Do means-tested child care subsidies discourage mum and dad to work?

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Abstract

An important rationale for providing universal child care subsidies is to encourage maternal labor market participation. However, universal subsidy schemes are expensive and may be undesired when subsidies are financed by distortive income taxes. Often governments choose to prioritize means-tested child care subsidies, targeted at low-income families. It is well-known that means-tested transfers discourage the effect of working by high effective marginal tax rates in the interval where transfers are phased out, but this effect is rarely discussed and compared to the assumingly positive effect of child care subsidies on maternal labor marked participation. In the present study we discuss the ambivalent effects on parents' labor supply both analytically and by microsimulations of a structural labor supply model. We then utilize the introduction of a national scheme of means-tested child care subsidies in Norway, which offers a natural experiment to isolate the price effect (positive effect of subsidized care) and the strategic effects (negative effect of means-testing) on parents' labor supply. Surprisingly, we find no significant effect on neither of the two mechanisms. Effects close to zero are confirmed by the structural labor supply model, which helps us shed some light on the likely reasons for the small responses to means-tested child care subsidies in the Norwegian context.

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1. Introduction

Access to affordable and high-quality childcare is viewed by many as a key factor behind the growth in female employment, and it has been argued that differences in childcare policies may explain some of the variation in female employment levels across countries and over time. In both Europe and North America, government support for child care is an issue high on the political agenda; and recently, the Biden administration proposed a plan (the American Families Plan) to subsidize child care in the US. Economists argue that providing subsidized child care is desirable from an optimal taxation perspective both because it promotes equality within the household and because it is efficiency-improving to encourage labor market participation in the presence of children who impose a fixed cost of working.

A universal subsidy scheme is however expensive and may be undesired when subsidies are financed by distortive income taxes. Often governments rather choose to introduce means-tested child care subsidies, targeted at low-income families. A rationale for this is that child care subsidies targeted at low-income families may boost childcare enrollment and encourage parent's labor market participation among groups of the population with initially low labor market participation. At the same time, it is well-known that means-tested transfers discourage the effect of working by high effective marginal tax rates in the interval where transfers are phased out.

The ambivalent effect of means-tested child care subsidies on parents' labor supply has received little attention empirically. In the present paper we aim to close this gap by discussing the different effects on parents' labor supply both analytically and by using a simulation model estimated to fit the joint labor supply and child care choices of Norwegian families with pre-school children. We further utilize the introduction of a national scheme of means-tested child care subsidies, which offers a natural experiment to isolate the price effect (positive effect of subsidized care) and the strategic effects (negative effect of means-testing) on Norwegian parents' labor supply.

The means-tested child care subsidy schedule created a so called "notch" in Norwegian parents' budget constraint. We thus isolate the "price effect", by using a regression discontinuity approach on the first year when the new subsidy schedule was introduced. The reform was introduced unexpectedly, making it impossible for parents to alter the income for eligibility, which is determined by last year's household income. Thus, we can compare labor market outcome of families just above and just below the threshold, with the assumption that they are similar except the eligibility for reduced child care fees.

The strategic effect is evaluated by noting that parents have different incentives for strategic adjustments dependent on whether they have a child in the eligible age group the next year. We use two separate approaches. First, we look for possible bunching around the threshold for means-tested subsidies in the following years after the reform was introduced. Second, to separate out the broader strategic effect than what can be visible by bunching, we use an event study approach.

Surprisingly, we find no significant effect on neither the price effect nor the strategic effect. An effect close to zero is confirmed by structural model simulations, which helps us shed some light on some of the likely reasons for the small labor supply responses to means-tested child care subsidies in the Norwegian context. We discuss how results may reflect an already highly subsidized price level including high child care participation rates and high female labor force participation. We further note that real responses in labor supply are likely to be small and slow, as third party reported income leaves little scope for strategically misreported income. The effect on parents' labor marked outcome may also be small if parents in the relevant income range are restricted in terms of labor

market opportunities (labor demand). Lastly a lack of responses can be due to lack of information, where the municipalities may not have been able to reach out to the non-participating families when the reform was first introduced.

Lastly, we acknowledge that there exist alternative justifications for governments funding of child care than to stimulate parents' labor supply. Another important rationale is to promote early learning, especially among disadvantaged children (see e.g. Blau and Currie, 2006; Cascio and Schanzenbach, 2013; Corneliussen et al. 2018; Havnes and Mogstad, 2015). We touch upon this topic when describing how subsidies affect participation in child care for which we find small positive effects close to zero according to the simulation model. A further justification for child care subsidies is that this might raise fertility and thereby contribute to solving the problems generated by an ageing population. However, according to this argument, it can be questioned why low income families should be subsidized more than high income families. Effects on fertility was likely not a political intention of the newly introduced scheme for means-tested child care subsidies in Norway, and we also do not find any visible (short-term) effect of the scheme on fertility.

This paper is organized as follows. In section 2 we describe related literature, referring to two large strands of the literature discussing effects of child care subsidies on mothers' labor supply, and discussing the tax related literature on how effective marginal tax rates and participation tax rates affect labor supply. Next, section 3 describes the institutional background of child care in Norway and the introduction of a national means-tested scheme. Section 4 sets the scene analytically, by describing the different expected mechanisms of effects on parents' labor supply of means-tested subsidies, and section 5 describes the method of using a behavioral microsimulation model estimated to fit the behavior of Norwegian parents to quantify the expected effects. In section 6 we utilize the introduction of the means-tested schedule as a natural experiment to isolate the price effect and the strategic effect. Section 7 discusses the findings from the natural experiment in light of what was learned from the model simulations, and section 8 concludes the paper.

2. Related literature

In this section we briefly review the literature on optimal provision of child care subsidies from a welfare perspective, before going into more detail on the two related empirical strands of the literature. This paper relates both to the literature on how the cost of child care affects parents labor supply, and to the literature which evaluates the negative labor supply effect of high effective marginal tax rates created by means-tested support to families with children.

2.1 The literature on optimal provision of child care subsidies from a welfare perspective

There is an active field of research which investigates whether it is optimal from a welfare perspective to provide child care subsidies, and how such a scheme should be designed. Most of this research arises from a Mirrleesian framework of optimal taxation where the government aims at maximizing social welfare under asymmetric information, where the government does not observe the individual's market productivities, but only the labor market outcomes (earnings). Although there are some caveats of the stylized Mirlees framework, it provides a useful benchmark to understand the economic mechanisms at play.

Barnett (1993) argues for instance that child care subsidies should be offered to mothers with young children to counteract the disincentive effects of the current tax system on labor supply. A similar principle emerges in the representative agent models of Kleven, Richter, and Sørensen (2000), which studies linear commodity taxation in presence of home production. Also Domeij and Klein (2013) suggests that subsidizing childcare is optimal from a welfare point of view.

Ho and Pavoni (2020) goes more in detail on the optimal design of child care subsidies and concludes that it is optimal to pay a *positive child care subsidy* on formal child care costs and that higher child care subsidies should be paid to lower income earners. They thus conclude that a means-tested child care subsidies are optimal from a welfare perspective in a Mirrleesian framework, although it creates milder intensive margin incentives of labor supply.

It is also possible to analyze welfare effects of different designs of transfers to children in other modeling frameworks, such as an equilibrium overlapping life-cycle model with heterogeneous agents. Guner, Kaygusuz, Ventura (2020) use such an approach to study the expansion of existing programs in the U.S. where policies affect female skills over the life cycle. They conclude that a policy expansion that combines features of childcare subsidies and direct transfers generates the largest aggregate welfare gains and makes a majority of newborns better off. Interestingly for the present context, they also argue that means-tested child care subsidies lead to larger welfare gains than universal programs.

It should be emphasized that the optimal provision of child care subsidies crucially depends on how to optimally trade-off redistribution for effort incentives. A key input regarding efficiency is to understand to which extent parents (and especially mothers) react to the economic incentives, as described in section 2.2. and 2.3.

2.2 The empirical literature on how the cost of childcare affects maternal employment

There is a large empirical literature on how the cost of child-care affect female labor supply initiated by Heckman 1974. Subsequent studies do not show consistent results; according to a survey by Blau and Currie (2006) labor supply elasticities with respect to child care fees range from 0 to -3. Akgunduz and Platenga (2018) offers the most recent meta-study of the literature.

Research from the US and Canada shows a strong effect of the introduction of free preschool in the 1960s and 1970s, especially on single mothers' labor supply (Cascio 2009), while studies of recent reforms find little or no effects (Gelbach 2002, Fitzpatrick 2010). The expansion of public day care in Germany in the 1990s led to increased day care coverage and higher employment among mothers (Bauernschuster & Schlotter 2015), and Givord and Marbot (2015) find that extended subsidization of day care in France in 2004 had a positive effect on mothers' labor supply, especially among mothers with several children.

These studies have been conducted in countries where women's employment is lower than in the Nordic countries, and where childcare is a greater barrier to women's employment than in the Nordic countries. Lundin, Mörk and Öckert (2008) argue that the effects of further expansions or further subsidization of daycare places in the Nordic countries will not be very large. They examine effects of the introduction of the maximum price reform in Sweden, and their analyzes show that the effect on mothers' labor supply is about zero. In Norway, Hardoy and Schøne (2015) have made similar analyzes of the maximum price reform and kindergarten development in the 2000s. They find that the maximum price reform and increased capacity in Norway contributed to three to four percentage points higher employment among mothers, but not to more working days among employed people.

This suggests that the labor supply is more elastic on the extensive margin than on the intensive. The development at the end of the 1970s also did not contribute to increasing the labor supply, likely because women who at that time were already strongly connected to working life, switched from informal care or daycare to daycare (Havnes & Mogstad 2011).

