y_{1979}-Y_{1976}|Young=0,Treated=1\big]-E\big[Y_{1979}-Y_{1976}|Young=0,Treated=0\big]\Big\}\end{aligned}$$

where $Young$ again is equal to 1 when the youngest child of the mother is between 3 and 6 years old, and 0 when the the youngest child of the mother is between 7 and 10 years old. The first curly brackets corresponds to the DD estimator above, comparing the growth rate in employment of mothers with the youngest child aged 3 to 6 years who live in treatment areas, with the growth rate for mothers with children of the same age living in comparison areas. The second curly brackets makes the same comparison for mothers with slightly older children. The identifying assumption is that, on average, the difference between the employment rate of mothers with 3 to 6 year olds and mothers with 7 to 10 year olds would have changed similarly in treatment and comparison areas, in the absence of the reform.

The corresponding DDD regression, estimated over the sample of mothers with the youngest child 3 to 10 years old, can be expressed as

$$\begin{aligned}Y_{it}=\;&\gamma_0+\gamma_1 Treated_i+\gamma_2 Young_{it}+\gamma_3 Treated_i Young_{it}\\&+\big[\gamma_4+\gamma_5 Treated_i+\gamma_6 Young_{it}+\gamma_7 Treated_i Young_{it}\big]Post_t\\&+\mathbf{X}_{it}\beta'+\epsilon_{it}\end{aligned}\tag{2}$$

We can note how this regression, like the DD regression, allows for different intercepts by residency (γ_1), as well as by child age (γ_2) and their interaction

(γ_3). In addition, we also take into account changes coinciding with the reform, both in general (γ_4), by residence (γ_5) and by child age (γ_6).

By controlling for changes specific to the treatment area tha coincide with the reform (γ_5), we correct for e.g. unobserved differences in economic fluctuations between the treatment and comparison areas, or any unobserved differences in the policies stimulating maternal labor supply in general. Meanwhile, by allowing for coinciding changes specific for children of kindergarten age (γ_6), we ensure that we are not confounding age specific trend shifts with policy effects.

The effect of child care is now given by γ_7, and is identified from the time change in the employment rate in the treatment area relative to the comparison area, for mothers with 3 to 6 year olds relative to mothers with 7 to 10 year olds. The crucial assumption is therefore that there are no time varying factors (other than child care) that influence the growth in employment rate of mothers of 3 to 6 year olds in treatment municipalities, without having a similar impact on the growth in the employment rate of either mothers of 7 to 10 year olds in the treatment area, or mothers of 3 to 6 year olds in the comparison area. If treatment municipalities implemented policies that included both child care expansion and other measures targeted specifically at stimulating labor participation of only these mothers, then our estimates would be biased upwards. We have found no examples of such policies in the relevant period.

Previous studies of child care and maternal employment applying a DD approach have used repeated cross-sections. A potential problem is if there are any unobserved compositional changes within the treatment group. If e.g. mothers in the treatment group after the reform are of a type that are more inclined to work, then we would expect them to exhibit higher labor force participation regardless of the expansion in child care. Because we have access to panel data, we can restrict our sample to consider the same mothers before and after the reform, removing such biases.

Further, access to panel data allows us to include individual-specific fixed effects to eliminate biases in the comparison between mothers residing in treatment and comparison areas owing to time *invariant* unobserved differences. Section 6 report estimation results applying the DD and DDD regression to repeated cross-sections. In our robustness analysis in section 7, we reestimate the model with panel data, where we let ϵ_{it} in the regression equations above be a composite error term consisting of individual-specific fixed effects and an i.i.d. error term.[7]

5 Data

We have data on all formal child care institutions from 1972 (reported directly from the institutions themselves) and the number of children by age and hours of care. From this data we construct a time series of the number of child care places in each municipality in each year.[8] We also have access to panel data from Norwegian registers, covering the entire population from 1972. Taking the number of children from the registers and the number of child care places from above, we construct a time series of child care coverage rates for all municipalities. The aggregate data for the country as a whole is already presented in figure 2.

As mentioned, we sort the population of mothers into a treatment and a comparison group. We construct the groups by ordering the municipalities according to the percentage point increase in child care coverage rates from 1976 to 1979. We then split the sample in half, letting the upper half constitute the treatment group and the lower half the comparison group.[9] The municipalities with above median expansions in child care, ending up in our main treatment group, on average experience an expansion in child care coverage rates of more than 33 percentage points over the period. The municipalities that are below the median, ending up in our main comparison group, experience an average expansion of less than 6 percentage points in the same period.[10]

Figure 5 draws histograms of the actual child care coverage rates in 1976 and 1979, and indicates that the distributions of formal child care provision

[7] Bertrand et al. (2004) show that the standard errors in DD regressions may be misstated in the presence of serial correlation of outcomes of the same mother over time. Although our fixed effects specification directly account for time-invariant unobserved heterogeneity, the fixed effects do not capture time-varying dependence. However, as their analysis demonstrates, limiting the time dimension to before and after reduces the problem of serial correlation considerably.

[8] The data is reported in October of any given year.

[9] In our robustness analysis, we also separate the sample at the 33rd and 67th percentile, taking the upper third as the treatment group, the lower as the comparison group, and dropping the ones in between.

[10] The median expansion is 29.9 and 6.2 percentage points in the treatment and comparison group respectively.

prior to the reform, were quite similar within the treatment and comparison groups. In the appendix, we also include tables of political and demographic variables, as well as a table of municipal expenditures, taxes and fees, and some indicators of the population density (all per capita). We note that groups are quite similar in most all respects. Finally, the appendix also includes a map of Norway, marking the treatment and comparison municipalities in figure 7. The map shows that the municipalities are reasonably well spread out, covering all parts of the country.

To avoid migration induced by the child care reform, we exclude households that move between treatment and comparison municipalities. Since very few mothers with young children move during this short period of time, this should have negligible effects on our estimates. In our estimations, we control for relocation between municipalities within each group. We also exclude mothers who are currently in education, since it is difficult to assess their labor force status, and a small number of observations with key variables missing.[11]

[Table 1 about here.]

Table 1 presents descriptive statistics for main variables in our treatment and comparison groups. We see that the child care coverage rates increased from just over 15 to over 40 percent in the treatment group, and from about 9 to 16 percent in the comparison group.[12] The large difference in growth rates is important, since it is from this variation that we draw our identification.

