

Child care and parental labor supply: A new look*

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Abstract

Existing estimates of how child care affects parental labor supply suggest relatively modest impacts. One reason may be that parents have access to other informal sources of care that may be unobservable in the data, like family, friends or nannies. We revisit the question of how child care impacts parental labor supply using detailed Norwegian data on both parental earnings and work hours, as well as child care coverage and enrollment. Exploiting the variation in expansion across municipalities and over time following a large-scale Norwegian child care reform from 2002, we investigate the impact of child care use for toddlers on parents' labor supply. We reveal first that the impacts of child care on labor supply on mothers may be larger than suggested by much of the previous literature, both for single and married or cohabiting mothers. Next, we show that child care has little effect on the labor supply of fathers and grandparents, indicating that mothers are still the primary caretakers and that grandparents may not be important providers of informal care for this group of children. Despite the increased labor supply, we find little support for increases in the tax base mitigating the cost of the reform. The results are supported by a range of specification and robustness checks.

JEL-codes: H40, J13, J21

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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 recently 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 also means that the cost of providing child care is partly mitigated by increases in the tax base.

In this paper, we study 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 gender equality in the labor market ?. 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 of child care coverage 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 the child care use. Our estimates therefore reflect how the actual use of a full year of

child care affects mothers labor supply, rather than the effect of additional child care slots directly.

If we believe that initial take-up of child care is lower than long run take-up, then this scaling will be important to get good estimates of the impact of child care 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 in October, it should not come as a surprise that parents are not exploiting new places in child care to capacity over the year.

Ours is also among the first papers to provide evidence on how large-scale, universally available 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, if it is taken up by mothers, 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 2.6 married mothers enter the labor force.¹ For single mothers, we also find strong effects, with two mothers entering the labor force for every 10 children that enroll in full time child care, compared to a labor force participation of only 38 % among these mothers prior to the child care expansion. Overall, this corresponds to a modest earnings increase of 60,000 NOK (7,200 USD) and 30,000 NOK (3,600 USD) for cohabiting and single mothers, respectively. We also investigate persistence in the labor market responses, finding positive labor supply impacts 1–3 years later from the child care enrollment when the child was two.

These labor supply effects are larger than most of the credible estimates from the literature on older children. Despite these relatively large effects on labor supply, we find small and nonsignificant results for total tax payments. The lack of impact on taxes paid may be the result of nonlinearities in the tax schedule, whereby the increases from relatively low incomes that we estimate, would be taxed at a low rate. We therefore find little practical support for the argument that child care increases the tax base and offsets

¹Throughout, we refer to married and cohabiting mothers interchangeably - our main sample consists of both married mothers and unmarried mothers living together with the child and the father of the child, as cohabitation without marriage is common in Norway.

costs.

For fathers and grandparents we find no labor supply response. This may indicate that mothers are still the primary caretakers, who stay 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 is a key insight when considering how child care may affect child development.

Our paper contributes to a rapidly growing literature estimating how child care availability affects parental labor supply using quasi-experimental methods. In section 2 we review a range of studies of labor supply elasticities. The evidence from quasi experimental studies indicate relatively small effects of child care expansion. Out of the 16 reviewed studies using quasi-experimental methods, nine find effects that are not significantly different from zero, another three find elasticities below 0.1, while the last four studies find more sizable impacts ranging from 0.3 to 0.5.

Although newer methods are clearly driving some of the differences between these estimates, Cascio (2009) argues that the drop in estimated price elasticities might indicate that mothers are not as sensitive to prices of child care arrangements as they used to be. Another potential reason for the diversity is the differences in the complier groups affected. Some papers study expansions of relatively costly child care arrangements (Havnes and Mogstad, 2011). Under this scenario, mothers are less likely to respond to the expansion unless they intend to start working to offset the costs. In contrast, when the reform expands free preschool (Goux and Maurin, 2010; Gelbach, 2002), a mother's cost of enrolling even if she does not start working is low. This makes us expect lower impact when prices are low. Another potential explanation is differences in the counterfactual mode of care that could be driven by the age of the child or the culture of the country studied: Where mothers choose to stay home with the child in absence of formal care rather than rely on some form of informal care to be able to work, effects are likely to be larger.

The lack of robust evidence in favor 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 in part by less than full take-up of child care in the initial periods following child care expansions as well as informal substitution: Rather than releasing mothers to the labor market, child care is taken up by mothers already working and relying on some form of informal care arrangement.

The paper proceeds as follows: We first cover the previous literature in section 2, extending the literature review by Blau and Currie (2006) for the last decade and other countries than the US and Canada. The institutional setting and the child care reform is covered in section 3, while section 4 presents the registry data we use for estimation, the

samples of interest and some descriptive statistics. Section 5 presents our IV method with fixed effects. Results are found in section 6, including persistency analysis. We perform a range of robustness checks to support our estimates in section 7 before , while section 8 concludes.

2 Literature

We are not the first to investigate the impact of child care availability or prices on mothers' labor supply. Blau and Currie (2006) cover more than 20 early studies on labor supply elasticities with respect to child care prices in the US and Canada. Most of the literature focuses on the extensive margin of labor supply, and the estimates reported here are for this margin and for married or cohabiting mothers unless otherwise noted.

The earlier strand of the literature uses discrete choice models. These do not take into account the joint nature of child care and employment choices, and are likely troubled by endogeneity of child care prices. Studies find large diversity in the estimates of elasticities ranging from 0 to more than -1. Other scholars have estimated similar models for Europe, finding results in the same range or a little lower than those found for the US and Canada.² Previous estimates of price elasticities are shown in figure 1 and table 8 in the appendix.

Acknowledging that child care and employment should be considered joint decisions, later scholars investigated the issue with multinomial choice models. These studies find smaller effects of child care on labor supply than the discrete choice models, ranging from 0 to -.38, with the exception of Powell (2002) who finds unusually large, negative estimates.

Multinomial choice models were estimated for other advanced countries, finding estimates of -.1 for France (Choné et al., 2003), -.12 for Russia and Norway (Lokshin, 2004; Kornstad and Thoresen, 2007), -.04 for Germany (Wrohlich, 2011), -.25 for Australia (Breunig et al., 2014) and -.46 for Romania (Lokshin and Fong, 2006). These estimates fall in the same range as those for the US and Canada.

Although some of these studies try to account for the endogeneity of child care expenditure, they are still troubled by selection. To control for this, later scholars analysed the relationship using quasi-experimental methods, trying to isolate exogenous variation in either child care prices or actual enrollment.

The studies that most closely resemble the earlier studies are those that aim to isolate exogenous variation in prices. These include Lundin et al. (2008) and Hardoy and Schøne (2013), who use variation in prices coming from the introduction of max price reforms in Sweden and Norway and the geographical variation in price adjustments to the new

²These include no effects for Italy (Del Boca and Vuri, 2007; Del Boca, 2002), -.02 in Germany (Wrohlich, 2004), -.14 for the UK (Viitanen, 2005), -.29 for Australia (Breunig et al., 2012) and -.93 for Spain (Borra, 2010).

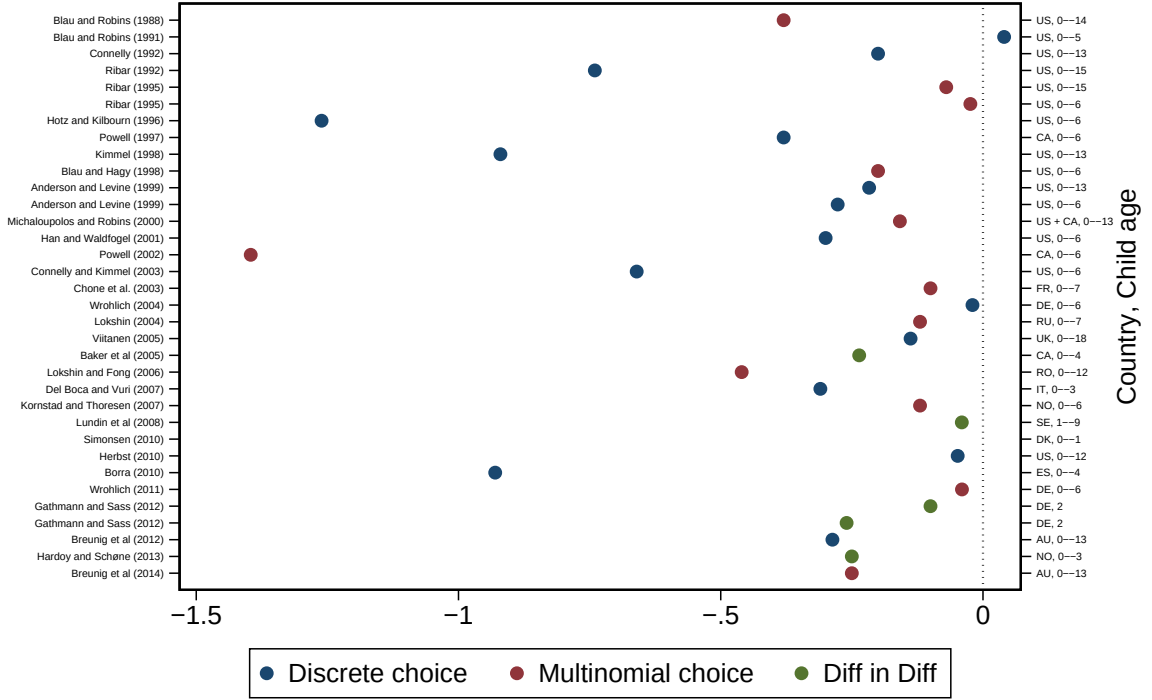


Figure 1: Estimates of elasticity of labor force participation with respect to child care prices

regulated price. Lundin et al. (2008) find no effects from the price change when using matching methods to construct control groups with similar observables, while Hardoy and Schøne (2013) find a price elasticity of -0.25 using a triple difference approach. Gathmann and Sass (2012) use the introduction of a home care benefit in Thuringia, Germany, as an implicit price increase in a Difference-in-Differences (DD) setup, finding a price elasticity of -0.1 or -0.26 , depending on the data source. Furthermore, Herbst (2013) focuses on the variation between US states in subsidy rates stemming from the Lanham Act during the Second World War. His estimates are hard to compare to price elasticities because data on total prices are not available, but he finds that a 45 dollar higher subsidy rate leads to a 4.3 percentage points increase in female labor supply 4--7 years later in a triple difference setup.

In many settings, exogenous variation in prices is not plausibly isolated, but variation in enrollment is. In these cases, we can estimate the enrollment elasticity of labor supply. These may be interpreted analogously to an elasticity—the percentage point change in labor force participation from a one percentage points change in enrollment. A set of previous studies that have investigated this elasticity using quasi-experimental methods are found in table 9 and figure 2.

Gelbach (2002) finds significant positive effects on maternal labor supply of preschool attendance of five-year olds. The magnitude is relatively small, with an enrollment elasticity around 0.05, meaning that 5 % of mothers who enroll actually enter the labor force.

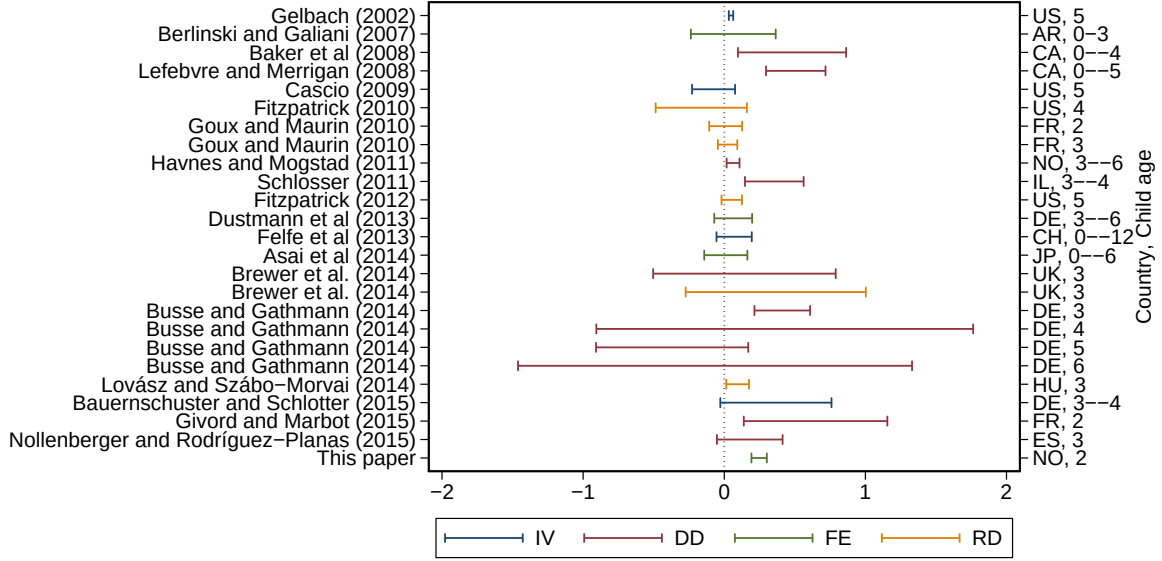


Figure 2: Estimates of enrollment elasticities of labor force participation

Furthermore, Fitzpatrick (2012) uses an RD approach and data from the 2000 census rather than 1980 in an otherwise similar approach, and find that the results hold only for single mothers without other young children.

