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Households' responses to price changes of formal childcare

BY

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Households' responses to price changes of formal childcare*

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The current understanding about how households respond to price changes in formal childcare is not extensive. This study examines this response through a 1998 reform in Norway that introduced a money transfer to families who did not send their child to childcare. This cash-for-care reform raised the price of formal childcare relative to its alternatives by about 115% for 1–2 year olds but not 3–5 year olds. Using household surveys conducted before and after the reform the analysis reveals that childcare attendance fell by 13.7 percentage points because of the reform. The results also indicate that the most important alternative to formal childcare is parental care. Furthermore, households of low socioeconomic status are more price sensitive, with the main substitute mode of care for this group being parental/relative care, whereas for high socioeconomic status families the substitutes include day parks and nannies as well as parental care.

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JEL codes: D10, J13, H31

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1 Introduction

Nowadays, much childcare takes place outside the family home in many European countries. For the youngest children, attendance rates at formal childcare are especially high in the Nordic countries. (Organization for Economic Co-operation and Development (OECD) 2016). Extensive public subsidization of the childcare sector has facilitated the expansion in childcare attendance rates, with public funding supported by arguments that formal childcare has beneficial effects for both parental employment and the children themselves.¹

Parents making decisions on the form of childcare used face a number of considerations. Labour market attachment, childcare quality assessments, childcare availability, and price are all components that can influence the decision process about the form of childcare. Nevertheless, given that formal childcare has beneficial effects for the children and their mothers, it is of great importance for both policy makers and researchers to know how parents respond to price changes in formal childcare. Child care subsidies are costly to the tax payers. Furthermore, subgroups in the population might respond differently to price changes. Given the importance of the question, it is safe to say that the number of studies analyzing this is fairly limited - possible because few studies have isolated large shocks to childcare prices unaccompanied by other (non-price) changes and examined their consequences. Evidence on the subject remains scarce because of either a lack of data or suitable natural experiments, and more work on this question is needed. In light of recent emphasis of the importance of childcare and other early influences on later life outcomes, our main contribution is to add evidence on how price sensitive parents are to changes in childcare prices, and show results on the conse-

¹For a discussion on the effects of public childcare on parental employment, see Lundin et al. (2008), Mogstad and Havnes (2011), Baker et al. (2008), Lefebvre and Merrigan (2008), Bettendorf et al. (2015) and Bauernschuster and Schlotter (2015). A recent survey on the literature of the effects of childcare on children can be found in Heckman and Mosso (2014).

quences of a price change in formal childcare for other care arrangements. In addition to this, how parents respond to price changes in childcare may provide a glimpse into the motivation behind intra-household caregiving (Grossbard (2014), Becker (1981)). It will then give a perspective on the role of altruism versus monetary incentives in intra-household decisions regarding caregiving for young children.

A possible way to study responses to changes in childcare prices is to analyze changes in childcare subsidies. However, this method has some limitations. For example, childcare subsidies tend to be means tested, which complicates the interpretation of any effects, and using subsidy eligibility cut-offs necessarily limits the validity of any effect to some specific subpopulation. As an alternative, we propose to use the introduction of a particular type of policy, namely, a cash-for-care (CFC) benefit reform, to examine how households respond to price changes for childcare in a way that does not suffer from these limitations. The Norwegian data provide an **almost** ideal context to investigate the question of how households respond to price changes in formal childcare. The introduction of the CFC reform was not followed by tax cuts nor transfers, i.e. no simultaneous changes in income. Neither was the reform followed by changes in means testing. Furthermore, the capacity was unchanged, i.e. the CFC reform affected only the demand, and not the supply of formal daycare. Finally, the introduction of the CFC reform was introduced at the same time in whole Norway. Thus, our analysis does not suffer from potential biases from unobserved characteristics that may vary if one would do a analysis between different states. The CFC benefit was available in Norway for 1-year-old children from 1 August 1998 and for 2-year-old children from 1 January 1999. The reform would eventually provide 3,000 Norwegian kroner (NOK) per month to parents choosing not to send their 1–2-year-old children to formal childcare providers receiving public funds.² As the benefit was unavailable for 3–5 year olds, we employ difference-in-

²1 NOK \approx 0.122 USD.

differences (DID) estimation to compare the rates of childcare for eligible and ineligible children before and after the introduction of the reform. Thus, our treatment group and our control group are extremely similar.

At the time of the reform, the average monthly price for a municipal childcare seat in Norway was about 2,600 NOK for a middle-income household; thus, the reform represented a nearly 115% price increase for formal childcare relative to any other forms of childcare, which is quite significant.³ For this reason, the CFC reform may yield valuable information on how price sensitive parents are to the price of formal childcare in general, as well as what the alternative modes of care is. It may also highlight the price sensitivity of particular subgroups in the population as there may be many reasons why some demographic groups are more likely to receive the CFC benefit and less likely to send their children to formal childcare. This paper attempts to address this issue by separating the effect of the benefit for groups of different socioeconomic status (SES).

Some literature have already estimated the effect of childcare prices on childcare attendance. The most recent contributions use policy reforms or rules that provide exogenous shocks to childcare prices and study its impact.⁴ For example, Baker et al. (2008) examined a reform in the childcare sector in Quebec that included a generous childcare subsidy that set the price of childcare at just 5 Canadian dollars per day. After comparing childcare attendance in Quebec to the rest of Canada, before and after the reform, Baker et al. (2008) found that childcare use increased, while there was a shift from care by relatives and non-licensed non-relatives. An important factor that separates that study from the current analysis is that the childcare subsidy coincided

³Eibak (2002) reports results from a survey conducted in 2002. Private childcare centers had somewhat higher prices. Middle income households refers to households with a yearly income of 250 000 - 375 000 NOK

⁴An older literature estimates the price elasticities of childcare, including Blau and Hagy (1998), Powell (2002) and Connelly and Kimmel (2003). These studies report price elasticities ranging from -0.3 to -1 . A contribution of this analysis relative to that literature is the use of a different identification strategy.

with an expansion in childcare capacity. Therefore, Baker et al. (2008) did not isolate the effect of the subsidy on childcare attendance. Another separating feature is that prior to the reform identified in Baker et al. (2008), other childcare subsidies depended on family income. The effective price change in childcare following the new program therefore also depended on family income. In contrast, the CFC subsidy in Norway is uniform for all families, which makes it easier to more directly interpret and compare any price responses.

Gathmann and Sass (2012) is closest in spirit to the present analysis because it also used a nationwide population survey to analyze the consequences of the introduction of a CFC program, but in a single German state. As the benefit applied in only one state, a factor that separates our studies is that Gathmann and Sass (2012) mainly compared the childcare outcomes in the reform state relative to those in other states, whereas we compared the childcare outcomes for eligible and ineligible children across different ages. However, the development of the childcare sector may be different in various states in Germany, and if so, this invalidates the key identification when doing a DID analysis. In contrast, our study examines the development of a control and treatment group in a childcare sector under the same contexts before and after the reform.