Previous research on the effects of expanding the daycare offer and experimental schemes of free part time attendance find few or no signs that this has affected the connection in working life. Black, Deverieux, Løken and Salvanes (2012) analyze long-term effects on children's outcomes and mothers' employment by using the municipalities' various points of moderation in parental pay for children born in the years from 1986 to 1992. They find that moderation schemes have small and non-significant effects on the labor supply for mothers of five-year-olds. Studies of experiments with free part time attendance in Oslo indicate that finances are of great importance for whether vulnerable families choose to use the daycare offer. This especially applies to minority language families. Drange and Telle (2015) show that the difference in kindergarten participation between children with and without an immigrant background was halved in the districts with such an offer compared with the control group. They do not find any effect of the increased kindergarten participation on parents' labor market participation.

Although the approach of the present paper is mostly related to the literature which uses microsimulation models and quasi-experimental evidence to analyze labor supply effects, we also note that there is a large literature using macro models of overlapping generation or dynamic structural models of labor supply which also aims at numerically predicting labor supply effects from policy reforms. Work in this area shows that childcare costs and child-related transfers are important determinants of married female labour supply; e.g., Attanasio et al. (2008) and Hannusch (2018). Expansions of childcare subsidies can lead to large increases in married female labour supply; e.g., Bick (2016) and Domeij and Klein (2013). Guner, Kaygusuz, and Ventura (2020) finds that an increase in child care subsidies in the US context may increase labor supply, especially along the extensive margin of participation. Blundell and Shephard (2012) estimate a structural labor supply model and focus on single mothers in the United Kingdom. According to numerical models the employment elasticities with respect to cost of child care for US single mothers with children aged below 6 range from −0.5 (Connelly 1992) to −1.29 (Connelly and Kimmel 2003).

2.3 The empirical literature on how means-tested family support discourage parents labor supply

Family support is often means-tested to target low-income families. Examples include the Earned Income Tax Credit (EITC) and the Child Tax Credit (CTC) of the U.S. and the Working Tax Credit (WTC) and the Child Tax Credit (CTC) of the U.K. These schedules play a vital anti-poverty role and provide increased assistance to poor parents. But given the means testing of the support, as credits are phased-out, the labor supply effects might be detrimental, see Hotz and Scholz (2003), Eissa and Hoynes (2011), Nichols and Rothstein (2016), and Chan and Mofitt (2018).[3]

As emphasized by Brewer, Saez, and Shephard (2010), the argument for means testing is strengthened if families are not very responsive to changes in the budget constraint. Several studies find that females are more responsive to changes in the budget constraint than males. For example, in the comprehensive review of labor supply responsiveness in Blundell and MaCurdy (1999), one

[3] There is also a related literature on mean-tested support concerning take-up, pointing to social stigma and complexity/ lack of information. As we do not have information on take-up we assume full take-up in the present paper.

finds large gender differences in own wage elasticities, with men's elasticities near zero and women's substantially higher, between 0.5 and 1.

The last decade there has been an emerging literature which suggest that elasticities can be measured by observed bunching at kinks or notches created by the budget constraint, see Saez (2010) and Kleven (2016). However, it seems that bunching is only observed in cases where individuals are able to self-report their income level. It seems more difficult to observe "real" labor supply bunching due to optimization frictions and small possibilities to adjust accurately (see e.g. Chetty, 2012; Bastani, Selin, 2014; Bosch et al, 2019).

One reason why there has been little focus on the negative effect of labor supply on means-tested child care subsidies in the literature, could be the view that parents only use child care while working, or that the government only provide subsidized child care given labor market participation of the parents. There is no such working requirement in Norway, as the main motivation is to improve access to childcare for low-income families and improve the early childhood investment of these kids. Also as argued in Thoresen and Vattø (2019) parents tend to have preferences for formal child care irrespectively of their labor market participation. Thus, means-tested child care subsidies are likely to induce similar negative labor supply effects as with means-tested income transfers (see e.g. Apps, Reese, Thoresen and Vattø, 2020).

3. Institutional background

3.1. Public childcare in Norway

Norwegian policy-makers formalized their efforts to increase the supply of formal child care through the so-called "child care compromise", approved by parliament in spring 2003. The agreement included a plan for eliminating queues for care at child care centers, and introduced a substantial reduction in child care fees, regulated by a maximum monthly parental pay. Today the availability of and enrollment in childcare is wide. Each municipality is required to provide public subsidized childcare, although the owner of the facility does not have to be the municipalities themselves. As such, 53 percent of the childcare centers are privately owned and receive public subsidies per child (Statistics Norway 2019).

The cost for childcare is regulated by the authorities and applies to all childcare centers either private or public. This price – known as the maximum price – which is currently 3040NOK (approx. 350 USD/Euro) per month is regulated each year. Child care is heavily subsidized, the parental fee covers approximately 14% of the costs for children under 3 years old and approximately 25% for children aged 3–5 (Lunder, 2015).[4] It follows that gross child care fees, measured as a percentage of the average wage, are very low in Norway compared to most other countries (OECD, 2014).

Part-time places of child care as usually unavailable. In practice the parents pay for a full-time place regardless of how many hours the child spends in child care centers. The opening hours are restricted to daytime, usual opening hours are Mon-Fri 07.30-16.30.

Children may enroll in childcare centers from the year they turn one until they start school in August the year they turn six years.[5] There is no working requirement on the parents in order to get access

[4] The cost difference reflects the fact that care for small children involves a higher staff-to-child ratio.

[5] As the enrollment-process follows the school year, availability is highest in august. All children born by the end of August are eligible for enrollment by the end of August next year. Thus, children born in January for instance, are not eligible for childcare enrollment before they are one-and-a-half-years old. Enrollment before August is subject to availability. Children born after August, in September, October or November, are eligible for enrollment by the month the child turns one year old.

to subsidized childcare. Enrollment is high. Nearly 92 percent of all eligible pre-school children are enrolled in childcare centers. Enrollment rates are highest among the oldest children: 97 percent of children aged 3-5 and 83.5 percent of children aged 1-2 years are enrolled.

3.2 The national scheme for reduced parental fees

The national scheme for reduced parental payment is administered by the municipalities and financed by the government. Thus, the moderation scheme does not directly affect municipalities' spending on childcare. The reform was implemented in two stages: Reduced parental payments for low-income families was implemented May 1st 2015. 20 hours free childcare per week for four- and five-year-olds from low-income families was implemented on August 1st 2015. Prior to this, the municipalities were required to have schemes for reduced co-payment for low-income families. However, these were developed and implemented at the local level. The reform in 2015 represented a change from local to a nationwide minimum requirement.

The national scheme for reduced childcare cost applies to all childcare centers in Norway, both municipal and private. The parental payment for both schemes of moderation is based on the household's total taxable capital and personal income. As a household, spouses, registered partners and cohabitants are considered. If a child lives permanently with one parent, the parental co-payment is based on the income in the household where the child is registered as resident. Moderation is granted for one school-year (Aug-July) at a time. Families with permanent low income must apply each year. The documentation of income comes from tax return(s) the previous year. If tax returns from the previous year are unavailable, or there is a significant and permanent change in household income, households may provide other types of documentation and gain access to this scheme.

Trætteberg & Lidén (2018) show that 88 per cent of the municipalities follow the national scheme, while 12 per cent have a more comprehensive moderation scheme as a whole. There is some variation in how actively each municipality recruit families into the scheme. The municipalities with a slower recruitment process catch most of the families when they submit the application for enrollment, or through contact with the center where the child is enrolled. In these cases, the schemes act as a subsidy to increase disposable income, but will not necessarily contribute to higher coverage. In municipalities with a more active recruitment, reduced prices is used as an argument to make childcare centers more attractive to families in the group targeted.

Reduced parental payment

The minimum requirement in the national scheme is that the parental payment for the first child should not exceed six percent of the household's total taxable earnings and capital income. If the household has several children, the payment is in line with the scheme of sibling moderation. Thus, parents must pay 70 per cent of the (reduced) price for their second child and 50 per cent of the (reduced) price for their third child or more. As of August 2019, the maximum price was 3040 NOK per month, and families with a yearly taxable household income of less than 557 333 NOK were eligible for reduced payment.

20 hours free attendance

The free attendance scheme was first introduced as a national scheme for four and five year olds on 1 August 2015, and later extended to three year olds on 1 August 2016. From 1 August 2019, the scheme also included two year olds. Free attendance amounts to 20 hours of free daycare per week

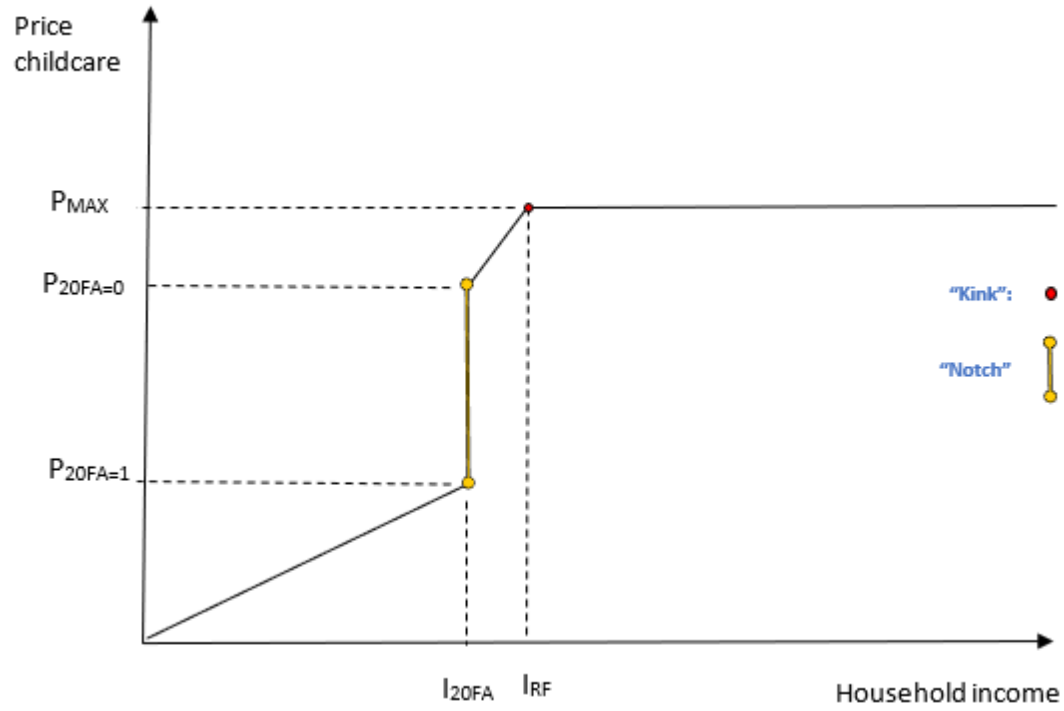
for households that meet the current income requirement (NOK 548 500 as of August 1, 2019). If the child is enrolled for more than 20 hours a week (40 hours per week is the norm), parents must pay for attendance over 20 hours. Free attendance normally means that parents pay 50 percent for full time enrollment.

