

WOMEN AND CHILDREN FIRST?

Labor market effects of universal child care for toddlers

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Master of Philosophy in Economics

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University of Oslo

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Preface

The work with this thesis, although tiresome, has been unusually inspiring. The main reason is the inspiration, motivation, availability and help provided by my supervisor Tarjei Havnes, who has introduced me to the wonders of empirical economic research and constantly expected me to improve.

I am indebted to the Centre for Equality, Social Organization and Performance (ESOP) for awarding me their scholarship in gender and economics for working with this thesis and for all the amazing people that I have come to meet through the centre. Besides that, Johannes Elgvin at ESOP has been exceptionally forgiving when the work with this thesis has occasionally been more interesting than my work as a research assistant.

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Martin Eckhoff Andresen, May 2013

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1 Introduction

The last decade, more industrialized countries and international organizations have shown interest in different types of government intervention in the market for child care. Among these is the OECD, who has covered the introduction of early childhood intervention programs in their member states in several reports (OECD, 2006; Field, Kiczera, and Pont, 2007). In his most recent State of the Union address, President Obama proposed to make “high-quality preschool available to every single child in America” (Obama, 2013), and similar moves towards child care reform have been made in Germany and other European countries.

Such interventions are usually claimed to have at least one of three effects. First, they are said to affect fertility and combat low fertility. Economists have documented a negative relationship between income and fertility (Jones, Schoonbroodt, and Tertilt, 2010; Jones and Tertilt, 2006), and high-income countries like Germany have seen universal subsidized child care as a tool for increasing below-reproduction fertility. Second, universal child care is said to benefit child development and equalize differences in initial endowments. Although the evidence on the effects of child care on development is inconclusive, some economists have suggested to use early child care intervention as a cost-efficient way to combat social reproduction (Currie, 2001). Last, and most importantly for this thesis, universal child care is claimed to be an efficient tool for increasing female labor force participation by reconciling work and family responsibilities (OECD, 2006).

In Norway, female labor force participation and gender equality in the labor market is an important goal for the universal child care system and welfare state in general. The government has a clear goal of full child care coverage, claiming among other things that universal child care is “central to parents’ labor market inclusion” (Kunnskapsdepartementet, 2007-2008). Subsidized, universal child care has been an integral part of the Norwegian and Nordic welfare states since the second World War, and so these countries provide natural cases for evaluating the efficiency of such measures. Until recently, however, the availability of such child care has been more or less restricted to preschoolers, children of 3 to 6 years. Several scholars have evaluated the effect on labor supply of child care for these children, among them Havnes and Mogstad (2011) using a 1975 reform.

For younger children such research is scarce. There are several reasons to believe that the labor market response of mothers of toddlers (1- and 2-year olds) differ from mothers of older children. For one, the alternative mode of care may differ: If formal child care is not available, mothers of toddlers might take care of the children themselves while mothers of preschoolers find informal solutions enabling them to work. Second, the preferences of the mothers might differ so that they are more or less responsive to the availability of care.

The purpose of this thesis is to evaluate the third claimed effect of child care: Is

universal child care an efficient tool for increasing the labor force participation of mothers of toddlers?

To investigate causal effects, we need a valid estimation strategy that can disentangle causality from correlation. Simply comparing the labor supply of mothers in municipalities with high child care coverage to that of mothers in low coverage municipalities is not likely to yield unbiased estimates of the true effects.

A reform from 2003 leading to large increases in child care for toddlers in Norway provides a natural way to evaluate the impact of toddler care. The reform increased government subsidies to investment in and running of child care institutions, and generated large variation in expansion rates between municipalities and over time. This variation provide a unique possibility to evaluate the effects of toddler care, and as will be shown, there are reason to believe that the changes in child care coverage following the reform can be regarded as exogenous.

To solve the unobserved heterogeneity problem, we make use of high quality Norwegian registry data in a fixed effects method, explaining changes in labor supply by changes in child care coverage. As long as the composition of unobserved determinants of labor supply is constant, this approach will control for them. Since the main regression uses fixed effects at the municipality level, we also control for a range of individual characteristics that might affect labor supply decisions. These include education of the parents, immigrant status, household characteristics, age and family structure.

In a non-rationed market, changes in both demand and supply will affect the observed coverage rates and we might worry for reverse causality: That increases labor supply lead to increases in coverage rates, rather than the opposite. As will be argued, the child care market following the reform is severely rationed. In a rationed market, the observed child care coverage rates will be driven by changes in supply only until the rationing is lifted. The rationing in the child care market therefore strengthens our empirical approach and reduces problems of reverse causality.

Using this method, we find that a full scale expansion of child care for toddlers will lead to an 11 percentage point increase in the share of working toddler mothers. This constitutes around 16% increased participation compared to the mean over the period. For full-time employment, the effects are smaller at around 5 percentage points or 13% increase in mothers working full time. These effects are significantly different from zero at any conventional significance level.

We also find some evidence of persistency in the labor supply response, indicating that child care for toddlers have effects on labor supply that last several years after the toddler period. This improves the cost efficiency of the reform as it increases the tax base over several years.

We also find slight evidence of a reverse response to child care among fathers: Fathers seem to reduce their labor supply as a response to increased child care coverage, or possibly

increasing labor supply when child care is not available to support a mother not working. Although these estimates are imprecise, the effect of this is to lower the cost efficiency of the reform.

These estimates are larger than other estimates in the literature, particularly Havnes and Mogstad (2011), who find effects around half the size for mothers of 3- to 6-year olds following a similar reform in 1975. Although preferences of mothers of older children might be different and preferences may also change over the three decades between the two reforms, our main explanation of this difference is that the alternative mode of care for toddlers and preschoolers differ. The results from this thesis indicate that the alternative mode of care for toddlers when formal care is not available to a larger extent is parental care than informal care such as relatives or unregistered child minders. This makes a child care reform for toddlers more efficient than for other children when considering the effect on female labor supply only.

Although a cost-benefit analysis of the child care reform is not the purpose of this thesis, the economic implication of these results is that the government must construct around 9 slots in child care to induce one more mother to enter the labor market. These slots are costly, considering the large government subsidies involved, and these are not covered by the increased tax income from one more working mother.

We might worry that our results are driven by self-selection into or out of treatment. This can happen if work-prone mothers migrate into or out of municipalities with large expansion of child care. If there is selective migration, the effects we have found might simply be the result of mothers interesting in working more sorting into municipalities with high expansion. To investigate this, we perform several robustness tests where this sort of selective migration is ruled out, and find that the results are stable to these tests.

Our approach is based on an assumption that is very similar to the common trend assumption of Difference in Differences setups: Conditional on the control variables and with no changes in child care coverage, the municipalities would follow a common trend in labor supply. To test this assumption, we perform two tests where we relax this assumption using separate time trends for each municipality and time shocks that vary by municipality characteristics. These tests also support our approach.

We last perform an overall placebo test explaining the labor supply of mothers of older children by the child care coverage rate for toddlers. This should not have any explanatory power, as these mothers no longer have toddlers. If they do, we would worry for misspecification of our model. This test shows little evidence of misspecification. These and more robustness tests lend support to our empirical approach, indicating that we have identified a causal effect of child care on female labor supply.

The results from this analysis are highly relevant to current political debates in Norway, considering that the demand for child care for older children is more or less covered. The findings in this thesis therefore speak to the efficiency of further expansion. They

will also provide evidence for other countries and governments considering a move towards universally accessible, subsidized child care for young children. Universal child care expansion for toddlers are not necessarily money for nothing as Havnes and Mogstad (2011) find, but still a rather cost-inefficient way to increase female labor force participation.

This thesis progresses as follows: Section 2 presents an overview of the most important empirical evidence evaluating the effect of child care availability and prices on female labor supply. Section 3 presents the child care reform of 2003 and the institutional setting and discusses the system for distribution of child care in a rationed market. Section 4 presents existing theory on labor supply for mothers and develops a model of child care more suited to a rationed formal market.

Section 5 presents the data and the construction of the sample, while section 6 presents the empirical strategy and performs a preliminary analysis of the exogeneity of the expansion. Section 7 presents the results of the main specification, several subsample analyses and discusses and tests for a range of possible problem for the identification, while section 8 concludes.¹

2 Literature

The determinants of female labor supply have attracted considerable interest in economic literature. We therefore start by reviewing notable contributions to the literature investigating the effects of subsidized child care on female labor supply.

Blau and Currie (2006) review more than 20 studies on the labor market elasticities with regards to prices of child care. They report estimates of elasticities ranging from 0.06 to -3.60. This means that we should expect a -.06 % to 3.6% increase in labor supply if the price of child care is lowered by 1%. According to the authors, most of the variation in these estimates comes from the different empirical strategies used, not from variation in samples or data sources. Therefore, it is important to thoroughly examine the different empirical strategies.

A strand of the literature (Anderson and Levine, 1999; Blau and Robins, 1991; Connelly, 1992; Connelly and Kimmel, 2003; Han and Waldfogel, 2001; Kimmel, 1998; Powell, 1997; Ribar, 1992) use discrete choice models to explain the extensive work decision of mothers. They account for selection effects into employment, for example with the two-stage process suggested by Heckman (1979). Baum (2002) uses a discrete time logistic hazard rate model to explain the timing of return to work after birth. As explanatory variable, these studies use child care expenditure data from surveys to measure child care costs and explain variation in employment. The studies using this approach yield elasticities between .04 and -1.26, but as Blau and Currie (2006) note, the methodology used

¹All statistical analyses have been performed using Stata version 12.

can be problematic for two reasons: First, it is bivariate, and does not properly take into account the substitution to formal care among already working mothers.

Second, the variation in child care prices that they exploit might not be exogenous. Most of these studies use average child care expenditure data per hour of care, but these prices are calculated as average expenditures over all mothers who work, also those who use unpaid care and therefore spend less on child care. The measurement error in child care prices might therefore give rise to bias in the estimates. As in other selection model studies, they also rely on exclusion restrictions, variables that can explain the employment decision, but does not have an effect on the use of child care, and if these are not valid, the estimates will be biased.

Tekin (2007), Blau and Hagy (1998) and Ribar (1995) study the same problem, but take into account the joint decisions of labor supply and child care. Ribar (1995) use a structural multinomial choice model to explain married women's child care and employment decisions, and treats the price of child care as a variable that affects the underlying utility of the choices involving formal care. He splits the employment choice into not working, working part time and working full time, and the child care mode into formal or informal care. He then estimates a multinomial choice model with five possible choices, assuming that a mother who does not work takes care of her child herself and that no mother combine formal and informal care. He finds a price elasticity of $-.088$ for women with children under 6 years old, indicating a relatively small labor supply response to price reductions in child care. Tekin (2007) uses a similar multinomial choice model on single mothers, and finds a price elasticity of $-.12$. Blau and Hagy (1998) also model the joint choice of labor supply and mode of care, but use data on prices from a survey of prices in day care centers. Their estimate is $-.20$. The three studies which most thoroughly deal with employment and mode of care as joint decisions thus all fall in the lower end of the studies reviewed in Blau and Currie (2006).

The above mentioned studies do to a limited degree account for selection effects. Another strand of research uses various quasi-experimental approaches to try to control for the selection problem. Baker, Gruber, and Milligan (2008) study the introduction of a universal, subsidized child care system in Quebec in the late 90's that open for other identification strategies to the effect of subsidized child care. They use a cross section survey among families in a Difference in Differences setup to evaluate the effect of child care prices on extensive and intensive labor supply. The control group is the rest of Canada, and as in all DiD-setups, their crucial assumption is that Quebec would have experienced a similar trend in labor supply as the rest of Canada in the absence of the policy change. They find a price elasticity of $-.236$, but also that around one third of the new formal child care users were mothers who were previously working and relying on informal care arrangements.

Gelbach (2002) investigates the effect of a universal intervention that made free public

preschool available for all five year olds. He uses quarter of birth as an instrumental variable for preschool usage, and finds sizable effects of the subsidy on a range of measures of maternal labor supply.

Lundin, Mörk, and Öckert (2008) use a Swedish reform from 2002 that introduced a maximum price for full time child care to study the effect of reduced prices on labor participation when initial participation is high. They exploit survey and register data on municipal child care prices before and after the reform and household characteristics, and use a difference-in-difference matching estimator. They compare households which are identical with respect to a range of household characteristics before and after the reform, but who reside in different municipalities and thus are subject to different price changes. Their approach yield small and insignificant results of the price reduction, and they conclude that further reductions of prices in countries with high initial female labor participation will have limited effect.

Hardoy and Schøne (2010) evaluate the labor market effect of a similar max price reform in Norway in 2006. The reform reduced the maximum price for a full time slot in child care to 2,250 NOK per month, and Hardoy and Schøne argues that this introduced exogenous variation in the price, as municipalities with higher initial prices had to reduce the prices more to meet the max price cap.² They use a triple difference approach, and start by comparing the change in labor supply for mothers giving birth in 2005 and affected by the reform to the change in labor supply for mothers giving birth in 2001, not affected by the reform. To account for contemporaneous macroeconomic shocks during this period, they also compare this change to the change in the labor supply of mothers with older children that are not eligible for the max price guarantee. Using this triple difference approach, they find that the average price reduction of 400 to 500 NOK increased labor supply by 3 to 4 percentage points along the extensive margin, or around 5 per cent. They find no evidence of response on the intensive margin.

Using a similar method, Schøne (2004) studies the introduction of the cash-for-care (CFC) system that introduced cash transfers to mothers opting out of the formal market. Using the same triple difference estimator as above, he finds a 4% reduction in female labor supply as a result of the cash for care system. This is evidence that mothers are sensitive to the price of the outside option of caring for the children themselves.

Drange and Rege (2012) study the same reform to evaluate long term labor supply responses. They use a difference-in-difference approach with mothers of older children as the control group. They find significant negative effects on maternal labor supply, also after the child is no longer eligible for the transfer. The effects on labor force participation and earnings persist until the child is around 6 years old. Rønsen (2000) studies the CFC

²This assumption could of course be questioned - municipalities with higher initial child care prices may have this for example because the parents are richer or willing to pay more, casting doubt on the common trend assumption of the DiD-approach.

reform using a multinomial logit model that takes the different modes of care into account. She compares the estimates of the probabilities of making the different choices before and after the reform, and finds small, but significant labor supply reductions for most women and increased use of non-formal care.

