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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 reform from 2002. Our IV-estimates indicate that child care use causes an increase in the labor supply of mothers. Results suggest that cohabiting mothers move towards full time employment, while single mothers move to part time. Meanwhile, we find no impact for fathers or grandparents. We also find an increase in the taxes paid from cohabiting mothers, lending some support to the argument that parts of the cost of child care is offset by increased taxes.

Keywords: Child care, female labor supply, tax revenue, instrumental variables.

JEL classification: H24, H52, J13, J22

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Sammendrag

Offentlig støtte til barnehager er betydelig i Norge og i mange andre land. I flere land har det også vært et sterkt politisk ønske å bidra til økt tilbud av barnehager. En viktig målsetning er og har vært å legge til rette for yrkesdeltakelse blant mødre. I tillegg kan høyere inntekter hos foreldrene bidra til at kostnadene ved barnehage kan kompenseres av økte skatteinntekter.

En voksende litteratur fra mange land, også Norge, viser at effekten av barnehager på kvinnelig yrkesdeltagelse historisk har vært relativt liten, dog med stor variasjon mellom ulike studier. Disse funnene svekker et av de sentrale politiske argumentene for stor offentlig støtte og innebærer at de ventede økningene i skatteinntekter har uteblitt. Samtidig har få studier sett på barnehagebruk for de yngste barna, der brorparten av utvidelsene i barnehage har kommet siden 1990-tallet.

Vi studerer effekten av barnehagebruk for to-åringer på foreldre og besteforeldres yrkesdeltagelse på 2000-tallet. Siden foreldre som sender sine barn i barnehage må antas å skille seg fra andre foreldre, særlig hva gjelder tilknytning til arbeidsmarkedet, vil en direkte sammenlikning basert på barnehagebruk ikke gi rimelige anslag på effektene av barnehage. For å estimere den kausale effekten benytter vi i stedet den store variasjonen i utbygging av barnehager i etterkant av barnehageforliket fra 2003. Forliket økte subsidier til utbygging og drift av barnehager, åpnet for større grad av private tilbydere og la til rette for den påfølgende maksprisreformen. Resultatet var store sjokk til tilbudssiden på barnehagemarkedet, som for de minste var sterkt rasjonert gjennom store deler av 2000-tallet.

Resultatene viser at barnehage for de minste har hatt en betydelig virkning på arbeidstilbudet blant mødre i Norge. For hvert tiende barn som begynner barnehage fulltid finner vi at omtrent tre mødre begynner å jobbe. Mens gifte og samboende mødre i hovedsak begynner å jobbe fulltid, begynner enslige mødre i større grad å jobbe deltid. Videre finner vi at effektene, særlig for gifte og samboende mødre, vedvarer over de neste 3-4 årene, men blir gradvis mindre.

Når vi ser på yrkesdeltagelsen til fedre, finner vi ingen effekter av økt barnehagebruk. Dette kan peke mot at mødre fortsatt er de primære omsorgspersonene for de yngste barna. Heller ikke besteforeldrenes yrkestilbud ser ut til å endre seg med økt barnehagebruk. Dette kan peke mot at besteforeldre for de fleste familier ikke er et alternativ for daglig pass av de minste barna når barnehage ikke er tilgjengelig. I sum tolker vi dette som at alternativet til barnehage for flesteparten av de minste barna er å være hjemme sammen med mor. Dette står i kontrast til tidligere studier på eldre barn, der uformelle passordninger ser ut til å være det viktigste alternativet til barnehage.

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 (OECD, 2006; Field et al., 2007); 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 bipartisan agreement in Parliament from 2002 to promote full child care coverage. 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, 2003). The reform increased government subsidies to investment in and running of child care institutions, and generated large variation in coverage between municipalities and over time. Our estimation strategy exploits the difference in child care expansion between municipalities to get credible estimates on how child care affects the labor supply of both mothers and fathers, as well as other potential caregivers. Using time and municipality fixed effects, we essentially relate the changes in labor supply to changes in child care availability, similar to a difference-in-differences strategy. 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 previous literature is that we are able to identify a large range of caregivers. In our analysis, we consider both married, cohabiting and single mothers, as well as fathers and grandparents. Because most studies have been restricted to look at mothers, often only married mothers, these studies might have missed part of the effect that child care availability has on labor supply. A further improvement is that we observe a continuous measure of the individual use of child care in our sample each month. This allows us to implement an IV strategy, using the availability of slots in child care as an instrument for the actual enrollment, while accounting for the intensity of child care use. Our estimates reflect, therefore, how the actual use of a full year of child care affects mothers labor supply, rather than the direct effect of additional child care slots which is estimated in most of the literature. If we believe that initial take-up of child care is lower than in the long run, this scaling will be important to get good estimates of the impact of child care availability on parents labor supply. Indeed, in our sample, the initial take-up of new child care places is about one third of the full potential. Because child care coverage is measured late in the year and child care availability is typically expanded in the fall, it should not come as a surprise that parents are not initially exploiting new places in child care to capacity.

Ours is also among the first papers to provide evidence on how large-scale, universal child care for toddlers affects parental labor supply. In contrast, the sizable literature on child care and mothers' labor supply has been mostly focused on child care for preschoolers (age 3–6) or even wider age groups. There are important reasons to believe that the impact on parental labor supply differs between parents of toddlers and preschoolers. 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 providers 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 a substantial effect on mothers' labor supply. For each 10 children enrolled in full time care, we estimate that almost 3 married mothers enter the labor force.[1] This is over a baseline of 63 % participation before the reform. For single mothers, we also find strong effects, with 2.3 mothers entering the labor force for every 10 children that enroll in full time child care, compared to a labor force participation of only 29 % among these mothers prior to the child care expansion. Overall, this corresponds to a modest earnings increase of 50,000 NOK (6,000 USD) and 20,000 NOK (2,400 USD) for cohabiting and single mothers, respectively. We also investigate persistence in the labor supply response, finding positive impacts 1–3 years later.

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 net tax payments, we can go beyond the back-of-the-envelope calculations in previous literature, to show that the increased

[1]Throughout, we refer to married and cohabiting mothers interchangably - 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.

taxes from the additional income is indeed considerable, implying a marginal tax rate of about 40 % on the extra income, close to the average tax rate. At the same time, the increased use of child care and employment does not seem to lead to significant reductions in benefits, which would further have reduced public spending.[2]

For fathers and grandparents we find no labor supply response. This may indicate that mothers are still the primary caretakers, staying home when child care is not available. The lack of response among working age grandparents could indicate that they are not important informal caregivers for young children in our sample, at least not to the extent that it affects their labor supply. This supports the notion that the counterfactual mode of care may be different for toddlers and preschoolers, which could be a key insight when considering how child care may affect child development.

Our paper contributes to the rapidly growing literature estimating how child care availability affects parental labor supply using quasi-experimental methods. The evidence from these studies usually indicates relatively small effects of child care expansion. In the US, for instance economically small effects are found by Gelbach (2002), Cascio (2009), and Fitzpatrick (2010, 2012). Similar patterns are found in several European countries, see e.g. Lundin et al. (2008), Goux and Maurin (2010), Havnes and Mogstad (2011), Dustmann et al. (2013) or Felfe et al. (2013). A prominent exception is the reform in Quebec studied by Baker et al. (2008) and Lefebvre and Merrigan (2008), where the estimated impact on mothers' labor supply is substantial.

The lack of robust evidence in previous literature in support of a tight link between parental labor supply and child care availability or prices is disappointing from a policy perspective, since increasing mothers' labor supply 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 expansions may not be complete. Indeed, in our case, the start of the child care year in August implies that the child will usually attend at most five months of care in the first year. 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 previous literature, particularly for younger kids.

[2]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 80,000 per year.

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 persistency analysis. We perform a range of robustness checks to support our estimates in Section 6, while section 7 concludes. Additional tables are found in the appendix.

2 Institutional setting and the child care reform

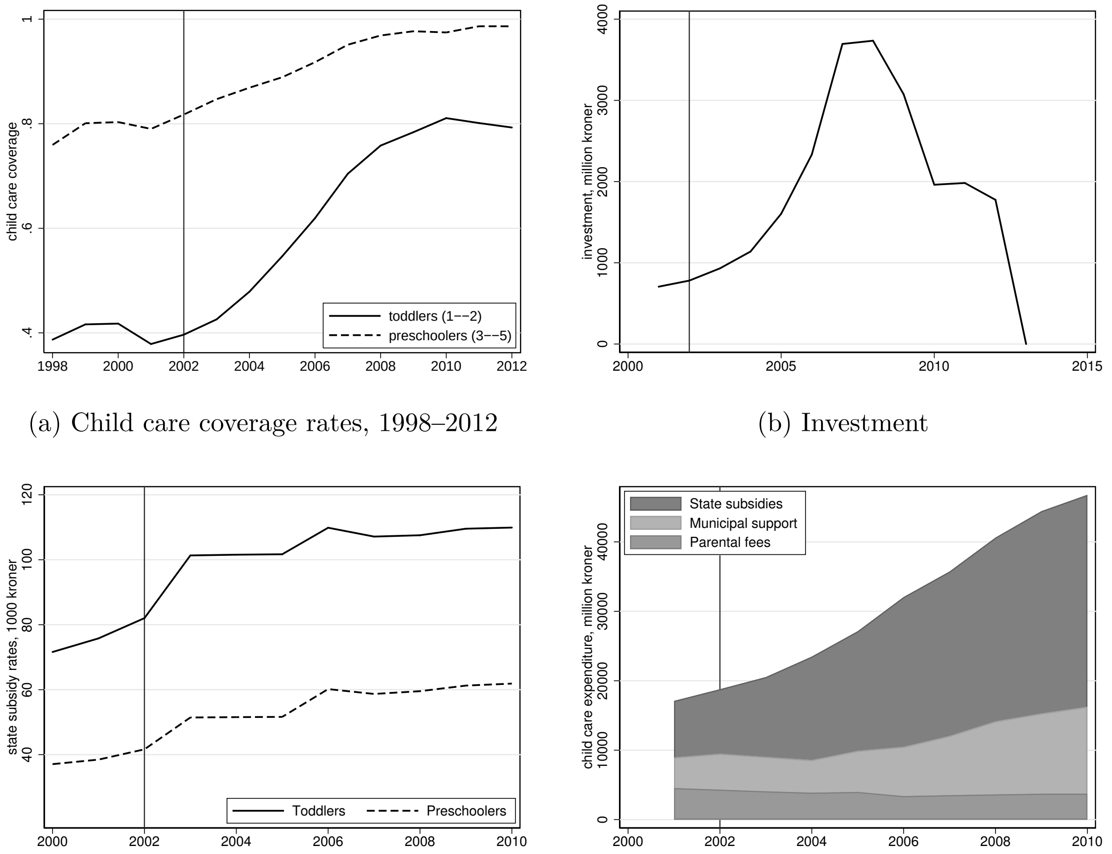
Although the roots of the Norwegian child care system date back to the early 19th century,[3] the system of universal child care was introduced after WWII as a response to increasing female labor force participation and the goal of gender equality in the Nordic welfare model (Ministry of Education and 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.

