Discussion Papers
Statistics Norway
Research department
No. 800
February 2015

Statistics Norway

Statistisk sentralbyrå

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Abstract:

This paper assesses the relationship between cash transfers to families and subsequent childbearing. We take advantage of a cash-for-care (CFC) policy introduced in Norway in 1998, and compare the fertility behaviour of eligible and ineligible mothers over a four year period. Contrary to theoretical expectations, the results show that CFC eligible mothers had a *slower* progression to both second and third births, and short term fertility is hence lower in this group. The patterns differ somewhat between different groups of mothers, and there seems to be a polarisation between non-employed mothers and mothers without upper secondary education, on one hand, and employed mothers and mothers with upper secondary and higher education, on the other. We suggest that this pattern may be driven by an interaction between the CFC benefit and the Norwegian parental leave scheme.

Keywords: Fertility, Family policy, Cash for care

JEL classification: J10, J13, J18

Acknowledgements: We thank Anna Matysiak, Kjetil Telle and Marit Rønsen for helpful comments during the writing process. Financial support from the Norwegian Research Council [grant number 217915] is gratefully acknowledged.

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ISSN 1892-753X (electronic)

Sammendrag

Sammenhengen mellom familiepolitikk og fruktbarhet er et sentralt samfunnsvitenskapelig og politisk spørsmål, og hvorvidt det kan sies å være en årsakssammenheng mellom politikk og atferd er stadig omdiskutert. I denne studien ser vi nærmere på betydningen av generelle familiepolitiske ordninger for fruktbarhetsatferd, ved å undersøke hvorvidt innføringen av kontantstøtten i 1998 førte til endringer i forekomsten og timingen av videre barnefødsler blant norske mødre. Kontantstøtten medførte en reduksjon i «kostnaden» ved å få barn, og vi forventer derfor å se en økning i andelen mødre som får et andre eller tredje barn blant dem som omfattes av ordningen. Vi benytter registerdata over alle gifte eller samboende foreldre som fikk sitt første felles barn i 1998 og 1994, hvor 1998-kohorten utgjør den «eksponerte» kohorten med rett til 24 måneders kontantstøtte og 1994-kohorten utgjør sammenligningsgruppe uten rett til kontantstøtte. Videre barnefødsler registreres fram til barnet født i 1994 eller 1998 fyller fire år, og forskjeller mellom de to kohortene kontrolleres for ved et rikt utvalg kontrollvariabler.

I motsetning til de teoretiske forventningene indikerer analysen at innføringen førte til en *reduksjon* i andelen nye barnefødsler i løpet av en fireårsperiode, og kontrollert for sosiodemografiske kjennetegn er denne på 5.9 prosent for andre barns fødsel og 9.1 prosent for tredje barns fødsel. Dette mønsteret varierer imidlertid med mors utdanningsnivå og tilknytning til arbeidsmarkedet, og mens den ovennevnte nedgangen gjelder for mødre med videregående og høyere utdanning, samt deltid- og heltidsjobb, ser vi ingen statistisk signifikant endring blant mødre med lav utdanning og mødre uten tilknytning til arbeidslivet. Vi anser det som sannsynlig at denne polariseringen kan knytte seg til samspillet mellom kontantstøtten og den allerede etablerte foreldrepermisjonsordningen, hvor kontantstøtten kan fungere som en forlengelse av permisjonsperioden blant sysselsatte mødre slik at både returen til arbeidslivet og videre barnefødsler utsettes i denne gruppen.

1 Introduction

The relationship between family policy and childbearing is of great interest to researchers and policy-makers alike. There is a concern in all modern societies about the dramatic changes in family dynamics, and in fertility levels in particular, that have taken place during the last forty to fifty years. Family policy has become an important aspect of this discussion. For instance, the Nordic countries are known to have both high levels of fertility and high female employment rates, and this is often ascribed to generous social policies directed at families. There is little evidence, however, to indicate whether introducing various policies can actually change people's fertility behaviour and whether there is a causal relationship between family policy and fertility.

Korpi (2000) argues that family policies can be divided into two main groups: general family policies directed at nuclear families, and dual-earner policies that focus on the mother's participation in the labour market and the father's participation in child care. Both groups of policies can affect continued childbearing by altering the direct and indirect costs of having another child (cf. Björklund, 2007; Gustafsson, 2001; Walker, 1995). In this article, we aim to approach the possible causal effect of introducing a general family policy – here represented by a cash benefit policy – on future childbearing. We make use of what is called the cash-for-care (CFC) benefit introduced in Norway in 1998, which entitled all parents of one and two-year old children who were not enrolled in public child care to a monthly payment equivalent to the public subsidy for a day care place (approx. EUR 360[1]). As cash benefits can be seen as a direct compensation for the cost of having children, our main expectation is that short-term[2] fertility increased after the introduction. However, as the benefit is a fixed amount per child, we expect to see different adaptions depending on the mother's human capital investments and opportunity costs of childbearing. For instance, mothers with low education may find the cash benefit an attractive alternative to employment, which consequently makes them more likely to withdraw from the labour market and care for the child than mothers who are more highly educated.

Previous studies have indeed shown a positive correlation between uptake of the CFC benefit and subsequent fertility *timing*, but this varies somewhat between different groups of mothers (Aassve and Lappegård, 2009; 2010). However, these studies cannot distinguish between possible effects of the

[1] Converted from Norwegian kroner to euros on 9 Jan. 2015. This applies to all monetary amounts mentioned throughout the article.

[2] Due to reasons that are explained more thoroughly in Chapter 3, we only measure subsequent childbearing for up to four years. We are therefore unable to draw any conclusions about completed fertility levels, and short-term tempo and quantum effects therefore remain the focus of this article.

policy reform itself and the fact that mothers taking advantage of the CFC benefit might be more inclined to have more children. The distinction between causal effects and selection effects is crucial if we wish to say something about the potential of family polices to change fertility behaviour. We therefore choose a slightly different approach and use the introduction – not the uptake – of the CFC policy as an exogenous variation in benefit eligibility to perform an 'intention-to-treat' (ITT) analysis (see, e.g., Angrist and Pischke, 2009:163). We compare the subsequent fertility behaviour of mothers who are eligible for the cash benefit for their first or second child (i.e. the treatment group whose child was born in 1998) to that of mothers whose child remains unaffected by the policy reform (i.e. the comparison group with children born before the introduction, in 1994). We are unable to apply a strictly quasi-experimental design, and we account for possible trends in fertility behaviour between 1994 and 1998 by including a rich set of covariates from Norwegian administrative registries.

Contrary to previous studies and our theoretical expectations, which suggest that the introduction of the CFC benefit should lead to faster parity progression and hence to higher short-term fertility, our results show an overall pattern of *slower* parity progression and hence *lower* short-term fertility. In sum, our results may therefore suggest that the results of previous studies might be driven by selection effects. Moreover, we see that the changes in fertility behaviour differ between different groups of mothers. There is a polarisation between non-employed mothers and mothers without upper secondary education, on the one hand, and employed mothers and mothers with upper secondary school and higher education, on the other. Moreover, while parity progression accelerates after the reform in the first group, it slows down in the latter. We suggest that this pattern may be mediated by the already established paid parental leave scheme and believe that the somewhat surprising results may be a good example of how one isolated policy can have unexpected consequences when implemented in a wider policy framework.

2 Theoretical expectations

Following classical economic theory (e.g. Becker, 1960, 1981; Easterlin, 1975), children can be regarded as a normal good analogous to other goods and services providing satisfaction (i.e. utility) to a family. The decision to have another child will hence depend on the cost or shadow price of childbearing, which, according to Walker (1995), can be said to comprise three components. Firstly, it comprises direct costs of, e.g., food, clothes and housing. Secondly, it comprises what is commonly referred to as the opportunity cost of childbearing, namely the loss of income from paid work while

the parent (most often the mother[3]) withdraws from the labour market and cares for the child. Thirdly, it comprises the loss of (or a lack of growth in) human capital investments while the mother is absent from the labour market (c.f. Björklund, 2007; Gustafsson, 2001; Walker, 1995). The latter two components are often referred to as the indirect costs of childbearing, and their size will depend on the expected levels of return from both income and human capital had the mother not withdrawn from the labour market (see Walker, 1995:237).

The potential impact of family policy on fertility behaviour can be outlined by assessing the impact of various policies on these three cost components (see Björklund, 2007). Cash transfers to families can affect fertility decisions by altering the *direct* cost of childbearing, and parental leave benefits (which compensate for lost income) by altering the *indirect* opportunity cost (see Walker, 1995:237). As the benefit we consider here is a universal benefit that is subject to some conditions (see Section 2.1 below), it may affect childbearing through different components, depending on the pre-birth labour market attachment of the mother in a given family. Firstly, the benefit would alter the direct costs of childbearing in families where the mother is non-employed and can provide care for the child without any opportunity cost, as well as in families where the child can be cared for (free of charge) by, e.g., relatives, while both parents maintain their pre-birth labour market attachment. In these families, the benefit would induce a pure income effect, as it would add to the family's existing disposable income. Secondly, the benefit would alter the indirect costs of childbearing in families where the mother temporarily exits the labour market to care for the child, as it provides (limited) compensation for lost income and hence a reduction in her opportunity cost. In all these families, the introduction of the benefit would lower the shadow price of childbearing, and, all other things being equal, we therefore expect to see an increase in the number of children being born to eligible mothers.

