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Child care, parental labor supply and tax revenue[☆]

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ABSTRACT

We study the impact of child care for toddlers on the labor supply of mothers and fathers in Norway. For identification, we exploit the staggered expansion across municipalities following a large child care reform from 2002. Our IV-estimates indicate that child care causes an increase in the labor supply of cohabiting mothers who move towards full time employment. Despite this, average taxes paid on the extra income is low, lending little support to the argument that parts of the cost of child care is offset by increased taxes. Meanwhile, we find no impact for fathers.

1. Introduction

Over the last decade, policymakers have shown increasing interest in government interventions in the market for child care. The OECD has discussed the introduction of early childhood programs in several reports (Field et al., 2007; OECD, 2006); in Germany, South Korea, Canada and the Scandinavian countries, governments have been pushing to expand access to subsidized care. In the US, President Obama proposed to make “high-quality preschool available to every child in America” in his State of the Union address 2013.

An important argument in favor of governments subsidizing and facilitating child care availability is the claim that child care helps reconcile work and family responsibilities, thereby increasing mothers’ labor force participation (OECD, 2006). A positive impact on labor supply may also mean that the public cost of providing child care is partly mitigated by increases in the tax base or reduced benefit dependence.

In this paper, we estimate the labor supply and tax impacts of child care using a large increase in child care availability for toddlers (age 2) in Norway following a reform from 2002 to expand child care to cover demand. The reform increased government subsidies to investment in and running of child care institutions, and generated large variation in the supply of child care between municipalities and over time. Our estimation strategy exploits the difference in child care expansion between municipalities following this reform to get credible estimates on how child care affects the labor supply of mothers and fathers, as well as other potential caregivers. Because child care for toddlers was strongly rationed in this period, changes in the availability should be driven primarily by the changes in supply as a result of the reform, and not by changes in local demand. To guard against omitted variable bias, we nonetheless document that our estimates are robust to controlling for a large set of observable characteristics and to a range of specification checks.

An important improvement over much of the literature is that we observe a continuous measure of the individual use of child care throughout the year. This allows us to implement an IV strategy, using the availability of child care as an instrument for the actual enrollment, which accounts for the intensity of child care use. Our estimates reflect, therefore, how a full year of child care enrollment affects mothers’ labor supply, rather than the direct effect of additional child care slots that open at some point during the year, which is estimated in most of the literature.

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There are at least two reasons why accounting for the intensity is important. First, new child care places are typically created in August, at the start of the school year. This mechanically gives a maximum child care use of around 42% for most children, i.e. 5 out of 12 months. Second, to the limited extent that child care places are not utilized to capacity, child care institutions have quite extensive discretion to scale provision to actual use, by e.g. adjusting employee hours. In this case, treating child care places as uniform independent of utilization will overestimate the cost of provision. A further improvement over much of the previous literature is that we identify both cohabiting and single mothers, as well as fathers and grandparents.

This is among the first papers to provide evidence on how large-scale, universal child care for toddlers affects parental labor supply.[1] In contrast, the sizable literature on child care and mothers labor supply has been focused mostly on child care for preschoolers (age 3–6), for whom theree are important reasons to believe that the impact on parental labor supply may differ. Descriptively, young children who are not in child care are much more likely to be cared for by one of the parents in the home (often the mother), while older children are more often cared for by informal childminders, like relatives, friends, or nannies. This may be due both to a stronger reluctance among parents of young children to use informal child care arrangements, or to less supply of informal care for young children. Either way, we would expect that the availability of child care for young children may have a stronger potential to increase labor supply of parents than availability of care for older children.

Our results indicate that child care for toddlers has substantial effects on mothers' labor supply. A married or cohabiting mother induced to use a full year of child care by the reform is 32 percentage points more likely to be employed, compared to mothers who do not use child care at all.[2] This is over a baseline of 63% participation before the reform, and associated with an earnings increase of 66,000 NOK (8,000 USD) for cohabiting mothers. We also investigate persistence in the labor supply response, finding positive impacts 1–3 years later. For single mothers, we find contradicting and imprecise results that preclude strong conclusions.

Proponents of subsidized child care commonly claim that parts of the cost of such subsidies are offset by the increased tax revenues or reduced benefits generated by the additional income of working mothers. Using data on actual tax payments, we can go beyond back-of-the-envelope calculations found in previous literature to show that the increased taxes caused by child care use is, at least in our case, insignificant and relatively small. Specifically, our estimates suggest an average tax rate of about 14% on the extra income, much lower than the average tax rate. At the same time, the increased access to child care and higher employment does not seem to lead to significant reductions in benefits, which would further have reduced public spending.[3]

For fathers, we find no labor supply response. This may indicate that mothers are still the primary caretakers, staying home when child care is not available. Meanwhile, estimates on working age grandparents show that there is no response among maternal grandparents, but modest effects on the labor supply of paternal grandparents. Interestingly, while paternal grandmothers work more, paternal grandfathers work less when the child is using child care. This may suggest that paternal grandmothers are important informal caregivers to some 2-year olds, while there is an indirect income effect on grandfathers.

We contribute to the rapidly growing literature estimating how child care availability affects parental labor supply. Most of the previous literature studies pre-school children at ages 3–6, and find modest effects.[4] For younger children, evidence is more limited, but some recent studies indicate that effects might be larger for this group than older children, in line with our findings. Goux and Maurin (2010), for instance, study a cutoff to school start for 2- and 3-year olds in France, estimating that about one single mother enters employment for every four 2-year olds enrolled. In comparison, they do not find effects on single mothers of 3-year olds or cohabiting mothers. Givord and Marbot (2015) study an increase in child care subsidies and estimate that the reform caused small but significant increases in the labor supply of mothers of 2-year olds, but not 3-year olds. Carta and Rizzica (2018) study a reform that expanded access to highly subsidized child care to 2-year olds in Italy in the mid-2000s, reporting effects of 5–7 percentage points on the labor participation of mothers. Bauernschuster and Schlotter (2015) study the introduction of a legal right to care in Germany for 3-year olds and find impacts of similar magnitude to what we find in the current paper, while the estimates for a Spanish child care reform expanding access for 3-year olds are around half as large (Nollenberger and Rodríguez-Planas, 2015). Estimates for the Netherlands in Bettendorf et al. (2015) are modest. Finally, estimates for 1–3 year old children are zero or negligible in studies from South Korea, Germany and Sweden (Busse and Gathmann, 2018; Lee, 2016; Lundin et al., 2008).

An important issue in comparing estimates across studies, is that the intensity of treatment, and hence the implicit scaling of estimates, differs widely. Goux and Maurin (2010), for instance, estimate a fuzzy RD-design using French census data, where school enrollment is instrumented with an age cutoff and the outcome is stated labor force participation. This generates estimates per child enrolled in school on a flow measure of labor supply. In contrast, Carta and Rizzica (2018) follow much of the literature on preschool aged children to consider reduced form effects on yearly measures of labor supply directly. To interpret these estimates, they consider changes in child care access that do not capture the intensity of use. Since new child care places according to the authors were opened in September, the utilization rates in the first year should be expected to be much lower than in following years. In this case, the estimates reported will understate the effects of the child care expansion on labor supply over a full year.

Our IV-strategy resolves the issue of partial treatment and scales the effect to the increased use of child care caused by the expansion. This should, on the one hand, ensure that our estimates are closer to the expected impact of the child care expansion over time, when child care places are available throughout the year. It should, on the other hand, facilitate the application of our estimates in other contexts, where the take-up rates may be expected to differ from what we observe in our data. Our IV-estimates can arguably be compared more directly to those in Goux and Maurin (2010), given that actual use in France was substantial after enrollment. In contrast, estimates in Carta and Rizzica (2018) should be scaled by the intensity of the reform to give

[1] See, however, Givord and Marbot (2015); Goux and Maurin (2010) for two studies on child care for 2-year olds and the studies on the Quebec reform (Baker et al., 2008; 2015; Haeck et al., 2015; Kottelenberg and Lehrer, 2013; 2017; Lefebvre et al., 2009) discussed below, which investigates child care for various ages from 0 to 5 years old.

[2] Throughout, we refer to married and cohabiting mothers interchangeably - our main sample consists of both married mothers and unmarried mothers living together with the child and the father of the child, as cohabitation without marriage is common in Norway.

[3] Note that this does not include the mechanical effect on the substantial cash-for-care benefit tied to child care use in subsidized child care, nor does it include the parental copayment. Combined, these imply a parental cost of full time child care use of about NOK 90,000 per year, more than canceling out the positive effects on after-tax income.

[4] In the US, for instance economically small effects are found by Gelbach (2002), Cascio (2009), Fitzpatrick (2010); Fitzpatrick (2012)) and Barua (2014). Similar effects are found in several European countries see e.g. Finseraas et al. (2017); Goux and Maurin (2010); Havnes and Mogstad (2011); Lundin et al. (2008), while several papers on a child care reform in Quebec suggest more sizable effects, see Baker et al. (2008), Lefebvre and Merrigan (2008), Lefebvre et al. (2009) and Haeck et al. (2015). For reviews of this literature, we refer readers to Blau and Currie (2006) and more recent overviews in Akgunduz and Plantenga (2018) and Morrissey (2016).

ATT-estimates before being compared to our reduced form estimates. These latter estimates are in fact quite comparable in size.

Mixed support for a tight link between parental labor supply and child care availability or prices is disappointing from a policy perspective, since increasing mothers' labor force participation is a key policy goal in many countries. Our estimates suggest that the modest effects could be explained by two things: First, the take-up of child care in the initial periods following child care expansion may not be complete. Indeed, in our case, the start of the child care year in August implies that the children who occupy the newly expanded slots will usually attend at most five months of care in the first year. Our study allows to refine previous estimates by exploiting data on the intensive margin of child care use rather than relying on crudely aggregated data. Second, as emphasized by e.g. Havnes and Mogstad (2011), substitution into formal care from informal sources rather than home care would suggest that effects of child care are smaller than might be initially expected. In this case, rather than releasing mothers to the labor market, child care is taken up by mothers who are already working and relying on some form of informal care arrangement. Our results then suggests that informal sources of care may be less important as an alternative to formal care for younger compared to older children. Both alternatives imply that the potential for child care policies to stimulate mothers' labor supply may be larger than suggested in much of the recent literature on preschool children.

The paper proceeds as follows: We first cover the institutional setting and the child care reform in Section 2, while Section 3 presents the registry data used for estimation, the samples of interest and some descriptive statistics. Section 4 presents our IV method with fixed effects. Results are found in Section 5, including persistence analysis. We perform a range of robustness checks to support our estimates in Section 6, while Section 7 concludes. Additional results, primarily for fathers and other caregivers, are found in an online appendix.

2. Institutional setting and the child care reform

Although the roots of the Norwegian child care system date back to the early 19th century,[5] the system of universal child care was introduced after WWII as a response to increasing female labor force participation and the goal of gender equality in the Nordic welfare model (Ministry of Education and Research, 1998). Increasing excess demand for formal care in the 60's and 70's led to the Kindergarten Act of 1975, and a strong increase in the supply of formal child care for preschool children (Havnes and Mogstad, 2011), eventually leading to a high coverage rate for preschool children by 1990.

Fig. 1 (a) shows the trends in child care access for preschoolers and toddlers from 1998–2012. Throughout, we use as our measure of child care access the reported child care coverage rate, i.e. the share of children of a particular age that are enrolled in any child care on December 15th of each year. By 2000, close to 80% of 3–5 year olds were enrolled in formal child care. At the same time, child care access was much lower for younger children. In 2000, less than 50% of 2-year olds and 30% of 1-year olds in Norway were enrolled in child care, and there was substantial excess demand for child care, see the discussion in Section 3 below.

This excess demand for formal child care was the background for the Kindergarten concord, a reform that was formally passed in the Norwegian Parliament in 2003 with broad bipartisan support, but whose main lines were agreed upon and made public the year before. A key goal of the reform was to facilitate parental labor force participation, under the premise that universal child care is central to promoting gender equality in the labor market (Ministry of Education and Research, 2002–2003) The reform aimed to offer affordable child care to all children, and to secure quality and diversity in child care services (Ministry of Education and Research, 2002–2003). The concord aimed to achieve these goals through increased subsidies, lower parental fees and investment subsidies for the construction of new child care slots. Also, while state subsidies were allocated based on a fixed piece rate per child in care, with children below 3-years old earning double rates, municipalities before the reform had local autonomy over their allocation of subsidies. In practice, municipal subsidies covered on average 34% of costs in municipal child care institutions but only 9% of costs in private institutions, even though costs were substantially lower in private institutions (St.prp. 1 (2003–2004)). The concord aimed to make it easier for private suppliers of care to enter the market by mandating equal economic treatment of private and public child care institutions.

Fig. 1 presents some important changes in the child care sector following the reform. Fig. 1(b) depicts the total investment in child care institutions over the period.[6] Note that most investments appear with a lag of 1–2 years, as they were applied for and disbursed after the slot was opened. The figure shows clearly how total investments increased rapidly following the reform. Fig. 1(c) shows the increase in state subsidy rates per child per year. Fig. 1(d) shows the changes in the composition of the costs covered by the municipality, the central government and parental fees. It is clear that the share of costs covered by parents has declined significantly. This figure also shows that municipal support was not reduced as a response to the increased government subsidies. The large overall increase in expenditures over the period is a result of both more children in care, higher subsidy rates, and an increasing share of toddlers, requiring more staff and resources per child.