Reduced parental pay and free attendance are two separate schemes, but families may be entitled to both forms of moderation. In such cases, a low-income household with children aged three to six will first receive payment reduction so that it does not exceed six percent of household income, and then receive 20 hours of free attendance off the reduced price. If the household is entitled to sibling moderation, this is deducted prior to the free attendance deduction.

4. Predicted effects of the national scheme for reduced parental payment

Figure 1 illustrates how the price the parents pay for one full time place at the childcare center depends on the household's income under the national scheme of reduced parental child care fees as described above. Note that the reduced parental payment for low-income households creates a kink in the parent's budget constraint (at I_{RF}), whereas the scheme for 20 hours free attendance creates a notch (a discontinuity at I_{20FA}) in the parent's budget constraint.

Figure 1: A graphical illustration of the national scheme for reduced parents' fees



Notes: An illustration of child care expenses for a full-time slot (40 hours) at a childcare center for families with one child in the age group eligible for free part time attendance. The price of childcare increases with the household's income according to the regulation that childcare expenses should make up maximum 6 percent of

household income, until the maximum price of childcare is reached. On top of this, the regulation of 20 hours free attendance applies when parents' income is lower than I_{20FA}.

Table 1: Expected effects of the scheme for reduced fees relatively to maximum price for all

		Parents labor supply		*Demand for child care*
		Participation	Working hours	
(i)	Reduced cost of working	+	(+)	+
(ii)	Income effect	(-)	-	+
(iii)	Strategic effect	-	-	+

From economic theory, there are three possible effects of the scheme for reduced fees on parents' labor supply relatively to a scheme with the maximum child care price for all, as summarized in Table 1.

First of all, child care expenses can be seen as a fixed cost of working. Thus, when child care fees are reduced, the fixed cost of working is reduced such that it is more attractive for the parents who are eligible for reduced fees to work. This means that we expect a positive effect on participation in the childcare centers and a positive effect on parent's participation in the labor market. There might also be a positive effect at the intensive margin of working hours if parents are induced to increase hours spent in child care, or if parents who previously did not use child care although participating in the labor market, now have the possibility to work longer hours. This mechanism is typically highlighted as one of the main reasons for providing subsidized child care.

But there are also two other effects on parent's labor supply drawn from economic theory which may possibly work in the opposite direction. One of the effects is the well-known income effect, as reduced fees is equivalent to a positive income transfer to families using child care. This means that families (who use childcare in both cases) can afford more of all goods, including leisure and child care (note that child care can be seen as a good in itself). Income effects are typically found to be rather small. Moreover, the income effect is not seen as distorting labor supply, and thus does not impose an efficiency/welfare loss.

The last "strategic" effect described in Table 1, can on the other hand, be seen as distorting parents labor supply negatively. This effect arises when families adjust their labor supply to get below the income limit, in order to obtain reduced child care fees. This leads to a reduction in the labor supply, which can be either at the extensive or the intensive margin. This mechanism follows from the high participation tax rates and the high effective marginal tax rates caused by the phase-out of the reduced child care fee schedule, which is a well-known disadvantage of all income-tested transfer schedules.

If the reduced fees are tied to the parents' participation in the labor market, either by law or by parents' preferences, then the expected "strategic" effect on the participation decision can be considered low (this strong tie is however not the case in Norway). In either case the strategic effect at the intensive margin can be considered strong, as the effective tax rate at the margin of labor supply gets very high in the phase-out interval. This is particularly the case with the 20 hours free attendance, which creates a notch in the parents budget constraint, where the parents are strictly worse off by working one more hour above I_{20FA} in Figure 1. According to economic theory this

creates incentives for some individuals to reduce working hours from above the income limit to right below the income threshold.

In sum, see Table 1, we expect the effect on childcare participation to be positive; there are no reasons for negative effects on child care participation. However, it is clear that the total effect of reduced fees on parents' labor supply (relatively to maximum fees to all) is ambiguous. Thus, the national scheme for reduced fees may both promote and discourage parental labor supply. We will therefore in the following evaluate which effect is larger empirically.

In the empirical evaluation, we must take into account that already before the national scheme was introduced the municipalities themselves were required to provide some reduced fees to low-income families. Moreover, also after the reform the national scheme provides a minimum requirement for the municipality, but the municipalities are free to provide even more generous schedules for low-income families. We lack details on local price schedules both before and after the reform, but it seems evident that most municipalities follow the national scheme after the reform. We can therefore utilize the discontinuity in the budget constraint which was introduced after the reform.

Another important feature of the national scheme for reduced child care, which can be exploited in the empirical analysis is that it is the previous year's income which form the basis of whether a family is eligible for reduced child care fees and 20 hours free attendance. This together with the fact that it was not possible to anticipate the reform and the income threshold before the introduction of the reduced fee scheme, means that we can use a regression discontinuity (RD) approach at the first year when the national scheme was introduced to isolate the price effect.[6] The other years we assume that all three effects in Table 1 are in place, which can be visualized by simulated data from a structural joint labor supply and child care choice model as described in Section 5.

5. Structural labor supply simulations

5.1 The method of simulating parents' labor supply decisions

We first attempt to illustrate the price effect as well as the strategic effect of the means-tested schedule by applying a discrete choice labor supply model with simultaneous choice of child care developed in Thoresen and Vattø (2019). For the use of the present study, we extend the original model in two respects. First, we estimate the model not only for couples, but also develop similar models for singles, and parents where only one or none of them are assumed to have flexible labor supply. Second, we use preference parameters estimated on the basis of detailed survey data to simulate for the complete Norwegian population of families with children. Thus, we extrapolate the parameters estimated from survey data to the full population.

The theoretical framework

The joint labor supply and child care choice model is a unitary household model based on a discrete choice framework, influenced by several studies using the discrete choice formulation both in analysis of standard labor supply (Aaberge et al., 1995; van Soest, 1995; Dagsvik et al., 2014; Dagsvik and Jia, 2016) and in joint labor supply and child care choice setting (Kornstad and Thoresen, 2007; Apps et al., 2016; Gong and Breunig, 2017; Thoresen and Vattø, 2019).

[6] RD can be used in this case as families cannot manipulate their treatment status.

Following Thoresen and Vattø (2019), we depart from a static modeling approach where parents' choice of labor supply and child care are viewed as a discrete choice problem, where the choice is made from a set of combinations of jobs in the labor market and slots in child care centers. For couples we let z (z = 1, 2, ...) index the (triple) combinations of child care alternative and job pairs (for mother and father). Each combination has a set of observable characteristics given by (h_m ,h_f ,s_m ,s_f ,q), where h_m and h_f denote hours of work for mother and father, respectively, and q the hours spent in nonparental child care. Furthermore, we let s_m and s_f be dummy variables which indicate whether the jobs are shift jobs or ordinary day time jobs; i.e., s_k , k = m, f is equal to 1 if the job considered is a shift job, and zero otherwise. We allow for that each family has preferences for both observed and unobserved characteristics of the jobs and child care centers, in addition to consumption. The household makes choices conditioned on a number of observable and unobservable restrictions. Before considering how this framework accounts for unobservable constraints, let us first define the economic budget constraint. Consumption for a given job and child care combination is defined by disposable income, C = f($w_m h_m$, $w_f h_f$, p, I) , where f (.) is a function which transforms income from work, $w_k h_k$ (k = m, f), costs of child care, p, and nonlabor income, I, into disposable income, given that w_m and w_f are the offered wage rates for the mother and father, respectively.

For simplicity we denote h=(h_m ,h_f ,s_m ,s_f) such that

$$\max_{h,q} U(C,h,q,z) = v(C,h,q) + \varepsilon(z), \qquad (1)$$

$$st.\, C = f\left(w_m h_m, w_f h_f, p, I\right) \; and \; given \; b(h,q)$$

where *v* (.) is the deterministic part of the utility function, and ε(*z*) , *z* = 1 , 2 , . . . , are iid random terms with c.d.f. exp (−exp (−*x*)). The economic budget restriction relates C (disposable income) to the choice of h and q.

The term $b(h,q)$ accounts for that the household faces latent discrete choice restrictions. For example, that it may be more jobs characterized by full-time working hours available.

The probability of the household choosing jobs and nonparental care alternatives with corresponding characteristics equal to h=(h_m ,h_f ,s_m ,s_f) and q is then given by

$$P(h,q) = \frac{\exp\left(v(C,h,q) + logb(h,q)\right)}{\sum_d \sum_j \exp\left(v(C(d),d,j) + logb(d,j)\right)}, \qquad (2)$$

We specify functional forms of utility, v(C,h,q) and opportunities, b(h,q), and use survey data to estimate the parameters of the model by maximum likelihood. See Thoresen and Vattø (2019) for more details on the functional form assumptions.

Estimation data and parameters estimates

To estimate the parameters of the model we use data from the Child Care Survey 2010, which maps child care preferences for about 3,000 households (Moafiand Bjørkli, 2011; Wilhelmsen and Löfgren, 2011). The survey includes detailed information on family composition, main activity/labor market status of parents, socioeconomic background, and mode/intensity of child care. Information on reported income (wages, transfers, etc.) and tax payments is obtained from Income and Wealth

Statistics for Households (Statistics Norway, 2017), and linked to the Child Care Survey by using personal identification numbers. We limit the dataset to couples with at least one child in the age group 1-5 years.