In addition to the pre-birth labour market attachment of the mother, her educational attainment will likely also affect her indirect childbearing costs by determining her investment in human capital and expected return on this investment. We might therefore expect the response to the subsidy to be less strong among highly educated mothers compared to mothers with less education. To examine variation in policy adaptions between families, we will perform two subsample analyses based on the mother's educational level and labour market attachment prior to birth. Both these characteristics can proxy her current income level and human capital investments, as well as their potential rates of return in the

[3] As mothers take the lion's share of parental leave in Norway and are the primary caregiver during a child's first year (Lappegård, 2012), we will primarily refer to her behaviour surrounding childbirth. The arguments could apply to fathers as well, however.

future (see, e.g., Aakvik, Salvanes and Vaage, 2010). Moreover, the mother's labour market attachment could be a proxy for her family/work orientation as well as her entitlement to paid parental leave (see below).

It is important to note that this theoretical framework primarily addresses completed fertility, while we address *short-term* changes in fertility tempo and quantum. It has been thoroughly demonstrated in the demographic literature that there is no inevitable accordance between tempo and quantum effects,[4] but we anticipate that this will be the case in our analysis as we apply a follow-up period of just four years. Though unable to account for changes in completed fertility, we believe our results can provide a good indication of what these may be, since most new births occur within a four-year time frame (Lappegård, 2000).

2.1 The Cash-for-Care benefit

The Norwegian Cash-for-Care benefit was introduced by a minority centre-coalition government in 1998. The proposal was first presented by the Ministry of Children and Family Affairs in the Council of State on 30 April 1998, and the law on cash grants was sanctioned on 26 June 1998. The law took effect on 1 August 1998 (Stortinget, 2014). From this date, the law encompassed all one-year-old children, and it was expanded to also include two-year-olds from 1 January 1999. All children who turned two between August 1998 and January 1999 were eligible for the cash transfer also in this period in order to ensure that no children had a break in eligibility. The CFC benefit consisted of a monthly flat (and tax exempt) rate of NOK 3300 (= EUR 360), and the only requirement was that the child did not attend publicly subsidised child care.[5] Apart from this, the benefit was flexible in several respects; it could be claimed on a part-time basis[6] if the child attended a publicly subsidised child care centre for between 1 and 32 hours per week, and it was not a requirement that one of the parents stayed at home to provide care for their child, since the benefit could be spent on private care arrangements, such as childminders, friends or relatives. Eligibility started the month after the child turned one year old, and lasted until the month the child turned three (i.e. from 13 to 36 months). Hence, all children born from 1998 and onwards were eligible for 24 months of the benefit. Children

[4] See, e.g., Aassve and Lappegård (2010) for a demonstration of how the postponement of first birth among highly educated mothers not necessarily leads to lower completed fertility (c.f. Kreynfeld, 2002).

[5] Both public and private formal child care in Norway are subsidised as long as the centres meet certain structural requirements, such as size of play area, number of adults per child and number of teachers per child. The subsidy is substantial, and most centres meet the requirements for financial support.

[6] Five rates were used: 20 percent (1-8 hours), 40 percent (9-16 hours), 60 percent (17-24 hours), 80 percent (25-32 hours) and 100 percent (>32 hours).

born before 1996 remained unaffected by the benefit, while children born between 1996 and 1997 were eligible for between 1 and 23 months of benefit (see Drange, 2012:139).

The political motivation behind the introduction of the CFC benefit was threefold: 1) to provide parents with more time to take care of their children, 2) to improve freedom of choice regarding care practices within families, and 3) to ensure equality in public transfers between families irrespective of their care practices (the Act on Cash Grants to Families with small Children of 1998). The policy gave rise to considerable political debate, and, while those in favour saluted it for improving families' 'real freedom of choice' regarding care practices, its critics argued that the reform reduced incentives for women to participate in the labour market and therefore encouraged a more traditionally gender-differentiated family (Ellingsæter and Leira, 2006). Even though the policy is presumed to be gender-neutral, mothers are the most frequent recipients (NAV, 2013), and numerous studies have reported negative effects on mothers' labour market supply (Hardoy and Schøne, 2010; Rønsen, 2009; Schøne, 2004; Drange and Rege, 2013). In terms of fertility behaviour, Aassve and Lappegård (2009, 2010) show that those who utilise the CFC benefit accelerate childbearing significantly compared to those who do not. However, it seems likely that mothers who utilise a cash transfer (and thereby decline a full-time place in child care) are qualitatively different from mothers who do not. Hence the association between benefit uptake and fertility might be explained, in whole or in part, by selection. We account for such selection by exploring consequences of the *introduction* of the CFC policy rather than its uptake.

In the period before the introduction of the CFC policy in 1998, and hence of potential influence on the fertility decisions in the groups studied in our analysis, two main changes were implemented in the paid parental leave scheme – the very core of Norwegian family policy. Firstly, there was a large step-wise extension of the paid parental leave period, from 18 weeks in 1986 to 42 weeks in 1993. The scheme entitled (in the period we consider here) all Norwegian parents who had worked at least 50% for six out of the ten months before the child was born to up to one year[7] of paid parental leave, after which a return to the same job was guaranteed by Norwegian labour market legislation. After this period – when the child became eligible for the CFC benefit – the parents were entitled to one year of unpaid leave with the same labour market protection as before. During the 1990s, the father's

[7] The leave could then be taken for 42 weeks with 100 percent income coverage or 52 weeks with 80 percent income coverage, with 52 weeks being the most popular option.

eligibility for paid leave depended on the mother's eligibility.[8] Mothers who were not entitled to leave received a lump sum, which was considerably lower than the parental leave payments. One feature of the leave scheme that is particularly important to keep in mind when considering the CFC policy is that full eligibility requires employment prior to birth. This means that mothers who wished full income replacement for subsequent children had to return to work before having another child, unless they decided to have two very closely spaced children[9] or accepted the low lump sum. The latter option would, however, lead to a large decrease in disposable income for most families. The uptake of this extended leave was relatively immediate, and we might therefore expect that both the uptake and the duration of paid parental leave to be similar for both the comparison group (1994) and the treatment group (1998) in the analysis.

The second change was the introduction of the father's quota for paid parental leave in 1993. This reserved four weeks of the parental leave period exclusively for the father, but, contrary to the extension of the leave, the uptake of this father's quota was more gradual. This means that those who had children in 1998 had a higher uptake of the father's quota than those who had children in 1994, and, if this in turn affects subsequent fertility behaviour, our estimates may be biased either downwards or upwards, depending on the direction of this effect. However, studies of the father's quota find no significant causal effect of the introduction of the father's quota on continued childbearing (Cools, Fiva and Kirkebøen, 2011; Duvander, Lappegård and Johansson, 2013). We do therefore not consider this to be a likely confounder in this analysis.

2.2 Hypotheses

We start off with a general hypothesis of increased (short-term) fertility following the introduction of the CFC benefit. Due to the fixed size of the cash transfer, we expect to see more substantial changes among mothers with low indirect costs of childbearing, and less – or no – change among mothers with higher costs.

In addition to the economic incentives resulting from the CFC benefit, we see it as useful to consider the possible spill-over from the incentives already built into the complexities of existing Norwegian

[8] This was changed in 2000 and fathers can now take leave independent of mothers' right, except for their use of the father's quota (see below), which still depends on mothers' eligibility.

[9] For those choosing one year of leave, having two children with less than 16 months spacing entitles the mother to parental leave benefit for the second child based on the parent leave benefit for the first (i.e. 80 per cent of 80 per cent of income before the first birth). This is because parental leave benefit is regarded as income from work.

family policies, where the paid parental leave scheme is of particular importance. As already described in more detail, this gives employed parents the opportunity to stay out of employment for one year on paid leave and another year on unpaid leave. If we assume that a mother uses all the paid leave she is entitled to, she can claim CFC benefit and stay at home with a child for a total of two years before she needs to return to her job. Furthermore, the fact that paid parental leave depends on employment before (a new) birth means that the mother must return to work before giving birth to another child in order to again become eligible for paid leave. With these two considerations in mind, we can outline two main scenarios for how the introduction of the CFC policy – in interaction with the already established paid parental leave scheme – may have affected fertility behaviour among *employed* mothers.

Firstly, the CFC benefit might be used as an alternative source of income for up to four years if the mother has her next child within two years (and for up to six years if she has yet another child within four years). This might be a plausible adaption among mothers with low indirect costs of childbearing, and some, although weak, attachment to the labour market. In this scenario, we might see an acceleration in the transition to a new birth, and hence also an increase in short-term fertility.

In a second scenario, the CFC benefit might be used as an extension of the paid parental leave period, which is then followed by a (delayed) return to the labour market in order to regain full eligibility for paid parental leave for future children, as well as to maintain one's foothold in the labour market. Delayed return to the labour market could, in turn, delay the next birth, and in this scenario we expect to see a slower transition to a new birth and a decrease in short-term fertility. We see this as a plausible adaptation among mothers with moderate indirect costs of childbearing.

Based on these considerations, we outline two main hypotheses:

The acceleration hypothesis: The introduction of the CFC benefit decreased the cost of childbearing, which, in turn, increased the demand for children. All other things being equal, the introduction of the CFC benefit therefore speeded up subsequent childbearing and increased short-term fertility. We expect this pattern to be evident among three groups of mothers: 1) mothers with no attachment to the labour market, who experienced a pure income effect of the CFC benefit and therefore have more children, 2) mothers without upper secondary education whose indirect costs of childbearing are low, and 3) part-time employed mothers who accelerate their childbearing to use the CFC benefit as an alternative source of income while completing (or exceeding) their fertility intentions.

The postponement hypothesis: The introduction of the CFC policy created a possibility of compensation for lost income while the mother extended her leave period, which, in turn, delayed her labour market return, as well as her subsequent childbearing. This led to a postponed transition to subsequent childbearing and lower short-term fertility, which – due to relatively low indirect costs of childbearing – was most evident among two groups of mothers; 1) part-time employed mothers and 2) mothers with upper secondary (but not higher) education.