A central part of the reform was the implementation of a maximum price on child care. This was implemented from 2004 and put a cap of 2,750 NOK on the monthly fee that could be charged from parents for a full time slot. In 2006, the cap was lowered further to 2250 NOK per month.[7] In addition, all families with children below three years old *who were not enrolled in subsidized child care* were eligible for a substantial cash benefit under the cash-for-care (CFC) scheme. This implies that the price to parents was effectively about twice the level suggested by the cap.[8] Throughout the period we consider, formal childcare was highly regulated. To be eligible for the generous subsidies, both private and public child care institutions were subject to strict quality criteria, e.g. to the ratio of pedagogical staff to children, opening hours, parental involvement and available playing space per child (Kindergarten Act, 2005). The maximum price and the strict regulation of quality, ensures that formal child care institutions are relatively homogenous in terms of observable attributes of quality and price.

The responsibility for meeting child care demand fell on municipalities, who could expand care by direct investment or with the help of private suppliers. The expansion, however, seems to be hard to predict for local governments, who often struggled to meet demand over the years following the reform. The most common reasons for undersupply reported by the municipalities themselves were a) demographic shocks, particularly unexpected changes in the number of children, b) local geographic mismatch of supply and demand, c) unexpected increases in demand and d) unexpected delays in construction projects (Asplan Viak, 2007). Many municipalities also seem to be overly optimistic in regards to covering all child care demand, and write in their annual reports in several successive years that they expect to reach full child care coverage the following year (Asplan Viak, 2004–2010).

[5] See Ministry of Education and Research (2008–2009) for a thorough treatment.

[6] All monetary values are given in thousands of 2017-NOK unless otherwise noted.

[7] Nominal values, comparable numbers in 2017 NOK are 3600 NOK for the 2004 cap and 2800 NOK for the 2006 cap. NOK/USD $\approx$ 8.4.

[8] Over the period the CFC benefit varied from 2200 to 2800 NOK per month in 2017 NOK per child, equivalent to approximately 270–350 USD per month using the December 2017 exchange rate. Note that these changes were nationwide and should not affect our estimation which is based on variation across municipalities.

As illustrated in Fig. 1(a), the reform resulted in a sharp increase in municipal child care access for 1 and 2-year olds. Over a nine year period, child care coverage rates for toddlers increased by around 40 percentage points for both age groups. In our empirical analysis, we aim to evaluate the impact of this massive increase in child care access on the labor supply of mothers and fathers using the variation in timing of the expansion between municipalities.

3. Data and descriptive statistics

Our data is based on rich administrative registers available from Statistics Norway, and cover the entire resident population. The data contain individual information on demographics (e.g. sex, age, immigrant status, marital status, number of children), socioeconomic status (e.g. years of education, income, taxes paid, employment status), and municipality of residence. Income and employment data are collected from tax records and other administrative registers. The household information is from the Central Population Register, which is updated annually by the local population registries and verified by the Norwegian Tax Authority. We also have access to national registry data on municipal child care enrollment reported by the child care institutions themselves and aggregated at the municipality level. Importantly, the data contain unique personal identifiers that allow us to match children to parents, grandparents and other caregivers residing with the child. Data on child care use comes from the cash for care-registers as detailed below. We finally utilize various municipality characteristics from Statistics Norway, including data on rural and urban population, employment by sector and gender, political representation and municipal income and spending.

To define the **population of interest**, we start with all two-year olds residing in Norway in 2002–2008. Following most of the literature, we focus attention on the youngest child in the household, and exclude multiple births and children with younger siblings born to the same mother or father.[9] We also exclude children with unknown mothers and a handful of households with more than one child in the sample (born to different parents but living in the same household). This leaves us with a sample of 325,396 children.

We next identify all working-age household members of these children (ie. aged 18 to 67). We consider cohabiting mothers and fathers to be parents that live in the same household as the child. Notice that our definition of cohabiting parents includes both married and non-married couples. If both parents are not present in the child's household, we identify suitable stepfathers or stepmothers from age, family relations and gender of the present parent. This allows us to identify the likely caregivers relevant for the vast majority of the children in the sample. From population registers, we can also identify mothers and fathers that do not reside with the children, as well as grandmothers and grandfathers. Our main samples of interest will be cohabiting mothers and fathers, as well as single mothers and non-residing fathers. We have also considered other potential caregivers, like step-parents, but these samples are small and do not give enough power to allow meaningful analysis.

To measure individual **child care use**, we exploit the administrative registers of cash-for-care (CFC) recipients, available since 1999. As discussed above, under the CFC-scheme, all families with children below three years old *who were not enrolled in subsidized child care* were eligible for a substantial cash benefit. From the CFC-registers, we know which children receive the CFC-benefit each month and the exact amount disbursed. As long as eligible parents take up the benefit, we can infer the child care use of each child. Because children who attend subsidized care part time are eligible for part time cash for care, we can also measure the intensity of child care use each month. This approach has the advantage of giving a comprehensive measure of child care use over the year, to allow correctly scaling the impact of child care expansions that are not immediately taken up. Our final measure gives for each child the fraction of full-time equivalent months of child care use during the year.[10] Note that child care use by this definition will relate only to the use of subsidized child care. This is consistent with our definition of child care coverage rates and in line with the definitions used in most of the previous literature where unsubsidized child care is included in the category of "informal care".

To measure **number of available child care slots** in each municipality, we use the number of children in child care by age from municipal reports, measured on December 15th each year, when enrollment in the regular calendar year is completed. Our measure of **child care access** converts these numbers to coverage rates by taking the number of slots as a fraction of the population of the same age as measured on January 1st in the following year, around two weeks after the head count child care measure. These data are readily available from Statistics Norway.[11] Similar measures are used regularly in the literature, see e.g. (Dustmann et al., 2013; Havnes and Mogstad, 2011; Nollenberger and Rodríguez-Planas, 2015).

One potential issue with these measures is that in clearing markets, child care access will be determined jointly by supply and demand. Exploiting variation in child care access may then pick up demand shocks, which would raise the potential for reverse causality and estimation bias. Anecdotal evidence suggests, however, that there was large undersupply of child care to toddlers in Norway in the period we consider. If so, the variation in child care access we exploit should be driven by variation in the supply of child care, rather than by increased utilization of existing child care places. In order to verify the anecdotal evidence, we would ideally like to have data on the number of applications in each municipality. Unfortunately, this data is not available. As an alternative, we secured access to survey data on waiting lists collected on behalf of the Ministry of Education. The survey collected information from municipalities on the number of children 0–2 years old who were on waiting lists for child care in each municipality by September 20th over 2004–2009 Asplan Viak (2004–2010). By adding children enrolled and children on waiting lists, we can construct a measure of the child care application rate.[12]

Fig. 2 plots the child care coverage rate and our measure of the child care application rate for 0–2 year olds over our estimation period. Throughout the period we can measure, the application rate is substantially higher than the coverage rate, by between 10 and 15 percentage points, indicating excess demand. Note that these data were collected for the combined group of 0–2 year olds only, so do not speak directly to the instrument we will use below (child care coverage for 2-year olds). However, this is likely to cause us to underestimate the level of rationing, since the vast majority of children will neither apply nor enroll in child care in the first year of life, when their parents are on parental leave.[13] The online appendix provides figures of the

[9] Fertility could be considered endogenous to the child care expansion. In Section 6, we include also children with younger siblings to verify that this sample restriction is not driving our results.

[10] We thus consider a full year of half time care (about 20 h per week) to be equivalent to one half year of full time care. We have experimented with other measures, without this substantially changing the results. In practice, part time care is relatively rare, as shown in Fig. 6 below.

[11] http://www.ssb.no/statistikkbanken, table 04683. In a few cases a municipality will have a coverage rate slightly above 1 because children from neighboring municipalities attend care. These have been adjusted to 1.

[12] In a contemporary report for the Ministry of Education, ECON Analyse (2004) concluded that hidden demand from parents who do not apply when they want care for their children, was negligible.

[13] In 2002, Norwegian parents were entitled to 42 (52) weeks of parental leave with 100% (80%) wage compensation, expanded to 43 (53) weeks in 2005 and 44 (54) weeks in 2006. Parents are further entitled to one year each of unpaid leave in immediate continuation of regular parental leave.

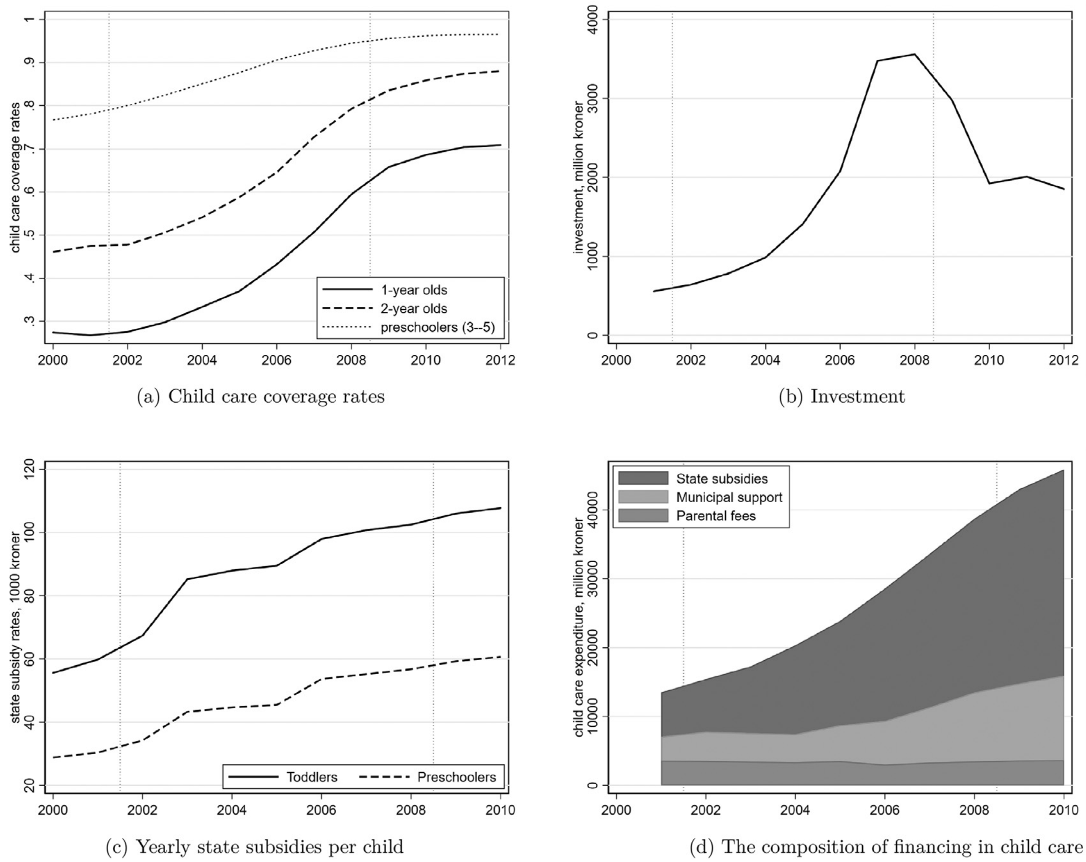


Fig. 1. Child care access, investments and financing in the 2000’s. *Note:* Child care coverage rates are defined as the number of children in care to the overall population of children in that age group. Investmentsrefers to total gross investment in the child care sector from municipal and institutional reports. Yearly state subsidies per child are the national subsidies paid to municipalities per child in care. Composition of financing are from municipal and institutional reports. *Sources*: Statistics Norway and regjeringen.no.

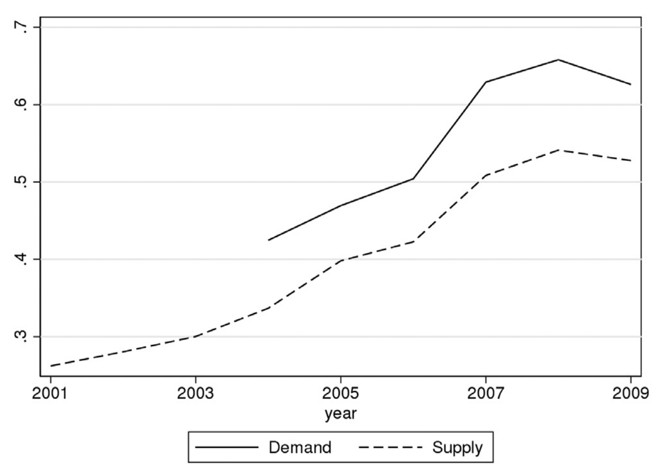


Fig. 2. Child care application and coverage rates for 0–2 year olds. *Note:* Application rates are constructed by adding the number of children in care to the reported number of children on waiting lists for care and dividing by the number of children in the municipality. Note that the numbers are not directly comparable to Fig. 1(a), because they include 0 and 1-year olds.*Sources:* Reports from Asplan Viak (2004–2010) and Statistics Norway.

distribution of rationing across municipalities and years for which we can measure, indicating that more tnan 55% of the municipality-years we can measure, covering around 90% of the children in our sample, were subject to at least some degree of rationing, a fair share of them to more severe rationing. In our robustness analysis below, we use this data to investigate the potential for reverse causality by restricting focus to more severely rationed municipalities.