Figure 1a shows the trends in child care coverage rates for preschoolers and toddlers from 1998–2012. By 2000, about 80% of 3–6 year olds were enrolled in formal child care. At the same time, coverage was much lower for younger children. In 2000, less than 40% of 1–2 year olds in Norway were enrolled in child care, and there was substantial excess demand for child care, see. the discussion in Section 4 below.

This excess demand for formal child care was the background for the Kindergarten concord, a reform passed in the Norwegian Parliament in 2002 with broad bipartisan support. The main goal of the reform was to offer affordable child care to all children, and to secure quality and diversity in child care services (Ministry of Education and Research, 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. The concord also established equal treatment of private and public child care institutions, where private institutions had previously been awarded only 85 % of the subsidy rate offered to public institutions, and thus made it easier for private suppliers of care to enter the market.

Figure 1 presents some important changes in the child care sector following the reform. Panel 1b depicts the total investment in child care institutions over the period.[4] 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. Panel 1c shows the increase in total subsidies per child per year. Panel 1d shows the changes in the composition of the costs covered by the municipality,

[3] See Ministry of Education and Research (2009) for a thorough treatment.
[4] All monetary values are given in 2014 Norwegian crowns.



(a) Child care coverage rates, 1998–2012

(b) Investment

(c) Yearly state subsidies per child

(d) The composition of financing in child care

Figure 1: Child care coverage rates, investments and financing in the 2000's.

Sources: Statistics Norway and regjeringen.no.

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.

Throughout the period we consider, formal childcare was highly regulated in Norway. In order to be eligible for the generous subsidies, both private and public child care institutions have to adhere to strict criterias governing the quality of the services supplied. These criteria relate to for instance the ratio of pedagogical staff per to children, opening hours, parental involvement and available playing space per child (Kindergarten Act, 2005). Since 2004, the institutions also must adhere to the max price reform, that put a cap of 2,750 NOK (around 330 USD) on the fee that can be charged from parents for a full time slot. In 2006, this cap was further lowered to 2,250 NOK. In practice, this ensures that formal child care institutions are relatively homogenous in terms of observable attributes of quality and price.

As illustrated in Figure 1a, the reform resulted in a sharp increase in municipal coverage rates for 1 and 2-year olds. Over a nine year period, coverage for toddlers increased by over 40 percentage points, from 37 % in 2001 to 80 % in 2010. In our empirical analysis, we will attempt to evaluate the impact of this massive increase in child care coverage on the labor supply of mothers and fathers.

3 Data and descriptive statistics

Our data is based on rich administrative registers available from Statistics Norway, and cover the entire resident population in 2002–2008. 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 coverage reported by the child care institutions themselves. Importantly, the data contain unique personal identifiers that allow us to match children to parents, to grandparents and to other caregivers residing with the child. We also 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.

Below, we first define our estimation sample and then discuss how we measure the key variables in our analysis, namely child care use, child care coverage and labor supply. We conclude this section by providing descriptive statistics for our sample.

To define the **population of interest**, we start with all 2-year olds alive and resident 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.[5] We also exclude children with unknown mothers and a handful of families with more than one child in the sample. This leaves us with a sample of 323,164 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

[5]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.

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.

To measure individual **child care use**, we exploit the administrative registers of cash-for-care (CFC) recipients, available for two year olds since 1999. 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.[6] 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. 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.[7]

To measure **child care coverage** in each municipality, we use the share of children in child care to the population of the same age from municipal reports, measured around October each year, when enrollment in the regular calendar year is completed. These data are readily available from Statistics Norway.[8] Similar measures are used regularly in the literature, see e.g. Havnes and Mogstad, 2011; Dustmann et al., 2013; Nollenberger and Rodríguez-Planas, 2015.

One potential issue with these measures is that in clearing markets child care coverage will be determined jointly by demand and supply. Exploiting variation in coverage rates may then pick up demand shocks, which would raise the potential for reverse causality and estimation bias. If the demand for child care exceeds supply, then the variation in coverage should be determined uniquely by the variation in supply. In order to verify anecdotal evidence that rationing is prevalent here, we would 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 0–3 years old who were on waiting lists for child care in each municipality by September 20th over 2004–2009.[9] By adding children enrolled and children on waiting

[6] Over the period the CFC benefit varied from 2,200 to 3,600 NOK per month per child, equivalent to approximately 260 - 430 USD per month using the December 2017 exchange rate.

[7] We thus consider a full year of part time care (about 20 hours 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.

[8] `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.

[9] The data were collected by Asplan Viak in order for the Ministry to monitor the progress towards full child care coverage.

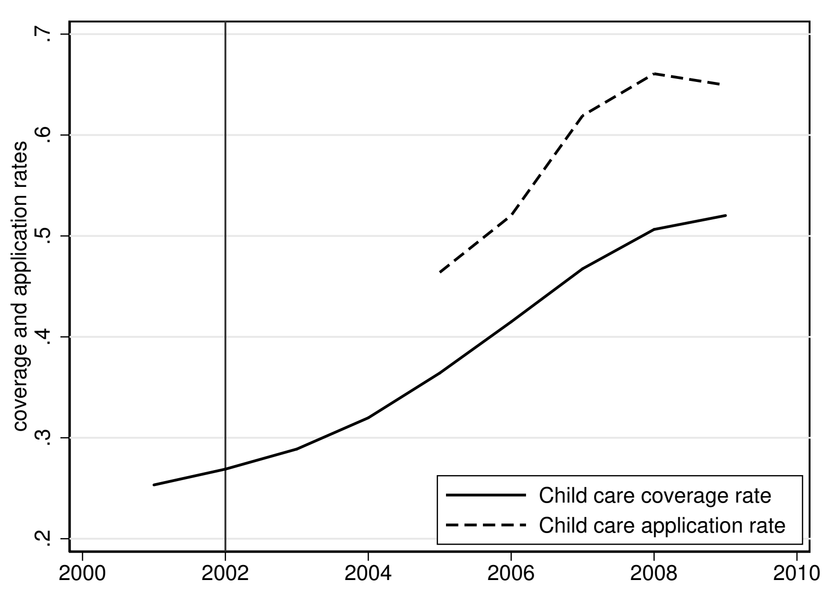


Figure 2: Child care application and coverage rates for 0–3 year olds

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. Sources: Reports from Asplan Viak (2010) and Statistics Norway.

lists, we can construct a measure of the child care application rate.

Figure 2 draws the child care coverage rate and our measure of the child care application rate for 0–3 year olds over our estimation period. Throughout the period, the application rate is substantially higher than the coverage rate, by between 10 and 15 percentage points, indicating excess demand. Additionally, we suspect that the strong growth in demand over time may be due to applications from parents who were discouraged from applying when coverage was particularly low. If so, then rationing in the early period is likely to be even stronger than reflected in these numbers. Note that these data were collected for the combined group of 0–3 year olds only, so do not speak directly to the instruments we will use below (coverage for 1 and 2-year olds). However, this is again 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.[10] 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. As outcome variables, we use both earnings directly, as well as dummy variables for labor market par-

[10]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.

ticipation based on the basic amounts in the Norwegian Social Insurance Scheme (used to define labor market status, determe 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 2014, one basic amount equaled $88,370$ NOK, or approximately $11,000$ USD. The tax records additionally provide data on total tax payments and 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 4 hours and above 30 hours. 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.

Descriptive statistics

Summary statistics for the 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 5.3 months of full time child care use. 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 15 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, Figure 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, both cohabiting and single mothers experienced substantial increases in the labor force participation over these years. In particular, employment rates among both sets of mothers increased by about 10 percentage points, driven largely by full time-equivalent employment among cohabiting mothers, but less so among single mothers. Also non-residing fathers experienced an increase in labor force participation from the mid 2000's, following a slump in the early period.

Given the large increase in child care coverage rates affecting these groups, it is tempting to interpret this as first evidence of how the child care reform impacted on parents' employment. This would be premature, however, since we cannot at this point distinguish between general increases in labor force participation of mothers, and the potential impact of the child care expansion. We discuss how we tackle this question in the next section.

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 flat trend over time, which suggests that the composition of mothers is relatively stable over time. The exceptions are the growth in immigrant background from about 8 % to 11 % and a small increase in education. Similar patterns are seen for the other groups of caregivers, which are reported in the appendix for brevity, see tables A.1–A.3.

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

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

	Children			
Variable	Mean	SD	Min	Max
Male	0.49	0.50	0	1
Child care use	0.47	0.42	0	1
Older paternal siblings	0.99	1.00	0	15
Older maternal siblings	1.00	1.06	0	17
Child care coverage rates				
One year olds, $t-1$	0.41	0.17	0	1
Two year olds	0.67	0.18	0	1
Observations	323,164			

Note: Variables are defined in Section 3.

Table 2: Descriptive statistics for caregivers at child age 2.

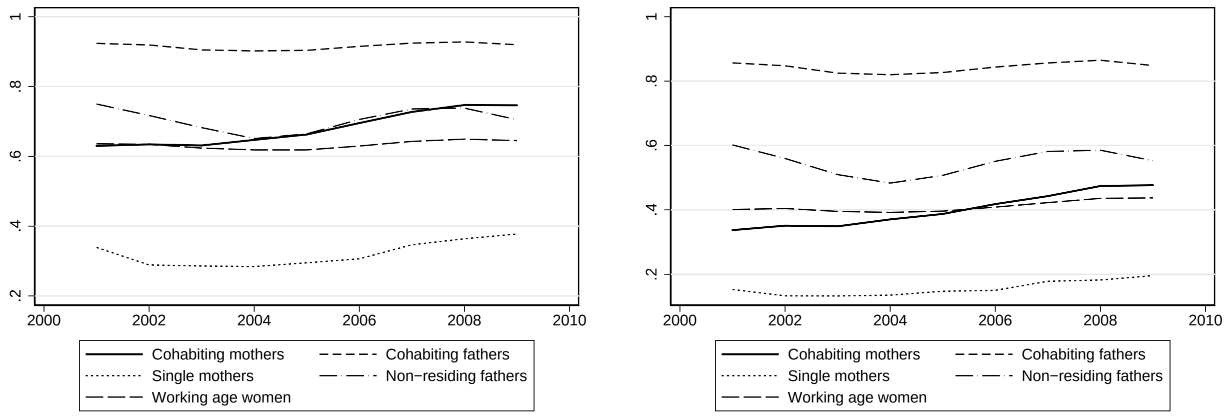
		Cohabiting mothers	Cohabiting fathers	Single mothers	Non-residing fathers
A. Outcome variables					
Earnings (NOK)		205,350	408,563	94,361	253,305
Tax		73,520	172,905	31,146	103,635
Transfers, excl. cash for care		44,331	14,474	155,084	39,550
Employed		0.678	0.914	0.309	0.700
Full-time eq.		0.399	0.841	0.150	0.540
Weeks of	4 hours	37.18	43.59	21.36	32.16
employment above	30 hours	26.47	41.70	13.17	29.01
B. Control variables					
Age		32.65	35.55	29.52	32.47
Immigrant		0.0977	0.0742	0.0878	0.0799
Years of education		15.36	15.04	13.15	12.92
Number of children		2.028	2.056	1.720	1.820
below 6 years old		1.517	1.516	1.282	1.331
below 13 years old		1.893	1.878	1.554	1.597
below 18 years old		1.994	1.986	1.667	1.720
Local labor market control		0.719	0.719	0.715	0.714
Observations		283,868	283,687	33,291	29,276

Note: Outcome and control variables are defined in Section 3.