Further, in 1976 the mothers with the youngest child 3 to 6 years old (from here on mothers with kindergartners) in the treatment group are almost 12 percent more likely to work, and more than 20 percent more likely to work full time.[13] Similarly, mothers of school children living in treated municipalities, are in 1976 over 11 percent more likely to work, and over 19 percent more likely to work full time. Three years later, the gap has grown some for

[11]We allow missing values for the father's characteristics, creating a separate control for these observations.

[12]The level in the treatment group is inflated by the inclusion of Oslo, Norway's largest city by far. With our fixed effects approaches, and since Oslo is not in the treatment group in other definitions of the treatment yielding similar results, e.g. when we make the cutoff at the 33rd and the 67th percentile, we are confident that this is not driving results.

[13]We define labor force participation as earning a taxable income in the current year larger than two times the base rate of pensions, approximately 19400 USD (2006), and full time participation as earning twice that.

mothers of kindergartners, standing at 12 and 30 percent, while narrowing slightly for mothers of school children, to 8 and 19 percent.

For our DD-estimation, we prefer that the changes in the exogenous variables from the pre-reform period to the post-reform period are not too different between mothers of 3 to 6 year olds in the two groups. Large differences in variables that should have little or no relation with the reform, could indicate that there are unobserved processes taking place in the period. Failure to properly control for these processes could cause bias in our estimates. From table A, we immediately assert that there are no apparent differences in the trends of the controls between the two groups of kindergarten mothers.

Considering again figure 3, we are also assured that the trends in maternal labor force participation prior to the reform (1972–1976) are similar for mothers of kindergartners in the treatment and comparison group.

[Figure 5 about here.]

We may note particularly that the groups are of similar sizes, which, since the number of municipalities is the same, indicates that the largest cities are not all concentrated in either treatment or control. Further, the trends in the local labor market, in parental education and age, and in the number of siblings of different ages is almost exactly the same in the two groups.[14] The slight difference in the change in the population share of immigrants is well within the standard errors. The relocation share does change differently, but constitutes only a small share of the population.[15]

We may still be concerned about biases from unobserved differences between the treatment and comparison groups, and therefore introduce mothers of young school children as another comparison group in the DDD approach. For this specification, we are no longer concerned with the difference in growth rates between treatment and comparison groups directly, but whether any such differences are mirrored in the other demographic group. As observed in table A, this is the case for the relocation share mentioned above, and also holds for the other variables.

[14] Note that the number of children is (by mistake) truncated at 2, such that the average number of children is too low in all groups.

[15] In any case, since the difference is mirrored for mothers of 7 to 10 year olds, the DDD estimation should account for it.

6 Main empirical results

Table A shows the results for the repeated cross section. The reform estimator is the coefficient on $Post * Treated$ in the DD regressions in columns (1)–(3). The regression equation is specified in equation 1. In the final three models in columns (4)–(6) of the table, we apply the DDD approach specified in equation 2. In these models, the reform effect is estimated by the coefficient on $Post * Treated * Young$. The dependent variable is a dummy for labor participation, equal to one if the individual works, and zero otherwise. Models (1) and (4) report estimates without any control for characteristics, models (2) and (5) are estimated with controls, but without municipal specific effects, which are added in models (3) and (6).

[Table 2 about here.]

The results are remarkably consistent across the different specifications. In the DD models, we estimate that the expansion in child care caused an increase in the maternal labor participation of between .00681 and .00918 percentage points. From table A, we find that child care coverage expanded by 17.86 points more in the treatment group than in the comparison group. This implies a marginal impact on maternal labor supply per percentage point increase in the child care coverage rate of between .038 and .055.

These estimates confirm the findings of Cascio (2009), who finds no effect on married mothers of expansions in child care. Our results are also in line with those reported by Lundin, Mörk and Öckert (2008). We may also provide some support for the hypothesis of Rosen (1996), who proposes that the high MLFP rates in Sweden can be explained mainly by their employment in social services in general, and in child care in particular.

To account for potential biases caused by unobserved characteristics of the treatment municipalities, influencing both child care policies and maternal labor force participation, we estimate a DDD model, introducing mothers with the youngest child between 7 and 10 as a comparison group in models (4)–(6). From table A, we note that this slightly increases our estimates of the reform effect to between .0108 and .0116. Notably, the estimates are now even more stable than before, though the additional variables increase the standard errors somewhat. The estimated effect per child care place is still slight, only about .061 in model (6).

This implies that the massive expansion in child care in the late 1970s, had almost no impact on the labor participation decision of the mother.

While statistically significant, the estimated 95 percent upper bound is less than .022 (.125 per additional child care place) in all the models. Taking our simple model above at face value, our estimates therefore indicate that almost all the new child care places are filled by children of mothers who were previously using informal sources of care. Interpreting our results in this way, we estimate that the crowding out of informal care by formal care is almost complete, at about 94 percent (model (3)).

Using the estimate from model (3), we apply our results to the historic trends in child care coverage rates and maternal labor force participation in Norway. This indicates that the massive child care expansion since the early 70s, can explain about 2 of the 55 percentage point rise in maternal labor force participation in Norway from 1972 to 1996 (the upper bound at about 4.5 points). In figure 6, we draw the predicted maternal labor force participation rate indicated by the child care coverage rate from 1972 to 1996 and model (6). The figure demonstrates the almost complete lack of explanatory power from child care to maternal employment. By the same token, cross-country differences in child care are only able to account for a small fraction of the gap in maternal employment between Norway and other western countries.

[Figure 6 about here.]

7 Robustness analysis

7.1 Transitions from part time to full time

While the decision of actually participating therefore seems to be at best marginally impacted by child care expansion, the expansion could have had an impact on the probability of working full time. Since the price of child care per hour reasonably goes down when the mother shifts from informal to formal care, she might very well choose to work longer hours.[16] In table A, we re-estimate the model taking as the dependent variable whether the mother works full time, defined as earning a pensionable income over $4G$.[17] If the

[16]Mothers could generally choose between half day and full day places in child care institutions. Informal sources of care could also still serve as a fallback option for odd numbers of hours.

