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Enticing Even Higher Female Labor Supply – The Impact of Cheaper Day Care

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Abstract

We ask whether cheaper child care can spur labor supply of mothers in an economy with high female labor supply. We exploit exogenous variation in child care prices induced by a public reform. A triple difference approach is put forward. The results show that reduced child care prices led to a rise in labor supply of mothers by approximately 5 percent. A "back-of-the-envelope" calculation estimates an elasticity of approximately -0.25, which is at the lower end compared to other studies, suggesting that labor supply is less elastic when female employment is high. Since a capacity-increase was introduced at the same time, the positive labor supply effect may be a result of both reduced prices and increased capacity.

JEL classification: J13, J18, J22

Keywords: labor supply, family policy, child care costs, difference-in-differences-in-differences

1. Introduction and background

In an international context, Norway stands out as a country with an ambitious and generous public family policy (OECD 2007). Full-day subsidized child care is one important instrument of this policy. Through subsidized child care, the ambition is to meet two goals: to provide high quality child care at low cost, and to enable parents with small children to reconcile child care with employment-related responsibilities. Moreover, public policies, which harmonise careers and parenthood and grant children security and a positive learning environment, are of primary importance if children are to acquire cognitive abilities and motivation for learning which in turn determine their working prospects.

The commitment to improve child care facilities started in the mid-1970s: from 1975 to 1985, the child care coverage rate increased from approximately 10 per cent to approximately 40 per cent. During the same period the female employment rate increased from 40 per cent to 73 per cent. In an international context, female labor market participation in Norway is among the highest in the world (OECD 2008), with 83 per cent female employment in 2008. Our main concern is: can reduced child care costs in such an environment — with the female labor supply already high — further serve as an effective tool for increasing labor supply among mothers? To answer this question, we use a quasi-experimental approach exploiting the introduction of a public policy reform that generates exogenous variation in the cost of child care. Historically, municipalities in Norway have been free to set their own child care prices, which meant large variations in prices between municipalities. As a consequence of the large price differences, and a stated political ambition to decrease overall child care costs, an important child care reform legislation was introduced in 2003 (Innst. S. nr. 50). The reform had two main objectives: to reduce child care prices and to increase child care coverage. The price reform was carried out stepwise. In April 2004, a cap on the price the municipality could charge parents was set at 2750 Norwegian kroner (NOK) per month for a full-time slot (approximately 460 US dollars).[1] In January 2006, the reform further reduced the cap to 2250 NOK per month for a full-time slot (280 US dollars). According to Statistics Norway income statistics, 2250 NOK represents approximately 7.5 per cent of the mean monthly household income (after tax) in 2006. In An integrated part of the reform stated that local governments must improve child care coverage such that all families who wanted a child care slot could be offered one. The combined result of the reform saw a fall in child care costs and a rise in capacity.[2]

[1] Evaluated at the present exchange rate .In the rest of the paper, when using US dollars, this is the exchange rate we use.

[2] Initially, the reform was set up in an even more ambitious manner. It said the cap should be set to 2500 NOK (420 US dollars) by May 2004, and reduced further to 1500 NOK (250 US dollars) in 2005. Full coverage rate

There is a substantial literature on the importance of child care costs on the female labor supply over the last two decades (see for example, Blau & Robbins 1988; Ribar 1992; Connelly 1992; Connelly & Kimmel 2003; Blau & Tekin 2007; Gelbach 2002; Blau 2003; Baker, Gruber, & Milligan 2008; Lefebvre & Merrigan 2008; Herbst 2010; and Cascio 2009). Despite the large number of studies, considerable uncertainty lingers about the magnitude of the maternal employment effect with respect to the price of child care (Blau 2003). Blau & Robins (1988) estimate childcare price elasticities for married women of –0.38 with respect to labor market participation. Ribar (1992) found some larger price elasticities for labor market participation, also for married women. Connelly (1992) reports labor force participation elasticity for married women of -0.2. Connelly & Kimmel (2002) report larger employment elasticities for single than for married mothers (-1.221 versus -0.709 for full-time employment). Lower elasticities are found in Gelbach (2002), ranging from -0.13 to -0.36. Finally, for single mothers. Herbst (2010) reports elasticities in the lower end of the reported elasticities above (-0.05). In a sample of studies, summarised in Blau (2003), the reported estimated elasticities range from 0.06 to –1.26. As ascertained by Blau (2003), one important reason for the large difference in results is differences in methodology and econometric modeling. One key problem already identified by Blau (2003) is the use of household expenditures in day care when analyzing the importance of child care costs. Even though several studies use selection-corrected models, they base their identification on exclusion restrictions that can be questioned.

Our response to this critique is the use of a quasi-experiment set-up, thus providing potentially exogenous variation in the eligibility to reduced child care costs. Some representative studies using this approach are mentioned below. Baker et al. (2008) analyse the impact of the introduction of universally accessible subsidized child care in Quebec in the late 1990s on the labor supply of women with children. They find a highly significant positive labor supply effect. This is an interesting finding especially since Canada and Norway share some similar institutional features. Another study from Canada is Lefebvre and Merrigan (2008). They exploit the same quasi-experiment and conclude that the policy has had a large impact on employment of women with preschool children.

U.S evidence has recently put much attention on the role of child care subsidies and increasing the capacity of public preschool. Gelbach (2002) and Gascio (2009) report positive effects of expansion of preschool on single mothers' labor supply. Fitzpatrick (2010) exploits more recent U.S. census data, together with

should be met during 2005. Public economic constraints and dispute over the timing of capacity rise versus price decrease hampered the process. In addition, the four parties who agreed on the reform were not part of the government at the time, and the government (which was a minority government) had as its goal to ensure a high capacity before prices decreased to the full amount; even though the capacity increased considerably from 2003 and onwards, it did not quite reach the intended speed.

birthday-based eligibility cut-offs, and estimates the effects of universal free pre-Kindergarten availability for 4-year old children on overall preschool enrolment and maternal labor supply using a regression discontinuity framework. The study reports only small effects. Fitzpatrick (2011) is a study that resembles that of Gelbach (2002), using more recent data. The results show positive effects on labor supply for single mothers with five year old children, but no effects for younger children. The more modest effects compared to Gelbach (2002) are partly explained by changes in female labor supply and childcare policy environment. Even though these studies come from a different institutional environment than the Norwegian, they are interesting since they contrast the Norwegian environment, where these policies already are in place.

