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The cash-for-care reform and immigrant fertility

Fewer babies of poorer families?

TALL SOM FORTELLER

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Abstract:
Cash-for care policies are contested in many contexts, as they represent an incentive for childrearing over work that may reduce labour market participation, especially among immigrant women. From 1 July 2017, immigrants (both the mother and the father) from outside the European Economic Area must have at least 5 years of residence in Norway to be entitled to cash-for-care benefits. Previous research indicates that this reform did not lead to increased labour market participation of mothers and fathers treated by the reform. In this article, we examine whether the changes in the cash-for-care benefits policy have resulted in a substantive change in income and if the reform had an impact on the childbearing behaviour among those affected by the reform. Our descriptive analyses indicate no change in employment rates and household income. To detect possible changes in fertility, we employ a Difference-in-Difference approach, in which we compare the treatment group with four comparison groups. Overall, we find no substantial effect of the cash-for-care reform on childbearing behaviour.

Keywords: fertility, cash-for-care, immigrant fertility

JEL classification: J13, J6, J15

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Sammendrag

Etter en endring i kontantstøtteloven som trådte i kraft 1. juli 2017, ble kontantstøttesatsen økt fra 6 000 kr til 7 500 kroner i måned. I tillegg har innvandrere fra land utenfor EU/EØS bare rett til kontantstøtte hvis de har minst fem års botid i Norge. Før lovendringen hadde denne gruppen innvandrere en høy andel mottakere av kontantstøtte. Hensikten med lovendringen, også kalt botidskrav, var «å gi innvandrerne sterkere insentiver til raskt å komme i arbeidsrettet aktivitet» (Innst. 368 L – 2016-2017, s. 8). Siden småbarnsforeldrene som omfattes av reformen kjennetegnes av lav sysselsetting, undersøker vi om lovendringen har ført til en vesentlig endring i inntektsgrunnlaget for de berørte familiene og muligens økt barnefattigdom. I tillegg undersøker vi om lovendringen hadde noen uforutsette konsekvenser. Det kan tenkes at innvandrere som omfattes av reformen utsetter planlagte barnefødsler inntil de har bodd lenge nok i Norge til å oppfylle botidskravet når barnet fyller ett år. Vi undersøker derfor om kvinner som er berørt av reformen har fått færre barn etter at lovendringen ble innført.

I tråd med tidligere analyser, finner vi ingen økning i yrkesdeltakelse som følge av reformen blant familiene som er omfattet av botidskravet. Til tross for det finner vi heller ikke en markant økning i andelen lavinnteksthusholdninger eller husholdninger i laveste inntektskvartil. Det er imidlertid verdt å merke seg at berørte familier er sterk overrepresentert blant lavinntektshusholdningene. I tillegg tyder våre resultater på at innføringen av botidskravet ikke har påvirket fruktbarhetsatferden til de berørte kvinnene.

Background

In 1998, Norway introduced a non-taxable cash-for-care benefit which was paid to parents of one-year-old children that did not make use of public subsidized childcare. The benefit was introduced to give parents with young children more choices to organize childcare for their offspring. In the following years, the exact target group (age of the child) and the size of the benefit were adjusted several times. For example, also children or parents that made use of subsidized kindergarten only ‘part-time’ could receive a reduced cash-for-care benefit (Arntsen et al., 2019). From August 2014 on, the maximum cash-for-care benefit was 6 000 NOK per month and 3 000 NOK if the child used public childcare up to 19 hours per week. With a law amendment implemented from 1 July 2017, the maximum benefit increased to 7 500 NOK per month. Until July 2017, all children registered as residents in Norway and in the right age group (13-23 months) were in principle eligible for the cash-for-care benefit. However, the new law amendment changed the eligibility criteria and since then, only parents that were members of the National Insurance Scheme for at least 5 years could apply for the cash-for care benefit. As Norway is part of the European Economic Area (EEA), a membership in national insurances in these countries is also taken into account. In practice, this means that both parents (or the single parent if the other parent does not live in Norway) must have lived at least 5 years in Norway or another EAA-country before they (or he/she in the case of single parents) can apply for the cash-for-care benefit for their child. As such, the law amendment applies to immigrants moving from countries outside the EAA to Norway (Syse, 2018).

The introduction of this residency requirement was the result of a long-lasting discussion about the cash-for-care benefit, including repeated recommendations to abolish the benefit, due to its’ potential negative impact on parents’ labour supply and especially on the labour supply of immigrant women (Drange & Rege, 2013; Hardoy & Schøne, 2010; Hedding, 2016; NOU 2017:6, page 56). In addition, the role of the kindergarten as an arena for language training for children of immigrants has been underlined. A study on cash-for-care use among immigrants in Oslo and Akershus (the surrounding area of Oslo) indicated a decline in the proportion of children of immigrants in public kindergartens after the cash-for-care benefit was introduced (Kavli, 2001). When the reform was announced in 2017, the Ministry of Children and Families explicitly referred to these two aspects and pointed out that immigrants should participate actively in the labour market as soon as possible after their immigration to Norway and that children of immigrants would profit from language training in kindergarten (Ministry for Children and Equality, 2017).

After its implementation in 2017, possible consequences of the reform have been evaluated in several studies. Firstly, existing analyses confirm that fewer immigrants receive cash-for-care benefits after the residency requirement has been introduced (Arntsen et al., 2019; Sandvik & Gram, 2019). However, this has not led to an increase in employment rates or labour market related measures (such as qualification programs) among mothers and fathers affected by the reform (Lima et al., 2020). Furthermore, no increase in the number of recipients of social assistance in the target group could be observed (Lima et al., 2020). These results indicate that mothers or fathers of children in the treatment group do not adapt their labour market behaviour as a response to the reform. The uptake of the cash-for-care benefit was comparatively high among immigrants from non-EEA countries prior to the reform (Arntsen et al., 2019). The shortfall of the cash-for-care benefit may increase poverty among vulnerable households and thus worsen their living conditions. In the first part of our analyses, we describe the development in household income and labour market participation among parents affected by the reform in comparison to other families, in the period before and after the residency requirement was introduced.

The finding of Lima et al. (2020) that parents in the treatment group have not adapted their labour market participation after the reform was introduced, may also be related to an unintended side-effect of introduction of the residency requirement. Couples affected by the reform and that intended to use the cash-for-care benefit may have postponed their childbearing plans until they are eligible for the cash-for-care benefit or have abandoned their childbearing plans completely. If such a mechanism is in place, a decline in fertility rates for this group should be seen after the reform was implemented. Opposite effects have been observed in previous studies, finding that an introduction or raise of cash transfers explicitly aimed to increase fertility have a positive – but mainly temporary – impact on fertility (Bergsvik et al., 2021). In contrast, the implementation of a residency requirement reduces cash transfers and thus may lead to a decline in birth rates. In the second part of our analysis, we therefore investigate possible changes in childbearing behaviour due to the reform. Before conducting the two analysis we provide necessary background information, including a summary of changes in the uptake of the cash-for-care benefit, important aspects of fertility among immigrants and a brief overview of possible effects of policy changes on fertility. Thereafter, we examine if the income situation of affected parents has changed after the reform was introduced and whether we can detect any substantial changes in the fertility behaviour among those encompassed by the introduced residency requirement.

Changes in the uptake of the cash-for-care benefit

Until June 2017, there were no specific requirements to be eligible for the cash-for-care benefit aside from the age of the child (Syse, 2018). In principle, the child itself is the recipient of the cash-for-care benefit and as such, the child had to be registered as a resident in Norway and had to be in the right age range (13 to 23 months). Before the reform, the maximum monthly rate for the cash-for-care benefit was 6 000 NOK and together with the introduction of the residency requirement, the rate was increase to 7 500 NOK per month. In practice, the benefit is paid out to the parents of the child: if both parents live together with the child, the benefit is paid out to the parent that has applied for the benefit; if only one parent lives together with the child (or the child is registered in one parental household in the case of shared custody), this parent can apply for the benefit, and it is paid out to him or her. With the reform that was passed 16 June 2017, a residency requirement for the parents was introduced from 1 July 2017. From this date, the parent(s) of the child must have been a member of the National Insurance Scheme for at least 5 years. When the child lives with both parents, 5 years of national insurance coverage is required for both parents (Syse, 2018). Documented membership periods of national insurances in other EAA countries are considered, but this applies only to EEA citizens and third countries citizens considered to be in a family relationship with an EEA citizen. Only if the insurance coverage is from another Nordic country, the nationality is deemed unimportant (NAV, 2021). In practice, this means that immigrants from non-EAA countries with a residence time of less than 5 years when their child is 13 months old, were no longer eligible for the cash-for-care benefit.

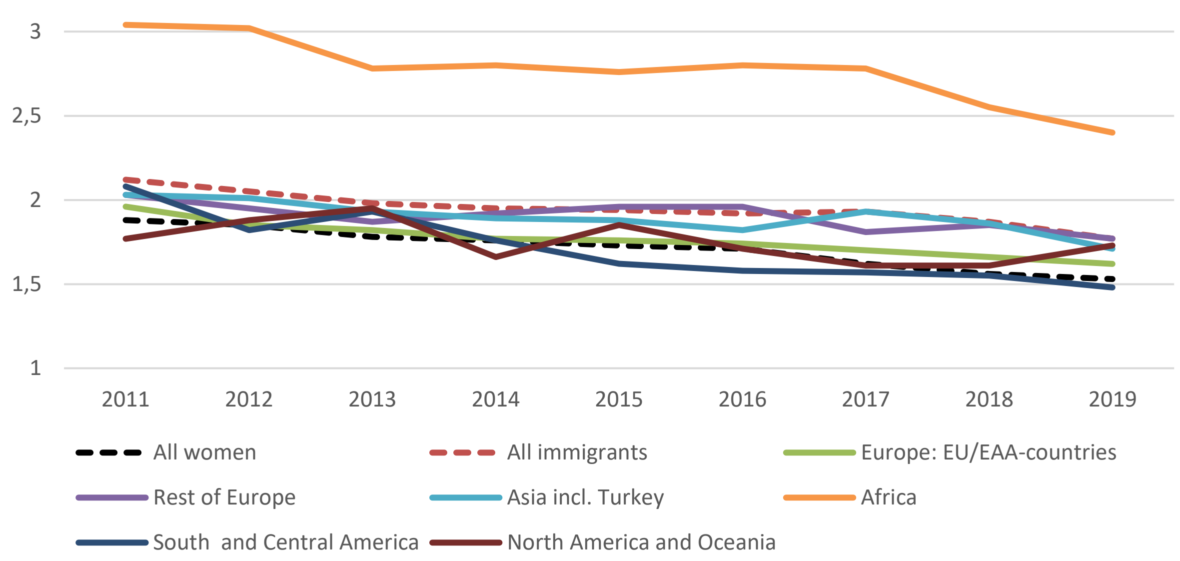
Often, the cash-for-care benefit is used in a transitional period after the paid parental leave has ended, typically twelve months after birth, and until the child is assigned to a place in kindergarten. Theoretically, children are assigned to kindergartens continuously. In practice however, most new places in kindergartens are assigned in September each year, following the timing of the school year. In addition, some parents choose to extend the paid parental leave period with an unpaid period of leave and may thus apply for the cash-for-care benefit. In line with this, the cash-for-care benefit is often used in a transitional period directly after the paid parental leave has ended and until the child is assigned a place in kindergarten. Thus, the use of cash-for-care declines with increasing age of the child (13 to 23 months) (Bakken & Myklebø, 2010; Sandvik & Gram, 2019). Next, research that monitored the use of cash-for-care benefits shows that the uptake of the cash-for-care benefit has declined over time (Arntsen et al., 2019; Egge-Hoveid, 2012; Sandvik & Gram, 2019). Right after the introduction of the cash-for-care benefit in 1998, over 90% of one-year-old children received the benefit at least for one month. As more kindergarten places for one-year-old children were established, the uptake of cash-for-care benefits has declined. In 2003, the Norwegian Government formulated the aim that all one-year-old children should be offered a place in kindergarten from 2005, and this was accomplished only a few years later (Arntsen et al., 2019). Since 2011, the proportion of one-year-old children receiving cash-for-care benefits for at least one month has been relatively stable at around about 55% (Arntsen et al., 2019). Still, many use the benefit only until they are assigned to a kindergarten place in autumn that given year. Before the residency requirement was introduced, non-EAA citizens with one-year-old children received cash-for-care benefits more often than natives or EEA citizens. Monitoring the uptake of cash-for-care benefits of one-year-old children by mothers' country of birth, both Arntsen et al. (2019) and Sandvik & Gram (2019) observe a decline among mothers from non-EAA-countries after the residency requirement was introduced in July 2017, with the steepest decline among children with mothers from Asia and Africa (Arntsen et al., 2019). In contrast, the overall average uptake of the cash-for-care benefit decreased only slightly from 2016 to 2017 and increased among Norwegian mothers in 2018 (Arntsen et al., 2019; Sandvik & Gram, 2019).