As an alternate, Black et al. (2014) considered the consequences of childcare subsidies by utilizing the fact that eligibility depends on sharp family income cut-offs. By comparing families immediately below and right above the income cut-offs, they found, among other things, that there was no effect of the subsidy on formal childcare attendance for children aged 5 years.

One explanation for this finding is that there is an excessive demand for childcare, which is therefore price inelastic. Another possible explanation is that information about the subsidy is not easily available to parents before they actually apply for childcare. Lastly, an important point when comparing the analysis in Black et al. (2014) to

ours is that the subsidy eligibility cut-offs they considered were for 5-year-old children, while the children in our study are much younger (1–2 year-olds).

Other studies that have specifically looked at the Norwegian CFC reform have mostly focused on the effects of maternal labour force participation.⁵ For instance, Schøne (2004) associated a modest reduction in the female labour supply with the reform, while Naz (2004) identified a relatively larger labour participation response among more highly educated mothers. Elsewhere, Hardoy and Schøne (2008) focused on the labour supply of non-Western immigrant females and found that the CFC reform reduced immigrant female labour supply more than it did for non-immigrant females. This suggests that immigrants are more responsive to the reform on the labour supply margin. Drange and Rege (2013) look at long term outcomes and finds that the effect on mothers labor market outcomes persist even after the children become CFC ineligible, but disappears when the children are aged 6-7. In contrast to the previous studies, Rønsen (2001) also studies childcare attendance, but use a different estimation strategy. Bettinger et al. (2014) explores what happens to older siblings of CFC eligible children, while Drange (2015) focus on both parents time allocation.

Of course, the impact of the price increase in formal childcare may affect attendance at other childcare alternatives. There are two main motivations for knowing these alternative modes. One reason may be that a price increase in childcare can have direct effects on the labour market attendance of mothers. If an important alternative to formal childcare is nanny care, then the employment effects of childcare prices on mothers are not clear. A second reason is that knowing about alternative modes of care improves the interpretation of the effects of formal childcare attendance on children's future outcomes. Given the discussion in the literature on the effect of early intervention, it is important to know the main alternative(s) for formal childcare (Heckman and

⁵This follows an international literature on the labour supply effects of childcare subsidies. Examples are Blau and Robins (1988), Leibowitz et al. (1992), Lundholm and Ohlsson (1998), and Ribar (1992).

Mosso (2014)). Since this literature often estimates and compares effects by parental SES, knowing the alternative for both groups are important for the interpretation.

Four main findings arise from the analysis. First, the results demonstrate that the reform reduced usage among eligible children by 13.7 percentage points. In the absence of income effects, this points to a childcare price elasticity of about -0.354 . Second, the effect of the price change of childcare on attendance suggests that low-SES families are more price sensitive. The point estimate of the effect for the subsample of low-SES families are -21.0 percentage points. Furthermore, the reform affected attendance most among the youngest CFC eligible children. Lastly, the most important alternative to formal childcare are parental care. It also appears that parental/relative care is relatively more important as an alternative for younger CFC eligible children and for children from low-SES families.

The remainder of the paper is organized as follows. Section 2 discusses the institutional framework for the CFC policy. Section 3 describes the data and details the econometric model used in the analysis. Section 4 reports and discusses the results and Section 5 concludes.

2 Institutional Setting

The development of a public childcare sector in Norway relates to the increase in female labour market participation. In the mid-1960s, few mothers were active labour market participants, and correspondingly there were relatively few childcare centers. As female labour participation accelerated in the 1970s, there was a corresponding increase in childcare attendance. In the early stages of the development of a public childcare sector in Norway, the focus was on giving alternatives to older children (those aged 3–6 years). As shown by Table A.1 in the Appendix, this has changed over time with

labour market attachment by mothers of children aged 0–3 years increasing from 29% in 1972 to 70% in 1991.

The 1990s were subject to two reforms other than the CFC reform that could have consequences for the public childcare sector. In 1993, maternity leave extended up until the child turned 1 year old, and in 1997, a process of including 6 year olds in the school system was finalized. These reforms in effect excluded two cohorts of children from childcare. However, while the maternity leave reform may have had only small consequences for the childcare market, especially as few 1 year olds or younger attended childcare, the school starting-age reform would have had much larger implications for the childcare market.⁶

Outside the family, ordinary childcare centers, family childcare, relatives, or nannies normally care for children in Norway. Family childcares are usually smaller groups where the care is run by one of the parents in private homes. The professional alternatives can have private or public ownership, but all types of childcare receive operating funds from public sources, except for nannies, with funds channelled through either the central government or the municipality. Before 2003, all childcare providers received funding from the central government, while not all privately owned providers received funding from the municipality.

In Norway, formal public or private childcare is centrally regulated through the Day Care Act (Barenhageloven) and by different prescripts to the act. This provides a set of common rules for childcare across Norway, as childcare centers are administered at the municipal level. A consequence of the local administration is that prices varies between municipalities. Childcare centers are normally open during daytime working hours from 7 am to 5 pm.

There were three main purposes of the CFC reform: (i) provide more freedom of

⁶We discuss these findings in Section 4.1.

choice to parents of the form of childcare, (ii) provide parents with more time to be with their children, and (iii) to redistribute funds to families that did not receive services from heavily subsidized childcare providers (Kontantstøtteloven 1998). The eligibility criteria for the CFC benefit are nationally regulated. Appendix Table A.2 shows that there have been some changes to the law over time concerning the size and age criteria, but the main features of the benefit have remained largely the same, with the benefit paid to parents of children aged 1–2 years old who do either not or only partially attend childcare that receives public funding. The subsidy is granted to the parent with whom the child lives. Parents need to apply for the benefit, and it can be paid for up to 3 months prior to application if the eligibility criteria were fulfilled during the period.

3 Data and econometric approach

The data are from national living standard surveys administered in the spring of 1998, 1999, and 2002, i.e., before and after the implementation of the CFC reform. These surveys collected information about the usage of different forms of childcare, as well as background characteristics of the families surveyed. Statistics Norway collected the data with the purpose of evaluating the effects of the reform.

We mainly concentrate on the following question asked in the surveys, “What form of care does your child have during daytime/working hours?” The question asked before and after the reform was in the form of a multiple-choice question, where the respondent indicated one or more care alternatives. The question in the 2002 survey was slightly different in that it comprised a separate question concerning parental care. To obtain a consistent measure of parental care across the three survey years, we coded “Parental care” for those respondents that did not identify any of the other care alternatives in all years. The choice of how to code parental care does not matter for the results. Both

ordinary childcare centers and family childcare groups are defined as formal childcare. The surveys were conducted using self-reported questionnaire and telephone interviews.