To measure **labor supply** we exploit two alternative data sources. First, we use yearly earnings from wages and self-employment collected from tax records. This measure includes parental leave and sickness absence benefits. As outcome variables, we use both earnings directly, as well as dummy variables for labor market participation based on the basic amounts in the Norwegian Social Insurance Scheme (used to define labor market status, determine eligibility for unemployment benefits as well as disability and old age pension). Specifically, we follow Havnes and Mogstad (2011) and construct dummy variables for employment and full-time equivalent status that equal one if earnings exceed 2 and 4 basic amounts, respectively, and zero otherwise. In 2017, one basic amount was around 94,000 NOK, or approximately 11,300 USD. The tax records additionally provide data on total tax payments, transfers and benefits that we use as additional outcomes to evaluate the impact on public spending and income.

Second, we use data from the matched employer–employee register, with information about all public employees and for about 80% of private employees. These data give information on start and end dates of employment spells, and bracketed information on contracted hours in each spell. From these data, we construct yearly measures of labor supply as the number of weeks during the year when contracted hours are above four hours and above 30 h. A caveat in using these outcomes is that we set labor supply to zero when information is missing in the data. This will tend to drive down the level of labor supply compared to the true level. This should not, however, affect our estimates unless the pattern of missing observation is changing over time and these changes are correlated with changes in child care coverage rates.

Table 1
Descriptive statistics for all children at age 2, 2002–2008.

Children						
Variable	All years	All years SD	2002	2004	2006	2008
Female	0.49	0.50	0.49	0.49	0.49	0.49
Older maternal siblings	1.00	1.06	1.01	0.99	0.99	0.97
Child care use	0.47	0.42	0.32	0.39	0.51	0.67
Child care access	0.66	0.16	0.51	0.59	0.73	0.83
Observations	325,396		48,603	45,363	45,939	47,279

Table 2
Descriptive statistics for caregivers at child age 2.

		Cohabiting mothers	Cohabiting fathers	Single mothers	Non-residing fathers
A. Outcome variables					
Earnings		260.2	518.7	119.7	322.0
Tax		66.11	155.4	27.93	93.57
Transfers, excl. cash for care		56.74	18.49	198.1	50.51
Employed		0.678	0.914	0.309	0.699
Full-time eq.		0.398	0.841	0.150	0.539
Weeks of	4 h	37.17	43.58	21.35	32.17
employment above	30 h	26.44	41.69	13.15	29.02
B. Control variables					
Age		32.65	35.55	29.52	32.46
Immigrant		0.097	0.074	0.087	0.080
Years of education		14.53	14.19	12.60	12.33
Number of children		2.029	2.056	1.719	1.818
below 6 years old		1.517	1.516	1.282	1.331
below 13 years old		1.893	1.878	1.553	1.596
below 18 years old		1.995	1.987	1.666	1.718
Observations		285,860	285,670	33,564	29,545

Note: Outcome and control variables are defined in Section 3. Monetary values in 1,000s of 2017 NOK.

3.1. Descriptive statistics

Summary statistics for the sample of children and the four main samples of caregivers are given in Table 1 and 2. We see that the sample of children is well balanced with respect to gender, and that children have one older sibling on average. The average child care use is 0.47, corresponding to 5.6 months of full time care. Table 2 gives descriptive statistics for four groups of caregivers: cohabiting mothers, cohabiting fathers, single mothers and non-residing fathers. We notice immediately in panel A the substantial differences in terms of labor supply. Single mothers are least attached to the labor market, with only 31% employed and 15% in full time-equivalent employment. In comparison, cohabiting fathers are strongly attached to the labor market, with 91% employed and 84% full time. Also cohabiting mothers are much more attached to the labor market than single mothers, with about 68% employed, and 40% in full time. These differences are mirrored in their earnings, where cohabiting fathers on average make almost twice that of cohabiting mothers, and more than four times that of single mothers. We also note that labor supply measured in terms of the number of weeks with contracted hours above 4 and 30 hours reflect well the overall picture from the earnings-based measures.

Panel B of Table 2 presents means of control variables for the four groups of caregivers. We note that cohabiting mothers and fathers have about 14 years of completed education. The table also shows single mothers and non-residing fathers have substantially less education and are around three years younger than cohabiting parents. Meanwhile, fathers are about three years older than mothers, and somewhat less likely to be of immigrant background.

To get a first look at the trends in labor force participation among these groups, Fig. 3 investigates labor force participation over the period for the four groups of caregivers separately. For comparison, we also include the trend for working age women with no school aged children. We note that just over 60% of these women are employed, of which about two thirds is full time. For both measures, however, the overall trend is relatively flat over the period.

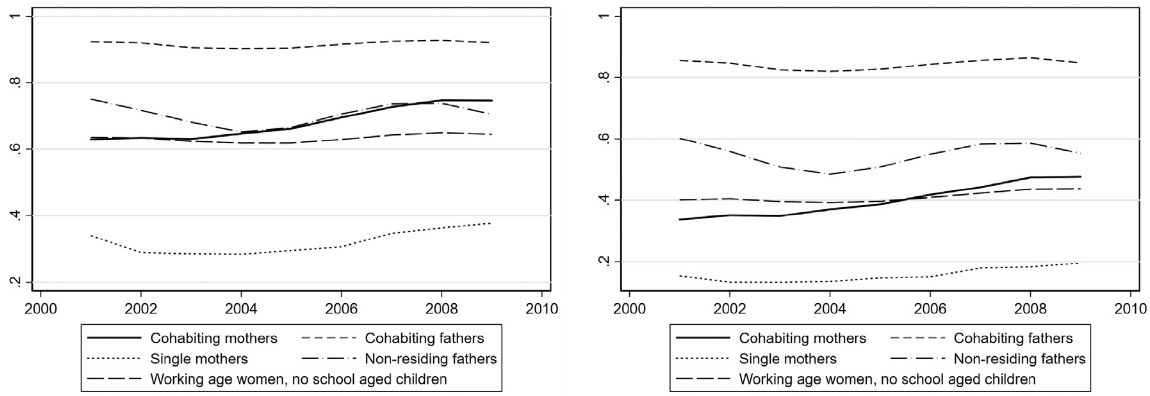
If the large increase in child care availability has an effect on parents' labor supply, we would expect that this was evident in the figure by an increase in labor supply relative to the observed pattern for the rest of the labor force. For cohabiting fathers in our sample, the trend is very similar to the overall trend, with little change over time, though with employment above 90%, the level is clearly higher. In contrast, cohabiting mothers experienced substantial increases in the labor force participation over these years. In particular, employment rates increased by about 10 percentage points, driven largely by full time-equivalent employment among cohabiting mothers. For single mothers, there is also a slight upward trend, but the picture is less clear.

Finally, Table 3 presents means of control variables for cohabiting mothers biennially over our estimation period 2002–2008. We note that most variables exhibit a relatively flat trend over time, which suggests that the composition of mothers is stable over time. The exceptions are the growth in immigrant background from about 8%–12% and a small increase in education. Similar patterns are seen for the other groups of caregivers, which are reported in the online appendix.

4. Empirical strategy

The most straightforward way to estimate the effect of child care on labor supply is to regress a measure of labor supply on child care use. This ignores, however, that child care use is endogenous to parents' labor supply decisions. Clearly, parents that enroll their children in child care are likely to be more closely tied to the labor market. This simple approach is therefore likely to yield estimates of how child care use affects labor supply that are biased upwards.

Imagine instead a social experiment that randomized child care access at the municipal level. This randomization breaks the correlation between child care access and unobserved determinants of parental



(a) Employment (b) Full time-equivalent

Fig. 3. Labor force participation for caregivers of 2-year olds and working age women without children below 18, 2001–2009. *Note:* Employment and full time-equivalent is defined as earnings above 2 and 4 basic amounts (BA), respectively, see Section 3. $1\ BA \approx NOK\ 94,000 \approx USD\ 11,300$.

Table 3
Descriptive statistics for cohabiting mothers, 2002–2008.

	2002	2004	2006	2008
A. Outcome variables				
Earnings	221.3	240.7	271.2	313.7
	(181.9)	(230.0)	(211.0)	(220.1)
Tax	57.8	64.3	67.8	76.1
	(63.8)	(70.5)	(74.7)	(138.2)
Transfers,	66.8	58.8	52.9	47.7
excl. cash for care	(57.9)	(64.0)	(60.2)	(58.9)
Employed	0.63	0.65	0.70	0.75
	(0.48)	(0.48)	(0.46)	(0.43)
Full-time eq. employment	0.35	0.37	0.42	0.47
	(0.48)	(0.48)	(0.49)	(0.50)
Weeks of employment above 4 h	35.0	36.3	37.8	40.0
	(22.4)	(22.2)	(21.3)	(19.9)
30 h	23.5	25.4	27.3	29.9
	(24.6)	(24.8)	(24.7)	(24.4)
B. Control variables				
Age	32.3	32.6	32.8	32.9
	(4.81)	(4.83)	(4.83)	(4.92)
Immigrant	0.084	0.090	0.10	0.12
	(0.28)	(0.29)	(0.30)	(0.32)
Years of education	14.1	14.4	14.7	14.9
	(2.93)	(2.93)	(2.95)	(2.97)
Number of children	2.05	2.03	2.03	2.00
	(0.99)	(1.00)	(0.98)	(0.98)
below 6 years old	1.51	1.52	1.52	1.52
	(0.58)	(0.58)	(0.58)	(0.58)
below 13 years old	1.91	1.89	1.89	1.87
	(0.84)	(0.83)	(0.83)	(0.82)
below 18 years old	2.01	2.00	1.99	1.97
	(0.94)	(0.94)	(0.92)	(0.92)
Observations	42,587	39,648	40,444	41,817

Note: The table gives biennual means and standard deviations (in parentheses) in the estimation sample. Variables are defined in Section 3. Monetary values in 1,000s of 2017 NOK.

labor supply. Comparing labor supply of parents in municipalities with and without child care access would give a reduced form estimate of the effect of child care access on parental labor supply. Comparing child care use in municipalities with and without child care access would give a first stage estimate of the effect of child care access on child care use. Taking the ratio between the two, we would get an IV-estimate of the effect of child care use on parental labor supply.

The intention of our IV-approach is to mimic this hypothetical experiment. We exploit the staggered expansion in child care following the 2002 child care concord, which generated large spatial and temporal variation in child care access. The distribution across municipalities of child care availability for 2-year olds is illustrated in Fig. 4, where we draw municipal child care coverage rates over 2002–2008. The figure shows both the strong increase in child care access over the period we consider, and the large variation across municipalities.

We start by considering the reduced form effect of this expansion, relating changes in child care access to changes in labor supply. This suggests the following model:

$$y_{ikt} = \kappa_k + \eta_t + \delta CC_{kt} + \mathbf{X}_{ikt}\boldsymbol{\gamma} + \zeta_{ikt} \tag{1}$$

where y_{ikt} is a measure of labor supply for caregiver i in municipality k in year t, κ_k and η_t are municipality and year fixed effects and $\boldsymbol{X_{ikt}}$ is a vector of parent and child controls. The parameter of interest δ captures the reduced form effect of increases in child care access on parents' labor supply. Given exogeneity, i.e. that changes in access are as good as random to changes in labor supply, this gives an unbiased estimate of the average impact of an additional child care place on the labor supply of parents. Such reduced form estimation will usually be most informative about the return to the public investment, and is as such often regarded to give the most policy relevant estimates. Indeed, this is the margin that is estimated in most papers in the literature on how child care affects parental labor supply, and we present these estimates below for comparison with the existing literature.

There are at least two reasons why accounting for the intensity is important. in our setting. First, new child care places are typically created in August, at the start of the school year. This mechanically gives a maximum child care use of around 42% for most children, i.e. 5 out of 12 months.[14] This should lead the reduced form to underestimate the impact of a full year of child care access. Second, to the extent that child care places are not utilized to capacity, child care institutions have quite extensive discretion to scale provision to actual use, by e.g. adjusting employee hours. In this case, treating child care places as uniform independent of utilization will overestimate the cost of child care provision. Note that this would imply that it is not correct in our case to use the reduced form estimates to evaluate cost efficiency.[15]

Given that new child care places affect labor supply of parents only by affecting the use of child care among parents, we can use an IV-approach to get an estimate of the impact of child care on labor supply which accounts for the intensity of treatment following the expansion. This suggests the following IV-strategy: For each municipality and every

[14] This could in principle be taken into account directly by scaling the estimates to the potential take-up, i.e. dividing the reduced form-estimates by 5/12.

[15] Of course, we could scale the costs to reflect the intensity rather than the estimates, but this would in any case require a first stage estimate for the intensity of use and would introduce statistical uncertainty also in the cost measures, complicating the analysis.