The main inspiration for this thesis is Havnes and Mogstad (2011), who use the variation in child care availability stemming from the child care reform in 1975 to estimate the labor supply response of mothers of 3- to 6-year olds. The reform led to a large-scale expansion of child care by subsidizing the construction and running of child care institutions, similar to the reform under study in this thesis. Havnes and Mogstad split the municipalities in two groups. The municipalities with above median percentage point growth in child care coverage from 1976 to 1979 constitute the treatment group, while the below median growth municipalities constitute the control group. They then use a Difference in Difference approach and compare the change labor supply for women in the treatment municipalities to the change in labor supply for women in the control municipalities. They find significant, but small effects of the child care expansion on female labor supply. Out of the approximately 17,500 more child care slots constructed in the treatment municipalities compared to the comparison municipalities, only 640 women were induced to start working. Their conclusion is that the expansion primarily crowded out informal care arrangements. In the empirical part of this thesis, this relationship will be investigated also for younger children in the 2000's using a similar child care reform.

3 Institutional setting and the child care reform

The Norwegian child care system has its roots back to the 19th century, with child care asylums for children of the poor appearing in several Norwegian cities as early as the 1830's.³ The system of universal child care as we know it was developed after the second World War as a response to the increasing female labor force participation and the goal of gender equality in the Nordic welfare model (The Norwegian Ministry of Children and Family Affairs, 1998). The demand for child care led to increased rationing of formal care in the 60's and 70's, eventually leading to the Kindergarten Act of 1975, a broad agreement aiming to increase the provision of universal child care (Havnes and Mogstad, 2011). The goal of the reform was to strengthen and secure child development and stimulate the municipalities to expand child care coverage. This reform introduced large subsidies for child care expansion, and eventually led to large increases in the coverage rates for 3-6 year olds.

The younger children, however, were not subject to the same expansion of child care coverage as a result of the first Kindergarten Act. The increase in the coverage rates for toddlers and preschoolers is pictured in figure 1. In 2001, 80% of the 3-5 year olds

³See the appendix of Stortingsmelding nr. 41 (2008-2009) for a thorough treatment.

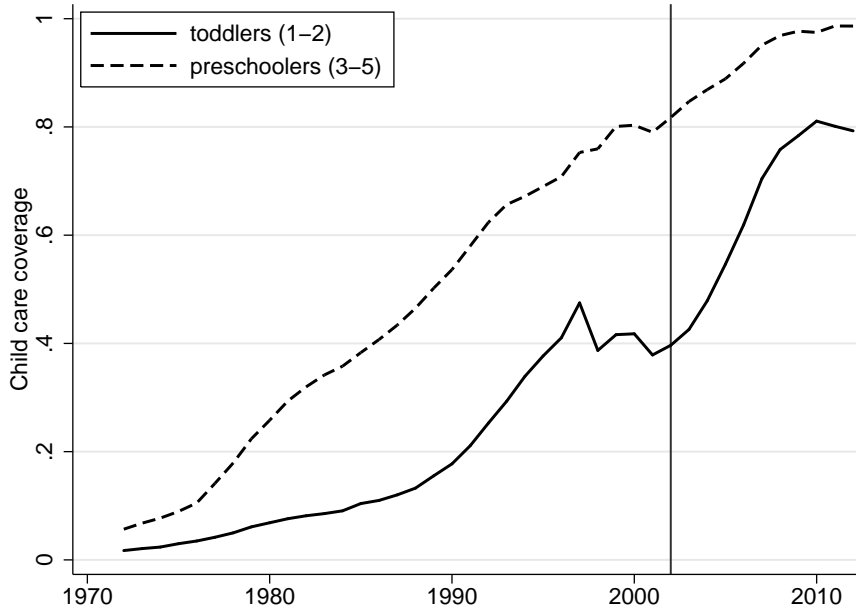


Figure 1: Child care coverage rates in Norway, 1972-2012

Source: Statistics Norway.

in Norway were in formal child care, while only 37% of the 1- and 2-year olds were the same. This was the background for the kindergarten concord,⁴ a reform that led to large increases in child care for young children that will be exploited in this thesis.

On June 11th 2002, a majority of the Norwegian Parliament agreed on the main principles for financing and governing the child care sector. The government was asked to present specific plans for how to carry out the agreement. These plans were put forward in Stortingsmelding nr. 24 (2002-2003), a plan for financial and legal changes in the child care sector, with broad bipartisan support. The main goals of the concord was to supply universal child care to all children, provide equal treatment of public and private suppliers of child care, lower parental fees and secure quality and diversity in child care services.

The most important means for obtaining these goals were a legal obligation for the municipalities to supply child care, equal eligibility for subsidies for private and public child care institutions, increased subsidies, a specific investment subsidy to encourage the construction of new child care institutions and simplifications of the rules for kindergartens, aiming at making it easier to start and run kindergartens privately.

Figure 2 presents some important changes in the child care sector following the reform. All monetary values are indexed to the price level in 2000 so that the real spending is comparable over time. After the reform, investment in new child care institutions increased substantially, as can be seen in panel A. We also see that the total number of child care institutions increase slightly (panel B), primarily due to the increase in private institutions. The total hours spent in formal care has increased quite a lot, as can be seen

⁴Or “barnehageforliket”, in Norwegian.

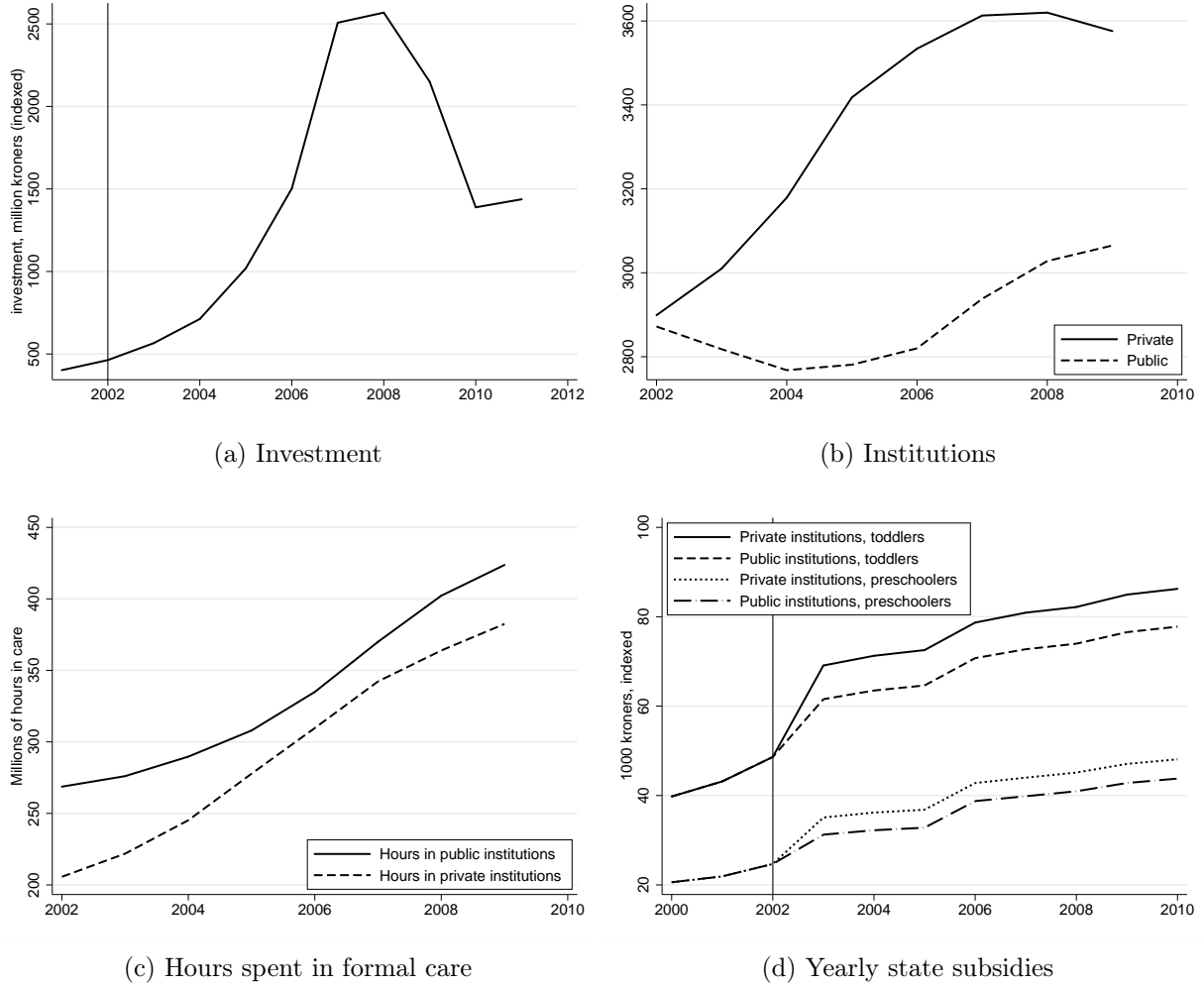


Figure 2: Changes in the child care sector in the 2000s
Source: Statistikkbanken 04683. Subsidy rates from regjeringen.no.

from Panel C.⁵

The increase in the government subsidies that was announced was also carried through. Panel D shows the government subsidy rates per child in full-time care, and we see a considerable increase in the subsidy rates over the period. The subsidies are marginally higher for private institutions than public institutions since 2003, a difference that was motivated by the need for higher public subsidies to allow the private institutions to lower the prices, which was previously higher than in public institutions (Kunnskapsdepartementet, 2007-2008).

The composition of parental fees, municipal support and government subsidies in the financing of child care is depicted in figure 3. As we can see, the composition has changed dramatically after the reform. The parental fees have stayed more or less constant over the period, while the municipal support and government subsidies have increase dramatically.

⁵These hours are age corrected, meaning that one hour of toddler care accounts for 1.8 hour of preschooler care. The real number of hours spent in child care has also increased.

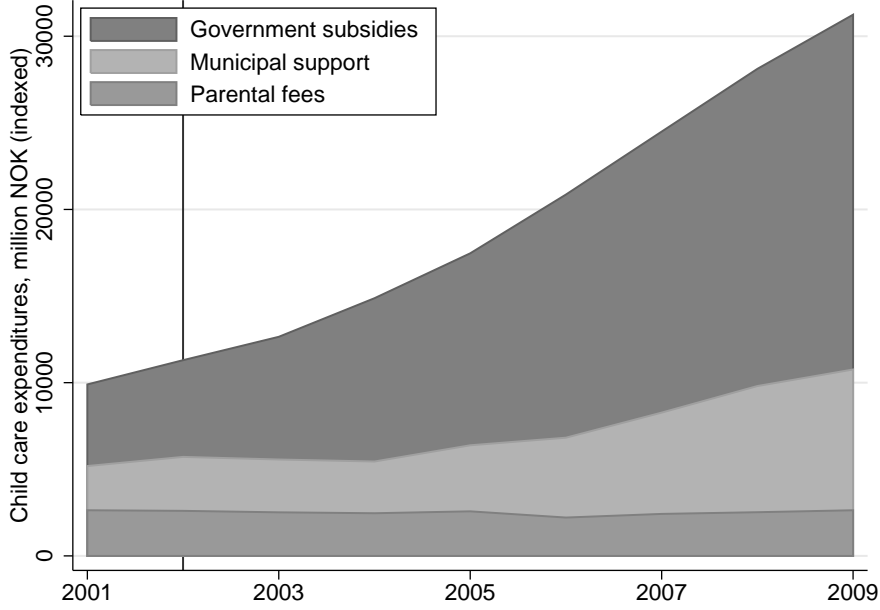


Figure 3: The composition of financing in child care
Source: Statistikkbanken 04683.

This table also shows that the overall municipal support was not reduced as a response to the increased government subsidies. The large overall increase in expenditures in the sector over the period is a result of both increases in the number of children and the increased share of toddlers, requiring more staff and resources per child.

The total result of the reform was a sharp increase in the municipal coverage rates for 1- and 2-year olds. As can be seen from figure 1, there has been an increase in the coverage rates for older children, from 80% in 2001 to more than 95% in 2010, while the toddler coverage has increased more, from 37% to 80%.

The reform also generated large spatial differences in the municipal coverage rates. Some municipalities responded quickly and more strongly, while others had later or slower expansions of child care. A graphical representation of the municipal coverage rates in some selected years can be seen in figure 4. Judging from this map, it is hard to find any geographical pattern of expansion. The municipal child care coverage for 1 and 2-year olds is also illustrated in figure 5, showing the distribution of the municipal coverage rates over the period. This variation will be used to identify the effect of child care for toddlers in the empirical part of this thesis.

The child care coverage rates that have been reviewed so far and will be used in the empirical analysis are average rates. They are calculated simply as the fraction of appropriately aged children in child care to the total population of the same age in the municipality. These rates, however, aren't necessarily the chances of obtaining a slot in child care that the individual mother faces. As there is rationing in the child care market,⁶

⁶This rationing is an important assumption for our empirical strategy, and so the degree of rationing

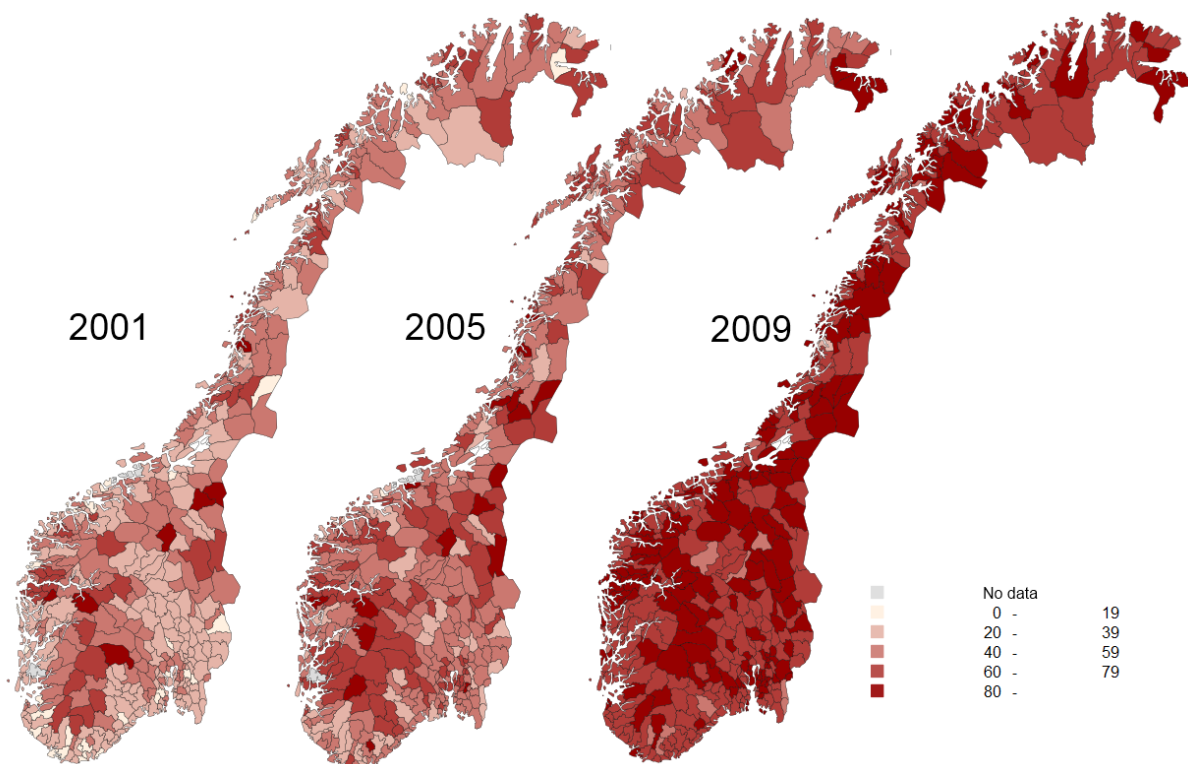


Figure 4: Municipal coverage rates in 2001, 2005 and 2009
Source: Statistikkbanken 04683.