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

		2002	2004	2006	2008
A. Outcome variables					
Earnings (NOK)		165,232	184,939	216,557	261,708
		(135,929)	(176,877)	(168,514)	(183,528)
Tax		67,042	86,315	71,621	76,947
		(74,096)	(94,643)	(78,988)	(139,871)
Transfers,		49,843	45,097	42,144	39,825
excl. cash for care		(43,246)	(49,117)	(47,954)	(49,185)
Employed		0.634	0.647	0.695	0.747
		(0.48)	(0.48)	(0.46)	(0.43)
Full-time eq. employment		0.351	0.370	0.418	0.474
		(0.48)	(0.48)	(0.49)	(0.50)
Weeks of	4 hours	35.00	36.28	37.79	40.05
employment		(22.4)	(22.2)	(21.3)	(19.9)
above	30 hours	23.57	25.45	27.37	29.89
		(24.6)	(24.8)	(24.7)	(24.4)
B. Control variables					
Age		32.28	32.56	32.84	32.88
		(4.82)	(4.83)	(4.83)	(4.92)
Immigrant		0.0838	0.0903	0.103	0.116
		(0.28)	(0.29)	(0.30)	(0.32)
Years of education		14.96	15.26	15.52	15.70
		(3.21)	(3.20)	(3.20)	(3.22)
Number of children		2.048	2.033	2.026	2.002
		(0.99)	(1.00)	(0.98)	(0.98)
below 6 years old		1.513	1.515	1.517	1.524
		(0.58)	(0.58)	(0.58)	(0.58)
below 13 years old		1.912	1.893	1.892	1.873
		(0.84)	(0.83)	(0.83)	(0.82)
below 18 years old		2.013	1.999	1.994	1.967
		(0.94)	(0.94)	(0.93)	(0.92)
Participation, working age childless men		0.676	0.706	0.726	0.800
		(0.052)	(0.051)	(0.052)	(0.047)
		42,186	39,309	40,211	41,762

Note: The table gives biannual means and standard deviations (in parentheses) in the estimation sample. Variables are defined in Section 3.



(a) Employment

(b) Full time-equivalent

Figure 3: Labor force participation for different caregivers in our estimation sample and for working age women in Norway over 2001–2009.

Note: Employment and full time-equivalent is defined as earnings above 2 and 4 basic amounts (BA), respectively, see Section 3. $1BA \approx NOK88,000 \approx USD11,000$.

use is endogenous to parents' labor supply decisions. Clearly, parents that enroll their children in child care are likely to be more 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 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 coverage rates. The distribution of municipal child care coverage rates for 1–2-year-olds is illustrated in Figure 4. The figure shows both the strong increase in child care coverage over the period we consider, and the large variation across municipalities.

Our basic IV strategy is the following: For each municipality and every year, we instrument individual child care use, m_{ikt}, with the child care coverage rates of 1-year-olds in $t-1$, $CC^1_{k,t-1}$, and of 2-year-olds in t, CC^2_{kt}.[11] Specifically, we estimate the following

[11]In the first stage of our IV model below, we regress child care use on municipal child care coverage rates. Some readers may find that this resembles a poorly designed peer effects analysis, regressing

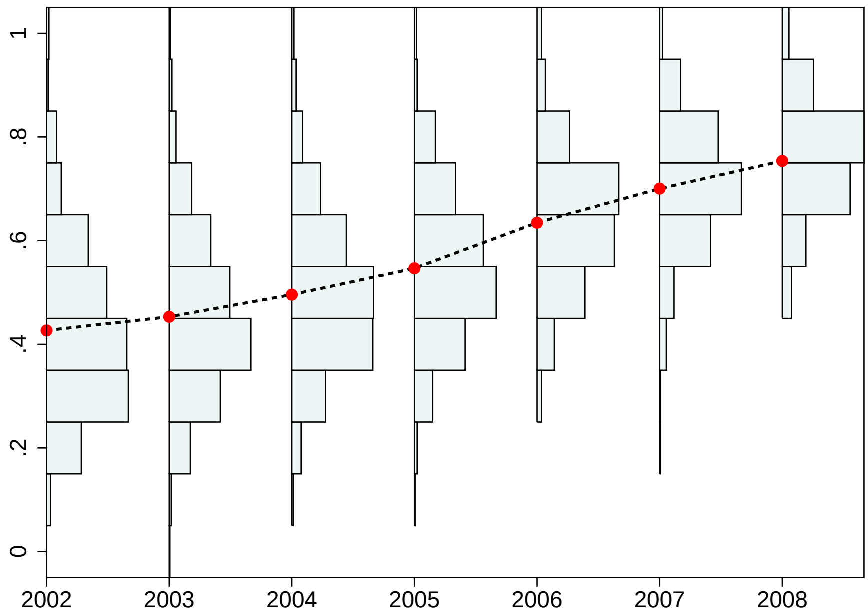


Figure 4: The distribution of child care coverage rates for 2-year-olds across municipalities over 2002–2008.

Note: The figure draws the mean child care coverage rate nationally over time (bullets and dashed line) and the distribution across municipalities (bars). Data, estimation sample and variable definitions are discussed in Section 3.

2SLS-model in our sample of caregivers of 2-year old children:[12]

$$y_{ikt} = \alpha_k^2 + \tau_t^2 + \beta m_{ikt} + \mathbf{X}_{ikt}\theta_1 + \epsilon_{ikt}^2 \tag{1}$$

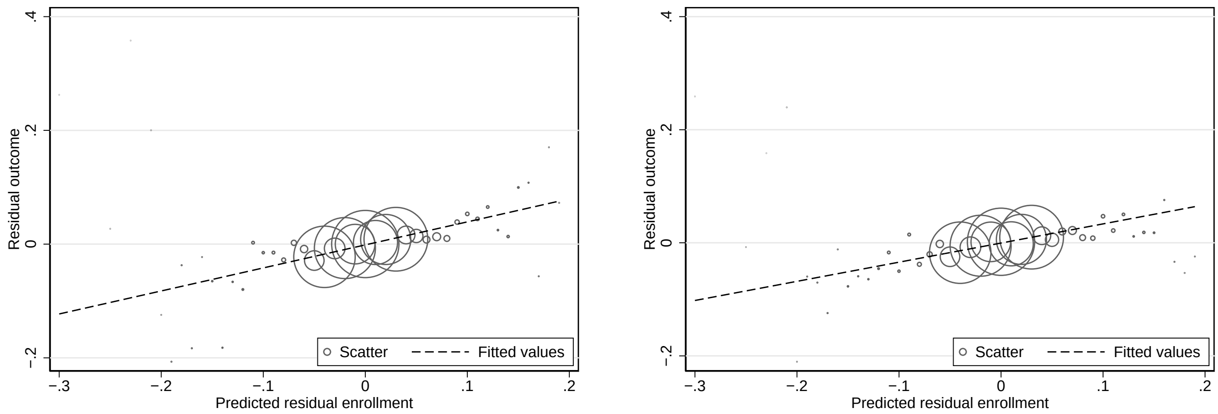
$$m_{ikt} = \alpha_k^1 + \tau_t^1 + \pi_1 CC_{k,t-1}^1 + \pi_2 CC_{kt}^2 + \mathbf{X}_{ikt}\theta_2 + \epsilon_{ikt}^1 \tag{2}$$

where α_k^s and τ_t^s are municipality and time fixed effects in stage s. In our robustness analysis, we show that our estimates do not depend on the inclusion of controls. In our baseline model, however, we include controls for the vector $\mathbf{X}_{ikt}$ which contains child, caregiver and municipality characteristics: Child characteristics include dummies for gender and month of birth, the number of older paternal and maternal siblings, and dummies for parents' level of completed education.[13] Caregiver characteristics include age, age squared, a dummy for immigrant background and the number of children below ages 6,

individual outcomes on group means (for a detailed discussion of why this should be a bad idea, see Angrist (2014)). But note that our measures of municipal child care coverage rates are distinct from the individual child care user rates, both due to the time of measurement, in the stock versus flow properties, and in the reflection of extensive and intensive margins. Indeed, if we use the municipality-year means of child care use as an instrument, the F-statistic is 8,300 with a coefficient close to 1, as expected. In contrast, our first stage yields an F-statistic of 45, with coefficients of .3 and .05. Note also that this discussion has no bearing on the validity of our instruments or the reduced form.

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

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



(a) Employment

(b) Full time-equivalent

Figure 5: Illustration of the IV strategy: Predicted residual labor supply versus predicted residual child care use.

Note: The figure graphs child care use against labor supply outcomes after removing time and municipality fixed effects, see discussion in the main text. Scatter points are binned to 0.005 and the size of each point represent the number of observations in that bin. Data, estimation sample and variable definitions are discussed in Section 3.

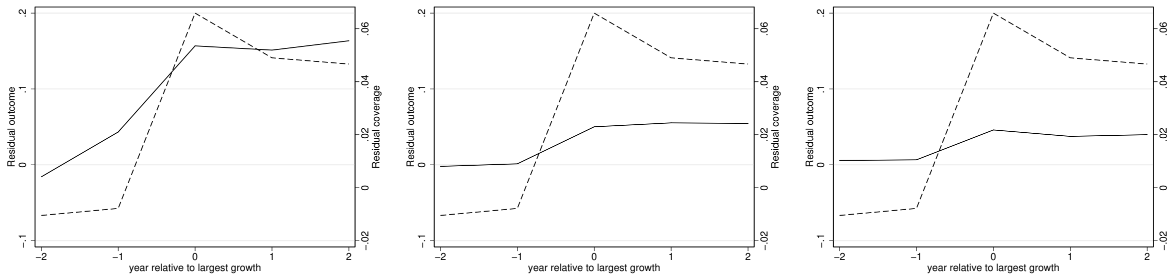
11 and 18. We also include dummies for level of education to control flexibly for returns to education, including potential sheepskin effects. Finally, we control for local labor market conditions in the municipality of residence, as measured by the labor force participation rate of working age men (20–67 years old) without children. Descriptive statistics for these variables are reported in Table 2. Standard errors are clustered at the municipality level and robust to heteroskedasticity.

To illustrate our IV strategy graphically, the panels of Figure 5 draws scatter plots of labor supply against predicted child care use, purged of time and municipality fixed effects.[14] The figure suggests that there is a strong effect of predicted child care use on labor supply, and that the relationship is well approximated by a linear function in the bulk of the data.