Scandinavian studies on the importance of child care subsidies and increased capacity on labor supply are sparse. Lundin et al. (2008) use a difference-in-differences regression-matching estimator to evaluate the effect on female labor supply of a child care price reform introduced in 2002 in Sweden, whereby a cap on child care prices was set according to family type. Their analyses show no effect of the reduced child care prices on labor supply, something they interpreted as suggesting that, in a well-developed and highly subsidized child care system, further reductions seem to have a negligible impact. Havnes and Mogstad (2011) investigate the impact on maternal employment of a large expansion in child care coverage in Norway in the 1970s, focusing on mothers of children aged 3 to 6. Using a difference-in-differences approach, they find no effect of the increased capacity on maternal employment and suggest that the new subsidized child care may have crowded out informal child care arrangements. Black et al. (2012) analyse the effects of childcare subsidies on long run child outcomes and maternal employment using a sharp discontinuity in the price of childcare in Norway. They find small and statistically non-significant effects of childcare subsidies on the employment of mothers of four year old children. Blix and Gulbrandsen (1993), based on survey data from the early 1990s, conclude that it is not price which is important but the availability of slots. Simonsen (2013) uses local variation in child care prices between municipalities in Denmark to analyze the degree to which price and availability of publicly subsidized child care affect female employment following maternity leave. The estimates show a price elasticity of -0.17; an effect which prevails during the first 12 months after childbirth. The findings also indicate that availability of day care seems to affect employment.

We contribute to the literature in several ways. First, we provide further evidence on the relationship between child care costs and labor supply by exploiting a quasi-experiment generated from a public family and we use high-quality data with consistent information across different data sets expected to increase the reliability of results. Second, Norway ranks at the top among OECD countries with family-friendly public policies (OECD

2008) and is also among the OECD countries with highest female employment. Hence, these two factors further knowledge as to whether reduced child care costs in such an environment can effectively spur female labor supply.

Our identification strategy is to use triple differences-in-difference-in-difference (DDD), suitable when the reform is equally applicable to all citizens and takes place nationwide. Our analyses support the hypothesis that reduced prices of child care slots have a positive impact on the labor supply of mothers, as measured by their employment. The size of the effect ranges from 3 to 4 percentage points or approximately 5 per cent. We find only small effects on hours worked among working mothers, which is in line with results suggesting that stimulating economic incentives through reduced child care prices has more effect on the extensive than on the intensive margin (see e.g., Berger & Black 1992).

The paper proceeds as follows: the next section describes the institutional setting in Norway, and the child care price reform in particular. Section 3 presents the data, the sample, and the variables utilized. Section 4 presents the methods and the identification strategy. Section 5 presents the results, and section 6 concludes the paper.

2. The institutional setting and the reform

2.1. The institutional setting

Norway has a tradition of having generous family policy arrangements. Long paid parental leave and subsidized child care facilities are two important examples of such generosity. Parental leave arrangements have undergone several reforms in the last two decades. As of July 2010, all working parents in Norway were entitled to 46 weeks with 100 per cent compensation (or 56 weeks with 80 per cent wage compensation), in addition to 5 weeks paid vacation, so that the actual parental leave is a whole year with full wage compensation. To be entitled to parental leave, the mother has to have worked at least six of the last ten months. Such a system aims to give young women incentives to start their labor careers before giving birth, provides a stronger attachment to the labor market, and eases re-entry into the labor market after maternity leave.[3]

To increase the involvement of fathers in the household, an amendment was passed in 1993 which entitled fathers to four weeks' parental leave. The so-called 'daddy quota' was prolonged several times, and as of

[3] There is an on-going debate in Norway on whether the generous and long parental leave (mostly taken up by women) may hamper the career development of women. The stubborn gender wage gap and the very low share of women in top positions in the labor market are by some taken as indications of the "boomerang-effect" from the generous family policies. However, no conclusive research evidence on this mechanism has been reached (see Datta Gupta et al., 2008 for a general discussion of the potential "boomerang" effect in a Nordic setting).

July 1st ,2013 it will be 14 weeks. It cannot be transferred to the mother, which means that paternal leave is lost if the father does not use it. The initial reform, of 1993, has been evaluated with respect to the labor supply effects of mothers and fathers (Cools et al. 2011). Only small effects are reported.

Finally, one large and relevant family policy was introduced in 1998; the so-called ‘Cash-for-Care’ reform (CFC). It was introduced in January 1999, and it entitled all parents with one- and two-year old children, who did not use publicly subsidized day care, to a non-means tested monthly benefit. The CFC reform increased the price of publicly subsidized day care relative to the price of own care. Results in Schøne (2004a) show that the CFC reform has had negative labor supply effects for mothers in the short run. However, Schøne (2004b) finds no negative labor supply effects beyond the eligibility period. Later, we return to the CFC-reform and its importance for our identification strategy.

2.2. The reform

The “Child Care Centre Agreement” (Barnehageforliket) was reached in the spring of 2003 by broad political consensus (Innst. S.nr. 50200-2003) with the aim that neither private economic conditions nor lack of day care slots should prevent families from using formal child care if desired. The reform consisted of two parts: reduced costs and increased capacity. The first cap of 2750 NOK (approximately 460 US dollars) per month for a full-time slot was set in April 2004 and was further adjusted to 2250 NOK (380 US dollars) per month for a full-time slot in January 2006. Reduced child care prices were available to all parents with children of eligible age, irrespective of whether the parents participated in the labor market or not.[4] Later we show that it was the reform of 2006 that resulted in the largest reduction in child day care prices, and consequently this is the reform we exploit in this paper. The cap of 2006 resulted in a large and almost uniform nationwide reduction in child day care prices.

Parallel to these reductions in day care prices, a large increase in the capacity of day care slots took place. This was due to the second part of the reform, which required all municipalities to offer day care slots to all parents with children in the age range of 1–5 years.