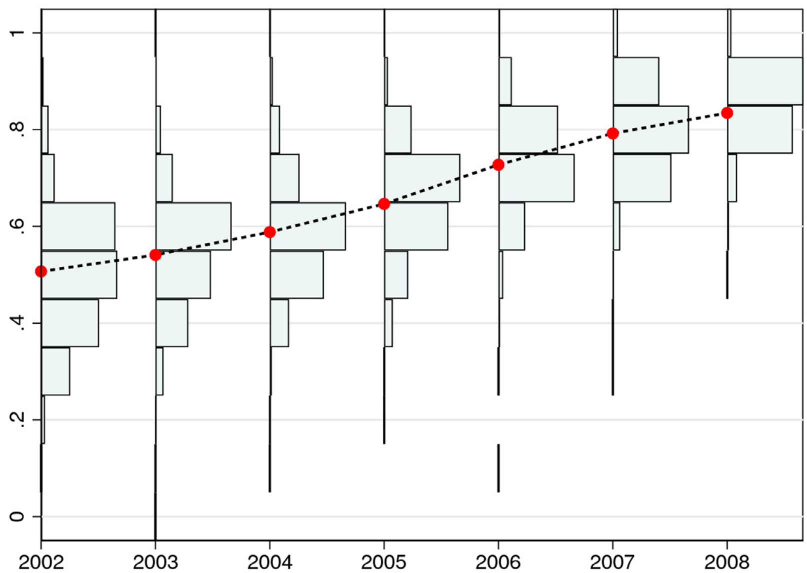


Fig. 4. The distribution of child care access for 2-year-olds across municipalities over 2002–2008. *Note*: The figure draws the mean child care coverage rate nationally over time (bullets and dashed line) and the distribution across municipalities (bars), weighted with population size. Data, estimation sample and variable definitions are discussed in Section 3.

year, we instrument individual child care use over the year, m_{ikt}, with the child care coverage rate, CC_{kt}. Specifically, we estimate the following 2SLS-model in our sample of caregivers of 2-year old children:[16]

$$y_{ikt} = \alpha_k + \tau_t + \beta m_{ikt} + \mathbf{X}_{ikt}\theta + \epsilon_{ikt} \tag{2}$$

$$m_{ikt} = \tilde{\alpha}_k + \tilde{\tau}_t + \pi CC_{kt} + \mathbf{X}_{ikt}\tilde{\theta} + \tilde{\epsilon}_{ikt} \tag{3}$$

where α_k and τ_t are municipality and year fixed effects. Note that the municipality and year fixed effects, along with the rationing of the child care market discussed above, ensures that we exploit variation only from newly opened child care places. In our baseline model, we include controls for the vector $\mathbf{X}_{ikt}$ which contains child and caregiver characteristics: Child characteristics include dummies for gender and month of birth, the number of older siblings, and dummies for both parents' level of completed education.[17] Caregiver characteristics include age, age squared, a dummy for immigrant background and the number of children below ages 6, 11 and 18. We also include dummies for level of education to control flexibly for returns to education, including potential sheepskin effects. Descriptive statistics for these variables are reported in Table 2. In our robustness analysis, we show that the overall pattern of our estimates does not depend on the inclusion of controls. Standard errors are clustered at the municipality level and robust to heteroskedasticity.

In the first stage of the IV model, we regress our measure of child care use over the year on municipal child care access. Our model therefore accounts for the intensity of child care induced by the child care expansion following the reform. Fig. 5(a) clarifies the data used in our first stage in the main sample of cohabiting mothers. In the left panel, we plot a dummy for whether the child used any child care in December from the cash-for-care register versus municipal child care access for our sample of cohabiting mothers, which is from municipal reports in the same month. Data have been binned by percentile. As is evident from the figure, the points line up closely along the diagonal, as expected, and with a correlation close to 1.[18] This indicates that the two measures line up very well in December, verifying that our measure of child care use is sound.

In the middle panel of Fig. 5(a), we perform the same analysis replacing the binary December measure with the continuous measure of child care use over the entire year that we use in our main analysis. This causes a downward shift which reflects that the average child who was enrolled in child care in December did not use child care the entire year. Specifically, the child care places counted in December were utilized on average only around 70% of the year. This average utilization rate will reflect mostly children who enrolled for the first time during the year they turn 2 years old, but also the (relatively few) children who were enrolled part time. The correlation is still close to 1, however, indicating that the average place is utilized to roughly the same extent in municipalities both with low and with high levels of child care access.

In the right panel of Fig. 5(a), we have controlled for municipality and year fixed effects to get residualized measures of child care use and access. This reflects the variation that we use in our IV-model, coming from newly opened child care places. As these mostly open in August, it is no surprise that the children who occupy these slots in December use less child care than the average two-year old in care in December, who may have started in the previous year and attended child care through the spring. Specifically, the association in the right panel indicates that a newly opened child care slot causes about one half year of child care use in that year.[19]

To understand more about the take-up of child care, Panel A of Fig. 6 shows the child care use of children in our sample between ages 14 and 35 months, separately for children born in different months. For expositional clarity, we have highlighted children born in January. We see that these children are almost uniformly more likely to use child care as they grow older. Importantly, we see that there is little take-up over the year, with the vast majority of children starting child care in August: For children born in January, about 20% start child care in August of the year they turn two years old (when they are 20 months of age) and a further 20% in August of the year they turn three years old (when they are 32 months of age). In comparison, only about 5% start child care in the intermediate months.

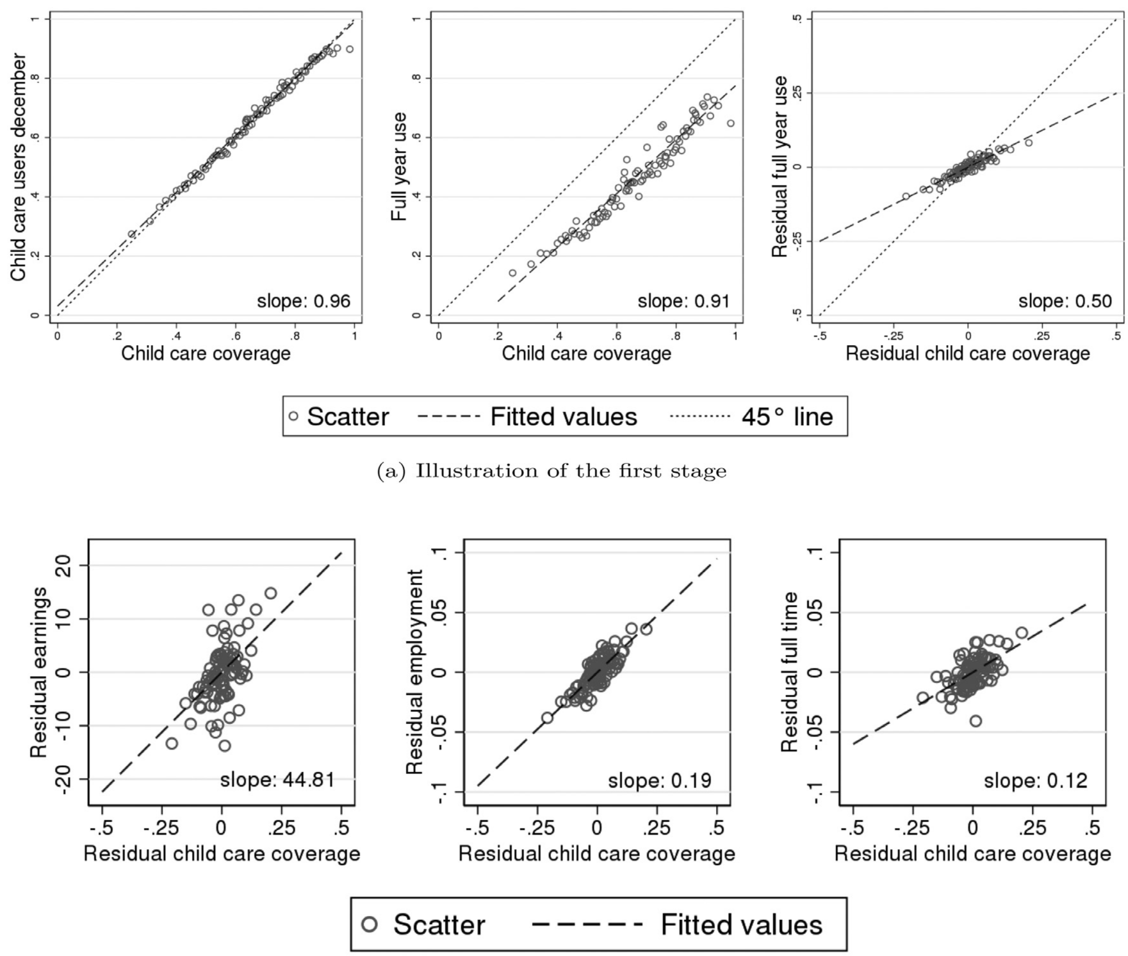
A concern could be that many children use child care only part time, and perhaps expand their use during the year. In Panel B of Fig. 6, we

[16] All models are estimated using the Stata command reghdfe (Correia, 2014).

[17] Rather than dropping a handful of observations with missing education data, we use a separate dummy for caregivers with missing education.

[18] The slight variation in the two measures could be due to a combination of measurement error, moves during the year, that some eligible parents do not apply for cash-for-care, that our sample does not include children with younger siblings or that some children attend care in neighboring municipalities.

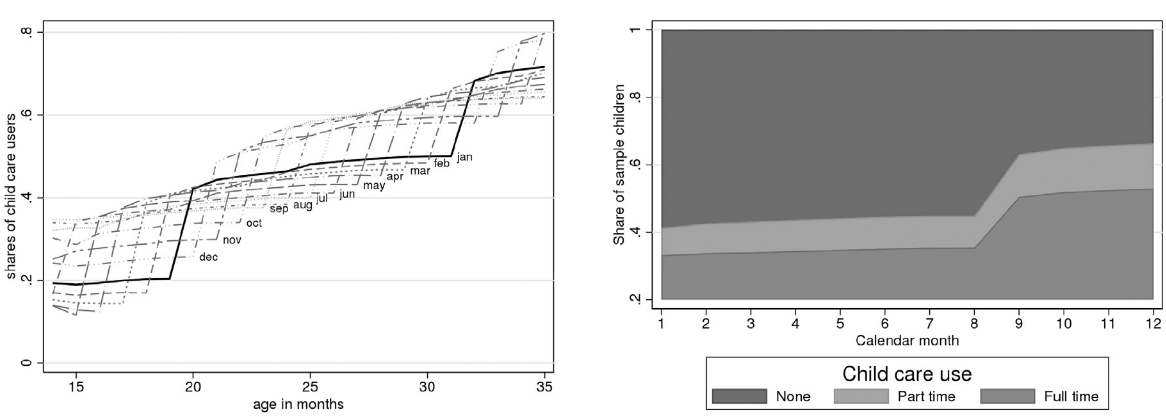
[19] Note that the above discussion shows that we are not regressing individual outcomes on group means, as in a poorly designed peer effects analysis. For a detailed discussion of why this should be a bad idea, see Angrist (2014).



(a) Illustration of the first stage

(b) Illustration of the reduced form

Fig. 5. Individual child care use (top) and married and cohabiting mothers' labor supply (bottom) vs. child care access. *Note:* Scatter plots of individual child care use (top) and outcomes (bottom) against child care coverage rates for 2-year olds, binned by percentile. For variables labeled "residual", we have removed year and municipality fixed effects.



(a) Child care use by birth month and age

(b) Child care use by calendar month

Fig. 6. Patterns of child care use across age and calendar months. *Note:* Panel a) plots the average share of child care users at child age 14 to 35 months of age by birth month. The graph for children born in January is highlighted for expositional ease. Panel b) depicts shares of children using part-time, full time or no child care by calendar month during the year they turn 2, inferred from cash-for-care data. "None" indicates zero hours, "Part time" indicates 0–40 h, and "Full time" indicates 40 or more hours.

show the share of children in our sample who are enrolled in part time, full time and no child care in each calendar month of the year they turn two years old. Again, we note that the vast majority of children enroll in August. Furthermore, we see that children are usually enrolled in full time care and that the share of part time use is relatively small and quite stable over the year.

To illustrate the reduced form of our model, Fig. 5(b) show scatter plots for our three main outcomes against child care access after removing municipality and year fixed effects. We note that there is a strong positive association between child care access and labor supply, and that the relationship is well approximated by a linear function, especially for our measures of employment and full time-equivalent employment. Specifically, the correlation in the left panel suggests that the child care expansion increased earnings of cohabiting mothers by around 45,000 NOK. Scaling this by the associated first stage from above, yields an IV-estimate of around 89,000 NOK, close to the IV-estimates reported below.

There are two potential selection issues when estimating our parameter of interest β. The first is selection on gains: If parents in some municipalities respond more strongly to child care access than others, and this is correlated with the child care expansion, then our estimates of how child care affects parental labor supply will differ from the average effect in the population. It will be a consistent estimator of the effect for the subpopulation that is affected, but not for the whole population. In the terminology of Imbens and Angrist (1994), our estimate will be a local average treatment effect (LATE). In the presence of heterogeneous treatment effects, our estimates reflect the average treatment effect among compliers, roughly parents of children that took up the newly available slots. We think this is a policy relevant group whether or not they are representative of the population at large.