Fitzpatrick (2010) uses a regression discontinuity approach around the birth date cutoff for free child care eligibility for two states in the US, but find no labor supply response among mothers in two-parent households. Among single mothers, she finds an enrollment elasticity around 0.4, which is in the larger end of the range of these estimates. Goux and Maurin (2010), one of the few papers to investigate child care enrollment among two year olds, use a similar discontinuity in age of school enrollment in France. They find some labor market effects among single mothers, but no response among mothers in two-person households. In contrast, Lovász and Szabó-Morvai (2014) supplements an RD-design with a DD because of simultaneous changes to other family policies in Hungary, finding an enrollment elasticity of around 0.1, indicating that one mother starts working for every 10 children that are induced to enroll. Felfe et al. (2013) use discontinuities in child care provision across cantonal borders in Switzerland as an instrument for enrollment, and find a nonsignificant enrollment elasticity of 0.07.

Another set of studies use DD-setups where a treatment group that is affected by a policy reform can be separated from a control group that is not. Baker et al. (2008) and Lefebvre and Merrigan (2008b) provide among the first studies of universal subsidized child care systems, both studying a Quebec reform that lowered prices of child care. In contrast to the aforementioned studies, they find enrollment elasticities(?) of around .48 and .50,³ respectively, indicating much larger effects. On the other hand, other DD studies find lower estimates: Havnes and Mogstad (2011) find a low 0.06 following a Norwegian

³These are not directly reported, but calculated based on other reported estimates in the papers.

reform in 1975, Schlosser (2011) finds around .35 for Arab mothers in Israel following a subsidized reform rollout, and Nollenberger and Rodríguez-Planas (2011) find an elasticity of 0.3 for mothers of 3 year olds in Spain.

Similarly, several authors have exploited variation in child care availability across time and space, often following reforms aiming to expand access to care, in fixed effects setups. Asai et al. (2014) use variation across time and between prefectures in Japan, finding no effect. Dustmann et al. (2013) find the same from an expansion in availability in Germany, and Berlinski and Galiani (2007) find non-significant labor supply response to an Argentinian expansion. Lastly, Cascio (2009) investigates expansions in the availability of public child care in the US in the period 1960--1980, finding no effect of increased enrollment on maternal labor supply. Among single mothers, the elasticity of enrollment is around 0.4.

3 Institutional setting and the child care reform

Although the roots of the Norwegian child care system date back to the early 19th century,⁴ 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). Increased 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 in 2000.

Figure 3 shows the trends in child care coverage rates for preschoolers and toddlers from 2001–2012. By 2001, 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 2001, less than 40% of 1–2 year olds in Norway were enrolled in child care, and there was substantial excess demand for child care. We discuss excess demand at some length in Section 5.

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 (?). The most important means for obtaining these goals were increased subsidies, lower parental fees and an investment subsidy for construction of new child care institutions. 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 4 presents some important changes in the child care sector following the reform.

⁴See Ministry of Education (2008-2009) for a thorough treatment.

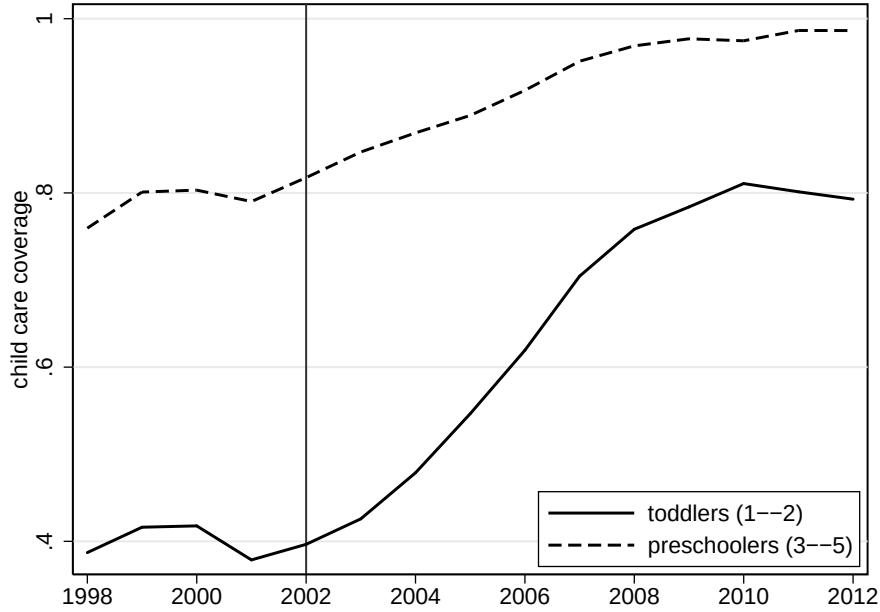


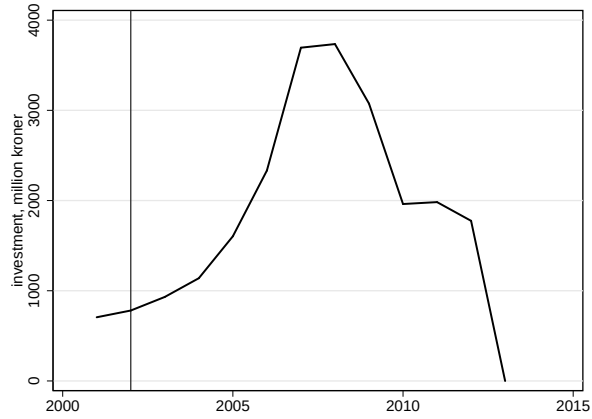
Figure 3: Child care coverage rates, 1998–2012

Note: Growth in child care coverage rates for toddlers and preschoolers. Source: Statistics Norway.

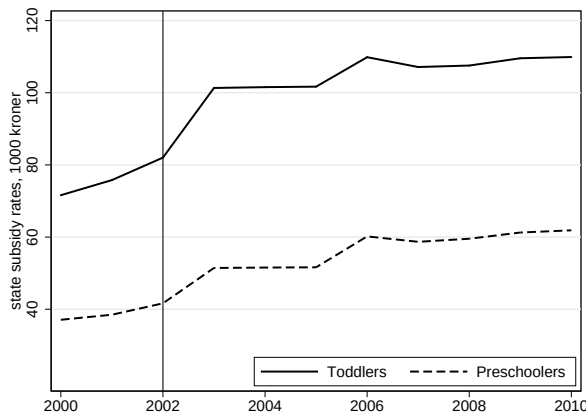
Panel 4a depicts the total investment in child care institutions over the period.⁵ Note that most investments appear with a lag of 1–2 years. The figure shows clearly how total investments increased rapidly following the reform. Panel 4b shows the increase in total subsidies per child per year, and also shows steady increases over the period. Panel 4c shows the changes in the composition of the costs that was covered by the municipality, the central government and the parental fees. It is clear that the share of the costs covered by the parents has declined significantly. This figure also shows that the 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 large subsidies offered following the reform, both private and public child care institutions have to adhere to strict quality criteria governing the quality of the services supplied. These criteria relate to for instance the teacher–child ratio, opening hours, and available playing space per child. Since 2004, the institutions also have to adhere to the max price reform, that put a cap of 2,750 NOK (around 350 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 the formal child care institutions are relatively homogenous in terms of observable attributes of quality and

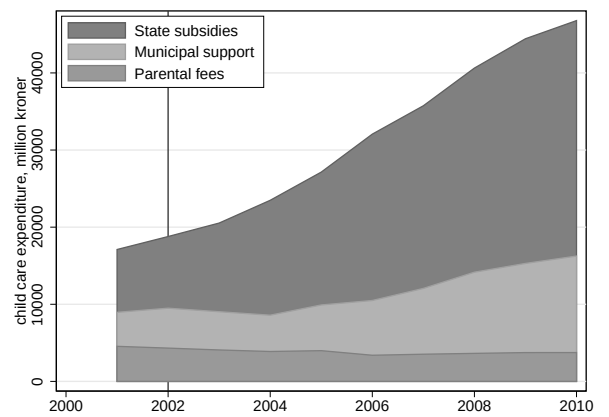
⁵All monetary values are indexed to 2014-kroners.



(a) Investment



(b) Yearly state subsidies per child



(c) The composition of financing in child care

Figure 4: Changes in the child care sector in the 2000's

Sources: Statistics Norway and regjeringen.no.

price.

As illustrated in Figure 3, 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.

4 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 discuss how we measure the key variables in our analysis, namely child care enrollment, child care coverage and labor supply. Next, we define our estimation sample, before we conclude by providing descriptive statistics for this sample.

To measure individual **child care enrollment**, we exploit the administrative registers of cash-for-care (CFC) recipients, available for two year olds since 2002. 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. As long as eligible parents take up the benefit, we can infer the child care enrollment of each child. This approach has the advantage of giving a comprehensive measure of child care enrollment 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 enrollment 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, 2011. An immediate potential issue with these measures is that in clearing markets the equilibrium level of the child care coverage rate will be determined jointly by demand and supply. Exploiting variation based on demand shocks raises the potential for reverse causality, which may bias the estimates. If the demand for child care exceeds supply, however, then the market is rationed, and variation in coverage should be determined uniquely by variation in supply. In order to verify anecdotal evidence

⁶Over the period the CFC benefit varied from 2,200 to 3,600 NOK per month per child, equivalent to approximately 350 - 583 USD per month.

⁷We 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.

that this is the case here, we got access to survey data on the number of children 0–3 years old on waiting lists for child care in each municipality over 2004–2009.⁹ By adding children enrolled and children on waiting lists, we can construct a measure of the child care application rate. Figure 5 draws the trends in the child care coverage rate and the child care application rate for 0–3 year olds over our estimation period, as measures of, respectively, the supply and demand for child care in this age group. Throughout the period, demand is substantially higher than supply, by between 10 and 15 percentage points. Additionally, we suspect that the strong growth in demand over time may be due to applications from parents who are discouraged when coverage is particularly low. If so, then rationing in the early period may 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 separately). However, this is likely 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 pay close attention to the potential for reverse causality, e.g. 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 participation based on the basic amounts in the Norwegian Social Insurance Scheme (used to define labor market status, determine eligibility for unemployment benefits as well as disability and old age pension). Specifically, we follow Havnes and Mogstad (2011) and construct dummy variables for employment and full-time equivalent status that equal one if earnings is above 2 and 4 basic amounts, respectively, and zero otherwise. In 2014, one basic amount equaled 88,370 NOK, or approximately 12,000 USD.

Second, we use data from the matched employer–employee register, available for 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

⁹The data were collected by Asplan Viak on behalf of the Norwegian Ministry of Education, and should give the number of children on waiting lists by September 20th each year from 2004 to 2009. The data were collected in order for the Ministry to monitor the progress towards full child care coverage.

¹⁰In 2003, Norwegian parents were entitled to 43 (53) weeks of parental leave with 100 % (80 %) wage compensation, expanded to 44 (54) weeks in 2006. Parents are further entitled to one year each of unpaid leave in immediate continuation of regular parental leave.

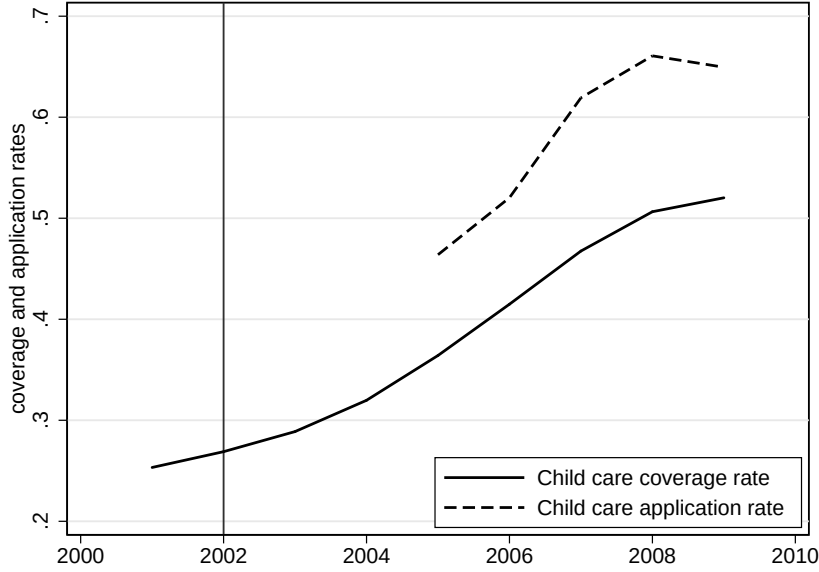


Figure 5: Child care application- and enrollment rates for 0–3 year olds, 2000–2010.