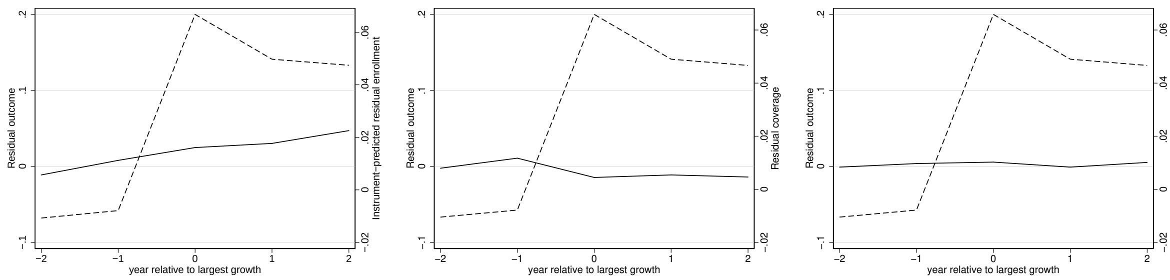
Next, we graphically present the changes around the timing of the greatest growth in each municipality in Figure 6. To this end, we first purge all variables of municipality means using fixed effects. We then regress residual child care use on residual coverage rates, controlling for year fixed effects, and predict a scalar instrument that represents only the variation we exploit. Then we recenter the data so that time zero is the year with the highest increase in predicted child care use for each municipality relative to the aggregate increase.

The top panels of Figure 6 graphs the trends in labor supply alongside the trend in child

[14]Specifically, we estimated Equation (2) without covariates and predicted the individual child care use from the instruments. We then regress both the predicted child care use and the outcomes individually on calendar time and municipality fixed effects, and predict the residuals.



(a) Outcome: Earnings (b) Outcome: Employment (c) Outcome: Full time-equivalent



(d) Control: Immigrant (e) Control: Number of children (f) Control: Female child

Figure 6: Event study graphs: Changes in outcomes (top panels) and around the time of the largest growth in child care coverage rates.

Note: The figure graphs outcomes and some important covariates over time after removing effects of time, municipality and controls, see equations (1) and (2). Data are recentered so that year 0 is the year with the largest growth in predicted child care use for each municipality. The scale of the outcome variables (left axis) is in standard deviations of the variable in the sample of cohabiting mothers, while the scale of predicted child care coverage is in percentage points. Data, estimation sample and variable definitions are discussed in Section 3.

care use as predicted from our instruments. At time zero, we see a substantial increase in predicted child care use of around 7 percentage points (relative to the aggregate increase that year), while the trend is otherwise relatively flat. This illustrates that our first stage is relevant and quite strong. We also see that as predicted child care use jumps up, labor participation also increases, by around .1, .05 and .04 standard deviations for income, employment and full time-equivalent, respectively. Taking the fraction of these estimates suggests an IV-estimate of around 32 percentage points increase in labor participation from full time child care use for the employment outcome. Below, we elaborate on this within our estimation framework.

To evaluate our IV-strategy, note that the identifying assumption roughly relies on common time shocks across municipalities that expand early and late. If this assumption is violated, for instance by labor supply leading child care expansions, then this should show up in Figure 6 as trends or jumps in the outcome prior to the expansion that is unrelated to increases in predicted child care use. On the contrary, the figure shows that the trends in labor participation are relatively flat in the pre-period, following closely the trend in predicted child care use. This suggests that our IV-strategy is sound.

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), or more precisely a weighted average of all potential LATEs as our instruments are continuous. 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 coverage 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 controll 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 coverage 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. However, to further investigate this issue we check whether the composition in our sample is changing over time by looking at observables. Even though we control for these flexibly in our regressions, changes in the composition of observables might raise concerns about changes in the composition also in terms of unobservables. In the lower panels of Figure 6, we have reproduced the graphical illustration above for some key predictors of parental labor supply, where we have again recentered our data so that time zero is the year with the highest increase in predicted child care use, and we have purged the variables of municipality fixed effects. It is reassuring to see that, across the bottom panels of Figure 6, the jump in predicted child care use is not associated with changes in the covariates. On the contrary, the changes in the covariates are quite smooth over the period, with no apparent break at time zero.

Finally, even with the large set of control variables that we include in our estimations, one may still worry about changes in unobservable determinants of labor supply. After our main results, we will 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. It is comforting to see that these specification checks by and large lend support to our estimation strategy.

5 Results

Baseline results for the four main samples of interest are reported in Table 4. In panel A, we report estimates from our first stage. The instruments are relevant and strong, with F-statistics on the excluded instruments above 30 in all samples. Estimates in row 1 indicate that an additional child care slot for one year olds increases by about 30 percentage points the child care use of two year olds the following year. Estimates in row 2 indicate that an additional child care slot for two year olds increases by about 4 percentage points the child care use of two year olds in the same year. The coefficients are similar across groups, suggesting that the take-up of child care slots is relatively homogenous across children when we consider different caregivers.

In panel B, we report estimates from our second stage for a number of different outcomes, where reported coefficients are on child care use in separate IV regressions. We also include for each outcome the mean of the dependent variable prior to the child care expansion as a point of reference (underlined).

Our IV estimates indicate quite substantial labor supply responses among mothers.[15] In particular, employment among **cohabiting mothers** is estimated to increase by 29 percentage points when a child is enrolled in full time child care, of which around half is to full time-equivalent employment. This is confirmed by the estimated effect on weeks of employment from the matched employer-employee register, which show that child care use causes mothers to increase their employment by about 10 weeks. Compared to the prereform means, these effects imply an increase of around 50 % in the mean employment rates of cohabiting mothers of two-year olds and suggest that child care plays an important role in getting mothers of small children (back) into the labor market.

When we consider earnings of cohabiting mothers directly, our estimates indicate that child care use caused an increase of about 50,000 NOK per year, a 30 % increase over the pre-reform mean. At the same time, we estimate that taxes paid increase by about 19,000 NOK, while there is a small decrease in transfers. Combined, this reduces the individual benefit of child care in terms of disposable income by about 40 %.[16]

Though imprecise, the estimates on **cohabiting fathers** show that their earnings drop with child care use, by an amount similar to the increase among cohabiting mothers. That

[15]Note that the IV estimates contrast starkly with the OLS estimates. As an example, the OLS coefficients for earnings are more than twice as large as the IV coeffcients for both single and cohabiting mothers. This is in line with our intuition about the direction of the bias of the OLS coefficient, which is caused by reverse causality: Mothers who work more demand more child care.

[16]Note that this is very close to the average tax rate of 41 % that these women paid before the reform. Although we might expect that the nonlinearities in the tax schedule could make the marginal tax rate different from the average tax rate, this seems not to be the case. This means that the simple back-of-the-envelope calculations in e.g. Baker et al. (2005), wouldn't be far off in our setting. This does not hold, however, in the other samples we consider, where the implied marginal tax rate is about 18 % among cohabiting fathers and is even negative for single mothers and non-residing fathers, albeit very imprecisely estimated.

Table 4: IV-results: The impact of child care use on labor supply

		Cohabiting mothers	Cohabiting fathers	Single mothers	Non-residing fathers
A. First stage					
$CC^1_{k,t-1}$		0.311***	0.309***	0.285***	0.400***
		(0.0331)	(0.0328)	(0.0382)	(0.0325)
$CC^2_{k,t}$		0.0416**	0.0440**	0.0254	0.0624**
		(0.0207)	(0.0202)	(0.0334)	(0.0307)
F		45.23	45.84	30.59	161.0
B. Second stage					
Earnings		49,231***	-45,971	19,340	-8,241
		(12,612)	(32,269)	(28,999)	(26,111)
		165,232	356,127	76,279	227,717
Tax		19,018***	-8,411	-6,350	-17,492
		(5,467)	(11,901)	(13,986)	(13,658)
		67,042	162,260	32,572	96,308
Transfers,		-3,360	-584	38,780**	6,401
excl. cash for care		(4,153)	(3,647)	(18,020)	(7,757)
		49,843	12,953	156,275	35,143
Employed		0.294***	-0.0187	0.225**	-0.0158
		(0.0298)	(0.0219)	(0.107)	(0.0502)
		0.634	0.919	0.289	0.717
Full-time		0.222***	0.00848	0.0486	0.0330
equivalent		(0.0278)	(0.0288)	(0.0737)	(0.0624)
		0.351	0.848	0.133	0.560
Weeks of	4 hours	9.741***	-0.0966	12.24**	-1.343
employment		(1.527)	(1.351)	(5.817)	(2.615)
above		35.00	43.38	19.49	31.67
	30 hours	11.02***	-0.568	2.424	-1.053
		(1.588)	(1.500)	(4.485)	(2.612)
		23.57	41.65	11.49	28.85
Observations		283,868	283,687	33,288	29,272

Note: Estimates are from equations (1) and (2). 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 children born in 20022-year olds in 2002. * (p<0.10), ** (p<0.05), *** (p<0.01) fathers work less when mothers work more is consistent with joint labor supply decisions in the household. Note however, that there are virtually no effects among fathers on the qualitative employment or full time margins, suggesting that effects on fathers are on the intensive margin, not the extensive. This is in line with the contention that father's labor force participation is largely independent of the family situation.

Considering that cohabiting mothers and fathers are usually members of the same household, our point estimates suggest a small drop in disposable household income. Add to this the mechanical drop in cash-for-care subsidies and the copayment for child care, each about 40,000 NOK per year, and it is clear that child care use is quite costly to parents. Indeed, the overall drop in disposable income corresponds to almost the full after-tax earnings of cohabiting mothers before the child care expansion.

Turning to **single mothers**, the estimates indicate that they also respond strongly on the employment margin, with an increase of about 23 percentage points. In contrast, there is hardly any impact on full time-equivalent employment. This is supported by the estimates from the employer-employee register, which records effects only on employment below 30 hours per week. These estimates support the notion that the effect for single mothers represent shifts to part-time employment, not low-wage jobs. That single mothers respond mostly on the employment margin may be explained in part by a sizable increase in transfers, likely due to an increase in the transitional benefit for single parents. With access to child care, these parents would be expected to have a much easier time meeting the activity requirements linked to this benefit.[17] In addition, full time work is likely more costly for single parents, who donot have the same possibility to share household tasks. Though the estimates are somewhat imprecise, when we factor in the estimated increases in earnings and transfers and the drop in taxes paid, our results indicate that the increase in disposable earnings is almost sufficient to make up for the parental copayment and the loss of cash-for-care.

Estimates on **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 among grandparents, under the hypothesis that these may be relevant informal caregivers for these very 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. However, we find no systematic evidence of labor supply responses among grandparents, despite a strong first stage and relatively high labor force participation rates. This may suggest that grandparents are not important caregivers for toddlers in our sample, at least not to the extent that it

[17] 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. In 2014, the transitional benefit is 2.25 times the basic amount, i.e. about 200,000 NOK per year. The benefit is subject to regular income tax and is reduced by 45 % of any income above 0.5 basic amounts.

affects their labor supply. Results are reported in Table A.4 in the appendix.[18]

We can use our results to calculate the net budgetary cost of an additional child in full time care at the time of use. Using 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 19,000 NOK, meaning that around 13% of the costs of the subsidies are offset by increased tax revenues. In addition, the cash for care benefits are mechanically reduced by around 36,000 NOK, increasing the share of covered subsidies to around 37%. For single mothers, there is no cost offset because the reduction in cash for care benefits are offset by increases in other benefits.