We divide the sample into four groups dependent on whether mother and father can be characterized as (potential) wage earners: Couple households, single-mother households, single-father households and households where none of the parents are (potential) wage earners.

We consider parents as (potential) wage earners if they are not registered as students, unemployed, self-employed, or the recipient of parental leave payments.

As opposed to Thoresen and Vattø (2019) we make use of contractual hours in child care centers over the year (based on the report of parents) and not actual time use. This modelling choice is motivated by the wish to analyze the effect of changes in the child care expenses effect which only depends on the contractual time.

We assume that couples choose among 3*7*7=147 alternatives, whereas single choose among 3*7=21 alternatives, and parents without flexible labor supply choose among 3 alternatives.

Individual wages, reported in Table A.X in the Appendix, are obtained from OLS wage regressions, one for mothers and one for fathers; see estimation results in Table A.1 in the Appendix.

The estimates of the four household types: Couple households, single-mother households, single-father households and households where none of the parents are (potential) wage earners are reported in Table A.X in the Appendix. We find that all parameters are according to theory, with positive preferences for consumption and leisure at the margin.

Description of data used for simulations

Next, the estimated model is simulated on the basis of the complete population of families with at least one child in the age group 1-5 years. This is based on detailed data from the Income statistics of households at statistics Norway (similar to the data applied to obtain RD estimates in Section 6). Now, we divide the observed data into four groups corresponding to the four estimation groups as described above, dependent of the (potential) wage earner status of the parents.

Low-income families and immigrants were oversampled in the child care survey, but this is not critical with respect to the estimation of the model, as observed characteristics such as education level, and immigrant status, enter the preference parameters of the model. The critical assumption is that households participating in the child care survey in 2010 are representative for the preferences of other households with equal observed characteristics in the simulation years (2014/2015/2016).

5.2 Simulation results of means-tested reduced fees relatively to the regulated maximum price for all

The outcome of the simulation is a probability distribution for each household for choosing each of the discrete alternatives of labor supply (h) and child care (q). This probability distribution is altered when the price schedule for child care fees changes.

First we present simulated labor supply and child care demand elasticities. The elasticities are obtained from simulations, in which child care fees are increased by 10% from the baseline. The elasticity estimates are reported in Table 2.

We find that the elasticity of child care demand with respect to the fee is very small. Similarly, we find that fees have almost no effect on the labor supply of both parents. Another study that finds response estimates close to zero is Lundin et al. (2008), with data for Sweden. They argue that in countries with a well-developed and highly subsidized child care system, further reductions in the price of child care have limited effects on mothers' labor supply. This may explain the low simulated responses in the Norwegian case too.

Next, in Table 3, we use the simulation model to look into the price effect we can expect from reducing child care fees to one half (reflecting 20 hours free of charge) relatively to the regulated maximum child care fee. We then again find that the price effect of reduced child care fees are very small. The effect is as expected largest for mothers. Naturally a smaller effect arises when the price effect only is directed towards a subset of the families (those with fixed low income). When income is determined endogenously by parents labor supply decisions, the effect on labor supply turns from positive to negative. This suggests that the negative strategic effect outweighs the positive price effect, and thus that the means-tested price schedule actually discourage parents to work.

In Table 4 we demonstrate the effects on the probability of eligibility to reduced prices. In line with that price has a positive effect on labor supply when income is considered fixed, we see that there is a small negative effect on the probability of eligibility. Whereas when income is endogenously determined by parents' labor supply the probability of eligibility increases, reflecting the strategic effect that it is relatively more attractive to stay below the income limit to get access to free part-time child care. Again, the effects are very small.

Table 2: Simulated elasticities of labor supply and child care demand with respect to child care fee.

	Labor supply, mother	Labor supply, father	Demand for center-based care
Price, child care	*-0.004*	*-0.002*	*-0.013*

Notes: Pre-reform 2014 schedule serve as the baseline.

Table 3: Simulated effect of 50% reduction in child care prices. Average over all households with children 2-5 years old.

	Hours of work (per week)		Child care, participation
	Mothers	Fathers	
Baseline, working hours/ use of care	29.99	36.01	92.98%
Prices reduced by 50 % for all	0.06	0.02	0.27%
Prices reduced by 50% for income (fixed) < I_{20FA}	0.01	0.00	0.04%
Prices reduced by 50% for income (endogenous)< I_{20FA}	-0.01	-0.05	0.05%

Notes: A price schedule with the regulated maximum price for all serve as the baseline. Average effect over all households with children in the age group 2-5 years old. I_{20FA} reflects the income limit for 20 hours (50% of full-time) free attendance, and is set to xxx NOK in 2014 (pre-reform)-prices.

Table 5: Predicted effects on the probability of eligibility. Probability over all households (age>=2 years old).

	Probability of eligibility
Baseline	23.94%
Prices reduced by 50 % for all	-0.04%
Prices reduced by 50% for income (fixed) < I_{20FA}	-0.03%
Prices reduced by 50% for income (endogenous)< I_{20FA}	0.03%

Notes: A price schedule with the regulated maximum price for all serve as the baseline. Probability of eligibility refers to the probability over all households with children in the age group 2-5 years old. I_{20FA} reflects the income limit for 20 hours (50% of full-time) free attendance, and is set to xxx NOK in 2014 (pre-reform)-prices.

6. Quasi-experimental evidence to isolate the price effect and the strategic effect on parents' labor supply

In this section, we evaluate whether we find evidence for the price effect and strategic effects on parents' labor supply by using the implementation of the nationwide free part-time scheme as a natural experiment.

6.1 Evaluating the price effect of reduced child care prices on parents' labor supply

Data and empirical approach

Data

The data is compiled from several Norwegian administrative records, including income registers, educational registers, family registers and employment registers. We consider a sample of all residents in Norway by January 1st in 2015. Families are identified by a unique link between the mother and child and the other parent. We restrict our sample to individuals who have children in the relevant age group – that is 4- and 5 year olds on 2015.

Most of our data are annual and follow the calendar year, while the income limit for eligibility are set in August and applies for the school-year (August-July). Nevertheless, eligibility is based on last year's tax records, which is consistent with our observation period. However, newly arrived immigrants, refugees or others who for some reason did not have taxable income the previous year and did not submit any tax return, are unobserved in our data. If a household experience drastic changes in their income, say due to unemployment, disability, divorce or death, they may apply and receive payment reduction and free attendance if they are able to document their new situation. Information of this kind is not available to us in our registers.

Unfortunately, we do not have administrative information on which children are enrolled in a child care center. We can only identify this indirectly by looking at whether the parents have tax deductions related to child care expenses (these expenses are pre-filled in the parents' tax returns). But we cannot distinguish whether a child is enrolled in a child care center free of charge or whether the child is not using childcare.

We include the universe of households with at least one child of 4- or 5 years of age – which is the targeted age group for free attendance when the nationwide scheme was first implemented. Our assignment variable is household income, which is the aggregate of the mother's personal taxable income and her potential partner's personal taxable income. Personal taxable income include earnings, pensions, taxable welfare benefits such as sick pay, disability or unemployment benefits, and positive capital incomes. We normalize household income by dividing it by the relevant cutoff income and subtracting it by 1.[7] The normalized family income will be 0 at the cutoff and takes positive values above and negative values under the cutoff.

[7] $I_{i,t-1} = \frac{HI_{i,t-1}}{c_t} - 1$

We include a set of pre-determined variables (pre first birth): years of schooling, age at first birth, immigration status (immigrant/native) and whether the mother is married or cohabiting with the current partner at first birth.

Our interest is the price effect of reduced child-care costs on labor supply. We estimate the effects on overall labor supply (including non-participants), and distinguish between effects on the extensive margin (employed/non-employed) and the intensive margin (hours per week). A person is registered as employed if he/she is registered as an employee at a firm and received non-zero and non-negative wage during a calendar month. Others are considered as non-employed that month. Individuals are assigned a category based on their employment spells over the relevant period from August 2015 through July 2016. Hours per week measures the average of weekly hours in the observation period when employed, while overall labor supply is weekly hours including the zeroes. This measure captures both the extensive and intensive margin.

Empirical approach of regression discontinuity (RD)

To estimate the price effect of childcare subsidies on parental employment, we implement a standard regression discontinuity analysis where we compare employment outcomes of parents whose income is just below the cutoff to those that are just above the cutoff. Since enrollment is below 100 percent we estimate an intention to treat effect, which also means that the treatment effect on the treated might be slightly higher than the RD-estimates.

For each labor supply outcome y_i we estimate the following model:

$$(1)\ y_{i,t} = \beta_0 + \beta_1 \mathbf{1} \cdot \{I_{i,t-1} < 0\} + \beta_2 x_i + \varepsilon_{i,t}$$

where y_i denotes the outcome for individual *i* at year *t*, $\mathbf{1} \cdot \{I_{i,t-1} < 0\}$ is and indicator for whether the family income of individual *i* is under the cutoff, x_i is a vector of predetermined control variables, and $\varepsilon_{i,t}$ is the conventional error-term. The estimate of interest is β_1, which is the effect of being eligible for free attendance on labor supply.

If our research design is valid, the inclusion of covariates in model (1) should have little effect on our estimates, other than a potential increase in the precision. We assess this assumption by including pre-determined characteristics as a robustness check.

We run a RD-model with a linear spline function in the assignment variable and triangular weights using the *rdrobust* command in STATA. We follow Calonico, Cattaneo and Titunik (2014) and choose the optimal bandwidths around the income cutoff for all of our labor supply outcomes. To assess whether our results are sensitive to the selected optimal bandwidth, we present results from alternative bandwidths in robustness checks in the Appendix.