Parents may respond to the reform by changing the amount of time in formal childcare, and the decision to enter the child into childcare and not. Since the questions only indicate attendance at a particular care alternative, we are analyzing the consequences on the extensive margin.

The respondents were drawn from mothers with preschool-aged children. As the surveys were intended to study the CFC reform, additional mothers with CFC-eligible children were drawn for each survey. For the survey conducted during spring 1998, only 1 year olds were considered CFC eligible, whereas for the surveys in 1999 and 2002, both 1 and 2 year olds were considered CFC eligible. To adjust for differences in the survey draw process, age dummies are included in all regressions.

Data from 1998 were collected by drawing a sample of 3,500 observations from the population of mothers with preschool-aged children, for which 84.9% of the mothers chose to respond. In the 1999 survey, 3,848 women were drawn, of which 86.6 % responded. For the 2002 survey, 3,886 mothers with preschool-aged children were drawn, of which 86.8% chose to respond. We start with a sample containing childcare information on the first- and second-born children of the respondents in the household survey. We choose to concentrate on the married/cohabiting households. To get a valid control group, we exclude children aged 3-5 with CFC eligible siblings. We then excluded information on those children aged under 1 year and older than 5 years, leaving us with a baseline sample of 6,763 children.

[Insert Table 1 here]

Descriptive statistics for the sample are reported in Table 1. The table provides the averages and proportions of the most important variables used in the analysis. Panel A details descriptive information on the background variables. The parental income

and education variables are based on self-reported pre-birth income and education. Income is measured in NOK. We note that the parents of 1–2 year olds and those of the 3–5 year olds appear similar in terms of background characteristics. For the subgroup analysis, we should also note that the immigrant population represents a relatively small proportion of the sample. We therefore expect the estimates for this group to be somewhat noisy. Panel B details the proportion of children across the different types of care arrangements. The biggest difference between the care of 1–2 and 3–5 year olds is that many more of the older group are in childcare.

Appendix Table A.3 compares the proportion in formal childcare as judged from the survey with the official coverage rates. As shown, the rates we retrieve from the household surveys generally correspond with the official figures, but are slightly lower. We interpret this small discrepancy as resulting from the different definitions of childcare attendance. In the survey, we define care attendance as the proportion of parents responding as to what form of daytime care the child receives in the spring of each year, while the official statistic reflects childcare centers reporting the number of children in formal childcare part- or full time at the end of year. Panel C of Table 1 provides the formal childcare attendance rates by subgroups. Children are defined as low SES if the mother does not have a university/college degree, and the father earns less than the 25th percentile in the fathers’ earnings distribution in the sample. The child is grouped into the immigrant category if the mother has any immigrant background. These sample splits show that children with high-SES backgrounds and non-immigrant mothers are more likely to attend formal childcare.⁷

To analyze the effect of the CFC reform on formal childcare attendance, we could compare formal childcare rates before and after the reform for eligible children. However, there could be underlying trends in formal childcare attendance rates for 1–2 year

⁷The two surveys after the reform lacked information on gender, something which prevents us from exploring rates separately for girls and boys.

olds that have little or nothing to do with the CFC reform. To overcome this, we compare the change in formal childcare rates for eligible children to the change in formal childcare rates for ineligible children. The difference in the change in childcare rates is then attributed to the reform. The following DID approach is specified as:

$$Y_{it} = \beta_0 + \beta_1 D_{it}^{age1-2} + \beta_2 D_{it}^{1999} + \beta_3 D_{it}^{2002} + \beta_4 D_{it}^{1999} \cdot D_{it}^{age1-2} + \beta_5 D_{it}^{2002} \cdot D_{it}^{age1-2} + \eta Controls_{it} + \varepsilon_{it} \quad (1)$$

where subscript i indexes the individual family child and t indexes time. The dependent variable Y_{it} is a binary variable equal to one if the parents responded that the daytime caregiver for their child is a formal childcare centre, and zero otherwise. D_{it}^{age1-2} is a binary variable denoting children aged 1–2 years at the time of the survey while D_{it}^{1999} and D_{it}^{2002} are dummies for recordings in 1999 and 2002, respectively. The coefficients of main interest are β_4 and β_5 , where β_4 captures the short-run effect (i.e., the effect the year after the reform was implemented), while β_5 , captures the long-run effect (being the effect 3 years after the reform was fully implemented).

$Controls_{it}$ consists of a set of control variables. The controls are the logarithms of the mother’s and father’s incomes, the immigrant dummy, child age dummies, dummies for the mother’s and father’s educational level (lower secondary, upper secondary, college and university), and regional dummies (Oslo (Oslo, and Akershus), east excluding Oslo/Akershus (Hedmark, Oppland, Østfold, Vestfold, Buskerud and Telemark), south-west (Vest-Agder, Aust-Agder and Rogaland), west (Hordaland and Sogn-og Fjordane), middle (Sør-Tr. and Nord-Tr.), and north (Nordland, Troms, and Finnmark)). Finally, ε_{it} is the error term. To correct for intragroup correlation in the error terms, standard errors are clustered at region–age level.

4 Results

The main results are reported in Table 2. The table provides estimates of the coefficients for β_4 and β_5 in our DID model using equation (1), i.e., the short- and long-run effects. Starting with the results in Column (1) which included all children, we obtain a negative significant coefficient of -13.2 percentage points in the short run, and a negative and statistically significant coefficient of -13.7 percentage points in the long run.

[Insert Table 2 here]

In Column (2), we concentrate on the youngest age group and therefore exclude 2-year-old children from the treatment group. Similarly, in Column (3), we focus on those 2-year-old children and exclude 1-year-old children. Comparing the results in Columns (2) and (3), we can see the effects for younger children are much stronger in both the short and long run. The estimates suggest that other factors that altruism explain care decisions even for very young children.

[Insert Table 3 here]

Table 3 details estimates of β_5 , the long-run coefficient, when the dependent variables are indicators of different forms of care.⁸ Column (1) provides estimates of the effect on all eligible children; Column (2) shows the effect only for 1-year-old children, while Column (3) shows the effect only for 2-year-old children. The Column (1) results indicate that “Parental care” use increased by 8.7 percentage points and “Nanny” use increased by about 4 percentage points while “Other” use also increased care attendance by around 4 percentage points. This latter alternative includes day parks and au pairs. Increased use of part time care is a possible reason for why the point estimates of the effects on the four alternatives to formal childcare in sum are larger than the negative effect on formal childcare.

⁸Table A.4 in the Appendix provides the short-run coefficients.

An interesting observation is that even though “Relative care” is an important care alternative for children, there is no observed increase in this type of care arrangement following the reform. There are several possible explanations. It could be that demand for this type of care is particularly inelastic, or that relative care is a complement to formal childcare. An alternative explanation is that relative care is an inferior good. Comparing the results in Columns (2) and (3), an observation is that “Parental care” is more important as an alternative care option for childcare for 1-year-old children than 2-year-old children, which is also consistent with the descriptive analysis reported in Table 1.