In sum, we expect three patterns to be evident: 1) an *increase* in short-term fertility among mothers with no or part-time employment and mothers without upper secondary education, 2) a *decrease* in short-term fertility among mothers with upper secondary education and mothers in part-time employment, and 3) *no change* among mothers with higher education and mothers in full-time employment. This entails that our expectations for part-time employed mothers not are unanimous, and suggest two different outcomes depending on which mechanism that is at work.[10]

3 Analytical strategy

Ideally, we would like to use a quasi-experimental design to identify the *causal* effect on fertility behaviour of cash transfers to families, but the universal nature and gradual introduction of the CFC benefit hindered both random allocation to the treatment and comparison groups based on date of birth (i.e. it violates the identifying assumption of a regression discontinuity analysis) *and* the possibility of comparing changes in fertility behaviour from a pre to a post period in one group of eligible and one group of ineligible mothers (i.e. a difference-in-differences design). We therefore relax our causal ambitions and rely on a rich set of covariates to account for differences between the eligible and ineligible mothers.

3.1 Treatment and comparison groups

The CFC policy created exogenous variation in the framework conditions facing parents of young children before and after its introduction. We take advantage of this change, and compare the fertility behaviour of mothers eligible for the benefit to the fertility behaviour of mothers ineligible for the benefit. Our treatment group comprises mothers who gave birth to their first or second child in 1998, since children born in 1998 are the first 'fully treated' cohort that was eligible for 24 months of CFC

[10] Note that findings in line with these patterns rest on the assumption that a wish for another child (a child the mother would not have had were if not for the subsidy) has short-term tempo and quantum effects.

benefit. The universal nature of the CFC policy meant that there were challenges finding a suitable comparison group for this analysis, and we had to go back in time to find one. Thus, for the comparison group we use mothers who gave birth to their first or second child in 1994. The reason for choosing this cohort is twofold. Firstly, we needed a cohort whose fertility behaviour we could monitor for some time without them entering the post-introduction period (and hence CFC 'treatment'). A large proportion of new births occur within four years, and using the 1994 cohort allowed us to follow mothers for four years after the birth of their first/second child (i.e. until 30.12.1998 for children born 31.12.1994). It is worth noting that some of the children subsequently born to mothers in the comparison group may be either partially or fully eligible for the CFC benefit (i.e. those born in 1996/1997, and 1998, respectively), but that does not challenge our empirical strategy because the *decision* to have these children was unaffected by the introduction of the CFC benefit.[11] Secondly, we needed to pay particular attention to relevant policy reforms or social, political or economic 'shocks' that might affect subsequent childbearing in the two cohorts differently. Because of the extension of parental leave in 1990 and the introduction of the fathers' quota in 1993, we chose to avoid this period when constructing our analysis. This ensures that we compare families facing similar framework conditions, but with the important distinction that mothers in the 1998 cohort are eligible for the benefit, whereas mothers in the 1994 cohort are not.

Two main challenges remain to this strategy. Firstly, a key question is whether it was possible for the treatment group to strategically time the birth of their child so that it took place in the eligible period, as this would violate the assumption that the treatment and the comparison groups are close to similar – apart from the 'random' eligibility of the 1998 cohort. Keeping in mind, from Section 2.1, that the final decision on the implementation of the subsidy was made in June 1998, the timing of a birth in 1998 should not be a major concern. Moreover, Drange and Rege (2013) find no evidence that birth patterns in 1998 differ from patterns in 1997 and 1996. Secondly, since all mothers became eligible at the same time, we do not have a group of non-eligible mothers after the policy change to help us to control for general trends. We will rely on a large set of covariates measured before eligibility to ensure that observable characteristics are not themselves a product of the treatment.[12] To account for economic trends resulting in differences in labour market conditions facing the different groups of mothers, we will rely on municipal unemployment rates. Although we account for economic trends

[11] See Drange and Rege (2013).

[12] Most covariates are measured the year before the child is born, i.e. in 1993 for the pre-reform cohort and in 1997 for the post-reform cohort.

and observable differences between mothers as best as possible, the question of comparability still remains our most prominent challenge.

3.2 Data and sample

We base all analyses on data from Norwegian population registers covering the time period between 1993 and 2002. The data set comprises demographic information on all married or cohabiting parents who had their first or second common child in either 1994 or 1998, given that the child was born in Norway to Norwegian-born parents or immigrant parents with permanent residence permits. We exclude parents who experience multiple births and births occurring with less than nine months spacing, as we consider these to be special cases. We then record any subsequent birth occurring before the child born in 1994 or 1998 turns four (i.e. for up to 47 months), and register the spacing (in months) between these births. These demographic data on birth histories are merged with other socio-demographic information, such as income, education and union status, which is retrieved from other administrative registers.

The covariates are included to control for any differences in birth occurrence and timing between our treatment and comparison groups that can be explained by other observable factors than the introduction of the CFC benefit. To avoid the covariates being endogenous to the reform, we mainly take them from the year before the child in the treatment and control group was born (i.e. in 1997 and 1993, respectively).

The parents' ***age at birth*** of the treatment or comparison child is included as a continuous variable, with the polynomial ***age2*** for each parent. These variables are included because the samples differ in terms of the age at which they had their first or second child.

The ***union status*** at the birth of the treatment or comparison child is captured as a dummy variable, labelled 1 if the parents were cohabiting and 0 if they were married. As already mentioned, those who were not living together the year they became parents are not included in the analysis.

The highest completed ***educational level*** the year before the birth of the treatment or comparison child is included as two dummy variables for each parent; *Upper secondary* indicates whether the parent has completed upper secondary education (labelled 1) or not (labelled 0), while *Higher education* indicates whether the parent has completed higher education (labelled 1) or not (labelled 0). Moreover, we include the variable *Student,* labelled 1 if the mother had more than 5 months' (i.e. 50 percent) student workload in the year before giving birth (and 0 otherwise).

The joint ***family income*** from paid work (i.e. the sum of both the mother's and the father's earnings) the year before the birth is measured as a continuous variable with the polynomial ***inc2***. This sum is inflation-adjusted by dividing it by the basic amount thresholds in the Norwegian National Insurance scheme, which is updated each year to equal the national wage growth. [13] It should be noted that this income measure does not include social transfers and hence cannot be seen as a measure of total spending power. We do, however, expect earnings to be a rather good proxy for both spending power as well as labour market attachment, given that social transfers would add rather equally to the total income for everyone except those with the absolute lowest earnings, for whom social transfers are crucial.

The mother's relative ***contribution to the family income*** is measured as the proportion of her earnings relative to the joint earnings of both parents. This is included as a continuous variable aimed at capturing the pre-birth specialisation within the couple.

The mother's ***labour market attachment*** the year before giving birth is determined by comparing her wages to the basic amount thresholds in the Norwegian National Insurance scheme, which were mentioned earlier. We consider mothers whose labour market income was less than half the basic amount to be *non-employed.* Hence, we allow for some labour market activity, but consider it to be negligible. We then distinguish between part-time and full-time employed mothers, where those with earnings of more than half but less than four times the basic amount are considered to be *part time employed*, while those who earn four times the basic amount and above are considered to be *full-time employed* (see Havnes and Mogstad (2011) for similar definitions).

The ***immigrant background*** of the parents is included as a dummy variable labelled 1 if both parents are born abroad and 0 if at least one parent is born in Norway.

A final variable captures the ***unemployment rate*** in the municipality the year before birth. This variable is included to account for economic trends occurring between the two periods.

Our subsamples are described in relation to these demographic variables in Table 1 below.

Starting with the first line in Table 1 and the information on births, we see that around 63 per cent of one-child mothers proceeded to have a second child during the follow-up period, while about 23 per cent of the two-child mothers had a third child. These numbers follow a general pattern of more

[13] One basic amount equalled about EUR 4000 EUR (NOK 37 300) in 1993 and EUR 4600 (NOK 42 500) in 1997.

mothers proceeding to a second birth than a third birth (e.g. Lappegård, 2000). We note that the 1998-cohort had a somewhat lower likelihood of having another child during the follow-up period: a 1.1 percentage point reduction for one-child parents and a 0.98 percentage point reduction for two-child parents.

Table 1: Descriptive statistics, by parity and birth year of child. Percentages unless otherwise specified

	One child		Two children	
	1994	**1998**	**1994**	**1998**
Had another child during f.-up	63.34	62.24	23.67	22.69
Mother's age[a] (mean(st.d.))	26.30 (4.3)	27.03 (4.4)	28.95 (4.1)	29.64 (4.1)
Father's age[a] (mean(st.d.))	28.84 (4.8)	29.57 (5.0)	31.55 (4.7)	32.13 (4.7)
Union status[a]				
Married	41.13	39.06	67.84	61.04
Cohabiting	58.87	60.94	32.16	38.96
Mother's educational level[b]				
Without upper secondary education	10.62	8.74	9.95	7.58
With upper secondary education	59.49	53.73	60.32	54.99
With higer education	29.90	37.53	29.73	37.43
Father's educational level[b]				
Without upper secondary education	9.45	9.62	11.5	9.14
With upper secondary education	62.34	58.69	61.27	59.29
With higer education	26.20	31.42	27.22	31.57
Mother's immigrant background				
Non-immigrant	90.48	88.08	90.88	89.71
Immigrant	9.52	11.92	9.12	10.29
Father's immigrant background				
Non-immigrant	90.26	88.49	90.62	89.59
Immigrant	9.74	11.51	9.38	10.41
Family income (inflation adjusted) [b] (mean(st.d.))	8.44 (4.7)	9.09 (5.8)	9.00 (4.4)	9.62 (7.6)
Mother's contribution to family income (mean (st.d.))	41.67 (22.7)	40.77 (21.4)	36.58 (20.7)	36.43 (19.6)
Mother's labour market attachment[b]				
No work	29.88	29.91	19.59	18.18
Part time	24.19	20.39	42.29	38.75
Full time	45.93	49.70	38.12	43.07
Mother >50% student[b]	22.42	22.25	8.27	8.46
Mean unemployment rate[b]				
N	**18566**	**18208**	**16948**	**16128**

[a] Measured at the birth of the treatment or comparison child.
[b] Measured the year before birth of the treatment or comparison child.