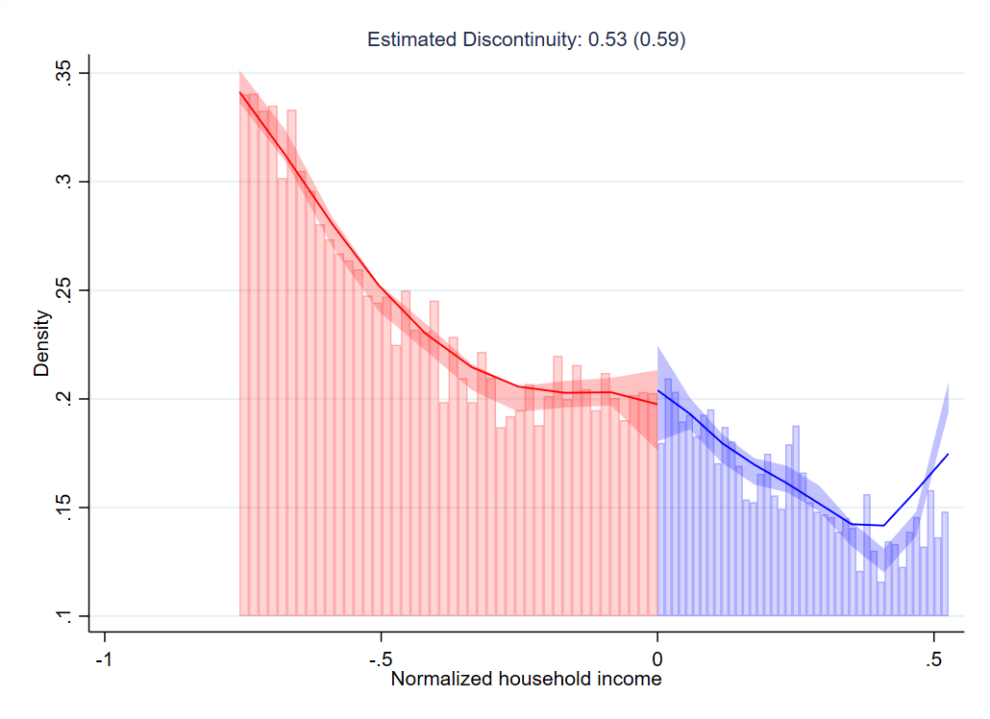
Test of identification assumptions

Households just above and just below the cutoff differ in their eligibility to free attendance in pre-schools, but we assume that they are similar in all other observable and unobservable predetermined dimensions. In order to test the validity of these identification assumptions, we check two potential threats to identification: Strategic income manipulation and compositional differences among the treatment and control group.

Strategic income manipulation around the cutoff

One threat to identification is strategic income manipulation – or bunching of households just below the income limit. Such strategic behavior on part of the household could be either reducing their labor supply or not increasing it in order to meet the income requirement for the subsidy. Given that the introduction of the free attendance scheme was introduced in June 2015 and eligibility is determined by household income in 2014, there is no concern that such manipulation is widespread. We formally test for bunching around the income limit using the well-known McCrary density test following Cattaneo, Jansson and Ma (2017a; 2017b). We do not reject the null-hypothesis of no bunching.

Figure 2. Nonparametric density test of household income manipulation

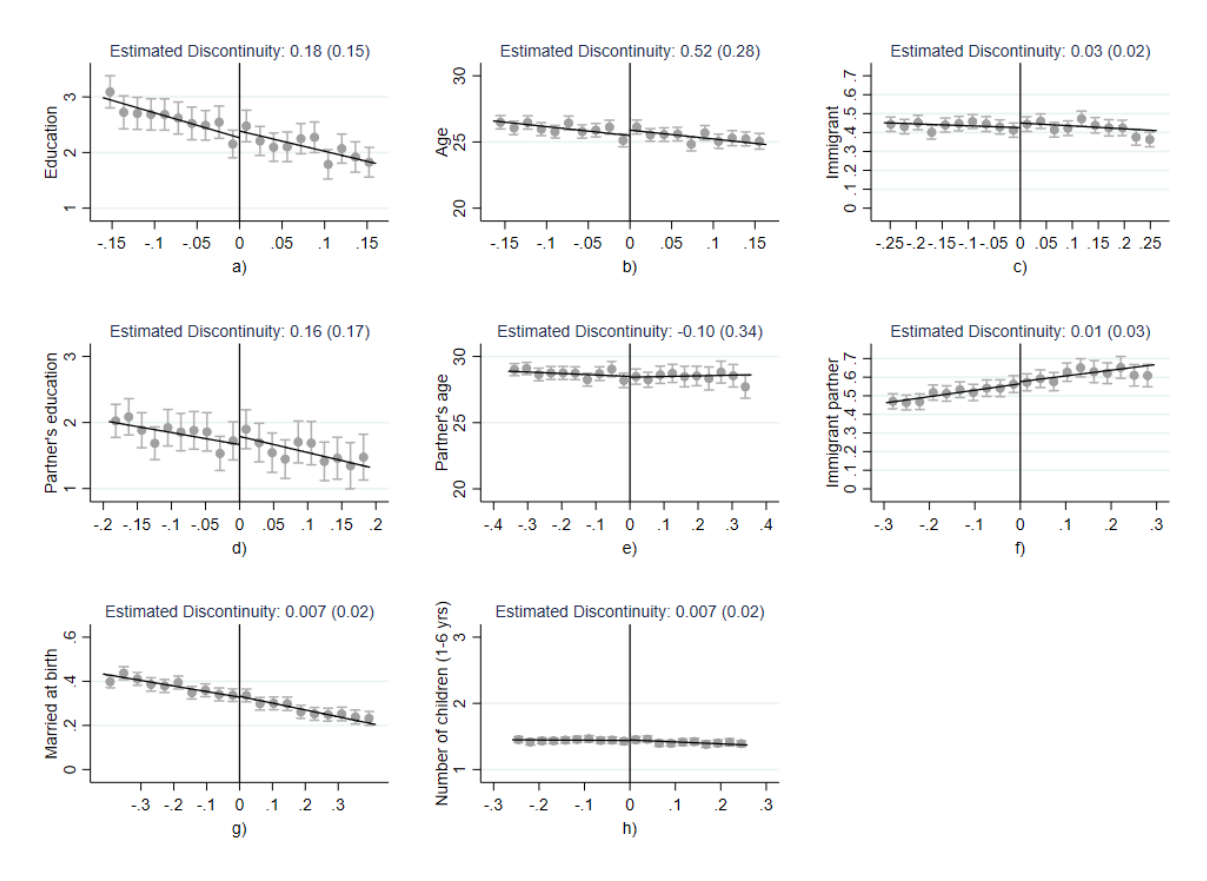


Note: These figures and tests were generated by the STATA-command *rddensity* with default options on our sample of households.

Covariate Balance

We assess the assumption of no unobserved determinants of household income at the cutoff by a covariate balance test. These are presented in table A1 and figure 3. It is clear from the results that we do not observe any consistent discontinuities in the predetermined characteristics around the cutoff.

Figure 3. Balancing of covariates.



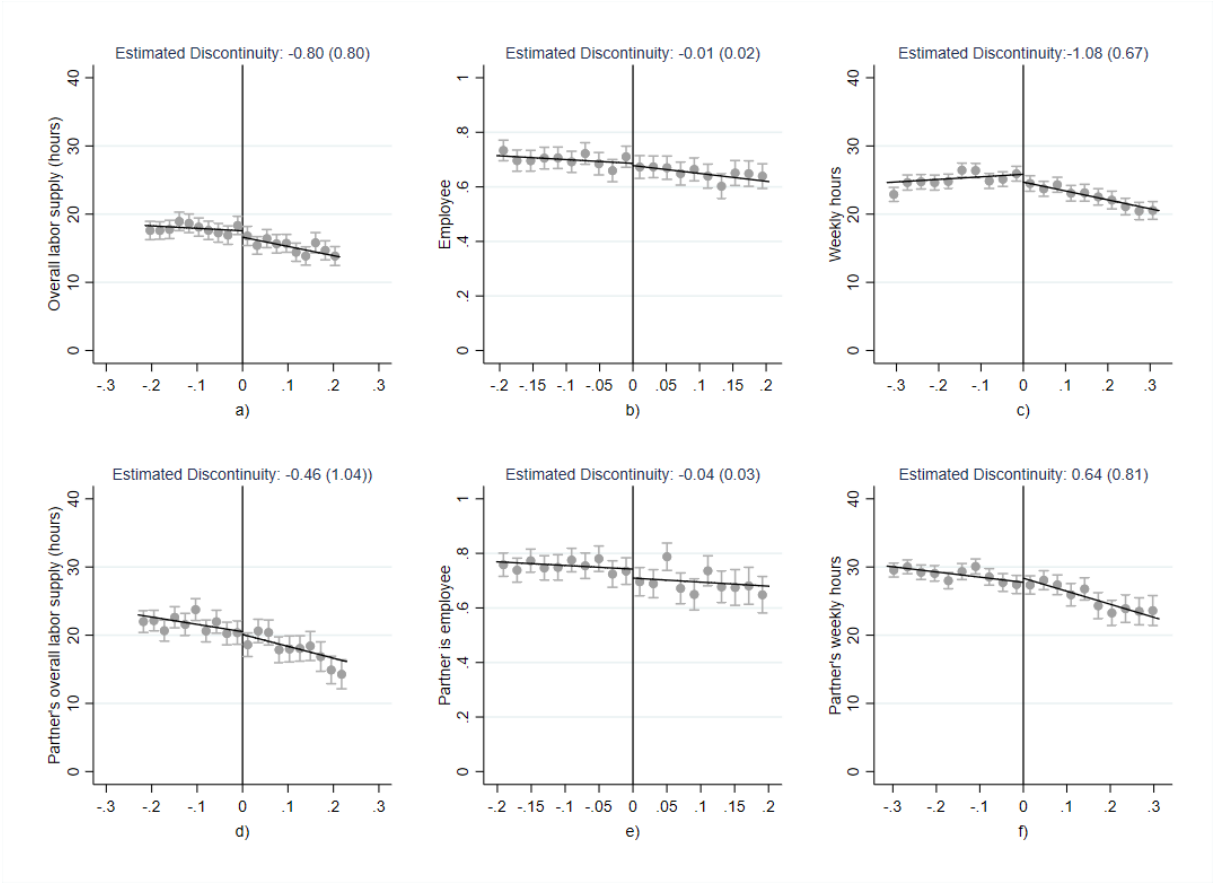
Note: Circles represent conditional mean values of the representative variable for each bin (10 groups) and capped spikes represent the 95 percent confidence interval. Sold line is the fitted value from a local linear regression with the optimal bandwidth. The vertical line is the cutoff point in the assignment variable.