Note: Coverage rate and application rates for child care for 0–3 year olds. Application rates constructed by adding children in care to children on waiting lists for care. Sources: Reports from Asplan Viak (2004–2010) and Statistics Norway.

missing observation is changing over time and these changes are correlated with changes in child care coverage rates.

To define the **estimation sample**, 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.¹¹ We also exclude children with unknown mothers and a handful of children who reside in households with another child also in the sample. This leaves us with a sample of 323,168 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 way, we identify the likely caregivers relevant for the vast majority of the children in the sample. From population registers, we can also identify mothers and fathers that do not reside with the children, as well as grandmothers and grandfathers. Our main samples of interest will be cohabiting mothers and fathers, single mothers and non-residing fathers.

¹¹Fertility could be considered endogenous to the child care expansion. In section 7, we include also children with younger siblings to verify that this sample restriction is not driving our results.

Descriptive statistics

Summary statistics for the children and the four main samples of caregivers are given in table 1. 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 enrollment is 0.46, corresponding to 5.3 months of full time child care enrollment. 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 the least attached to the labor market, with only 30 % employed and 15 % in full time equivalent employment. In comparison, cohabiting fathers are strongly attached to the labor market, with 92 % employed and 84 % full time. Also cohabiting mothers are much more attached to the labor market than single mothers, with about two thirds employed, and one third in full time. Non-residing fathers stand between cohabiting mothers and fathers in terms of labor market attachment. These differences are mirrored in their earnings, where cohabiting fathers on average make almost twice that of cohabiting mothers and non-residing fathers, and almost five 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 about 61 % of cohabiting mothers and fathers are married, and that they have about 15 years of completed education. We also note that earnings the year before birth are substantially higher for mothers, likely reflecting reduced labor supply following child birth, but higher for fathers, likely reflecting wage growth. The table also shows that parents differ significantly with respect to age and education. 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.

Next, Figure 6 investigates the trends in labor force participation over the period we study for the four groups of caregivers. For comparison, we also include the trend for working age women with no school aged children. We note that these women have a quite high employment rate hovering just over 60 %, of which about two thirds is full time. For both measures, however, the overall trend is essentially flat over the period. This flat time pattern is similar to the one we observe for cohabiting fathers in our sample, though the level is higher with employment above 90 %. In contrast, both cohabiting and single mothers experienced substantial increases in the labor force participation over these years. In particular, their employment rates 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

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

Variable	Children			
	Mean	SD	Min	Max
Male	0.49	0.50	0	1
Child care enrollment	0.46	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	0.41	0.17	0	1
Two year olds	0.67	0.18	0	1
Observations	323,168			

Note: The table gives descriptive statistics for children in the estimation sample. Variables are defined in Section 4.

Table 2: Descriptive statistics for caregivers at child aged 2, 2002–2008.

		Cohabiting mothers	Cohabiting fathers	Single mothers	Non-residing fathers
A. Outcome variables					
Earnings (NOK)		293,353	585,771	134,986	364,369
Employed		0.675	0.914	0.307	0.698
Full-time eq.		0.389	0.837	0.146	0.534
Weeks of	4 hours	37.20	43.60	21.37	32.18
employment above	30 hours	26.48	41.72	13.17	29.03
B. Control variables					
Age		32.65	35.55	29.52	32.47
Married		0.606	0.606	0.0482	0.0487
Immigrant		0.097	0.074	0.088	0.080
Years of education		15.10	14.90	12.92	12.77
Number of children		2.03	2.06	1.72	1.82
below 6 years old		1.52	1.52	1.28	1.33
below 13 years old		1.89	1.88	1.55	1.60
below 18 years old		1.994	1.986	1.667	1.72
Local labor market control		0.861	0.86	0.86	0.86
Earnings year before birth (NOK)		318,703	531,255	189,369	332,940
Observations		283,706	283,494	33,262	29,202

Note: The table gives means in the estimation sample for outcome and control variables for different caregivers. Variables are defined in Section 4.

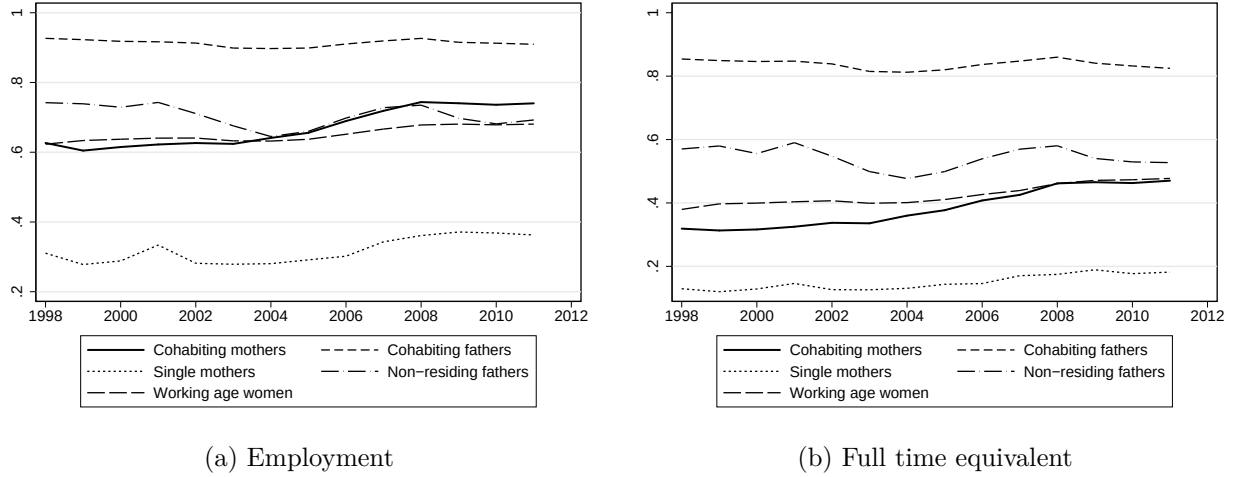


Figure 6: Trends in labor force participation for caregivers of children age 2 and working age women overall.

Note: Trends in labor force participation for the main samples of caregivers and all working age women: Share of the sample earning more than 1 basic amount.

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 impacts of the child care expansion. We discuss how to 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 fixed over time. The exceptions are a fall in the fraction of married from about 63 % to 58 % and the growth in immigrant background from about 8 % to 11 %¹². Tables for the other groups of caregivers are presented in the appendix, tables 10, 11 and 12 for brevity.

5 Empirical strategy

The most straightforward way to evaluate the effect of child care is to regress a measure of labor supply on enrollment. This ignores, however, that enrollment in child care 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

¹²We have estimated our model both with and without control variables. The model without controls shows similar, but somewhat larger effects, and are available upon request.

Table 3: Descriptive statistics for cohabiting mothers at child aged 2, 2002–2008.

	2002	2004	2006	2008
A. Outcome variables				
Earnings (NOK)	269,532 (221,727)	278,064 (265,940)	304302 (236,768)	329,899 (230,504)
Employed	0.630 (0.48)	0.644 (0.48)	0.693 (0.46)	0.746 (0.44)
Full-time eq. employment	0.340 (0.47)	0.362 (0.48)	0.410 (0.49)	0.463 (0.50)
Weeks of 4 hours empl. above	35.00 (22.4)	36.29 (22.2)	37.80 (21.3)	40.14 (19.9)
30 hours	23.57 (24.6)	25.45 (24.8)	27.37 (24.7)	29.97 (24.3)
B. Control variables				
Age	32.28 (4.81)	32.56 (4.83)	32.84 (4.83)	32.88 (4.92)
Married	0.628 (0.48)	0.612 (0.49)	0.597 (0.49)	0.582 (0.49)
Immigrant	0.0838 (0.28)	0.0902 (0.29)	0.103 (0.30)	0.114 (0.32)
Years of education	14.81 (3.35)	15.01 (3.45)	15.23 (3.50)	15.34 (3.60)
Number of children	2.048 (0.99)	2.033 (1.00)	2.026 (0.98)	2.003 (0.98)
below 6 years old	1.513 (0.58)	1.515 (0.58)	1.518 (0.58)	1.523 (0.58)
below 13 years old	1.912 (0.84)	1.893 (0.83)	1.892 (0.83)	1.874 (0.82)
below 18 years old	2.013 (0.94)	1.999 (0.94)	1.994 (0.93)	1.967 (0.92)
Local labor market control	0.837 (0.033)	0.830 (0.038)	0.876 (0.033)	0.915 (0.032)
Earnings year before birth (NOK)	310,347 (213,279)	323,853 (219,946)	322262 (220,798)	317254 (225,874)
Observations	42,186	39,300	40,209	41,613

Note: The table gives means in the estimation sample for control variables among cohabiting mothers. Variables are defined in Section 4.

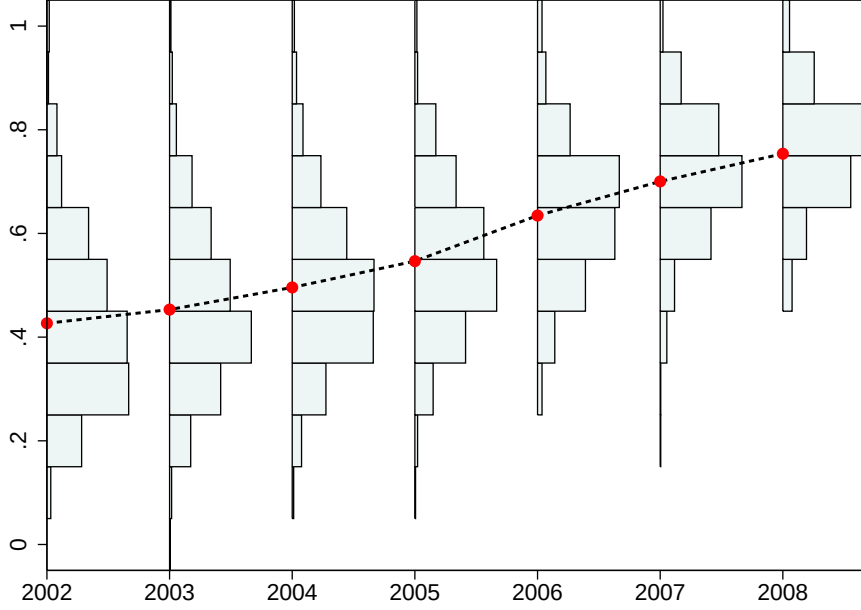


Figure 7: Mean and distribution of child care coverage rates for 2-year olds across municipalities, 2002–2008.

Note: Data, estimation sample and variable definitions are discussed in Section 4.

therefore likely to yield estimates of how child care enrollment affects labor supply that are biased upwards.

Imagine instead a social experiment that randomized child care access at the municipal level. The 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 enrollment in municipalities with and without child care access would give a first stage estimate of the effect of child care access on child care enrollment. If we take the ratio between the two we would get an IV-estimate of the effect of child care enrollment on parental labor supply, using random variation in child care access as an instrumental variable for child care enrollment.

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, generating 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 7. 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 enrollment, m_{ikt} , with the child care coverage rates of 1

year olds in $t - 1$, $CC_{k,t-1}^1$, and of 2 year olds in t , CC_{kt}^2 .¹³ Specifically, we estimate the following 2SLS-model in our sample of caregivers of children aged two years old in t .

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

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

where α_k^r and τ_t^r are municipality and time fixed effects. $\mathbf{X}_{ikt}$ is a vector of child, caregiver and municipality characteristics: Child characteristics include a dummy for gender and month of birth, the number of older paternal and maternal siblings, and 7 dummies for parents' years of completed education.¹⁴ Caregiver characteristics include age, age squared, a dummy for immigrant background, yearly earnings in $t-3$,¹⁵ number of children below ages 6, 11, and 18 and 7 dummies for years of completed education. This controls 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 left panels of Figure 8 draw scatter plots of labor supply against predicted child care enrollment, purged of time and municipality fixed effects.¹⁶ The figure suggests that there is a strong effect of predicted child care enrollment 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 9. To this end, we first purge all variables of municipality means using fixed effects. We then regress the residual enrollment on the residual coverage rates and predict a scalar instrument that represents the variation we exploit. Then recenter the data so that time zero is the year with the highest increase in predicted child care enrollment. The top panels of figure 9 graphs the trend in labor participation alongside the trend in child care enrollment as predicted

¹³In the first stage of our IV model below, we regress enrollment in child care on municipal child care coverage rates. Some readers may find that this resembles a poorly designed peer effects analysis, regressing 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 enrollment 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 enrollment as an instrument, the F-statistic is 4,493 with a coefficient close to 1, as expected. In contrast, our first stage yields an F-statistic of 44, with coefficients of .3 and .05. Note also that this discussion has no bearing on the validity of our instruments or the reduced form.