Fertility of immigrants in Norway

With the stepwise enlargement of the European Union after the turn of the millennium, working immigrants from especially Eastern- European countries have dominated the annual numbers of immigration to Norway. Until 2011, the annual numbers of immigrations to Norway increased, with almost 80 000 arriving in that year. Thereafter, the annual number of immigrations to Norway has declined to 52 000 in 2019, which is the last year we consider here (Statistics Norway 2022a). In total, immigrants accounted for about 15% of the total population in Norway in 2019 (Statistics Norway 2022b). Among women aged 15-45 years, immigrant women accounted for about 21.5% in the same year, reflecting that most immigrants move to Norway in typical childbearing ages. Many immigrants chose to have children within a few years after they have moved to Norway (Tønnessen, 2014). Similar findings have also been reported for other countries, as for example an increase in transitions to first births among Polish women immigrating to the UK (Lübke, 2015). In addition, the childbearing behaviour of immigrants is strongly related to their country of origin. The birth rate of immigrant women typically lies between the birth rate of women in the country of origin and the destination country (Tønnessen, 2014). This is also the case in Norway and in 2019, about a fourth of all new-born in Norway had a mother that had immigrated to Norway (Statistics Norway 2022a).

Analysing the fertility of immigrants in Norway in the period 1990 to 2012, Tønnessen (2014) concluded that developments in the total fertility rate (TFR) of immigrant women goes in the same direction as the TFR of non-immigrant women in Norway. Figure 1 displays changes in the TFR in the years 2011 to 2019. Beside a general decline in the TFR, we observe a similar trend among all immigrant women, also when they are differentiated by regions of origin. Regarding differences between region of origin, women from Africa have the highest TFR, but this group has also experienced the highest decline in this period (from 3.04 in 2011 to 2.40 in 2019). While immigrant women from Africa stand out with a higher TFR in Norway, the differences between other women are comparatively small. Some groups include comparatively few women, as for example women from North America and Oceania, and thus we observe a more random fluctuation in TFR.

Figure 1. Total fertility rate, by women's country background



Source: Statistics Norway 2020, 2022c.

Tønnessen (2014) also evaluated the impact of immigrants' residence time for their childbearing behaviour in Norway. In line with results from other comparable countries, she found that also in Norway immigrant women with short residence time often have the highest fertility rates and that these fertility rates decline with increasing residence time. Some women migrate to Norway due to family reasons and plan to form a family in Norway or get another child with their partner in Norway (Tønnessen, 2014). In addition, refugees may have postponed family formation before they migrated to Norway. For example, in 2018 the TFR in Norway was highest among women from Syria (3.51) and Eritrea (3.27). Most of them had a relatively short residence time in Norway. In contrast, we could observe in Figure 1 a decline in the TFR among women from Africa, which is mainly driven by women from Somalia. Many of them arrived in the beginning of the period shown in Figure 1, while they have a longer residence time in 2019 and thus also lower fertility rates.

Effects of policy changes on fertility

How responsive are (potential) parents to policy changes or law adjustments in their childbearing behaviour? Despite the obvious relevance of this research question and topic, there are comparatively few empirical studies that assess exact effects of (family) policies, or changes in such policies, on fertility. The main reason for this is that assessing such direct effects requires specific data and methods. A recently published systematic review of studies analysing effects of policies on fertility distinguishes between policies and changes related to parental leave, childcare, health services and universal child transfers (Bergsvik et al., 2021). The cash-for-care benefit is mainly a policy using direct transfers, as a monthly non-taxable amount (6 000 NOK before the reform implemented by 1 July 2017 and 7 500 NOK thereafter) is given to parents with children that are eligible for the benefit. However, as the cash-for-care benefit is defined as an alternative to public childcare and can only be received if the child is not, or only partly, assigned to a kindergarten, findings on the impact of childcare or childcare-use on fertility may also be relevant in the context of our study. Bergsvik et al. (2021) conclude that there is solid evidence that expansions of public childcare have positive effects on fertility. In addition, there is evidence that lower costs of public childcare increase fertility (Bergsvik et al., 2021). Regarding the effect of cash transfers to parents on fertility, the picture is somewhat ambiguous. Even though there is some evidence that universal transfers can have an immediate impact on fertility, it is less clear if the introduction or changes in the size of such transfers lead to a change in the total number of children born by women (Bergsvik et al., 2021). An evaluation of increasing transfers to families in Quebec (compared to the rest of Canada) had, for instance, no long-lasting impact on fertility (Parent & Wang, 2007). Furthermore, an increased cash transfer to parents in one region in Norway mainly led to a tempo-shift in first births among younger women (Galloway & Hart, 2005). Interestingly, a 2006 reform in the federal state of Thuringia in Germany transferred more money (and larger amounts to larger families) to those families not sending their two-year old child to public childcare (Gathmann & Sass, 2018). Comparing childbearing behaviour in Thuringia with nearby federal states in Germany, the authors of this study found positive effects of the reform on higher order births. The effects were concentrated among groups more prone to home care, including large families, single mothers, low-income households, and foreign parents. To our knowledge, there is no existing research study that causally evaluates how a reduction or withdrawal of a cash benefit affects fertility.

Taken together, this implies that the guaranteed access to subsidized and affordable public childcare in Norway should have a positive short and long-term impact on fertility, as it lowers the direct and indirect costs for childcare. In contrast, the introduction of the residency requirement for the cash-for-care benefit may have had a negative effect on the fertility of the affected group. The uptake of cash-for-care benefits was previously particularly high among one-year-old children with immigrant mothers from Africa and Asia (Arntsen et al., 2019). Fertility rates of immigrant women are closely related to residence time (Lübke 2015; Tønnessen 2014) and the residency requirement affected only those who immigrated in the past few years and typically have higher birth rates. Thus, the residency requirement restricts access to the cash-for-care benefit to a group with comparatively high fertility rates in exactly that period in their life course, when they often plan to have children. It might be that some women and couples affected by the reform abandoned or postponed their childbearing plans until they fulfilled the residency requirement. Thereby, the reform may has led to a decline in fertility rates among those affected by the residency requirement. We will examine if such an effect of the reform on fertility rates can be detected in the second part of our analysis.

Analysis

Part 1. Household income before and after the cash-for-care reform

The aim of this part of our analysis is to describe if the introduction of the residency requirement has had an impact on the household income of children affected by the reform. Our analysis is based on annual files from the so-called register-based housing conditions (Statistics Norway 2022d). In these files, individuals can be identified within households. We select the households of children aged 13 to 23 months (the age groups that is eligible for the cash-for-care transfer) as our target population. We add information on their parents (country of birth, date of immigration and resident time in Norway, independent of whether the parents live together or not) and differentiate if they are affected by the reform (treatment group) or not. We define a child as part of the treatment group if at least one of his/her parents is an immigrant born in a non-EEA country and with a residence time less than 5 years when the child turns one year. We apply these criteria, as we have no access to detailed data on migration histories and changes in citizenship. Our descriptive analysis of cash-for-care uptake (see Figure 2 below) indicates that we capture the treatment group applying this approach.

The annual files of the register-based household statistics reflect the housing conditions and household composition by 01 January each year, while the annual income of the household is added for the previous year. For example, a child born in June 2018 will have the right for cash-for-care from July 2019 on. Thus, we are interested in the child's household income in 2019 and identify the household and this income in 2019 based on the registered status 01 January 2020.

Applying different income measures, we analyse descriptively if there are any substantial changes in the income situation for children in the treatment group. This group comprises children that were eligible for the cash-for-care benefit until the residency requirement was implemented, but not thereafter. We plot the income situation for the treatment group and selected comparison groups for the years 2015 to 2019. Before doing so, we describe the number of children potentially affected by the reform in these years (see Table 1).

Table 1. Number of children eligible for cash-for-care by income year and parents' immigration status, 2015-2019*

	2015	**2016**	**2017**	**2018**	**2019**
Both parents born in Norway	75 223	74 276	73 446	71 053	68 001
One parent born in Norway, one in EEA-country					
short residence time	2 065	1 902	1 799	1 682	1 520
long residence time	4 042	4 180	4 499	4 465	4 430
Both parents born in EEA-country					
short residence time	5 260	4 950	4 595	4 095	3 623
long residence time	2 052	2 560	2 919	3 305	3 562
One parent born in Norway, one in non-EEA-country					
short residence time	*3 000*	*2 786*	*2 487*	*2 362*	*2 246*
long residence time	4 727	4 880	5 166	5 176	5 040
One parent born in EEA-country, one in non-EEA-country					
short residence time	*807*	*746*	*782*	*777*	*734*
long residence time	450	570	629	661	728
Both parents born in non-EEA-country					
short residence time	*6 757*	*7 226*	*7 593*	*7 653*	*7 556*
long residence time	5 844	6 241	6 452	6 622	6 754
Total number of children	110 227	110 292	110 337	107 851	110 209
Children in treatment group*	*10 564*	*10 758*	*10 862*	*10 792*	*10 536*