Moving on to the socio-demographic variables, we see that one-child and two-child parents differ somewhat in their socio-demographic composition, depending on whether the child is born in 1994 or 1998. It is therefore important to control for these characteristics in a regression model, as the apparent differences in subsequent births (and their timing) described above may be due to the relative composition of the subsamples rather than to eligibility for the CFC benefit.

Starting with one-child parents, we see that the mean age at birth is slightly higher in the 1998 cohort, for both mothers and fathers. There is an increase of about 2 percentage points in the proportion of cohabiting couples, and a corresponding decrease in the proportion of married couples. Moreover, a

higher proportion of both mothers and fathers have completed higher education in the 1998 cohort, which is possibly related to general trends in education during the period (see Statistics Norway (2014)). There are slightly fewer Norwegian-born parents in the 1998-cohort. The inflation-adjusted joint family income is higher as well, while the mothers' relative contribution to the family income has decreased from 41.7 to 40.8 percent. We do, however, see that slightly more women work full-time, while fewer work part-time. The decrease in the mothers' contribution to family income could therefore reflect the income development of the fathers. The unemployment rate is higher among those giving birth in 1994 than those giving birth in 1998, which reflects overall labour market conditions at the time (OECD, 2005:237). The picture is rather similar for two-child parents, but it should be noted that there is a decline (rather than an increase) in the proportion of mothers without labour market attachment from the comparison to the treatment group.

3.3 Models

All models are linear regression models, as these provide more intuitive estimates than the logit model without compromising the validity of the results (see Angrist and Pischke, 2009; Hellevik, 2009).[14] We start by estimating the overall change in short-term fertility during the follow-up for the full sample, split by the parity of the child born in 1994 or 1998. This model takes the form

$$Y_i = \alpha + \beta treat_i + \delta X_i + \varepsilon_i,$$

where Y_i is a dummy variable taking the value 1 if mother *i* gives birth to another child before the first/second child turns four years old. $treat_i$ is a dummy variable taking the value 1 if mother i gave birth in 1998 and 0 if she gave birth in 1994, and this is the main variable of interest. X_i is a vector of covariates and ε_i is a random error term.

To examine *when* any difference between the two groups emerges, and how this may change during our follow-up period, we move on to a model estimating the proportion of parents who have had another child when their first/second child is *m* months old. This model, in the following referred to as 'the cumulative model', takes the form

$$Y_{im} = \alpha + \beta treat_i + \delta X_i + \varepsilon_i,$$

[14] We have compared the estimates to the marginal effects from logit models, and the differences are negligible.

where Y_{im} is a dummy variable taking the value 1 if mother i have had a new child when the previous child is m months old. This is a cumulative variable, valued 0 until the mother has a new child and 1 thereafter.[15] $treat_i$ is a dummy variable taking the value 1 if parents i gave birth in 1998 (and 0 if they gave birth in 1994). X_i is a vector of covariates and ε_i is a random error term.

We wish to briefly highlight that we choose this approach to analysing fertility behaviour over e.g. survival models as our focus is on the *difference* in fertility behaviour between eligible and non-eligible mothers. This model provides us with more hands-on information than would e.g. separate survival models for the treatment and comparison group (wherein the significance level of the difference in question would be unknown) or models with interaction terms between time and treatment status (wherein the number of estimates would become unreasonably high).

In addition to estimating these models for the full sample, we reiterate them for subsamples split by the mother's educational level and labour market attachment the year before birth. This is done to further test the postulations in our two hypotheses, as we see education and employment as good proxies for the mother's opportunity costs of childbearing. Moreover, her labour market attachment may provide insight into the possible influence of the paid parental leave scheme.

4 Results

The results are presented in two main subsections, one for the full sample and one for the subsamples. All analyses are performed separately for one-child and two-child parents, but, since figures take a lot of space and the results are very similar for one and two-child parents, we have chosen to include the cumulative figures for the parity one analyses only, and simply comment on any divergences worth noting between parity one and parity two.[16]

4.1 Full sample

Table 1 presents the *treat*-parameters from the main models, both without (model 1) and with (model 2) covariates (hereafter referred to as the simple and the extended model, respectively). As these parameters are derived from a linear model, the estimates show the difference in proportion points

[15] Say, for instance, that a new child is born when the previous child is 25 months old. m_0-m_{24} then has the value 0, and m_{25}-m_{47} has the value 1.
[16] Figures for parity two can be found in Appendix B.

between those who had their child in 1998 and those who had their child in 1994. We will primarily denote the estimates as *percentage points* (by multiplying the coefficients by 100), as this gives a more intuitive interpretation.

Table 2: Treat-estimates, full sample

	One child		Two Children	
	b	SE	b	SE
Without covariates	-0.0106*	0.0050	-0.0097*	0.0046
With covariates	-0.0375**	0.0066	-0.0216**	0.0063
N	**36 768**		**33 076**	

*: p<0.05 **: p<0.01

Starting with one-child parents, we see that a simple model without control variables yields an estimate of -0.0106 that is statistically significant at the 5 per cent level. This number denotes the crude difference in the share of mothers having another child during the follow-up period and mirrors the difference between the treatment and comparison groups we saw in Table 1. However, as we will also recall from this table, the 1994 and 1998 cohorts differed on a range of socio-demographic characteristics. When controlling for these differences, the estimate drops to -0.0375, and is now significant at the 1 per cent level. It is the inclusion of the variables on educational level and unemployment rates that drive the change in the estimate.[17] The pattern is similar for two-child mothers, and the simple model without covariates yields a statistically significant estimate of -0.0097. As was the case for one-child mothers, the estimate drops when the covariates are included, and the extended model yields an estimate of -0.0216. These estimates equal a relative decline in short-term fertility after the introduction of CFC of 5.9 per cent for one-child parents and 9.1 per cent for two-child parents.[18]

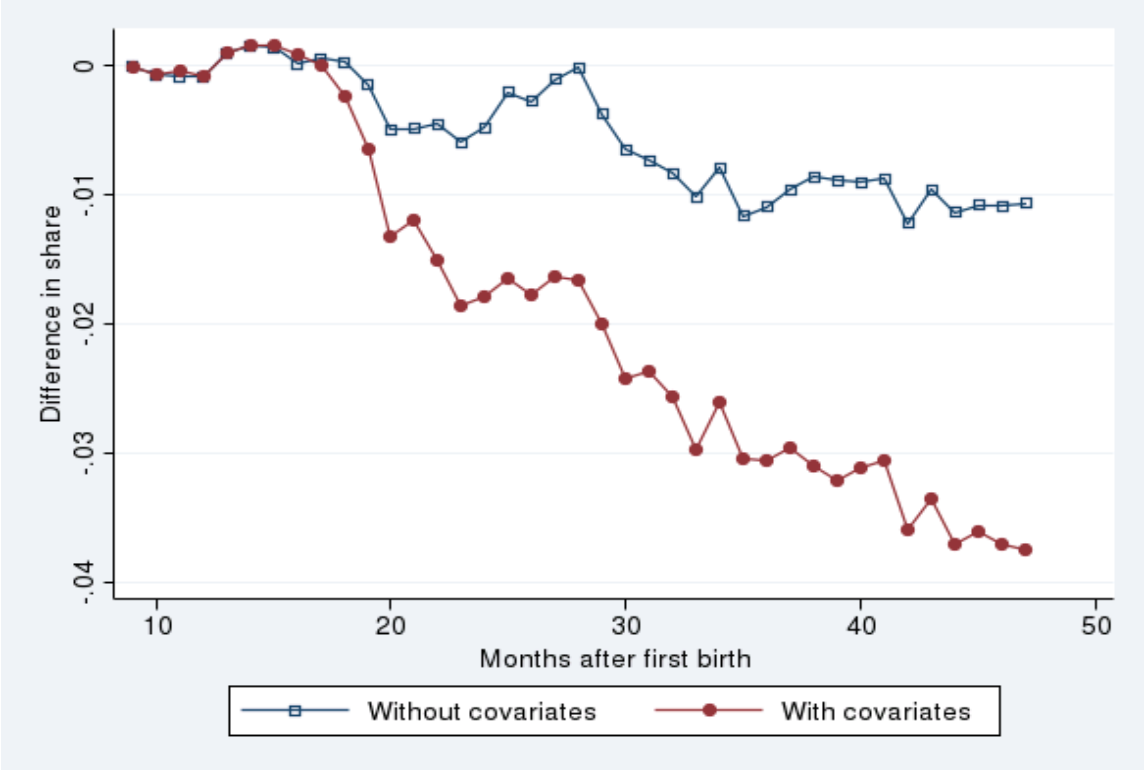
In sum, the overall results for the entire sample indicate that mothers whose child was eligible for CFC had fewer children over a four-year period than did mothers who gave birth before the policy was introduced. This is quite the opposite of what we expected based on the theoretical postulation that the CFC would decrease the price of children and hence should *increase* short-term fertility. To examine when these changes in fertility behaviour emerge, we now move on to the cumulative model providing information about the timing of this change. The results for one-child parents are presented in Figure 1

[17] See Appendix A, Table A1 and A2.