¹⁴Rather than dropping a handful of observations with missing education data, we set the education to the lowest level and include a separate dummy for caregivers with missing education.

¹⁵For approximately 2 % of the sample, earnings in $t - 3$ is missing. Rather than exclude these observations, we set earnings to 0 and include a dummy equal to 1 for these individuals.

¹⁶Specifically, we estimated our first stage equation (2) without covariates, and predicted the individual child care enrollment from the instruments. We then regressed both the predicted child care enrollment and the outcomes individually on calendar time and municipality fixed effects, and predicted the residuals.

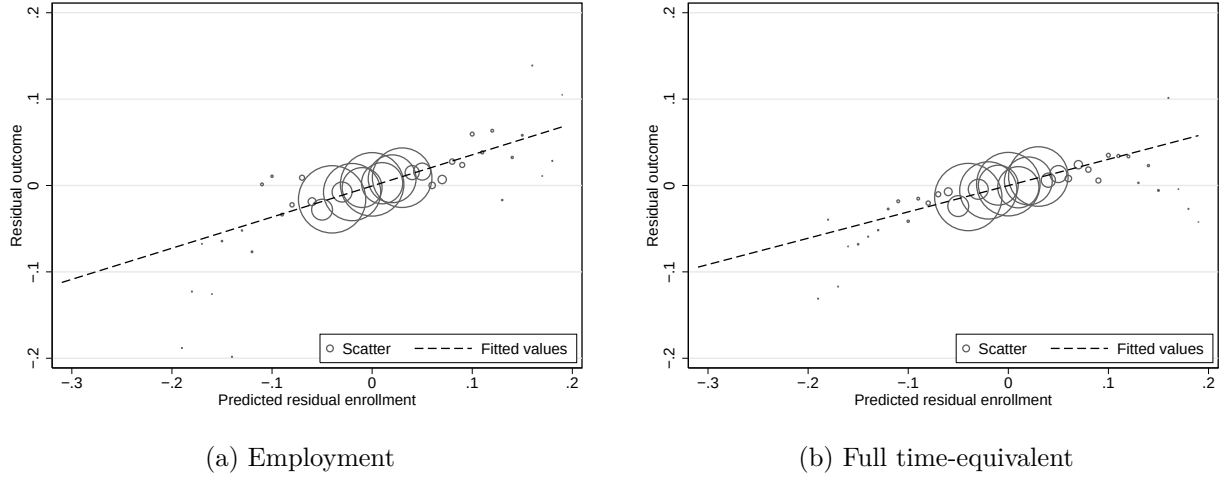


Figure 8: Illustration of the IV approach: Scatter plot of predicted residual employment versus predicted residual enrollment.

Note: Data, estimation sample and variable definitions are discussed in Section 4.

from our instruments. At time zero, we see a substantial increase in predicted child care enrollment of around 17 percentage points, 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 enrollment jumps up, labor participation also increases, by around 3.3 and 2.9 percentage points for employment and full time-equivalent, respectively. Taking the fraction of these estimates suggests an IV-estimate of around 19.4 and 17 percentage points increase in labor participation and full time employment from child care enrollment. Note also that the growth in employment continues from year 0 to year 1, indicating that the full impact of the expansion may not be seen the first year. Below, we elaborate on this formally in our estimation framework.

To evaluate our IV-strategy, note that the identifying assumption roughly relies on common time shocks across municipalities that expand and do not expand child care coverage. If this assumption is violated, for instance by labor supply leading child care expansions, then this should show up in figure 9 as trends or jumps in the outcome prior to the expansion that is unrelated to increases in predicted enrollment. 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 enrollment. 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).

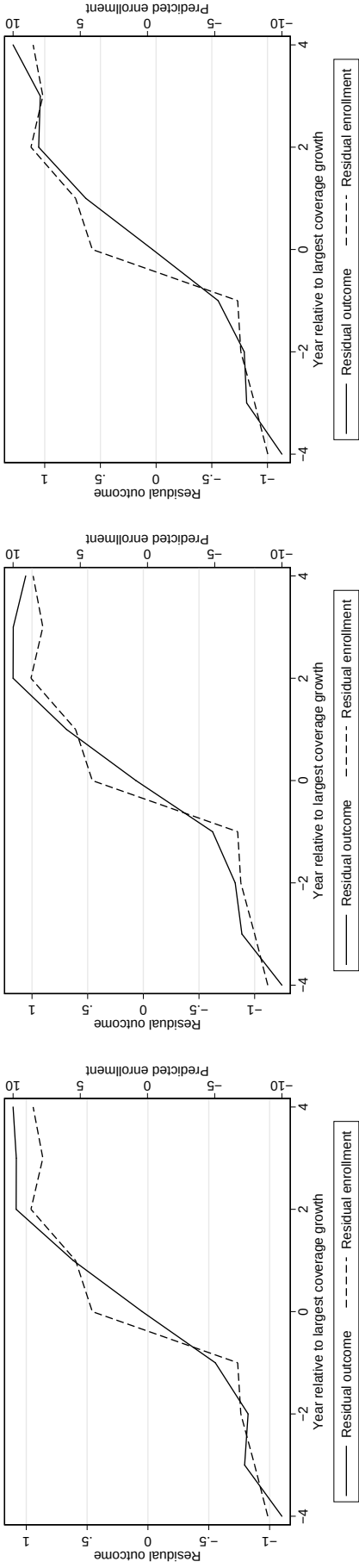
Second, we could worry about selection on unobservables: If expansions in child care coverage are, for instance, positively correlated with other determinants of labor supply, then our estimates will be biased upwards. Note first that the fixed effects are controlling for all time invariant differences 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, such that our estimates are biased upwards. In Section 4 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.

Furthermore, we investigate 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 observable composition might raise the concern about changes in the composition also in terms of unobservables. In Figure 9, 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 enrollment, and we have purged the variables of municipality fixed effects. It is reassuring to see that, across the bottom panels of Figure 9, the jump in predicted child care enrollment 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.

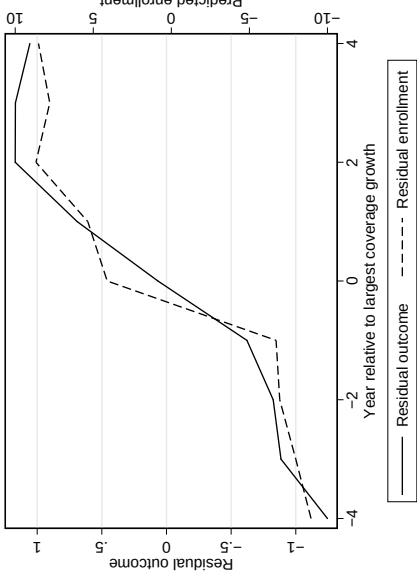
Finally, even with the large set of control variables that we include in our estimations, we may still worry about changes in unobservable determinants of labor supply. After our main results, we will further challenge the validity of our empirical strategy by probing the stability of the IV estimates to alternative specifications, and by performing placebo tests.

6 Results

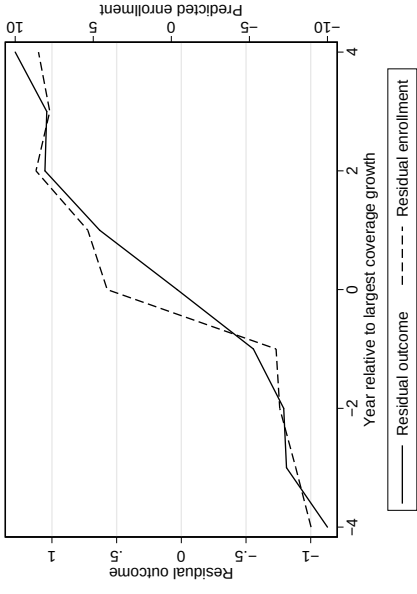
Baseline results for the four main samples of interest are given in Table 4. Start by considering the first stage estimates reported in column 1. The instruments are clearly relevant, with F -statistics for the excluded instruments above 25 for all samples. Estimates in column 1 indicate that an additional child care slot for one year olds causes increases of about 30 percentage points in the child care enrollment of two year olds in the following year. Estimates in column 2 indicate that an additional child care slot for two year olds increases by about five percentage points the child care enrollment 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 in different caregiver groups.



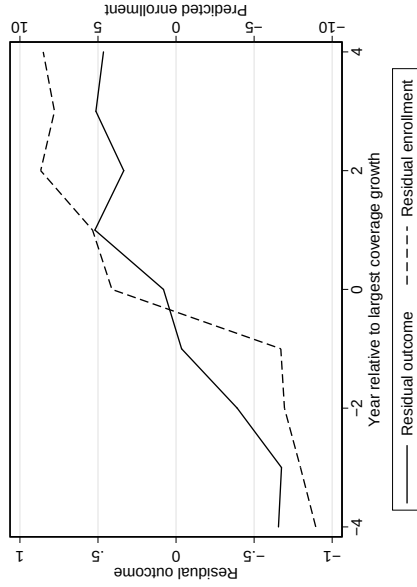
(a) Income



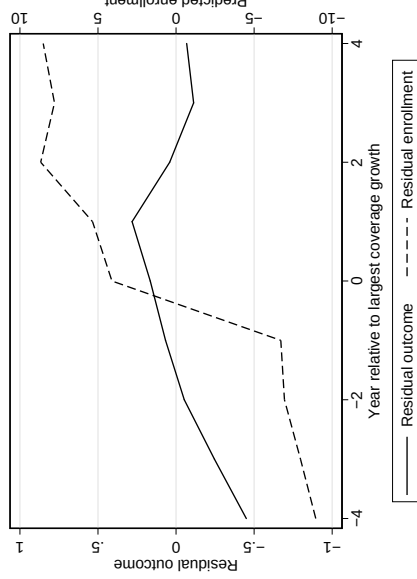
(b) Employment



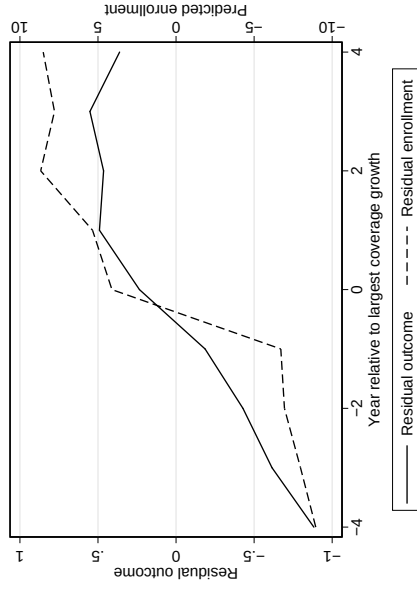
(c) Full time



(d) Age



(e) Earnings year before birth



(f) Education

Figure 9: Predicted child care enrollment, labor supply (top pane) and key controls (bottom pane) around the largest increase in predicted child care enrollment.

Note: Illustration of our IV strategy. We first purge all variables of municipality fixed effects, then regress residual enrollment on the two residual coverage rates. We then recenter the data so that year 0 is the year with the largest growth in predicted enrollment for each municipality, and plot the predicted enrollment and the residual outcomes against the years relative to this base year. The scale of the outcome variables (left axis) is $1/10$ th of a standard deviation of the variable in the sample of cohabiting mothers, while the scale of predicted enrollment is in percentage points. Sample is balanced to the 191 municipalities where we have data for at least two years around the base year. Data, estimation sample and variable definitions are discussed in Section 4.

Second stage estimates are reported in column 2 of Table 4. The estimates suggest substantial labor supply responses among mothers, but little if anything among fathers. In particular, employment among cohabiting mothers is estimated to increase by 26 percentage points when a child is enrolled in full time child care, of which almost two thirds are to full time equivalent employment. Single mothers also respond strongly to employment, with an increase of about 20 percentage points, but with no impact on full time equivalent employment. Overall, the estimates indicate an increase in annual earnings of around 15–20 % among mothers, though the estimate is statistically significant only among cohabiting mothers. In contrast, we find no evidence of any impact on fathers, though it should be noted that the estimates on non-residing fathers are too noisy to draw firm conclusions. Finally, the estimates on weeks of employment confirm the earnings based estimates, by and large: Cohabiting mothers increase their labor supply by about eight weeks, almost uniquely on the full time margin, while single mothers respond by increasing labor supply, but mostly to part time employment. Again, there is no response among fathers.