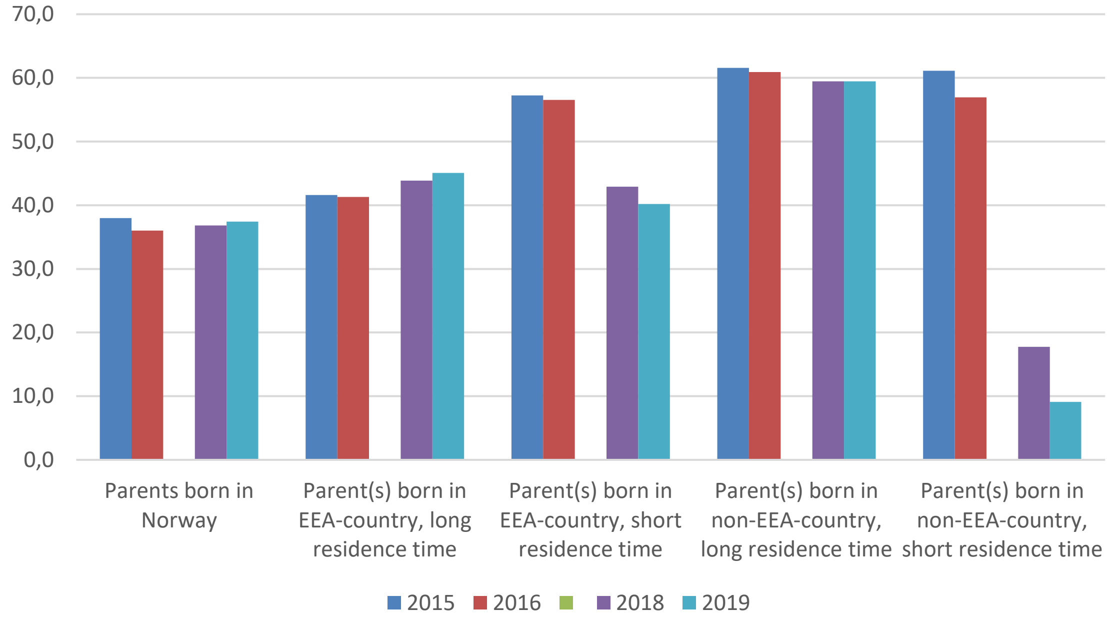
Notes: * Year refers to the income year and includes children aged 13 to 23 months in each given year.
** The treatment group consists of the sub-groups in *italic.*

As we see in Table 1, over 10 500 children aged between 13 and 23 months were not eligible for the cash-for-care benefit in 2018 and 2019 due to the introduced residency requirement. Interestingly, in both years this includes also about 2 300 children with one Norwegian born parent and over 700 children with a parent born in an EEA-country, as their other parent was born in a non-EEA country and has lived less than 5 years in Norway. Thus, the reform also affected parents (and children), that may not have been a direct target group of the implemented reform in 2017.

In our descriptive analysis of the development of the household income, we differentiate between the treated group, children with a least one parent from a non-EEA-country not fulfilling the residency requirement (short-residence time), and four control groups; (i) children with Norwegian born parents, children with parent(s) from EEA-countries with (ii) short- and (iii) long residence time, and (iv) children with parent(s) from non-EEA-countries fulfilling the residency requirement when the child turns one year.

Figure 2 displays the proportion of households that received the cash-for-care benefit before and after the reform in these five groups. Due to a planned reform in the processing of cash-for-care applications, The Norwegian Labour and Welfare Administration did not deliver solid data on cash-for-care uptake to Statistics Norway for 2017 (see Sandvik & Gram, 2019), and this year is thus not included. In addition, this is the year when the reform was implemented, and our annual data do not indicate in which month of the year the cash-for-care benefit was paid out. As expected, we see a sharp decline in the treatment group, in which at least one parent of the child does not fulfil the residency requirement when the child turns one year. Still, some households receive the cash-for-care benefit in 2018 (and to a lower extent in 2019), which might have various explanations. Firstly, parents that already received the cash-for-care benefit before the reform was implemented, where not affected even if they did not fulfil the residency requirement (Lima et al., 2020). Secondly, they may fulfil the residency requirement at a later point in a given year. Thirdly, a household may include another child actually eligible for the cash-for-care benefit (e.g., children where both parents fulfil the requirement). Finally, it cannot be ruled out that we assign some parents to the treatment group that were actually eligible for the cash-for-care benefit (for example non-EEA citizens that have lived in other EEA-countries before moving to Norway).

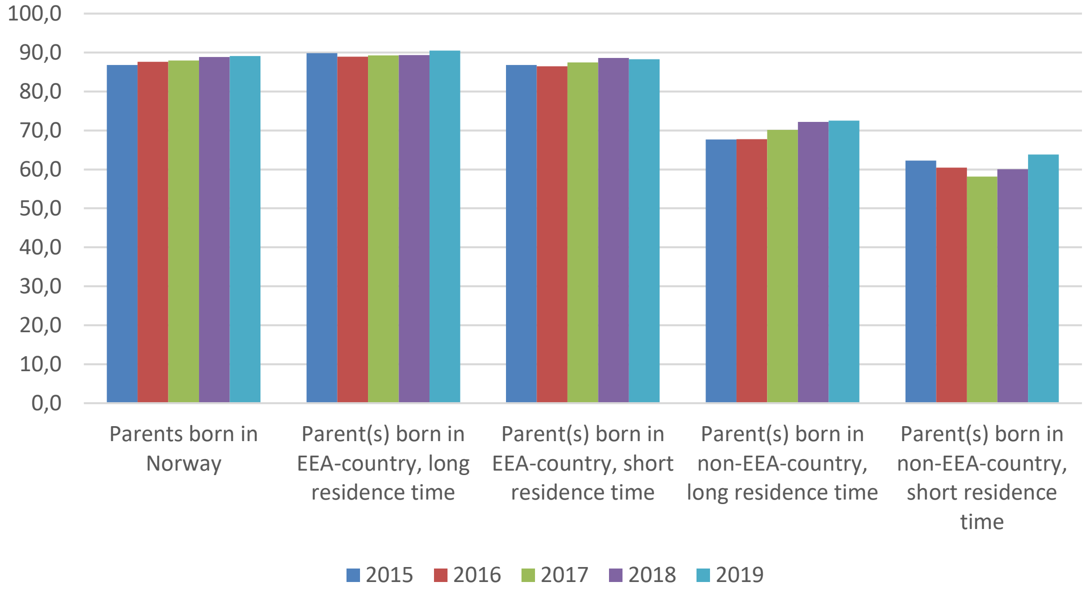
Figure 2. Proportion of households receiving cash-for-care by parent groups, 2015-2019*



* The authors had no access to data on cash-for-care payments in 2017 (for details, see Sandvik & Gram, 2019).

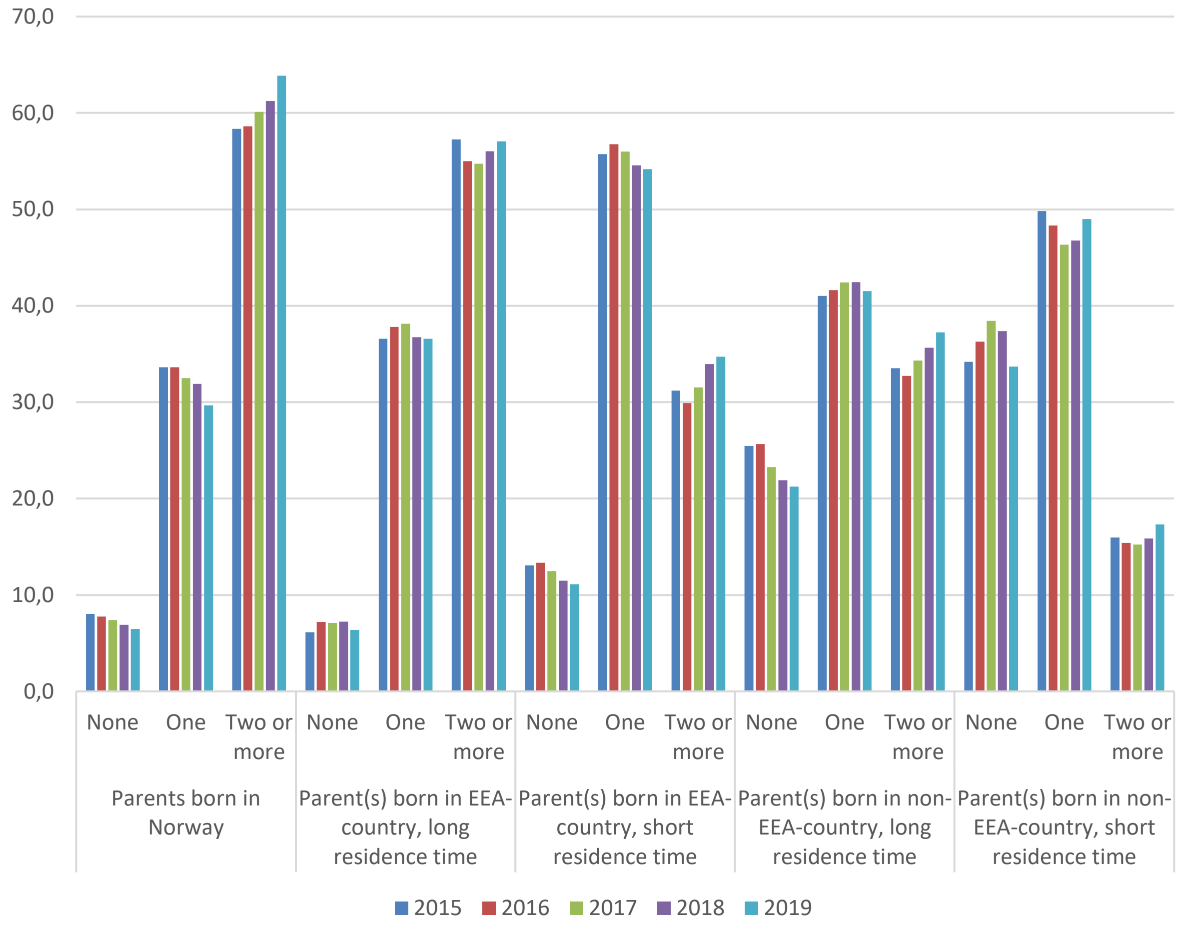
When the reform was announced in 2017, the Ministry of Children and Families underscored that immigrants should participate actively in the labour market as soon as possible after their immigration to Norway (Ministry of Children and Equality, 2017), suggesting that the cash-for-care programme may be used as a temporary substitute for labour income by immigrant parents, deferring or delaying their integration. Lima et al. (2020) found no significant increase in labour market participation, including labour market related qualification measures, among mothers and fathers affected by the reform. Instead of separate analysis for mothers and fathers, we apply a household perspective and describe first if labour income is the main source of income of the households including one-year-old children (Figure 3) and secondly, apply a measure capturing the number of employed persons per household (Figure 4). In the latter case, a person is defined as employed if she/he has income from employment or self-employment that is greater than twice the Basic Amount of the National Insurance Scheme (for example 2*93 634 NOK in 2017).

Figure 3. Proportion of households with one-year-old children with labour income as their main source of income, 2016-2019



For about 90% of the households in the first three control groups (parents born in Norway or in EEA-countries with long or short residence time), labour income is the main source of income (see Figure 3). This is also true for parents from non-EEA-countries, but at lower levels. Among those with a long residence time, about 70% of the households have labour income as their main source of income, while this is true for about 60% of those in the treatment group (parents born in non-EEA-countries with short residence time). In addition, the proportion with labour income as the main source of income increases slightly in all four control groups. In contrast, this proportion declines somewhat in the treatment group in the years 2016 and 2017, before it again increases in 2018 and 2019.

Figure 4. Number of employed household members in households with one-year-old children (percentages), 2015-2019



The number of employed persons in each household (see Figure 4) are in line with this pattern. In the control groups, we observe a decline in households without employed persons. In the treatment group, the proportion without employed persons is not only higher than in all control groups, but also increases from 2015 to 2017 and then decreases in the two subsequent years. The proportions with one or several employed persons follow the opposite pattern. Taking into account only the period since 2017, it may seem that the introduction of the residency requirement had a positive impact on the labour market of the affected households. However, the proportion of employed parents (Figure 4) and households with labour income as their main source of income (Figure 3) is in 2019 only slightly higher than in 2015. This indicates that the increase in employment in 2018 and 2019 only balances out the decline prior to the reform.