[18] These percentages are based on the proportion of the comparison groups who had another child during the follow-up period, shown in Table 1. For instance, 63.34 per cent of first-time parents in 1994 had a second child. A decrease of 3.75 percentage points from the 1994 to the 1998 cohort then equals a relative decline of (3.75*100)/63.34=5.92 per cent.

below, while the corresponding figure for two-child parents is shown in Appendix B (figure B1). All treat-estimates and their corresponding p-values are shown in Appendix C.

Figure 1: Difference in share of mothers (reported on y-axis) having a second child by a certain month (reported on x-axis). With and without control variables. Estimates and p-values are found in Appendix C, Table C1 (and C2 for parity two)



The point estimates in this figure show the difference (in percentage points) in the share of mothers who had given birth to another child when the first child was *m* months old, when comparing those who gave birth in 1998 to those who gave birth in 1994. For instance, the first statistically significant estimate in the extended model (lower, red line) is that after 20 months. This estimate of -0.0132 indicates that the share of mothers who had had another child when their first child was 20 months old is 1.32 percentage points lower among mothers whose first child was born in 1998 than among those whose first child was born in 1994. As the model is cumulative, the new child could have been born in any of the months preceding and including the 20th month.

Looking closer at the upper, blue line in Figure 1, we see that the estimates from the simple model fluctuate around zero throughout the follow-up period. The difference between the treatment and the comparison groups is only statistically significant at between five and ten per cent after the 33rd month, and at the end of the 47-month follow-up period, it has reached the already well-known estimate of -0.0106 or -1.06 percentage points. However, as we can see from the lower, red line, when adding the

control variables the estimates drop at about 18 months, after which a negative relationship emerges and becomes statistically significant at the 1 per cent level from the 22nd month onwards. The estimates remain rather stable between 23 and 28 months, before they drop even further and end up at -0.0371 after 47 months.

The pattern is similar, yet less pronounced, for parity two parents (see Figure B1 in Appendix B). The estimates in the extended model are negative from 27 months onwards, but do not reach statistical significance until the 38th month. In sum, however, it seems that the change in the mean estimates in Table 2 is driven by changes in births occurring towards the middle or later half of the follow-up period. It is interesting to note that 18 months is just above the amount of time it would take for an employed mother to complete the paid parental leave period, return to work and regain eligibility for future paid parental leave. That the negative treat-estimates (i.e. the decline in parity progression in the treatment group) emerge after this amount of time might suggest that this became a less feasible strategy after the CFC introduction, and that eligible mothers did indeed use the benefit to prolong the initial leave period. This might therefore support the postponement hypothesis.

To further test our two hypotheses, and thereby gain further insight into the mechanisms underlying this somewhat surprising negative pattern, we now move on to the results from the subsample analyses.

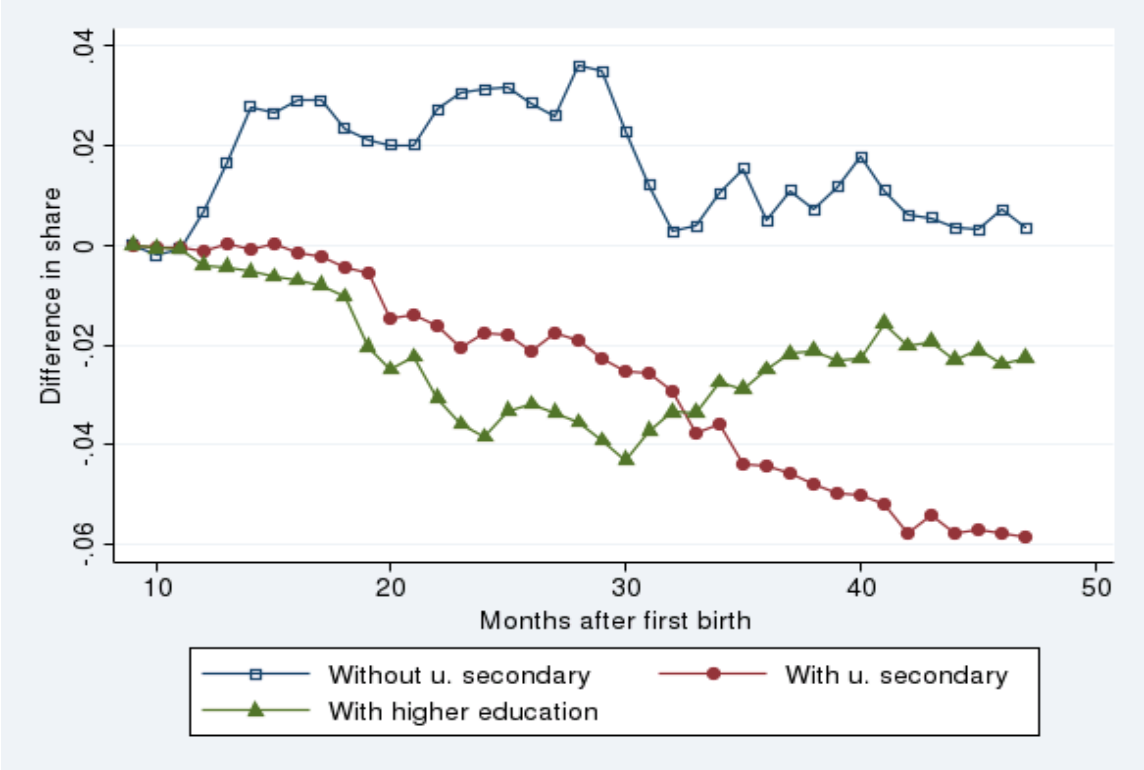
4.2 Different mothers, different adaptations?

In this section, we look more closely at the difference between mothers who have similar socio-economic characteristics. We start by splitting the sample by educational level, before proceeding to a split by labour market attachment. We only assess the estimates from the extended, cumulative models here, presenting them in plots similar to the one above.

Starting with Figure 2, which shows the cumulative estimates split by the mother's educational level, there seems to be a rather clear distinction between mothers without upper secondary education, on the one hand, and those with upper secondary and higher education, on the other. For mothers without upper secondary education, there is an overall positive trend from 12 months and throughout the remainder of the period, which is statistically significant at the 5 per cent level between 13 and 17 months. This early speed-up is reduced later on, however, and, when 47 months have passed, the difference in second births among these mothers is non-significant at only 0.35 percentage points. This pattern demonstrates two important points: firstly, how the cut-off of the observation period may be crucial to the final results of an overall analysis like the ones in Table 2, and, secondly, how the

negative result in the full sample analysis (and hence its support for the postponement hypotheses) does not necessarily apply to all groups of mothers.

Figure 2: Difference in share of mothers (reported on y-axis) having a second child by a certain month (reported on x-axis), by the mother's educational level before birth. Control variables are included. Estimates and p-values are found in Appendix C, Table C1 (and C2 for parity two)

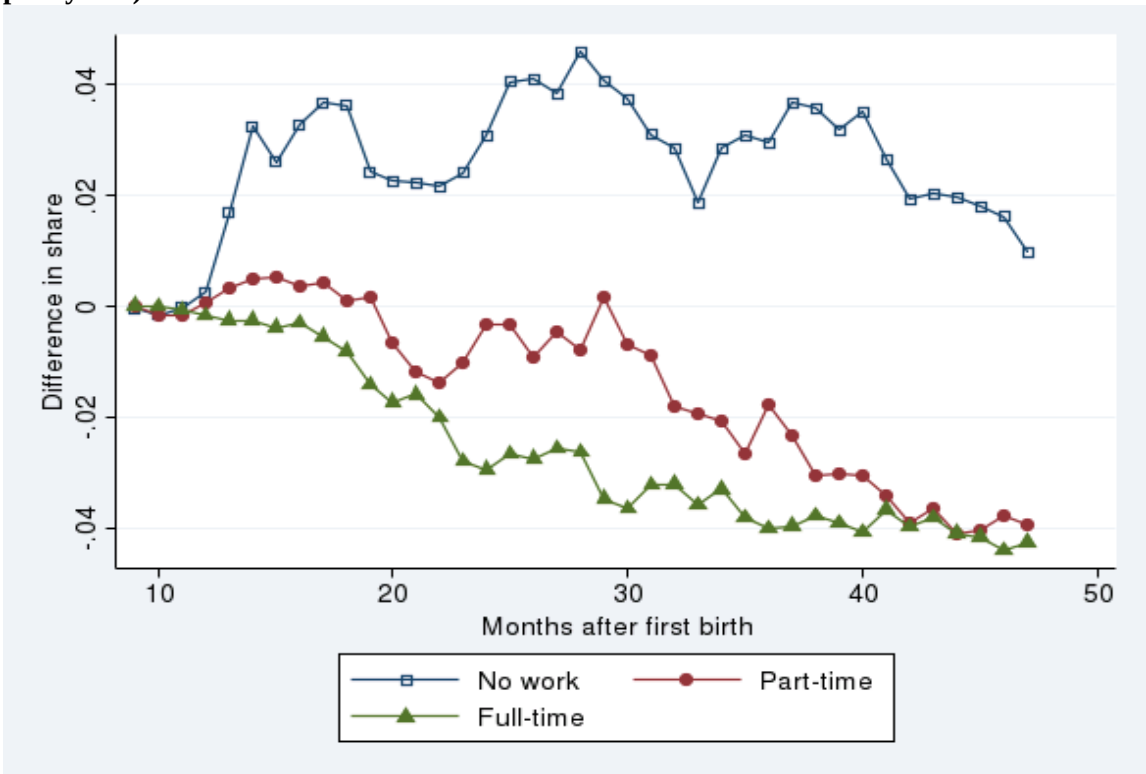


Moving on to mothers with completed upper secondary education, we see that the difference between the treatment and comparison groups is close to zero until about 18 months have passed, before it decreases rather steadily and is statistically significant at the 1 per cent level from 20 months and throughout the remainder of the follow-up period. Similarly, the line for higher education drops more or less immediately, but is only statistically significant at the 1 per cent level between 19 and 33 months. Thereafter, we see a small recuperation in this group, and, after 47 months have passed, the difference between the treatment and the comparison groups is -2.26 percentage points and significant at the 5 per cent level. The results nonetheless reject our postulation that the fertility behaviour of highly educated mothers would remain unchanged by the CFC introduction.