Columns (4)–(6) provide the results for low-SES children, while columns (7)–(9) show the results for high-SES children. Comparing the results for the two subgroups shows that a difference is that the main alternative for formal childcare is parental care for low-SES children, while the alternative forms of care are more mixed for high-SES children. Nanny care and alternatives included in the “Other” category are both important alternatives for the latter group. We also note that a significant negative coefficient shows up for “Relatives” for the high-SES group, suggesting that it is either a complement of formal childcare or an inferior good for this group. In contrast, the short run effects reported in Table A.4 shows an increase in the use of relative care for 1-year-olds of low SES parents.

[Insert Table 4 here]

Table 4 provides estimates of the long run coefficient, β_5 , for the SES and immigrant status subgroups. The motivation behind this is to explore whether particular subgroups are more or less sensitive to a price change in childcare. Column (1) details the estimates for all children in subgroups, while Columns (2) and (3) detail the estimates by child age. Panel A splits children into groups based on SES. Low SES children experience a much stronger response to the reform than high SES children. The de-

rived elasticities will be even larger for low SES children because the initial utilization is much lower (see Table 1). There is an argument that the CFC benefit redistributes to low-SES households since a lower proportion of children in these households attends childcare. Childcare is a in-kind public good that affects the distribution of (extended) income in the population (Aaberge and Langøren (2006)). The total distributional effect of the CFC reform must take into account both the direct redistribution, the effect on parental labor force participation, and the effect of change in childcare use.⁹

In Panel C, the sample is stratified by mother’s immigration status. The results for non-immigrants are almost identical to the main results, while the results for immigrants differ somewhat. We obtain a relatively large and significant coefficient for 1-year-old children, but not for all eligible children or 2 year olds. This could be explained by the small sample size and the corresponding decrease in precision.

4.1 Robustness

Identification relies on a common trend in childcare rates over time for 1–2 year olds and 3–5 year olds. Unfortunately, it is not possible to derive earlier trends because the first survey including these questions was only conducted in 1998. However, there are official statistics on children in childcare by age for the total population from 1990 to 2003 (Statistics Norway 2005), as illustrated in Figure 1. These numbers are based on childcare centers reporting the number of children in care to Statistics Norway for general administrative purposes.

[Insert Figure 1 here]

⁹Another perspective on distributional effects is how the CFC affects well-being across the income distribution (Burton and Phipps (2007)). Both income and parental time may affect family well-being, and low-SES families may gain more in terms of these factors. This can be an explanation for the observed estimates. Furthermore, the CFC benefit is a transfer to family households. The benefit will increase income inequalities between family and non-family households for men (see for instance Kunze (2016)).

The trends in formal childcare attendance for 1–2 and 3–5 year olds moved very closely together before the reform.¹⁰ Childcare rates for 1–2 year olds then increased on average 3.5 percentage points each year from 1990–1997, while those for 3–5 year olds increased at 3.2 percentage points each year for the same period.¹¹ The bump in coverage rate in 1997 is likely the result of the school starting-age reform that excluded a full cohort from childcare. Finally, the increase in the attendance rate tapers off from 1997 to 2000 for 1–2 year olds.¹²

Figure 1 suggests that the childcare growth rate for 3–5 year olds slows somewhat after the CFC reform. The yearly increase before the reform was 3.2 percentage points, while after the reform it was 2.1 percentage points.¹³ There are three main explanations for this. First, children with siblings in childcare age could be affected by the reform.¹⁴ Since we exclude this group from the treatment group, our main estimate based on the household surveys are unaffected by this. Second, the school starting-age reform excluded a full cohort of children from childcare from the 1997 onwards, and thus saturated the market for childcare slots sometime after 1997. The spike in attendance in 1997 provides some evidence in support. This would explain the lower growth rates in childcare after the reform among 3–5 year olds observed in Table 1, but would not affect identification under the assumption that it influenced 1–2 and 3–5 year olds in the same manner. The observed common pre trends support this theory. Lastly, there could be

¹⁰Note that the observed common pre-trends reported in Figure 1 are for formal childcare rates. Preferably, we would have like to have similar rates for the use of parental, relative or nanny care, but such information is not available.

¹¹A *t*-test on the difference in mean yearly changes in the pre-reform period between the two groups shows that they are not statistically different. Inference based on this test is valid if we assume that the yearly rate changes are independent.

¹²From 1999, “open childcare” providers were no longer included as attending childcare. In 1998, children in these centers represented about 2% of all children in childcare (Statistics Norway 2005).

¹³Testing for the difference in mean yearly changes in the pre- and post-reform periods using a regression with Newey-West standard errors show that they are statistically different.

¹⁴Appendix Table A.5 shows estimates of the effect on children aged 3–5 with siblings aged 1–2. Results are consistent with Bettinger et al. (2014) showing that older, ineligible siblings care type are affected by the CFC reform.

long-term effects of the CFC reform on the children affected. Children aged 3-5 after the reform could have been affected by the CFC reform since they were eligible when they were younger. We assess that any bias arising for this source should be small. In any case, this should not affect the short-term coefficients since it is measured shortly after the implementation of the reform. We conclude that 3–5 year olds without eligible siblings are unaffected by the reform, and thus comprise a valid control group.

The observed common pre trends are our main identification-test. Another way to explore whether the results reported in Table 2 are driven by factors other than the CFC reform is to run some simple regressions, as shown in equation (2). Differential trends in the observable variables would indicate that something other than the reform could explain the change in attendance rates.

$$Background_{it} = \alpha_0 + \alpha_1 D_{it}^{age1-2} + \alpha_2 D_{it}^{1999} + \alpha_3 D_{it}^{2002} + \alpha_4 D_{it}^{1999} \cdot D_{it}^{age1-2} + \alpha_5 D_{it}^{2002} \cdot D_{it}^{age1-2} + \varepsilon_{it} \quad (2)$$

[Insert Table 5 here]

Table 5 provides the results. The only significant difference is the likelihood of mother attending university, with six percentage points more university educated mothers found among 1–2 year olds than 3–5 year olds in 1999 than in 1998 in our sample. The coefficient is only significant at the 10 per cent significance level, something that could be expected given the number of estimated placebo coefficients had there been no difference.

Our study may also be invalidated because not all the randomly drawn mothers responded to the survey and this may relate to the extent families were affected by the reform. For example, if high-SES mothers were more likely to respond to the survey, and these mothers are less affected by the subsidy, it could bias the estimated effect of

the CFC benefit downwards. Further, if immigrant mothers who were more proficient in Norwegian were more likely to respond to the survey and less likely affected by the CFC subsidy, this would also weaken the results for this group. Table A.6 in the Appendix compares the education level of the mothers in the sample to the official statistics for women aged 30–34 years in 2000. Even though the two groups are not directly comparable, and one is self-reported and the other administrative, we note that the sample of mothers we used appears to be more highly educated. This is true for both non-immigrant and immigrant mothers. The subgroup estimations by SES and immigrant status reported in Table 4 suggest that the main estimates for the response to reform are higher for the sample population.