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 further, we construct dummy variables for earnings brackets based on the same basic amounts we used to construct our basic measures of labor participation (around 88,000 NOK or 11,000 USD). 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 (i.e. 528,000 NOK). We then estimate the same 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. Since the overall impact on fathers is small, we consider only mothers in this exercise.

Figure 7 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 Figure 7 is heavily skewed, with 62 percent in the lowest earnings bracket and population shares below 10 % and steadily declining as we move up in the distribution.

[18] We also looked at grandparents residing in the same muncipality as the child separately, believing that these might have an easier time providing care or an easier time combining care and work, but found no significant differences between these and grandparents living further away from the child. Results are available upon request.

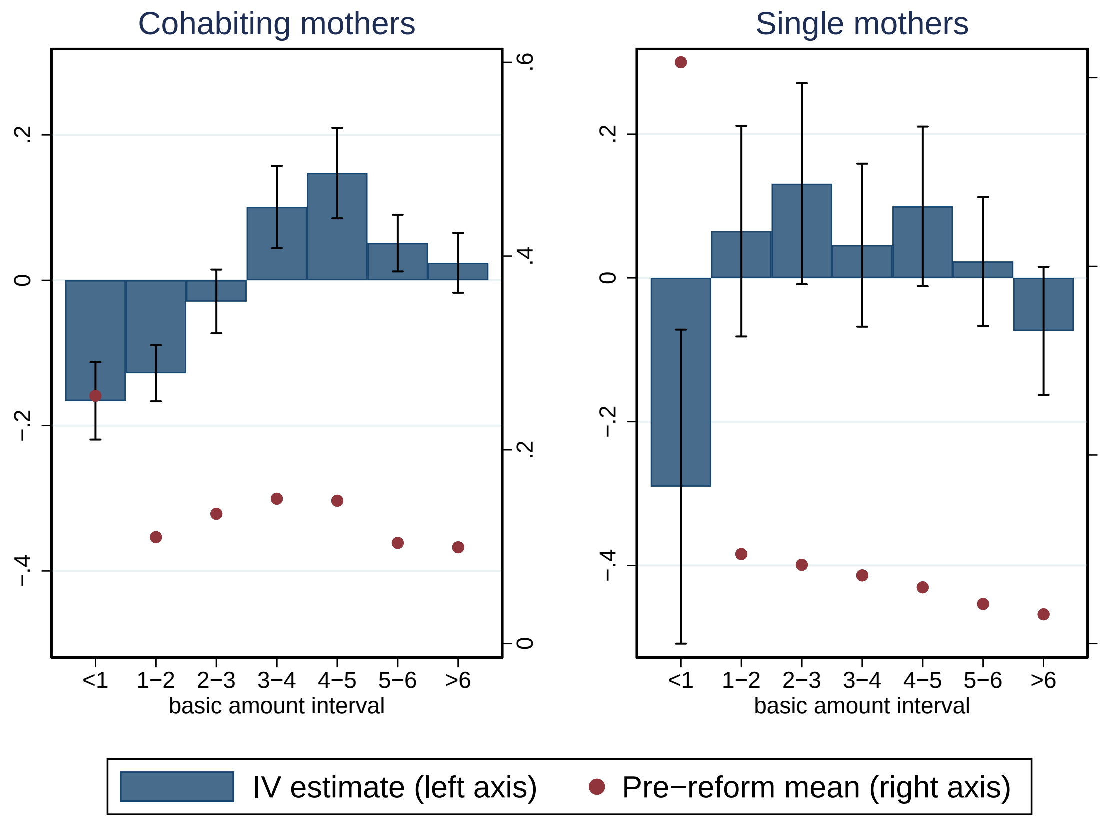


Figure 7: The impact of child care use on the earnings distribution of mothers.

Note: Estimates are from equations (1) and (2). 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.

The bars in Figure 7 (measured on the left axis) give the estimated impact of child care use on the probability of ending up in each bracket. The estimates 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. Single mothers, on the other hand, seem to shift away from the lowest bracket to the lower middle brackets, pointing towards an increased incidence of part time (or low wage) employment, though estimates are less precise in this smaller sample.

Persistency

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 in the labor supply response. To this end, we reestimate our baseline IV-model using the outcome variables measured one to four years into the future. Notice that we maintain the conditioning on control variables from year t in order to avoid issues associated with so-called "bad control" variables.[19] Also, we include controls for the child care coverage rate of older children in the treatment year, i.e. for the coverage rate for 3- and 4-year olds in year t when we consider impacts in $t+2$. 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 appendix.

Table 5 presents estimates from the IV model for cohabiting mothers. In column 2, we report the estimated impact on outcomes one year later, then in column 3 estimates on outcomes two years later, and so on. While effects fade out over time, our estimates suggest that there is still relatively strong persistence. In particular, the impact on full time-equivalent employment is almost as strong the following year and remains above 7 percentage points throughout this period. Effects on employment fade more quickly, but remain statistically significant 3 years after the treatment. Other outcomes lose statistical significance after year $t+1$. Note that the estimates for $t+1$ will partly include the direct effect of child care use, since the child care year straddles the years t and $t+1$.

Figure 8 illustrates our estimates for cohabiting mothers by predicting outcomes with and without any (formal) child care use and comparing this to the actual trend for pre-reform mothers. More specifically, we start by constructing time series for labor supply outcomes for mothers of 2-year olds in 2002. To construct the predicted series *without* child care, we subtract the predicted effect of child care use at age 2 in each year, i.e. $\hat{y}^t_{2002}\,(m=0) = \bar{y^t}_{2002} - \hat{\beta}_t \cdot \bar{m}_{2002}$ where $\hat{\beta}_t$ is the estimate from our IV-model using y^t as the outcome and $\bar{m}_t$ is mean child care use among the pre-reform mothers in year t. Analogously, to construct the predicted series with *full* child care use, we add the predicted effect of child care use at age 2 in each year, i.e. $\hat{y}^t_{2002}\,(m=1) = \bar{y^t}_{2002} + \hat{\beta}_t \cdot (1 - \bar{m}_{2002})$. Note that this should be interpreted as the predicted effect for a complier around this level, not the overall effect of full child care coverage. 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.

Figure 8 shows the estimated time series for earnings, employment and full-time equivalent for mothers. We first note that while about 70 % of mothers are employed prior to birth, just over 60 % are employed in the year following birth. Similarly, around 45 % of mothers work full time prior to birth while about 25 % 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.[20] The figure illustrates how the exogenously driven increase in child

[19]Some of the controls, like education, could be considered endogenous to the child care coverage. 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.

[20]Note that the generous Norwegian parental leave arrangements, see footnote 10, may explain why earnings and measures of employment don't fall further: In practice, almost all Norwegian mothers take

Table 5: Persistency: IV-results with lagged outcomes, cohabiting mothers

Outcome	t **(baseline)**	$t+1$	$t+2$	$t+3$	$t+4$
Earnings	49,231***	23,005*	10,376	6,164	8,367
	(12,612)	(13,289)	(14,070)	(15,105)	(15,679)
	165,232	238,203	255,636	276,045	298,470
Tax	19,018***	13,082***	5,783	5,044	9,316
	(5,467)	(4,912)	(6,754)	(6,269)	(8537)
	67,042	81,808	83,904	85,291	90,699
Transfers,	-3,360	-9,086**	-6,315	2,451	-3,120
excl. cash-for-care	(4,153)	(4,595)	(5,204)	(4,606)	(4,578)
	49,843	53,538	56,992	58,473	60,189
Employed	0.294***	0.128***	0.083***	0.069**	0.044
	(0.0298)	(0.0274)	(0.0269)	(0.0297)	(0.0296)
	0.634	0.745	0.763	0.773	0.787
Full time-	0.222***	0.198***	0.0916***	0.0709**	0.0762**
equivalent	(0.0278)	(0.0331)	(0.0349)	(0.0349)	(0.0301)
	0.351	0.469	0.480	0.503	0.530
Observations	283,868	282,667	281,874	281,311	280,818

Note: Estimates are from equation (1), with outcomes measured 1–4 years later. In addition to control variables defined in Section 3, we control for child care coverage rates for older children in year t. 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. * (p<0.10), ** (p<0.05), *** (p<0.01) care use at age 2 is predicted to have both hastened the return to work of mothers and that there is persistence in that mothers stay more attached to the labor market also in the following years.

6 Specification checks

Despite our estimation strategy 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 in several different ways and also investigate alternative explanations for our findings. Estimates for cohabiting mothers are reported in Table 6 with baseline estimates with and without controls reported at the top. For brevity, results for other groups may be found in the appendix, Tables A.5–A.7 . Overall, robustness checks support our empirical strategy for cohabiting mothers and fathers, while results for the smaller groups of single mothers and non-resident fathers tend to vary more, in line with the general impression of lower precision of these estimates.

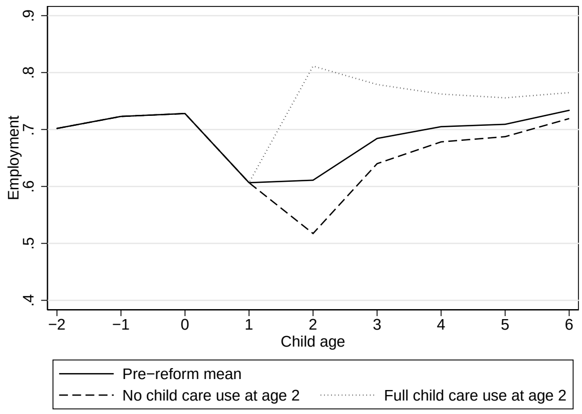
Common time trends

The basic identifying assumption in our study rests on common time shocks across municipalities with different growth rates in child care coverage rates of 1 and 2 year olds. We therefore start by challenging this assumption in a series of specification checks reported in panel A of Table 6.

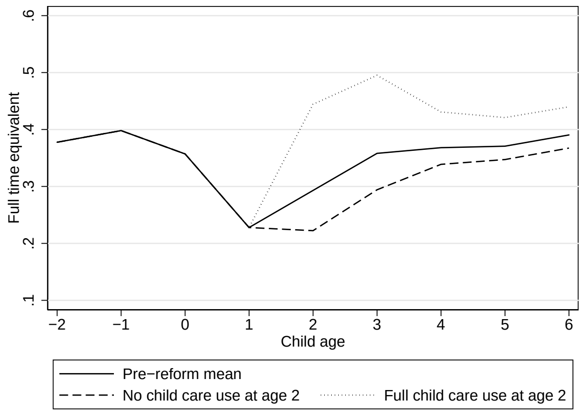
First, we perform a placebo test where we use as the outcome the labor supply of mothers in year $t-3$, i.e. one year before giving birth. Labor supply before birth should not be affected by changes in the child care coverage rate that occur several years later. If our estimations are, in contrast, picking up secular differences in the growth of maternal labor supply over time which is just correlated with child care coverage rates, 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 all close to zero and insignificant, lending support to our empirical strategy.