As a consequence, the share of children attending publicly subsidized day care centers has increased steadily since 2001. Even though the coverage rate has not increased as rapidly as initially intended (the proclaimed goal was full coverage by 2005), when the cap of 2250 was set in 2006, the coverage rate has been increasing from an

[4] Child day care centres in Norway can be either publicly or privately owned; as long as they are publicly approved, they are subsidized. Roughly 50 per cent of the market consists of private day care centres, the costs of which are shared by the state, the municipality, and the parents.

already high level, particularly for the age group 3–5 years old.[5] Since the increase in capacity took place at the same time as the decrease in prices it is in principle difficult to disentangle between the two effects. In the empirical analyses we estimate regressions where we include local child care coverage rates as control variables.[6]

3. Data and sample

The data set comprises several registers collected by Statistics Norway. The starting point is a public demographic register with information on all births in Norway, linked to information on the mother and the father. Information regarding the a child's mother comprises spells of employment, non-labor income, work experience, education, place of residence, presence of older children in the family, marital status[7], and age. If the mother is married, we have information on the husband's income, age, and educational attainment.[8] We also include information on child day care coverage rates for both 1–2 year-old and 3–5 year-old children at the municipality level.

We measure education using four dummy variables: compulsory school; secondary school; university/college degree, low level; and university/college degree, high level. Non-labor income is measured by capital income. Work experience is measured by the number of years with income above the minimum social security level. Place of residence is measured by a dummy variable that takes the value of 1 if the mother lives in Oslo (the capital) and 0 if otherwise. The presence of older children is measured by two variables: number of children below age 6 and number of children between age 6 and 11.

The dependent variable we wish to make inference about is labor supply, and is measured in two ways: as a binary measure of employment and a continuous measure of working days. *Employment,* a dummy variable, measures whether the mother was registered as an employee during the period of observation.[9] *Working days,* a

[5] Coverage rate is the relative share of children in a given age group with a slot in a day care center, and this is the standard measure of child care coverage. One weakness with the measure is that it says little about the number of children who actually demand and are offered a slot.

[6] Norway and the US are quite different when it comes to the distribution between formal and informal care. In Norway, the vast majority of child care is formal care. In the US much of day care is informal family dare. Therefore, limiting the study to formal care would lead to a much larger underrepresentation of the supply of child care slots in the US than in Norway.

[7] In the data at hand, we can only distinguish between married and not married mothers. We do not have information on cohabitation. This implies that cohabitating mothers will be included among the non-married.

[8] Non-married mothers, for which we do not have information about the husband, are given the value zero on the husband characteristics. This is the usual approach in these types of analyses. This strategy is also used for single mothers. Approximately 12 per cent of mothers in Norway are single mothers at time of birth.

[9] This means that we exclude self-employed. The share of women in Norway that are self-employed is very low, approximately 3 per cent (Rønsen 2012).

continuous variable, measures the number of full-time equivalent working days during the same observation period. Both are based on information from public administrative registers, taken from The Norwegian Labor and Welfare Service (NAV) and organized by Statistics Norway.[10] By *full-time equivalent* we mean that we weight the number of working days with information on working time.[11] Both variables are taken from the Register for Employers and Employees, administered by the National Insurance Administration.

Our four cohorts of mothers include those with children born in 1995, 1999, 2001, and 2005. The reason will become clear in the next section, where we describe the methodology. We restrict the analyses to mothers of 20 to 45 years of age the year they bore a child. Furthermore we restricted the sample to mothers who gave birth to their *youngest* child in these years. For all cohorts of mothers, we have panel information for the whole period of observation. We take advantage of this in the analyses by requiring that, to be included in the analyses, all mothers must be present in both the pre- and post-year periods. In the analyses of working days, we additionally require that all mothers included must be present with a positive number of working days in both the pre- and post-year periods. By utilizing the repeated observation structure of the panel data, we reduce problems related to composition effects, potentially present in repeated cross-sectional samples.

Furthermore, Statistics Norway has conducted a survey among 109 of the 435 municipalities. Even if we do not utilize this data in the empirical analyses they can give insight into the price reduction coming from the reform. A representative sample of Norwegian municipalities is selected such that both large and small municipalities and both urban and rural areas are included. The capital Oslo is included. The survey offers information on the development of the price of full-time day care slots in the period 2003–2006.

[FIGURE 1 ABOUT HERE]

Figure 1 shows that the change from one year to the next in the average monthly price for a full-time day care slot was considerably greater from 2005 to 2006 than in the previous years. This is the case for three income brackets: 250.000, 375.000, and 500.000. Between 2005 and 2006, the average price reduction ranged between 400 and 500 NOK per month (70 and 80 US dollars). Measured relative to the average price in 2005, this amounts to a reduction of approximately 20 per cent.

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[10] According to the rules, all leaves expected to last more than two weeks shall be reported to the register. Periods on paternity leave will therefore be reported to this register. While on leave, these individuals are reported as not working (zero working days). In the analyses of working days, mothers with zero hours of work are dropped.

[11] There are three categories of working time: full-time (30 hours or more per week), long short time (15–30 hours per week), and short part-time (fewer than 15 hours per week). If a worker works full time, she gets weight 1, if she works long part-time she gets weight 2/3, and if she works short part-time she gets weight 1/3.

4. Methodological approach

4.1. DDD approach

The empirical analysis aims to measure the effect of the maximum price (MP)-reform of 2006. Since the cap on child day care prices was made accessible nationwide to all parents with children of the same age, we do not have a natural comparison group. Our strategy is to utilize the quasi experimental nature of the reform. We start by comparing the change in labor supply from the period 2003-2004 to the period 2006-2007, for mothers who gave birth in 2005 (MP-eligible mothers). We choose to use a two-year prior window and a two-year post window. We compare this change with the change in labor supply from the two-year window of 1999 and 2000 to the two-year window of 2002 and 2003 for mothers who gave birth in 2001 (and are hence not eligible for MP). This is a version of the standard difference-in-differences (DD) approach.

However, if some contemporaneous macroeconomic shocks occurred during the period of the introduction of the MP reform, independent of the introduction of the MP reform, the DD estimate will yield biased estimates for the effects of the MP reform on labor supply. To deal with this problem, we compare the change in labor supply for the mothers presented above with the change in labor supply for the same two periods for mothers with *older* children, who are not eligible for MP guarantee. This latter group consists of mothers giving birth in 1999 and 1995.