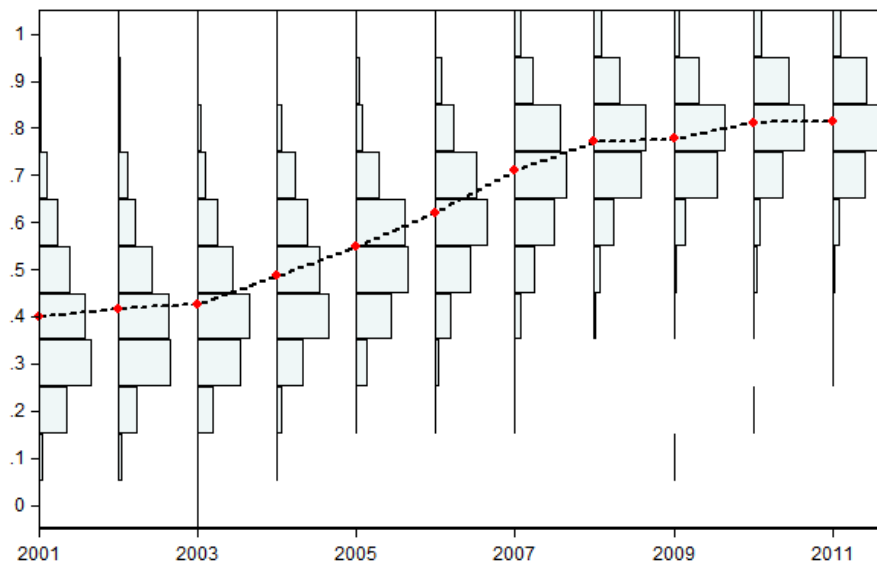


Figure 5: Distribution of municipal child care coverage rates for 1-2 year olds, 2001-2011
Source: Statistikkbanken 04683.

it is important to understand how this rationing works and how the slots are allocated.

According to the Kindergarten Concord, the municipalities are obliged to coordinate the application process for applying for child care in the municipality. Most municipalities have one yearly round of admissions in August with a deadline for application during the spring. In addition to this, most municipalities accept children during the year as slots become available, usually prioritizing by time spent on the waiting list. Two groups have priority by law in this process: disabled children and children enrolled in the child welfare system. In addition to this, the municipality and the individual child care institution are free to choose other rules for admission and priority. Common criteria for priority include children of single parents, children from families with extraordinary challenges due to health conditions or disabilities of siblings or parents, children of low earners, children with siblings in the same child care institution and children of immigrants.

After having supplied child care to the applicants from the priority groups according to the rules in the municipality, the rest of the slots are generally divided according to age or distributed randomly.

This means that the current child care coverage rate isn't necessarily the rate that the individual mother faces. A mother with a child in one of the priority groups, for example, will more or less know for certain that she will have access to a slot in child care. Since the priority groups generally constitute a very small share of total applicants, this will not be a major problem for the empirical strategy.⁷

4 Theory of the child care market

To understand the effects of a child care reform on female labor supply, some insights can be gained from economic theory. When we discuss labor participation, we usually distinguish between the extensive and the intensive margin of labor participation. Policy changes might induce changes in who choose to work and who do not (the extensive margin), or changes in the hours worked among those who already work (the intensive margin). In the following, we focus on the extensive margin.

4.1 The modes of child care

Labor supply and child care are joint decisions: They are dependent on each other. It's useful to structure the different combination of these choices that a mother can make. In general, we can distinguish between three modes of child care: Parental care, informal care and formal care. Some studies (e.g. Blau and Currie (2006)) also distinguish between relative and non-relative informal care, but for our treatment, grouping the two will be

will be explored further in section 6.2.

⁷This could be solved by instrumenting the actual child care usage with the municipal child care coverage rates, but since we don't have access to individual usage data, that is beyond this thesis.

sufficient. Informal care arrangements include non-parental care by relatives, friends and neighbors as well as unregistered child minders etc. Formal care arrangements include regulated and paid child care services.

This gives rise to the following combinations of care and employment:

Table 1: Care and employment combinations

| Alternative | Mother employed | Formal care used | Informal care used |
|-------------|--------------------|---------------------|-----------------------|
| 1 | No | No | No |
| 2 | Yes | No | Yes |
| 3 | Yes | Yes | No |

Table adapted from Blau and Currie (2006).⁸

There are four more possible alternatives, but these are of limited interest. Any combination of formal or informal care combined with not working will be a waste, as there is a price to these services and the mother could just as well have taken care of the child herself, given that her leisure time is equally valuable with and without caring for the child. Combining formal and informal care would mean more time spent on organizing care alternatives. Also, paid informal care alternatives are not subsidized the same way as formal alternatives, so that paid informal care alternatives will be relatively expensive. Of course, if we introduce quality concerns, some parents might believe that their child is better off in the part-time care of relatives, but this is nonetheless a rare combination of care and employment.

Increased availability of subsidized care will have two effects: It gives an incentive to work and use formal child care, and thus increases the probability of choosing alternative 3 and decreases the probability of choosing alternative 1. This is what we will call *efficient substitution*, given that it leads to increased female labor supply. The price decrease however also provides another incentive: To choose alternative 3 over 2 conditional on working. This is the *informal substitution*, it doesn't translate into increased labor force participation.

In addition to this, the price subsidy might give an incentive to increase labor supply for mothers who are already working and using formal care, but this depends on the relative size of the income- and substitution effects. This is the effect on the intensive margin.

4.2 Why subsidize child care?

In a perfect market with full information, there would be no reason to subsidize child care. The theory on the child care market does however supply arguments for doing so.

According to Blau (2003), these reasons may be roughly separated in three categories. There are self-sufficiency arguments, arguing that child care can help keep low-income caretakers stay in employment. This will reduce their welfare dependency and might reduce human capital depreciation from not working. If there is positive serial correlation in employment, this might raise a case for subsidizing child care as a way of keeping parents in the workforce.

The second class of arguments concerns imperfections in the market for child care. If there are positive externalities of child care or parents lack foresight on their children's behalf, they might not correctly evaluate the effects of child care, thus underinvesting in it. Walker, Goldberg, Schultz, Piel, Zigler, and Finn-Stevenson (1996) also argue that liquidity constraints - the inability to use future earnings as collateral when borrowing money for investing in child care - might lead to market imperfections. These market imperfections might be mitigated via state subsidies.

The third class of arguments for subsidies is distributional - that universal or targeted child care can be a tool for redistribution. Inherent in this argument is the willingness to accept efficiency losses to achieve redistribution. We leave paternalistic arguments and arguments of child care as merit goods out for now.

4.3 Child care models

To understand how mothers make labor supply decisions, fairly simple, one-person optimal labor supply models are common in the literature to predict the effects of subsidizing child care (Hardoy and Schøne, 2010; Blau and Currie, 2006). The mother is the agent in these models, maximizing a utility function over consumption and leisure. Assume that the child must be taken care of at all times, so that the mother must purchase formal child care services for all the hours she works. This simply reduces the net gain from an hour of work to $w - p$ where p is the price of an hour of care. The mother will equalize her marginal utility of leisure, measured in units of the consumption good, to the payoff of another hour of work net of the child care costs.

Subsidizing child care will make the mother instead equate this marginal rate of substitution to $w - p + s$, where s is the subsidy. This will yield higher labor supply if we assume that leisure and consumption are normal goods. This situation is presented graphically in figure 6. The mother allocates where her indifference curve intersects the budget constraint, and subsidizing child care will shift the slope of this budget constraint as illustrated.

The woman with the dotted indifference curves will be better off, but her total labor supply response cannot be signed, as the subsidy has income and substitution effects that work in opposite directions. The woman with the solid indifference curves, however, will unambiguously increase her labor supply. Without the subsidy, she had a reservation

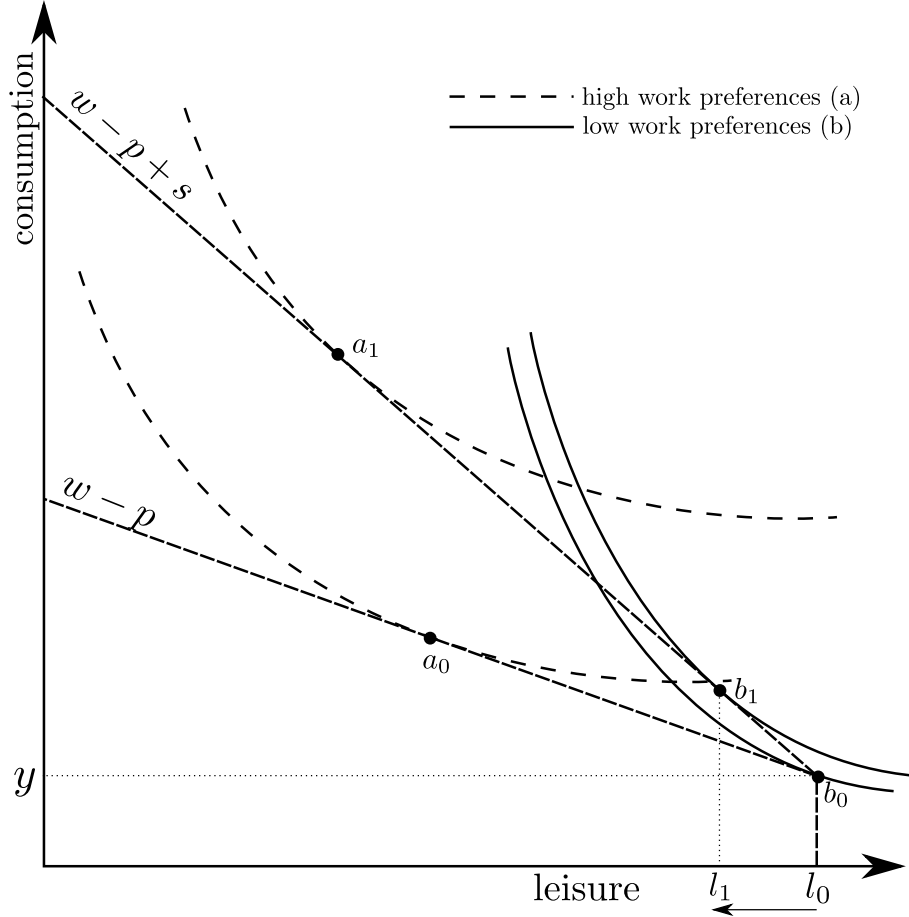


Figure 6: A simple labor supply model

wage given by the tangent of the indifference curve at b_0 , which was higher than the net payoff given by the slope of the budget line, and she chose not to work. After the subsidy, she will allocate at point b_1 , with less leisure and increased labor supply.

This simple model does not take into account informal care alternatives. Blau and Currie (2006) remedy this by presenting another model, in which informal care can be used as an alternative and the leisure of the relative providing this care is incorporated into the utility function of the mother. This yields the prediction that the mother will combine formal and informal care so that the marginal rate of substitution between consumption and the leisure of the relative equals the market price of care, while formal care will be used for the rest of the working hours of the mother.

None of these models take into account the effects different forms of care might have on the development of the child. Blau (2003) develops a model where the mother can freely purchase care of different quality that affects the development of the child, of which the mother cares. The effects of subsidies in such quality-quantity models are hard to sign, but it can be shown that there is a trade-off between the goals of having higher quality care for children and the goal of increasing female labor supply. If the main goal is to increase female labor supply, the subsidy should be tied to the amount of care used,

not the quality. If the goal of the subsidy is to further child development, the subsidy should rather be tied to the quality of the care than the amount.⁹

4.4 A model of rationed formal care

These models have interesting implications, but are not directly applicable to the Norwegian child care market. We do not observe widespread combinations of formal and informal care, as the informal care model of Blau and Currie (2006) predict. Rather, the Norwegian market is characterized by a heavily subsidized formal care sector at fixed prices. There are also strict regulations related to the pedagogical content and quality of the child care institutions to qualify for these subsidies.

The size of the subsidy makes sure that more or less no child care institutions are run without the support. Asplan Viak (2008b) conclude that a maximum of 60 child care institutions, out of a total 6,400, choose to charge more than the max price. In practice, this ensures that there is a large supply of formal child care with homogenous quality because they have to adhere to strict regulations.¹⁰ Therefore, the assumptions of the quality-quantity model do not fit the market either.

The market is also characterized by rationing. Asplan Viak (2008a, 2006) investigate waiting lists in Norwegian kindergartens. They find that 6.2% of all children of preschool age were on a waiting list for child care in 2006 and 3.7% in 2008. Since there were more or less full child care coverage for older children, a large majority of these children are 1- and 2-years old, so the rationing of child care for toddlers is much more severe.

This does not fit well with the assumptions of the models presented earlier, where a mother can freely choose the quality of care at an increasing price per hour for higher quality care, and where all markets clear. Rather, the Norwegian market is characterized by an informal sector consisting of paid and unpaid care primarily as an alternative to a rationed formal market for those who do not get a slot in formal care.

4.4.1 The model

To understand a how a child care expansion will work in a rationed market for child care, we therefore present a model more tailored to fit what we see in Norway. Assume that there is an underlying supply function in the formal market S_f , an increasing function of the price, illustrated by the dotted line in figure 7. There is a maximum price p_f that a child care provider can command in order to qualify for subsidies. The supply function that the consumer faces will thus have a discontinuity, as illustrated in figure 7. We adopt

⁹As mentioned, the total effect cannot be signed without further assumptions, but it can be shown that the effect on labor supply will be more positive or less negative from an amount-subsidy than a quality-subsidy.

¹⁰Even if there are differences in quality between formal child care institutions, these are very hard for a mother to evaluate.

the standard arbitrage market assumption,¹¹ which ensures that the available child care slots will be allocated to the mothers with the highest willingness to pay.