In addition, compositional changes in the treatment group may have contributed to these changes. Such compositional changes are also pointed out by Lima et al. (2020: 47) and they show, for instance, that mothers from Somalia, Eritrea, Syria, Pakistan and Afghanistan have lower levels of employment than mothers from the Philippines or Thailand. Taking into account the country background of both parents together, we also find some compositional changes among children with at least one parent from a non-EEA country and a short residence time, which is our treatment group (see Table 2). Not only do we observe a decline of parents from the Philippines and Thailand, which is even stronger if we only look at women, but we see a particularly strong decline among parents from Somalia. The share of parents from Syria and Eritrea increased, however, in our study period.

As the treatment group is relatively small, such compositional changes can have an impact on the developments in the income situation or other outcomes, as we will discuss in more detail in the analysis on fertility.

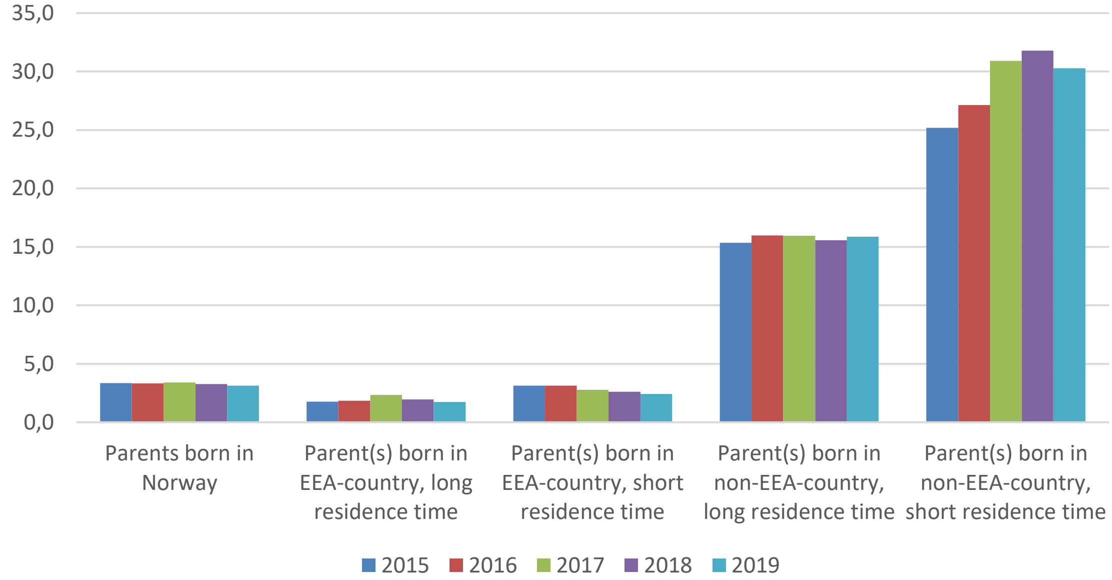
Table 2. Parents country background (treatment group) before and after reform, percent

	2015	2016	2017	2018	2019
Somalia	9.0	8.7	8.3	6.4	4.4
Eritrea	5.2	5.9	7.0	8.6	9.6
Pakistan	4.3	3.9	3.8	3.8	3.6
Irak	4.0	3.5	2.9	2.6	2.8
The Philippines	3.9	4.0	3.8	3.7	3.5
Afghanistan	3.6	3.2	3.0	3.3	3.5
Syria	3.1	7.6	11.3	13.2	13.8
India	3.0	2.9	3.1	3.3	3.3
Ethiopia	2.5	2.5	2.3	2.5	2.5
Russia	2.3	2.0	1.9	1.8	1.5
Thailand	2.3	2.0	1.8	1.5	1.5
Turkey	2.2	2.0	1.8	1.8	1.9
Kosovo	2.1	1.9	1.9	1.8	1.9
Iran	1.9	1.9	1.7	1.5	1.5
China	1.7	1.6	1.6	1.5	1.4
Vietnam	1.5	1.3	1.1	1.2	1.2
Sudan	1.4	1.6	1.7	2.0	2.4
USA	1.4	1.3	1.1	1.2	1.2
Morocco	1.4	1.2	1.1	1.2	1.2
Bosnia-Herzegovina	1.3	1.5	1.4	1.3	1.4
Brazil	1.2	1.0	1.0	0.9	0.9
Serbia	1.1	1.5	1.5	1.7	1.9
Ukraine	0.9	1.0	1.1	1.0	1.0
Albania	0.5	0.6	0.7	0.8	0.9
Nepal	0.4	0.4	0.5	0.6	0.6
Missing*	3.3	2.9	2.6	2.1	1.6
Norway**	13.9	12.6	11.1	10.6	10.4
All other countries	20.7	19.8	18.9	18.2	18.8
Total	100	100	100	100	100

Notes: * The group of missing consists mainly of non-registered fathers.
** Children with a parent born in Norway can be part of the treatment group if the other parent is a non-EEA immigrant with a short residence time.

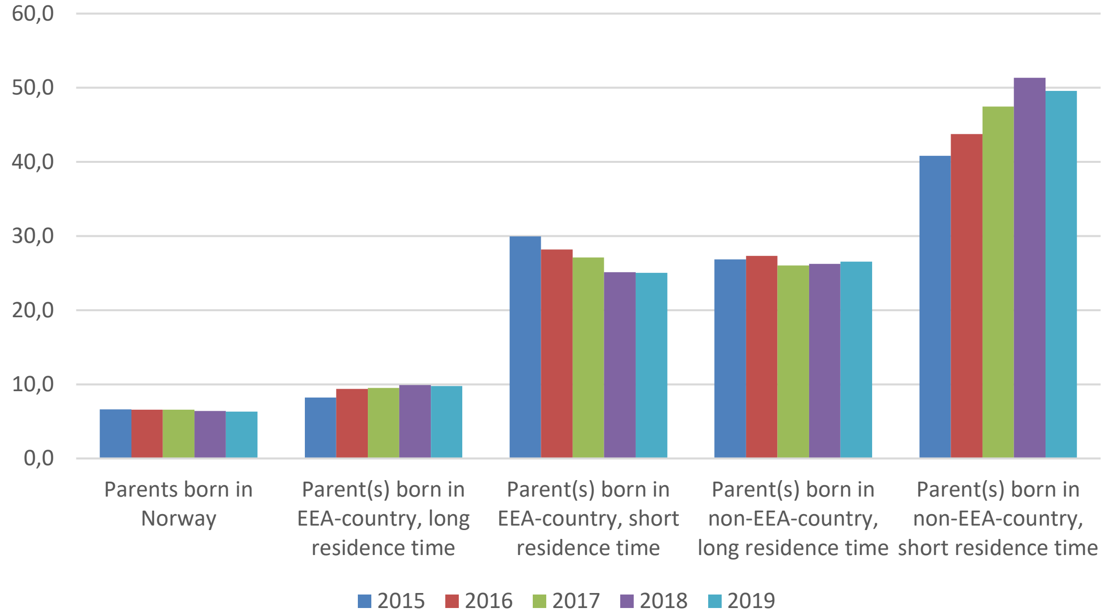
Examining the possibility that the shortfall of the cash-for-care benefit is compensated by other social transfers, Lima et al. (2020) consider if the proportion of women receiving social assistance has changed after the introduction of the reform but find no substantial change. Applying again a household perspective, we compare the proportion of households receiving social assistance of at least 1 000 NOK in each year, distinguishing between the treatment group and the four comparison groups (see Figure 5). While the proportion for this indicator is very stable in the four comparison groups, we observe a certain increase in households receiving social assistance in the treatment group in the period from 2015 to 2018, followed by a decline in 2019 compared to 2018. Given the shape of this development, it appears to be unconnected to the cash-for-care reform, which was implemented by July 2017. Possible compositional changes in the treatment group, as a higher proportion of newly arrived immigrants in 2016-2018, seem to play a stronger role than the cash-for-care reform.

Figure 5. Proportion of households with one-year-old children receiving at least 1 000 NOK social assistance, 2015-2019



Overall, these descriptive results (Figures 2 to 5) provide no evidence for a systematic compensation of the shortfall of the cash-for-care benefit in the treatment group through increased labour market participation or higher social assistance transfers. Lima et al. (2020) conclude that most of the affected families can rely on parental income and thus do not have to apply for additional social assistance, even if they have lost up to 82 500 NOK of income through the cash-for-care scheme due to the reform. Still, such an income loss may be substantial for some households and thus increase the proportion of low-income families in the treatment group. To detect if this is the case, we first apply the Eurostat definition of low-income households, EU60 (Figure 6). In this measure, the annual low-income threshold is set to 60 percent of the median after-tax income per consumption unit, in our case households. After-tax income per household equals the total household taxable and non-taxable incomes, minus taxes, divided on the number of consumption units in the household. For the calculation of consumption units, the first adult is given a value of 1, any additional adult is given the value of 0.5, and each child is given a value of 0.3 (Statistics Norway 2022e).

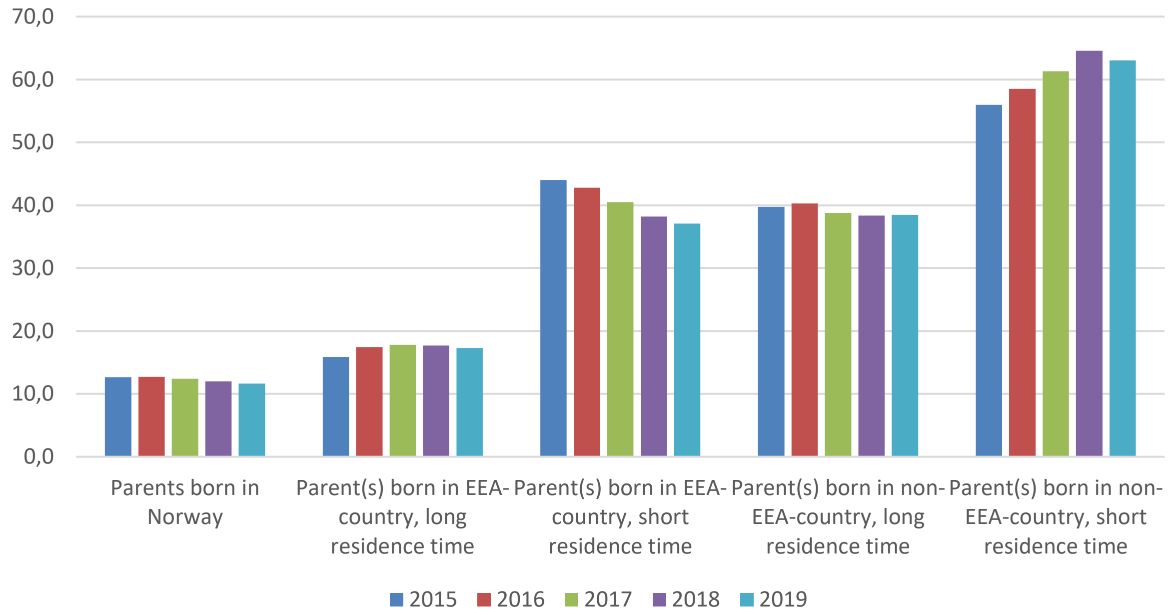
Figure 6. Proportion of households with one-year-old children and low-income (EU60-scale), 2015-2019



We see quite marked differences between the different type of parents, which only partly reflect the proportion of households with labour income as their main source of income (cf. Table 3), as for example the proportion of low-income households among parent(s) born in EEA-country with short residence time, is rather high. Most relevant for our purpose, however, Figure 6 indicates that the proportion of low-income households is relatively stable in the four comparison groups or at least do not follow a clear common trend. In the treatment group, the proportion of low-income groups is higher than in the comparison groups (partly due to the lower labour market participation). The proportions increase in the years 2015-2018 but declines somewhat in 2019. As the cash-for-care reform was implemented in 2017, these descriptive results do not indicate any direct or strong impact of the reform on the proportion of low-income households in the treatment group.