We are fortunate to have childcare attendance measurements from two sources: administrative childcare rates and household surveys. Thus, one way to validate our results from the surveys is to compare them to estimates based on administrative data. Table A.3 and Figure 1 in the Appendix detail the administrative rates. As the administrative data are reported at the end of year, the pre-reform rate we used is from 1997 as the reform was implemented in August 1998. The surveys were conducted during spring each year. Computing the long-run DID coefficient yield an estimate of the effect of the CFC reform of -9.3 percentage points.¹⁵ Given that the rates are based on slightly different measures of formal childcare attendance, this is not too far from the survey estimate of -13.7 percentage points. As discussed, the lower estimate using administrative data could be because the control group using administrative data is not valid, or that the survey population is not fully comparable.

Assuming formal childcare is a normal good, the substitution effect should decrease the demand for formal childcare, whereas the income effect will shift demand in the other direction. Depending on the size of the income effect, our estimate will then

¹⁵The calculations are based on administrative numbers reported in Table A.3 recorded closest in time to the surveys (at the end of 2001 and 1997), such that $(0.815-0.742) - (0.38-0.4) = 0.093$.

underestimate the effect of a regular increase in the formal childcare price. Assuming no income effects, the (compensated) price elasticity of formal childcare is estimated to be -0.354 .¹⁶ If the other care alternatives are normal goods and substitutes for formal childcare, the substitution and income effect should both shift demand in the same direction.

A final aspect to consider concerning the validity of the estimates is the reform environment at the time of implementation. A concern here would be the existence of capacity constraints in the childcare market. In 1997, the school starting-age reform effectively reduced the number of children in childcare by a full cohort. This suggests that the capacity of childcare spaces in the following period were not constrained. Thus, our estimates are unlikely to be driven by capacity constraints in the childcare market. The fact that there are fewer 1-2 year olds to compete for seats with because of the reform, would then not affect the 3-5 year olds, since there were already free capacity.¹⁷

Of course, it would be useful to compare our measurement of the price sensitivity of formal childcare to other causal estimates in the literature. Baker et al. (2008) obtains a price elasticity of -0.58 for 0–4 year olds in Canada. This is of much larger magnitude than our estimate, and we suspect part of the reason is that the Canadian reform included additional measures aimed at increasing the use of childcare other than the introduction of a subsidy. In contrast, Gathmann and Sass (2012) estimate a compensated own-price elasticity of -0.30 , which is much closer to our estimate. However,

¹⁶The elasticity is calculated using the long-run coefficient of 0.137 shown in Table 2, the 2002 formal childcare attendance rate of 0.335, the size of the CFC benefit of 3,000 NOK and the pre-reform childcare price for formal childcare for a middle-income household of about 2,600 NOK. $(-0.137 \div 0.335) \div (3,000 \div 2,600) = -0.354$.

¹⁷Admittedly, one may argue that the CFC reform does not allow us to analyze the effect of price changes in other contexts, since household responses may depend on the type of price change (or type of reform) observed if there are capacity constraints. For example, the response in a scenario where the price change only would affect a small group of geographically dispersed households would be different from a response in a setting where all households with 1-2 year olds in Norway are affected by a price change. This is because the change in availability will be different in the two scenarios. However, if there are free capacity there are no change in availability in either contexts.

their estimates of the impacts on informal childcare alternatives differ somewhat from our. They do find that parental care increases with the CFC benefit but find no impact on the use of nannies or “child-minders” while they do find a strong negative effect for care provided by friends/relatives. The different estimates could be explained by cross-country differences in childcare systems. Lastly, Kornstad and Thoresen (2007) estimate an elasticity for childcare of -0.12 for preschool-aged children (1–6 years) while Black et al. (2014) are unable to reject the null hypothesis of no effect on childcare use for the childcare subsidy for 5-year-old children. Such inelastic demand for childcare for groups of older children is consistent with our finding that the demand for formal childcare for younger children is more elastic than that for older children.

5 Conclusion

This study has focused on the effect of a price change on formal childcare attendance. Few studies have isolated large shocks to childcare prices unaccompanied by other changes and studied the consequences in detail. The introduction of the CFC subsidy in Norway therefore provides a good opportunity to examine the response of families to price changes in formal childcare. Recent emphasis of the importance of childcare for later life outcomes motivates our main contribution of adding evidence on how price sensitive parents are to changes in childcare prices, and show results on the consequences of a price change in formal childcare for other care arrangements.

The implementation of the subsidy accounted for a decrease in childcare attendance of 13.7 percentage points and increased parental care. This means that the benefit fulfilled its policy goal of facilitating the ability of parents to spend more time with their youngest child. The magnitude of the effect implies a compensated price elasticity for formal childcare of about -0.354 . The findings also suggest the greater price sensitivity

of low-SES households. This finding should inform discussion on the introduction of CFC benefits as well as the general price levels of formal childcare.

Given the number of studies analyzing the consequences of childcare on children's future outcomes, it is important to have detailed information on what the alternative modes of care is. The most important substitute to formal childcare in this context are parental care. That nanny care and day parks also is an alternative to formal childcare suggests that it attenuates the effect of providing affordable formal childcare on parental labour market participation. Thus, formal childcare crowds out other out-of-home care alternatives, but not fully. Furthermore, the results show that parental and relative care is relatively more important for low-SES families. This is consistent with low-SES parents being less connected to the labour market. High-SES parents choose to work and use other out-of-home care alternatives. Thus, the study demonstrates that the alternative form of care depends on important observable characteristics of the family.

Further studies should aim to explore the responses of different subgroups (such as gender or immigrants) more so than has been possible with the data available in this analysis. Moreover, how the price sensitivity varies with different levels of supply (or capacity constraints) would also enhance our understanding of childcare demand. While the reform took place in Norway at a time when the childcare sector was relatively well developed, there were still a significant number of children not attending childcare. Assuming that the elasticity is constant for different supply levels may then be insufficient, because price responses in situations when coverage is nearly full, or for very low coverage levels, may differ from the responses we observed.