After the slots in formal child care have been distributed to the consumers with the highest willingness to pay, we assume that the excess demand is such that no consumer would be willing to pay the full, non-subsidized price of a formal slot in child care. Looking at figure 3, the formal care is financed by only a small share parental fees, indicating that the price for formal care without subsidies will have to be very high. The excess demand for child care will thus have to be met by an informal market.

We furthermore assume that there is a utility cost of having a child in informal care rather than formal care. This cost can represent the disutility a mother inflicts upon her relatives, the decreased child development from the inferior quality of informal care or some other utility cost. This cost comes in addition to the monetary cost of informal care. For expositional clarity, we choose to add this cost to the informal supply curve rather than subtract it from the demand curve. This allows us to represent one unified demand function D for the two markets. The supply curve S_i thus represents the total cost of producing an informal child care slot that is equivalent in utility to the mother as a slot in the formal market. The vertical axis no longer measures only the monetary price, but rather the total utility cost of informal care measured in monetary terms. For the formal market, the vertical axis still measures monetary price.

Lastly, we assume for simplicity that each mother has only one child in need of care, that the mother will not utilize any care if she does not work and that the price of informal care is such that a mother will not combine formal and informal care.¹² All increase in use of formal or informal care will thus translate directly into increased labor supply.

This situation is illustrated in figure 7. There is a maximum price p_f that decides the size of the rationed formal market, a_0 . This point is the starting point for a new, informal market, which clears at the intersection of the demand and informal supply functions. This means that there is an informal market of size $b_0 - a_0$. To completely lift the rationing if the formal market, c slots in formal care would have to be supplied.

4.4.2 The effect of child care expansion

We are now ready to analyze what happens when the government increases the subsidies to increase the supply of formal child care. We start out in a situation as discussed previously, with a formal market of size a_0 and an informal market of size $b_0 - a_0$, as illustrated in figure 8. The government then increases the subsidy by ρ , leading the formal supply function to shift downwards, from S_f^0 to S_f^1 . This will increase the size

¹¹This will be relaxed in section 4.4.3.

¹²We are looking at the external margin of labor supply, so this assumption is not so unlikely. The change in labor supply would require child care for at least a substantial amount of the normal working day.

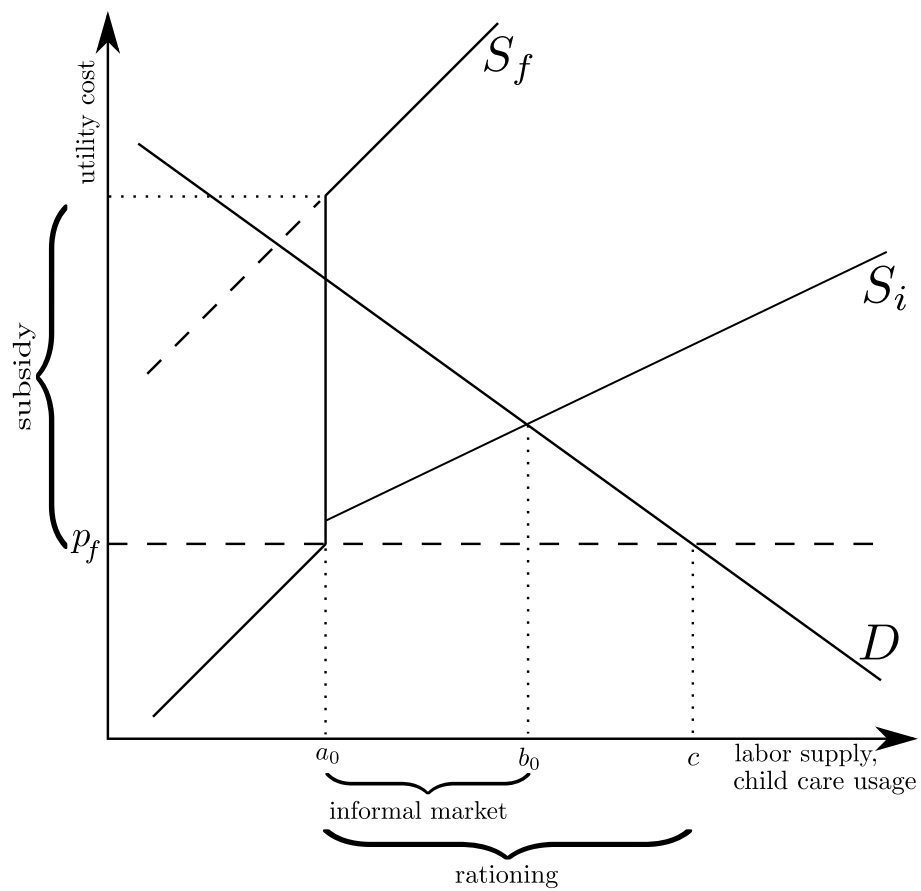


Figure 7: A model of rationed child care

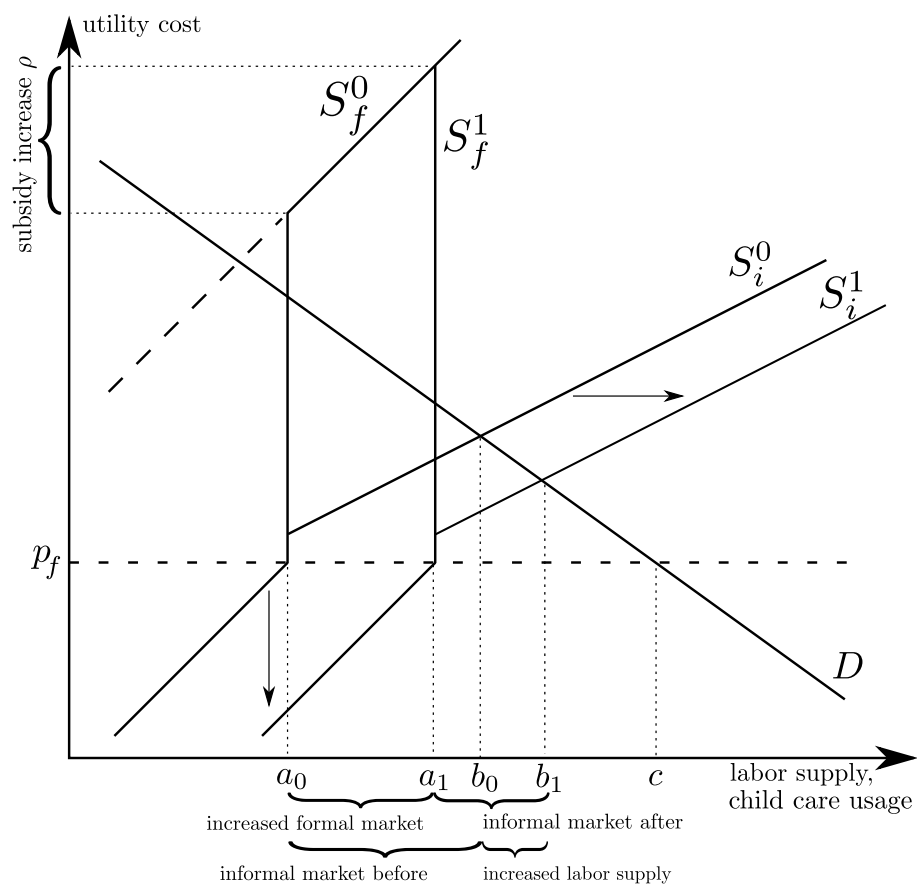


Figure 8: Increased subsidies in the rationing model

of the formal market to a_1 . The new informal market will start in this point, and the informal supply function will shift from S_i^0 to S_i^1 . The new informal market will clear at b_1 and the size of the informal market will be reduced from $b_0 - a_0$ to $b_1 - a_1$. The number of children in non-parental care, and under our assumption also the number of working women, will increase by $b_1 - b_0$. This is smaller than the increase in formal child care, $a_1 - a_0$.

The reform thus has the following effect: Spending $\rho * a_1$, the government creates $a_1 - a_0$ new slots in child care. This expansion leads to a total of $b_1 - b_0 < a_1 - a_0$ mothers beginning to work, while the rest of the expansion is taken up by mothers moving from informal care arrangements to formal care. The expansion thus crowds out informal care, and the increase in labor supply is smaller than the increase in formal care.

If we for the sake of the argument assume that the S_f , S_i and D functions are linear, we can find an expression for the efficiency of the reform. Using linear algebra, we find that

$$b_1 - b_0 = \frac{\beta_i}{(\beta_d + \beta_i)\beta_f} \rho = \frac{\beta_i}{\beta_i + \beta_d} (a_1 - a_0) < a_1 - a_0 \quad (1)$$

Where β_i is the slope of the informal supply function, β_f the slope of the formal supply curve and β_d the slope of the demand function. The degree to which the expansion will lead more women to start working will depend on the slopes of the demand and supply functions. Since the prices in this model are utility costs, not monetary cost, we cannot use this model to construct a theoretical prediction for the ratio $\frac{b_1 - b_0}{a_1 - a_0}$, which is analogous to what will be estimated in the empirical part of this thesis, but this nonetheless serves to illustrate the substitution that takes place following a child care reform.

4.4.3 Relaxing the arbitrage market assumption

Like most simple supply and demand models with rationing, this model is based on the assumption that there is an arbitrage market, a side market where child care slots are bought and sold until the slots are allocated to the mothers with the highest willingness to pay. This sort of trade is beneficial to both parties, so that a trade of a child care slot to a mother with a higher willingness to pay will give rise to a Pareto improvement, leaving both women better off. We thus get an ordering of the market by willingness to pay, ensuring a smooth, decreasing demand function.

In general, this is not how child care slots are allocated in Norway. Slots in child care are not bought or sold privately, and willingness to pay is not part of the criteria for how the slots are allocated. There are therefore reasons to believe that the arbitrage assumption is not reasonable, and we must ask how this affects the analysis.

The arbitrage assumption is investigated in figure 9. To put it simply, lack of arbitrage means that one mother in area A that would get a slot in formal care under arbitrage,

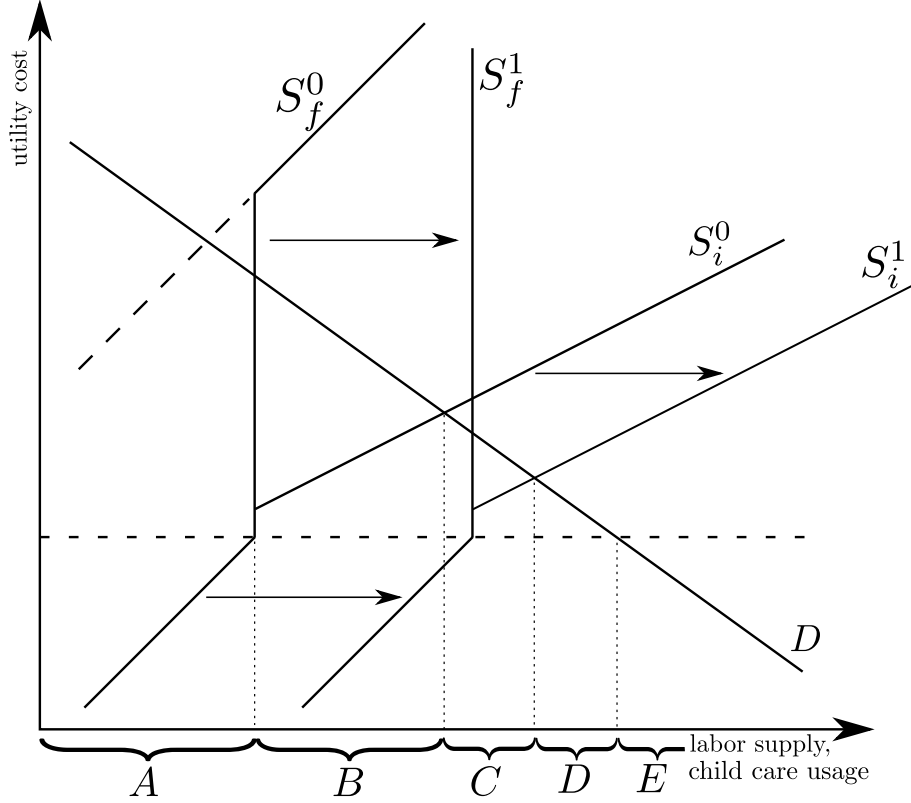


Figure 9: Effects of relaxing arbitrage in the rationing model

| | B | C | D | E |
|----------------------------|---|---|---|---|
| Before expansion | 0 | + | + | 0 |
| After expansion | 0 | 0 | + | 0 |
| Total effect of the reform | 0 | - | 0 | 0 |

The effects on labor supply of relaxing the arbitrage assumption by switching one slot in formal care from a mother in area A to a mother in area B, C, D or E, before and after the reform.

does not. Instead, this slot is allocated to a mother in one of the other areas B, C, D or E. To analyze the effect of the missing arbitrage market, we therefore look at how the effects of an expansion would change if a slot in formal care was transferred from a mother in area A to a mother in area B, C, D or E in figure 9. In general, relaxing the arbitrage assumption in this way will have positive or no effects on labor supply, of course at the cost of being Pareto inefficient.

We start out by looking at the mother in A who does not get a slot in formal care. Since she has a high willingness to pay, she will buy care in the informal market and continue working, so relaxing the arbitrage market assumption will have no effect on her labor supply.

If a mother in B gets a slot in care, we will not get a change in labor supply, since this

woman was working also under arbitrage. The effect of this is simply a swap of child care slots with no effect on labor supply. For a mother in C, however, we will see a difference before and after the reform. Before the reform, the woman in C would start working if she got access to formal care, which she was not under arbitrage. This will not happen after the reform, as she is now covered by the formal market. Relaxing the arbitrage market assumption will decrease the effect of the child care expansion.

A mother in area D is not willing to pay the price in the informal market, neither before nor after the reform. She is however willing to pay the price in the formal market, and so she will accept and start working if she is offered a slot in formal care. Relaxing the arbitrage market assumption this way therefore increases labor supply, both before and after the reform. Lastly, a mother in area E is not willing to pay the price for formal care, so she will not apply and cannot alter the conclusion.

The results of this analysis are summed up in table in figure 9. As we can see, relaxing the arbitrage assumption does at no point increase the labor supply response of the reform. Rather, it may decrease the effect of the reform, depending on how the rationing works. If the rationing works so that at least some mothers in area C is offered slots in formal care at the expense of mothers with higher willingness to pay, the expansion will have a smaller effect on labor supply than argued in the previous section. How the rationing of child care works is briefly described in section 3. To assess how much smaller effect a reform will have due to the missing arbitrage market, we would have to analyze how the willingness to pay¹³ correlates with the criteria for getting a slot in formal care, which is beyond the scope of this thesis.