Lastly, we have also used total taxes paid as an outcome measure to investigate the argument that the cost of the reform is mitigated by increases in the tax base. We find no increases in taxes paid for either single or cohabiting mothers, despite the increases in labor supply. The effective marginal tax rates on the extra income of 60,000 NOK for cohabiting mothers is a nonsignificant 5.3%. We expect that this is driven by the nonlinearities in the tax schedule, particularly the low taxes paid on low incomes. Likewise, there is no significant impact on taxes from fathers. Hopes of increases in tax income from expansions of child care boosting labor supply, seem not to be fulfilled.

We have also investigated the 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 response among grandparents, despite a strong first stage and relatively high labor participation rates among grandparents. This may suggest that grandparents are not important caregivers for toddlers in our sample, at least not to the extent that it affects their labor supply. Results are reported in Table 15 in the appendix.¹⁷

Heterogeneity over the earnings distribution

To understand more about the labor supply response, 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). Specifically, we construct seven

¹⁷We also looked at grandparents residing in the same municipality 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.

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

First stage			Second stage			Mean	N
CC_{t-1}^1	CC_t^2	F	Outcome			2002	
Cohabiting mothers							
0.304***	0.046**	45.6	Earnings	59,709***	(13,333)	269,532	283,715
(0.032)	(0.021)		Tax	3,184	(7,518)	70,366	
			Employed	0.261***	(0.0293)	0.630	
			Full-time eq.	0.166***	(0.0268)	0.340	
			Wks > 4h	8.157***	(1.586)	35.00	
			Wks > 30h	8.960***	(1.647)	23.57	
Cohabiting fathers							
0.307***	0.0472**	45.8	Earnings	-24,798	(35,303)	580,956	283,516
(0.0325)	(0.0213)		Tax	-24,330	(16,768)	170,326	
			Employed	-0.0199	(0.0237)	0.918	
			Full-time eq.	0.00535	(0.0310)	0.843	
			Wks > 4h	-0.308	(1.421)	43.38	
			Wks > 30h	-0.759	(1.585)	41.65	
Single mothers							
0.293***	0.0259	29.45	Earnings	30,596	(34,400)	124,459	33,259
(0.0394)	(0.0352)		Tax	-15,461	(14,383)	34,195	
			Employed	0.204**	(0.0988)	0.284	
			Full-time eq.	-0.00757	(0.0664)	0.128	
			Wks > 4h	10.32*	(5.505)	19.49	
			Wks > 30h	1.352	(4.132)	11.49	
Non-residing fathers							
0.401***	0.0542*	149.3	Earnings	13,817	(35,954)	371,117	29,244
(0.0324)	(0.0303)		Tax	-10,044	(13,723)	101,019	
			Employed	0.00126	(0.0497)	0.715	
			Full-time eq.	0.0491	(0.0596)	0.551	
			Wks > 4h	-0.229	(2.669)	31.67	
			Wks > 30h	0.139	(2.567)	28.85	

Note: First- and second stage estimates from equation (2) and (1) of the impact of child care enrollment on parental labor supply. Outcome and control variables are defined in Section 4. Standard errors in parentheses are clustered at the municipality level and robust to heteroskedasticity.

* (p<0.10), ** (p<0.05), *** (p<0.01)

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 having earnings above six basic amounts (i.e. 528,000 NOK). We then estimate the same IV-model using these dummy variables successively as the dependent variable. In this way, we can study binned parts of the marginal distribution of labor supply, which is helpful in order to get an impression of what parts of the distribution are being affected by the child care expansion.

Figure 10 shows the mean of these dummy variables in 2002 as bullets, and the estimated second stage coefficients in bars with associated confidence interval, for each of the four groups of caregivers. In the top left panel, we see that prior to the child care expansion, almost 30 % of cohabiting mothers had earnings below one basic amount, and were essentially out of the labor force. For the remaining mothers in this group, the distribution of earnings is relatively flat, with 10–15 % in each bracket. In contrast, the distribution for cohabiting fathers is skewed, with much higher probability of being in the top brackets, while the reverse is true for single mothers, who are much more likely to be in the lowest bracket.

The bars in Figure 10 give the estimated impact on each margin, and document some important heterogeneity. In particular, cohabiting mothers seem to be shifting away from 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 middle bracket, pointing towards an increased incidence of part time (or low wage) employment, though estimates are more noisy in this much smaller sample. Again, there is no evidence for impacts on fathers.

Persistence

Next, we 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, two or three 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.¹⁸ Because expansion of child care for older children is likely to be correlated with earlier expansion for younger children, we also control for the coverage rate for children aged $2 + s$ in the base year, again hoping to account for issues with bad controls. For brevity, and because estimates are strongest for this group, we will focus on cohabiting mothers.

Table 5 presents estimates from the IV model for cohabiting mothers, while persistency estimates for the other three main samples are found in table 16 in the appendix. In row 1, we repeat the baseline estimates for ease of comparison. In row 2, we use as the

¹⁸Some 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).

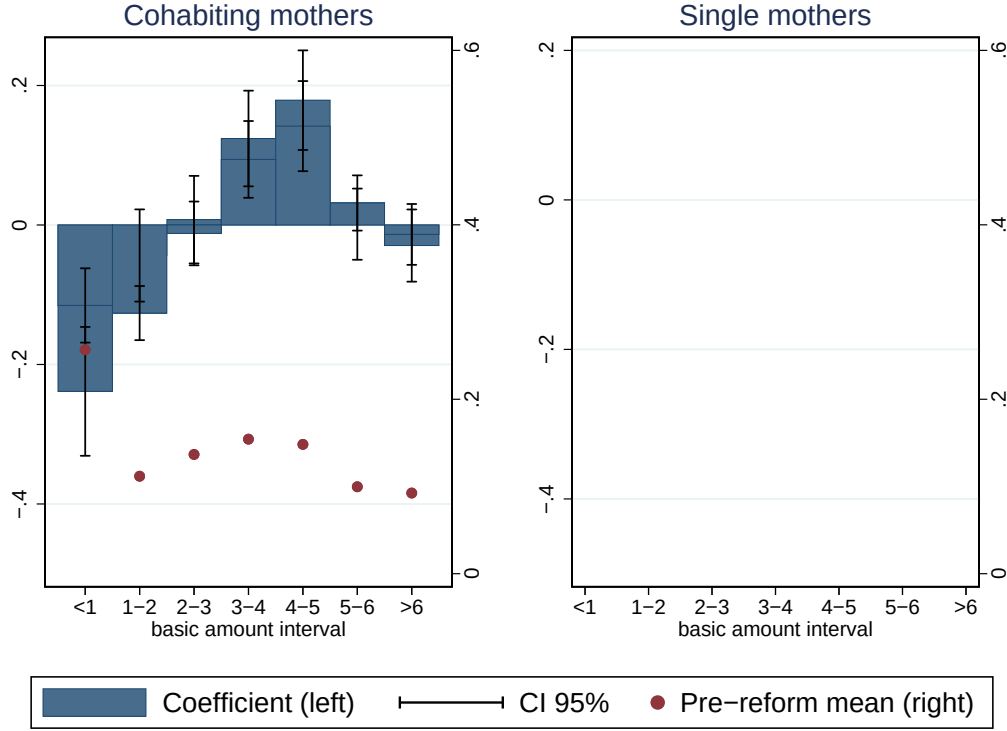


Figure 10: Impact of child care enrollment on labor supply over the earnings distribution
Note: IV estimates of enrollment on the probability of earnings in income intervals (1BA $\approx$ 12,000 USD)

outcome the labor supply one year later. As expected, impacts are lower both because mothers increase their labor participation as their children age, and because mothers who failed to enroll children in child care in t may succeed at this in $t + 1$. The effects remain substantial, however: Enrolling a child in child care at age two is estimated to cause an increase in employment of about 10 percentage points, while full time-equivalent employment increases by 15 percentage points, when the child is three. Note, however, that the estimates include partly the direct effect of child care enrollment, since the child care year straddles the years t and $t + 1$. In row 3, we look one period further on, to estimate the impact on labor supply in $t + 2$. Again the estimates drop, and now suggest quite modest increases of about 5 and 7 percentage points for employment and full time-equivalent, respectively. Finally, row 3 presents estimates for labor supply in $t + 3$. The estimated impact remains modest, and is roughly stable compared to the previous year, but is no longer statistically significant.

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

	<u>Earnings</u>	<u>Labor participation</u>		<u>Weeks of employment</u>		N
		<i>empl.</i>	<i>full time</i>	<i>>4h</i>	<i>>30h</i>	
t (baseline)	59,709*** (13,333) <u>269,532</u>	0.261*** (0.029) <u>0.630</u>	0.166*** (0.027) <u>0.340</u>	8.157*** (1.59) <u>35.00</u>	8.960*** (1.65) <u>23.57</u>	283,715
$t + 1$	17,801* (10,501) <u>296,235</u>	0.0403* (0.023) <u>0.691</u>	0.153*** (0.038) <u>0.394</u>	-1.033 (0.687) <u>47.64</u>	5.764*** (1.689) <u>32.57</u>	217,618
$t + 2$	5,634 (10,295) <u>304621.4</u>	0.0084 (0.023) <u>0.710</u>	0.055 (0.039) <u>0.405</u>	-0.751 (0.727) <u>47.31</u>	5.185*** (1.589) <u>33.13</u>	212,016
$t + 3$	553.3 (14,781) <u>314345.3</u>	0.0172 (0.024) <u>0.717</u>	0.0187 (0.047) <u>0.423</u>	0.743 (0.784) <u>48.07</u>	5.560*** (1.855) <u>34.40</u>	177,598

Note: Second stage estimates from equation (1) of the impact of child care enrollment on cohabiting mothers' labor supply, with the outcome lagged 1–3 years. In addition to the control variables defined in Section 4, we control for child care coverage rates for older children in the base year. Standard errors in parentheses are clustered at the municipality level and robust to heteroskedasticity. Mean of dependent variable underlined. * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$)

7 Robustness analysis

Despite our estimation strategy and the undersupply of formal care in the period, the expansion in child care that we exploit is not strictly exogenous. Therefore, we might worry that the results are driven by endogeneity problems rather than reflect causal effects. In this section, we test for alternative explanations for our findings, and perform two overall placebo tests. Robustness checks for cohabiting mothers are reported in the text, while results for the other three samples are found in the appendix. The results from these support our estimation strategy, by and large.

Reverse causality

Our main tool for guarding against reverse causality is instrumenting for the individual child care enrollment using municipal coverage rates. This relies on rationed markets for formal care as discussed in section 5.

To investigate whether our results could be driven by reverse causality, we employ more conservative measures of rationing, excluding municipalities with less excess demand from the sample. We start by including only the years 2004-2008, for which rationing data is available, and then rerun the baseline specification, excluding municipalities that do not have excess demand or have less than 10% excess demand. If our results were indeed reflecting reverse causality, we should expect our estimates to move towards zero as we exclude municipalities where this is more likely to be a problem.

Results from this exercise are provided in table 6. It is reassuring to see that the results are very similar to the baseline results, and that the pattern of coefficients over the income distribution is similar.

Unobservable trends and shocks

Next, we might worry that the estimates are driven by time-varying unobservable drivers of labor supply that correlate with the child care expansion. One such spurious correlation could be heterogeneity in the counterfactual trends of labor supply. If some municipalities have a positive counterfactual trend in labor supply, and these are also the municipalities that expand child care, this would lead to upward bias in our estimates. To test for this, we control for municipality specific linear time trends, taking out linear trends in labor supply in each municipality. If this was driving our results, we would expect estimates to drop towards zero. As can be seen from table 6, the estimates are stable to this inclusion.