Second, we could worry about selection on unobservables: If expansions in child care access are, for instance, positively correlated with other determinants of labor supply, then our estimates will be biased upwards. Note first that the fixed effects will control for all time invariant differences between municipalities, and for all common time shocks. Our concerns should therefore be focused on *changes* in potential confounders *within municipalities*.

The most immediate threat to our identification is arguably that child care demand is driving the variation in child care access that we exploit, which might bias our estimates upwards. In Section 3 above, we discussed this issue at some length, and concluded that this is unlikely, since demand is substantially higher than supply over the period we study. To investigate this issue further, we now consider the patterns of expansions of care across municipalities.

To illustrate our empirical approach we recenter the data so that year 0 is the year with the largest increase in child care access for each municipality. We then graph the changes in child care supply over time alongside changes in child care use and key outcome variables around year 0, after removing municipality fixed effects. To facilitate interpretation, we have not removed year effects. These trends are drawn in Fig. 7.

We start by considering the timing of events in panels (a)–(d) of Fig. 7. If our strategy is sound, then we would expect that changes in the labor supply of caregivers should follow changes in child care access and not vice versa. The figure shows the association between child care access (dashed line), on the one hand, and child care use and labor supply (solid lines), on the other. By construction, we see a substantial increase in child care access in year 0, with a jump of around 15 percentage points in the coverage rate. This corresponds to a little under half of the overall increase in child care access over the period we consider, so will represent a substantial fraction of the variation that we use in our estimations below. Because we have not removed year effects, it should not be surprising that the trend in child care access is positive also in other years.

In panel (a) of Fig. 7, we see that child care use follows closely the trend in child care access, with a substantial jump in child care use in year 0. In panels (b)–(d), we further see that labor supply also increases at the same time for both earnings and our measures of employment and full time-equivalent. In contrast, we see no indication of shocks in the outcomes systematically preceding large expansions in child care access. On the contrary, the trends in the outcomes are remarkably smooth both before and after year 0, lending support to our empirical strategy. In panels (e)–(h) of Fig. 7, we present similar graphs for key control variables. Since these can be controlled for, they do not pose a direct threat to our estimates. Strong correlation between child care expansion and observable factors would, however, raise concern about potentially uncorrelated time shocks. Unlike child care use and the labor supply outcomes that we considered above, the controls change little around year 0. This suggests that these variables are not driving the association between labor supply and child care. The exception is years of education, where there is a slight increase at year 0. In practice, we will control for all of these variables in our estimation, to guard against potential omitted variables bias.

Next, we check the balance of our sample across municipalities that expand child care access at different times. To this end, we run a regression of child care access for two year olds on municipality and year fixed effects and a set of municipality characteristics measured in 2001, before the reform, interacted with year dummies:

$$CC_{kt} = \rho_k + \sigma_t + \boldsymbol{\varphi_t V_{k,2001}} + \mu_{kt}$$

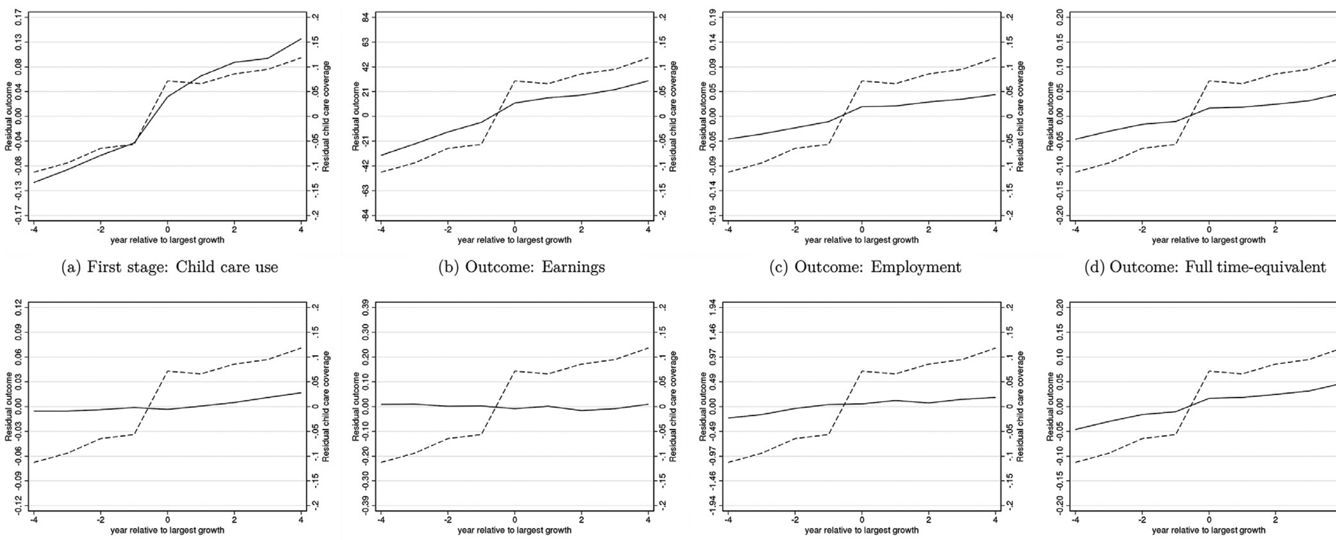
where $\boldsymbol{V_{k,2001}}$ is a vector of pre-reform municipality characteristics. Fig. 8 plots coefficients on the interactions between year and a set of key characteristics (see footnote 29 for details), denoted $\boldsymbol{\varphi_t}$ above. A systematic relationship over time would suggest that municipalities of particular types have a different expansion profile. Estimates are presented in Fig. 8 and show that there is little systematic correlation with initial characteristics of the municipality. Unsurprisingly, low initial child care access for 2-year olds has a strong relationship with the rollout of access for this age group.[20] It is reassuring, however, that the rollout appears to be mostly uncorrelated with other municipality characteristics. In particular, there is no indication of a relationship between the rollout of child care and the initial level of female labor force participation. A particular concern might be that our estimates may be influenced by changes in labor demand. Fig. 3 shows that the overall trend in employment, as measured by employment of working age women without school-aged children, was roughly flat over the period we study (a similar picture can be drawn for male employment). While overall economic trends cannot impact our fixed effects estimates in any case, we could worry that differential economic changes across municipalities may be correlated with child care expansion and cause bias in our estimates. Fig. 8 shows that initial differences in municipality characteristics, including income and unemployment, does not seem to predict instrument rollout. Finally, our estimates are stable to the inclusion or exclusion of a local labor market control (municipal employment share of working age men without school-aged children). We therefore conclude that differential growth in labor demand is unlikely to cause bias in our estimates.

Finally, even with the large set of control variables that we include in our model, one may still worry about changes in unobservable determinants of labor supply. After our main results, we therefore further probe the validity of our empirical strategy by assessing the stability of the IV-estimates to alternative specifications, and by performing placebo tests. These specification checks by and large lend support to our estimation strategy.

5. Results

Table 4 shows the reduced form estimates from our model among the four main groups of caregivers. Focusing on cohabiting mothers, an additional slot in child care leads to increases in earnings of around 31,000

[20] Note that this relationship becomes mechanical when municipalities approach full coverage.



(a) First stage: Child care use (b) Outcome: Earnings (c) Outcome: Employment (d) Outcome: Full time-equivalent

(e) Control: Immigrant (f) Control: Number of children (g) Control: Age (h) Control: Years of education

Fig. 7. Event study graphs for cohabiting mothers: Changes in treatment (top left) and outcomes around the time of the largest growth in child care access *Note:* The figure graphs outcomes, treatment and controls over time after removing municipality fixed effects. Data are recentered so that year 0 is the year with the largest growth in child care coverage rates for each municipality after netting out yearly shocks. The left axes (solid lines) is sized to go from −0.4 to 0.4 standard deviations of the variable in the sample of cohabiting mothers, but labels still indicate absolute values of the variable. The scale of the child care coverage rate(right axis, dashed line) is in percentage points. Data, estimation sample and variable definitions are discussed in Section 3. More details in online appendix.

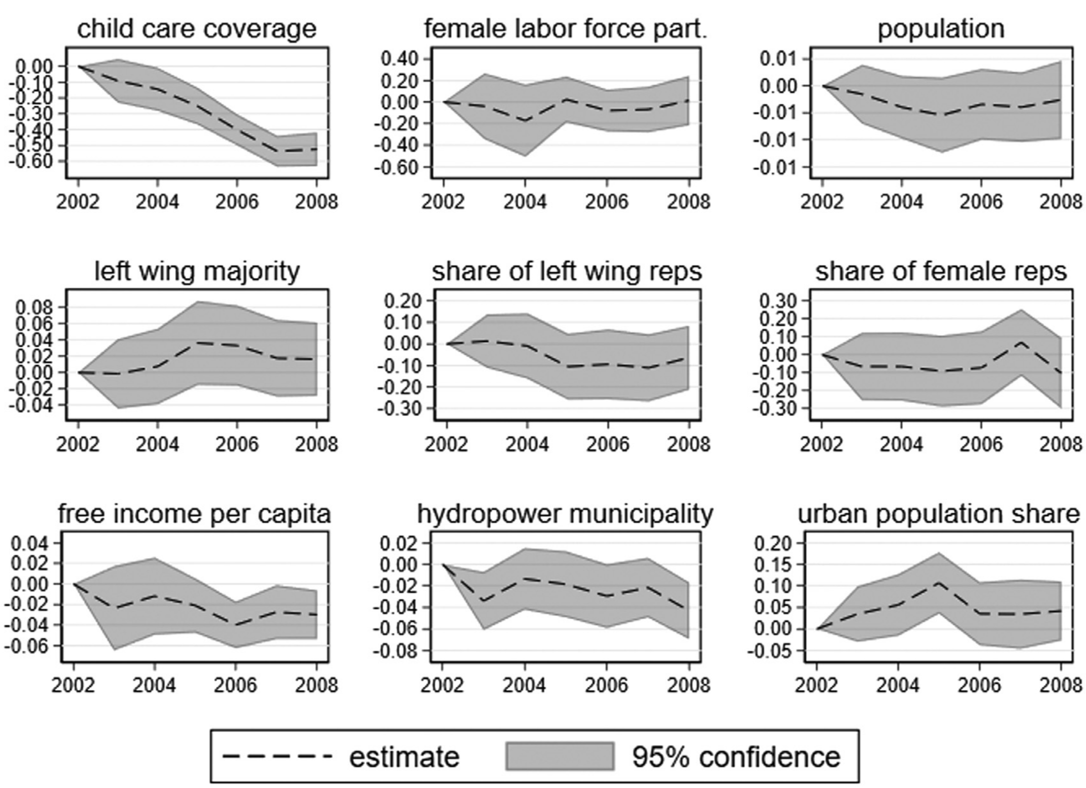


Fig. 8. What predicts timing of child care expansion? *Note:* The figure plots estimates from a regression of the child care coverage rate on municipality and time fixed effects and a set of pre-reform characteristics interacted with year dummies. Plotted are the coefficients on the interaction of the characteristics (measured in 2001) with time, which can be interpreted as the difference in expansion of child care for a municipality with one unit higher level of that characteristic in 2001 in a particular year to the average expansion that year. Free income per capita and population have been standardized so that the units are in standard deviations. Standard errors are clustered at the municipality. See footnote 29 for a list of characteristics.

Table 4
Reduced form estimates of the effect of an extra child care slot on annual labor market outcomes.

		Cohabiting mothers	Cohabiting fathers	Single mothers	Non-residing fathers
Earnings		31.0***	0.33	−10.9	−2.19
		(5.71)	(17.9)	(10.3)	(23.4)
		221.3	477.2	102.1	304.9
Taxes		4.20	−9.51	−14.2***	5.57
		(2.66)	(10.2)	(4.86)	(10.9)
		57.8	139.9	28.1	82.9
Transfers		−0.30	0.35	20.7**	6.84
excl. cash for care		(2.03)	(2.28)	(8.41)	(7.18)
		66.8	17.4	209.5	47.2
Employed		0.15***	−0.011	0.0097	0.046
		(0.017)	(0.010)	(0.033)	(0.040)
		0.63	0.92	0.29	0.72
Full-time		0.081***	−0.0055	−0.036	0.037
		(0.017)	(0.012)	(0.023)	(0.039)
		0.35	0.85	0.13	0.56
Weeks of	above 4h	4.91***	−0.55	1.74	−0.48
employment		(0.56)	(0.66)	(1.72)	(1.96)
		35.0	43.4	19.5	31.7
	above 30h	5.16***	−0.65	−1.90	0.19
		(0.62)	(0.73)	(1.50)	(2.02)
		23.5	41.6	11.5	28.8
	285,860	285,670	33,561	29,272	

Note: Reduced form estimates of the outcomes as listed in row headers on the instrument (child care access), fixed effects and controls. Pre-reform means of outcomes are underlined and refer to grandparents of 2-year olds in 2002. * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$)

NOK, 15 percentage points increased probability of being employed and 8 percentage points increased probability of full time-equivalent employment. As discussed, because new child care places are usually opened in August, these estimates will capture only a part year effect of the increase in child care access. Effects on our yearly measures of labor supply are therefore likely to understate the effect of a full year of child care use. To correctly interpret these estimates, we should correct for the intensity of the treatment that parents experience.