We achieve similar results, plotting the proportion of households in the lowest income quartile (Statistics Norway 2022f) in Figure 7. Also here, the total annual household-income (labour income, capital income and social transfers) is taken into account. The increase of households in the lowest income quartile in the treatment group started before the reform was announced or implemented and declines in the last year.

Figure 7. Proportion of households with one-year-old children in the lowest income quartile, 2015-2019



Overall, our descriptive findings are in line with existing research results, indicating no impact of the cash-for-care reform on labour market participation or household income in the treatment group. However, our results do suggest that the treatment group is overrepresented among low-income households.

Part 2. Birth rates before and after the cash-for-care reform

An unintended side-effect of the reform might be that individuals and couples affected by the reform who intended to use the cash-for-care benefit may have postponed their childbearing plans until they fulfil the residency requirement of 5 years, or abandon their childbearing plans completely. If we observe a decline in birth rates in the treatment group, this may also explain the missing labour market impact of the withdrawal of the cash-for-care benefit on income among treated households (see part 1 of our analysis). Thus, the aim of this second part of our analysis is to detect if a decline in fertility can be observed among those affected by the reform. We first describe the data sources, methods, and our analytical approach, before we present and discuss our results in more detail.

Data

We take advantage of administrative register data provided by Statistics Norway, covering the complete resident population of Norway. In a first step, we construct a dataset including all resident women in childbearing ages (18 to 45 years) in the period 1 January 2014 to 31 December 2019. Next, we differentiate between parity transitions, as previous research on childbearing behaviour has shown substantial differences in the decision-making process depending on the number of children one already has (Miller, 1995). Especially the decision to become a parent for the first time can be based on different motivations (e.g., general wish to be a mother) than to have another child (e.g., wish that children grow-up together with a sibling). In line with diverging motivations between childless women and mothers, the relevance of the cash-for-care benefit may also vary across parities. Thus, we construct three different datasets to capture such possible differences. The first dataset includes childless women to examine transitions to a *first birth.* The second dataset captures *higher order parity transitions* (second, third or fourth births), while a third dataset combines both datasets to study *all four parity transitions* together.

Women enter each dataset when they turn 18 years (if resident in Norway at that time), or when they emigrate to Norway, or – in the dataset for higher order parity transitions – one month after the previous birth (e.g., for second births, one month after they gave birth to their first child). Each woman is followed (and contributes with a new observation month) until the month of a birth, until she emigrates from Norway, until she reaches the upper age limit or until she is registered as dead, whichever occurs first. However, each woman may be included in several datasets. After a birth event occurs, a woman does not contribute with further person-months to this specific parity transition, but will not be automatically censored, as she from the following month may be included in analyses of the next parity transition, if she is still resident in Norway and within the defined age-range. Thus, in the combined dataset for higher order parity transitions, a woman having a second child will thereafter be “under risk” for a third birth and again contribute with new person-months to the dataset.

Next, we identify all women that are affected by the residency requirement defined in the reform of the cash-for-care benefit, by considering their country of origin as well as their residence time in Norway. This is a time-varying variable, with the value 0 when a women does not fulfil the defined requirement and the value 1 when a women does fulfil the requirement. Norwegian citizens or women born in Norway are per definition not affected by the reform. In addition, the residency requirement can be fulfilled through a membership in the national insurance of other Nordic or *EEA-countries*, namely: Austria, Belgium, Bulgaria, Croatia, Cyprus, Czech Republic, Denmark, Estonia, Finland, France, Germany, Great Britain, Greece, Hungary, Ireland, Italy, Latvia, Lichtenstein, Lithuania, Luxembourg, Malta, Netherlands, Poland, Portugal, Romania, Slovakia, Slovenia, Spain, Sweden and Switzerland. We do not have access to detailed data on migration histories and citizenship and presume that most immigrants born in another Nordic country or EEA-countries fulfil the requirement, while immigrants in Norway who were not born in one of these countries (*non-EEA-countries*) might be affected by the law adjustment. Our descriptive analysis of cash-for-care uptake (Figure 2) indicates that we capture the treatment group adequately applying this approach. We distinguish between immigrants from non-EEA-countries who fulfil the residency requirement (*long residence-time*) and those that do not (*short residence-time*). With the implementation of the reform in July 2017, residents from a non-EEA country must have lived at least 5 years (or 60 months) in Norway when they apply for the cash-for-care benefit. Children are eligible to the benefit when they are one year old (between 13 and 23 months old). Therefore, the time-varying variable indicating the eligibility for the cash-for-care program switches from 0 to 1 when a person from a non-EEA country has lived at least 48 months in Norway (48 months resident time + 12 months since birth = 60 months or 5 years). If a woman from a non-EEA country gives birth to child at least 48 months after she registered in Norway, she will always fulfil the defined residency requirement when the child turns one year.

It must be noted that according to the law adjustment, both parents must fulfil the residency requirement. This means that we also have to take the characteristics of the male partner and potential father (country of birth and residence time in Norway) into account. Even though there have been made attempts to improve household statistics based on the population register, data on cohabitations among couples without children in the available data sources are imperfect, especially in the beginning of the observation period. Regarding the transition to first birth, we therefore assess the eligibility for the cash-for-care benefit using only the information on the potential mother. Thus, some women that are defined as eligible for the benefit in the analysis of first births may have a partner who does not fulfil the requirements. For second and higher order births, we also capture if a potential male partner (either married or a cohabitant) fulfils the residency requirement. The same definition as for women is applied to identify if the male partners fulfil the requirement or not (from non-EEA country and short residence time). In the case of married persons, the spouse must be registered at the same unique address. In the case of cohabitations, this includes the father of a previous child if registered in the same household. When no partner is identified in the data or in cases where the father is not registered (including for example if the father is unknown, the mother rejects to declare who the father is, the father has never been registered as resident in Norway, or there has been an in vitro fertilisation with an unknown sperm donor), only the mother must satisfy the residency requirement. Table 3 gives a brief overview of the individual-level data, separating between women defined as born in Norway, in EEA-countries, and in non-EEA countries.

Table 3. Descriptive statistics of the three datasets

	First births	Higher order parities (2-4 births)	All births
Person-months	32 956 456	31 732 298	64 688 754
Single persons			
Norwegian born	512 307	578 881	1 091 188
EEA-country	72 871	79 424	152 295
Non-EEA country	87 448	126 993	214 441
Total *N* single persons	672 626	785 298	1 457 924
Births			
Mother born in Norway	107 398	133 556	240 954
Mother born in EEA-country	17 534	17 843	35 377
Mother born in non-EEA-country	22 134	335 22	55 656
Total N births	147 066	184 921	331 987

Methods – A Difference-in-Differences Approach

To estimate the short-term effect of the residency requirement on fertility, we employ a Difference-in-Differences (DiD) approach with the birth of a child as the outcome (or multiple births if relevant). Briefly described, this method is an empirical strategy to identify causal effects in non-experimental settings, in which the value of the independent variable isn't randomized, but affected by an exogenous event, such as a policy reform, such that one or more groups are affected while one or more groups remain unaffected (Angrist & Pische, 2009; Winship & Morgan, 2007). We argue that the introduction of the residency requirement in the cash-for-care benefits scheme satisfy the requirements for such a 'natural experiment'. Without any remarkable public or political discussion ahead, the reform was passed in the Norwegian parliament in June 2017 and was implemented almost immediately, from 1 July 2017. From that point, one-year-old children with a parent form a non-EEA country were only eligible for the cash-for-care benefit, if the parent had lived at least 5 years in Norway. Assuming that the reform has an impact on the decisions-making process related to fertility after its' implementation, our aim is to capture the possible effects of the reform on fertility. Therefore, our DiD-approach compares the fertility change for the *treatment group* around the time of the reform to the fertility change in the *control group(s)* in the same period. Assuming that fertility trends before and after the implementation of the reform would be parallel in absence of the reform, we may ascribe differences in the changes in fertility rates to the withdrawal of the cash-for-care benefit for the treatment group. Note that since the decision making (and other necessary actions) associated with conceiving a child are usually done at least around nine months prior to birth (except in cases of premature births), we set the reform time to 01. April 2018, treating those having children before this as untreated.

Formally, our DiD models, based on monthly data, aggregated to month and treatment group-level, are estimated as linear regressions, and can be expressed as:

$$Birth\ rate_{tg} = \alpha + \beta_1 \times Month_t + \beta_2 \times Group_g + \beta_3 \times Treated\ post_{tg} + \varepsilon_{tg}$$

where *Birth rate* is a variable measuring the number of births per woman in the specific group g and month t ($Birth\ rate_{tg} = Births_{tg}/Women_{tg}$)[1], *Month* is a set of dummies indicating the calendar month, and *Group* is a dummy that takes the value 1 for the treatment group and 0 for the control group. *Treated post* is a dummy variable that takes the value 1 for the treatment group nine months after the reform was implemented (from 01 April 2018 onwards) and takes the value 0 otherwise. α is a constant term, βs are regression coefficients, and ε is our error term. Our coefficient of interest is β_3. All models use frequency weights based on group size and standard errors are clustered on treatment/control groups. The models are estimated separately for each control group, and separately for first births, higher-order parity births and all births combined. Models based on years (see below) use the same setup, but with years as time units, and with each year beginning on 01 April to maintain clear pre- and post-treatment periods.

Analytical approach

Based on the details above, and restricting our sample to those aged 18 to 45, we define our *treatment group* on a monthly basis in the following ways:

> *First birth transition:* all childless women who are immigrants from a non-EEA country, and who have been registered as resident in Norway for less than 48 months (short residency)
> *Higher order parity transitions:* all individuals and couples where the female or male partner (or both) is an immigrant from a non-EEA country who has been registered as resident in Norway for less than 48 months (short residency) and has not had a child in the previous month

[1] Births are the number of births in the time interval, while the number of women is measured at the beginning of the time interval.

Since we have access to rich population-wide data, we may distinguish between several different control groups that may serve as relevant comparisons. We apply different control groups, as the fertility behaviour of immigrants has a distinct development over time, including that the residence time of immigrants itself is relevant. We define four different and partially overlapping control groups, based on specific restrictions related to country of origin and residence time (see Table 4). In comparison A, we compare the treated group to all other women (or couples). In comparison B, the control group consists of only immigrant women (or couples) who are immigrants, either from an EEA country or who originate from a non-EEA country but have residency exceeding 48 months, to capture the possible impact of being an immigrant. In comparison C, the control group consists of women (or couples) who are immigrants form a non-EEA country but have a residency exceeding 48 months. This is to account for potential systematic differences in fertility trends between immigrants from EEA and non-EEA countries. Finally, in comparison D, the control group consists of immigrant women from EEA countries who have a residency of less than 48 months. This is to account for potential systematic differences in fertility trends associated with length of residency. Note that individuals or couples may move from the treatment group to the control group after 48 months of residency. Still, the treatment and control groups are stable over time in the sense that they include individuals that, in addition to our sample restrictions, either meet or do not meet the eligibility criteria, not in the sense that the groups consist of the same individuals over time.