This approach takes into account that the MP reform from 2006, as we evaluate it, creates variation along three dimensions: between mothers with children of different ages, between pre- and post-periods, and between periods with MP guarantee and periods without MP guarantee. The identification assumption of this DDD-estimator is that there is no contemporaneous shock that affects the relative outcome of the treatment group (mothers with young children relative to mothers with older children) in the same treatment period as the introduction of the MP reform. The DDD approach can be illustrated as follows:

$$\begin{aligned}\mathrm{DDD} = \ & \{\underbrace{(\mathrm{Y}^{\mathrm{T}}{}_{2007\_2006} - \mathrm{Y}^{\mathrm{T}}{}_{2004\_2003})^{\text{gave birth in 2005}} - (\mathrm{Y}^{\mathrm{T}}{}_{2003\_2002} - \mathrm{Y}^{\mathrm{T}}{}_{2000\_1999})^{\text{gave birth in 2001}}\}}_{\mathrm{DD}} \quad (1) \\ & - \{\underbrace{(\mathrm{Y}^{\mathrm{C}}{}_{2007\_2006} - \mathrm{Y}^{\mathrm{C}}{}_{2004\_2003})^{\text{gave birth in 1999}} - (\mathrm{Y}^{\mathrm{C}}{}_{2003\_2002} - \mathrm{Y}^{\mathrm{C}}{}_{2000\_1999})^{\text{gave birth in1995}}\}}_{\mathrm{DD}}\end{aligned}$$

The first bracket shows DD-estimates for mothers with young children, called the *treatment group*. First, $(Y^{T}{}_{2007\_2006} - Y^{T}{}_{2004\_2003})^{\textit{gave birth in } 2005}$ measures the change in labor supply of MP-eligible mothers with young children from 2003-2004 to 2006-2007. Similarly, $(Y^{T}{}_{2003\_2002} - Y^{T}{}_{2000\_1999})^{\textit{gave birth in } 2001}$ measures the change in labor supply of mothers *not* eligible for MP, having had young children in 2001. The difference between these two components is the DD-estimate.

To control for calendar effects, we run the same exercise in the same period for mothers with older children who have just started primary school. The second bracket presents DD-estimates for mothers with older children, called the *control group*. First, $(Y^{C}_{2007\_2006} - Y^{C}_{2004\_2003})^{\text{gave birth in 1999}}$ measures the change in labor supply from the period 2003-2004 to 2006-2007 of mothers with older children from 1999. Finally, $(Y^{C}_{2003\_2002} - Y^{C}_{2000\_1999})^{\text{gave birth in 1995}}$ measures the change in labor supply of mothers with older children from the pre period 1999–2000 to 2002–2003. The difference between these two components gives us the second DD-estimate, while the difference between the two DD-estimates provides the DDD-estimate. If the MP reform has increased labor supply, then the DDD-estimate in equation (1) should be positive.

Needless to say, the world is not a laboratory; it is difficult to find a completely clean experimental environment. One potential problem is that the DDD-set up in equation (1) includes year 2004 as one of the pre-periods, while mothers with MP-eligible children in this year will be treated by the first round of the reform. Secondly, as mentioned earlier, the introduction of the CFC-reform in 1999 had negative short-term effects on mothers' labor supply. This could potentially affect the control group asymmetrically, since 1995-mothers are not affected by the CFC-reform, but the 1999-mothers are. However, as shown in Schøne (2004b), the reform had only short term negative effects, i.e. there was an effect only until the child reached the age of three. As seen from equation (1), the first pre-period year for the 1999-mothers is 2003, when the child is already four years old. Hence the CFC-reform is unlikely to constitute a threat to our identification strategy.

An important assumption is that the control group adjusts for macroeconomic shocks, i.e., we assume that eventual macroeconomic shocks affects treatment and control equally. This is not a trivial assumption. However, we present figures on pre-treatment trends in labor supply, we carry out composition tests, and we include a wide set of control variables in the regression analyses, which together can give an indication of how appropriate it is. We return to this in the empirical section.

Treatment and control groups may differ systematically with respect to important labor supply determinants such as education, age, place of residence, the presence of other children in the household and marital status. Observed differences in outcomes may, therefore, reflect differences between the treatment and control group rather than a treatment effect. To deal with this problem, we run a regression-adjusted DDD:

$$
\begin{aligned}
Y_{ijkt} \quad &= \alpha_1 + \alpha_2 Z_{ijkt} + \alpha_3 MP_{ijk} + \alpha_4 POST_{itk} + \alpha_5 TREAT_{ik} \\
&+ \alpha_6 (MP_{ijk} xPOST_{itk}) \\
&+ \alpha_7 (MP_{ijk} xTREAT_{ik}) \\
&+ \alpha_8 (POST_{itk} xTREAT_{ik}) \\
&+ \alpha_9 (MP_{ijk} xTREAT_{ik} xPOST_{itk}) + \varepsilon_{ijkt}
\end{aligned} \tag{2}
$$

Where i indexes individuals, t indexes time (1 = after, and 0 = before), k indexes group of mothers (1 if mother of young children; 0 if mother of older children), and j indexes MP-status (1 if the period is 2003–2007; 0 if the period is 1999–2003). Y is the dependent variable, measuring labor supply, either measured as a binary variable (employed or not), or a continuous measure (number of working days).

Z is a vector of variables affecting labor supply, containing individual as well as regional variables. Variables in the Z-vector for mother include work experience, education (five dummy variables), place of residence (Oslo), presence of older children in the family (number of children younger than 6, number of children 6-11 years), marital status (married, divorced), and non-labor income (capital income). Variables for the husband include age and education (five dummy variables). For the municipality, the vector includes child care coverage rates and unemployment rates. The variables are explained in more detail in section 3.

It is important to notice that the variable for day care coverage is municipality specific and categorical: day care coverage for children 1–2 years old and day care coverage for 3–5-year-old children. MP is a dummy variable that assumes the value 1 if the period is the maximum-price period (2003-2007), and 0 if it is not the maximum-price period (1999–2003). POST is a dummy variable that takes the value 1 if the years are 2006–2007 (for the MP-group) or 2002–2003 (for the non-MP-group), and 0 if the year is 2003–2004 (for the MP-group) or 1999–2000 (for the non-MP group). TREAT is a dummy variable that assumes the value 1 if the mother has small children (born in 2005 and 2001) and 0 if the mother has older children (born in 1999 and 1995).