To further challenge the assumption of common trends, 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. This is a simple yet demanding test of the assumption of common time shocks, that will remove systematic trends in labor supply within municipalities. If, e.g., unobserved trends in labor supply

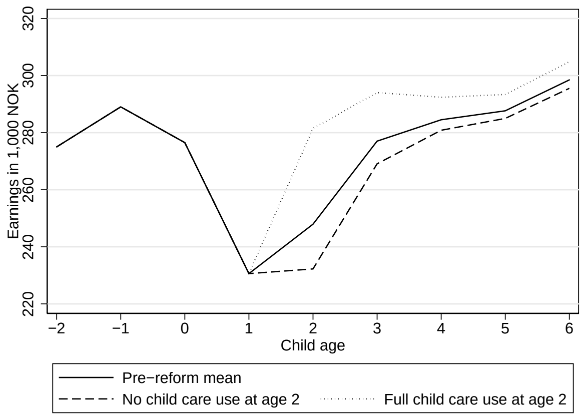
several months of parental leave, many for more than one year.



(a) Employment



(b) Full time-equivalent



(c) Earnings

Figure 8: Persistency: Labor supply of cohabiting mothers around birth, pre-reform means and predicted with and without child care at age 2.

Note: The solid line plots average labor supply of cohabiting mothers of children born 2000. To construct the dotted and dashed lines, we use the estimate from our baseline model, cf. Table 4. The lower dashed line subtracts from the solid line the estimated effect of mean child care use of 2-year olds. The upper dotted line adds the effect of increasing to full child care use at age 2.

Table 6: Specification checks for cohabiting mothers

	Labor supply			**Weeks of empl.**		***N***
	Earnings	*Empl.*	*Full time*	*>4h*	*>30h*	
Baseline results	49,231***	0.294***	0.222***	9.741***	11.02***	283,868
	(12,612)	(0.0298)	(0.0278)	(1.527)	(1.588)	
No controls	65,647***	0.342***	0.272***	11.51***	13.17***	283,868
	(18,212)	(0.0328)	(0.0311)	(1.915)	(1.860)	
A. Common time trends						
Placebo: Outcome	-6,525	0.0216	-0.0575	0.480	1.918	193,567
in $t-3$	(13,582)	(0.0456)	(0.0530)	(2.192)	(2.538)	
Municipality-specific	72,859***	0.317***	0.270***	11.51***	12.66***	283,868
linear time trends	(14,267)	(0.0493)	(0.0512)	(2.316)	(2.675)	
Municipality-specific	82,108***	0.355***	0.261***	10.89***	14.12***	283,868
quadratic time trends	(18,672)	(0.0645)	(0.0638)	(3.055)	(3.538)	
Interacted	76,944***	0.286***	0.231***	10.93***	11.05***	283,624
time shocks	(11,049)	(0.0405)	(0.0373)	(1.810)	(2.113)	
B. Reverse causality: Excess demand						
Waiting lists	54,569***	0.278***	0.236***	8.547***	10.77***	201,321
	(11,887)	(0.0393)	(0.0392)	(1.693)	(1.985)	
Waiting lists above 10	40,091***	0.215***	0.223***	5.637***	7.724***	117,790
percent of available places	(14,803)	(0.0486)	(0.0509)	(1.765)	(2.188)	
C. Alternative drivers						
Selective migration:	31,658*	0.295***	0.185***	8.504***	10.73***	271,963
residence fixed in $t-3$	(18,478)	(0.0377)	(0.0329)	(1.801)	(2.216)	
Endogenous fertility:	30,371***	0.201***	0.0904***	8.590***	8.780***	344,123
include younger siblings	(10,413)	(0.0285)	(0.0306)	(1.417)	(1.379)	
Population growth:	74,946***	0.272***	0.221***	10.26***	11.65***	283,929
instruments = $\frac{\text{slots}}{\text{pop2001}}$	(9,950)	(0.0286)	(0.0282)	(1.294)	(1.538)	
Older siblings	56,826***	0.295***	0.240***	9.883***	11.32***	283,868
	(11,312)	(0.0330)	(0.0331)	(1.588)	(1.744)	
D. Family-specific fixed effects						
Baseline,	56,399***	0.261***	0.198***	8.479***	8.033***	70,907
sample > 2 children	(15,490)	(0.0487)	(0.0551)	(2.187)	(2.770)	
Individual fixed effects	35,865**	0.195***	0.144**	4.501*	3.408	70,907
sample > 2 children	(16,075)	(0.0577)	(0.0664)	(2.355)	(3.068)	

Note: The table reports IV-estimates on child care use from a series of specification checks described in the main text. Standard errors in parentheses are clustered at the municipality level and robust to heteroskedasticity. * (p<0.10), ** (p<0.05), *** (p<0.01)

were pushing up child care coverage rates and thereby driving our results, we would expect estimates on child care coverage rates to drop towards zero when we include these trends. Reassuringly, these estimates are remarkably stable to this inclusion.

Of course, we cannot be sure that unobserved trends are linear or quadratic. In row 4 of panel A, we therefore instead allow for different time shocks to labor supply depending on pre-reform characteristics in 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.

$$m_{ikt} = \alpha_k^1 + \tau_t^1 + \pi_1 CC_{k,t-1}^1 + \pi_2 CC_{kt}^2 + \boldsymbol{\varphi_t^1 V_{k,2001}} + \boldsymbol{X_{ikt}\theta_1} + \epsilon_{ikt}^1 \tag{3}$$

$$y_{ikt} = \alpha_k^2 + \tau_t^2 + \beta m_{ikt} + \boldsymbol{\varphi_t^2 V_{k,2001}} + \boldsymbol{X_{ikt}\theta_2} + \epsilon_{ikt}^2 \tag{4}$$

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, economical and geographical characteristics measured in 2001, before the sample period.[21] The yearly shocks to labor supply thus consist of two parts: The common shock τ_t and a set of shocks $\boldsymbol{\varphi_t}$ that are propagated through the pre-reform characteristics of the municipality. Again, we find that estimates are very stable to this inclusion, cf. the fourth row in panel A of Table 6.

Excess demand

Our empirical strategy relies on growth in child care coverage rates being exogenous 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 coverage rates could, at least in part, be driven by growth in labor supply generated by increased demand, and not vice versa. To investigate whether this might be driving our estimates, we could narrow down our sample to municipalities with significant excess demand, where such reverse causality should not be an issue. To this end, we exploit the data on waiting lists for 0–3 year olds. As discussed above, this measure may be likely to underestimate total demand.

In panel B of Table 6, we reduce our sample by narrowing down first to municipality-years with 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.[22] In these samples, excess demand should be strong enough to mitigate worries about reverse

[21]Specifically, these include the share of left wing representatives in the municipal council, share of females 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 child care coverage rate and initial unemployment rate.

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

causality. Estimates in these samples are very similar to our baseline estimates.

Alternative drivers

In panel C of Table 6, we investigate some alternative drivers that might have explained our estimates. First, if caregivers with high levels of labor supply move to municipalies 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 caregiver'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. Estimates are relatively stable, indicating that selective migration is unlikely to be an issue.

Second, in line with previous literature we have considered the youngest child. This ensures that the systematic spacing of births does not influence our estimates. If fertility is endogenous to child care availability, however, 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. It is reassuring to see in Table 6 that the estimates are almost unchanged.

Third, our instruments are constructed as the ratio of children in care to the population of the same age. Changes in these rates will 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 population in 2001 in the denominator for the child care coverage rates. It is reassuring to see that the estimates in row 3 in panel C of Table 6 are again very similar to our baseline estimates.

Fourth, we might worry that a contemporary expansion among older children confounds the estimates. We see from Figure 1a that the reform is associated with increases in the coverage rate 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. It is comforting, however, to see that our estimates barely move when we include a control for the child care coverage rate of 3-5 year old children.

Family fixed effects

Finally, in our baseline specification, we use municipality fixed effects. These will 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 these characteristics are not controlled for, our results might be biased.

In Figure 6, we saw that the composition in terms of some important characteristics did not seem to change systematically with increases in the coverage rate. As an additional check, panel D of Table 6 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 coverage rates than the other. By relating the change in labor supply to the change in coverage rates within families, we can hold constant all observable and unobservable characteristics that are fixed within families over time. At the same time, the usual time effects account for changes over time that are comparable across families.

Because parents with siblings in the 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 overall 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 coverage rates, controlling for municipality and year fixed effects to exploit only changes in coverage. Our approach is supported by a battery of robustness tests that investigate alternative explanations for our findings.

Results show relatively large labor supply response 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 0.3, indicating that 3 cohabiting mothers enter the labor force for each 10 toddlers who enroll in full time care. 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.

Single mothers, on the other hand, seem to respond by moving from no employment to part-time work. Per 5 toddlers of a single mother enrolled full time, we find that one single mother enters the labor force, compared to a pre-reform mean of 62 % of single mothers being out of the labor force. However, we find no effect on the probability of holding a full-time job, indicating that these mothers move from no employment to part-time work in response to child care. We find no effects among fathers, nor among grandparents.

The labor supply estimates are in the upper range of the literature, which tends to find effects of child care in the range of 0 to 0.2. This might indicate that the response to child care availability differs for children of different ages. 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. This explanation may find some support in the fact that we find no effects on the labor supply of working age grandparents. For fathers, we find little effect, both for cohabiting fathers and for fathers not residing with the child. This is in line with the literature, and suggests that mothers are still the primary caretaker of young children in Norway.

Havnes and Mogstad (2011) study a Norwegian expansion of care for preschoolers in the late 1970s, and find estimates around a fifth of 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. To shed light on this, we have looked at survey data on the preferred mode of child care collected in 1968 and in 2002 (Pettersen, 2003; Ministry of Consumer Affairs and Administration, 1972; Moafi and Bjørkli, 2011; 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).

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 considerable increases in net tax payments, corresponding to a marginal tax rate of about 40 % on the extra 50,000 NOK income. This is close to the average tax rate paid by cohabiting mothers before the reform, and shows that there is some scope for the costs of child care subsidies to be offset by increased tax revenue.

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.

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A Additional tables

This appendix presents various results that we could not fit in the main text. In particular, most results for other samples than the main sample of cohabiting mothers are found here.

- Tables A.1, A.2 and A.3 report detailed summary statistics for single mothers, cohabiting fathers and non-resident fathers.
- Table A.4 reports main results for working age grandparents.
- Tables A.5, A.6 and A.7 report specification checks for single mothers, cohabiting fathers and non-resident fathers.
- Tables A.8, A.9 and A.10 report persistency analysis for single mothers, cohabiting fathers and non-resident fathers.