We thus turn to our IV-model to get estimates that more correctly scales the reduced form impact. Baseline results from equations (2) and (3) for the four main samples of interest are reported in Table 5. In panel A, we report estimates from our first stage. The instrument is relevant and strong, with *F*-statistics on the excluded instrument above 50 in all samples. Estimates in panel A indicate that new child care places are utilized on average about 43–47% of a full year, i.e.the equivalent of about five to five and a half months in that year. Notice that the scaling of the reduced form estimate implied by this first stage estimate turns out to match well with the basic scaling to potential treatment that we discussed above. This suggests that utilization of new child care places is close to saturation in the months when they are available. Looking across groups, coefficients are similar, suggesting that the take-up of child care slots is relatively homogeneous across children with different living arrangements.

In panel B of Table 5, we report IV-estimates for a number of different outcomes, where estimates on child care use are from separate regressions for each outcome. The estimates should be interpreted as the effect of adding a new child care place which is utilized to saturation over the year. Our first stage estimates suggest that the difference between these estimates and the estimates in Table 4 above, are mostly driven by the addition of new places in August rather than at the start of the year. As a point of reference, we also include in Table 5 the mean of the dependent variable in 2002, prior to the child care expansion (underlined).[21]

Our IV-estimates suggest substantial labor supply responses among the large group of **cohabiting mothers**. In particular, employment and full time-equivalent employment among cohabiting mothers is estimated to increase by 32 and 17 percentage points, respectively. Similar results are found for weeks of employment from the matched employer–employee register. Compared to the pre-reform means, these effects imply an increase of around 50% in the mean employment rates of cohabiting mothers of two year olds. These strong responses suggest that child care plays an important role in getting mothers of small children (back) into the labor market. In line with the findings below of persistent effects the following year, we interpret these results as driven by hastening the return to more or less full time work rather than effects at the intensive margin of labor supply.

When we consider earnings of cohabiting mothers directly, our estimates indicate that child care use caused an increase of about 66,000 NOK per year, a 30% increase over the pre-reform mean.[22] At the same time, we estimate that taxes paid increase by an insignificant 9,000 NOK, while there is no effect on transfers. Taking the ratio of the (insignificant) estimate of 9000 NOK increased taxes to the 66,000 NOK increased earnings yields a very low average tax rate on the additional earnings of around 13.5%, close to half of the average tax rates of these mothers before the reform. This is likely caused by nonlinearities in the tax schedule that makes the marginal tax rate different from the average tax rate, particularly for low earnings. This means that simple back-

[21] OLS-estimates are reported in the online appendix for comparison. These estimates are much larger than our IV-estimates, in line with our intuition that mothers who work more have higher demand for child care.

[22] It is common to take logs of the dependent variable in order to interpret the estimates as percentage changes. The presence of zeros in the outcome and the fact that the shift into employment is an important margin of response to child care makes this unattractive in our case. Other alternatives, relying on various more or less arbitrary transformations of the outcome variable that involve adding a small number to all observations before taking logs, in practice puts large weights on small changes close to zero. The inverse hyperbolic sine transformation is arguably less extreme in this case, but still strongly emphasizes changes in the lower part of the distribution. In line with this, estimated elasticities for both single and cohabiting mothers are very large when we apply this transformation.

of-the-envelope calculations may overestimate the extent to which the costs of child care subsidies are offset by increases in tax revenue.

The estimates for **cohabiting fathers** show no evidence of labor supply responses to child care, with precise point estimates close to zero and insignificant for all outcomes. This is in line with findings in previous literature and the contention that father's labor supply is largely independent of the family situation. Also in modern Norway, it seems, mothers are the primary caregiver and stay home with the child when child care is not available.

Our point estimates for cohabiting mothers imply an increase in after tax income of around 57,000 NOK. This ignores, however, the mechanical reduction of the cash-for-care benefit which, at around 50,000 NOK, largely cancels out the increased earnings in the first year. To verify this formally, we have also included estimates on disposable income directly. These estimates confirm that the total effect on households' finances is roughly null. Add to this the co-payment for child care, at about 40,000 NOK per year, and it is clear that child care is actually quite costly to parents even when increased earnings are taken into account. This suggests that there are non-pecuniary benefits associated with work or that future benefits of returning to work early are substantial. While we do not have data to investigate the former, we investigate persistence in the labor supply response below.

Turning to **single mothers**, the point estimates in column 3 of Table 5 are not sufficiently precise to allow for strong conclusions about the labor supply effects. As we see below, however, the mean impact seems to hide substantial heterogeneity over the earnings distribution, with positive impacts at the bottom and negative effects at the top. Meanwhile, the estimated effects on taxes and transfers are surprisingly large, with an estimated drop in taxes paid of about 30,000 NOK and an increase in transfers of nearly 50,000 NOK. A potential explanation for these results is that child care allows single mothers to (barely) meet the activity requirements linked to the transitional benefit for single mothers.[23] Note, however, that the results for single mothers are less robust to our specification checks below, suggesting that we should be careful in putting to much emphasis on these results.

Estimates for **non-residing fathers** are not sufficiently precise to draw firm conclusions, but are in general close to zero, lending little support to child care as an important driver of labor supply decisions for this group.

We have also investigated the labor supply response of **grandparents**, under the hypothesis that grandparents may be relevant informal caregivers for young children. If so, and if grandparents are still attached to the labor market, we would expect to see effects on the labor supply of working age grandparents. Estimates for these groupds are reported in the online appendix, and suggest that there is no response among maternal grandparents, but modest effects on the labor supply of paternal grandparents. Interestingly, while paternal grandmothers work more, paternal grandfathers work less when the child is in child care. This may suggest that paternal grandmothers are important informal caregivers to some 2-year olds, while there is an indirect income effect on grandfathers.

We can use our results to calculate the net contemporaneous fiscal cost to the government of an additional child in full time care at the time of enrollment. We do this for the current year only, but note that we estimate persistence in the labor supply response below, finding little evidence of increased taxes in the following year, indicating that any impact of persistence on these calculations should be relatively minor. Using 2017-prices and 2008 as the base year, the state subsidies for one full time child in care was 107,500 NOK. In addition, the municipalities covered on average 28.4% of the total subsidies to the sector, adding up to an estimated 150,000 NOK in state and municipal subsidies. The increased taxes from the additional labor supply of cohabiting mothers is 9000 NOK, meaning that around 6% of the costs of the subsidies are offset by increased tax revenues. In addition, the cash for care benefits are mechanically reduced by around 50,000 NOK, increasing the share of cost offset to around 39%. Note that this ignores the negative, but insignificant impact, on father's taxes, which would drive this number down.

5.1. Heterogeneity over the earnings distribution

Providing low-cost child care can also be a tool to level the playing field between families from different backgrounds. To investigate this, we construct dummy variables for earnings brackets based on the same basic amounts we used to construct our measures of employment (around 94,000 NOK). Specifically, we construct seven mutually exclusive dummy variables that are equal to one if earnings fall below one basic amount, between one and two basic amounts, between two and three basic amounts, and so on, with the last category being earnings above six basic amounts (around 564,000 NOK). We then estimate our IV-model using these dummy variables successively as dependent variables. This allows us to study binned parts of the marginal distribution of earnings to get an impression of what parts of the distribution are affected by the child care expansion.[24] Since the overall impact on fathers is small, we consider only mothers in this exercise.

Fig. 9 shows the marginal distribution in 2002, i.e. the mean of our dummy variables, as bullets. The IV-estimate is represented in bars with associated confidence intervals. In the left panel, we report estimates for cohabiting mothers. The bullets (measured on the right axis) show that prior to the child care expansion, almost 25% of cohabiting mothers earned below one basic amount, leaving them essentially out of the labor force. For the remaining cohabiting mothers, the distribution of earnings was relatively flat, with 10–15% in each bracket. In contrast, the distribution for single mothers reported in the right panel of Fig. 9 is heavily skewed, with 62% in the lowest earnings bracket and population shares below 10% and steadily declining as we move up the distribution.

The bars in Fig. 9 (measured on the left axis) give the estimated impact of child care use on the probability of ending up in each bracket, and reveal strong heterogeneity. In particular, cohabiting mothers seem to be shifting away from both of the two lowest brackets, and into the two middle brackets close to the threshold for full time-equivalent employment that we used above. The insignificant point estimate on the mean earnings of single mothers above, turns out to hide substantial heterogeneity over the earnings distribution. Estimates in Fig. 9 suggest that single mothers shift away from the lowest bracket and into the lower middle brackets, pointing towards an increased incidence of small part time (or low wage) employment, but also away from the top bracket, though estimates are less precise in this smaller sample.

5.2. Persistence

Though estimated impacts on earnings, transfers and taxes did not support the notion of child care as a public finance boon in the short run, we may question whether there are long run effects that can help mitigate the costs. We therefore investigate whether there is persistence

[23] The transitional benefit ("overgangsstønad") is paid to single mothers who are at least 50% employed, in work-related training or actively searching for a job. The transitional benefit is 2.25 times the basic amount, i.e. about 210,000 NOK per year. The benefit is subject to regular income tax and is reduced by 45% of any income above 0.5 basic amounts.

[24] Note that the evidence here is suggestive, since the estimates will reflect the effect on the distribution, not the distribution of effects. That is, though the estimates are sufficient to conclude about how the child care expansion affected the distribution overall, and hence for most welfare analyses, they do not provide information about the number of winners and losers or the size of individual gains or losses, unless we are willing to make the bold assumption of rank invariance (i.e. no reshuffling in the distribution with treatment). See e.g. Koenker and Hallock (2001) or Koenker et al. (2017) for extensive discussions of these issues.

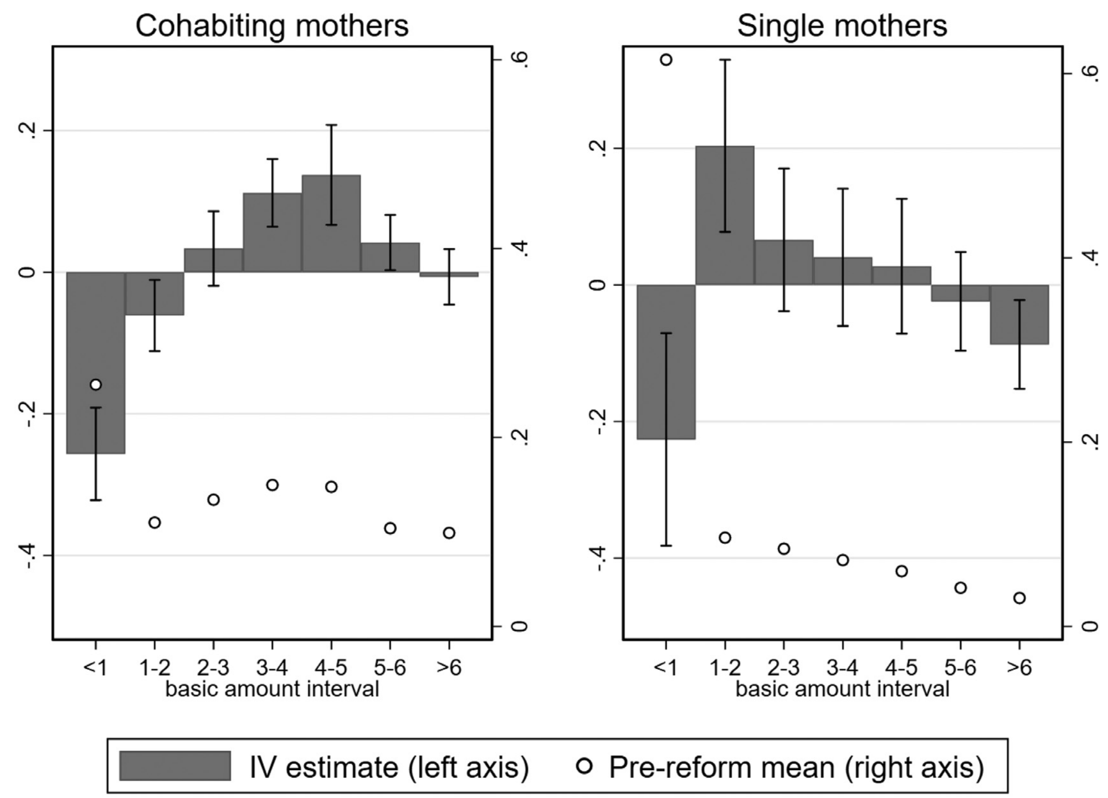


Fig. 9. The impact of child care use on the earnings distribution of mothers. *Note:* Estimates are from equations (2) and (3). Outcome and control variables are defined in Section 3. 95% confidence intervals are clustered at the municipality level and robust to heteroskedasticity. Pre-reform means refer to parents of 2-year olds in 2002.

in the labor supply response. To this end, we use the outcome variables measured one to four years into the future.[25] Notice that we maintain the conditioning on control variables from year t in order to avoid issues associated with so-called "bad control" variables.[26] Specifically, we estimate the following 2nd stage equation.

$$y_{ik,t+s} = \alpha_k + \tau_t + \beta_s m_{ikt} + \mathbf{X}_{ikt}\theta + \epsilon_{ikt} \quad (4)$$

where $y_{ik,t+s}$ and β_s is the outcome and the effect of child care use, respectively, s years after the child turned two years old. β_s captures the full effect of the expansion-induced increase in child care use on future labor supply. For brevity, and because estimates are not sufficiently precise on other groups, we focus on cohabiting mothers. Estimates for the other groups may be found in the online appendix.