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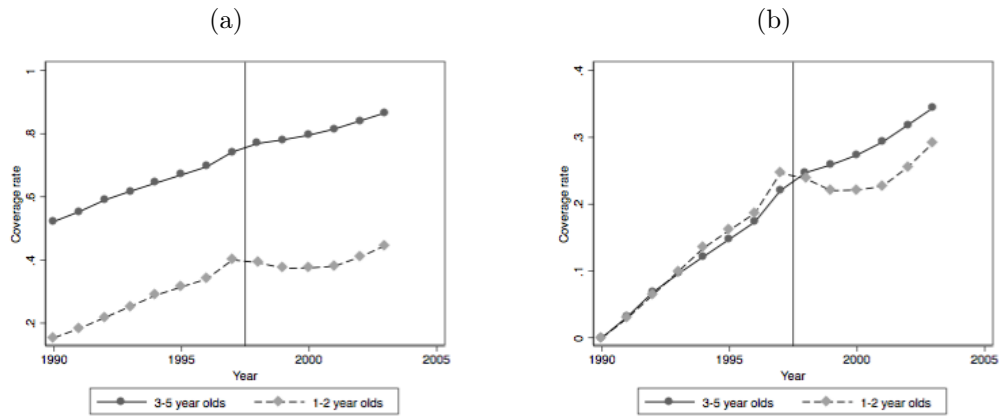
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Figure 1: Formal childcare rates



Notes: (a) End of year childcare coverage rates as reported in the official statistics based on yearly status reports from childcare providers and sent to Statistics Norway. (b) The percentage point change since 1990 (Statistics Norway 2005)

Table 1: Descriptives - Household survey

	1-2 year olds				3-5 year olds			
	1998	1999	2002	Change*	1998	1999	2002	Change*
<i>Panel A - Background</i>								
Mother income	155.744	179.266	210.837	55.092	158.637	186.550	226.476	67.839
Father income	269.508	286.905	287.291	17.783	286.684	313.693	318.808	32.123
Immigrant	0.056	0.062	0.073	0.017	0.062	0.064	0.072	0.010
Low-SES	0.178	0.207	0.154	-0.024	0.176	0.163	0.142	-0.034
Oslo/Akershus	0.193	0.213	0.207	0.014	0.199	0.192	0.198	-0.001
<i>Mother education</i>								
LS school	0.045	0.048	0.057	0.012	0.068	0.064	0.057	-0.011
US school	0.539	0.541	0.514	-0.026	0.527	0.592	0.558	0.031
University	0.376	0.373	0.411	0.035	0.370	0.312	0.371	0.001
<i>Panel B - Care alternatives</i>								
Formal childcare	0.356	0.326	0.335	-0.021	0.661	0.730	0.766	0.105
Parental care	0.314	0.323	0.330	0.016	0.173	0.114	0.105	-0.069
Relatives	0.180	0.149	0.144	-0.036	0.081	0.080	0.067	-0.015
Nannies	0.178	0.187	0.184	0.006	0.077	0.076	0.055	-0.022
Other	0.051	0.059	0.062	0.011	0.107	0.080	0.069	-0.038
N	1168	1627	1376		738	910	944	
<i>Panel C - Formal childcare by group (socioeconomic- and immigrant status)</i>								
Low-SES	0.226	0.226	0.203	-0.023	0.523	0.676	0.716	0.193
High-SES	0.384	0.352	0.359	-0.025	0.691	0.740	0.774	0.083
Immigrant	0.268	0.292	0.278	0.011	0.578	0.729	0.711	0.132
Non-immigrant	0.366	0.330	0.342	-0.023	0.672	0.730	0.774	0.102
<i>Treatment child age</i>								
Age = 2	0.457	0.428	0.455	-0.002	0.661	0.730	0.766	0.105
Age = 1	0.292	0.212	0.228	-0.065	0.661	0.730	0.766	0.105

Notes: Parental income (measured in 1,000 NOK) and education variables were based on self-reported pre-birth income and education. Childcare measures were based on responses to a survey of main daytime care of the respondents' children. Formal childcare includes municipal, private, and family childcare centers. Children were defined as low-SES if the mother did not have a university/college degree and the father earned less than the 25th income percentile. Pre-reform children are from surveys conducted before the reform during spring 1998, while post-reform children are from surveys conducted after the reform during spring 1999 and 2002. (*) The column "Change" shows the increase from 1998 to 2002.

Table 2: Main results

<i>Dep. var: Formal childcare attendance</i>			
	All	Age 1	Age 2
	(1)	(2)	(3)
Short-run effect ($\hat{\beta}_4$)	-0.132*** (0.033)	-0.154*** (0.044)	-0.098** (0.038)
Long-run effect ($\hat{\beta}_5$)	-0.137*** (0.028)	-0.165*** (0.028)	-0.096** (0.043)
Controls	x	x	x
N	6763	4801	4554

Notes: Estimates of the short-run ($\hat{\beta}_4$) and long-run ($\hat{\beta}_5$) effects of the CFC benefit on formal childcare attendance are shown. The dependent variable is an indicator for formal childcare attendance. Column (1) includes all treated children, while Column (2) includes 1-year-old children and Column (3) includes 2-year-old children. Control variables are the log of self-reported parental income, education level, region dummies, immigrant status, age dummies and married/cohabiting dummy. Standard errors clustered at region-age level and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 3: Childcare alternatives

	All			Low-SES			High-SES		
	All (1)	Age 1 (2)	Age 2 (3)	All (4)	Age 1 (5)	Age 2 (6)	All (7)	Age 1 (8)	Age 2 (9)
Parental	0.087*** (0.023)	0.130*** (0.025)	0.035 (0.027)	0.170** (0.070)	0.214** (0.079)	0.139 (0.110)	0.074*** (0.024)	0.120*** (0.024)	0.016 (0.027)
Relatives	-0.017 (0.022)	-0.021 (0.025)	-0.013 (0.023)	0.053 (0.063)	0.098 (0.065)	-0.022 (0.065)	-0.029 (0.019)	-0.044** (0.021)	-0.009 (0.023)
Nannies	0.038* (0.019)	0.023 (0.016)	0.051 (0.030)	0.006 (0.042)	-0.018 (0.054)	0.027 (0.053)	0.044* (0.022)	0.030 (0.019)	0.056 (0.036)
Other	0.044*** (0.014)	0.042** (0.015)	0.045** (0.019)	0.001 (0.028)	0.010 (0.029)	-0.017 (0.035)	0.052*** (0.015)	0.048*** (0.017)	0.056** (0.020)
Controls	x	x	x	x	x	x	x	x	x
N	6763	4801	4554	1168	814	766	5595	3987	3788

Notes: Estimates of the long-run ($\hat{\beta}_5$) effect of the CFC benefit on the care alternatives are shown. Dependent variable indicates attendance at care alternative. Column (1) includes all children, while Column (2) includes 1-year-old children, and column (3) includes 2-year-old children. Column (4)–(6) provides the results for low-SES children, while Column (7)–(9) provides the results for high-SES children. Control variables are the log of self-reported parental income, education level, region dummies, immigrant status, age dummies, and married/cohabiting dummy. Standard errors clustered at region–age level and shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Results by subgroup