Table A.1: Descriptive statistics for single mothers, 2002–2008

		2002	2004	2006	2008
A. Outcome variables					
Earnings (NOK)		76,279	81,964	95,042	128,101
		(114,835)	(120,484)	(135,387)	(166,619)
Tax		32,572	35,177	27,513	32,729
		(68,281)	(60,206)	(51,812)	(66,789)
Transfers, excl. cash for care		156,275	150,549	155,492	161,878
		(73,977)	(82,312)	(85,066)	(96,435)
Employed		0.289	0.284	0.306	0.364
		(0.45)	(0.45)	(0.46)	(0.48)
Full-time equivalent employment		0.133	0.135	0.150	0.182
		(0.34)	(0.34)	(0.36)	(0.39)
Weeks of employment above	4 hours	19.49	20.31	21.02	24.75
		(23.1)	(23.8)	(23.6)	(23.8)
	30 hours	11.49	12.58	12.90	15.78
		(20.3)	(21.2)	(21.1)	(22.4)
B. Control variables					
Age		28.99	29.27	29.79	29.98
		(6.09)	(6.27)	(6.30)	(6.55)
Immigrant		0.0674	0.0760	0.100	0.108
		(0.25)	(0.26)	(0.30)	(0.31)
Years of education		12.97	13.09	13.19	13.28
		(3.11)	(3.23)	(3.32)	(3.44)
Number of children		1.695	1.703	1.744	1.717
		(0.99)	(1.04)	(1.07)	(1.07)
below 6 years old		1.289	1.280	1.272	1.273
		(0.51)	(0.51)	(0.50)	(0.51)
below 13 years old		1.556	1.537	1.564	1.541
		(0.79)	(0.80)	(0.82)	(0.81)
below 18 years old		1.656	1.654	1.684	1.660
		(0.92)	(0.95)	(0.96)	(0.97)
Local employment rate, childless men		0.673	0.703	0.727	0.797
		(0.058)	(0.054)	(0.057)	(0.056)
Observations		5,278	4,871	4,491	4,619

Note: The table gives biannual means and standard deviations (in parentheses) in the estimation sample. Variables are defined in Section 3.

Table A.2: Descriptive statistics for cohabiting fathers, 2002–2008

		2002	2004	2006	2008
A. Outcome variables					
Earnings (NOK)		356,127	373,361	420,824	490,135
		(232,035)	(290,088)	(315,032)	(374,122)
Tax		162,260	204,359	168,930	171,219
		(180,796)	(303,939)	(244,858)	(285,603)
Transfers, excl. cash for care		12,953	16,446	14,028	13,012
		(41,721)	(51,166)	(49,043)	(49,992)
Employed		0.919	0.902	0.915	0.928
		(0.27)	(0.30)	(0.28)	(0.26)
Full-time equivalent employment		0.848	0.820	0.844	0.865
		(0.36)	(0.38)	(0.36)	(0.34)
Weeks of	4 hours	43.38	43.00	43.63	44.76
employment		(18.1)	(18.5)	(17.9)	(16.8)
above	30 hours	41.65	41.08	41.73	42.83
		(19.6)	(20.1)	(19.5)	(18.7)
B. Control variables					
Age		35.10	35.45	35.76	35.84
		(5.73)	(5.78)	(5.79)	(5.91)
Immigrant		0.0615	0.0713	0.0768	0.0903
		(0.24)	(0.26)	(0.27)	(0.29)
Years of education		14.83	14.98	15.11	15.21
		(3.16)	(3.18)	(3.19)	(3.20)
Number of children		2.068	2.055	2.054	2.039
		(1.05)	(1.05)	(1.04)	(1.04)
below 6 years old		1.510	1.516	1.516	1.523
		(0.58)	(0.58)	(0.58)	(0.58)
below 13 years old		1.895	1.875	1.874	1.861
		(0.85)	(0.84)	(0.83)	(0.83)
below 18 years old		2.001	1.987	1.982	1.966
		(0.96)	(0.95)	(0.94)	(0.94)
Local employment rate, childless men		0.676	0.706	0.726	0.800
		(0.052)	(0.051)	(0.052)	(0.047)
Observations		42,156	39,284	40,201	41,703

Note: The table gives biannual means and standard deviations (in parentheses) in the estimation sample. Variables are defined in Section 3.

Table A.3: Descriptive statistics for non-residing fathers, 2002–2008

		2002	2004	2006	2008
A. Outcome variables					
Earnings (NOK)		227,717	215,469	263,236	315,172
		(185,741)	(182,193)	(224,525)	(294,446)
Tax		96,308	116,361	102,842	107,709
		(111,020)	(142,343)	(141,550)	(133,229)
Transfers, excl. cash for care		35,143	44,022	39,549	37,934
		(58,343)	(66,673)	(74,148)	(73,574)
Employed		0.717	0.652	0.707	0.738
		(0.45)	(0.48)	(0.46)	(0.44)
Full-time equivalent employment		0.560	0.484	0.552	0.585
		(0.50)	(0.50)	(0.50)	(0.49)
Weeks of employment above	4 hours	31.67	30.19	32.87	34.85
		(23.4)	(23.8)	(23.0)	(22.5)
	30 hours	28.85	26.66	29.84	31.93
		(24.0)	(24.3)	(23.7)	(23.5)
B. Control variables					
Age		31.92	32.23	32.66	32.85
		(7.24)	(7.43)	(7.41)	(7.73)
Immigrant		0.0710	0.0706	0.0923	0.0964
		(0.26)	(0.26)	(0.29)	(0.30)
Years of education		12.86	12.93	12.92	12.93
		(2.90)	(2.97)	(2.98)	(3.11)
Number of children		1.785	1.792	1.822	1.857
		(1.12)	(1.15)	(1.15)	(1.18)
below 6 years old		1.333	1.323	1.323	1.337
		(0.57)	(0.57)	(0.56)	(0.59)
below 13 years old		1.585	1.580	1.596	1.610
		(0.83)	(0.85)	(0.85)	(0.87)
below 18 years old		1.694	1.697	1.729	1.744
		(0.97)	(1.01)	(1.01)	(1.02)
Local employment rate, childless men		0.674	0.702	0.724	0.794
		(0.057)	(0.054)	(0.061)	(0.060)
Observations		4,650	4,249	3,967	4,026

Note: The table gives biannual means and standard deviations (in parentheses) in the estimation sample. Variables are defined in Section 3.

Table A.4: IV-results for working age grandparents

		Maternal grandmothers	Maternal grandfathers	Paternal grandmothers	Paternal grandfathers
A. First stage					
$CC^1_{k,t-1}$		0.538***	0.541***	0.533***	0.536***
		(0.0185)	(0.0183)	(0.0217)	(0.0185)
$CC^2_{k,t}$		-0.0153	-0.00914	-0.00302	-0.00154
		(0.0146)	(0.0150)	(0.0184)	(0.0187)
F		590.7	632.6	501.4	743.3
B. Second stage outcomes					
Earnings		825	3,927	5,655	-11,274
		(7029.6)	(12636.2)	(6,303)	(10,875)
		<u>148,787</u>	<u>271,721</u>	<u>142,207</u>	<u>260,781</u>
Employed		0.0112	-0.0311*	0.00336	-0.0260
		(0.0176)	(0.0165)	(0.0190)	(0.0194)
		<u>0.588</u>	<u>0.709</u>	<u>0.556</u>	<u>0.677</u>
Full-time		0.0106	-0.0298	0.0361**	-0.0150
		(0.0166)	(0.0183)	(0.0171)	(0.0204)
		<u>0.353</u>	<u>0.628</u>	<u>0.334</u>	<u>0.595</u>
Weeks of	above 4h	0.240	-0.652	-0.0122	-0.850
employment		(0.925)	(0.968)	(0.913)	(1.008)
above		<u>31.72</u>	<u>32.25</u>	<u>30.38</u>	<u>31.04</u>
	above 30h	1.052	-0.856	1.960*	0.206
		(0.916)	(0.966)	(1.047)	(1.037)
		<u>20.18</u>	<u>29.85</u>	<u>18.98</u>	<u>28.52</u>
Observations		217,210	182,801	192,324	155,629

Note: Estimates are from equations (1) and (2). 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 grandparents of 2-year olds in 2002. * (p<0.10), ** (p<0.05), *** (p<0.01)

Table A.5: Specification checks for single mothers

	Labor supply			Weeks of empl.		N
	Earnings	*Empl.*	*Full time*	*>4h*	*>30h*	
Baseline results	19,340	0.225**	0.0486	12.24**	2.424	33,288
	(28,999)	(0.107)	(0.0737)	(5.817)	(4.485)	
No controls	46,898	0.307***	0.106	15.7***	5.75	33,288
	(31,542)	(0.110)	(0.0757)	(5.83)	(4.56)	
A. Common time trends						
Placebo: Outcome	1,872	0.0127	0.102	-1.207	-2.459	20,947
in $t-3$	(42,915)	(0.184)	(0.153)	(7.837)	(8.301)	
Municipality-specific	86,762*	0.592***	0.302**	22.10*	8.973	33,288
linear time trends	(50,344)	(0.224)	(0.151)	(11.44)	(9.211)	
Municipality-specific	81,412	0.708**	0.225	27.17*	6.711	33,288
quadratic time trends	(72,065)	(0.320)	(0.220)	(16.09)	(13.02)	
Interacted	81,412	0.708**	0.225	27.17*	6.711	33,288
time shocks	(72,065)	(0.320)	(0.220)	(16.09)	(13.02)	
B. Reverse causality: Excess demand						
Waiting lists	10,161	0.296*	0.0645	10.93	-2.666	22,925
	(42,795)	(0.152)	(0.112)	(7.779)	(6.370)	
Waiting lists above 10	2,859	0.181	0.121	6.040	-3.325	13,270
percent of available places	(62,090)	(0.215)	(0.151)	(9.788)	(7.904)	
C. Alternative drivers						
Selective migration:	32,228	0.279**	0.0914	8.462	5.910	31,815
residence fixed in $t-3$	(33,157)	(0.117)	(0.0847)	(6.381)	(5.286)	
Endogenous fertility:	31,543	0.182*	0.144**	11.95**	6.352	30,910
include younger siblings	(21,759)	(0.110)	(0.0644)	(6.053)	(4.545)	
Population growth:	-887	0.129	-0.0182	10.17*	0.0240	33,299
instruments = $\frac{\text{slots}}{\text{pop2001}}$	(27,684)	(0.106)	(0.0743)	(5.516)	(5.030)	
Older siblings	23,512	0.241**	0.0678	11.12*	1.926	33,288
	(30,985)	(0.117)	(0.0816)	(6.103)	(4.936)	
D. Family-specific fixed effects						
Baseline,	88,433	0.310	-0.0933	16.59	-13.74	1,790
sample > 2 children	(64,574)	(0.280)	(0.203)	(13.62)	(11.48)	202
Individual fixed effects	88,302	0.340	-0.0417	4.879	-10.22	1,790
sample > 2 children	(72,261)	(0.340)	(0.247)	(16.79)	(13.87)	202