The *DDD* estimator (α_9) measures the impact of the interaction term between MP, *POST*, and *TREAT*. This coefficient measures all variation in for the MP group relative to the non-MP group, for mothers with young children (treatment group) relative to mothers with older children (control group) between the before and after period. When testing for the presence of second-order interactions, it is important to include first-order interactions as well. If this is not done, the second-order interaction effect will be confounded with the omitted first-order interactions, and this will most likely lead to biased estimates. To reach unbiased estimates, we must have $E[\varepsilon_{ijkt} \mid MP\text{xTREATxPOST}] = 0$. This means that there is no correlation between the error term measuring unobservable individual-transitory shocks and the variables measuring the effect of the MP reform. In other words, it means that the error term is assumed to be independent of the explanatory variables indicators measuring the effects of the MP reform.

In general, two important assumptions must be fulfilled when using DD and DDD (Blundell & MaCurdy 1999). The first is the assumption that time effects are common across treated and controls. The second assumption is that the composition of both treated and controls must remain stable before and after the policy change (Blundell & MaCurdy 1999). In the result section, we present results from some simple exercises to shed light on these issues.

Table 1 presents descriptive statistics for the two groups: the *treatment group*, consisting of mothers with young children, and the *control group*, consisting of mothers with older children. For both groups, we take the mean values from the "pre" years.

[TABLE 1 ABOUT HERE]

The length of work experience is naturally longer in the control group, since parents of older children are on average older themselves, but there is little variation within each group. The fraction having less than a university or college degree is somewhat lower for "newer" mothers, i.e., for mothers of 2005 in the MP period and for mothers from 1999 in the non-MP period. The share that is married is lower among mothers in the treatment group. Again, this is, of course, mainly due to them being younger.

5. Results

5.1. Descriptive DDD-results

Table 2 presents DDD-estimates of the effects of the MP guarantee on labor supply, based on the set-up presented in equation (1). Labor supply is measured by employment rates (top half) and working days (bottom half). Each cell contains the mean level for the group specified, along with standard errors. The upper half shows DDD estimate, equal to 4.3 percentage points (5.7–1.4), and is statistically significant, suggesting that the MP reform has increased the labor supply by 4.3 percentage points. Compared to the mean level of the employment rate in the pre period for the treatment group, this represents an increase of approximately 5 per cent. Based on the mean percentage increase in labor supply and the mean price reduction (approximately 20 per cent), we calculate a "back-of-the-envelope" labor supply elasticity equal to approximately -0.25.[12]

[TABLE 2 ABOUT HERE]

As mentioned earlier, there is a vast supply of empirical literature focusing on the impact of child care prices on the labor supply of mothers. Almost all analyses report a significant negative effect of child care costs on women's employment. However, the estimated child care price elasticity of employment ranges from about -.2 to -0.9 (Connelly and Kimmel 2003). Compared to these estimates our elasticity is in the lower range. One possible explanation is that the impact of reduced child care prices varies with the level of labor supply such that for higher levels labor supply is less responsive. This could well be the case for Norway, which has a very high female employment rate, meaning that a large number of responsive women are already participating in the labor market.

The child care coverage rate increased considerably during the period under study. The number of individuals working in day care centers also increased considerably, from approximately 52000 in 1999 to approximately 76000 in 2007. One might suspect that part of the increased labor supply reported in Table 2 is a labor-demand effect arising from increased demand for employees in the child care industry as a consequence of the increased coverage rate. We approach this issue by repeating the exercise in Table 2 but excluding all employees in the day care industry (based on information on five-digit NACE codes). The results show that estimates are almost unaltered (results available upon request). Hence, in the rest of the paper we proceed with the full sample of individuals.

The lower half of Table 2 presents estimates for the continuous measure, i.e. *working days*. We confine the exercise to workers registered with a positive amount of working days in both pre- and post-period. Results show a small positive effect of MP on labor supply for working mothers (statistically significant at 10 per cent level). The DDD estimate equals approximately eight days. Measured relative to the mean number of working days for the treatment group in the MP-period, this corresponds to a 1 per cent reduction. Together, the results in Table 2 suggest that the MP reform has had a positive effect on the employment decision, but it has not affected

12 The mean price reduction is calculated for the household income group in the middle (income 375), see Figure 1. The formula for the "back-of-the-envelope" labor supply elasticity is:

$$Elasticity = \frac{(Mean\ persentage increase\ in\ labour\ \sup ply)}{Mean\ percentage\ price\ reduction\ in\ childcare\ prices\ between\ 2005\ and\ 2006)}$$

Where the nominator is 5 per cent and the denominator is 20 per cent.

the labor supply for mothers already working. These results are in line with previous studies reporting higher labor supply elasticities on the extensive margin than on the intensive margin (see e.g., Berger & Black 1992).

As mentioned earlier, the cleanness of the experiment is jeopardized by the fact that including 2004 as a pre-period involves including a group potentially affected by the first stage of the reform. Even though the 2004 part of the reform had a minor impact on prices (see Figure 1, which shows that the 2006 reform matters most), we check for the severity of this matter by carrying out a DDD-analysis separately for mothers giving birth to their firstborn child. When doing this, we leave out the mothers affected by the 2004 part of the reform. We have run the same exercise as in Table 2 with this set-up. The results do not change in any significant way (results available upon request). Therefore, in the rest of the paper we proceed with the specifications in equation (1) and the corresponding regression equation (2), to which we now turn.

5.2. Regression-adjusted DDD-results

To obtain the regression adjusted DDD estimates we use a linear probability model.). Estimate using probit maximum likelihood procedure produced qualitatively equal results. Hence, we report results from the linear probability model only.

The results for the binary measure are presented in the upper half, while the results for duration measure in the lower half. The first three columns of Table 3 present estimates of the model with core explanatory variables only. The DDD-coefficient shows a positive and significant effect of the MP reform equal to 0.042. This is almost identical to the DDD estimate in Table 2. The last column shows that adding the full battery of controls leaves the DDD estimate almost unaltered. This is reassuring, considering the quasi-experiment approach utilized. The message from the models using the binary measure is that the MP reform has increased employment by approximately 4 percentage points, or approximately 5 per cent.[13] In the full model, we also include child care coverage rates. One could well argue that variables related to the child care coverage are endogenous and therefore should not be included as explanatory variables (see e.g., the discussion in Angrist and Pischke 2009 on the issues of "bad controls"). To look into the potential severity of this, we ran a regression with the full set of covariates, but without the child coverage rates. This did not change the DDD-coefficient.