[17]For completeness, table A in the appendix reestimates the models with our panel data approach.

estimated effects are larger in this estimation, then this would indicate that while the child care expansion did not have an effect on the extensive margin of maternal labor supply, it did have an effect on the intensive margin. The estimated effects are in a tight range close to zero, .0069–.0085 in the DD models and .0054–.0058 in the DDD models, yet statistically significant. For every percentage point increase in child care coverage, full time participation increases by less than .05 percentage points. Our results therefore indicate that the effect of the child care expansion on the intensive margin, though marginally significant, is quite small, and of the same order of magnitude as the effects on the extensive margin discussed in the section 6.

[Table 3 about here.]

7.2 Compositional changes and individual specific effects

As mentioned in section 4 above, when estimating DD models in repeated cross sections, we might be concerned about bias from unobserved changes in the composition of mothers in the treatment group. We address this problem by using the panel dimension in our data, restricting our sample to the same mothers before and after the reform. We therefore include in our sample only mothers whose youngest child was either eligible for child care in both 1976 and 1979 or in neither. This gives a sample of mothers whose youngest child was born in either 1973 or 1969. The former is then 3 years old prior to the reform, and 6 after, and eligible for child care in both periods, while the latter is 7 before and 10 after, and ineligible in both periods.

In table A, we report the results from this sample. In columns (1)–(2) and (4)–(5), we reestimate the DD and the DDD models without any controls and with all controls, including municipality specific effects. The results from the DD models are only marginally different from those reported in table A for the repeated cross section. From our baseline specification in model (2), we estimate an impact of .0094, or .053 per additional child care place, almost exactly what we estimated for the full sample. The estimated effects in the DDD models are less precise and turn slightly negative, but are statistically insignificant and still essentially zero.

We may also be concerned that there is selection of individuals over time invariant unobservables. We therefore re-estimate the model including individual specific fixed effects. This purges the variation of anything that does not change over time, and makes control for other time invariant controls redundant. We therefore exclude these from the estimation. The results are reported in models (3) and (6) in table A, and are very similar to those previously estimated. Both estimates are essentially zero, with the 95 percent upper bound of the effect at about .02.

[Table 4 about here.]

7.3 Alternative treatment definitions

Finally, to ensure that our estimates are not simply an artefact of the specific definition of treatment or time frame considered, we reestimate the model where these are varied. Table A shows the estimates and associated standard errors from separate DDD regressions including all controls and municipality specific fixed effects. In the first row, we keep the time frame unchanged, but use only observations from those municipalities that were above the 67th percentile (treated) or below the 33rd percentile (comparison) in the expansion of child care. In rows 2–4, we cut the data at the 50th percentile, but vary the time frame according to the entry in the first column. The reform effect is precisely estimated and very close to zero in all estimations.[18]

[Table 5 about here.]

8 Concluding remarks

The question of what caused the astounding growth in maternal labor force participation, has spurred much research. The experience of the Scandinavian countries where participation has become especially high, is perhaps particularly important as a potential guide for international policy makers. An important characteristic of Scandinavian countries are the extensive and generous family policies in general, and child care policies in particular. In recent years, both the OECD and the EU have suggested expanded child care policies to tackle the low maternal employment rates in other European

[18] We have also estimated the models with cutoffs at the 25th and 75th percentile. All treatment definitions have also been applied to the reported time frames. None of the results differ significantly from those reported, and are available from the authors upon request.

countries (EU 2002, OECD 2004). The EU Presidency for instance stated in 2002 (p. 13) that:

> Member States should remove disincentives to female labour force participation and strive [...] to provide childcare by 2010 to at least 90% of children between 3 years old and the mandatory school age and at least 33% of children under 3 years of age.

It is therefore of significant importance to determine what impact such policies have had where they have already been implemented.

By exploiting the large variation created after the 1975 Kindergarten Act, we are able to robustly estimate the causal effect of a large child care expansion in an environment resembling other western countries today. With a general specification, and access to data on all child care institutions and the entire population of Norway over a long time period, we may control extensively for unobserved heterogeneity. The estimates show that the more than doubling in child care coverage over three years, from 12 percent in 1976 to 28 percent in 1979, had essentially no impact on mothers' decision to participate in the labor market. Further, we find no evidence of more than a marginal effect on the decision to work full time.

Our interpretation of the negligible impact of the child care expansion, is that when forming local child care policies, municipalities responded to local political pressure. Such pressure was plausibly stronger where many mothers were already working, using more expensive and/or inferior care. The causality therefore runs through political economy, from maternal labor force participation, to local access to formal child care. In this case, we are implicitly estimating that informal arrangements for child care are readily available and relatively inexpensive. Access to child care is therefore not a binding constraint on mothers. However, mothers seem to prefer formal child care, since places are indeed taken up as they are built.

Our results imply that child care policies are relatively impotent in terms of influencing employment decisions of mothers. As a result, policies promoting free and available child care, are mostly facilitating a transfer to households with small children. In this respect, the policies are therefore quite expensive, since they do not seem to stimulate an expansion of the tax base.

While higher availability of affordable and high quality child care does not seem to cause higher maternal employment, it may well have other important effects. Firstly, an important motivation for the heavy focus on formal child care arrangements in Norwegian family policies, was to establish good environments for the mental and physical development of children. On the other hand, many authors have argued that promoting child care has a detrimental effect on children's development, particularly in early years. In a current project, we study the impact that the child care expansion had on children's long term outcomes.

Second, Scandinavian countries are also quite remarkable for the ability to combine high maternal employment with high fertility. Since mothers seem to prefer formal child care, and there is indeed much evidence that this is the case (see e.g. NOU 1972:39 or Guldbrandsen, Lea and Stokke, 1982), good access and low prices may be one explanation for the relatively high rates of fertility. We are therefore also attempting to estimate whether there is any effect on fertility. Other questions we are currently pursuing include the effects on marriage and divorce, on intergenerational mobility and geographical mobility.

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A Appendix

[Figure 7 about here.]

[Table 6 about here.]

[Table 7 about here.]