Table 6 presents estimates from the IV model for cohabiting mothers. Column 2 presents estimates from the baseline model for comparison, while columns 3–6 present results on labor supply measured at child age 3 to 6l. While effects fade out over time, our estimates suggest that there is relatively strong persistence over the first years following child care expansion. In particular, the impact on earnings is about the same in year $t+1$ as in year t. Note, however that this is partly due to the fact that the school year straddles the years t and $t+1$, such that, in practice, the child care expansion will be of similar magnitude in the two years.[27] The impact on taxes is also roughly the same as in year t, suggesting that the marginal tax rates on the extra income in year $t+1$ is very similar to year t at around 15%.

The fact that the coefficients for employment and full time is almost equal in year $t+1$ suggests that the effects we have found so far is driven by mothers returning to full time work, not to part time work. Estimates drop significantly in $t+2$, and are mostly insignificant from $t+3$ onwards. Nonetheless, this provides evidence of persistence in labor market response to child care start throughout the child's preschool years, which might be driven by child care largely being an absorbing state. All estimates are insignificant by year $t+4$, when the child turns 6 and enters primary school. Although not as long lasting as the effects from Haeck et al. (2015) or Lefebvre et al. (2009), this supports the findings from these papers that child care may have effects on maternal labor supply that outlast the period of actual use.

Fig. 10 illustrates our persistence estimates for cohabiting mothers by predicting outcomes with and without child care use and comparing this to the actual trend for pre-reform mothers. Specifically, we construct the following variable to predict counterfactual labor supply for mothers of 2-year olds in 2002 s years later, where $\hat{\beta}_s$ is from Table 6 and m_{it} is the actual care use.

$$\hat{y}_{it+s}(m) = y_{it+s} + \hat{\beta}_s(m - m_{it}) \quad (5)$$

If a child used no child care in 2002, then $m = 0$ and we effectively subtract the predicted effect of the child care use in 2002. If they had used full child care in 2002, then $m = 1$ and we effectively add the predicted

[25] Because the expansion of child care for 2-year olds may affect labor supply both through the persistence of labor supply, e.g. through increased experience, it may also have an indirect impact by raising child care use at later ages. Unfortunately, we can only measure individual child care use up until the age of 35 or 36 months, and so are unable to address this concern directly. This does not, however, have any bearing on the reduced form, but the IV-estimates should be interpreted with some care.

[26] Some of the controls, like education, could be considered endogenous to child care access. This is more likely the longer the time period between the treatment and the outcome. See Angrist and Pischke (2008) for a discussion of bad controls.

[27] Panel A of Fig. 6 shows that most children start child care one year later, in August of the year they turn 3 years old. The policy should therefore shift parents into about the same amount of child care use in years t (Aug–Dec) and $t+1$ (Jan–June).

Table 5
IV-results: The impact of full-year child care use on annual labor supply.

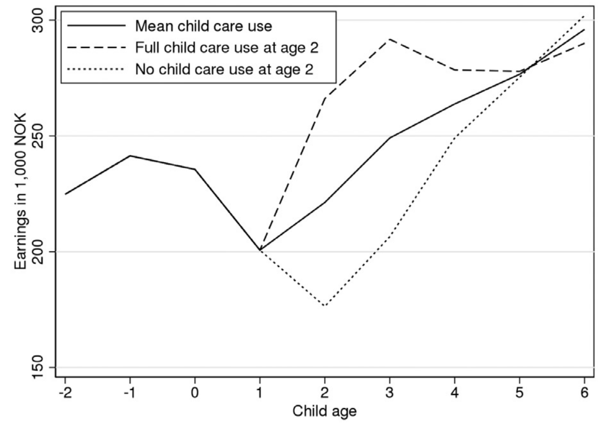
		Cohabiting mothers	Cohabiting fathers	Single mothers	Non-residing fathers
A. First stage					
Child care access		0.47***	0.47***	0.43***	0.43***
		(0.044)	(0.042)	(0.059)	(0.051)
F		114.9	122.4	51.8	68.9
B. Second stage					
Earnings		65.8***	0.70	−25.5	−5.15
(1,000 NOK)		(15.8)	(38.2)	(24.2)	(55.2)
		221.3	477.2	102.1	304.9
Disposable income		2.14	20.6	3.6	−1.83
(1,000 NOK)		(7.96)	(18.3)	(18.0)	(38.1)
		263.2	355.8	316.0	269.6
Tax		8.92	−20.3	−33.2***	13.1
(1,000 NOK)		(6.23)	(20.2)	(9.44)	(25.3)
		57.8	139.9	28.1	82.9
Transfers,		−0.64	0.74	48.5***	16.1
excl. cash for care		(4.34)	(4.82)	(18.5)	(16.6)
(1,000 NOK)		66.8	17.4	209.5	47.2
Employed		0.32***	−0.024	0.023	0.11
		(0.027)	(0.020)	(0.076)	(0.096)
		0.63	0.92	0.29	0.72
Full-time		0.17***	−0.012	−0.084	0.087
equivalent		(0.027)	(0.025)	(0.057)	(0.092)
		0.35	0.85	0.13	0.56
Weeks of	4 h	10.4***	−1.17	4.06	−1.14
employment		(1.70)	(1.34)	(4.19)	(4.59)
above		35.0	43.4	19.5	31.7
	30 h	11.0***	−1.38	−4.45	0.45
		(1.33)	(1.49)	(3.56)	(4.76)
		23.5	41.6	11.5	28.8
Observations		285,860	285,670	33,561	29,542

Note: Estimates are from equations (2) and (3). Outcome and control variables are defined in Section 3. Standard errors in parentheses are clustered at the municipality level and robust to heteroskedasticity. Pre-reform means of outcomes are underlined and refer to parents of 2-year olds born in 2002. Monetary values in 1,000s of 2017 NOK. * (p<0.10), ** (p<0.05), *** (p<0.01)

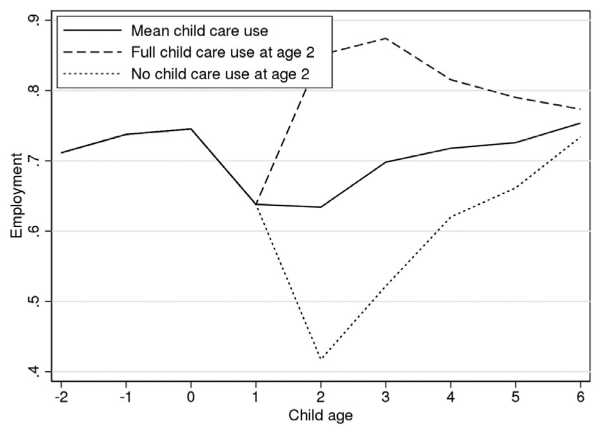
Table 6
Persistence: IV impacts of a full year of child care use on annual, lagged outcomes, cohabiting mothers.

Outcome		*t* (baseline)	$t+1$	$t+2$	$t+3$	$t+4$
Earnings		65.8***	62.5***	21.5*	1.86	−8.78
(1,000 NOK)		(15.8)	(17.9)	(11.7)	(12.3)	(13.5)
		221.3	296.1	311.8	330.1	350.6
Tax		8.92	9.37	−0.41	−2.60	0.70
		(6.23)	(6.03)	(5.06)	(5.37)	(7.01)
		57.8	75.4	79.4	85.0	92.2
Transfers		−0.64	−5.17	−4.11	11.0*	8.73
excl. cash for care		(4.34)	(5.83)	(6.20)	(5.88)	(5.46)
		66.8	66.6	69.7	70.2	71.0
Employed		0.32***	0.26***	0.14***	0.094***	0.029
		(0.027)	(0.032)	(0.025)	(0.031)	(0.028)
		0.63	0.75	0.77	0.78	0.80
Full time-		0.17***	0.23***	0.098***	0.079**	0.045
equivalent		(0.027)	(0.027)	(0.037)	(0.037)	(0.033)
		0.35	0.47	0.48	0.51	0.54
Weeks of	4 h	10.4***	10.4***	5.15***	0.77	−1.49
employment		(1.70)	(1.65)	(1.18)	(2.55)	(3.89)
above		35.0	38.3	38.8	41.3	43.4
	30 h	11.0***	12.1***	7.34***	3.84	1.22
		(1.33)	(1.56)	(1.34)	(2.85)	(4.46)
		23.5	27.4	27.8	29.9	31.6
Observations		285,860	284,641	283,833	283,267	282,769

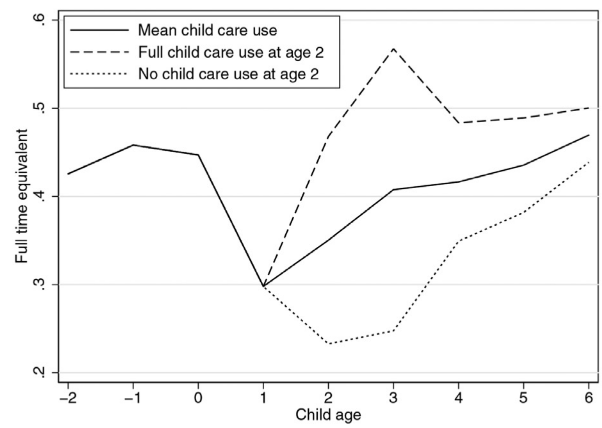
Note: Estimates are from equation (2), with outcomes measured 1–4 years later. Standard errors in parentheses are clustered at the municipality level and robust to heteroskedasticity. Pre-reform means of outcomes are underlined and refer to parents of 2-year olds in 2002. Monetary values in 1,000s of 2017 NOK. * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$)



(a) Earnings



(b) Employment



(c) Full time-equivalent

Fig. 10. Persistence: Labor supply of cohabiting mothers around birth, pre-reform means and counterfactuals with no and full child care use at age 2. *Note:* To construct the graph, we apply the ÷ estimates from our baseline and persistence model in Equation (5), cf. Table 5. The solid line plots predicted labor supply of cohabiting mothers of children born in 2000 if child care use had stayed at the observed 2002-level, i.e. $m = 0.32$ in Equation (5). The lower dashed line subtracts from the solid line the estimated effect of child care use of 2-year olds, i.e. $m = 0$ in Equation (5). The upper dotted line adds the effect of increasing to full child care use at age 2, i.e. $m = 1$ in Equation (5).

effect of increasing child care use from the observed level in 2002. For comparison, we also plot outcomes using the mean child care use in 2002 ($m = 0.32$). Note that this should be interpreted as the predicted effect for a complier around this level, not the overall effect of full child care access. Because effects are likely to be different outside of the complier group, particularly in the group of never-takers, who might be expected to be less strongly attached to the labor market, we should be cautious about extrapolating to the population at large.

Fig. 10 shows the estimated time series for the mean of earnings, employment and full-time equivalent employment for mothers. We first note that while about 75% of mothers are employed prior to birth, less than 65% are employed in the year following birth. Similarly, around 45% of mothers work full time prior to birth while about 30% work full time in the year following birth. This is reflected in their earnings, which drop by about 20% on average in the year following birth.[28] The figure illustrates how the increase in child care use at age 2 caused by the reform is predicted to have hastened mothers' return to work, and that effects are persistent, as mothers stay more attached to the labor market also in the following years. Note that the large drop in predicted labor supply without child care at age 2 should not be surprising, as wage compensation expires at most 54 weeks after birth.

6. Specification checks

Despite the reform and the undersupply of formal care in the period, the expansion in child care that we exploit is, of course, not randomized. In this section, we challenge our empirical strategy and investigate alternative explanations for our findings. Specification checks for cohabiting mothers are reported in Table 7. For brevity, results for other groups may be found in the online appendix. Overall, robustness checks for cohabiting mothers support our empirical strategy.

6.1. Common time trends

The basic identifying assumption in our study rests on common time shocks across municipalities with different growth rates in child care access. We report a series of specification checks challenging this assumption in panel A of Table 7. We first perform a placebo test where we use as the outcome the labor supply of mothers in year $t-4$, i.e. two years before giving birth, which should not be affected by changes in child care access that occur several years later and should be unrelated to pregnancy. If our estimates are, in contrast, picking up secular

[28] Note that the generous Norwegian parental leave arrangements, see footnote 13, may explain why earnings and measures of employment don't fall further: In practice, almost all Norwegian mothers take several months of parental leave, many for more than a year, with up to 44 (54) weeks of 100% (80%) wage compensation in our sample. Parental leave benefits are included in our earnings measure.

Table 7
Specification checks: IV results of a full year of child care use on annual labor supply.