[13] We have also carried out analyses using a somewhat stricter definition of labor supply. As an alternative approach, we defined employed as: 1) being registered in employment as above and 2) having an income above the minimum threshold in the Social Security System. For instance, in 2006 this sum was equal to 62892 NOK (approximately 10500 US dollars). The use of this alternative measure of labor supply did not alter the results in any significant way. Therefore, we proceed with the original measure.

Therefore, we proceed including the coverage rates as explanatory variables. In the full model we control region by including a dummy variable for whether the women lives in Oslo or not.

[TABLE 3 ABOUT HERE]

The lower half of the table shows the results for the continuous labor supply variable. We find a positive but small and insignificant DDD effect, both before and after the introduction of control variables. This result is in line with our previous finding for this variable, and confirms that the MP reform has not affected the labor supply for mothers already participating in the labor market.

The results for the control variables (not presented) do not reveal any surprises. Labor supply increases with work experience and level of education. It is lower for married and divorced mothers compared to mothers who are not married or divorced. Labor supply decreases with the number of older children. Regarding the child day care coverage rate (not shown), we find a positive correlation between the coverage rate for 1–2 year olds and the employment likelihood. The coverage rate for 3–5 year olds, on the other hand, is not significantly related to the employment rate.

As mentioned earlier, due to the closeness of the 2004 and 2006 reform it is difficult to find a 100 per cent "clean" pre-period. To check for the severity of this, we ran the regression in Table 3 separately for mothers who gave birth to their firstborn. This leaves out mothers potentially affected by the 2004 reform. We do this to check whether mothers that were eligible for the 2004 reform affect the results. This exercise shows that the results do not change in any significant way. We still find a positive, sizeable, and highly significant effect of MP reform on labor supply (results available upon request)

Analyses for subgroups

In this section, we present DDD-regression results for different subgroups, defined by household size, education, household income, and ethnic origin.

[TABLE 4 ABOUT HERE]

Regarding household size, we distinguish between three groups: i) those with no other children of day care age, ii) those with one additional child of day care age, and iii) those with two or more additional children of day care age. The more children who are eligible for day care, the lower are the cost of day care per child. Hence, we would expect that the effect of the reform should increase with the number of eligible children under the MP reform, which is what we find. The effect of the MP reform is higher for those households with two or more children in the age range eligible for the MP guarantee as compared to households with one child at the most. Moreover, the effect is smallest for households with no additional child in the eligible age group. This result is also in accordance with our hypothesis.

Regarding education, we distinguish between high and low. *Highly educated* individuals are defined as those with a university or college degree. *Low educated* individuals comprise those with completed secondary school at the most. The educational attainment is measured in the different pre-years (see equation 1). The results show that the MP reform has had a stronger impact on the labor supply of low educated mothers than on highly educated mothers. Since expenditure on day care is likely to account for a larger fraction of the total income in households where the mother has low education than in households where the mother is highly-educated, the impact on labor supply is probably larger. Higher responsiveness among the low educated may also be partially due to their lower average employment rates.

Regarding income, we divide the sample in two types of households: low-income households and high-income households. *Low-income* households are those with a maximum income of 300000 NOK per year (50300 US dollars). *High-income* households are those with an annual income higher than 300000 NOK. Household income includes income in the form of wage income, bonuses, capital income, and public transfers. Table 4 shows that low-income households are, by far, the most responsive. Again, this is as expected since child day care costs will take up a larger fraction of income in low-income households than in high-income households. Finally, the DDD-coefficients for natives and non-Western immigrant mothers do not differ much. We find a significant effect for native mothers only.

5.3. Sensitivity analyses

In this section, we present a series of sensitivity and robustness checks. We test for composition effects and parallel trends. An important assumption when using DDD analysis is that the composition of both treatments and controls are stable before and after the policy change. There are no standard ways to control for this, but we shed light on the issue as follows. The regression adjusted DDD equation with secondary education as the dependent variable becomes

$$\begin{aligned} \textit{Secondary education}_{ijkt} &= \alpha_1 + \alpha_2 MP_{ijk} + \alpha_3 POST_{itk} + \alpha_4 TREAT_{ik} + \alpha_5 (MP_{ijk} x POST_{itk}) + \\ &\alpha_6 (MP_{ijk} x TREAT_{ik}) + \alpha_7 (POST_{itk} x TREAT_{ik}) + \alpha_8 (MP_{ijk} x TREAT_{ik} x POST_{itk}) + \varepsilon_{ijkt} \end{aligned} \tag{3}$$

A simple test of no composition effects would be that $\alpha_8 = 0$. Table 5 presents results for two sets of variables: education and the presence of older children. The models include all the core explanatory variables.

[TABLE 5 ABOUT HERE]

The test for composition effects reveals no significant relationship between the DDD variable and education or the presence of older children. This result is as expected if no composition effects are present.

Another important assumption when using DDD is that time effects must be common across treatments and controls. One could be concerned with the fact that we compare two groups of mothers that are of different ages, and therefore are in different life periods. It is difficult to undertake a rigorous test, but we approach the severity of this problem by presenting a simple figure presenting the trends in mean labor supply in the period 2000-2005 for two groups of mothers: the treatment group who are mothers with small children (1-2 years old) and the control group who are mothers with older children (7-8 years old) *prior* to the introduction of the reform. Ideally we would like the pre-reform pattern of labor supply trends for the two groups to be parallel. Figure 2 presents the results.

[FIGURE 2 ABOUT HERE]

Figure 2 reveals that the pre-reform trends in labor supply are very parallel for the two groups, and both present very stable employment patterns. Based on this simple exercise, we find no evidence indicating that labor supply evolved differently between the treatment and control group in the pre-periods.