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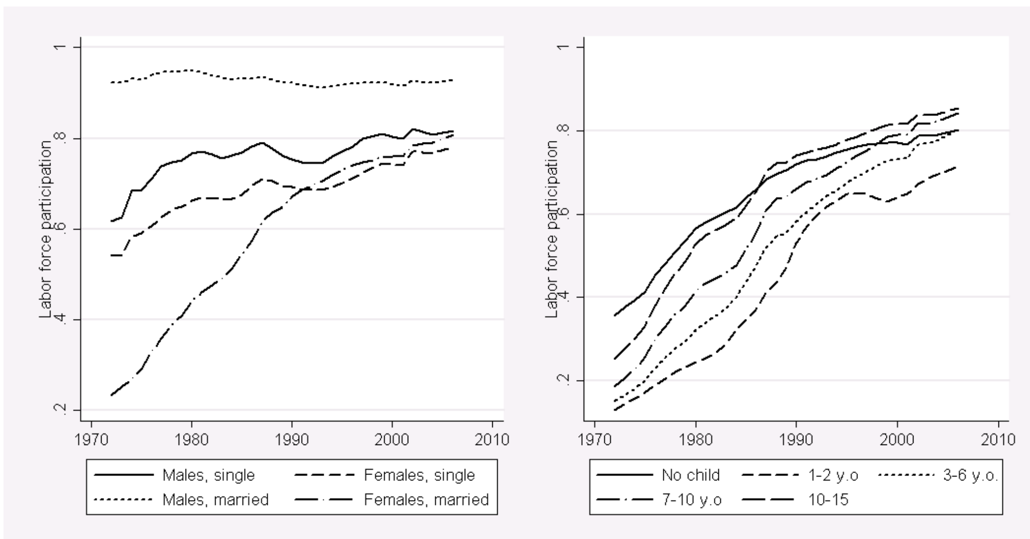


Figure 1: Labor force participation by gender and marital status (left panel), and employment rate of married mothers by age of youngest child (right panel), Norway: 1972–1996. Source: Administrative data.

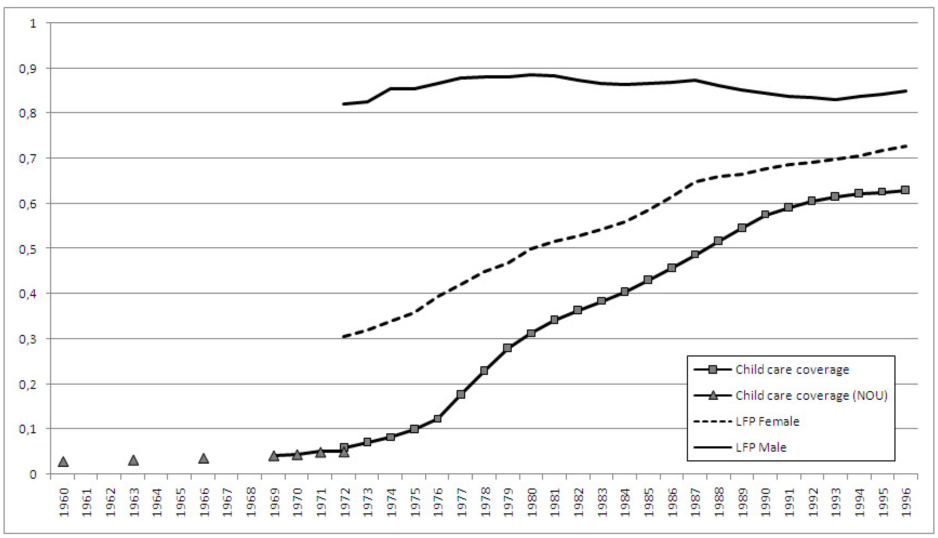


Figure 2: Child care coverage rate for children aged 3 to 6 years 1960–1996 and employment rate of married mothers with the youngest child aged 3 to 6 years 1972–1996. Source: Administrative data for 1972-1996. Data for 1960–1972 from NOU (1972), table II.1.

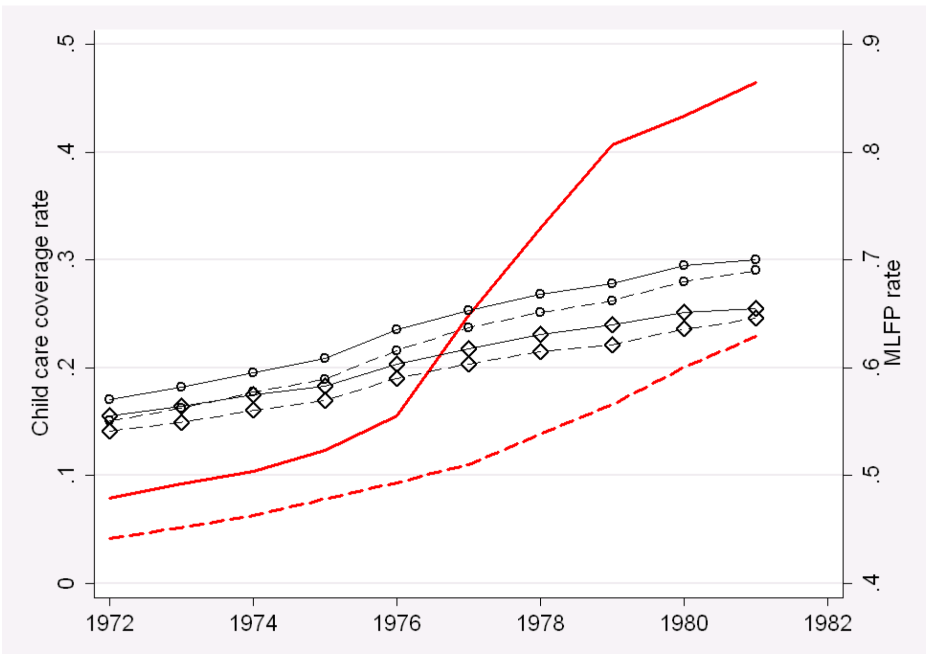
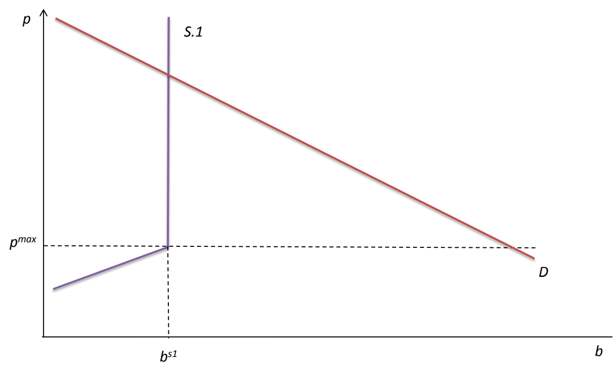
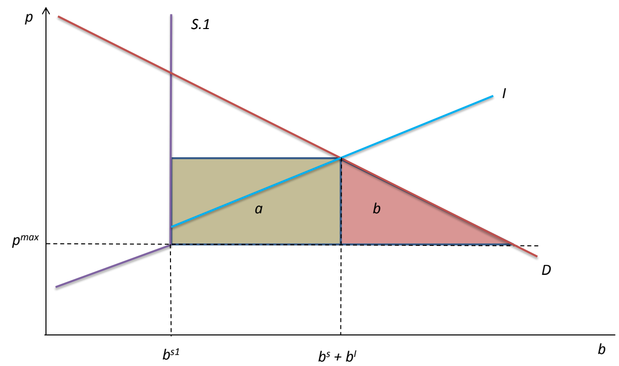