The estimates for parity two (see Figure B2 in Appendix B, and Table C2 in Appendix C) are similar to those for parity one with two exceptions: the change among mothers with upper secondary and higher education does not reach statistical significance until the last 3-7 months of the follow-up, and there is no sign of recuperation among mothers with higher education.

Splitting the sample by mothers' labour market attachment before birth, we get the estimates shown in Figure 3 below.

Figure 3: Difference in share of mothers (reported on y-axis) having a second child by a certain month (reported on x-axis), by the mother's labour market attachment before birth. Control variables are included. Estimates and p-values are found in Appendix C, Table C1 (and C2 for parity two)



In this figure, two main patterns seem to be emerging. Firstly, there is a positive, yet slightly unstable, trend for mothers without labour market attachment that is statistically significant at the 5 per cent level between 14 and 18, and 25 and 28 months. This pattern resembles the pattern we saw for mothers without upper secondary education in Figure 2 above, and the estimate after 47 months is non-significant at 0.0097. Secondly, there is a rather steady downward trend for both part-time and full-time employed mothers. The estimates are more uncertain, however, for the part-time employed, and do not reach statistical significance until 38 months have passed. At the end of the follow-up period, the difference between the treatment and control groups is -3.94 percentage points for the part-time employed and -4.27 percentage points for the full-time employed – both estimates significant at the 1 per cent level. These results thus reject our postulation that there would be no change in fertility behaviour among full-time employed mothers.

The pattern among two-child parents (Figure B2) is similar to what we see among one-child parents, but less pronounced. Moreover, at the end of the follow-up period, the difference between the treatment and control groups is -3.38 percentage points for the part-time employed and -2.71 percentage points for the full-time employed. This means that the change in fertility behaviour is more substantial among part-time employed mothers than among full-time employed mothers, while the opposite is the case for one-child parents.

4.3 Results: Summary

Our full sample analysis shows that mothers eligible for CFC have fewer children than ineligible mothers in the comparison group during the four-year follow-up period. The decline starts after the first child is approximately 20 months old and the second child is approximately 30 months. After controlling for the socio-demographic characteristics of both parents, of which educational level and local unemployment trends are the most influential, the proportion of mothers having another child before the previous child is four years old is 3.75 percentage points lower in the treatment group for parity one parents and 2.16 percentage points lower for parity two parents. This equals a relative decline of 5.9 and 9.1 percent, respectively.

When running the models separately by mother's educational level and labour market attachment, we see a polarisation between non-employed mothers and mothers without upper secondary education, on one hand, and employed mothers and mothers with upper secondary and higher education on the other. That is, a pattern of speeding up the progression to having another child in the former group and of slowing it down in the latter. This pattern demonstrates that the results from the full sample conceal rather heterogeneous adaptations depending on the mothers' income and indirect cost of childbearing. It could also suggest that that labour market attachment is one of the keys to explaining the rather surprising negative effect.

5 Discussion and conclusion

It is now time to go back and review the hypotheses set out in Section 2.2. To reiterate their contents, we will address each of them in their proposed order.

The acceleration hypothesis postulated that the introduction of the CFC policy reduced the cost of childbearing, which, in turn, would increase the demand for children. All other things being equal, we therefore expected the introduction of the CFC benefit to speed up future childbearing and increase short-term fertility. When looking at the analyses for the full sample, which show *lower* short-term

fertility among mothers eligible for the CFC benefit, this hypothesis does not receive any support from our data. However, the acceleration hypothesis also predicted that the speed-up pattern would primarily be evident among three groups of mothers: 1) mothers with no attachment to the labour market, 2) mothers without upper secondary education, and 3) part-time employed mothers. The subsample analyses provide some support as regards the first group, although estimates are uncertain and do not indicate any significant short-term quantum effects. It is therefore questionable whether the size of the CFC benefit was large enough to create any substantial income effect in these groups, although the periodically positive estimates might suggest otherwise.

The postponement hypothesis postulated that the introduction of the CFC policy would lower short-term fertility by creating a possible compensation for lost income while the mother extended her leave period, which, in turn, would delay her labour market return as well as her subsequent childbearing. Contrary to the acceleration hypothesis, this hypothesis is strongly supported by our data. The overall pattern is a consistent downward trend, which seems to be particularly evident from 18-20 months for parity one parents and from around 30 months for parity two parents. We regard it as likely that this pattern is amplified by the Norwegian institutional setting, where mothers must return to work for six months before they can take a renewed period of paid parental leave. Based on this consideration, the postponement hypothesis suggested that the postponement of parity progression and decline in short-term fertility would be most evident among two groups of mothers; 1) part-time employed mothers and 2) mothers with upper secondary (but not higher) education. Overall, but with some nuances during the follow-up period, these suggestions are supported by our subsample models. However, we do not see a clear distinction between either mothers with and without higher education, or part-time and full-time employed mothers as we anticipated. This might suggest that *some* education and/or employment may be as important to the mother's indirect costs of childbearing as the length of the education or degree of employment.

In addition to these more specific insights into the relationship between cash transfer policies and fertility behaviour, this analysis has provided two insights of more general value. Firstly, it suggests that family policies can have consequences for other outcomes than those that motivated their implementation. Although the CFC benefit was not based on a desire to change fertility behaviour in Norwegian families, we have nevertheless observed changes in the transition to having a second and third child after its introduction. Secondly, a given policy can have theoretically unexpected consequences when implemented in a wider policy framework that mediates its effect. In this particular case, an interaction between the CFC benefit and the paid parental leave scheme may explain the unexpected pattern we observe.

5.1 Conclusion

This analysis has addressed the implications of the introduction of a cash transfer to families for subsequent fertility behaviour among Norwegian families. Given that this policy reduced the costs of childbearing, we expected the eligible mothers to speed up their parity progression and hence have higher short-term fertility. The results indicate, however, that the CFC benefit has led to *slower* parity progression and lower short-term fertility. The changes in fertility behaviour differ between different groups of mothers, and we see a polarisation between non-employed mothers and mothers without upper secondary education, on one hand, and employed mothers and mothers with upper secondary and higher education, on the other – with the acceleration of parity progression being more prominent in the former group and the slowing down being more prominent in the latter. Our results support the idea that family policies can change fertility behaviour, although not necessarily in the theoretically anticipated way. On a more general level, the results demonstrate how a universal family policy can have different consequences for family behaviour depending on its income effect and the mothers' opportunity cost of childbearing, as well as its interaction with other family policies.

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Appendix A: Estimates from stepwise model

Table A1: Parity one

	Post	Age	Immi-grant b.g.	Full time	Education	Income	Union status	Unemplo-yment
Intercept	0,6334**	-1.3472**	-1.3450*	-1.2419**	-0.8582**	-0.8465**	-0.6165**	-0.5551**
Post	**-0,0106***	**-0.0115***	**-0.0114***	**-0.0115***	**-0.0188****	**-0.0191****	**-0.0137****	**-0.0375****
Mother's age		0.1048**	0.1045**	0.0991**	0.0750**	0.0750**	0.0675**	0.0676**
Father's age		0.0372**	0.0374**	0.0358**	0.0295**	0.0285**	0.0260**	0.0259**
Mother's age^2		-0.0019**	-0.0019**	-0.0018**	-0.0014**	-0.0014**	-0.0013**	-0.0013**
Father's age^2		-0.0006**	-0.0006**	-0.0006**	-0.0005**	-0.0005**	-0.0005**	-0.0005**
Immigrant parents			-0.0111	-0.0012	0.0369**	0.0407**	-0.0037	-0.0082
Mother full time				0.0296**	0.0059	0.0009	0.0031	0.0033
Mother student					-0.0191*	-0.0173*	-0.0116	-0.0109
M. upper secondary					0.0240*	0.0228*	0.0291**	0.0313**
M. higher					0.0992**	0.0990**	0.0881**	0.0885**
F. upper secondary					0.0599**	0.0585**	0.0567**	0.0568**
F. higher					0.0511**	0.0499**	0.0415**	0.0425**
Family income						0.0014	0.0016*	0.0015*
Income^2						-2.31e-06	-2.51e-06	-2.38e-06
Cohabiting							-0.0992**	-0.0975**
Unemployment rate								-1.5858**
Unemployment missing								-0.0349*

*: p<0.05 **: P<0.01

Table A2: Parity two

	Post	Age	Immi-grant b.g.	Full time	Education	Income	Union status	Unemploy-ment
Intercept	0.2367**	0.3326**	0.2984**	0.2735**	0.4987**	0.4567**	0.5700**	0.6214**
Post	**-0,0097***	**-0.0018**	**-0.0025**	**-0.0023**	**-0.0078**	**-0.0073**	**-0.0029**	**-0.0216****
Mother's age		-0.0017	0.0018	0.0031	-0.0081	-0.0068	-0.0109	-0.0108
Father's age		0.0066	0.0053*	0.0056	0.0025	0.0046	0.0032	0.0031
Mother's age^2		-0.0002	-0.0002	-0.0002*	-0.0001	-0.0001	0.0000	0.0000
Father's age^2		-0.0001	-0.0001**	-0.0001	-0.0001	-0.0001	-0.0001	-0.0001
Immigrant parents			0.0751**	0.0735**	0.0782**	0.0698**	0.0538**	0.0543**
Mother full time				-0.0074	-0.0304**	-0.0223**	-0.0210**	-0.0206**
Mother student					0.0254**	0.0219*	0.0245*	0.0256**
M. upper secondary					-0.0105	-0.0088	-0.0102	-0.0108
M. higher					0.0910**	0.0915**	0.0878**	0.0881**
F. upper secondary					0.0116	0.0136	0.0111	0.0113
F. higher					0.0440**	0.0478**	0.0443**	0.0447**
Family income						-0.0028**	-0.0031**	-0.0032**
Income^2						0.0000**	0.0000**	0.0000**
Cohabiting							-0.0452**	-0.0438**
Unemployment rate								-1.2157**
Unemployment missing								-0.0357*