We have also conducted a supplementary test for parallel trends by running a DDD-regression for the pre-treatment period, while interacting all the variables with the pre-years. If the parallel trend assumption is fulfilled, the pre-DDD coefficients should be zero. As pre-years we use 2004, 2003, and 2001 for the mothers from the MP-period, and 2000, 1999, and 1998 for mothers in the not-MP-period. Both DDD-coefficients are small and not significant, which is reassuring.[14]

Finally, our identification also hinges on the assumption that the MP-reform does not affect fertility. We do no perform a rigorous test of this. However, we conduct an analysis where do not limit the analyses to mothers who gave birth to their youngest child in the respective years. If the MP-reform affects fertility one could think that MP-eligible mothers who give birth to additional children were systematically different than

[14] Results not presented in this article, but available in working paper; Hardoy and Schøne (2013).

non-eligible MP-mothers. A regression analysis shows almost unaltered results. The DDD-coefficient is reduced somewhat but is still sizeable and clearly significant (results available upon request).[15]

6. Summary, conclusion and discussion

In an international context, Norway has a generous public family policy with subsidized child care being an important component of this policy. Together with Denmark and Sweden, Norway has the highest female employment rate in the OECD area (OECD 2008). The main question raised in this paper is whether reduced child care costs in such an environment, where female labor supply is already high, is an effective tool to increase labor supply among mothers even further. To answer this question, we exploit exogenous variation in the eligibility to reduced child care costs created by the introduction of a public policy.

In the spring of 2003, a broad political agreement was reached with the overall goal that neither private economic conditions nor lack of child care slots should exclude families from using formal child day care (Innst. S. nr. 50200-2003). The intention was that all parents who wanted a child day care slot should have access to one, and that the costs of a slot should be so low that it became accessible to all parents. The goal of the reform was twofold: to increase child day care coverage rates and to reduce the day care prices paid by parents. Regarding reduced day care prices, cuts were implemented in two stages. In January 2006, a cap on the price the municipality could charge parents was set. The cap was set at 22250 Norwegian kroner (NOK) per month for a full-time slot (approximately 380 US dollars), resulting in a price reduction of approximately 20 percent. It is this reform we evaluate in this article.

All analyses are conducted using high-quality and detailed register data for the whole population of mothers in Norway. The group under study is mothers in the age range of 20–45 years old. We use two measures of labor supply: employment and the number of working days. We measure employment by a dummy variable taking the value 1 if the mother participates in the labor market, and 0 otherwise. We measure the number of working days, conditional on employment. Therefore, the first measure gives us the effect on the extensive margin while the second measure gives us the effect on the intensive margin.

[15] Finally, we also carry out the analyses as presented in Table 3, but for men. We do not expect to find any labor supply effects for fathers, since we know that in Norway the mother is the main caregiver for small children. Finding an effect would raise doubts as to whether the DDD set-up might be picking up some simultaneous labor market trend or occurrence affecting the general demand for parents with children of different ages taking place at the same time as the introduction of the MP reform. The results show that the effect of the MP reform for fathers is equal to zero.

The results show that the decrease in child day care prices did lead to a rise in the employment rate of women with children. The impact is in the range of 4 percentage points, or approximately 5 per cent. The reform seems to have only a minor impact on the number of working days, given employment. The positive and significant employment result and the positive but small and insignificant results for number of working days are in line with previous results, suggesting that labor supply is more elastic on the extensive margin than on the intensive margin. The positive and sizeable impact on employment is robust across different robustness checks. In summary, our results lend support to the hypothesis that cheaper child care is an effective tool to increase labor supply among mothers, even in an environment characterized by an already high female labor supply.

Finally, the Scandinavian countries share a common history and cultural heritage. Although there are some differences they are also characterized by having generous welfare system and ample family policies, have come a long way in terms of gender equality issues and have high female employment. Hence it seems relevant to compare our results to some of previously mentioned Scandinavian studies. The Swedish results in Lundin et al. (2008) reported almost zero effect from a reform with similar attributes as the Norwegian one. Although different methodological approaches could be part of the explanation our regional DD approach has clear similarities to the method used in Lundin (2007), suggesting that methodology cannot seem alone to explain the difference in results. Another plausible explanation for the divergent results is that child day care prices, historically, have been somewhat higher in Norway than in Sweden (OECD 2007). Reduced child care costs therefore, may be more effective when they are initially at a higher level and the drop is steep. Another factor that might contribute to differences in behavior is that Sweden has had full child care coverage, for both younger and slightly older infants, for several decades while for Norway it is a recent phenomenon. Lastly, we cannot disregard the possibility of normative differences between these two countries as to whether small children should be put in day care or not. Regarding the Norwegian studies that of Black et al (2012), cover mother of four year old children, i.e. older children compared to the treatment group in our DDD-analyses. It is expected that, if anything, response is greater among mothers of younger children. Havnes and Mogstad (2011) use Norwegian data and find no impact on maternal employment of a large expansion in child care coverage in the 1970s, when child care prices remained unchanged. Here again, one possible explanation for the differences in results is that their sample comprises mothers of older children (aged 3 to 6) compared to our sample that include children below 3 years of age. Our results are in line with the Danish study of Simonsen (2013) who focuses on younger children, as we do. Nonetheless, a final explanation for the positive findings in our paper compared to some of the above mentioned Scandinavian studies is the potential combined effect of reduced prices and increased capacity. Although we include controls for the local coverage rate we cannot rule out the possibility that we estimate a combined effect. However we can deduce that introducing both a price reduction and increased capacity, are more likely to spur labor supply, compared to introducing only one of the policies, which is the case in the analyses mentioned above.

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Table 1. *Descriptive statistics for the treatment and control groups.*

	Treatment group		Control group	
	Birth year	Birth year	Birth year	Birth year
Variables	2005	2001	1999	1995
Work experience	7.463	7.119	12.761	12.252
Compulsory school	0.206	0.217	0.218	0.281
Secondary education	0.350	0.409	0.436	0.430
University/college – low level	0.345	0.303	0.277	0.236
University/college – high level	0.074	0.058	0.053	0.037
Unknown education	0.025	0.012	0.015	0.016
Married	0.392	0.401	0.611	0.635
Divorced	0.049	0.050	0.109	0.109
Child care coverage rate 1–2 years	44.201	37.430	43.630	37.074
Child care coverage rate 3–5 years	85.158	78.717	85.048	78.601
Number of children under 6 years	0.653	0.656	0.805	0.801
Number of children 6-11 years	0.873	0.889	1.145	1.133
Capital (Oslo)	0.156	0.138	0.096	0.097
Capital income (1000 NOK)	8.280	5.468	9.116	5.760
Unemployment rate municipality	3.269	2.116	3.208	2.119
N	40340	41492	27546	27134

Table 2. *DDD-estimates of employment and working days. Mean values and standard error in parentheses*