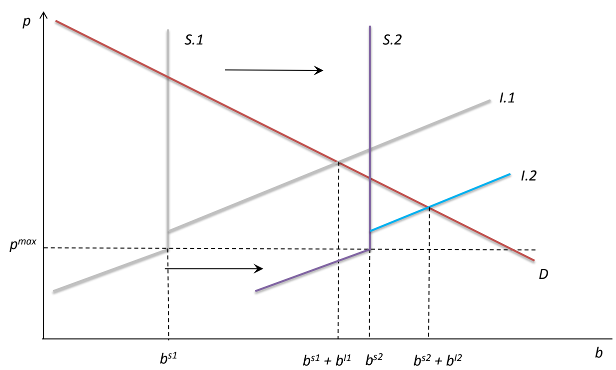
Figure 3: Child care coverage rates for children 3-6 by treatment (solid) and comparison group (dashed) and employment rate of married mothers with the youngest child aged 3–6 y.o (diamonds) vs 7–10 y.o. (circles).



(a) Supply and demand of formal care

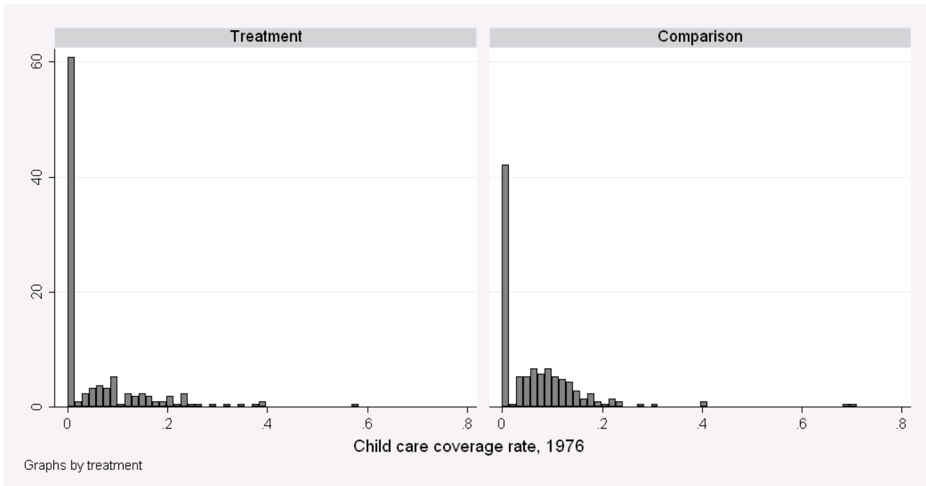


(b) Formal and informal supply of care, and political pressure

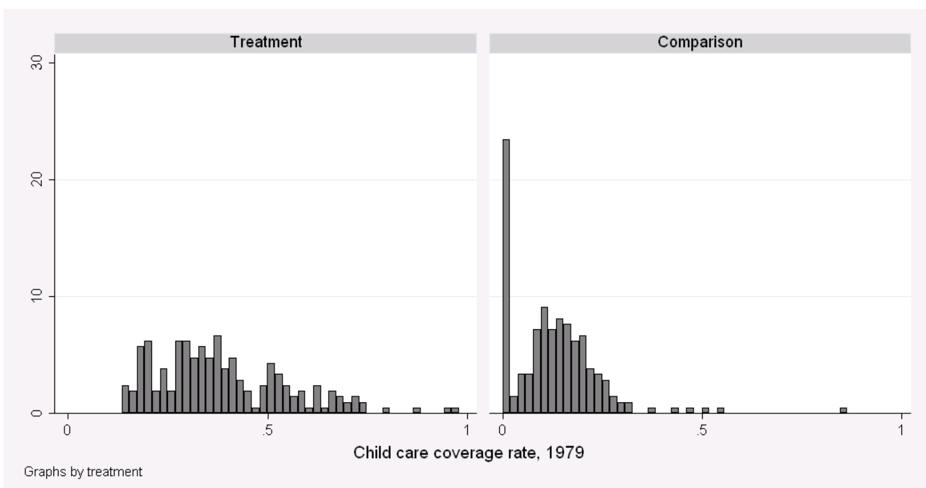


(c) Formal child care expansion crowds out informal care

Figure 4: The market for formal and informal child care, local political pressure (areas a and b), and the municipal specific expansions in the supply of formal child care.



(a) 1976



(b) 1979

Figure 5: Child care coverage rate for 3 to 6 year olds in 1976 (top panel) and 1979 (bottom panel), frequency distribution over municipalities in treatment and comparison group.