To sum up, the exercise in this subsection suggests that there is no evidence of full manipulation of the assignment variable in our RD framework and there are no observed differences in the pre-determined covariates for households just above or just below the income limit.

Results

In this section, we analyze the impact of eligibility of free attendance on labor supply. Panel a-f in figure 4 plots the relationship between having access to the free attendance scheme or not on three measures of labor supply for the mother (a-c) and her partner (d-f). The circles represent conditional mean values of the representative variable for each bin (10 groups) and capped spikes represent the 95 percent confidence interval. Sold line is the fitted value from a local linear regression with the optimal bandwidth. The vertical line is the cutoff point in the assignment variable.

Figure 4. Effects of child care subsidies on labor supply.



Note: Circles represent conditional mean values of the representative variable for each bin (10 groups) and capped spikes represent the 95 percent confidence interval. Sold line is the fitted value from a local linear regression with the optimal bandwidth. The vertical line is the cutoff point in the assignment variable.

Figure 4 and table 7 depicts no discontinuities around the cutoff in any of our labor supply measures, neither at the extensive or the intensive margin. There results confirm our findings from simulations in section 5.

Table 7. Effects of access to free attendance on labor supply – Regression Discontinuity

	Mother			Partner		
	Overall labor supply	Employed	Weekly hours	Overall labor supply	Employed	Weekly hours
House income < Limit	-0.80	-0.007	-1.09	-0.46	-0.04	0.64
	(0.80)	(0.02)	(0.67)	(1.04)	(0.03)	(0.81)
Optimal bandwidth	0.21	0.21	0.32	0.23	0.20	0.31
Eff. no. of obs.	10 340	9 910	10 224	6 794	6 009	6 799
No. of observations	125 713	125 713	100 255	108 234	108 234	96 437

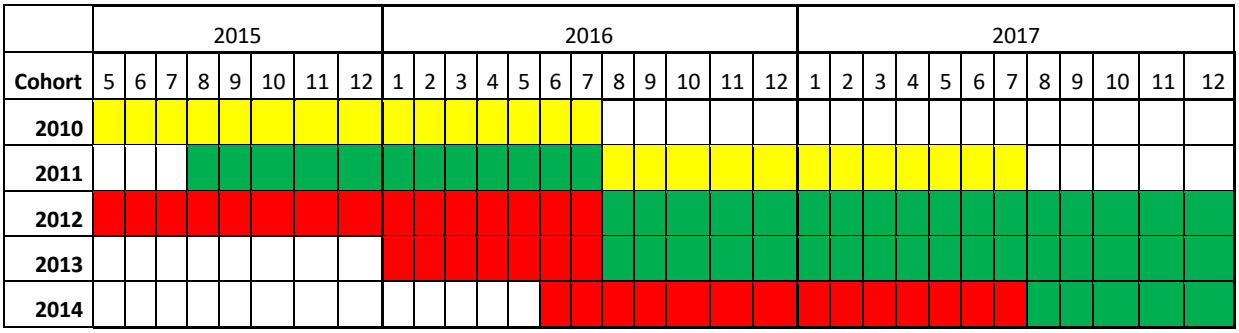
Note: Bias-corrected estimates and robust standard errors in parenthesis. Significance: * p < 0.1, ** p < 0.05, *** p < 0.01

6.2 Evaluating the strategic effect of means-tested child care prices on parents' labor supply

As shown in figure 2, we fail to reject the hypothesis that households bunch below the income limit for eligibility when the scheme was implemented. However, families with children younger than the targeted age group may strategically manipulate their income in 2015 (2016) in order to meet the income requirement in year 2016 (2017). In this section, we investigate strategic behavior among households who are eligible for free attendance at some point in 2016 and 2017.

Based on the scheme and the birth cohort of the child, we identify three possible effects from the free attendance scheme on labor supply: a) strategic effects, b) strategic- and price effects or c) price effects. We focus on the pure strategic effect, i.e. periods where there is no additional price effect on their labor supply. We identify periods for strategic effects based on the child's age, the observation year and the month the scheme changes. For clarity, we present an illustration of the three effects in figure 6.

Figure 6: Strategic- and price effects for eligible households (2015-2017).



Note: Numbers in headers represent months of year. a) strategic effects in red, b) strategic- and price effects in green and c) pure price effects in yellow.

When the scheme was announced in May 2015 and introduced in August 2015, families of 4- and 5-year olds born in 2010 or 2011 could not manipulate their income-level back in 2014. At that same time, families of 3-year olds (born in 2012) could in theory reduce (or not increase) their labor supply in June-December 2015 in order to reduce (or not increase) their household income to become (or remain) eligible for the subsidy in August 2016. Furthermore, children born in 2013 would get free-attendance in August 2017 in the initial regime and had strategic incentives to reduce their household income in the entire year of 2016. However, by August 2016 the free-attendance scheme was expanded to include 3-year olds as well and these families encountered a price effect as well. Children born in 2014 knew in June 2016 that they were eligible for free attendance in august 2017. They had a strategic effect from June- December 2016 towards the school year in 2017 and a pure strategic effect in January-July 2017 towards 2018 (2018 is not included in our analysis).

Evaluating local strategic effects (bunching)

We find no visible evidence of bunching around the threshold in 2015 and 2016 for families with economic incentives to stay below the income limit. [Show this here]

Evaluating broader strategic effects (not necessarily observed as bunching)

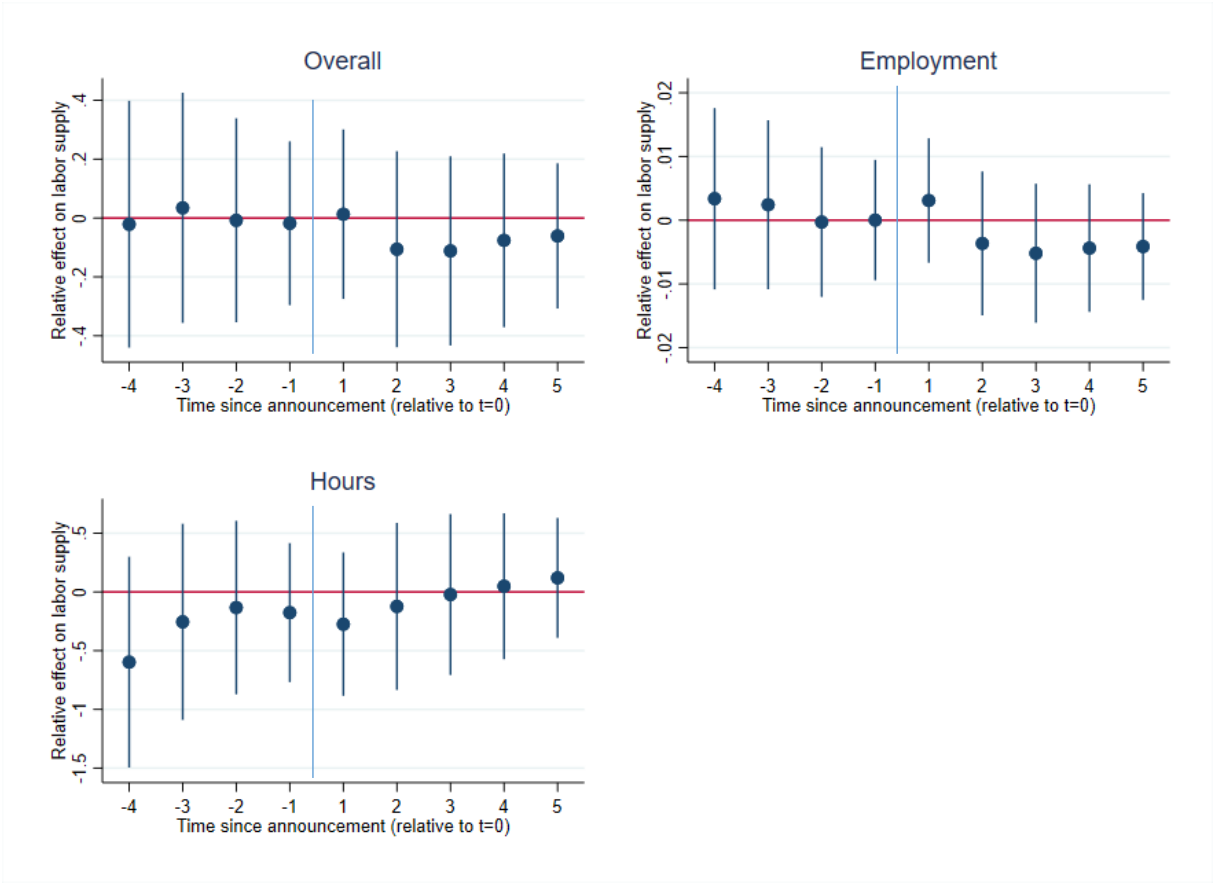
We use an event study approach to establish whether there are any strategic effects on labor supply among eligible families[8] from the time of announcement (t=0) of either the introduction of the nation-wide scheme in 2015 or relevant changes in 2016. Formally, we estimate the following equation:

$$y_{it} = \sum_{y=-4,\, y\neq 0}^{6} \delta_t I(t = t' + y) + \tau_{t-1} + \mu_t + \theta_t + \rho_{it} + u_{it} \,,$$

where τ_{t-1} are income-year fixed effects, μ_t are observation year fixed effects, θ_{it} are observation months fixed effects, ρ_{it} are birth-cohort fixed effects.