	Employment rates				
	Treatment group: Mothers with young children				
Birth year	Evaluation period	Pre	Post	Change	DD-estimate
2005	2003_2004-2006_2007	0.851 (0.001)	0.811 (0.001)	-0.040 (0.002)	
2001	1999_2000-2002_2003	0.854 (0.001)	0.757 (0.001)	-0.097 (0.002)	0.057 (0.003)
	Control group: Mothers with older children				
Birth year	Evaluation period	Pre	Post	Change	DD-estimate
1999	2003_2004-2006_2007	0.790 (0.002)	0.821 (0.002)	0.031 (0.002)	
1995	1999_2000-2002_2003	0.797 (0.002)	0.814 (0.002)	0.017 (0.002)	0.014 (0.003)
DDD-estimate					0.043 (0.005)
	Working days				
	Treatment group: Mothers with young children				
Birth year	Evaluation period	Pre	Post	Change	DD-estimate
2005	2003_2004-2006_2007	504.847 (1.288)	455.693 (1.255)	-49.154 (1.798)	
2001	1999_2000-2002_2003	499.766 (1.323)	429.150 (1.328)	-70.616 (1.874)	21.462 (2.590)
	Control group: Mothers with older children				
Birth year	Evaluation period	Pre	Post	Change	DD-estimate
1999	2003_2004-2006_2007	505.343 (1.572)	554.469 (1.469)	49.126 (2.150)	
1995	1999_2000-2002_2003	491.241 (1.598)	526.647 (1.535)	35.406 (2..215)	13.720 (3.081)
DDD-estimate					7.742 (4.035)

Table 3. DDD-regression results. Binary and duration measures

	Binary outcome					
	Without control variables			With control variables		
	DD-Treatments	DD-Controls	DDD-Treatments and controls	DD-Treatments	DD-Controls	DDD-Treatments and controls
Post	-0.096***	0.017***	0.017***	-0.138***	-0.015***	-0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
MP	-0.003	-0.007**	-0.007	-0.004	-0.018***	-0.017***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
PostXMP	0.056***	0.013***	0.014***	0.035***	0.006	-0.002
	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)
Treat			0.056***			0.116***
			(0.003)			(0.003)
TreatXPost			-0.114***			-0.145***
			(0.004)			(0.004)
TreatXMP			0.005			0.009**
			(0.004)			(0.004)
DDD			0.042***			0.041***
			(0.006)			(0.006)
N	163660	109360	273024	163660	109360	273024
R^2 adj	0.010	0.010	0.010	0.159	0.174	0.164
	Duration measure					
	Without control variables			With control variables		
	DD-Treatments	DD-Controls	DDD-Treatments and controls	DD-Treatments	DD-Controls	DDD-Treatments and controls
Post	-70.626***	35.405**	16.722***	-112.130***	12.917***	16.722***
	(1.851)	(2.185)	(2.152)	(1.885)	(2.196)	(2.152)
MP	5.071***	14.101***	5.820***	-4.284**	3.506	5.820***
	(1.836)	(2.174)	(2.203)	(2.011)	(2.343)	(2.203)
PostXMP	21.473***	13.724***	13.720***	12.495***	2.319	4.000
	(2.597)	(3.074)	(3.129)	(2.816)	(3.292)	(3.107)
Treat			8.535***			47.714***
			(2.036)			(2.021)
TreatXPost			-106.032***			-127.156***
			(2.880)			(2.732)
TreatXMP			-9.031***			-10.363***
			(2.862)			(2.687)
DDD			7.753*			6.305*
			(4.048)			(3.796)
N	119824	80706	200530	119824	80706	200530
R^2 adj	0.019	0.018	0.026	0.153	0.122	0.152

NOTE. The control vector includes the full set of control variables presented in the data section. These are: Work experience (and squared), Education (compulsory school; secondary school; university/college degree, low level; and university/college degree, high level). Non-labor income (capital income), place of residence (Oslo), number of children below age 6, number of children between age 6 and 11, marital status (married, divorced), age of the husband, education of the husband. In addition we include child-care coverage rate in the municipality and unemployment rate in the municipality. Level of significance: *** 1 per cent, ** 5 per cent, 1 per cent.

Table 4. DDD-regression results for different subgroups. Binary measure

	Additional children in child care age			Education		Household income		Ethnic origin	
	Zero	One	Two or more	Low	High	Low	High	Natives	Non-western
DDD	0.027*** (0.006)	0.047*** (0.006)	0.054*** (0.007)	0.065*** (0.011)	0.022*** (0.007)	0.065*** (0.011)	0.021*** (0.007)	0.040*** (0.009)	0.036 (0.028)
R^2 adj	0.141	0.157	0.235	0.228	0.082	0.228	0.135	0.115	0.261
N	109642	133746	29636	96185	100682	96185	176389	246100	19633

NOTE: In all models we include (but do not present) the other core variables and the full set of control variables. Level of significance: *** 1 per cent, ** 5 per cent, * 10 per cent.

Table 5. DDD-regression results. A test of composition effects

	Education				Older children	
	Compulsory	Secondary	University/ college low	University/ college high	N children <6	N children <11
DDD	0.001 (0.006)	0.003 (0.008)	-0.006 (0.007)	0.001 (0.004)	0.002 (0.003)	-0.001 (0.003)
R^2 adj	0.011	0.008	0.007	0.009	0.011	0.010
N	273024	273024	273024	273024	273024	273024

NOTE: In all models we include (but do not present) the other core variables plus the full set of control variables. Level of significance: *** 1 per cent, ** 5 per cent, * 10 per cent.

Figure 1. Mean monthly change in day care costs between pair of years (in Norwegian kroner)

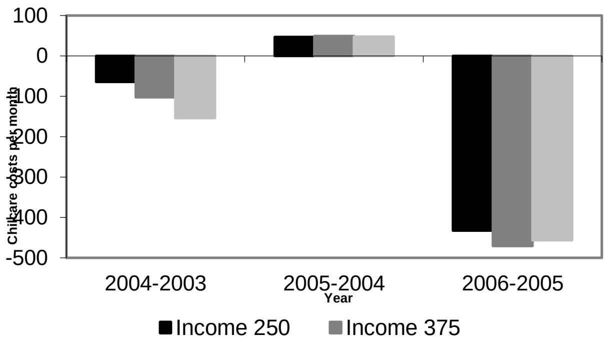

Figure 2. Pre-reform development in labor supply for mothers with small and older children