To sum up, the models reviewed in this section predict that labor supply should increase as a response to increased availability of child care. There is however also substitution from mothers already working and using informal care, so that an expansion crowds out informal care. The relative magnitudes of these two effects and the total effect of a child care expansion on female labor supply are an empirical question.

5 Data and descriptive statistics

Our data come from administrative registers from Statistics Norway.¹⁴ The data covers the entire resident population of Norway from 2001 to 2009. According to a review by Atkinson, Rainwater, and Smeeding (1995), this data is of excellent quality and coverage, and received the highest rating. The data contains a unique personal identification number, which allows us to match each mother with her children and the father of the children.

Data on annual earnings come from the national tax records. This is supplemented

¹³Or actually, being in area C.

¹⁴The data is made available through the Frisch Centre for Economic Research.

with demographic data and data on residency from the Central Population Register. Education data comes from the official education registers.

Data on the municipal use of child care is readily available through Statistics Norway’s online database Statistikkbanken.¹⁵ These data are reported annually by the municipalities themselves, and are the basis for subsidy eligibility. We construct the child care coverage rate as the ratio of registered 1- and 2-year olds in child care to the total population of the same age. The result of this definition is that a couple of municipalities will have child care coverage rates above 1 for a few years, probably due to children from neighboring municipalities attending child care institutions. For these few cases, the coverage rates have been adjusted to 1.

There are registers covering hours worked,¹⁶ but these registers only cover non-random parts of the population.¹⁷ Therefore, we follow Havnes and Mogstad (2011) and measure labor market attachment by annual earnings, constructing dummy variables equal to one if earnings exceed specific thresholds. These thresholds are multiples of the Basic Amount of the Norwegian welfare system, which is consumer price indexed annually. In 2012, one basic amount equaled 82,122 NOK, or approximately 14,100 USD. In the baseline specification the main outcome variables are dummies equal to 1 if earnings exceed 2 or 4 basic amounts, where 2 basic amounts can be considered at least a substantial amount of part-time employment and is interpreted as labor force participation. 4 basic amounts can be considered more or less full-time employment. We have also investigate linear earnings as an outcome variable, as well as each percentile of the income distribution as thresholds in figure 11.

We also utilize various other municipality characteristics from Statistikkbanken. Among them are population data, data on rural and urban population, employment in different sectors, employment by gender, registered unemployment, political representation and data on municipal income and spending.¹⁸ These data are used to construct subsamples, to investigate different trends in child care expansion in different municipality types and to perform the preliminary exogeneity tests in section 6.2. For an overview of the most important variables used in this thesis and their sources, see tables 8 and 9 in the appendix.

5.1 The sample of interest

To construct the main sample, we start with the entire Norwegian population of mothers in 2001 to 2009. We include only mothers whose youngest child turns 1 or 2 in the sample year if the child is born before September 1st, or who turns 2 or 3 in the sample year if the child is born after August 31st. The reason for using this definition is to avoid

¹⁵<http://www.ssb.no/statistikkbanken>, specifically table 04683.

¹⁶The Register for Employers and Employees, or “Arbeidstakerregisteret”

¹⁷Specifically, they exclude employees in small private firms.

¹⁸Tables 07459, 05212, 03324, 01610, 01603, 01182 and 04901, respectively.

the heavy impact the early months of maternity leave would have on labor supply, as well as acknowledge that the time of starting child care often follow the school year. This definition thus ensures that all mothers in the sample have children aged between 5 and 35 months.¹⁹ We exclude women with another, younger child and women who are registered as students at least one month during the year, and also exclude two observations with data abnormalities.

The education data is a variable on the highest attained education. It is a categorical variable coded according to the Norwegian Standard Classification for Education (NUS2000). To allow for flexible effects of education, for example through sheepskin effects, we use seven dummy variables, equal to 1 if a person has primary education (7 years), lower secondary education (10 years), basic upper secondary education (12 years), final upper secondary education (13 years), post-secondary non-tertiary education (14 years), undergraduate tertiary education (17 years) and graduate tertiary education (19 years). These dummies are included to control for the impact of education on labor supply in a flexible way.

Education data is missing for some observations, to a larger extent than others immigrants. Instead of excluding these observations, we set their education to the lowest level and include a separate dummy. This also ensures that an observation is not dropped even if data on the father is missing.

The resulting sample consists of 746,941 observations. There are observations on a total of 347,787 women, with an average of 2.15 observations per woman. The multiple observations per women stems from two sources: Most women are observed twice for each child,²⁰ and some women have more than one child in the sample period.

Summary statistics for the main variables of interest used in the empirical analysis can be found in table 2. We see that an average of 67% of mothers of 1- and 2-year olds work according to our participation definition, while around 38% work full-time. The average age of the mother is around 32 years, while they on average had their first child at almost 27 years. Education average around 14 years, approximately one year more than high school. We also see that education among fathers are slightly lower than among mothers, although this is probably because education data is more often missing for fathers than mothers, and observations with missing education data have had their education set to the lowest level (7 years).

¹⁹We have also tried a separate definition where women are simply included the year where their youngest child turns 1 or 2, without this having any impact on the results.

²⁰Unless they have another child or have a child at the start or end of the sample period.

Table 2: Summary statistics

| Variable | Mean | Standard deviation | Min | Max |
|-------------------------------|---------|--------------------|-----|-----|
| Age | 32.41 | 5.05 | 15 | 69 |
| Age at first birth | 26.80 | 4.90 | 7 | 54 |
| Earnings (BA equivalents) | 3.27 | 2.66 | 0 | 244 |
| Earnings > 1 BA | 0.76 | 0.42 | 0 | 1 |
| Earnings > 2 BA | 0.67 | 0.47 | 0 | 1 |
| Earnings > 4 BA | 0.38 | 0.48 | 0 | 1 |
| Earnings > 8 BA | 0.04 | 0.19 | 0 | 1 |
| Immigrant | 0.19 | 0.39 | 0 | 1 |
| Cohabitation | 0.55 | 0.50 | 0 | 1 |
| Number of older siblings | 1.01 | 1.01 | 0 | 15 |
| Child is female | 0.48 | 0.50 | 0 | 1 |
| Child care coverage, toddlers | 0.56 | 0.17 | 0 | 1 |
| Education (years) | 13.82 | 3.40 | 7 | 19 |
| Education of father (years) | 13.53 | 3.30 | 7 | 19 |
| N | 746,941 | | | |

Summary statistics of all non-studying mothers with their youngest child aged 1 or 2 in 2001 to 2009.

6 Empirical strategy

To study the effect of increased coverage of child care for toddlers, we exploit the temporal and spatial variation in municipal coverage rates following the Kindergarten Concord that is illustrated in figure 5. We employ a fixed effects linear probability model where we explain changes in labor supply by changes in the municipal child care coverage rate for toddlers. For a thorough treatment of unobserved heterogeneity panel data model, see (Wooldridge, 2010, chapter 10). The model estimates

$$y_{ikt} = \alpha_k + \tau_t + \gamma CC_{kt}^{12} + \delta u_{kt}^m + \beta \mathbf{X}_{it} + \delta \mathbf{Z}_{it} + \epsilon_{ikt} \quad (2)$$

y_{ikt} is a measure of labor supply for mother i living in municipality k in year t . CC_{kt}^{12} , the child care coverage rate for toddlers in municipality k in year t , is the main explanatory variable of interest. u_{kt}^m is the unemployment rate for men in municipality k in year t , and is included to control for local labor market conditions. \mathbf{X}_{it} is a vector of individual control variables containing information on the mother and the father of the youngest child of mother i , while \mathbf{Z}_{it} is a vector of child characteristics.

α_k and τ_t captures municipality and time-fixed effects. The reason for using these dummies is that we worry that there is unobserved heterogeneity at the municipality level,

so that women in some municipalities have higher or lower labor supply independently of the coverage rates of child care. By using fixed effects, we explain the changes in labor supply by the changes in child care coverage, and thus get rid of the unobserved heterogeneity as long as the composition of this heterogeneity is constant within the municipality.²¹ In practice, what we do is calculate the deviation from the mean in the municipality for all variables. The time fixed effects τ_t accounts for yearly shocks to labor supply that affect all women the same way.

We also want to control for other variables that could affect changes in labor supply and correlate with child care coverage, thus confounding our estimate of γ . Therefore, we include \mathbf{Z}_{it} and \mathbf{X}_{it} in the regression. In \mathbf{X}_{it} , we include dummies for different levels of education of the mother and the father, a dummy for immigrant status, a cohabitation dummy equal to one if the parents are living together, age and age squared and age and age squared at first birth. In \mathbf{Z}_{it} we include the gender of the child, number of older siblings, dummy variables for the birth month of the child and a dummy if the child is in its last toddler year. For a full overview of the variables used, see table 8 and 9 in the appendix.

When designing our regression model, we must be careful not to include so-called bad controls. These are variables that could themselves be outcome variables. The problem with bad controls is that they introduce a variant of the selection problem: Even if our treatment is initially randomly determined, the introduction of conditioning on bad controls breaks the random assignment and creates selection bias. Bad controls change the composition of the treatment and control groups, and so we no longer compare apples to apples, as Angrist and Pischke (2008) put it.

For this reason, we should not use variables that could be affected by changes in the child care coverage rates as control variables. Among the control variables we use, very few could be suspected of being determined by changes in the child care coverage rate. There is little reason to believe that civil status could be influenced by changes in child care coverage rates, at least not within the same year. The only individual level control variable that can be suspected of being a bad control is education - mothers might be more or less likely to take more education when the changes in child care coverage differ. However, since we only look at changes within the same year and since active students are excluded, this is a minor problem.²²

u_{kt}^m is the unemployment rate for men in the municipality. We include this to control for local labor market conditions that might affect labor supply. The overall unemployment

²¹We also run a robustness analysis with fixed effects at the individual level to test whether the results could be driven by changes in the composition of the unobserved, constant heterogeneity at the individual level.

²²When performing persistency analysis, thus looking at longer time intervals, this becomes a bigger problem. We therefore use 2001 values of the control variables in the persistency analysis to avoid the bad control problem, see section 7.1.

could be suspected of being a bad control since it can be affected by changes in child care coverage. Therefore, we rather use the unemployment among men only to minimize this problem, reducing the spillover effects. Since there are less variation in labor market participation among men than women, the unemployment for men also better reflect conditions in the labor market.

If the changes in coverage rates, conditional on the other control variables, is uncorrelated to the error term ϵ_{ikt} , then the estimate of γ from equation 2 is unbiased. This is the strict exogeneity assumption. For this to hold, changes in child care coverage rates must be exogenous to changes in labor supply. This assumption is violated if there are omitted time-varying variables at the municipality level or omitted variables in general at the individual level that is correlated with both changes in female labor supply and changes in child care coverage rates. Therefore, an analysis of what drives expansion of child care is important to determine whether this variation can be regarded as exogenous. For a preliminary analysis of this exogeneity, see section 6.2. Several possible violations of the assumptions underlying our estimation strategy are discussed and tested for in section 7.2.

Throughout this thesis, we cluster the standard errors at the municipality level. If there is reason to believe that changes in labor supply is correlated within a municipality, the normal standard errors obtained from the fixed effect regression will be too small. Even if the estimate itself is unbiased, using incorrect standard errors can lead to wrong conclusions. There are many reasons to believe that observations within a municipality are correlated, both over time and across women. First, the number of women moving is small, so the population within one municipality over time will to a large degree consist of the same women. If there is serial correlation in changes in a woman's labor supply, there will also be serial correlation in the changes of labor supply within one municipality.

There are also reasons to believe that there is correlation between women within a municipality in the same year. There might be other local labor market conditions that affect (some) women in the municipality the same way, or there might be some structure of the child care system itself that creates correlation between the labor market outcomes of women living in the same municipality. For these reasons, we use clustered standard errors at the municipality level for all regressions. Since there are 430 municipalities in the sample, which are well above the rule-of-thumb level of 50 clusters for clustered standard errors to be consistent.

6.1 The rationing of the child care market

In a non-rationed market, the number of children in child care can change due to changes in both supply and demand. This means that observed correlation between changes in child care coverage and changes in labor supply could be a result of reverse causality:

Table 3: Rationing index

| Year | Mean | SD | Rationed | Non-rationed | Rationed share |
|-------|-------|------|----------|--------------|----------------|
| 2001 | 1.730 | .690 | 391 | 39 | 90.9% |
| 2002 | 1.611 | .666 | 379 | 51 | 88.1% |
| 2003 | 1.476 | .694 | 358 | 72 | 83.3% |
| 2004 | 1.365 | .572 | 354 | 76 | 82.3% |
| 2005 | 1.206 | .359 | 326 | 104 | 75.8% |
| 2006 | 1.062 | .237 | 265 | 165 | 61.2% |
| 2007 | 1.009 | .257 | 203 | 227 | 47.2% |
| 2008 | .967 | .150 | 148 | 282 | 34.4% |
| 2009 | .938 | .166 | 122 | 308 | 28.4% |
| Total | 1.263 | .550 | 2,546 | 1,324 | 65.8% |

That changes in female labor supply lead to increased observed child care coverage. In a rationed market, however, the observed child care coverage rates will not change due to changes in demand until the rationing is lifted.²³ The rationing of the formal market is thus an important assumption supporting our estimation strategy and reducing problems of reverse causality. This section further investigates this rationing assumption.

Reports from 2006 and 2008 (Asplan Viak, 2006, 2008a) find that an average of 6.2% and 3.7% of children in preschool age was on a waiting list for child care on September 20th these two years. There was more or less full coverage for older children in this period, so that the waiting list consists primarily of toddlers, making the real rationing for toddlers a lot more severe. The average time on a waiting list was 3.7 weeks in 2008. No such number is available for 2006, but since the general availability was lower, we can assume it was at least as long. The 2006 report also cover some numbers that might indicate increases in demand for child care following the max price reform, but as long as child care is rationed, this should not affect the observed child care coverage rates.

To further investigate the rationing of child care, we construct a rationing index for each municipality. Using the 2 BA definition labor force participation definition,²⁴ we compare the share of mothers of toddlers that work to the child care coverage rates. We use the baseline sample of mothers.²⁵ The larger this rationing index, the more severe is the rationing. We consider a municipality rationed if the index is above 1. This index can

²³Of course, we could think that municipalities or private child care institutions might respond to increases in demand for child care by constructing more child care institutions, thereby creating a channel for lagged reverse causality. As construction of new child care institutions takes a long time and we only investigate changes in child care coverage and labor supply within the same year, this is a minor problem.

²⁴See section 5 for a description of these definitions.

²⁵Because there might be demand for child care also for mothers that are not part of our sample, for example active students and mothers with another, younger child, the real rationing may be bigger than what reflected in the rationing index.

be interpreted as the number of working mothers per child in child care. Table 3 presents the mean and standard deviation of the rationing index for each year, and also the number of municipalities that are rationed and non-rationed according to this definition. We see that for most of the period, a large majority of the municipalities were indeed rationed according to this definition.