	Earnings	Empl.	Full time	Weeks > 4h	Weeks > 30h	*N*
No controls	88.9***	0.37***	0.23***	12.8***	13.4***	285,860
	(24.3)	(0.036)	(0.029)	(2.32)	(1.86)	
Controlling for lagged	50.3***	0.28***	0.15***	8.68***	8.44***	279,996
dependent var. $(t-3)$	(14.8)	(0.027)	(0.027)	(1.74)	(1.62)	
A. Common time trends						
Placebo: Outcome	1.80	0.069***	0.029	3.01**	3.27*	274,644
in $t-4$	(9.93)	(0.025)	(0.026)	(1.27)	(1.86)	
Municipality-specific	91.3***	0.33***	0.17***	11.9***	13.8***	285,860
linear time trends	(18.2)	(0.054)	(0.049)	(2.41)	(2.64)	
Municipality-specific	94.3***	0.34***	0.19***	13.5***	17.0***	285,860
quadratic time trends	(22.3)	(0.064)	(0.063)	(3.06)	(3.38)	
Interacted	99.2***	0.31***	0.18***	12.6***	11.3***	285,555
time shocks	(14.8)	(0.043)	(0.040)	(1.90)	(2.08)	
B. Reverse causality: Excess demand (2004–2008 only)						
Any waiting lists	83.1***	0.31***	0.18***	9.51***	10.3***	202,261
	(15.1)	(0.038)	(0.041)	(1.64)	(1.84)	
Waiting lists above 10	77.2***	0.24***	0.27***	6.50***	9.07***	118,351
percent of population	(22.4)	(0.051)	(0.049)	(1.72)	(1.81)	
C. Alternative drivers						
Endogenous fertility:	57.6***	0.29***	0.17***	8.90***	9.41***	346,309
include younger siblings	(15.6)	(0.026)	(0.026)	(1.39)	(1.27)	
Population growth:	83.2***	0.32***	0.18***	11.3***	12.4***	285,690
instrument = *log*(*slots*)	(16.3)	(0.039)	(0.035)	(1.94)	(1.93)	
Control for child care	74.9***	0.33***	0.19***	11.1***	11.5***	285,860
access for older kids	(14.5)	(0.029)	(0.029)	(1.72)	(1.48)	
Selective migration:	51.4**	0.34***	0.16***	8.22***	10.4***	274,630
residence fixed in $t-3$	(23.0)	(0.032)	(0.032)	(1.68)	(1.80)	
D. Family-specific fixed effects						
Baseline,	64.7***	0.23***	0.17***	6.63***	8.64***	71,509
sample > 2 children	(17.0)	(0.044)	(0.047)	(1.76)	(2.25)	
Individual fixed effects	49.2***	0.19***	0.15***	3.62*	3.78	71,509
sample > 2 children	(15.2)	(0.050)	(0.053)	(1.94)	(2.35)	

Note: The table reports IV-estimates on child care use from a series of specification checks described in the main text for cohabiting mothers. Specification checks for other caregivers are provided in the online appendix. Standard errors in parentheses are clustered at the municipality level and robust to heteroskedasticity. Monetary values in 1,000s of 2017 NOK. * (p<0.10), ** (p<0.05), *** (p<0.01)

differences in the growth of maternal labor supply over time which is correlated with child care access, then we would expect the placebo estimates to yield effects that go in the same direction as estimates using contemporaneous outcomes. The placebo estimates are small for most outcomes, supporting our estimation strategy. We note, however, some significant impacts for the employment margin which may be a worry. This estimate is, however, substantially smaller than our baseline results.

We next allow for different time trends in different municipalities. In the second and third rows of panel A, we admit municipality-specific time trends by including a linear time trend (row 2) and a linear and quadratic time trend (row 3) as controls in our baseline specification. Reassuringly, our estimates are stable to this inclusion, and are, if anything, larger than in the baseline.

Of course, we cannot be sure that unobserved trends are linear or quadratic. In row 4 of panel A, we instead allow for different time shocks to labor supply depending on pre-reform characteristics of each municipality. Note that we cannot include year-by-municipality fixed effects, since this is the variation we exploit in our identification strategy. Instead, we interact the yearly shocks with pre-reform characteristics, estimating the following specification.

$$y_{ikt} = \alpha_k + \tau_t + \beta m_{ikt} + \boldsymbol{\varphi_t V_{k,2001}} + \boldsymbol{X_{ikt}\theta} + \epsilon_{ikt} \tag{6}$$

$$m_{ikt} = \tilde{\alpha}_k + \tilde{\tau}_t + \pi CC_{kt} + \boldsymbol{\tilde{\varphi}_t V_{k,2001}} + \boldsymbol{X_{ikt}\tilde{\theta}} + \epsilon_{ikt} \tag{7}$$

Where φ_t is the time-varying coefficient on the prereform characteristics, denoted $\boldsymbol{V_{k,2001}}$. In this set we include a range of political, economic and geographic characteristics measured in 2001, before the sample period.[29]. Again, we find that our estimates are stable to this inclusion, cf. the fourth row in panel A of Table 7.

6.2. Excess demand

Our empirical strategy relies on growth in child care access being uncorrelated to growth in labor supply within municipalities. As discussed in Section 4, a potential worry could be that markets are not rationed, in which case growth in child care access could be driven by increased demand, and not vice versa. To investigate this, we narrow down our sample to municipalities with significant excess demand, where such reverse causality should be less of an issue. To this end, we exploit the data on waiting lists for 0–2 year olds. In panel B of Table 7, we reduce our sample first to municipality-years with children on the waiting lists and then further to municipality-years where waiting lists make up at least 10% of the children below 3 years in the municipality, and run our baseline model for the available years 2004–2008, when data on waiting lists are available.[30] Estimates in Panel B are similar to our baseline estimates.

[29] Specifically, we include the share of left wing representatives in the municipal council, a dummy for left wing majority, the share of women in the municipal council, the non-earmarked income per capita in the municipal accounts, a dummy for municipalities with hydropower income, the share of the population living in urban areas, the initial female labor force participation, initial levels of child care access and population.

[30] Note that baseline results for 2004–2008 are very close to results for the full period.

6.3. Alternative drivers

In panel C of Table 7, we investigate some alternative drivers that might explain our estimates. First, if caregivers with high levels of labor supply move to municipalities that will expand child care, our estimates could be driven by selective migration. To test for this, we ignore recent changes of residence: We consider a mother's municipality of residence to be the one where she resided in year $t-3$, the year before giving birth. If selective migration was driving the results, we should see estimates dropping towards zero as we limit this possibility. In contrast, estimates are relatively stable. We have also estimated our IV-model using as the dependent variable a dummy for whether the mother moves to a different municipality between the year before birth and the year when the child turns two. This gives no indication that selective migration is likely to be important in our setting.

Second, in line with previous literature we have considered the youngest child. If fertility is endogenous to child care availability this restriction might imply conditioning on an endogenous variable and could bias our estimates. To check whether this might be driving our results, we rerun the baseline specification, including also mothers that have younger children. Estimates are almost unchanged.

Third, our instrument is constructed as the ratio of children in care to the population of the same age. Changes in these rates could be driven both by changes in population as well as changes in the supply of care. If population growth drives labor supply, or if, as above, child care availability increases fertility, then this could cause bias in our estimates. To investigate this, we change our specification by using the log of the number of places in care as the instrument. The estimates in row 3 in panel C of Table 7 are again similar to our baseline estimates.

Fourth, we might worry that a contemporary expansion among older children confounds the estimates. We see from Fig. 1(a) that the reform is associated with (smaller) increases in the child care access also for 3–5 year olds, and these children might have younger siblings in our sample. If the child care use of these children correlates with that of their younger siblings and also affects maternal labor supply, our estimates would be biased upwards. Our estimates does not move when we include a control for child care access for 3–5 year old children.

6.4. Family fixed effects

Our municipality fixed effects account for all time-invariant determinants of labor supply at the municipality level and from individual characteristics for which the composition across municipalities is fixed over time. However, if the composition changes over time, and the changes in these characteristics correlate with the child care expansion, our results might be biased.

To check this, panel D of Table 7 reports estimates from a model with family-specific fixed effects. This exploits that some parents have more than one child in the sample period to compare parental labor supply outcomes across siblings, where one child is exposed to lower child care access than the other. By relating the change in labor supply to the change in child care access within families, we can hold constant all observable and unobservable characteristics that are fixed within families over time.

Because parents with multiple children inthe sample period may be different from the rest of the sample, we first rerun our baseline specification on this sample. To maintain clustering at the municipality level, we include only mothers who reside in the same municipality at each birth. The results in panel D show that in the reduced sample our baseline model yields estimates that are almost identical to the baseline. When we include family fixed effects, estimates are somewhat smaller but still quite similar and not significantly different from baseline estimates. This indicates that our main results are unlikely to be driven by changes in the composition of individuals across municipalities.

7. Conclusion

We investigate the labor supply effects of the use of child care for toddlers following a Norwegian reform in 2002, exploiting the staggered expansion of child care across municipalities. To guard against endogeneity problems, we instrument individual child care use with rationed municipal child care access, controlling for municipality and year fixed effects to exploit only changes in availability of care. Our approach is supported by a battery of robustness tests that investigate alternative explanations for our findings. The information on child care use that we exploit ensures that our estimates represent the effect of a full year of child care use. This is important because newly expanded slots are not exploited to capacity in the first year, largely because they are usually opened in the middle of the year. In practice, our estimates suggest that take-up of new child care places is in fact roughly complete in the period after they are opened.

Results show relatively large labor supply responses among mothers of toddlers compared to most of the existing literature. Cohabiting mothers seem to respond by moving from no or little employment to more or less full-time work: We find an elasticity of labor force participation with respect to child care use of around 0.32, indicating that for every 10 cohabiting mothers induced to use full time care by the expansion, 3 more mothers will be employed than would be the case had they used no care at all. These estimates should be compared to the pre-reform means: Before the reform, 63% had at least substantial part-time work. We also find significant and positive effects on full time employment and income, but find contradicting and insignificant estimates for single mothers.

We also investigate the response on total tax payments from mothers and fathers to assess the degree to which increases in the tax base can offset some of the costs of the subsidy. We find relatively small and insignificant effects on tax revenue, corresponding to a marginal tax rate of about 14% on the extra 66,000 NOK earnings for cohabiting mothers. This is around half of the average tax rate paid by our sample of mothers before the reform, and shows that back-of-the-envelope calculations using average tax rates may considerably overstate the extent to which the costs of subsidizing care is offset by increased tax revenues.

Our results indicate that the net contemporaneous fiscal cost of an additional child in full time care is substantial: Overall, our estimates suggest that around 6% of the costs are offset by increased tax revenues. In addition, the cash for care benefits are mechanically reduced by around 50,000 NOK, increasing the cost offset to around 39%. Of course, there may be other potential costs and benefits from the labor supply effects that we have estimated, beyond the fiscal effects for the government. In particular, our estimates do not capture potential effects of earlier return to the labor market, either to workers from e.g. increased work experience or to employers from e.g. shorter work disruption or potentially lower worker turnover.

While much of previous literature on child care and mothers' labor supply points towards mixed and often relatively small impacts of child care on parental labor supply, particularly for older kids, our results indicate that impacts may be relatively strong. This supports some recent evidence which suggests that labor supply effects of child care are larger for younger children, see e.g. Bauernschuster and Schlotter (2015), Nollenberger and Rodríguez-Planas (2015) and Carta and Rizzica (2018). One explanation may be that the counterfactual mode of care is different: In the absence of formal care, mothers of 2-year-olds are more likely to stay at home, while mothers of older children may find informal care arrangements, possibly using grandparents or other relatives, that enable them to work even when child care is not available. While we do find modest (and opposite signed) effects on the labor participation of paternal, but not maternal, grandparents, these cannot explain the magnitude of the effects on the labor supply of mothers.

A study that may be particularly relevant for ours is Havnes and Mogstad (2011). They study a Norwegian expansion of care for preschoolers in the late 1970s, and find estimates that are much smaller than what we do in this paper. While our study focuses on younger children and on a much later time period, it is useful to consider what may be driving the differences in the estimated effects. First, note that our estimates are scaled to the intensity of child care use. As discussed above, this seems to be important in this context, where most child care places are opened in August. Our first stage estimates and the scatter plots in Fig. 5(a) suggest that utilization of both new and existing child care places is close to saturation. Second, we have looked at survey data on the preferred mode of child care collected in 1968 and in 2002 (Ministry of Consumer Affairs and Administration, 1972; Moafi and Bjørkli, 2011; Pettersen, 2003; Reppen and Rønning, 1999). For preschoolers in 1968, around 25% of mothers stated a preference for informal solutions such as childminders, nannies or relatives. The comparable number for toddlers in 2002 is around 6%, indicating that preferences of mothers considered in Havnes and Mogstad (2011) are quite different from preferences of mothers in the 2000s. This could drive differences in the demand for informal care, and in turn explain the large effects today compared to the 1970s. Additionally, because maternal employment is much more prevalent in the period we consider, the availability of informal sources of care could be more limited. This may be particularly true because mothers in the 1970s typically relied on relatives, neighbors or friends for child care needs (Ministry of Consumer Affairs and Administration, 1972).

The results from our analysis are relevant to current political debates, considering that the demand for child care for older children is more or less covered in Norway and many western countries. Our findings therefore speak to the efficiency of further expansion, and provide evidence for other countries and governments considering a move towards universally accessible, subsidized child care for young children.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at 10.1016/j.labeco.2019.101762.

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