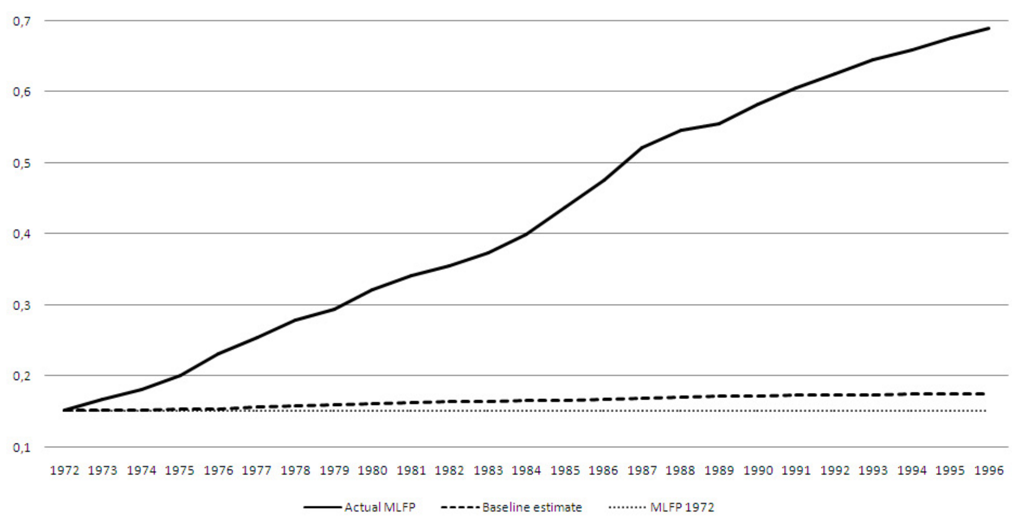


Figure 6: FLP observed (solid) and projected from growth in child care (dotted) given our baseline estimate, for married mothers with youngest child 3 to 6 years old.

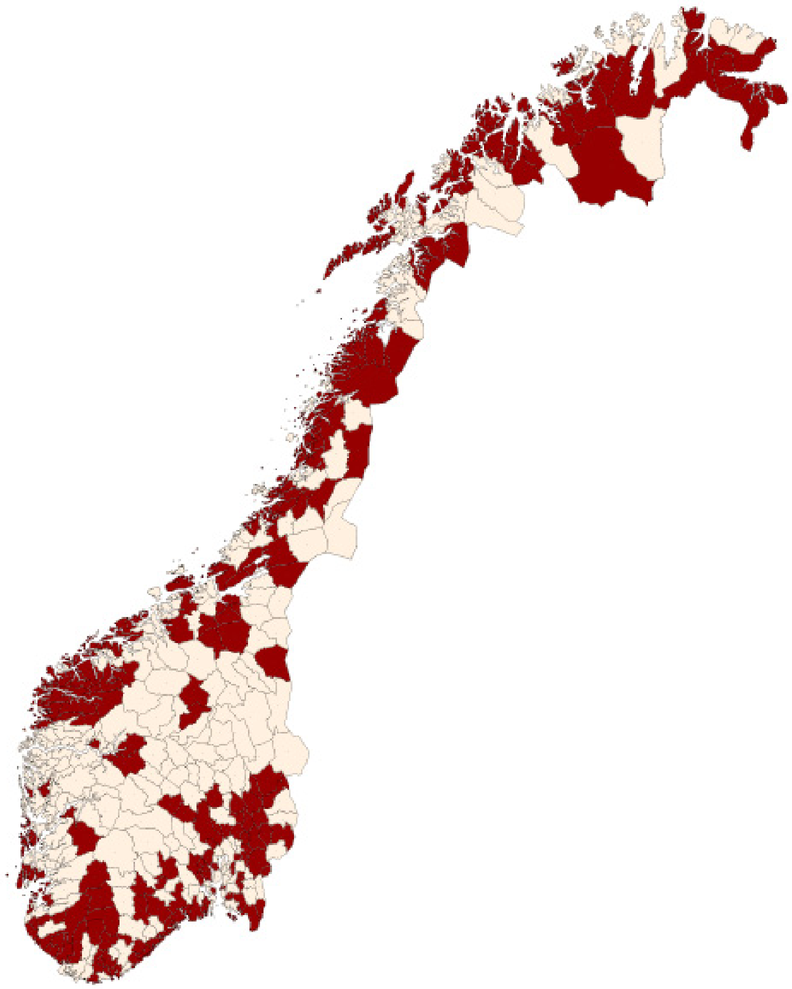


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Table 1: Descriptive statistics for households with youngest child 3 to 6 years old and 7 to 10 years old, before and after the reform.

	3 to 6 year olds				**7 to 10 year olds**			
	Treatment		Comparison		Treatment		Comparison	
	1976	1979	1976	1979	1976	1979	1976	1979
Female labor participation	0.245	0.312	0.219	0.278	0.315	0.394	0.283	0.365
– full time	0.0889	0.104	0.0739	0.0803	0.101	0.118	0.0847	0.0992
Local employment rate	0.869	0.882	0.869	0.882	0.869	0.882	0.869	0.882
Mother's age	32.32	32.17	32.06	31.85	38.49	37.50	38.24	37.29
Mother's education (years)	9.924	10.23	9.642	9.948	9.479	9.777	9.181	9.485
Father's age	35.26	34.90	35.14	34.66	41.88	40.62	41.72	40.52
Father's education (years)	10.69	10.95	10.30	10.57	10.39	10.59	9.965	10.21
Children 3–6	1.214	1.174	1.218	1.175	0	0	0	0
Children 7–10	0.594	0.574	0.611	0.598	1.228	1.222	1.234	1.219
Children 11–15	0.349	0.320	0.385	0.356	0.778	0.783	0.799	0.805
Immigrants (share)	0.0382	0.0386	0.0301	0.0293	0.0367	0.0393	0.0281	0.0298
Relocated (share)	0.0452	0.0460	0.0424	0.0393	0.0203	0.0218	0.0204	0.0191
Child care coverage rate	0.1551	0.4065	0.0932	0.166	.	.	.	.
N	61874	57029	68676	63268	48135	48169	52012	53899

Averages over municipalities are weighted by population size. Female labor participation is defined as pensionable income larger than 2 times G, the pensionable base amount, respectively NOK 11,800 and 15,200 in 1976 and 1979 (full time = 4 times G). Local employment rate is the employment rate of males in the region. Data from Statistics Norway and Norwegian national registers.

Table 2: Estimates from repeated cross-section sample 1976 to 1979, divided at 50th percentile. Dependent variable is female labor participation, defined as pensionable income $> 2G$.