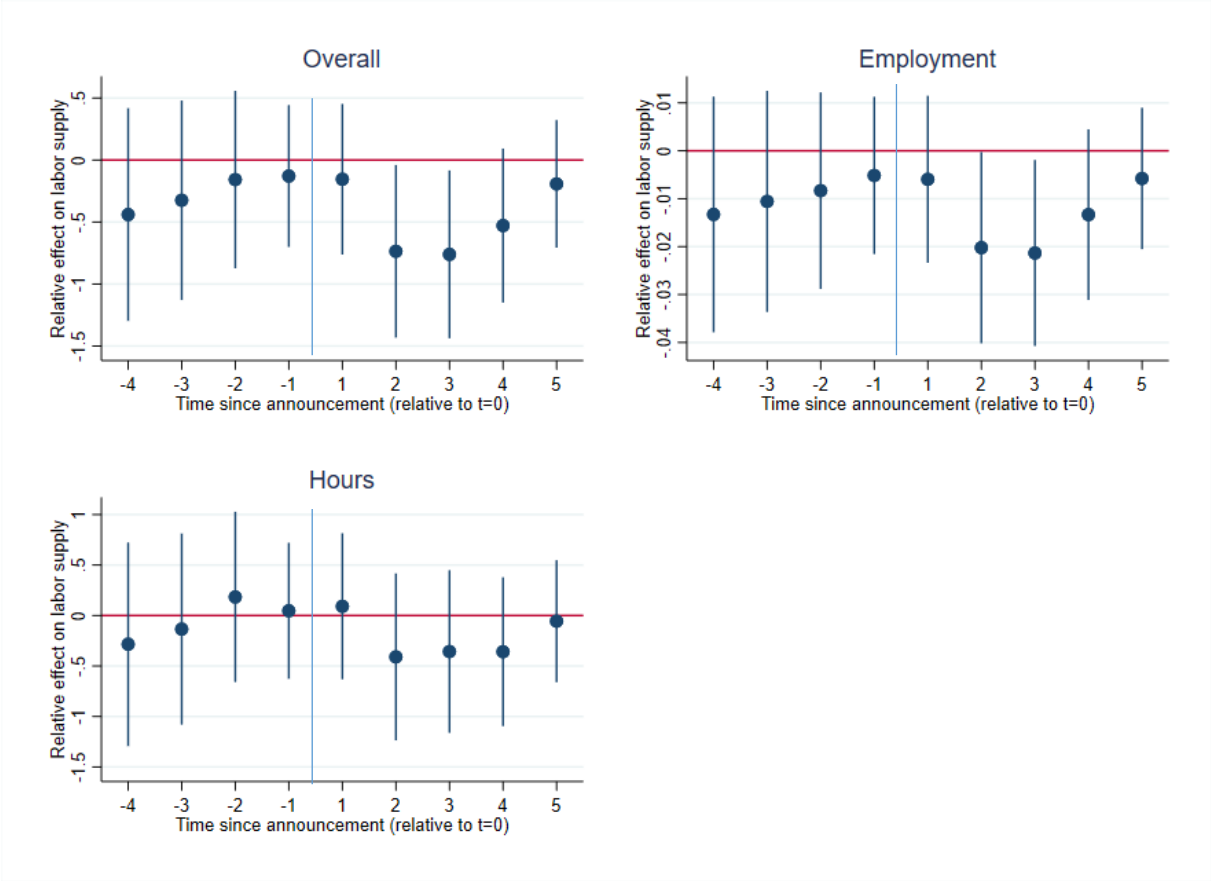
In the case of strategic effects, we expect the estimate coefficients after the base period to be significantly negative. As shown in figure 7, there are no clear strategic effects among mothers, however the evidences is less clear among partners, as shown in figure 8.

Figure 7. Effect of announcement of free attendance on labor supply of mothers.



[8] Based on age of child and the relevant household income

Figure 8. Effect of announcement of free attendance on labor supply of partners.



7. Discussion of results

Surprisingly, we do not find any statistically significant evidence for neither the price effect nor the strategic effect on parents' labor supply in section 6. This is in line with the very small predicted effects from the microsimulation presented in section 5. In this section we discuss possible reasons for the small (or possibly non-existent) responses in the Norwegian context.

Salience and information

Real working hours responses – likely small and slow

Static vs dynamic decisions- how important are short-term economic incentives vs future economic gains/losses of more/less labor market experience

Adjustment costs and small vs large changes in economic incentives

The Norwegian setting of already highly subsidized child care and high labor market participation

The decision to work may be independent from the decision to use formal child care if informal care is available

Do income effects influence our evaluations of the price effect and the strategic effects?

8. Conclusion

Introducing a means-tested schedule of subsidized childcare has two potential opposite effects on parents' labor supply. First, increased subsidies lower the fixed cost of working and induce parents to work more. But at the same time the phase-out of a means-tested subsidy creates high effective marginal tax rates and high participation tax rates, where parents' incentives to work are reduced.

Simulations from a structural labor supply and child care demand model suggest that both effects are small in the Norwegian context. This is confirmed by using the newly introduced free part time child care for low-income households in Norway as a natural experience to isolate the price effect and the strategic effects.

Finally, we discuss possible reasons for why the price effect and the strategic effect are so small (or possibly non-existing) in the Norwegian context.

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Appendix

A1. Descriptive statistics and balancing tests of the RD-analysis

Descriptive statistics

Table A1 provides descriptive statistics for two samples: our overall sample of households with kids in the relevant age group and for the households located +/ 25 percent around the cutoff, which comprise the most relevant sample for our estimations. The main take-away is that these two samples are distinctly different along some important dimensions. The share of immigrants is larger around the cutoff than overall, mothers are younger, less educated, more likely to be single, less likely to be married, less likely to be employed. The same applies for partners, although the difference in employment is smaller than among mothers. These differences clearly show that the subsidy scheme is targeted at low-income families.

Table A1. Descriptive statistics for all households and households +/- 25 percent around the cutoff. Means and standard deviations in parenthesis

	All	**+/- 25 percent around cutoff**
Immigrant	0.26	0.43
	(0.44)	(0.50)
Age first birth	27	25
	(5)	(5)
Education pre birth	3.99	2.39
	(3.10)	(2.81)
Single	0.14	0.38
	(0.35)	(0.49)
Married pre birth	0.52	0.33
	(0.50)	(0.47)
Childern under 18	2.23	2.09
	(0.87)	(1.00)
Children under 6	1.49	1.43
	(0.59)	(0.60)
Oslo	0.13	0.15
	(0.34)	(0.35)
Employed	0.80	0.67
	(0.40)	(0.47)
Working hours	28.47	23.30
	(11.60)	(13.03)
Student	0.10	0.12
	(0.30)	(0.33)
Partner		
Immigrant	0.23	0.55
	(0.42)	(0.50)
Age first birth	30.04	28.59
	(5.59)	(6.79)
Education per birth	3.78	1.78
	(2.99)	(2.52)
Employed	0.89	0.73
	(0.31)	(0.44)
Working hours	33.66	26.80
	(9.02)	(12.75)
Student	0.05	0.09
	(0.22)	(0.29)
Household income	901660	409270
	(1719126)	(57554)

Balancing tests

Table A2. Balancing tests.

	(1) Education	(2) Age	(3) Immigrant	(4) Partner's education	(5) Partner's age	(6) Immigrant partner	(7) Married at birth	(8) Number of children
House income	0.12	0.41	0.03	0.12	-0.06	0.01	0.004	0.007
< Limit	(0.13)	(0.25)	(0.02)	(0.14)	(0.29)	(0.02)	(0.01)	(0.02)

Optimal bandwidth	0.16	0.16	0.26	0.19	0.36	0.30	0.41	0.26
No. observations	7857	7987	12 536	5743	10 604	8710	19 299	12 369

Note: Significance: * p < 0.1, ** p < 0.05, *** p < 0.01

A2. Robustness checks of the RD-analysis

In this section we assess the robustness of our results in section 6.2 and check whether our main findings are sensitive to the choice of control variables, to different functional forms, alternative bandwidths or inclusion of large cities with similar moderations schemes before the introduction of the nation wide scheme.

The robustness checks lend support to our identification strategy and the validity of our estimation. We conclude that there are no significant effects of the price effect of free attendance scheme on household labor supply.

Inclusion of covariates

In our main specification, we do not control for any pre-determined covariates. Our balance-tests show that there are no observed differences between households who are located just above and just below our cutoff. Therefore, inclusion of pre-determined covariates should not produce different estimates, but could improve precision of the estimation.

Table 8 presents results from estimations where we include the mother's age, education and first birth and whether she is immigrant, her partner's age, educational level at first birth and whether he/she is immigrant. These estimates show that our main findings are robust to the inclusion of covariates. The estimates are mostly slightly smaller than in table 7, but we do not find any significant effects on labor supply from eligibility of the free attendance scheme.

Table A3. Robustness checks – adding controls

	Mother			Partner		
	Overall labor supply	Employed	Weekly hours	Overall labor supply	Employed	Weekly hours
House income < Limit	-0.84 (0.74)	-0.006 (0.02)	-0.95 (0.69)	-0.56 (1.01)	-0.04 (0.03)	0.52 (0.73)
Optimal bandwidth	0.22	0.21	0.29	0.23	0.20	0.37
Eff. no. of obs.	10 709	10 207	9 130	6 906	6 086	8 021
No. of observations	125 713	125 713	100 255	108 234	108 234	96 437

Note: Included controls are: mother's age, education and first birth and whether she is immigrant, her partner's age, educational level at first birth and whether he/she is immigrant.
Bias-corrected estimates and robust standard errors in parenthesis. Significance: * p < 0.1, ** p < 0.05, *** p < 0.01

Parametric estimation

Our preferred model controls for a linear spline in the assignment variable. In table 9 we allow for alternative functional forms of that spline: A second-order polynomial and a Thrid-order polynomial. These results are also similar to our main findings in table 7.