Table 4. Analytical approach for the DiD-analysis with four comparisons

	Treatment group	**Control group**
Comparison A: Treated vs. all	Non-EEA country, short residency	Norwegian born, EEA-country & non-EEA country w. long residency
Comparison B: Treated vs. other immigrants	Non-EEA country, short residency	EEA-country + non-EEA countries, long residency
Comparison C: Treated vs. non-EEA long residency	Non-EEA country, short residency	Non-EEA countries, long residency
Comparison D: Treated vs. EEA short residency	Non-EEA country, short residency	EEA-country, short residency

Note: For first births transitions, the group-classification is based on characteristics of the women. For second, third and fourth births, also characteristics of potential partners are taken into account when groups are defined. For all birth transitions, the aggregated data for first births are added to the data for higher parities.

Key assumptions

Our identification strategy essentially rests on two key assumptions. First, we assume that fertility trends in the treatment and control groups would be parallel in absence of the reform. This cannot be tested directly, but we assess whether fertility trends are parallel prior to the reform, below. One issue with our design in this regard is that births are relatively rare events, especially when using individual level data with monthly time intervals. Therefore, we first apply aggregated data on a monthly level. This allows us to provide a comprehensive description of the fertility behaviour of the defined groups over time, test if the necessary assumptions for the DiD-models are met and run the DiD-models, while maintaining a parsimonious approach. However, applying aggregated data on a monthly level still results in noisy trends, and we have attempted to work around this issue by aggregating to 12-month time intervals (with each interval beginning on 01 April). This reduces the noise considerably, but it does not solve the more important issue; the fertility trends are actually diverging prior to the reform. As we discuss more detailed below, fertility in the treatment group generally increased prior to the reform. In contrast, fertility declined in the control groups (or remained stable/increased only slightly in the case of Control group D, which consists of immigrants from EEA-countries with short residency). Formal tests of parallel trends reveal that the parallel trends assumption does not hold for any comparison with any control-group (see Table 6). This is a major concern for the interpretation of our results, that we return to below.

Second, and this is strongly related to the first point, we assume that there are no other, time-varying factors that may cause non-parallel shifts in fertility around the time of the reform. Such factors may include other reforms or other events in society that differentially affected the fertility of the treatment and control groups. We are not aware of any such specific external changes or events but cannot rule out this possibility. In addition, and more importantly, such factors may also include change over time in the composition of the treatment and control groups. This especially pertains to the age- and country of origin composition of the treatment group.

If, for instance, women from origin countries with relatively high fertility make up a larger share of the treatment group prior to the reform than after the reform, this may lead to a downward bias in our estimates of the reform effect. Such compositional changes in the treatment group may come about because immigration from non-EEA countries fluctuates substantially over time. 'Peaks' in the migration flow from specific countries will cause a period where these migrants are overrepresented in the treatment group, until they transition into the control group as they become eligible for the benefit. In the first part of our analysis, we have already described that such a compositional change among parents with a one-year-old child. We observed a decline of parents from Somalia, The Philippines and Thailand, while the proportion of parents from Syria or Eritrea increased (see Table 2). In this analysis we consider women (and their partner) 'under risk' for having a first child (or a second, third or fourth birth). Comparing the country background of all women in the treatment group prior and after the reform, reveals a similar compositional change (see Table 5). Women from Somalia contribute with a higher share to the treatment group before the reform, while the proportion of women from Syria is substantially higher after the reform due to changes in the composition of the immigration flows to Norway.

Table 5. Country background of women in the treatment group before and after the reform (person-months, all birth transitions), for the 25 largest country of origin groups and all other countries of origin combined, percent.

	Before the reform	After the reform
The Philippines	14.9	12.7
Thailand	7.4	7.0
Eritrea	6.7	6.1
Somalia	5.3	1.5
Syria	5.2	11.5
India	4.1	5.0
Russia	3.8	2.8
China	3.3	2.8
Iran	3.1	2.7
Ukraine	2.7	2.5
Afghanistan	2.6	2.3
Ethiopia	2.5	1.9
Serbia	2.4	3.2
USA	2.4	2.7
Brasil	2.3	2.4
Pakistan	2.3	2.4
Vietnam	1.9	2.0
Irak	1.7	1.8
Sudan	1.4	1.4
Bosnia-Herzegovina	1.2	1.2
Turkey	1.1	1.4
Nepal	1.0	1.3
Morocco	0.9	1.0
Albania	0.8	1.5
Kosovo	0.9	0.9
All other countries	18.4	17.9

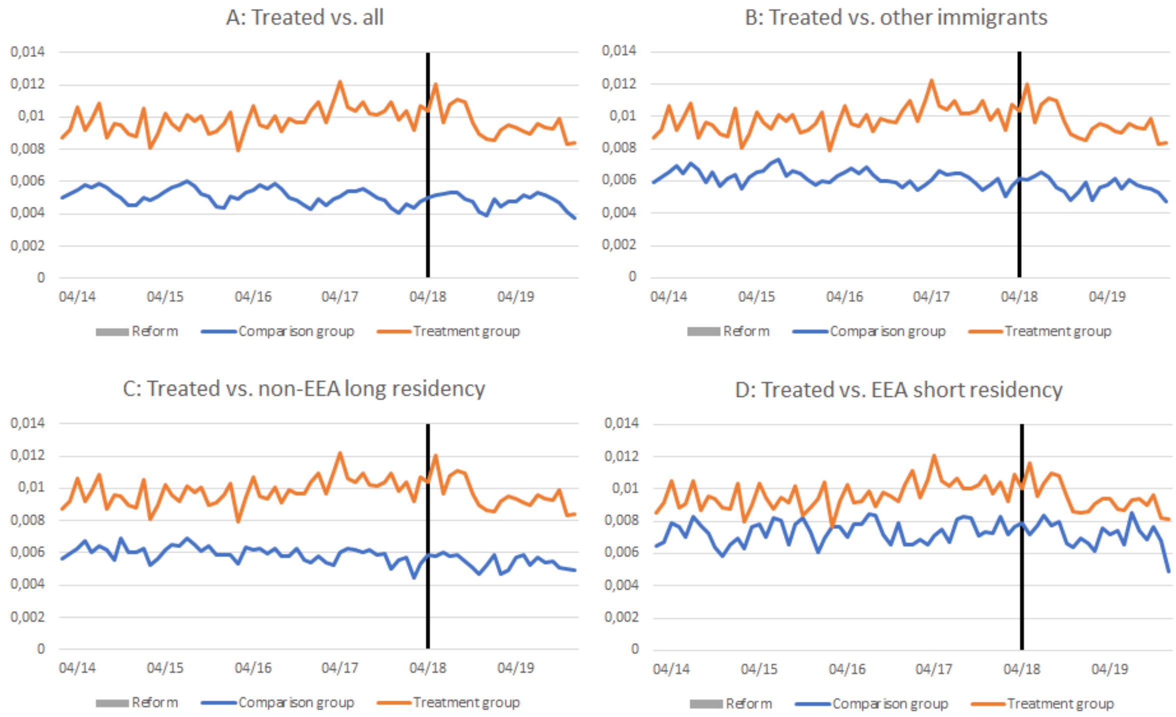
Since such changes in the composition of the groups may cause non-parallel trends in fertility, and bias our results, we have also tested models based on individual-level data, where we control for age and country of origin, as a robustness check.

Descriptive results

Before we present the results of the DiD-models, we describe the development in birth rates. Figure 8 aggregates all four parity transitions and displays the pre- and post-reform trends in births rates on a monthly basis for Comparison A-D. Corresponding Figures are documented in the Appendix for first births (Figure A1) and higher order parities (Figure A2).

The Figures themselves may serve as a descriptive summary of our findings regarding the potential impact of the reform on the childbearing behaviour of the treatment group, and a visualization of the pre-treatment trends in all groups and the possible reform effect. Regarding the first point, we observe that the birth rate for the treated group does not undergo a clear trend-shift at the cut-off point in April 2018, which is nine months after the reform entered into force. Right after this point, we first see a peak in the birth-rate, which most likely reflects a typical seasonal upswing with more births during the summer months. This is followed by a drop in the months thereafter, again an upswing, then a certain decline, but not lower than in previous years.

Figure 8. Aggregated monthly birth rates (first to fourth birth) of the treatment group versus the four comparison groups



Regarding the second point, the pre-treatment trends, we observe that birth rates of the treated group compared to the untreated groups do not follow a strictly parallel trend before the reform was implemented in any of the four comparisons. By including all four parity transitions at the same time, we incorporate as many observations and births as possible (see Figure 8). In Comparison A, the line for the untreated group (which includes all women not affected by the reform), shows a clear pattern with seasonal variation in birth rates: more children are born during spring and summer months compared to autumn and winter months. In addition, we observe a slight decline in birth rates over the years. This is in line with the decline in the total fertility rate during the observation period (Andersen, 2021). In comparisons B-D, we restrict the untreated group to specific women and couples. With fewer observations and births per months, the systematic seasonal variation and long-lasting decline in birth rates are less clear. Thus, Figure 8 suggest that the DiD-assumption requiring parallel pre-trends prior to the reform is not to be met in our data. Overall, the patterns are similar when we differentiate between first births (Figure A1 in the appendix) and higher order parity transitions (Figure A2 in the appendix), as in all cases pre-trends are not parallel. Despite this, we have opted to present the DiD estimates, as they may say something valuable about the potential magnitude of the treatment effects. We do, however, emphasise two important points. First, these estimates should not be given a causal interpretation, because of the issues related to the parallel-trends assumption, and due to changes over time in the composition of the treatment and control group, which is one of the causes of the violation of the parallel-trends assumption. Second, our analyses based on individual-level data presented below, suggest that our estimates are biased by such compositional effects, as individual-level analyses accounting for compositional changes produce estimates of the reform effect that are very close to zero.

DiD models with aggregated monthly and annual data

Our conclusion drawn from the descriptive analysis that the DiD-assumption of parallel pre-trends is not met, is confirmed in formal pre trend tests using the ‘didregress’ package in Stata (StataCorp, 2021) (see Table 6; note that the null hypothesis, which we reject in all model specifications, is that trends are parallel). Table 6 displays the results based on our aggregated monthly data including all four birth transitions.

Table 6. DiD-models with aggregated monthly data (first to fourth birth transitions)

	Comparison A: *Treated vs. all*	Comparison B: *Treated vs. other immigrants*	Comparison C: *Treated vs. non-EEA long res.*	Comparison D: *Treated vs. EEA short residence*
Reform = 1	0.000110*	0.000334*	0.000302	-0.0000373
	(0.00000198)	(0.0000172)	(0.0000349)	(0.0000335)
Observations	142	142	142	142
R-squared	0.974	0.953	0.956	0.877
Pre trend test (p-value)	0.0005	0.0048	0.0088	0.0130

Notes: Standard errors in parentheses / * p<0.05, ** p<0.01, *** p<0.001

Regarding Comparison A, we find a difference of 0.00011, which corresponds to a 0.01 percentage point increase in monthly birth rates for the treated group relative to the untreated group after the reform. This equates to 0.00132 (0.00011*12) more births per woman annually, assuming that all births are single births. Similar small differences are found in Comparisons B and Comparison C, while Comparison D (comparing the treated with EEA-immigrants with similar short residence time) indicates a slight, but non-significant decline of births in the treated group, compared to a control group of EEA-immigrants with short residence. Running similar models separately for first births (see Table A1 in the appendix) and second to fourth birth transitions (see Table A2 in the appendix), suggest that the small observed increase in Table 6 for Comparison A and Comparison B is mainly driven by first births, while the decline in Comparison D is related to higher order births. Overall, the possible impact of the reform on fertility remains very small in these parity-specific models applying aggregated monthly data. Despite the violation of the parallel-trends assumption, these analyses do suggest that the potential effects of the reform are very small.