*: p<0.05 **: p<0.01

Appendix B: Figures for parity two

Figure B1: Difference in share of mothers (reported on y-axis) having a third child by a certain month (reported on x-axis). With and without control variables. Estimates and p-values are found in Appendix C, Table C2

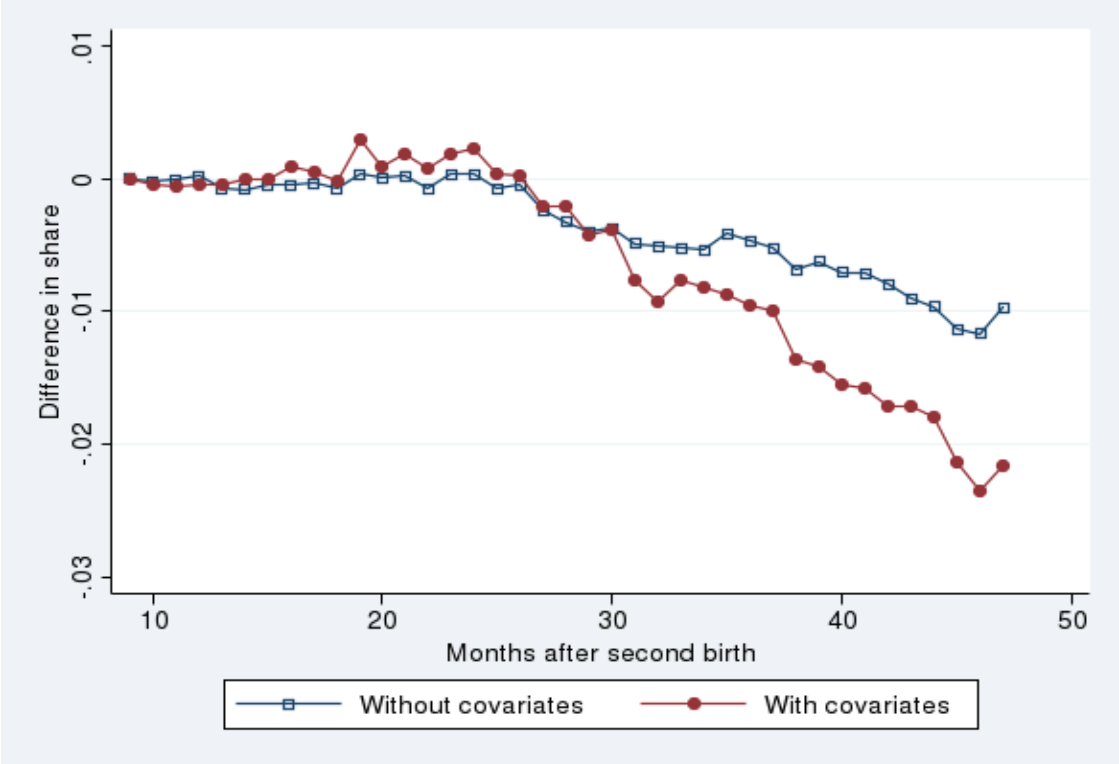


Figure B2: Difference in share of mothers (reported on y-axis) having a third child by a certain month (reported on x-axis), by the mother's educational level before birth. Control variables are included. Estimates and p-values are found in Appendix C, Table C2

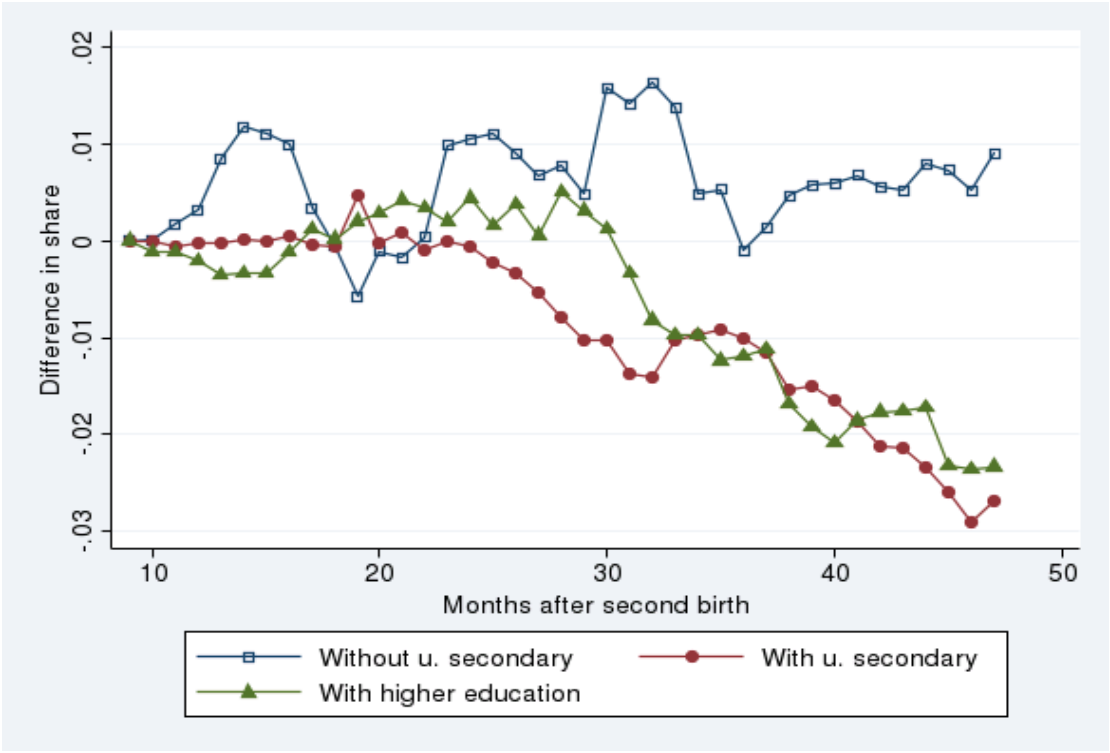
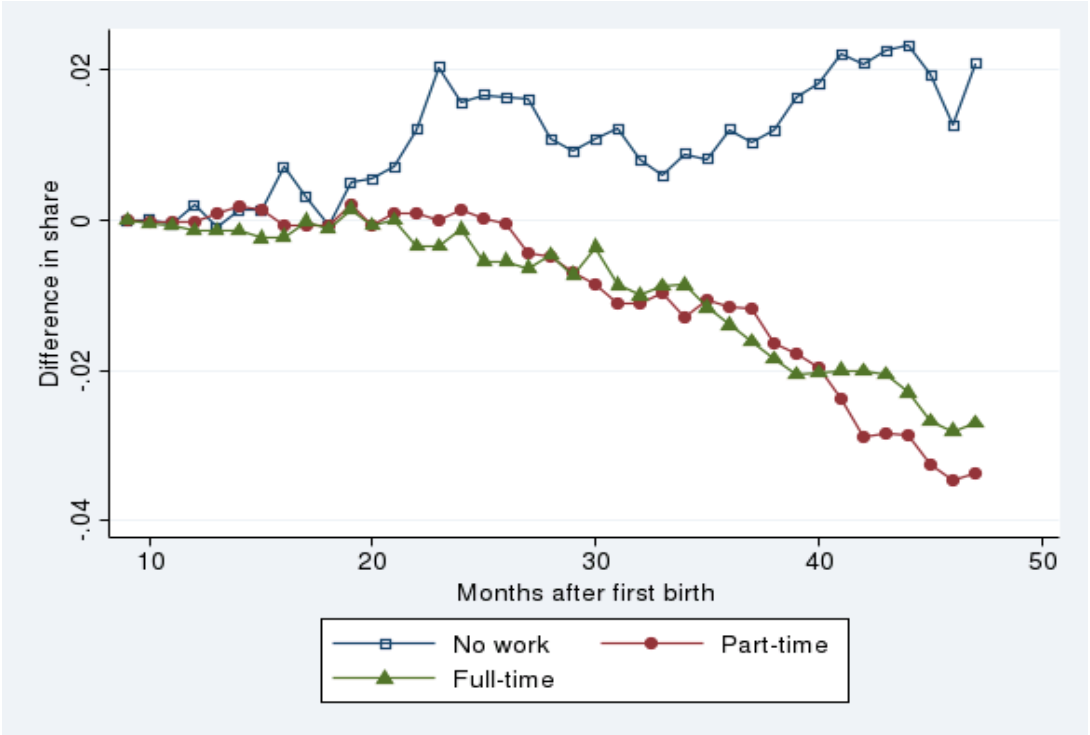


Figure B3: Difference in share of mothers (reported on y-axis) having a third child by a certain month (reported on x-axis), by the mother's labour market attachment before birth. Control variables are included. Estimates and p-values are found in Appendix C, table C2