Based on this rationing index and the reports reviewed above, we find support for the rationing assumption. Changes in observed child care seem to be driven primarily by changes in supply, not demand. This reduces problems of reverse causality and simplifies the analysis of the exogeneity of the child care coverage: We need not fear unobserved variables that correlate with changes in female labor supply and changes in demand for child care.

6.2 Testing the exogeneity of the child care expansion

In most empirical research, the laboratory experiment is the gold standard to investigate causality. In a lab, you randomly determine who get the treatment, and so ensure that the treatment and control groups are similar except from treatment status. In empirical social science, a laboratory setting or random allocation of treatment is rare. In that case, drawing conclusions on causality is more complicated.

In our case, the treatment is a continuous measure of child care coverage. Expanding child care is of course a conscious choice among private firms or municipalities, and not randomly determined, but we will in this section investigate whether the changes in child care coverage can be regarded as random and exogenous to the changes in female labor supply. An analysis of what drives child care expansion can shed light on this question.

If determinants of the expansion are systematically related to underlying trends in maternal employment, we may be worried about differences in the characteristics of early and late expanders. It is useful, therefore, to understand the determinants of the expansion across municipalities. To further test the exogeneity of the child care expansion, we use a fixed effects regression to explain the changes in child care coverage. This way, we can see if there are municipality characteristics that seem to determine what municipalities expand early. To check this, run

$$CC_{kt}^{12} = \lambda_k + \pi_t + \sum_{m=1}^M \varphi_{mt} V_{m,k,2001} + e_{kt} \quad (3)$$

Where λ_k and π_t are year and municipality dummies, $V_{m,k,2001}$ is characteristic m in municipality k in 2001 and φ_{mt} is the effect of this characteristic on child care coverage in year t . This relates the changes in child care coverage to municipality characteristics. We use municipality characteristics from before the expansion to ensure that the characteristics are not themselves results of the reform. This way, we can investigate whether

some municipality characteristics were associated with earlier or later expansion.

In the m municipality characteristics we investigate, we include economic, political and geographical municipality characteristics as well as labor market indicators that we suspect can explain child care expansion. All characteristics are measured in 2001, before the reform. The labor market indicators consist of initial female labor force participation,²⁶ registered unemployment among women and the initial child care coverage rate for toddlers.

The economic indicators are the non-earmarked income per capita in the municipal accounts and a hydropower dummy indicating whether the municipality had any extraordinary tax income from hydropower between 1992 and 2001. This sort of income is not earmarked, and so increases the possibility of prioritizing child care expansion. Both these are included to see whether there are systematic differences in expansion between municipalities with a lot of financial freedom and those with tight budgets.

In the political indicators we include the share of female representatives in the municipal council and the share of representatives from left-wing parties. These are included to see whether political priorities or gender can affect child care expansion. Last, we include a measure of urbanity: The share of the population living in urban areas.²⁷

Finding significant differences in expansion rates based on these characteristics isn't necessarily a problem - only if we also believe that these characteristics may be correlated with changes in female labor supply. This is a preliminary examination of the common trend assumption underlying our method.

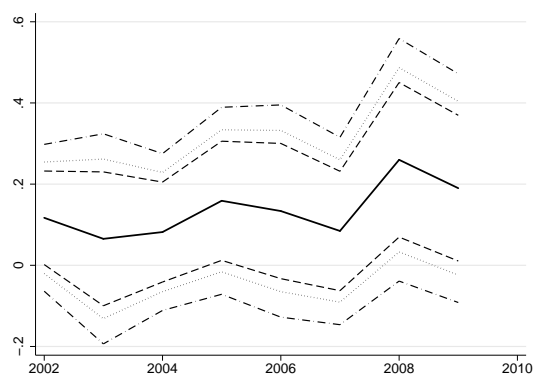
The results from this exercise are depicted in figure 10. The φ_{mt} coefficients of are drawn for each year for each of the characteristics, as well as confidence intervals using 1%, 5% and 10% significance levels. As expected, we see that the initial child care coverage rate is significantly and negatively associated with expansion in child care. The municipalities with high initial coverage expand less simply because there is less to go, and expand even less later in the period. Initial female labor participation seems to be positively related to expansion, especially in the later years, but only borderline significant, while initial female unemployment have little explanatory power over the pace of expansion.

Neither the political characteristics nor the urbanity index yield any significant results. No significant differences in the expansion rates among municipalities with different political representation reinforce the view that the reform had broad bipartisan support.

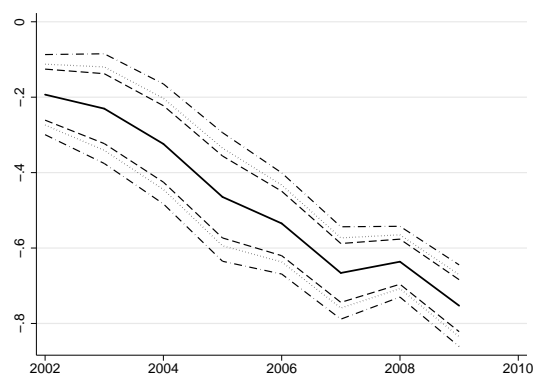
Last, we find little effect from the economic indicators. It seems that initial differences in municipality revenues do not affect child care expansion rates, probably because these differences are largely offset by ear-marked grants from the central government and because tax rates and tax bases are centrally determined.

²⁶This is measured as the share of registered female employees to the total female workforce aged 16 to 67.

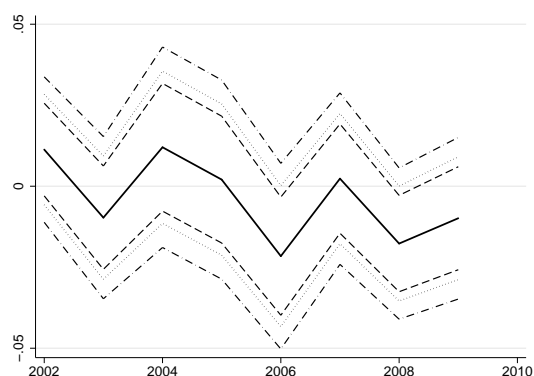
²⁷We have also performed the same exercise using the share of employees in the primary, secondary and tertiary sector as well as overall unemployment rates, without this yielding any significant results.



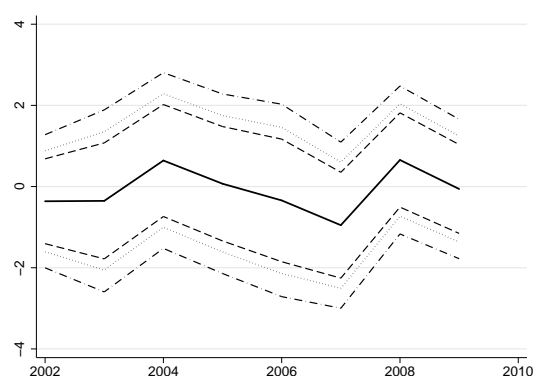
(a) Initial FLP



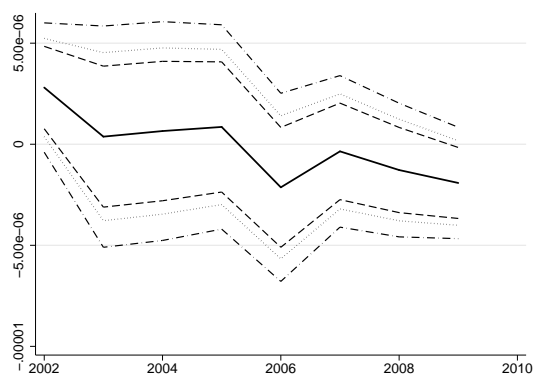
(b) Initial toddler care coverage



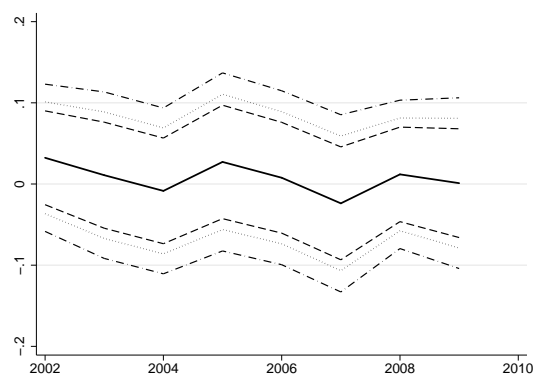
(c) Hydropower income dummy



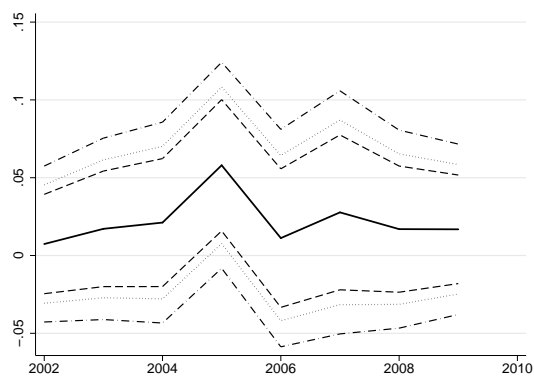
(d) Initial unemployment among women



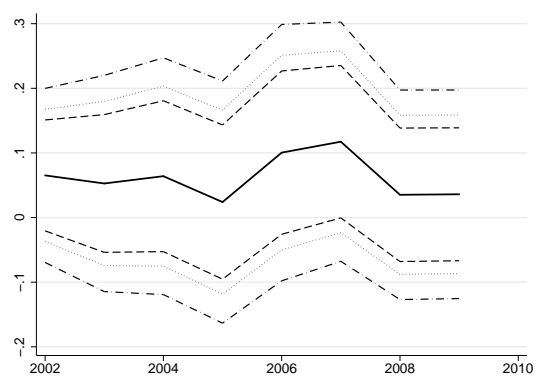
(e) Free income per capita



(f) Socialist share in municipal council



(g) Urban population share



(h) Female share in municipal council

Figure 10: Testing exogeneity of the child care expansion

Overall, there is little reason to worry when performing this exercise. Among the characteristics investigated, none except the initial child care coverage itself has any explanatory power over the pace of expansion. This strengthens our belief that the child care expansion can indeed be considered exogenous. Further robustness checks to test the exogeneity of the expansion will be performed in section 7.2.

7 Results

This section presents results based on equation 2. The main outcome variable is the 2 basic amounts (BA) labor supply definition, which we will interpret as labor force participation. When only one results of a regression is reported in the text, this is the 2 BA outcome. The 4 BA threshold constitutes full-time employment. Only the coefficient for the child care coverage rate is reported, not the control variables, but the controls are always included in the regression.

The main results are presented in table 4, the baseline estimate of γ in equation 2 in row 1. We find that a 1 percentage point increase in municipal child care coverage rate for toddlers is associated with a .11 percentage point increase in women working at least enough to earn two basic amounts. Put another way, going from no to full coverage will yield an 11 percentage point increase in female labor force participation. This constitutes an increase of 16.4% relative to the mean labor supply. The effect on full-time employment is lower, at around .05, indicating a 5 percentage points, or 13%, increase in full-time employment from a full-scale expansion of child care. These effects are significantly different from zero at the 0.1% level.

The baseline regression has also been evaluated for a range of other thresholds of the outcome variable. Figure 11 presents the estimates for γ using all the percentiles of the income distribution as cutoff threshold. From this figure, we see that the coefficient is positive and significantly different from zero up until around the 70th percentile, after which it eventually drops to be negative, but never significantly different from zero. This indicates that child care coverage does not affect the probability of having top wages.²⁸

There might be differences in the response to a child care reform from different women. If so, the baseline results will constitute a mean of these effects. We therefore want to evaluate the effect in different subsamples to look at heterogeneity in the labor supply response.²⁹ These results are reported in rows 2 to 7 in table 4.

The results for married and non-married mothers (rows 2 and 3) are fairly similar to the baseline regression, maybe with a slightly bigger response to child care coverage

²⁸We have also investigated equation 2 using linear earnings as the outcome variable, finding results very close to and not significantly different from zero. This indicates that there is no effect on earnings of increased child care coverage.

²⁹In addition to the subsamples presented here, a series of municipal level subsamples are reported in table 7 in the appendix. In general, these results are close to the baseline estimate.

Table 4: Main results

| Sample | 2 BA | 4 BA | N |
|----------------------------|---------------------------------------|---------------------------------------|---------|
| | Participation | Full time | |
| (1) Baseline | 0.1096*** (0.0105) <u>0.666</u> | 0.0508*** (0.0102) <u>0.378</u> | 746,941 |
| (2) Married | 0.1209*** (0.0125) <u>0.661</u> | 0.0771*** (0.0130) <u>0.391</u> | 411,959 |
| (3) Non-married | 0.1004*** (0.0152) <u>0.672</u> | 0.0291** (0.0137) <u>0.361</u> | 334,982 |
| (4) High education only | 0.0531*** (0.0136) <u>0.861</u> | 0.0709*** (0.0205) <u>0.622</u> | 304,015 |
| (5) Low education only | 0.1351*** (0.0148) <u>0.533</u> | 0.0498*** (0.0110) <u>0.210</u> | 442,926 |
| (6) Mothers of 1-year olds | 0.1176*** (0.0140) <u>0.647</u> | 0.0257* (0.0173) <u>0.347</u> | 390,909 |
| (7) Mothers of 2-year olds | 0.1020*** (0.0127) <u>0.683</u> | 0.0792*** (0.0164) <u>0.409</u> | 324,058 |
| (8) Fathers | -0.0274 (0.0180) <u>0.906</u> | -0.0389* (0.0170) <u>0.830</u> | 780,964 |

Fixed effects regression estimates of γ in equation (2), the effect of child care for toddlers on maternal labor supply, measured by earnings above 2 basic amounts (part-time employment) and 4 basic amounts (full-time employment) of the welfare system. Full sample and subsamples.