Next, we aggregate the monthly data to annual data, in an attempt to bypass the strong impact of fluctuating seasonal variation on the trends and reduce the noise in our data. We treat the March/April transition as the start of a year for these annual data, as this allows us to keep the cut-off point for the possible impact of the reform on births rates from April 2018. Thus, we end up with four aggregated pre-reform years (04.2014-03.2015; 04.2015-03.2016; 04.2016-03.2017 and 04.2017-03.2018) and one post reform year (04.2018-03.2019). This reduces the noise in birth rates considerably (see Figure A1 in the appendix), but it does not solve the more important issue; the fertility trends are not parallel but diverge prior to the reform. While fertility in the treatment group generally increased prior to the reform, both for first and higher-order parity births, it declined in the control groups (or increased only slightly, in the case of Control group D; immigrants from EEA countries with short residency). When we apply aggregated annual data, formal tests of parallel trends reveal again that the parallel pre-trend assumption is not met in any comparison (see Table 7). Again, this is a major concern for the interpretation of our results, that we return to below. Otherwise, the results of the DiD-models based on the annual aggregated data (see Table 7) are mainly in line with the results from the model using aggregated monthly data (see Table 6). Comparing the treated group to the control groups A-C, indicate a slight increase in birth rates for the treated group after the reform. However, all in all, these estimates are relatively unstable and indicate only minimal changes in fertility around the time of the reform. Still, it is unexpected that these results rather indicate a slight increase for the treated group after the reform. As a robustness check, we ran separated models for first births and second to fourth births based on the aggregated annual data. Again, we find that there is a slight positive impact of the reform in Comparison A and D for first births. But the models applying the annual data suggest also a positive impact for higher-order births in Comparison B and C, which we did not find when applying aggregated monthly data. Still, the differences are only minimal and should not be given a causal interpretation, as they may also reflect compositional changes.

Table 7. DiD-models with aggregated annual data (first to fourth birth transitions)

	Comparison A: *Treated vs. all*	Comparison B: *Treated vs. other immigrants*	Comparison C: *Treated vs. non-EEA long res.*	Comparison D: *Treated vs. EEA short residence*
Reform = 1	0.00469**	0.00715*	0.00702*	0.00223
	(0.0000272)	(0.000222)	(0.000451)	(0.000316)
Observations	10	10	10	10
R-squared	0.990	0.982	0.982	0.981
Pre trend test (p-value)	0.0005	0.0034	0.0058	0.0122

Notes: Standard errors in parentheses / * $p<0.05$, ** $p<0.01$, *** $p<0.001$

DiD-models with monthly individual-level data

As mentioned above, compositional changes in the treatment group may affect the fertility trends. Our descriptive analysis indicates for example that the proportion of women from Somalia is higher in the treatment group prior to the reform than after the reform, while the proportion of women from Syria increased after the reform was implemented (see Table 5). Previous research has pointed out that immigrant women from Somalia have comparatively high fertility rates in Norway (Tønnessen, 2014).

To assess the potential impact of compositional changes in the treatment group and as a robustness check, we run similar models as displayed in Table 6 with aggregated monthly data, but now on monthly individual-level data. For each comparison, we run two such models, one without controlling for age and country of birth and one controlling for these two characteristics (see Table 8). The results without the controls (in the upper part of the Table 8) are in line with our previous results based on aggregated monthly data (see Table 6). However, after we include country background and age in the model and thus account for possible compositional changes in the treatment group regarding these characteristics, we no longer find any difference between the treatment group and the control group in comparisons A to C, but a slight negative impact of the reform on births for the treatment group compared to EEA-immigrants with similar short residence time (Comparison D). Note that this latter comparison is not ideal in this setting, as no country of origin group is represented in both the treatment and the control group.

Table 8. DiD-models with monthly individual data (first to fourth birth transitions), without and with control for age and country background

	Comparison A: *Treated vs. all*	Comparison B: *Treated vs. other immigrants*	Comparison C: *Treated vs. non-EEA long res.*	Comparison D: *Treated vs. EEA short residence*
		Not controlling for age and country background		
Reform = 1	0.000106**	0.000325*	0.000266	0.0000123
	(0.0000272)	(0.0000127)	(0.0000250)	(0.0000222)
Observations	63785369	13586161	8008591	4346470
R-squared	0.000	0.000	0.001	0.000
		Controlling for age and country background		
Reform = 1	-0.0000274	-0.0000720	-0.0000417	-0.000274*
	(0.0000598)	(0.0000680)	(0.0000744)	(0.0000156)
Observations	63785369	13586161	8008591	4346470
R-squared	0.003	0.002	0.003	0.002

Notes: Standard errors in parentheses / * p<0.05, ** p<0.01, *** p<0.001

We performed similar analyses for first births (see Table A5 in the Appendix) and second to fourth births (see Table A6 in the Appendix). Again, the model results without controls are in line with our previous findings on aggregated monthly data (see Table A1 and A2 in the Appendix), but after control for country background and age, we no longer find any differences between the treatment group and the control group(s) in any of the four comparisons.

Based on the descriptive analyses of the birth rates and DiD-analyses, we conclude that the childbearing behaviour of the treatment group was not substantively affect by the introduction of the residency requirement in the cash-for-care benefit scheme. Although some estimates were statistically significant, they were substantively minor and appear to have been driven primarily by compositional changes, rather than the reform itself.

Conclusion

Our results suggest that the introduced residency requirement for non-EEA immigrants in the cash-for-care benefit had no significant impact on either the labour market participation, the income or childbearing behaviour of immigrants affected by the reform. It might be that children encompassed by the residency requirement were more likely to be enrolled in public childcare than comparable children prior to the reform. However, as we do not have access to detailed information on uptakes of public childcare, we were not able to evaluate this part of the reform. Comparing the total number and proportion of one-year-old children enrolled in kindergartens provide no evidence for a substantial increase in the use of childcare two years after the reform was introduced (Statistics Norway 2022g).

The reform might have been introduced with the intention to improve the situation of the target group, by signalizing to them to defer childbearing in the years immediately after immigration to Norway and instead direct them towards the labour market. However, there are no signs that the reform has any substantial impact on the immediate life course plans of these immigrants. Partly, they moved to Norway due to family unification (Statistics Norway, 2022a). In addition, many of them are refugees, who may already have postponed their childbearing intentions before fleeing from their home country and a comparatively high proportion of them realizes their childbearing desires after settling in Norway. Furthermore, in the years after arrival the lack of relevant education, work-experience, and/or sufficient language knowledge is rather typical for this group of immigrants. Thus, it should not be surprising that most mothers affected by the reform were not employed before they gave birth to their child (Lima et al., 2020). Entering the labour market thereafter still requires overcoming barriers, and further research to examine possible long-term effects is warranted.

Disregarding potential effects on kindergarten use, our results suggest that the reform effectively excluded immigrant origin families with small children from the cash-for-care programme, without having the desired effects on the parents' labour market integration or leading to reduced or delayed childbearing among women encompassed by the reform. At the same time were children in the target group of the reform overrepresented among low-income households, before and after the residency requirement was introduced. Even we did not observe an average increase in poverty or low income due to the reform in our descriptive figures, it is likely that the withdrawal of the cash-for-care benefit had a worsening effect on the economic situation of some of the children affected by the reform.

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Appendix

Tables

Table A1. DiD-models with aggregated monthly data, first birth transitions

	Comparison A: *Treated vs. all*	Comparison B: *Treated vs. other immigrants*	Comparison C: *Treated vs. non-EEA long res.*	Comparison D: *Treated vs. EEA short residence*
Reform = 1	0.000442*	0.000645*	0.000465	0.000358*
	(0.00000600)	(0.0000259)	(0.0000674)	(0.0000146)
Observations	142	142	142	142
R-squared	0.955	0.899	0.915	0.769
Pre trend test (p-value)	0.0018	0.0118	0.0216	0.0168

Notes: Standard errors in parentheses / * p<0.05, ** p<0.01, *** p<0.001

Table A2. DiD-models with aggregated monthly data, second to fourth birth transitions

	Comparison A: *Treated vs. all*	Comparison B: *Treated vs. other immigrants*	Comparison C: *Treated vs. non-EEA long res.*	Comparison D: *Treated vs. EEA short residence*
Reform = 1	-0.000338**	-0.0000296	0.0000319	-0.000659
	(0.000000932)	(0.0000852)	(0.0000137)	(0.0000700)
Observations	142	142	142	142
R-squared	0.969	0.959	0.961	0.882
Pre trend test (p-value)	0.0004	0.0014	0.0027	0.0050

Notes: Standard errors in parentheses / * p<0.05, ** p<0.01, *** p<0.001

Table A3. DiD-models with aggregated annual data, first birth transitions

	Comparison A: *Treated vs. all*	Comparison B: *Treated vs. other immigrants*	Comparison C: *Treated vs. non-EEA long res.*	Comparison D: *Treated vs. EEA short residence*
Reform = 1	0.00806**	0.0108*	0.00960	0.00581**
	(0.0000940)	(0.000402)	(0.00100)	(0.000185)
Observations	10	10	10	10
R-squared	0.978	0.952	0.959	0.941
Pre trend test (p-value)	0.0016	0.0092	0.0168	0.0134

Notes: Standard errors in parentheses / * p<0.05, ** p<0.01, *** p<0.001

Table A4. DiD-models with aggregated annual data, second to fourth birth transitions

	Comparison A: *Treated vs. all*	Comparison B: *Treated vs. other immigrants*	Comparison C: *Treated vs. non-EEA long res.*	Comparison D: *Treated vs. EEA short residence*
Reform = 1	-0.000338**	0.00293*	0.00354*	-0.00366
	(0.00000387)	(0.0000934)	(0.000145)	(0.000436)
Observations	10	10	10	10
R-squared	0.998	0.994	0.994	0.990
Pre trend test (p-value)	0.0002	0.0004	0.0011	0.0138

Notes: Standard errors in parentheses / * p<0.05, ** p<0.01, *** p<0.001

Table A5. DiD-models with monthly individual data for first birth transitions, without and with control for age and country background

	Comparison A: *Treated vs. all*	Comparison B: *Treated vs. other immigrants*	Comparison C: *Treated vs. non-EEA long res.*	Comparison D: *Treated vs. EEA short residence*
Not controlling for age and country background				
Reform = 1	0.000408**	0.000597*	0.000421	0.000328
	(0.00000367)	(0.0000176)	(0.0000427)	(0.00000980)
Observations	32491478	5898738	3274920	2568816
R-squared	0.000	0.000	0.001	0.000
Controlling for age and country background				
Reform = 1	0.000136	0.000174	0.0000555	0.000164
	(0.0000208)	(0.0000352)	(0.0000857)	(0.0000156)
Observations	32491478	5898738	3274920	2568816
R-squared	0.003	0.002	0.003	0.003