	All (1)	Age 1 (2)	Age 2 (3)	Observations+ (4)
<i>Panel A) Socioeconomic status</i>				
Low-SES	-0.210** (0.076)	-0.275*** (0.078)	-0.143 (0.090)	1168
High-SES	-0.129*** (0.028)	-0.153*** (0.030)	-0.093** (0.043)	5595
<i>Panel B) Immigrant status</i>				
Immigrants	-0.123 (0.081)	-0.208*** (0.074)	-0.009 (0.079)	752
Non-Immigrant	-0.142*** (0.029)	-0.164*** (0.030)	-0.108** (0.046)	6011

Notes: Estimates of the long-run ($\hat{\beta}_5$) effect of the CFC benefit on formal childcare attendance are shown by subgroups. Column (1) provides estimates for all children, while Columns (2) and (3) provide estimates by child age. Column (4) includes the number of observations in each subgroup. (+) Number of observations are for the overall sample. Standard errors clustered at region-age level and shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Robustness

	Mother income	Father income	Mother att. university	Immigrant	Oslo/Akershus
	(1)	(2)	(3)	(4)	(5)
$\hat{\alpha}_4$	-4.39 (9.62)	-9.61 (8.93)	0.06* (0.03)	0.01 (0.01)	0.03 (0.05)
$\hat{\alpha}_5$	-12.75 (9.53)	-14.34 (12.71)	0.03 (0.03)	0.01 (0.01)	0.01 (0.04)
N	5585	6332	6763	6763	6763

Notes: Tests of different trends in background characteristics for the treatment and control groups are shown. The estimates ($\hat{\alpha}_4$) and ($\hat{\alpha}_5$) from equation (2) are shown. The dependent variables are the background characteristics of the parents of the children in the sample. There are some missing observations for income variables. Standard errors clustered at region–age level and shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix

Table A.1: Maternal labour market participation

Year	In percent of all mothers in group				
	Total	Age of youngest child			
		0-2	3-6	7-10	11-15
1972	43	29	41	54	55
1973	44	30	43	53	59
1974	44	31	41	51	59
1975	48	35	46	57	60
1976	53	40	48	64	67
1977	55	40	52	64	69
1978	58	42	53	67	71
1979	60	45	56	67	75
1980	62	46	57	70	75
1981	64	49	59	73	78
1982	65	49	60	72	80
1983	66	53	62	71	80
1984	68	55	64	75	79
1985	70	58	69	75	80
1986	75	65	73	78	84
1987	77	66	76	80	85
1988	77	68	74	82	85
1989	76	68	75	82	83
1990	77	69	74	82	86
1991	77	70	75	81	85

Notes: Mothers who registered as employed or unemployed (in workforce) are shown. Source: Statistics Norway:

<http://www.ssb.no/a/histstat/tabeller/9-10.html>, accessed 21 August 2016.

Table A.2: Cash-for-Care benefit (in NOK)

From	01.08.1998	01.01.1999	01.05.1999	1.8.2003	1.1.2006	1.8.2012	1.8.2014
To	01.01.1999	01.05.1999	1.8.2003	31.12.2005	31.7.2012	31.7.2014	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Child age							
13-18 months	3000	2263	3000	3657	3303	5000	6000
19-23 months	3000	2263	3000	3657	3303	3303	6000
24-35 months		2263	3000	3657	3303	0	
35-36 months		2263	3000	3657	0	0	

Notes: Source: Bakken and Myklebø (2010), Dahl and Ellingsen (2015)

Table A.3: Attendance rates administrative data and household survey

Year	Administrative*		Survey+	
	Age 1-2	Age 3-5	Age 1-2	Age 3-5
1990	0.154	0.522		
1991	0.183	0.553		
1992	0.218	0.590		
1993	0.253	0.618		
1994	0.288	0.644		
1995	0.314	0.669		
1996	0.340	0.695		
1997	0.400	0.742		
1998	0.392	0.769	0.356	0.661
1999	0.374	0.781	0.326	0.730
2000	0.375	0.795		
2001	0.380	0.815		
2002	0.409	0.840	0.335	0.766
2003	0.446	0.866		

Notes: Estimated rates of children in childcare from 1990 to 2003 are shown.
Source: Statistics Norway (2005). (*) End of year figures. (+) Spring figures

Table A.4: Childcare alternatives - short run effect

	All			Low-SES			High-SES		
	All (1)	Age 1 (2)	Age 2 (3)	All (4)	Age 1 (5)	Age 2 (6)	All (7)	Age 1 (8)	Age 2 (9)
Parental	0.084*** (0.024)	0.064** (0.026)	0.097*** (0.030)	0.099 (0.084)	0.004 (0.074)	0.208* (0.109)	0.082*** (0.020)	0.080*** (0.023)	0.074*** (0.026)
Relatives	-0.018 (0.021)	0.004 (0.026)	-0.042* (0.022)	0.052 (0.060)	0.133** (0.052)	-0.060 (0.072)	-0.032* (0.018)	-0.024 (0.026)	-0.038** (0.018)
Nannies	0.023 (0.023)	0.037 (0.032)	0.009 (0.021)	-0.001 (0.045)	0.014 (0.059)	-0.016 (0.058)	0.031 (0.025)	0.046 (0.036)	0.016 (0.019)
Other	0.027* (0.014)	0.032** (0.015)	0.022 (0.020)	-0.008 (0.036)	0.021 (0.035)	-0.048 (0.047)	0.033** (0.013)	0.032* (0.016)	0.036* (0.018)
Controls	x	x	x	x	x	x	x	x	x
N	6763	4801	4554	1168	814	766	5595	3987	3788

Notes: Estimates of the short-run ($\hat{\beta}_4$) effect of the CFC benefit on the care alternatives are shown. The dependent variable indicates attendance at care alternative. Column (1) includes all treated children, while Column (2) includes only 1-year-old children and Column (3) includes only 2-year-old children. Columns (4)–(6) provide the results for low-SES children, while Columns (7)–(9) are the results for high-SES children. Control variables are the log of self-reported parental income, education level, region dummies, immigrant status, age dummies, and married/cohabiting dummy. Standard errors clustered at region–age level and shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: Effect of CFC on older siblings

<i>Dep. var: Care alternative</i>					
	Formal childcare	Parental care	Relatives	Nannies	Other
	(1)	(2)	(3)	(5)	(6)
Short-run effect ($\hat{\beta}_4$)	-0.087*** (0.030)	0.088*** (0.022)	-0.076*** (0.023)	-0.032 (0.021)	0.029 (0.019)
Long-run effect ($\hat{\beta}_5$)	-0.038 (0.031)	0.057* (0.027)	-0.080*** (0.022)	-0.025 (0.021)	0.008 (0.020)
Controls	x	x	x	x	x
N	4578	4578	4578	4578	4578

Notes: The table shows estimates of the short-run ($\hat{\beta}_4$) and long-run ($\hat{\beta}_5$) effect of the CFC benefit on care alternative of older ineligible siblings of eligible children. The dependent variable is an indicator for care type attendance. The treatment group are now children aged 3-5 with sibling aged 1-2 before and after the reform, while the control group are children aged 3-5 without siblings aged 1-2 before and after the reform. Control variables are self reported log parental income, education level, region dummies, immigrant status, age dummies and married/cohabiting dummy. Standard errors clustered at region-age level and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table A.6: Sample selection

Percentage (%)	All females		Immigrant females	
	Official stats	Sample	Official stats	Sample
Education level				
Lower secondary school	21.4	5.51	30.8	9.34
Upper secondary school	42.1	54.70	31.2	33.15
University	36.5	37.51	38	24.36
Missing	0	2.28	0	33.15

Notes: Education levels of the sampled mothers are compared with those from the official statistics for women aged 30–34 years in 2000. The average age of the mothers in the sample is 32.7 years.