Of course, we cannot know that the unobserved trends are linear. Therefore, we perform another test where we allow the time shocks to labor supply to differ between municipalities depending on their pre-reform characteristics. We cannot include year times municipality fixed effects, as this is the variation we exploit in our identification

Table 6: Robustness checks for cohabiting mothers

Specification	Earnings	Employment		Weeks of employment		<i>N</i>
		<i>empl.</i>	<i>full time</i>	<i>>4h</i>	<i>>30h</i>	<i>G</i>
Baseline results	59,709*** (13,333)	0.261*** (0.0293)	0.166*** (0.027)	8.157*** (1.586)	8.960*** (1.647)	283,715 428
Selective migration? Residence as in $t - 3$	37,955** (18,053)	0.224*** (0.040)	0.122*** (0.039)	5.979*** (1.906)	7.688*** (2.432)	235,978 425
Endogenous fertility? Include young siblings	52,995*** (11,855)	0.224*** (0.026)	0.143*** (0.025)	7.414*** (1.465)	7.167*** (1.416)	343,818 428
Population growth? ins: log number of slots	55,924*** (11,353)	0.256*** (0.026)	0.131*** (0.023)	8.873*** (1.418)	9.538*** (1.341)	283,613 428
Municipality-specific linear time trends	69,942*** (16,543)	0.277*** (0.046)	0.229*** (0.047)	9.945*** (2.249)	10.47*** (2.609)	283,775 428
Characteristic specific time shocks (eq. 3& 4)	68,599*** (14,870)	0.255*** (0.0395)	0.175*** (0.0374)	9.507*** (1.751)	9.520*** (2.076)	283,470 427
Reverse causality? Excess demand	51,598*** (14,777)	0.189*** (0.040)	0.173*** (0.041)	4.920*** (1.681)	7.012*** (2.124)	184,339 360
Reverse causality? >10% excess demand	77,764*** (28572)	0.188*** (0.072)	0.149** (0.074)	4.317 (2.827)	10.71*** (3.084)	74,889 138
Baseline, sample with more than 2 children	89,308*** (18,122)	0.246*** (0.046)	0.191*** (0.051)	8.056*** (2.171)	7.189*** (2.671)	70,989 424
Individual fixed effects sample with more than 2 children	62,183*** (20,371)	0.175*** (0.060)	0.150** (0.061)	4.164* (2.374)	2.640 (3.081)	70,989 424

Note: Various robustness checks as described in the text. Coefficients reported are the second stage IV estimates. Standard errors in parentheses are clustered at the municipality level and robust to heteroskedasticity. * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$)

strategy. Instead, we interact the yearly shocks with pre-reform characteristics, estimating the following specification.

$$m_{ikt} = \alpha_k^1 + \tau_t^1 + \pi_1 CC_{k,t-1}^1 + \pi_2 CC_{kt}^2 + \varphi_t^1 \mathbf{V}_{k,2001} + \mathbf{X}_{ikt} \boldsymbol{\theta}_1 + \epsilon_{ikt}^1 \quad (3)$$

$$y_{ikt} = \alpha_k^2 + \tau_t^2 + \beta m_{ikt} + \varphi_t^2 \mathbf{V}_{k,2001} + \mathbf{X}_{ikt} \boldsymbol{\theta}_2 + \epsilon_{ikt}^2 \quad (4)$$

Where φ_t is an m -vector of shocks in year t , each component related to a particular pre-reform characteristic of the municipality. The yearly shocks to labor supply thus consist of two parts: The common shock τ_t and a set of characteristic specific shocks φ_t that are propagated through the pre-reform characteristics of the municipality, $\mathbf{V}_{k,2001}$. In this set we include a range of political, economical and geographical characteristics measured in 2001, before the sample period.¹⁹

This approach allows for flexible time shocks to labor supply that affect the municipalities differently depending on their characteristics. Again, if shocks of these types are driving our results, we would expect estimates to drop when including them in the regressions, but that is not what we find in table 6 - estimates are very stable.

Selective migration

If caregivers with high labor supply move to municipalities that will expand child care, we could fear that our estimates could be driven by selective migration. This would show up as significant estimates despite no true relationship, simply due to sorting of work prone caregivers into expanding municipalities.

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. Again, our estimates are stable, indicating that selective migration is not driving the results.

Endogenous fertility

When constructing the sample, we follow the literature and consider only children with no younger siblings. If child care availability affects childbearing decisions, we are then conditioning on an endogenous variable. This could bias our results. We therefore re-run the baseline specification, including also mothers that have younger children. It is reassuring to see in table 6 that the estimates are almost unchanged.

¹⁹Specifically, 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 rates.

Furthermore, our instruments are constructed as the ratio of children in care to the population of the same age. Changes in these rates could be driven by changes in population growth as well as increasing supply of care. This type of endogenous population growth could threaten our empirical strategy if it is correlated with changes in labor supply.

To investigate this, we need instruments that are not affected by population growth. To this end we instead

use the log number of children in care as instruments rather than the shares.²⁰ If population growth was driving our results, we should expect a remarkably different set of estimates from this specification, where population growth can not affect the estimates. Reassuringly, that is not what we find in table 6.

Changes in distribution of individual characteristics

In the baseline specification, we use fixed effect at the municipal level. This will take account of all unobservable, time invariant determinants of labor supply at the municipality level, and also for time invariant unobservable characteristics at the individual level, as long as the distribution of these do not change. However, if the distribution of these unobservables at the individual level changes over time, our results might be biased.

To test for this, we exploit the fact that some parents have more than one child in the sample period. For these parents, it is possible to use fixed effects at the individual level. There are reasons to believe that these parents are different than the rest of the sample, so we compare this estimate to the baseline specification on the same sample of mothers giving birth at least twice in the period. To maintain clustering at the municipality level, we include only mothers who do not change municipality between births.

The results from this exercise are provided in table 6. It is reassuring to see that the results in the smaller sample is relatively similar to the full sample. When including individual fixed effects, we obviously lose precision. Even so, the estimates when including these effects are similar to the municipality fixed effects specification, and results are still significant for labor force participation and earnings.

Placebo tests

Lastly, we perform two placebo tests to investigate the total robustness of our estimates. As placebo outcomes we use the labor supply of mothers of four year olds and the baseline mothers in year $t - 3$, one year before giving birth. These outcomes should not be affected by the child care coverage rate for toddlers. However, since the individual enrollment is

²⁰We could also have used the raw enrollment numbers. Because of the large difference in population size between the municipalities, however, this would essentially leave only the variation within Oslo and a few other large urban municipalities, and after controlling for municipality and year fixed effects, there would be little variation left to exploit.

Table 7: Placebo tests, reduced form

	Earnings	Labor participation		Weeks of employment		N
		Empl.	Full time	>4h	>30h	
Child care for 1-year olds in $t - 1$						
Baseline,	14,251***	0.0715***	0.0576***	2.485***	2.888***	283,706
reduced form	(5,041)	(0.015)	(0.016)	(0.565)	(0.677)	
Placebo: Mothers	-212	0.012	0.015	-0.302	0.692	196,867
of 4-year olds	(6,323)	(0.015)	(0.015)	(0.642)	(0.964)	
Placebo: Baseline	-982	0.017	0.021	-0.348	-1.186	159,390
mothers in $t - 3$	(8,229)	(0.022)	(0.024)	(1.102)	(1.287)	
Child care for 2-year olds in t						
Baseline,	7,892*	0.0222*	-0.00019	0.395	0.223	283,706
reduced form	(4,456)	(0.012)	(0.012)	(0.521)	(0.543)	
Placebo: Mothers	10,125*	0.0239*	0.0161	0.805	0.606	196,867
of 4-year olds	(5,404)	(0.013)	(0.0134)	(0.602)	(0.779)	
Placebo: Baseline	4,562	0.0074	-0.0061	0.952	1.809*	159,390
mothers in $t - 3$	(6,700)	(0.015)	(0.017)	(0.797)	(0.971)	

Note: Reduced form estimates of the impact of child care coverage on parental labor supply as well as various placebo samples. Model using both instruments simultaneously. Outcome and control variables are defined in Section 4. Standard errors in parentheses are clustered at the municipality level and robust to heteroskedasticity. * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$)

not available for these samples (they either don't have children or have older children, for which there is no cash for care data), we run the reduced form regression:

$$y_{ikt} = \alpha_k + \tau_t + \pi_1^R CC_{k,t-1}^1 + \pi_2^R CC_{kt}^2 + \boldsymbol{\theta} \mathbf{X}_{ikt} + \epsilon_{ikt} \quad (5)$$

Since we have two instruments, we will have two coefficients of interest for the reduced form and placebo specifications. Estimates for these coefficients are given in table 7. The estimate of $\hat{\pi}_1^R$ from the baseline sample should be compared to the same estimates in the placebo samples, and similarly for $\hat{\pi}_2^R$.

The coverage rates have significant effects in the reduced form specification, both for earnings and the labor force attachment indicators. Reassuringly, we find no effects for neither of the two placebo outcomes, supporting our overall empirical strategy.

8 Conclusion

We investigate the labor supply effects of the use of child care following a Norwegian reform in 2002. Rather than looking at preschoolers like most of the literature, we look at parents of two year olds. We use a fixed effects approach that exploits variation in child care coverage between municipalities and over time stemming from the variation in expansion following the child care reform. To guard against endogeneity problems, particularly reverse causality, we instrument individual child care enrollment with rationed municipal coverage rates. Our approach is supported by a battery of robustness tests that investigate alternative explanations for our findings.

Using this method, we find relatively large labor supply response among mothers of toddlers. Cohabiting mothers seem to respond by moving from no or little employment to more or less full-time work: We find an enrollment elasticity of around .26, indicating that .26 cohabiting mother enter the labor force for each toddler 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 respond by moving from no employment to part-time work. Per toddler of a single mother enrolled, we find that .2 single mother enter 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.

These results contrast much of the literature, which tends to find relatively small effects of availability or price of child care on labor supply. This might indicate that the

response to child care availability differs for children of different ages. One explanation is 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 find informal care arrangements, possibly using grandparents or other relatives, that enable them to work even when child care is not available. This is supported by the fact that we find no effects on the labor supply of working age grandparents. For fathers, we find little effect, neither for cohabiting fathers nor 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, staying home when child care is not available.

It may be of particular interest to compare our study to that of Havnes and Mogstad (2011), who study Norway in the late 1970s. A set of roughly comparable surveys that were performed in 1968 and 2002 (Pettersen (2003); White Paper no. 39 (1972), but see also Moafi and Bjørkli (2011); Reppen and Rønning (1999)) may shed light on the preferences of mothers of toddlers and preschoolers. In these surveys, representative samples of mothers were asked about their preferred mode of child care, irrespective of price and availability. For preschoolers in 1968, around 25 % of mothers prefer informal solutions such as childminders, nannies or relatives. The comparable number for toddlers in 2002 is around 6 %, indicating that preferences of mothers in the 1970s are quite different from preferences of mothers in the 2000s. This could drive differences in the demand for informal care, and in turn drive the differences in the estimates in the two studies.

Furthermore, we 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. Despite the relatively large results on labor force participation among both single and cohabiting mothers, our estimates show insignificant effects on total taxes paid. Child care enrollment does not seem to increase the taxes paid by mothers, and we find little support for the argument that cost of child care subsidies are offset by increases in the tax base. This illustrates how nonlinearities in the tax schedule may complicate conclusions on the effects on the tax base based on estimates of labor supply effects - even for the cohabiting mothers, the effective average tax levied on the extra 60,000 NOK of income was only a nonsignificant 5.3%. Overall, there seems to be very limited evidence supporting the cost mitigation argument for child care subsidies. A full cost-benefit analysis would have to take into account other effects, particularly the decrease in cash for care benefits or other forms of welfare migration, but any effects going through the tax base seem negligible.

The results from this 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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Table 8: Previous estimates of elasticities of maternal labor force participation with respect to child care prices

Source	Child age	Country	Method	Year	Data source	Elasticity
Wrohlich (2004)	0–6	Germany	Discrete choice	2002	SOEP	-.02
Lokshin (2004)	0–7	Russia	Multinomial choice	1994-1996	RLMS	-.12
Viitanen (2005)	0–18	UK	Discrete choice	1997-2003	FRS	-.14
Baker et al. (2005)	0–4	Canada	Difference in Differences	1994-2003	NLSY	-.24
Lokshin and Fong (2006)	0–12	Romania	Multinomial choice	1999	RCCES	-.46
Del Boca and Vuri (2007)	0–3	Italy	Discrete choice	1998	SHIW, Multiscopo	-.31
Kornstad and Thoresen (2007)	0–6	Norway	Multinomial choice	1998	CFC survey	-.12
Lundin et al. (2008)	1–9	Sweden	Difference in Differences	2001, 2003	Administrative	-.04 [†]
Herbst (2010)	0–12	US	Discrete choice	1990-2004	SIPP, CPS	-.05
Borra (2010)	0–4	Spain	Discrete choice	2002-2003	Time use survey	-.93
Wrohlich (2011)	0–6	Germany	Multinomial choice	2001-2003	SOEP	-.04
Gathmann and Sass (2012)	2	Germany	Difference in Differences	2006	Microcensus	-.1
Gathmann and Sass (2012)	2	Germany	Difference in Differences	2006	SOEP	-.26
Breunig et al. (2012)	0–13	Australia	Discrete choice	2005-2007	HILDA	-.29
Hardoy and Schöne (2013)	0–3	Norway	Difference in Differences	1995-2005	Administrative	-.25
Breunig et al. (2014)	0–13	Australia	Multinomial choice	2012	HILDA	-.25

Estimates of elasticity of labor force participation with respect to child care prices after Blau and Currie (2006): Per cent change in labor force participation from a 1% increase in price of child care. Attention restricted to samples based on the youngest child to follow the literature. Results for married mothers, all mothers or cohabiting mothers, depending on authors' specification. [†]Underlying main effect not significantly different from zero at the 5% level.