Appendix C: Treat-estimates at month 9-47

Table C1: Parity one, with covariates unless otherwise specified

	All, no cov.	All	Without upper secondary	With upper secondary	Higher education	No work	Part-time	Full-time
9	-0.0001	-0.0000	0.0000	-0.0000	0.0000	-0.0004	-0.0002	0.0000
10	-0.0007**	-0.0006	-0.0020	-0.0005+	-0.0006+	-0.0016	-0.0016*	-0.0000
11	-0.0008	-0.0004	-0.0006	-0.0005	-0.0007	-0.0003	-0.0016	-0.0008
12	-0.0008	-0.0008	0.0068	-0.0020+	-0.0039*	0.0025	0.0005	-0.0017
13	0.0010	0.0010	0.0167*	-0.0013	-0.0044+	0.0168+	0.0032	-0.0026
14	0.0016	0.0016	0.0278**	-0.0023	-0.0053+	0.0324**	0.0048	-0.0026
15	0.0015	0.0016	0.0266*	-0.0023	-0.0063+	0.0259*	0.0051	-0.0039
16	0.0001	0.0009	0.0291*	-0.0036	-0.0070	0.0328*	0.0036	-0.0030
17	0.0006	0.0001	0.0290*	-0.0044	-0.0081	0.0365*	0.0044	-0.0056
18	0.0003	-0.0023	0.0234+	-0.0063+	-0.0103+	0.0362*	0.0007	-0.0082+
19	-0.0014	-0.0065	0.0210	-0.0106*	-0.0204**	0.0243	0.0017	-0.0143*
20	-0.0049	-0.0132**	0.0201	-0.0179**	-0.0250**	0.0225	-0.0067	-0.0174**
21	-0.0048	-0.0120*	0.0201	-0.0164**	-0.0224**	0.0222	-0.0118	-0.0160*
22	-0.0045	-0.0150**	0.0273	-0.0205**	-0.0307**	0.0217	-0.0138	-0.0201**
23	-0.0058	-0.0186**	0.0305+	-0.0246**	-0.0360**	0.0240	-0.0104	-0.0279**
24	-0.0048	-0.0178**	0.0312+	-0.0235**	-0.0385**	0.0307	-0.0034	-0.0295**
25	-0.0020	-0.0164**	0.0315+	-0.0217**	-0.0332**	0.0405*	-0.0033	-0.0268**
26	-0.0027	-0.0178**	0.0285	-0.0226**	-0.0319**	0.0409*	-0.0091	-0.0276**
27	-0.0010	-0.0164**	0.0260	-0.0203**	-0.0337**	0.0384+	-0.0047	-0.0258**
28	-0.0001	-0.0166**	0.0360+	-0.0217**	-0.0356**	0.0459*	-0.0078	-0.0263**
29	-0.0037	-0.0200**	0.0350+	-0.0248**	-0.0391**	0.0406+	0.0016	-0.0348**
30	-0.0064	-0.0242**	0.0228	-0.0280**	-0.0431**	0.0373+	-0.0069	-0.0366**
31	-0.0073	-0.0236**	0.0121	-0.0257**	-0.0373**	0.0309	-0.0089	-0.0323**
32	-0.0083	-0.0256**	0.0027	-0.0264**	-0.0336**	0.0285	-0.0180	-0.0322**
33	-0.0102*	-0.0297**	0.0038	-0.0310**	-0.0337**	0.0185	-0.0193	-0.0359**
34	-0.0079	-0.0260**	0.0103	-0.0275**	-0.0276*	0.0285	-0.0206	-0.0330**
35	-0.0116*	-0.0305**	0.0154	-0.0329**	-0.0289*	0.0307	-0.0267+	-0.0381**
36	-0.0109*	-0.0306**	0.0050	-0.0315**	-0.0250*	0.0293	-0.0179	-0.0401**
37	-0.0095+	-0.0296**	0.0110	-0.0310**	-0.0219+	0.0366	-0.0233	-0.0397**
38	-0.0085	-0.0310**	0.0070	-0.0321**	-0.0212+	0.0356	-0.0306*	-0.0379**
39	-0.0088+	-0.0322**	0.0117	-0.0341**	-0.0233*	0.0317	-0.0304*	-0.0392**
40	-0.0090+	-0.0312**	0.0178	-0.0339**	-0.0228+	0.0350	-0.0307*	-0.0408**
41	-0.0087+	-0.0306**	0.0110	-0.0323**	-0.0157	0.0264	-0.0341*	-0.0368**
42	-0.0122*	-0.0360**	0.0060	-0.0377**	-0.0203+	0.0193	-0.0392**	-0.0398**
43	-0.0095+	-0.0335**	0.0055	-0.0351**	-0.0194+	0.0201	-0.0366*	-0.0382**
44	-0.0113*	-0.0370**	0.0036	-0.0387**	-0.0230*	0.0195	-0.0410**	-0.0410**
45	-0.0108*	-0.0360**	0.0031	-0.0375**	-0.0211+	0.0179	-0.0405**	-0.0416**
46	-0.0109*	-0.0370**	0.0070	-0.0392**	-0.0240*	0.0161	-0.0379**	-0.0442**
47	-0.0106*	-0.0375**	0.0035	-0.0392**	-0.0226*	0.0097	-0.0394**	-0.0427**

+: p<0.1, *: p<0.05, **: p<0.01

Table C2: Parity two, with covariates unless otherwise specified

	All, no cov.	All	Without upper secondary	With upper secondary	Higher education	No work	Part-time	Full-time
9	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
10	-0.0002	-0.0004	0.0001	-0.0004	-0.0012+	0.0001	-0.0002	-0.0005
11	-0.0001	-0.0005	0.0017	-0.0007	-0.0012	-0.0005	-0.0001	-0.0008
12	0.0003	-0.0005	0.0032	-0.0008	-0.0021*	0.0020	-0.0002	-0.0015+
13	-0.0007	-0.0004	0.0085	-0.0014	-0.0035*	-0.0010	0.0009	-0.0014
14	-0.0008	0.0000	0.0118	-0.0011	-0.0034	0.0013	0.0018	-0.0014
15	-0.0005	-0.0001	0.0110	-0.0011	-0.0034	0.0014	0.0014	-0.0024
16	-0.0005	0.0009	0.0099	0.0000	-0.0012	0.0071	-0.0006	-0.0022
17	-0.0003	0.0005	0.0034	0.0004	0.0012	0.0031	-0.0007	-0.0002
18	-0.0007	-0.0002	-0.0005	0.0000	0.0001	-0.0008	-0.0006	-0.0011
19	0.0004	0.0030	-0.0057+	0.0041	0.0020	0.0050	0.0021	0.0014
20	0.0000	0.0009	-0.0010	0.0016	0.0028	0.0054	-0.0006	-0.0007
21	0.0002	0.0019	-0.0016	0.0029	0.0042	0.0072	0.0010	0.0000
22	-0.0007	0.0008	0.0004	0.0015	0.0034	0.0121	0.0010	-0.0034
23	0.0004	0.0018	0.0099	0.0020	0.0021	0.0204	-0.0001	-0.0034
24	0.0003	0.0023	0.0105	0.0029	0.0045	0.0156	0.0014	-0.0012
25	-0.0007	0.0004	0.0111	0.0009	0.0017	0.0167	0.0003	-0.0055
26	-0.0005	0.0003	0.0091	0.0013	0.0039	0.0164	-0.0004	-0.0055
27	-0.0023	-0.0021	0.0068	-0.0009	0.0006	0.0162	-0.0044	-0.0064
28	-0.0032	-0.0020	0.0078	-0.0007	0.0051	0.0109	-0.0048	-0.0047
29	-0.0040	-0.0043	0.0049	-0.0026	0.0032	0.0093	-0.0069	-0.0073
30	-0.0037	-0.0039	0.0158	-0.0030	0.0014	0.0109	-0.0086	-0.0036
31	-0.0049	-0.0076	0.0142	-0.0067	-0.0032	0.0122	-0.0110	-0.0086
32	-0.0051	-0.0092+	0.0164	-0.0086	-0.0081	0.0081	-0.0111	-0.0100
33	-0.0052	-0.0077	0.0138	-0.0066	-0.0096	0.0060	-0.0096	-0.0087
34	-0.0054	-0.0082	0.0049	-0.0060	-0.0097	0.0089	-0.0129	-0.0087
35	-0.0041	-0.0087	0.0053	-0.0062	-0.0123	0.0082	-0.0107	-0.0116
36	-0.0046	-0.0096+	-0.0010	-0.0063	-0.0118	0.0121	-0.0115	-0.0140
37	-0.0052	-0.0100+	0.0014	-0.0067	-0.0112	0.0103	-0.0117	-0.0162+
38	-0.0069+	-0.0137*	0.0047	-0.0110+	-0.0169	0.0120	-0.0164+	-0.0185*
39	-0.0062	-0.0141*	0.0057	-0.0115+	-0.0192+	0.0163	-0.0177+	-0.0206*
40	-0.0071	-0.0155**	0.0060	-0.0130*	-0.0210+	0.0182	-0.0196*	-0.0203*
41	-0.0071	-0.0158**	0.0067	-0.0131*	-0.0187+	0.0222	-0.0237*	-0.0200*
42	-0.0079+	-0.0171**	0.0056	-0.0140*	-0.0178	0.0209	-0.0289**	-0.0201*
43	-0.0090*	-0.0172**	0.0053	-0.0141*	-0.0177	0.0226	-0.0284**	-0.0206*
44	-0.0096*	-0.0179**	0.0080	-0.0150*	-0.0173	0.0233	-0.0287**	-0.0230*
45	-0.0114*	-0.0214**	0.0073	-0.0187**	-0.0233*	0.0194	-0.0325**	-0.0268**
46	-0.0117*	-0.0235**	0.0052	-0.0206**	-0.0237*	0.0126	-0.0346**	-0.0281**
47	-0.0097*	-0.0216**	0.0091	-0.0190**	-0.0234*	0.0209	-0.0338**	-0.0271**

+: p<0.1, *: p<0.05, **: p<0.01

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ISSN: 1892-753X

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Design: Siri Boquist