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2015

- 01/15** January, Antonio Mele, **Krisztina Molnár**, and Sergio Santoro, "On the perils of stabilizing prices when agents are learning".
- 02/15** March, **Liam Brunt**, "Weather shocks and English wheat yields, 1690-1871".
- 03/15** March, **Kjetil Bjorvatn**, **Alexander W. Cappelen**, Linda Helgesson Sekei, **Erik Ø. Sørensen**, and **Bertil Tungodden**, "Teaching through television: Experimental evidence on entrepreneurship education in Tanzania".
- 04/15** March, **Kurt R. Brekke**, **Chiara Canta**, Odd Rune Straume, "Reference pricing with endogenous generic entry".
- 05/15** March, Richard Gilbert and **Eirik Gaard Kristiansen**, "Licensing and Innovation with Imperfect Contract Enforcement".
- 06/15** March, **Liam Brunt** and Edmund Cannon, "Variations in the price and quality of English grain, 1750-1914: quantitative evidence and empirical implications".
- 07/15** April, Jari Ojala and **Stig Tenold**, "Sharing Mare Nostrum: An analysis of Mediterranean maritime history articles in English-language journals".
- 08/15** April, **Bjørn L. Basberg**, "Keynes, Trouton and the Hector Whaling Company. A personal and professional relationship".
- 09/15** April, Nils G. May and **Øivind A. Nilsen**, "The Local Economic Impact of Wind Power Deployment".
- 10/15** May, **Ragnhild Balsvik** and Stefanie Haller, "Ownership change and its implications for the match between the plant and its workers".
- 11/15** June, **Kurt R. Brekke**, **Chiara Canta**, Odd Rune Straume, "Does Reference Pricing Drive Out Generic Competition in Pharmaceutical Markets? Evidence from a Policy Reform".
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- 13/15** June, **Bjørn L. Basberg**, "Commercial and Economic Aspects of Antarctic Exploration - From the Earliest Discoveries into the 19th Century".

- 14/15** June, **Astrid Kunze** and Amalia R. Miller, "Women Helping Women? Evidence from Private Sector Data on Workplace Hierarchies"
- 15/15** July, **Kurt R. Brekke**, Tor Helge Holmås, Karin Monstad, Odd Rune Straume, «Do Treatment Decisions Depend on Physicians' Financial Incentives?"
- 16/15** July, **Ola Honningdal Grytten**, "Norwegian GDP by industry 1830-1930".
- 17/15** August, **Alexander W. Cappelen**, Roland I. Luttens, **Erik Ø. Sørensen**, and **Bertil Tungodden**, «Fairness in bankruptcy situations: an experimental study».
- 18/15** August, **Ingvild Almås**, **Alexander W. Cappelen**, **Erik Ø. Sørensen**, and **Bertil Tungodden**, "Fairness and the Development of Inequality Acceptance".
- 19/15** August, **Alexander W. Cappelen**, Tom Eichele, Kenneth Hugdah, Karsten Specht, **Erik Ø. Sørensen**, and **Bertil Tungodden**, "Equity theory and fair inequality: a neuroeconomic study".
- 20/15** August, Frank Jensen and **Linda Nøstbakken**, «A Corporate-Crime Perspective on Fisheries: Liability Rules and Non-Compliance".
- 21/15** August, Itziar Lazkano and **Linda Nøstbakken**, "Quota Enforcement and Capital Investment in Natural Resource Industries".
- 22/15** October, **Ole-Petter Moe Hansen** and Stefan Legge, "Trading off Welfare and Immigration in Europe".
- 23/15** October, Pedro Carneiro, Italo Lopez Garcia, **Kjell G. Salvanes**, and Emma Tominey, "Intergenerational Mobility and the Timing of Parental Income".
- 24/15** October, David Figlio, Krzysztof Karbownik, and **Kjell G. Salvanes**, "Education Research and Administrative Data".
- 25/15** October, **Ingvild Almås**, **Alexander W. Cappelen**, **Kjell G. Salvanes**, **Erik Ø. Sørensen**, and **Bertil Tungodden**: «Fairness and family background».
- 26/15** November, Lars Ivar Oppedal Berge, **Kjetil Bjorvatn**, Simon Galle, Edward Miguel, Daniel Posner, **Bertil Tungodden**, and Kelly Zhang "How Strong are Ethnic Preferences?".
- 27/15** November, **Agnar Sandmo**, "The Public Economics of Climate Change".
- 28/15** November, **Aline Bütikofer** and **Kjell G. Salvanes**, "Disease Control and Inequality Reduction: Evidence from a Tuberculosis Testing and Vaccination Campaign".

29/15 December, **Aline Bütikofer**, Katrine V. Løken and **Kjell G. Salvanes**, “Long-Term Consequences of Access to Well-child Visits”

30/15 December, **Roger Bivand**, “Revisiting the Boston data set (Harrison and Rubinfeld, 1978): a case study in the challenges of system articulation”.

2016

01/16 January, **Ingvild Almås** and Anders Kjelsrud, “Pro-poor price trends and inequality | the case of India”.

02/16 January, Tuomas Pekkarinen, **Kjell G. Salvanes**, and Matti Sarvimäki, «The evolution of social mobility: Norway over the 20th century”

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- 13/16** September, **Elias Braunfels**, “Further Unbundling Institutions”
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- 15/16** October, **Aline Bütikofer**, Eirin Mølland, and **Kjell G. Salvanes**, “Childhood Nutrition and Labor Market Outcomes: Evidence from a School Breakfast Program”
- 16/16** October, **Itziar Lazkano** and Linh Pham, “Do Fossil-Fuel Taxes Promote Innovation in Renewable Electricity Generation?”
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- 19/16** December, **Gernot Doppelhofer, Ole-Petter Moe Hansen**, Melwyn Weeks, “Determinants of long-term economic growth redux: A Measurement Error Model Averaging (MEMA) approach “
- 20/16** December, Leroy Andersland and **Øivind A. Nilsen**, “Households’ responses to price changes of formal childcare”



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