Table 9: Previous estimates of child care enrollment elasticities of maternal labor force participation

Source	Child age	Country	Method	Year	Identification	Elasticity	
						Cohab.	Single
Gelbach (2002)	5	US	IV	1980	Quarter of birth	.05	.04
Berlinski and Galiani (2007)	0-3	Argentina	FE	1994-2000	Staggered expansion of preschool		.06 ^{†*}
Lefebvre and Merrigan (2008a)	0-5	Canada	DD	1994-2003	Reform in Quebec	.51*	
Baker et al. (2008)	0-4	Canada	DD	1994-2003	Rform in Quebec	.48*	
Cascio (2009)	5	US	IV	1950-1990	Quarter of birth	-.08 ^{†*}	.45*
Fitzpatrick (2010)	4	US	RD	2000	Birth date eligibility cutoff	-.16 [†]	-.40 [†]
Goux and Maurin (2010)	2	France	RD	1999	Birth date eligibility cutoff	.01 ^{†*}	.10 ^{†*}
Goux and Maurin (2010)	3	France	RD	1999	Birth date eligibility cutoff	.02 ^{†*}	.25*
Havnes and Mogstad (2011)	3-6	Norway	DD	1974-1977	Staggered expansion	.06	
Nollenberger and Rodríguez-Planas (2011)	3	Spain	DD	1987-1997	Preschool reform, control age 2	.30	
Schlosser (2011)	3-4	Israel	DD	2000	Gradual rollout of free preschool	.35*	
Berlinski et al. (2011)	4	Argentina	RD	1995-2001	Birth date eligibility cutoff	.13*	
Fitzpatrick (2012)	5	US	RD	2000	Birth date eligibility cutoff	.05 ^{†*}	.19*
Dustmann et al. (2013)	3-6	Germany	FE	1994-2006	Gradual expansion following reform	.06 ^{†*}	
Felfe et al. (2013)	0-12	Switzerland	IV	2010	Variation across canton borders	.07 ^{†*}	
Asai et al. (2014)	0-6	Japan	FE	1990-2010	Variation across time and space	.01 [†]	
Lovász and Szabó-Morvai (2014)	3	Hungary	RD	2010	Age 3 vs age 4-5, birthdate cutoff	.10	
Busse and Gathmann (2014)	3	Germany	DD	2001-2012	Gradual rollout of free child care	.41	
Busse and Gathmann (2014)	4	Germany	DD	2001-2012	Gradual rollout of free child care	.43 [†]	
Busse and Gathmann (2014)	5	Germany	DD	2001-2012	Gradual rollout of free child care	-.37 [†]	
Busse and Gathmann (2014)	6	Germany	DD	2001-2012	Gradual rollout of free child care	-.07 [†]	
Brewer et al. (2014)	3	UK	RD	2004-2013	Birth date eligibility cutoff	.36 [†]	
Brewer et al. (2014)	3	UK	IV	2000-2008	Geographical rollout	.14 [†]	
Bauernschuster and Schlotter (2015)	3-4	Germany	IV	1995-20015	Quarter of birth	.36	
This paper	2	Norway	FE	2002-2008	Gradual expansion following reform	.26	.20

Note: Estimates of elasticity of labor force participation with respect to child care enrollment: Percentage point increase in participation following a 1 point increase in enrollment.

Attention restricted to samples based on the youngest child. “Cohab” reports results for married mothers, all mothers or cohabiting mothers, depending on paper. [†]Underlying main effect not significantly different from zero at the 5% level. *Enrollment elasticity not reported, roughly calculated based on reported estimates.

Table 10: Descriptive statistics for cohabiting fathers at child aged 2, 2002–2008.

		2002	2004	2006	2008
A. Outcome variables					
Earnings (NOK)		580,956	561,369	591,374	617,157
		(378,531)	(436,079)	(442,677)	(469,397)
Employed		0.918	0.902	0.915	0.928
		(0.27)	(0.30)	(0.28)	(0.26)
Full-time equivalent employment		0.843	0.817	0.841	0.862
		(0.36)	(0.39)	(0.37)	(0.34)
Weeks of employment above	4 hours	43.38	43.01	43.64	44.85
		(18.1)	(18.5)	(17.9)	(16.7)
	30 hours	41.65	41.08	41.73	42.92
		(19.6)	(20.1)	(19.5)	(18.6)
B. Control variables					
Age		35.10	35.45	35.77	35.84
		(5.73)	(5.78)	(5.79)	(5.91)
Married		0.628	0.613	0.598	0.583
		(0.48)	(0.49)	(0.49)	(0.49)
Immigrant		0.0615	0.0712	0.0767	0.0886
		(0.24)	(0.26)	(0.27)	(0.28)
Years of education		14.74	14.85	14.96	15.03
		(3.25)	(3.31)	(3.34)	(3.39)
Number of children		2.068	2.054	2.055	2.040
		(1.05)	(1.05)	(1.04)	(1.04)
below 6 years old		1.510	1.516	1.517	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 labor market control		0.837	0.830	0.876	0.915
		(0.033)	(0.038)	(0.033)	(0.032)
Earnings one year before birth		536,662	549,150	521,415	520,796
		(458,117)	(427,224)	(350,793)	(428,838)
Observations		42,157	39,270	40,197	41,541

Note: The table gives means in the estimation sample for control variables among cohabiting mothers. Variables are defined in Section 4.

Table 11: Descriptive statistics for single mothers at child aged 2, 2002–2008.

	2002	2004	2006	2008
A. Outcome variables				
Earnings (NOK)	124,459 (187,346)	123,321 (181,157)	133,515 (190,222)	160,896 (208,597)
Employed	0.284 (0.45)	0.283 (0.45)	0.304 (0.46)	0.362 (0.48)
Full-time equivalent employment	0.128 (0.33)	0.132 (0.34)	0.147 (0.35)	0.175 (0.38)
Weeks of employment above				
4 hours	19.49 (23.1)	20.33 (23.8)	21.02 (23.6)	24.82 (23.8)
30 hours	11.49 (20.3)	12.59 (21.2)	12.89 (21.1)	15.82 (22.5)
B. Control variables				
Age	28.99 (6.09)	29.27 (6.27)	29.79 (6.30)	29.96 (6.55)
Married	0.0375 (0.19)	0.0507 (0.22)	0.0561 (0.23)	0.0559 (0.23)
Immigrant	0.0677 (0.25)	0.0760 (0.27)	0.100 (0.30)	0.106 (0.31)
Years of education	12.82 (3.21)	12.88 (3.36)	12.97 (3.46)	13.00 (3.61)
Number of children	1.695 (0.99)	1.703 (1.04)	1.744 (1.07)	1.717 (1.07)
below 6 years old	1.289 (0.51)	1.280 (0.51)	1.272 (0.50)	1.272 (0.51)
below 13 years old	1.556 (0.79)	1.537 (0.80)	1.565 (0.82)	1.540 (0.81)
below 18 years old	1.656 (0.92)	1.654 (0.95)	1.684 (0.96)	1.660 (0.97)
Local labor market control	0.834 (0.036)	0.826 (0.041)	0.874 (0.037)	0.912 (0.039)
Earnings year before birth (NOK)	195,454 (215,320)	194,154 (201,531)	184,977 (200,881)	179,973 (209,263)
Observations	5,277	4,869	4,492	4,597

Note: The table gives means in the estimation sample for control variables among cohabiting mothers. Variables are defined in Section 4.

Table 12: Descriptive statistics for non-residing fathers at child aged 2, 2002–2008.

	2002	2004	2006	2008
A. Outcome variables				
Earnings (NOK)	37,1179 (301,942)	323,975 (273812.0)	370077 (315,590)	398,296 (370,505)
Employed	0.715 (0.45)	0.651 (0.48)	0.704 (0.46)	0.739 (0.44)
Full-time equivalent employment	0.551 (0.50)	0.480 (0.50)	0.545 (0.50)	0.584 (0.49)
Weeks of employment above				
4 hours	31.67 (23.4)	30.18 (23.8)	32.90 (23.0)	34.99 (22.5)
30 hours	28.85 (24.0)	26.66 (24.3)	29.85 (23.7)	32.07 (23.5)
B. Control variables				
Age	31.92 (7.23)	32.24 (7.43)	32.67 (7.41)	32.87 (7.71)
Married	0.0394 (0.19)	0.0516 (0.22)	0.0528 (0.22)	0.0561 (0.23)
Immigrant	0.0710 (0.26)	0.0707 (0.26)	0.0922 (0.29)	0.0955 (0.29)
Years of education	12.73 (3.00)	12.78 (3.08)	12.78 (3.09)	12.75 (3.23)
Number of children	1.785 (1.12)	1.793 (1.15)	1.823 (1.15)	1.860 (1.18)
below 6 years old	1.333 (0.57)	1.324 (0.57)	1.323 (0.56)	1.339 (0.59)
below 13 years old	1.586 (0.83)	1.581 (0.85)	1.597 (0.85)	1.613 (0.87)
below 18 years old	1.694 (0.97)	1.698 (1.01)	1.730 (1.01)	1.748 (1.03)
Local labor market control	0.835 (0.036)	0.826 (0.042)	0.873 (0.040)	0.911 (0.042)
Earnings year before birth (NOK)	347,329 (275,345)	330,757 (272,810)	316,054 (334,524)	321,328 (317,979)
Observations	4,647	4,243	3,958	3,990

Note: The table gives means in the estimation sample for control variables among cohabiting mothers. Variables are defined in Section 4.

Table 13: Reduced form results: The impact of child care coverage on labor supply

	CC_{t-1}^1	CC_t^2	Mean 2002	N
Cohabiting mothers				
Earnings	14,251*** (5.041)	7.892* (4,456)	269,532	283,706
Employed	0.072*** (0.015)	0.022* (0.012)	0.63	
Full-time eq.	0.058*** (0.016)	-0.0002 (0.012)	0.34	
Wks > 4h	2.49*** (0.567)	0.40 (0.52)	35.0	
Wks > 30h	2.89*** (0.677)	0.22 (0.54)	23.6	
Cohabiting fathers				
Earnings	-20,537 (12,741)	14,726 (9,169)	580,956	283,494
Employed	0.00023 (0.0092)	-0.0085 (0.00760)	0.92	
Full-time eq.	0.013 (0.011)	-0.013 (0.010)	0.84	
Wks > 4h	0.30 (0.56)	-0.49 (0.52)	43.4	
Wks > 30h	0.29 (0.62)	-0.66 (0.55)	41.7	
Single mothers				
Earnings	6,199 (12,355)	4,586 (9,218)	124,459	33,262
Employed	0.048 (0.036)	0.021 (0.027)	0.28	
Full-time eq.	0.0018 (0.026)	-0.0048 (0.0213)	0.12	
Wks > 4h	2.18 (1.86)	1.35 (1.59)	19.5	
Wks > 30h	0.55 (1.71)	-0.15 (1.45)	11.5	
Non-residing fathers				
Earnings	-13,089 (20,302)	22,581 (184,15)	371,179	29,202
Employed	-0.046 (0.030)	0.056* (0.029)	0.72	
Full-time eq.	-0.0089 (0.035)	0.038 (0.031)	0.55	
Wks > 4h	1.06 (1.65)	-1.61 (1.61)	31.7	
Wks > 30h	0.17 (1.55)	-0.30 (1.56)	28.9	

Note: Reduced form estimates of the impact of child care coverage on parental labor supply. Outcome and control variables are defined in Section 4. Standard errors in parentheses are clustered at the municipality level and robust to heteroskedasticity. * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$)

Table 14: IV results: Separate instruments

	Model 1 (separate)		Model 2 (separate)		Mean	N		
	First stage	Second stage	First stage	Second stage				
Cohabiting mothers								
Earnings	0.337*** (0.0377)	80.02	58,224*** (13,076)	0.219*** (0.0345)	40.33	72,411*** (19,576)	269,532	283,706
Employed			0.258*** (0.0296)			0.286*** (0.0382)	0.630	
Full-time eq.			0.168*** (0.0274)			0.147*** (0.0352)	0.340	
Wks > 4h			8.151*** (1.573)			8.223*** (2.134)	35.00	
Wks > 30h			9.006*** (1.666)			8.494*** (1.994)	23.57	
Cohabiting fathers								
Earnings	0.341*** (0.0375)	82.42	-29,591 (34,489)	0.222*** (0.0341)	42.44	13,546 (48,662)	580,956	283,494
Employed			-0.0174 (0.0238)			-0.0378 (0.0290)	0.918	
Full-time eq.			0.00969 (0.0310)			-0.0264 (0.0386)	0.843	
Wks > 4h			-0.155 (1.421)			-1.424 (1.870)	43.38	
Wks > 30h			-0.555 (1.594)			-2.241 (1.995)	41.65	
Single mothers								
Earnings	0.313*** (0.0444)	49.62	29,971 (34,357)	0.194*** (0.0440)	29.45	39,361 (44,325)	124,459	33,259
Employed			0.201** (0.0988)			0.240* (0.129)	0.284	
Full-time eq.			-0.00656 (0.0665)			-0.0217 (0.0880)	0.128	
Wks > 4h			10.13* (5.448)			13.03 (8.047)	19.49	
Wks > 30h			1.394 (4.152)			0.758 (5.645)	11.49	
Non-residing fathers								
Earnings	0.441*** (0.0257)	294.8	9,515 (35,992)	0.335*** (0.0273)	147.8	40741.8 (446,97)	370,1179	29,198
Employed			-0.00915 (0.0495)			0.0699 (0.0648)	0.715	
Full-time eq.			0.0443 (0.0597)			0.0945 (0.0739)	0.551	
Wks > 4h			-0.298 (2.684)			-2.536 (3.400)	31.67	
Wks > 30h			-0.0593 (2.563)			-0.433 (3.368)	28.85	