	Panel A: DD models			Panel B: DDD models		
	(1)	(2)	(3)	(4)	(5)	(6)
$Post$	0.0590**	0.0322**	0.0373**	0.0823**	0.0574**	0.0628**
	(0.00241)	(0.00225)	(0.00279)	(0.00278)	(0.00262)	(0.00292)
$Treated$	0.0258**	0.00587**	0.176**	0.0317**	0.0180**	0.188**
	(0.00242)	(0.00223)	(0.0299)	(0.00286)	(0.00268)	(0.0234)
$Post * Treated$	0.00832*	0.00681*	0.00918**	-0.00305	-0.00469	-0.00148
	(0.00349)	(0.00321)	(0.00320)	(0.00403)	(0.00377)	(0.00374)
$Young$				-0.0638**	-0.152**	-0.146**
				(0.00263)	(0.00271)	(0.00269)
$Treated * Young$				-0.00596	-0.0117**	-0.0110**
				(0.00381)	(0.00356)	(0.00353)
$Post * Young$				-0.0233**	-0.0282**	-0.0276**
				(0.00373)	(0.00350)	(0.00347)
$Post * Treated * Young$				0.0114*	0.0116*	0.0108*
				(0.00541)	(0.00506)	(0.00502)
Controls	No	Yes	Yes	No	Yes	Yes
Municipal dummies	No	No	Yes	No	No	Yes
R^2	0.006	0.162	0.177	0.014	0.139	0.156
Dependent mean	0.262	0.262	0.262	0.296	0.296	0.296
N	252704	252704	252704	455575	455575	455575

Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

G, the pensionable base amount, was respectively NOK 11,800 and 15,200 in 1976 and 1979. Controls are local employment rate of prime age males, dummy variables for age, education, immigrant status, husband's age and education, relocation within treatment/comparison group, and dummies for 0, 1, and 2 or more children in the age groups 3–6, 7–10, 11–15. Data from Statistics Norway and Norwegian national registers.

Table 3: Estimates from repeated cross-section sample 1976 to 1979, divided at 50th percentile. Dependent variable is full time female labor participation, defined as pensionable income > 4G.

	Panel A: DD models			Panel B: DDD models		
	(1)	(2)	(3)	(4)	(5)	(6)
$Post$	0.00672**	-0.00850**	-0.00649**	0.0145**	0.00271	0.00479**
	(0.00154)	(0.00143)	(0.00178)	(0.00178)	(0.00165)	(0.00185)
$Treated$	0.0152**	0.00134	0.0432*	0.0162**	0.00554**	0.0303*
	(0.00155)	(0.00142)	(0.0191)	(0.00183)	(0.00169)	(0.0148)
$Post * Treated$	0.00851**	0.00694**	0.00800**	0.00275	0.00143	0.00266
	(0.00224)	(0.00204)	(0.00204)	(0.00258)	(0.00237)	(0.00236)
$Young$				-0.0108**	-0.0537**	-0.0507**
				(0.00168)	(0.00171)	(0.00170)
$Treated * Young$				-0.00108	-0.00491*	-0.00476*
				(0.00243)	(0.00224)	(0.00223)
$Post * Young$				-0.00778**	-0.0125**	-0.0121**
				(0.00239)	(0.00220)	(0.00219)
$Post * Treated * Young$				0.00576+	0.00559+	0.00541+
				(0.00346)	(0.00319)	(0.00317)
Controls	No	Yes	Yes	No	Yes	Yes
Municipal dummies	No	No	Yes	No	No	Yes
R^2	0.002	0.170	0.178	0.002	0.154	0.162
Dependent mean	0.086	0.086	0.086	0.093	0.093	0.093
N	252704	252704	252704	455575	455575	455575

Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

G, the pensionable base amount, was respectively NOK 11,800 and 15,200 in 1976 and 1979. Controls are local employment rate of prime age males, dummy variables for age, education, immigrant status, husband's age and education, relocation within treatment/comparison group, and dummies for 0, 1, and 2 or more children in the age groups 3–6, 7–10, 11–15. Data from Statistics Norway and Norwegian national registers.

Table 4: Estimates from panel sample 1976 to 1979, divided at 50th percentile. Dependent variable is female labor participation, defined as pensionable income $> 2G$.

	Panel A: DD models			**Panel B: DDD models**		
	(1)	(2)	(3)	(4)	(5)	(6)
$Post$	0.119**	0.0833**	0.113**	0.173**	0.163**	0.167**
	(0.00524)	(0.00653)	(0.00688)	(0.00564)	(0.00628)	(0.00585)
$Treated$	0.0249**	0.0994		0.0290**	0.144**	
	(0.00538)	(0.0640)		(0.00580)	(0.0504)	
$Post * Treated$	0.00582	0.00997	0.00939^{+}	0.0128	0.0149^{+}	0.0146**
	(0.00762)	(0.00700)	(0.00508)	(0.00820)	(0.00764)	(0.00544)
$Young$				-0.0568**	-0.128**	
				(0.00554)	(0.00614)	
$Treated * Young$				-0.00409	-0.0107	
				(0.00806)	(0.00752)	
$Post * Young$				-0.0532**	-0.0807**	-0.0589**
				(0.00784)	(0.00783)	(0.00641)
$Post * Treated * Young$				-0.00700	-0.00501	-0.00556
				(0.0114)	(0.0106)	(0.00757)
Controls	No	Yes	Yes	No	Yes	Yes
Municipal dummies	No	Yes	Yes	No	Yes	Yes
Individual FE	No	No	Yes	No	No	Yes
R^2	0.021	0.184	0.104	0.038	0.171	0.128
Dependent mean	0.254	0.254	0.254	0.296	0.296	0.296
N	51392	51392	51392	99282	99282	99282

Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Models are estimated over the sample of mothers with children born in 1973 and 1969. G, the pensionable base amount, was respectively NOK 11,800 and 15,200 in 1976 and 1979. Controls are local employment rate of prime age males, dummy variables for age, education, immigrant status, husband's age and education, relocation within treatment/comparison group, and dummies for 0, 1, and 2 or more children in the age groups 3–6, 7–10, 11–15. Models (3) and (6) are estimated using the `xtreg`-command in Stata 9, and exclude time invariant controls. Data from Statistics Norway and Norwegian national registers.

Table 5: Estimates from separate regressions on repeated cross-sections. Dependent variable is female labor participation, defined as pensionable income > $2G$.

Period	Separation	DDD-estimate	SE	R^2	Dep. mean	N
1976 to 1979	33rd and 67th	0.0174	0.00714	0.159	0.296	237830
1977 to 1980	50th	-0.000979	0.00545	0.150	0.325	452670
1977 to 1979	50th	0.00106	0.00509	0.151	0.310	456385
1976 to 1978	50th	0.00565	0.00497	0.154	0.287	461562

Each line indicates a separate regression. G, the pensionable base amount, was respectively NOK 11,800 and 15,200 in 1976 and 1979. Controls are local employment rate of prime age males, dummy variables for age, education, immigrant status, husband's age and education, relocation within treatment/comparison group, and dummies for 0, 1, and 2 or more children in the age groups 3–6, 7–10, 11–15. Data from Statistics Norway and Norwegian national registers.