Standard errors in parentheses, clustered at the municipality level. Mean of dependent variable underlined.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

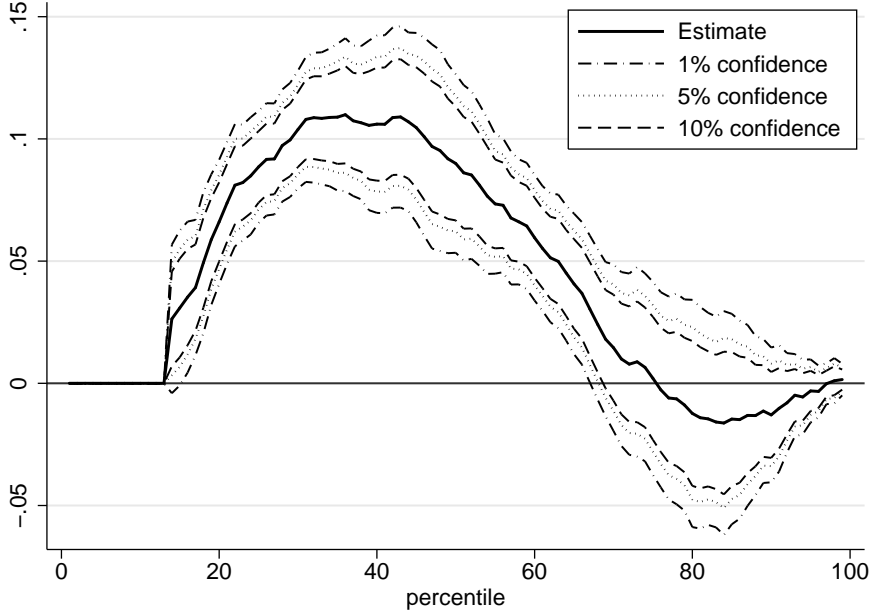


Figure 11: Baseline regression on the percentiles of the income distribution

among married mothers, especially when examining full-time employment. This might indicate that married mothers are more responsive to child care availability in their labor supply, maybe because they to a larger extent pool income with their husbands and thus have the possibility of opting out of the labor market and earn less for a period.

For the education subsamples, we find large differences between mothers with high and low education.³⁰ Mothers with low education seem to respond more strongly when we consider participation, while mothers with high education seem to respond more strongly when considering full-time employment. An important reason for this finding is that the potential wages for these women differ: If a well-educated woman chooses to work, she will more often earn wages above the 4 BA threshold, while the potential wage of the low-educated mother may lie between the two thresholds.

Even when taking this into account, the differences in response on the participation and full-time equivalents margins differ substantially between mothers of low and high education. This might indicate that low educated mothers respond to increases in child care coverage by participating in the labor market, while high educated mothers rather respond to differences in coverage on the intensive margin by adjusting working hours. The fact that the response on the full time equivalents are larger than the participation response for high educated mothers might indicate that at least parts of the adjustment takes place on the intensive margin. At least some mothers are induced to increase their labor supply so that earnings increase from between the two thresholds to above the 4 BA threshold.

³⁰Mothers are considered well educated if they have education beyond high school.

We can think of several reasons why the response differs like this. One is that the cost of dropping out of the labor market completely is higher for mothers with higher wages. High education mothers probably also work in occupations with higher human capital intensity than low education mothers, and thus the cost of human capital depreciation from opting out of the labor market might be more severe. Last, well-educated mothers may have jobs characterized by more flexibility in working hours.

We also investigate the results for mothers of one year olds and mothers of two year olds separately in rows 6 and 7. We find little difference from the baseline results on the part-time labor supply for both samples, but we do find differences on the full-time equivalent labor supply indicator: Among mothers of one year olds, the response is lower than the baseline result and only significantly different from zero at the 5% level. Among mothers of two year olds, the coefficient is .792, around 50% larger than the baseline result. This might indicate that mothers to a larger degree respond to the differences in child care coverage by starting or postponing full-time employment in the last toddler year.

Throughout this thesis, we have focused on mothers as they are traditionally the primary caretakers. We are however also interested in whether there is a response in the labor supply of fathers to a child care expansion. We therefore run the baseline regression for fathers. We use the unemployment for women to avoid spillover effects, but except from that, the regression is similar to equation 2. The results are reported in column 8, where we see that the results on both participation and full time employment are *negative*. The estimates are only significantly different from zero at the 5% level for full-time employment and borderline to be significant at the 10% level for part-time employment.³¹ Still, this indicates that there is some reverse response among fathers: They tend to decrease labor participation as child care increases, possibly to support the mother financially when she is not working to take care of the children when child care is unavailable. This is an indication that mothers are still the primary caretakers when it comes to young children at home. The fact that the coefficient are not only non-positive, but negative, might also indicate that child care can lead to more balance in taking care of children, both when in household and market work.

To sum up, we have found clear evidence of response to the child care availability among mothers of small children. The response varies somewhat between the different subsamples considered, especially between low- and high-educated women. There is slight evidence for opposite response among fathers, although these results are imprecise.

³¹p-values are 0.023 for full-time employment and 0.128 for part-time employment using two-sided tests.

7.1 Persistency analysis

If the effects on labor supply persist also after the toddler years, this will increase the cost efficiency of the reform. To investigate this, start out with the mothers of the baseline sample. We look at their labor supply one to three years after the toddler period, excluding women who have another child or study, as before.

As discussed in the empirical strategy, we must be cautious not to include bad controls when designing a regression model. When performing a persistency analysis like this, there will be a longer time interval between the explanatory and the dependent variable, and so there is more time for the changes in coverage rates to affect the control variables and create a bad control problem. We could fear for example that low child care expansion rates could induce women to take more education in the coming years, before we measure the persistency in labor supply. To minimize this problem, we use all time varying covariates from 2001, before the expansion.

We can then run the baseline regression, using the mean of the child care coverage rate in the toddler years as the variable of interest. Because the mothers are now subject to the child care for three to five year olds, we also control for the child care coverage rate for older children. Run the regression

$$y_{ik,t+s} = \alpha_k + \tau_{t+s} + \gamma CC_{k,t}^{12} + \eta CC_{k,t+s}^{s+2} + \delta u_{k,2001}^m + \beta \mathbf{X}_{i,2001} + \delta \mathbf{Z}_{i,2001} + \epsilon_{ikt} \quad (4)$$

where $s \in 1, 2, 3$ is the number of years after the toddler period, $CC_{k,t}^{12}$ is the mean of the coverage rates during the toddler years and $CC_{k,t+s}^{s+2}$ is the current coverage rate for children the age of the child. If we find significant estimates for γ , this is sign of persistency in the labor supply response.

The results from this exercise are reported in table 5. We start by repeating the separate estimates for mothers of 1- and 2-year olds from the main results. We see that the effect on participation is relatively strong both toddler years, while the response on the full-time equivalent margin is larger the second toddler year, indicating that mothers respond by starting or postponing full-time employment. In the three following rows, we report the estimates of the child care coverage during the toddler period on the labor supply of the mother when the child is 3, 4 and 5 years old. We see that the effect on participation drops dramatically and is not significant. Since employment for mothers of 3- to 5-year olds is very common in Norway, this indicates that most of these mothers work these years, regardless of the coverage rates during the toddler years. There is no sign of persistency effects in labor force participation.

For full time employment, however, we do find relatively strong signs of persistency. The coefficients of the mean child care coverage during the toddler period on the labor supply one to three years later barely drops, and are significantly different from zero at

Table 5: Persistency analysis

| Child age | 2 BA Participation | 4 BA Full time | N |
|-----------|-----------------------|-----------------------|---------|
| 1 | 0.1176*** (0.0140) | 0.0257* (0.0173) | 390,909 |
| 2 | 0.1020*** (0.0127) | 0.0792*** (0.0164) | 324,058 |
| 3 | 0.0363 (0.0203) | 0.0754** (0.0230) | 206,456 |
| 4 | 0.0222 (0.0220) | 0.0714** (0.0250) | 156,189 |
| 5 | 0.0306 (0.0268) | 0.0656** (0.0247) | 123,159 |

Fixed effects regression of equation 4. Effects of the mean of child care coverage during the toddler years 1-3 years later, controlled for the child care coverage rates of older children. Initially the baseline sample, excluding students and mothers who have another child.

Standard errors in parentheses, clustered at the municipality level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

the 1% level. This indicates that a 1 percentage point higher average coverage leads to around 0.08 percentage points higher full-time labor supply one to three years later among these mothers. There are several possible reasons for these findings: Staying in employment during the toddler years might increase the chances of getting a pay raise, promotion or full-time position later. These are important findings when discussing the costs and benefits of child care expansion.

7.2 Robustness analysis

In this section, we want to check the robustness of our previous results. To do this, we perform several different specification and robustness tests. The results from these tests are reported in table 6, showing only the coefficient for the child care coverage rate, although the control variables are always included in the regression.

In the baseline specification we used fixed effects at the municipality level. These also take account of individual fixed heterogeneity in labor supply as long as the composition of

Table 6: Robustness analyses

| | FE level | Sample / check | 2 BA Participation | 4 BA Full time | N |
|------|--------------|--|-----------------------|-----------------------|---------|
| (1) | Municipality | Baseline | 0.1096*** (0.0105) | 0.0508*** (0.0102) | 746,941 |
| (2) | Individual | Baseline | 0.0741*** (0.0166) | 0.0473*** (0.0122) | 746,941 |
| (3) | Individual | First child in sample only | 0.0778*** (0.0279) | 0.0298*** (0.0126) | 381,858 |
| (4) | Municipality | Excluding 4 urban municipalities | 0.1067*** (0.0120) | 0.0493*** (0.0123) | 562,042 |
| (5) | Municipality | Excluding 11 urban municipalities | 0.1068*** (0.0123) | 0.0501*** (0.0128) | 492,598 |
| (6) | Municipality | Using municipality of residence in 2001 | 0.0873*** (0.0093) | 0.0376*** (0.0093) | 708,808 |
| (7) | Municipality | Municipality changers dropped | 0.0927*** (0.0113) | 0.0310*** (0.0122) | 493,650 |
| (8) | Municipality | Municipality specific trends (eq. 5) | 0.0841*** (0.0139) | 0.0436*** (0.0136) | 746,941 |
| (9) | Municipality | Characteristic specific time shocks (eq. 6) | 0.0833*** (0.0127) | 0.0564*** (0.0120) | 746,208 |
| (10) | Municipality | Placebo control (eq. 7): Mothers of 3-year olds | 0.0256 (0.0179) | -0.0179 (0.0190) | 199,372 |
| (11) | Municipality | Placebo control (eq. 7): Mothers of 4-year olds | 0.0136 (0.0224) | -0.00988 (0.0224) | 151,405 |

Results from various robustness checks described in the text. Coefficient of child care coverage for toddlers reported, regressions include standard set of regressors from equation 2. Labor supply measured by participation (earnings > 2 basic amounts) and full-time equivalents (earnings > 4 basic amounts).

Standard errors in parentheses, clustered at the municipality level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

these individual fixed effects in the municipality is unchanged. To test whether this holds, we then restrict ourselves to considering only changes in labor supply among the same mothers over time, controlling for all time invariant characteristics using fixed effects at the individual level. Of course, this leaves out the time invariant covariates, as the effect of these covariates will be picked up by the individual fixed effect. If the coefficient is stable to this specification and doesn't change too much relative to the baseline regression, this is evidence that the effects are not driven by changes in the composition of these individual time invariant characteristics. As can be seen from row 2 of table 6, they are relatively stable. The coefficients are slightly lower than the baseline estimates, but relatively close and still significant at the 0.1% level. This is evidence that the effects we have estimated so far are not primarily driven by changes in the composition of such unobserved constant characteristics.

If there are unobserved, time varying covariates that confound our estimates, we could think that these covariates change more over longer time periods. Therefore, we perform another robustness check using individual fixed effects on a subsample of the original sample. Here, we include only the observations from a mother's first child in the sample, and thus we relate the changes in a mother's labor supply when the child goes from 1 to 2 to the changes in the child coverage rate. This leaves less room for changes in time-varying covariates since we only look at the variation within two subsequent years. As can be seen from row 3, the estimates are relatively stable to this specification, supporting our empirical strategy.

Next, we want to investigate whether our estimates could be driven primarily by the response among urban or rural women. We therefore estimate the baseline regression on a subsample excluding all women residing in the 4 or 11 largest urban municipalities.³² As can be seen from rows 4 and 5, the estimates are extremely stable to this exclusion and the effects are almost identical to the baseline estimates. In a similar robustness test, we can investigate subsamples based on a range of different municipality characteristics before the expansion to see whether the effects differ between municipality types. The results of this exercise is reported in table 7 in the appendix. In general, we find very little differences in response to child care coverage among different municipality types, supporting our empirical approach.

Because our data is non-experimental, the composition of the treatment and control groups are not random, and we need to tackle the possibility of self-selection into or out of treatment. In our case, self-selection problems may arise due to selective migration: If mothers move to municipalities with large or small changes in coverage rates, our estimates might be biased. For a thorough treatment of self-selection in non-experimental data, see for example Blundell and Dias (2000). If mothers who will increase their labor supply

³²These are the cities of Oslo, Bergen, Trondheim and Stavanger with more than 100,000 inhabitants and Fredrikstad, Tromsø, Sandnes, Drammen, Sarpsborg and Skien with more than 50,000.

move to municipalities with high expansion rates, this will show up in our estimates as an effect of child care on labor supply, when in reality it is just systematic sorting of work prone mothers to high expansion municipalities. The selection might also work the other way: If work prone mothers move to municipalities with high initial coverage, we already know from section 6.2 that these municipalities expand less. This will dampen the observed effect of child care on labor supply.

To investigate whether our results are driven by selective migration, we run two specification checks. First, we use each mother’s municipality of residence in 2001 to determine what child care coverage rate she will be subject to throughout the period. This way, women who move to another municipality will be regarded as if they did not. The results of this exercise are presented in row 6, and we see that the estimates are slightly lower, but still definitely significant at the 0.1% level.

Next, we simply drop all women who change municipality of residence some time during the period. This amounts to roughly 250,000 observations. From row 7 we see that the estimates are stable to this exclusion as well, lending support to the idea that the baseline results are not primarily driven by self-selection into high- or low expansion municipalities.

Underlying our fixed effects method is an assumption that is very similar to the common trend assumption of Difference in Differences setups: Conditional on the control variables and with no changes in child care coverage, the municipalities would have followed the same trend in labor supply. To investigate whether our results are driven by differences in underlying trends, we perform two specification checks. First we run a variant of the baseline regression where we include municipality-specific linear time trends. This allows for differing trends in labor supply between different municipalities, and thus weakens the common trend assumption. We run

$$y_{ikt} = \alpha_k + \tau_t + \gamma CC_{kt}^{12} + \theta_k t + \delta u_{kt}^m + \beta \mathbf{X}_{it} + \delta \mathbf{Z}_{it} + \epsilon_{ikt} \quad (5)$$

Where θ_k is the parameter of the linear time trend for municipality k and t is the year number. This exercise is similar to testing the common trend assumption in a Difference-in-Difference setup: We estimate the effects of child care only on the deviations from the municipality-specific trend in labor supply. If the estimates of γ are stable compared to the baseline estimate, this is evidence that the results we have found so far are not driven by these omitted linear trends. As can be seen from row 8 in table 6, the estimates are very robust to this test, and the results are not driven by differences in (linear) trends in labor supply between municipalities.