Note: Second stage estimates from equation(1) of the impact of child care enrollment on parental labor supply. Outcome and control variables are defined in Section 4. Standard errors in parentheses are clustered at the municipality level and robust to heteroskedasticity. * (p<0.10), ** (p<0.05), *** (p<0.01)

Table 15: IV results for working age grandparents

	First stage		F	Second stage		Mean	N
	CC_{t-1}^1	CC_t^2					
Maternal grandmothers							
Earnings	0.505*** (0.0178)	-0.00355 (0.0154)	595.1	-8,034 (7433.2)	242,939	218,896	
Employed				-0.0122 (0.0144)	0.584		
Full-time eq.				-0.0109 (0.0145)	0.342		
Wks > 4h				-0.855 (0.809)	31.71		
Wks > 30h				-0.0848 (0.816)	20.19		
Maternal grandfathers							
Earnings	0.511*** (0.0172)	0.00241 (0.0154)	638.9	8,320 (16,247)	443,331	184,380	
Employed				-0.0345** (0.0150)	0.707		
Full-time eq.				-0.0371** (0.0173)	0.624		
Wks > 4h				-0.935 (0.944)	32.25		
Wks > 30h				-0.948 (0.938)	29.85		
Paternal grandmothers							
Earnings	0.502*** (0.0211)	0.0103 (0.0189)	484.0	3,004 (6148.0)	231,995	193,312	
Employed				-0.00915 (0.0155)	0.553		
Full-time eq.				0.0272* (0.0146)	0.324		
Wks > 4h				-0.588 (0.791)	18.96		
Wks > 30h				1.285 (0.888)	30.36		
Paternal grandfathers							
Earnings	0.509*** (0.0189)	0.0110 (0.0192)	684.2	-12,615 (13,210)	426,054	156,648	
Employed				-0.0290 (0.0189)	0.676		
Full-time eq.				-0.0207 (0.0195)	0.592		
Wks > 4h				-0.578 (1.049)	31.06		
Wks > 30h				0.406 (1.048)	28.53		

Note: First- and second stage estimates from equation (2) and (1) of the impact of child care enrollment on parental labor supply. Outcome and control variables are defined in Section 4. Standard errors in parentheses are clustered at the municipality level and robust to heteroskedasticity. * (p<0.10), ** (p<0.05), *** (p<0.01)

Table 16: Persistency analysis: IV results with lagged outcomes

	Baseline	One year later	Two years later	Three years later
Cohabiting mothers				
Earnings	59,793*** (13,336)	17,801* (10,501)	5,634 (10,295)	553.3 (14,781)
Employed	0.261*** (0.0292)	0.0403* (0.0229)	0.00839 (0.0226)	0.0172 (0.0241)
Full-time eq.	0.166*** (0.0268)	0.153*** (0.0380)	0.0553 (0.0393)	0.0187 (0.0466)
Wks > 4h	8.159*** (1.587)	-1.033 (0.687)	-0.751 (0.727)	0.743 (0.784)
Wks > 30h	8.949*** (1.646)	5.764*** (1.689)	5.185*** (1.589)	5.560*** (1.855)
Cohabiting fathers				
Earnings	-24,705 (35327.5)	17,239 (21,679)	14,690 (18,787)	12,429 (29,353)
Employed	-0.0197 (0.0237)	0.00463 (0.0121)	0.00128 (0.0131)	0.00234 (0.0142)
Full-time eq.	0.00561 (0.0311)	0.0120 (0.0194)	-0.0126 (0.0187)	0.00658 (0.0227)
Wks > 4h	-0.299 (1.422)	0.108 (0.493)	-0.0883 (0.514)	0.468 (0.628)
Wks > 30h	-0.746 (1.587)	0.150 (0.805)	-0.0633 (0.807)	0.592 (1.002)
Single mothers				
Earnings	30,596 (34,400)	22,184 (41,440)	13,751 (39,148)	-1,539 (41,604)
Employed	0.204** (0.0988)	0.0995 (0.125)	-0.0346 (0.112)	-0.0648 (0.104)
Full-time eq.	-0.00757 (0.0664)	0.176* (0.106)	0.106 (0.116)	0.0125 (0.117)
Wks > 4h	10.32* (5.505)	-0.611 (3.954)	3.193 (3.506)	-1.644 (3.707)
Wks > 30h	1.352 (4.132)	-8.990 (7.711)	-8.390 (7.111)	1.604 (7.474)
Non-residing fathers				
Earnings	12,652 (36,065)	18,344 (31,581)	10,780 (39,415)	30,446 (38,000)
Employed	-0.00121 (0.0496)	-0.00356 (0.0422)	-0.0103 (0.0433)	-0.0130 (0.0475)
Full-time eq.	0.0493 (0.0597)	0.0191 (0.0598)	-0.0874 (0.0667)	-0.0560 (0.0702)
Wks > 4h	-0.523 (2.679)	-0.193 (1.804)	2.596 (1.839)	1.973 (1.944)
Wks > 30h	-0.0968 (2.573)	-0.190 (2.766)	1.731 (2.701)	1.450 (2.893)

Note: Second stage estimates from equation (1) of the impact of child care enrollment on cohabiting mothers' labor supply, with the outcome lagged 1–3 years. In addition to the control variables defined in Section 4, we control for child care coverage rates for older children in the base year. Standard errors in parentheses are clustered at the municipality level and robust to heteroskedasticity. * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$)

Table 17: Robustness checks for cohabiting fathers

Specification	Earnings	Employment		Weeks of employment		<i>N</i>
		<i>employed</i>	<i>full time</i>	<i>>4h</i>	<i>>30h</i>	
Baseline results	-29,591 (34,489)	-0.0174 (0.0238)	0.00969 (0.0310)	-0.155 (1.421)	-0.555 (1.594)	283,494 428
Selective migration? Residence as in $t - 3$	-105,357* (55,518.8)	-0.0145 (0.0291)	0.0293 (0.0368)	-1.448 (1.618)	-1.089 (1.781)	239,274 425
Endogenous fertility? Include young siblings	-20,574.2 (34,117)	0.000470 (0.0239)	0.00964 (0.0273)	0.0389 (1.212)	-0.199 (1.384)	343,579 428
Population growth? ins: log number of slots	-4,632 (33,701)	-0.00515 (0.0211)	0.00665 (0.0273)	0.551 (1.345)	0.417 (1.472)	283,407 428
Municipality-specific linear time trends	22,312 (36,768)	0.0208 (0.0312)	0.0694 (0.0421)	1.120 (2.084)	0.651 (2.187)	283,569 428
Characteristic specific time shocks (eq. 3& 4)	28,428 (31,755)	0.0224 (0.0241)	0.0573* (0.0342)	1.856 (1.668)	1.482 (1.801)	283,264 427
Reverse causality? Excess demand	-12,491 (36,722)	-0.0325 (0.0293)	0.00368 (0.0361)	0.483 (1.649)	-1.500 (1.801)	184,194 360
Reverse causality? >10% excess demand	-39,362 (8,9173)	-0.0310 (0.0399)	-0.00278 (0.0522)	1.017 (2.377)	0.977 (2.444)	74,820 138
Baseline, sample with more than 2 children	-68,213 (46,329)	-0.0393 (0.0292)	0.000436 (0.0453)	1.627 (1.955)	2.047 (2.452)	70,843 424
Individual fixed effects sample with more than 2 children	-169,940* (86,652)	-0.0706** (0.0300)	-0.0212 (0.0490)	0.849 (1.648)	1.131 (2.069)	70,843 424

Note: Various robustness checks as described in the 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 18: Robustness checks for single mothers

Specification	Earnings	Employment		Weeks of employment		<i>N</i>
		<i>employed</i>	<i>full time</i>	<i>>4h</i>	<i>>30h</i>	
Baseline results	29,971 (34,357)	0.201** (0.0988)	-0.00656 (0.0665)	10.13* (5.448)	1.394 (4.152)	33,259 424
Selective migration?	30,173	0.278**	0.0423	7.614	3.931	26,218
Residence as in $t - 3$	(51,459)	(0.136)	(0.0864)	(7.405)	(6.154)	421
Endogenous fertility?	59,879*	0.241**	0.0378	10.47*	4.871	30,890
Include young siblings	(33,900)	(0.0996)	(0.0638)	(5.650)	(4.173)	425
Population growth?	9,712	0.153**	-0.0325	8.520*	1.488	33,254
ins: log number of slots	(26,600)	(0.0772)	(0.0591)	(4.476)	(3.805)	424
Municipality-specific	88,145	0.506**	0.213	18.25*	6.460	33,270
linear time trends	(61,249)	(0.203)	(0.138)	(10.52)	(8.437)	424
Characteristic specific	92,859**	0.347***	0.0880	18.35***	5.601	33,244
time shocks (eq. 3& 4)	(39,945)	(0.133)	(0.0903)	(6.677)	(5.680)	423
Reverse causality?	-14,409	0.196	-0.0430	9.999	-6.799	20,828
Excess demand	(64,551)	(0.174)	(0.115)	(8.997)	(6.266)	324
Reverse causality?	70,876	0.188	-0.161	19.99	-10.31	8,218
>10% excess demand	(81,147)	(0.294)	(0.218)	(12.42)	(12.83)	122
Baseline, sample with more than 2 children	152,198* (84,160)	0.417 (0.307)	-0.0442 (0.184)	19.46 (14.45)	-8.861 (11.35)	1,788 202
Individual fixed effects sample with more than 2 children	140,825 (97,332)	0.367 (0.341)	-0.0101 (0.244)	0.704 (17.43)	-9.214 (15.12)	1,788 202

Note: Various robustness checks as described in the 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 19: Robustness checks for non-resident fathers

Specification	Earnings	Employment		Weeks of employment		<i>N</i>
		<i>employed</i>	<i>full time</i>	<i>>4h</i>	<i>>30h</i>	
Baseline results	13,817 (35,954)	0.00126 (0.0497)	0.0491 (0.0596)	-0.229 (2.669)	0.139 (2.567)	29,198 428
Selective migration?	-198,010**	0.113	-0.0545	-5.431	-4.353	22,511
Residence as in $t - 3$	(95,390)	(0.149)	(0.170)	(8.305)	(8.235)	418
Endogenous fertility?	-7,948	0.000939	0.00417	-0.110	0.449	32,586
Include young siblings	(38,950)	(0.0507)	(0.0619)	(2.765)	(2.741)	428
Population growth?	40,479	0.215*	0.168	-4.471	1.626	29,255
ins: log number of slots	(74,986)	(0.124)	(0.136)	(6.601)	(6.596)	424
Municipality-specific	3,842	-0.0117	0.0278	0.471	0.513	29,262
linear time trends	(45,844)	(0.0598)	(0.0658)	(3.060)	(2.995)	428
Characteristic specific	56,342	0.0734	0.141*	2.706	3.528	29,168
time shocks (eq. 3& 4)	(46,357)	(0.0633)	(0.0815)	(3.782)	(3.922)	423
Reverse causality?	32,006	0.0282	0.0968	1.037	1.587	18,108
Excess demand	(48,650)	(0.0691)	(0.0767)	(3.892)	(3.724)	328
Reverse causality?	63,966	0.0457	0.172*	2.651	3.089	6,999
>10% excess demand	(80,888)	(0.0987)	(0.101)	(5.457)	(5.100)	120
Baseline, sample with more than 2 children	185,058 (119686.2)	0.170 (0.223)	0.128 (0.267)	24.14* (13.74)	24.14* (13.74)	1,826 228
Individual fixed effects sample with more than 2 children	52,088 (95,541)	0.187 (0.205)	-0.00812 (0.226)	7.699 (9.982)	10.71 (10.20)	1,826 228

Note: Various robustness checks as described in the text. Standard errors in parentheses are clustered at the municipality level and robust to heteroskedasticity. * ($p < 0.10$), ** ($p < 0.05$), *** ($p < 0.01$)