Table A4. Robustness checks – parametric estimation

	Mother			Partner		
	Overall labor supply	Employee	Weekly hours	Overall labor supply	Employee	Weekly hours
Second-order polynomial						
House income < Limit	-0.43	0.0003	-0.74	-0.67	-0.04	0.77
	(0.82)	(0.009)	(0.79)	(1.04)	(0.03)	(1.14)
Optimal bandwidth	0.37	0.36	0.43	0.43	0.32	0.31
Eff. no. of obs.	17 172	16 889	13 342	12 897	9 648	6 809
No. of observations	125 713	125 713	100 255	108 234	108 234	96 437
Third-order polynomial						
House income < Limit	-0.76	-0.01	-0.49	-0.40	-0.04	0.82
	(0.97)	(0.03)	(0.92)	(1.32)	(0.04)	(1.25)
Optimal bandwidth	0.46	0.36	0.52	0.45	0.45	0.43
Eff. no. of obs.	21 490	16 698	16 170	13 780	13 679	9 493
No. of observations	125 713	125 713	100 255	108 234	108 234	96 437

Note: Bias-corrected estimates and robust standard errors in parenthesis. Significance: * p < 0.1, ** p < 0.05, *** p < 0.01

Heterogeneous effects

At the time the national scheme of free attendance was announced and implemented, Oslo and Drammen, the capital and a large neighboring city in the eastern part of Norway, had their own large-scale free attendance schemes. These were implemented in areas with a high share of low-income families. The existence of these schemes prior to the nation wide scheme, may influence our main results on labor supply responses. To this end, we report estimation results where we exclude these municipalities. Our main conclusion of no labor supply response is not changed. Additionally, targeted groups, such as single mothers, students and families of immigrant background may have heterogeneous responses to the scheme. Our results from table 9 does not provide any evidence of significant labor supply responses in these targeted groups.

Table A5. Heterogeneous effects

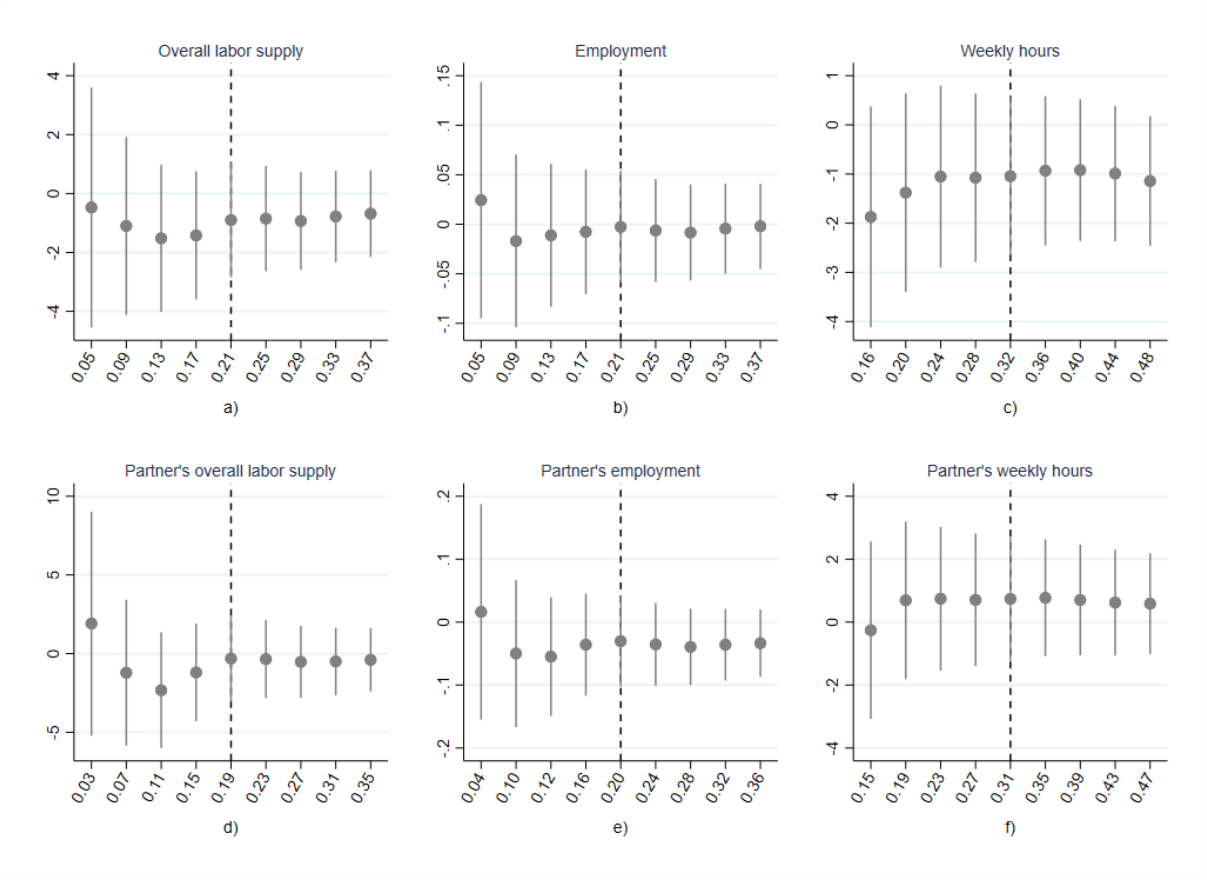
	Mother			Partner		
	Overall labor supply	Employee	Weekly hours	Overall labor supply	Employee	Weekly hours
Excl. Oslo and Drammen						
House income < Limit	-0.50	-0.002	-0.82	-1.18	-0.05	0.32
	(0.86)	(0 03)	(0.71)	(1.15)	(0.03)	(0.83)
Optimal bandwidth	0.22	0.20	0.33	0.23	0.20	0.38
Eff. no. of obs.	8726	8 225	9 033	5 369	4 803	6 774
No. of observations	107568	107 568	86 660	92 278	92 278	82 734
Single-mothers						
House income < Limit	0.09	0 03	-0.81			
	(1.10)	(0.03)	(0.73)			
Optimal bandwidth	0.18	0.18	0.27			
Eff. no. of obs.	3 392	3 342	4270			
No. of observations	17 479	17 479	9575			
Students						
House income < Limit	-1.75	0.15	-3.93	-1.05	0.03	-0.24
	(1.63)	(0.05)	(2.03)	(2.20)	(0.07)	(2.04)
Optimal bandwidth	0.39	0.45	0.29	0.38	0.26	0.32
Eff. no. of obs.	2 296	2 718	1 181	1 451	956	961
No. of observations	12 809	12 809	10 200	10 456	10 456	9 483
Immigrant family						
House income < Limit	-0.48	-0.01	-0.46	-0.45	-0.64	1.45
	(0.87)	(0.03)	(1.41)	(1.46)	(0.04)	(1.09)
Optimal bandwidth	0.38	0.42	0.33	0.24	0.20	0.25
Eff. no. of obs.	5278	5 753	2 226	3 444	2 938	2 857
No. of observations	18 002	18 002	10 641	18002	18002	13779

Note: Bias-corrected estimates and robust standard errors in parenthesis. Significance: * p < 0.1, ** p < 0.05, *** p < 0.01

Alternative bandwidths

In figure 5, we report the point estimates and the corresponding 95 percent confidence interval for our labor supply measures for alternative bandwidths around the cutoff. These results show that the confidence intervals become relatively larger when we use a smaller bandwidth, but our main result are robust to the choice of bandwidth in the neighborhood of the optimal bandwidth.

Figure A1. Alternative bandwidths



Note: Each point represent estimated bias-robust coefficients of from equation (1) with varying bandwidths. Capped spikes represent the 95 percent confidence interval. Dotted line is the optimal bandwidth from our main specification.

A3. Validating the structural labor supply model against the quasi-experimental findings

In the simulations in Section 5 we introduced hypothetical changes to illustrate the effect of means-tested child care subsidies relatively to a flat maximum price. Here, we instead try to use model simulations to predict which effects we can expect to find in the quasi-experimental evidence in Section 6. This both serve to validate the model predictions, and to better understand the outcomes of the quasi-experimental evidence. We thus re-do the quasi-experimental analysis with simulated data.

When simulating the effect of the reform, we account for that there existed price moderation schemes already before the introduction of the national scheme for reduced parents' fees.[9] We also account for tax deductions affected by expenses for childcare. In addition, single parents are entitled

[9] Sibling moderation is calculated on the basis of the minimum requirement from the regulations on parental pay of 30percent moderation for child no. 2 and 50 per cent for child no. 3 and others. Income moderation is calculatedby using information on actual average parental pay for various income in municipalkindergartens. By interpolating between the average price for households with incomes of resp.250,000 and 375,000 in January 2014 (Scheistrøen, 2014), we find a correlation of y=min(1969+0.872x,2405) where x is the annual income in 1000 and y is childcare fee per month for a full-time place.

to childcare benefit for single parents ("stønad til barnetilsyn") where 2/3 of their child care expenses are covered. These regulations are now all included in the 2014 baseline as well as in the post-reform years, which means that the reform's net-effect of child care expenses is lower than one would expect looking at the national scheme in isolation (similarly to the exercise in Section 5.2).

The simulated data is treated similarly to observed data in Section 6 in order to check how well the structural model simulations match observed outcome. Simulated data is formed on the basis of expected values of the outcome. First, we redo the RD analysis of section 6.2 on simulated data, next we show that the model predicts no strategic bunching effects (relevant to section 6.3).

Table A6. Regression Discontinuity (RD) results on simulated data. Mimic first year-responses (fixed income)

	Optimal bandwidth	RD-estimate (std error)
Child care		
Overall	0.082	0.0022 (0.0070)
Mothers		
Overall	0.085	-0.0775 (0.2180)
Participation	0.081	-0.0001 (0.0043)
Hours of work	0.091	-0.0641 (0.1114)
Fathers		
Overall	0.090	-0.2337 (0.2435)
Participation	0.088	-0.0053 (0.0051)
Hours of work	0.097	-0.0481 (0.0709)

Note: Simulated data are based on 2014 data, simulated on 2017 schedule. Income is negatively normalized such that 0 corresponds to the exact income threshold (I_{20FA}), and positive numbers refers to households with lower income. Positive estimates thus refer to an increase in hours/participation for eligible families.