Another way to weaken the common trend assumption is to allow for municipality specific time shocks. However, since our identification comes from the within-municipality variation in coverage rates, we could no longer identify the effect of the coverage rate if

we included completely flexible shocks to each municipality each year. Instead, we might think that there are some characteristics of the municipalities that determine how they are affected by the yearly shocks. These could be different economic or political structures that make the municipalities react differently to the shocks. To test for this, we can add a flexible time shock interacted with a set of municipality indicators from 2001:

$$y_{ikt} = \alpha_k + \tau_t + \gamma CC_{kt}^{12} + \sum_{m=1}^M \varphi_{mt} V_{m,k,2001} + \delta u_{kt}^m + \beta \mathbf{X}_{it} + \delta \mathbf{Z}_{it} + \epsilon_{ikt} \quad (6)$$

Where φ_{mt} is the shock connected to characteristic m in year t . The impact of this shock for municipality m is propagated through $V_{m,k,2001}$, the value of characteristic m for municipality k in 2001. The total shock to labor supply for a given municipality in a given year will thus be given by the overall shock τ_t plus the sum of the shocks through the M characteristics, $\sum_{m=1}^M \varphi_{mt} V_{m,k,2001}$.

This approach is very similar to the initial exogeneity test performed in section 6.2, and we use the same 8 economic, political and geographical characteristics as in the previous section. Again, stable estimates compared to the baseline regression are signs of robustness for our empirical strategy. Row 9 reports the results from this exercise, and again we see that the estimates are very stable, supporting our strategy.

Before summing up the results section, we want to evaluate the total robustness of the estimate using a placebo analysis. A placebo analysis is a regression designed to be as similar to the baseline regression as possible, but where the outcome variable cannot be affected by the explanatory variable. If we nonetheless find significant effects of our explanatory variable on this placebo outcome, this might be a sign of model misspecification that casts doubt on the estimation strategy.

As placebo outcome, we choose the labor supply of mothers of 3- and 4-year olds.³³ These mothers should not be affected by the child care coverage rate for toddlers, as their youngest child is at least three years old. For mothers of 3-year olds, we run:

$$y_{ikt} = \alpha_k + \tau_t + \gamma CC_{kt}^{12} + \kappa CC_{kt}^3 + \eta CC_{k,t-1}^{12} + \delta u_{k,2001}^m + \beta \mathbf{X}_{i,2001} + \delta \mathbf{Z}_{i,2001} + \epsilon_{ikt} \quad (7)$$

Since these children are subject to the child care for 3-year olds, we include the coverage rate for these children, CC_{kt}^3 , as a control variable. We also know from our previous analysis that there are persistency effects of toddler care, so we also include $CC_{k,t-1}^{12}$, the mean of the child care coverage rates during the toddler years. We exclude mothers who have another child or study. As in the persistency analysis, we use covariates from 2001 to minimize the bad control problem. The regression equation for mothers of four year

³³We use the same “efficient age” definition as earlier, where children born later than August 31st are considered one year younger than they actually are.

olds is equivalent.

Again, the parameter of interest is γ . If we find significant effects of the current toddler coverage rate on the labor supply of older mothers, conditional on the coverage rate for older children and the coverage rate during the toddler period, we would worry about some sort of model misspecification confounding our estimates.

The results of this exercise are reported in row 10 and 11 in table 6. We find effects that are much smaller than the baseline estimates for both placebo outcomes. The coefficients are not nearly significant at any conventional level. We find no sign of misspecification of our model from this placebo test.

To sum up, the battery of robustness tests and placebo controls performed in this section support the empirical strategy and strengthen our belief that we have found causal effects of child care coverage on female labor supply.

8 Conclusion

Child care expansion is often considered an important policy tool for reconciling family and work responsibilities and increasing female labor supply. Economic theory finds support for a positive relationship between the availability of universal, subsidized child care and maternal labor supply, but also predicts that a child care reform will crowd out informal care. The efficiency of child care expansion as a tool to increase female labor supply is therefore an empirical question.

Although there are readily available empirical research on the efficiency of subsidized child care on female labor supply, most of this research concern preschoolers. For younger children, such research is scarce, and there are reasons to believe that the labor supply response of mothers of toddlers may differ from that of mothers of older children.

A Norwegian child care reform from 2003 that led to large increases in coverage rates for toddlers provide a natural way to examine the efficiency of a child care expansion for mothers of these children. The Norwegian market for child care was characterized by rationing during the period under study, something that is important for our estimation strategy. Under rationing, changes in demand for child care will not affect the observed coverage rates until the rationing is lifted. The rationing in the market therefore reduces problems of reverse causality.

Using a robust fixed effects method, this thesis investigate the efficiency of child care for toddlers on mothers' labor supply, finding significant effects on both labor force participation and full time equivalent labor supply. We estimate that a full-scale expansion³⁴ of child care for toddlers increases the labor participation of these mothers by around 11 percentage point, or more than 16%. For full time employment, the effects are smaller at around 5 percentage points, constituting a 13% increase. These effects are significantly

³⁴Going from zero to 100% child care coverage.

different from zero at any conventional significance level. A battery of specification and robustness checks support our baseline fixed effects method, supporting of our empirical approach to finding the causal effect of child care.

This thesis also finds some evidence of persistency in the labor supply response, especially when considering full-time employment. This suggest that mothers who work more due to the higher availability of formal care, also work more in the subsequent years, possibly due to serial correlation in employment or less human capital depreciation. This is important when considering the efficiency of the reform

Last, we find slight evidence of a reverse response among fathers. Fathers of young children seem to reduce their labor supply when child care coverage increases, possibly because the mothers are now working more. This might indicate that child care lead to better balance in the responsibility for taking care of small children between mothers and fathers, both in market and household work.

These effects are significantly larger than other estimates from the literature, particularly Havnes and Mogstad (2011) who find an effect almost half the size from a very similar reform for older children. There are several possible reasons for this finding. Preferences for child care may have changed over the three decades between the reforms, and preferences may also differ between mothers of toddlers and preschoolers. Second the price of informal care from relatives and friends may have changed. Probably most importantly, however, the alternative mode of care may be different for toddlers and preschoolers. The findings in this thesis suggest that the alternative mode of care for toddlers when formal child care is not available is to a larger extent parental care than informal care. If this is the case, we will observe larger response to child care expansion for toddlers than preschoolers.

Our results have implications for contemporary political debates on family policies, welfare reform and gender equality. Even if the estimates are larger than previously found in the literature, the economic implication is that it takes around 9 slots in child care to induce one more mother enter the labor market. An important goal of the child care expansion and the family policies in Norway has been to further gender equality in the labor market. If the child care reform was enacted only to obtain this goal, the expansion would have to be regarded as relatively inefficient, given that the price for 9 slots in child care is very costly to the government in terms of subsidies. Although not the purpose of this thesis, a cost-benefit analysis of the child care reform would probably find that the cost of the slight increase in female labor participation is relatively high, and not nearly covered by the increased tax income from the increases in labor supply.

All in all, the results from this thesis suggest that expansion of child care for toddlers is a relatively cost-inefficient tool for increasing female labor participation. As Havnes and Mogstad (2012) find, however, universal child care also strengthens child development, increases labor market attachment, reduces welfare dependency and increases education

and earnings in the long run. If these effects also hold for toddlers, the results from this thesis suggest that the proponents of universal child care should base their arguments primarily on the effects on child development rather than female labor supply.

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Table 7: Subsample analysis: Municipality indicators in 2001

| Municipality characteristic | 2 BA | | 4 BA | | N | |
|---------------------------------------|---------------------------------------|---------------------------------------|---------------------------------------|---------------------------------------|---------|---------|
| | Big | Small | Big | Small | Big | Small |
| Primary share | 0.1233*** (0.0221) <u>0.638</u> | 0.1030*** (0.0126) <u>0.671</u> | 0.0448*** (0.0176) <u>0.270</u> | 0.0519*** (0.0114) <u>0.396</u> | 106,248 | 640,693 |
| Secondary share | 0.1166*** (0.0177) <u>0.635</u> | 0.0899*** (0.0118) <u>0.684</u> | 0.0715*** (0.0174) <u>0.303</u> | 0.0472*** (0.0134) <u>0.421</u> | 271,811 | 475,130 |
| Tertiary share | 0.0997*** (0.0132) <u>0.673</u> | 0.1327*** (0.0212) <u>0.625</u> | 0.0495*** (0.0111) <u>0.396</u> | 0.0493*** (0.0196) <u>0.265</u> | 643,127 | 103,814 |
| Share of female representatives | 0.1082*** (0.0119) <u>0.678</u> | 0.1025*** (0.0196) <u>0.634</u> | 0.0607*** (0.0114) <u>0.409</u> | 0.0421** (0.0182) <u>0.294</u> | 546,966 | 199,975 |
| Hydropower tax income | 0.0770*** (0.0184) <u>0.673</u> | 0.1281*** (0.0127) <u>0.662</u> | 0.0731*** (0.0189) <u>0.354</u> | 0.0437*** (0.0105) <u>0.390</u> | 264,205 | 482,736 |
| Female labor force participation | 0.1050*** (0.0123) <u>0.682</u> | 0.1236*** (0.0233) <u>0.620</u> | 0.0621*** (0.0106) <u>0.408</u> | 0.0301* (0.0202) <u>0.288</u> | 556,306 | 190,635 |
| Unemployment | 0.1189*** (0.0138) <u>0.651</u> | 0.0989*** (0.0153) <u>0.695</u> | 0.0536*** (0.0124) <u>0.369</u> | 0.0484*** (0.0176) <u>0.394</u> | 492,483 | 254,458 |
| Non-earmarked income | 0.1520*** (0.0224) <u>0.665</u> | 0.0981*** (0.0147) <u>0.667</u> | 0.0261 (0.0205) <u>0.416</u> | 0.0523*** (0.0147) <u>0.362</u> | 218,166 | 528,775 |
| Share of socialist representatives | 0.0854*** (0.0320) <u>0.694</u> | 0.1161*** (0.0121) <u>0.662</u> | 0.0477* (0.0300) <u>0.374</u> | 0.0527*** (0.0105) <u>0.378</u> | 105,115 | 641,826 |

Fixed effects regression estimates of γ in (2), in subsamples based on municipality characteristics in 2001. All characteristics are split at the median (among the municipalities), and regressions are performed separately for mothers in the municipalities above and below the median. The exception is socialist majority, where the threshold is a majority of the seats in the municipal council, and hydropower tax income, where the municipalities are split between those who had any hydropower tax income and those who did not. Municipality-level fixed effects throughout.

Standard errors in parentheses, clustered at the municipality level. Mean of dependent variable underlined. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8: Individual variables used

| Variable | Description | Source |
|----------------------|--|----------------------|
| snr_idnr | Individual identifier | Frisch centre |
| year | Year | |
| wyrkinnt | Taxable earnings including self-employment | Tax records |
| komnr | Municipality number | |
| sivil | Civil status, categorized | Demography registers |
| faar | Year of birth | Birth records |
| educ | Highest attained education, NUS2000-coded | Education registers |
| g | Value of 1 Basic Amount | regjeringen.no |
| immigrant | Immigrant status dummy | Demography registers |
| eldresosken | Number of older siblings | Birth records |
| educ_otherparent | Highest attained education of the father | Education registers |
| bmnth_current | Birth month | Birth records |
| oldchild | Dummy if the child is in it's last toddler year. | Constructed |
| iutd | Share of the year registered as a student | Education registers |
| ageyoungestchild | Age of the mothers youngest child | Constructed |
| eff_ageyoungestchild | Age of child, -1 if born in September or later | Constructed |
| otherparent_educmiss | Dummy if education data is missing | Constructed |
| educmissing | Dummy if education of the father is missing | Constructed |
| firstchildinsample | Dummy if child is mother's first in sample | Constructed |
| female_child | A dummy if the youngest child is female | Birth records |

List of individual variables used and their source.

Table 9: Municipality level variables used

| Variable | Description | Source |
|------------------------|---|-------------------------------|
| bhgnox | Number of X-year olds in child care. | Statistikkbanken 04863. |
| hours_private | Age-corrected hours in private child care institutions | Statistikkbanken 04863. |
| hours_public | Age-corrected hours in public child care institutions | Statistikkbanken 04863. |
| investment | Total investment in child care institutions, 1000 NOK | Statistikkbanken 04863. |
| municipal_support_own | Municipal support to own institutions, 1000 NOK | Statistikkbanken 04863. |
| municipal_support_priv | Municipal support to private institutions, 1000 NOK | Staticommonstikkbanken 04863. |
| net_expenses_cc | Net expenses for the child care sector, 1000 NOK | Statistikkbanken 04863. |
| parentalfees | Income from parental fees for child care, 1000 NOK | Statistikkbanken 04863. |
| state_subsidies_priv | Government subsidies to private institutions, 1000 NOK | Statistikkbanken 04863. |
| state_subsidies_public | Government subsidies to public institutions, 1000 NOK | Statistikkbanken 04863. |
| popX | Total population of X-year olds. | Statistikkbanken 07459 |
| workpop_m | Male population between 16 and 67. | Statistikkbanken 07459 |
| workpop_f | Female population between 16 and 67. | Statistikkbanken 07459 |
| pop | Total population. | Statistikkbanken 07460 |
| tett | Population in rural areas. | Statistikkbanken 05212. |
| spredd | Population in urban areas. | Statistikkbanken 05212. |
| institutions | Number of child care institutions. | Statistikkbanken 04863. |
| institutions_municipal | Number of municipal owned child care institutions. | Statistikkbanken 04863. |
| institutions_private | Number of private child care institutions. | Statistikkbanken 04863. |
| institutions_state | Number of state and county owned institutions. | Statistikkbanken 04863. |
| primary | Number of people employed in the primary sector. | Statistikkbanken 03324. |
| secondary | Number of people employed in the secondary sector. | Statistikkbanken 03324. |
| tertiary | Number of people employed in the tertiary sector. | Statistikkbanken 03324. |
| employees_m | Number of employed men. | Statistikkbanken 01610. |
| employees_f | Number of employed women. | Statistikkbanken 01610. |
| u_m | Average number of registered unemployed men. | Statistikkbanken 01603. |
| u_f | Average number of registered unemployed men. | Statistikkbanken 01603. |
| rep_m | Male representatives in the municipal council. | Statistikkbanken 01182. |
| rep_f | Female representatives in the municipal council. | Statistikkbanken 01182. |
| repR | Political representatives from left wing parties. | Statistikkbanken 01182. |
| freincome_abs | Non-earmarked income in the municipal accounts, NOK. | Statistikkbanken 04901. |
| hproptax9201 | Average yearly hydropower tax income in the 1992 to 2001. | The Frisch Centre. |

List of municipal level variables used and their sources.