Note: The table reports IV-estimates on child care use from a series of specification checks described in the main text. Standard errors in parentheses are clustered at the municipality level and robust to heteroskedasticity. * (p<0.10), ** (p<0.05), *** (p<0.01)

Table A.6: Specification checks for cohabiting fathers

	Labor supply			**Weeks of empl.**		*N*
	Earnings	*Empl.*	*Full time*	*>4h*	*>30h*	
Baseline results	-45,970	-0.0187	0.00848	-0.0966	-0.568	283,687
	(32,269)	(0.0219)	(0.0288)	(1.351)	(1.500)	
No controls	-18,586	0.00676	0.0431	1.391	1.093	283,687
	(31,619)	(0.0244)	(0.0314)	(1.494)	(1.658)	
A. Common time trends						
Placebo: Outcome	22,543	0.0210	0.0681	2.978	1.772	196,725
in $t-3$	(34,666)	(0.0342)	(0.0435)	(1.955)	(2.119)	
Municipality-specific	15,222	0.0218	0.0722*	1.333	0.885	283,687
linear time trends	(30,332)	(0.0313)	(0.0437)	(2.077)	(2.205)	
Municipality-specific	-7,384	0.0399	0.0573	2.829	2.352	283,687
quadratic time trends	(51,755)	(0.0381)	(0.0562)	(2.655)	(2.846)	
Interacted	2,803	0.0177	0.0563*	1.901	1.557	283,436
time shocks	(28,307)	(0.0241)	(0.0340)	(1.678)	(1.829)	
B. Reverse causality: Excess demand						
Waiting lists	-29,248	-0.0105	0.0292	0.737	0.246	201,193
	(34,933)	(0.0270)	(0.0344)	(1.619)	(1.767)	
Waiting lists above 10	-77,448*	-0.0274	0.0157	2.044	0.151	117,715
percent of available places	(42,057)	(0.0299)	(0.0380)	(1.621)	(1.897)	
C. Alternative drivers						
Selective migration:	-120,810*	-0.0271	0.0160	-1.473	-1.309	275,397
residence fixed in $t-3$	(63,504)	(0.0251)	(0.0352)	(1.579)	(1.748)	
Endogenous fertility:	-59,027**	-0.0102	-0.0270	0.0584	-0.189	343,898
include younger siblings	(22,867)	(0.0183)	(0.0244)	(1.205)	(1.378)	
Population growth:	-82	0.0158	0.0241	1.528	1.601	283,741
instruments = $\frac{\text{slots}}{\text{pop2001}}$	(29,403)	(0.0189)	(0.0263)	(1.122)	(1.243)	
Older siblings	-39,739	-0.0125	0.0281	0.362	-0.0394	283,687
	(29,777)	(0.0233)	(0.0297)	(1.404)	(1.553)	
D. Family-specific fixed effects						
Baseline,	-111,522**	-0.0459	0.00887	1.492	1.730	70,783
sample > 2 children	(46,367)	(0.0301)	(0.0454)	(1.965)	(2.443)	
Individual fixed effects	-151,387**	-0.0659**	0.0289	0.884	1.051	70,783
sample > 2 children	(75,823)	(0.0331)	(0.0519)	(1.642)	(2.147)	

Note: The table reports IV-estimates on child care use from a series of specification checks described in the main text. Standard errors in parentheses are clustered at the municipality level and robust to heteroskedasticity. * (p<0.10), ** (p<0.05), *** (p<0.01)

Table A.7: Robustness checks for non-residing fathers

	Labor supply			**Weeks of empl.**		*N*
	Earnings	*Empl.*	*Full time*	*>4h*	*>30h*	
Baseline results	-8,241	-0.0158	0.0330	-1.343	-1.053	29,272
	(26,111)	(0.0502)	(0.0624)	(2.615)	(2.612)	
No controls	43,809	0.0425	0.121*	0.990	1.404	29,272
	(28,737)	(0.0558)	(0.0670)	(2.808)	(2.614)	
A. Common time trends						
Placebo: Outcome	-46,404	0.00910	-0.114	-1.506	-7.057	19,381
in $t-3$	(36,332)	(0.104)	(0.0974)	(4.706)	(5.470)	
Municipality-specific	-3,866	-0.0107	0.0395	-0.0705	-0.0368	29,272
linear time trends	(28,935)	(0.0589)	(0.0669)	(2.942)	(2.930)	
Municipality-specific	10,641	-0.0116	0.0439	0.466	0.367	29,272
quadratic time trends	(33,407)	(0.0592)	(0.0684)	(3.056)	(3.031)	
Interacted	37,328	0.108	0.215*	2.757	3.826	29,196
time shocks	(54,771)	(0.0912)	(0.117)	(4.870)	(5.577)	
B. Reverse causality: Excess demand						
Waiting lists	-35,513	-0.0536	0.0222	-0.810	-1.044	20,133
	(35,620)	(0.0674)	(0.0780)	(3.622)	(3.581)	
Waiting lists above 10	-11,425	0.0854	0.0658	-0.190	-0.470	11,324
percent of available places	(47,610)	(0.104)	(0.114)	(5.606)	(5.458)	
C. Alternative drivers						
Selective migration:	-72,017	0.138	0.0330	-3.886	-1.973	28,500
residence fixed in $t-3$	(61,792)	(0.127)	(0.152)	(6.908)	(6.999)	
Endogenous fertility:	-43,588**	-0.0292	-0.0894	-1.569	-1.165	32,630
include younger siblings	(21,352)	(0.0543)	(0.0570)	(2.739)	(2.797)	
Population growth:	-136,199	-0.158	-0.0676	-6.728	-6.093	29,290
instruments = $\frac{\text{slots}}{\text{pop2001}}$	(94,641)	(0.167)	(0.159)	(7.333)	(7.492)	
Older siblings	-22,280	-0.0706	0.0172	-1.754	-1.880	29,272
	(30,375)	(0.0573)	(0.0684)	(2.990)	(2.848)	
D. Family-specific fixed effects						
Baseline,	7,264	-0.0908	-0.163	11.48	12.39	1,810
sample > 2 children	(94,931)	(0.246)	(0.276)	(12.95)	(13.48)	
Individual fixed effects	57,566	0.196	-0.0583	6.949	9.310	1,810
sample > 2 children	(66,962)	(0.205)	(0.220)	(10.14)	(10.53)	

Note: The table reports IV-estimates on child care use from a series of specification checks described in the main text. Standard errors in parentheses are clustered at the municipality level and robust to heteroskedasticity. * (p<0.10), ** (p<0.05), *** (p<0.01)

Table A.8: Persistency: IV-results with lagged outcomes, single mothers

Outcome	t **(baseline)**	$t+1$	$t+2$	$t+3$	$t+4$
Earnings	19,340	-5,793	-33,743	-30,653	-21,369
	(28,999)	(25,243)	(28,021)	(31,862)	(32,835)
	76,279	122,494	149,617	173,734	195,434
Tax	-6,350	6,738	-9,462	-4,307	94,13
	(13,986)	(9,352)	(10,464)	(10,336)	(10,875)
	32,572	36,102	40,166	44,678	50,143
Transfers,	38,780**	31,943*	38,610**	14,703	23,793
excl .cash for care	(18,020)	(18,690)	(19,260)	(18,509)	(19.038)
	156,275	149,231	134,009	126,253	122.663
Employed	0.225**	0.150	0.00248	0.0461	-0.0387
	(0.107)	(0.0944)	(0.0957)	(0.0940)	(0.0983)
	0.289	0.391	0.471	0.522	0.561
Full-time	0.0486	0.0508	-0.000325	0.00249	0.133
	(0.0737)	(0.0732)	(0.0746)	(0.0850)	(0.0959)
	0.133	0.197	0.237	0.279	0.313
Observations	33,288	33,062	32,885	32,754	32,653

Note: Estimates are from equation (1), with outcomes measured 1–4 years later. In addition to control variables defined in Section 3, we control for child care coverage rates for older children in year t. 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 children born in 2002. * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$)

Table A.9: Persistency: IV-results with lagged outcomes, cohabiting fathers

Outcome	t **(baseline)**	$t+1$	$t+2$	$t+3$	$t+4$
Earnings	-45,971	2,842	-5,067	1,519	2,639
	(32,269)	(21,120)	(19,196)	(20,314)	(20,278)
	356,127	435,776	463,734	49,4573	523,611
Tax	-8,411	9,471	14,929	31,774**	13,500
	(11,901)	(15,930)	(20,027)	(15,484)	(15,461)
	162,260	179,866	185,925	185,715	191,077
Transfers,	-584	619	-1,191	-6,253	-8,168*
excl .cash for care	(3,647)	(5,162)	(4,395)	(4,579)	(4,895)
	12,953	17,652	19,512	20,360	21,503
Employed	-0.0187	-0.0197	-0.0112	-0.0106	-0.0173
	(0.0219)	(0.0189)	(0.0215)	(0.0237)	(0.0229)
	0.919	0.913	0.912	0.910	0.908
Full-time	0.00848	0.000609	-0.0233	-0.00683	0.00878
	(0.0288)	(0.0235)	(0.0252)	(0.0251)	(0.0266)
	0.848	0.845	0.849	0.851	0.852
Observations	283,687	282,408	281,474	280,790	280,217

Note: Estimates are from equation (1), with outcomes measured 1–4 years later. In addition to control variables defined in Section 3, we control for child care coverage rates for older children in year t. 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 children born in 2002. * (p<0.10), ** (p<0.05), *** (p<0.01)

Table A.10: Persistency: IV-results with lagged outcomes, non-residing fathers

Outcome	t **(baseline)**	$t+1$	$t+2$	$t+3$	$t+4$
Earnings	-8,241	7,594	-28,234	-18,694	-46,837
	(26,111)	(55,793)	(30,637)	(28,529)	(33,966)
	227,717	270,457	292,054	312,342	334,088
Tax	-17,492	-15,022	-8,577	-346	-5,543
	(13,658)	(12,680)	(16,715)	(13,691)	(21,168)
	96,308	108,836	112,865	112,104	116,645
Transfers,	6,401	9,440	16,441	1,784	2,534
excl .cash for care	(7,757)	(8,227)	(11,850)	(10,160)	(11,565)
	35,143	45,805	49,769	5,2055	54,866
Employed	-0.0158	-0.0414	0.00333	-0.0224	-0.0369
	(0.0502)	(0.0515)	(0.0540)	(0.0498)	(0.0537)
	0.717	0.703	0.707	0.709	0.712
Full-time	0.0330	0.00222	-0.0402	-0.0674	-0.0683
	(0.0624)	(0.0553)	(0.0565)	(0.0574)	(0.0569)
	0.560	0.551	0.571	0.579	0.594
Observations	29,272	29,071	28,927	28,789	28,659

Note: Estimates are from equation (1), with outcomes measured 1–4 years later. In addition to control variables defined in Section 3, we control for child care coverage rates for older children in year t. 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 children born in 2002. * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$)

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