Table 6: Demographic descriptives for treatment and comparison municipalities.

	Treatment	Comparison
Females	0.489	0.491
	(0.000857)	(0.000785)
Married	0.466	0.461
	(0.00195)	(0.00239)
Divorced	0.0146	0.0153
	(0.000558)	(0.000557)
Males, 0 to 6	0.0598	0.0627
	(0.000644)	(0.000624)
Females, 0 to 6	0.0557	0.0595
	(0.000697)	(0.000706)
Males, 7 to 10	0.0376	0.0393
	(0.000377)	(0.000361)
Females, 7 to 10	0.0345	0.0365
	(0.000402)	(0.000417)
Males, 11 to 18	0.0723	0.0729
	(0.000539)	(0.000543)
Females, 11 to 18	0.0664	0.0678
	(0.000513)	(0.000520)
Males, 19 to 35	0.127	0.132
	(0.00100)	(0.000880)
Females, 19 to 35	0.109	0.115
	(0.00142)	(0.00129)
Males, 36 to 55	0.119	0.116
	(0.000660)	(0.000647)
Females, 36 to 55	0.109	0.108
	(0.000706)	(0.000650)
Males, 56 to 65	0.0638	0.0602
	(0.000894)	(0.000852)
Females, 56 to 65	0.0613	0.0590
	(0.000757)	(0.000778)
Males, more than 66	0.0748	0.0655
	(0.00156)	(0.00137)
Females, more than 66	0.0820	0.0756
	(0.00139)	(0.00135)

Standard errors in parentheses

Means are per capita and unweighted over the municipalities in the treatment and comparison groups.

Table 7: Political descriptives for treatment and comparison municipalities.

	Treatment	Comparison
Reg. voters	6557.1	5866.5
	(1879.4)	(589.8)
Reg. voters, share female	0.490	0.493
	(0.00117)	(0.00115)
Participation, female	0.708	0.697
	(0.00461)	(0.00435)
Participation, total	0.724	0.710
	(0.00406)	(0.00385)
Female candidates	0.293	0.287
	(0.00316)	(0.00284)
Female representatives	0.152	0.139
	(0.00567)	(0.00434)
Female mayor	0.00980	0.0145
	(0.00692)	(0.00833)
Socialist mayor	0.319	0.367
	(0.0327)	(0.0336)
Socialist vote share	0.392	0.405
	(0.0112)	(0.0113)
Mayor party undefined	0.0588	0.0580
	(0.0165)	(0.0163)

Standard errors in parentheses

Means are unweighted over the municipalities in the group. All means (except registered voters) are in the relevant shares. Sex specific means are per capita of the sex in the municipality. Socialist parties are defined as RV, SV and DNA, while local and apolitical lists without affiliations with major parties are labeled undefined.

Table 8: Financial descriptives for treatment and comparison municipalities.

	Treatment	Comparison
Expenditure primary school	1.592	1.469
	(0.0509)	(0.0441)
Expenditure total	6.267	5.897
	(0.138)	(0.111)
Ear marks primary school	0.586	0.587
	(0.0155)	(0.0170)
Ear marks total	3.720	3.539
	(0.104)	(0.0839)
Fees primary school	0.00574	0.00646
	(0.000664)	(0.000877)
Fees total	0.808	0.658
	(0.0428)	(0.0303)
Taxes	2.418	2.374
	(0.0469)	(0.0430)
Population in densely populated areas	0.401	0.495
	(0.0200)	(0.0208)
Distance to zone center (km)	0.939	0.814
	(0.0558)	(0.0495)
Distance to closest neighboring center (km)	3.989	3.572
	(0.184)	(0.200)

Standard errors in parentheses

Means are per capita and unweighted over the municipalities in the treatment and comparison groups.

Table 9: Estimates from panel sample 1976 to 1979, divided at 50th percentile. Dependent variable is full time female labor participation, defined as pensionable income $> 4G$.

	Panel A: DD models			Panel B: DDD models		
	(1)	(2)	(3)	(4)	(5)	(6)
$Post$	0.0212**	-0.000617	0.0281**	0.0413**	0.0361**	0.0480**
	(0.00332)	(0.00412)	(0.00406)	(0.00360)	(0.00397)	(0.00352)
$Treated$	0.0140**	0.104*		0.0136**	0.0409	
	(0.00341)	(0.0404)		(0.00371)	(0.0319)	
$Post*Treated$	0.00468	0.00681	0.00563+	0.00671	0.00817+	0.00711*
	(0.00482)	(0.00442)	(0.00300)	(0.00524)	(0.00483)	(0.00327)
$Young$				-0.00909*	-0.0439**	
				(0.00354)	(0.00389)	
$Treated*Young$				0.000373	-0.00380	
				(0.00515)	(0.00476)	
$Post*Young$				-0.0202**	-0.0342**	-0.0189**
				(0.00501)	(0.00495)	(0.00386)
$Post*Treated*Young$				-0.00203	-0.00112	-0.00149
				(0.00729)	(0.00671)	(0.00455)
Controls	No	Yes	Yes	No	Yes	Yes
Municipal dummies	No	Yes	Yes	No	Yes	Yes
Individual FE	No	No	Yes	No	No	Yes
R^2	0.003	0.174	0.034	0.006	0.162	0.032
Dependent mean	0.081	0.081	0.081	0.091	0.091	0.091
N	51392	51392	51392	99282	99282	99282

Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Models are estimated over the sample of mothers with children born in 1973 and 1969. G, the pensionable base amount, was respectively NOK 11,800 and 15,200 in 1976 and 1979. Controls are local employment rate of prime age males, dummy variables for age, education, immigrant status, husband's age and education, relocation within treatment/comparison group, and dummies for 0, 1, and 2 or more children in the age groups 3–6, 7–10, 11–15. Models (3) and (6) are estimated using the `xtreg`-command in Stata 9, and exclude time invariant controls. Data from Statistics Norway and Norwegian national registers.