Notes: Standard errors in parentheses / * p<0.05, ** p<0.01, *** p<0.001

Table A6. DiD-models with monthly individual data for second to fourth birth transitions, without and with control for age and country background

	Comparison A: *Treated vs. all*	Comparison B: *Treated vs. other immigrants*	Comparison C: *Treated vs. non-EEA long res.*	Comparison D: *Treated vs. EEA short residence*
Not controlling for age and country background				
Reform = 1	-0.000326***	-0.00000589	-0.0000375	-0.000501
	(0.00000485)	(0.00000740)	(0.0000114)	(0.0000481)
Observations	31293891	7687423	4733671	177654
R-squared	0.000	0.000	0.001	0.000
Controlling for age and country background				
Reform = 1	-0.000299	-0.000327	-0.000207	-0.000518
	(0.0000675)	(0.0000575)	(0.0000570)	(0.0000675)
Observations	31293891	7687423	4733671	1777654
R-squared	0.003	0.003	0.003	0.003

Notes: Standard errors in parentheses / * p<0.05, ** p<0.01, *** p<0.001

Figures

Figure A1. Aggregated monthly first birth rates of the treatment group versus the four comparison groups

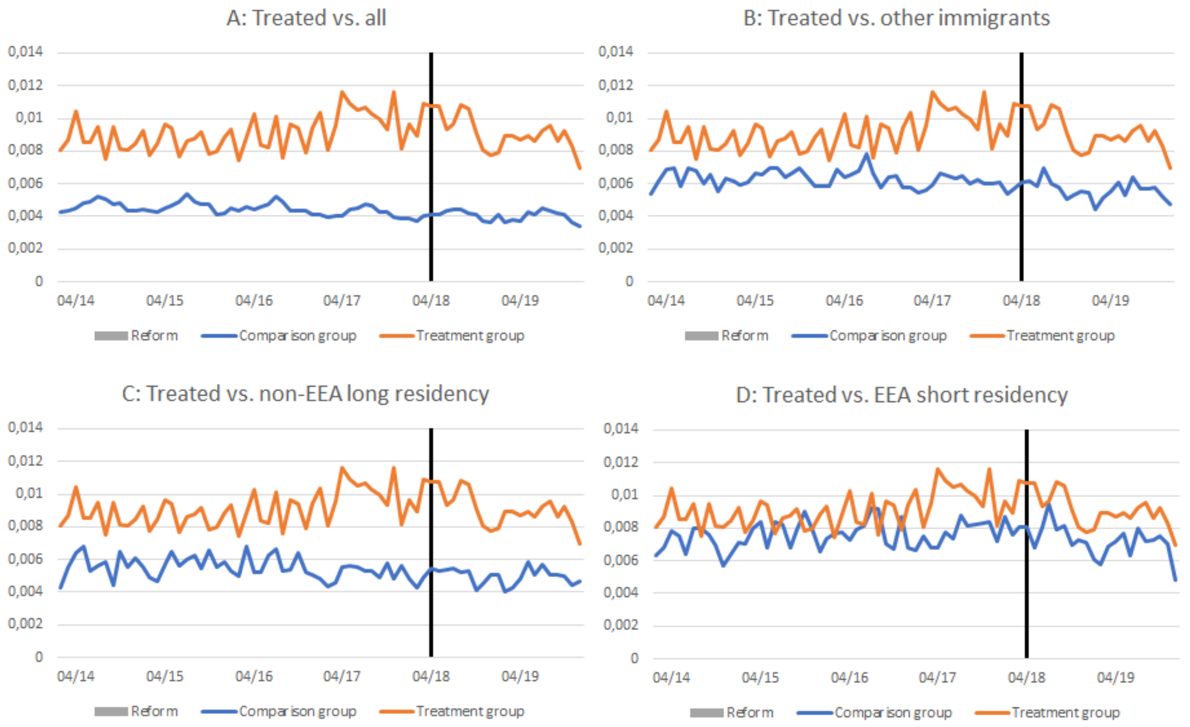

Figure A2. Aggregated monthly higher order parity birth rates (second to fourth birth) of the treatment group versus the four comparison groups

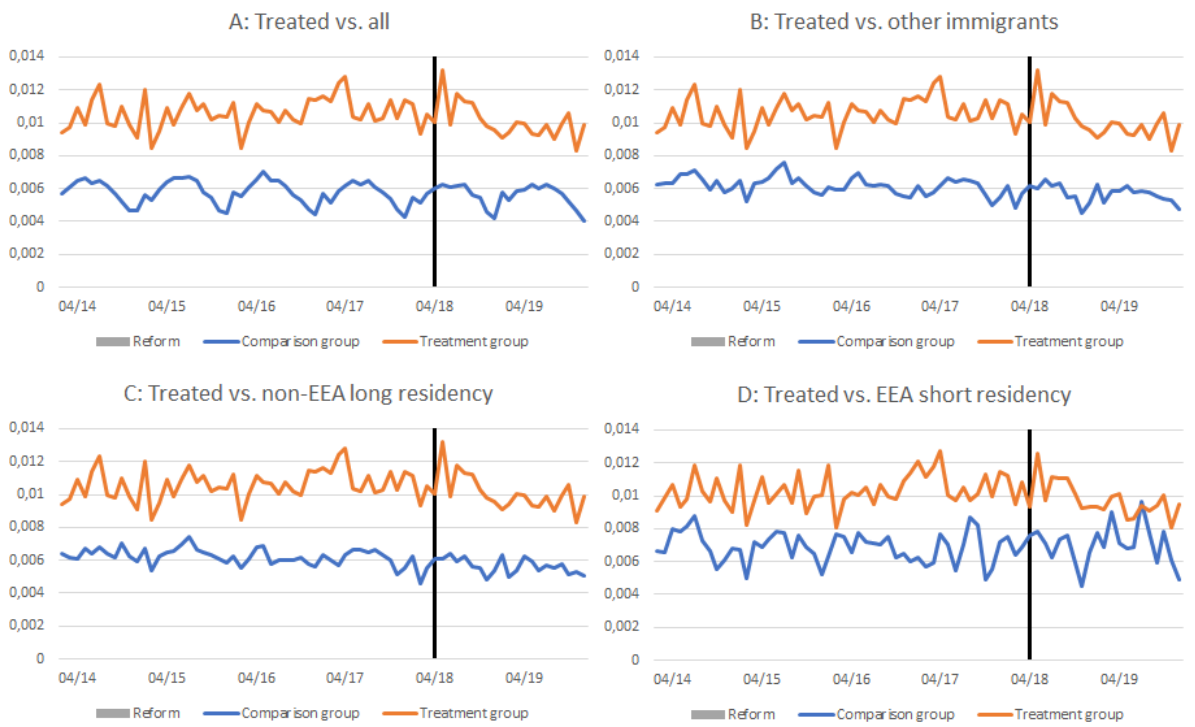

Figure A3. Aggregated annual birth rates (first to fourth parity) of the treatment group versus the four comparison groups

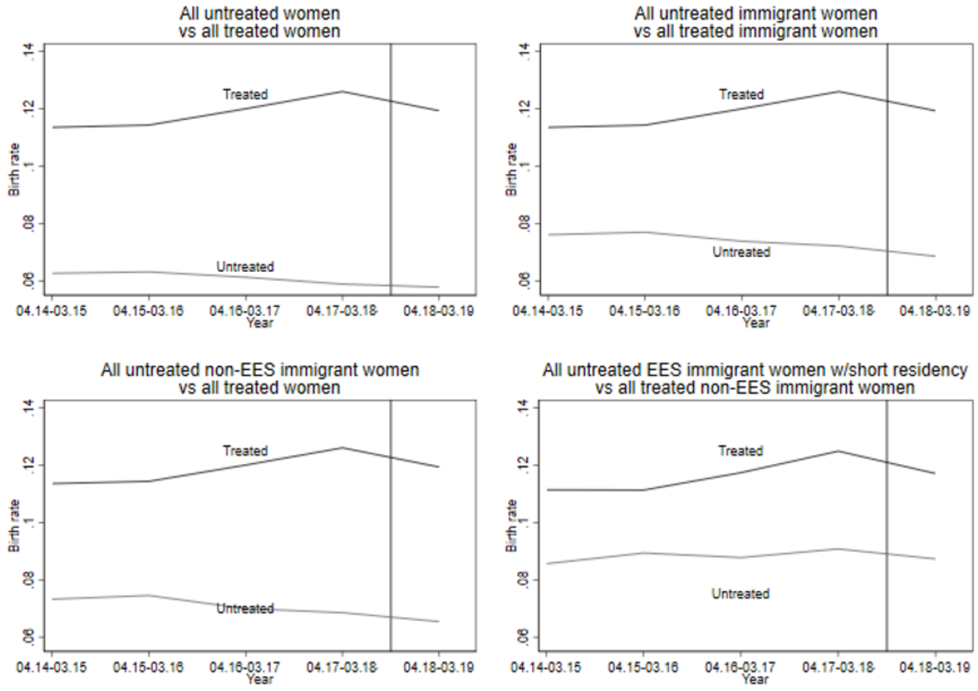

Figure A4. Aggregated annual first birth rates of the treatment group versus the four comparison groups

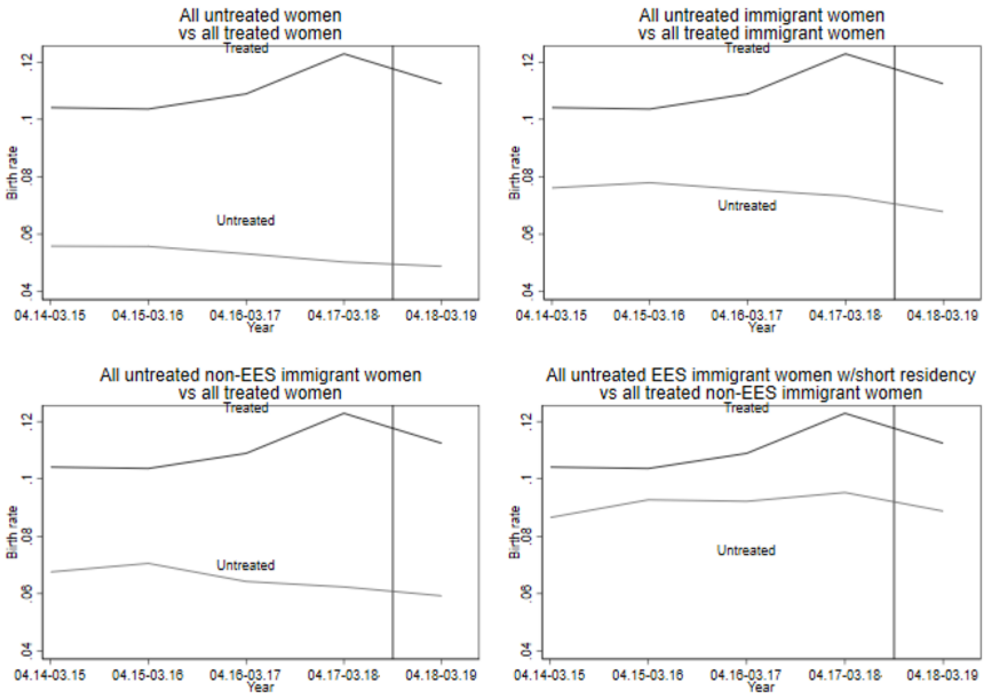

Figure A5. Aggregated annual higher order parity birth rates (second to fourth birth) of the treatment group versus the four comparison groups