Discussion Papers
Statistics Norway
Research department

No. 828
November 2015

Statistics Norway

Statistisk sentralbyrå

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Abstract:
The relationship between income, cost of childrearing and fertility is of considerable political and theoretical interest. We utilize exogenous variation in family income and the direct cost of children to estimate causal effects on fertility. The variation comes from a regional child benefit and tax reform implemented in the northern municipalities of the Norwegian county Troms. The southern municipalities of the same county constitute a plausible and empirically similar control group. Individual-level multivariate analysis suggests that a reduced direct cost of children increases fertility, mainly among unmarried women in their early 20s. We find little evidence of income effects on fertility. Our results are robust to a variety of specifications, including a standard difference-in-difference setup, and regional trend modeling. The findings indicate that lowering the direct cost of a child would shift childbearing to lower ages in Norway. However, a lower price of children is also likely to induce a shift towards non-union childbearing or childbearing in less stable unions.

Keywords: Fertility, Quasi experiment, Income effect, Public policy, Difference-in-difference

JEL classification: J13, J12, J18, H23

Acknowledgements: Previous versions have been presented at the Statistics Norway Research Seminar in 2012, the ``Winter seminar'' of the Norwegian Sociological Association in 2013, the 2013 Meeting of the PAA, and the gratuate students writing seminar at Institute for Sociology and Human Geography, University of Oslo, 2015. We are grateful to participants in these seminars, and Sara Cools, Nina Drange, Øystein Kravdal, Torkild H. Lyngstad, Kjetil Telle, Marianne Tønnesen, Kenneth Aa. Wiik and Arnstein Aasvee for helpful comments on previous drafts. The work was supported by the Norwegian Research Council under Grant number 202442/S20.

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ISSN 0809-733X
Print: Statistics Norway

Sammendrag

Sammenhengen mellom inntekt, den direkte kostnaden ved å oppdra barn og fruktbarhet har betydning for både politikkutforming og samfunnsfaglige teorier om fruktbarhetatferd. Vi bruker eksogen variasjon i inntekt (lønn og barnetrygd), og den direkte kostnaden ved å oppdra barn (endringer i barnetrygd) for å estimere kausale effekter på fruktbarhetsutfall. Variasjone kommer fra en regional reform som reduserte skattenivået og økte barnetrygden i Nord-Troms. Endringene ble ikke implementert i Sør-Troms, og denne regionen kan dermed brukes som kontrollgruppe i et kvasieperiment.

Vi estimerer forskjell-i-forskjellmodeller (difference-in-difference), der vi kontrollerer for observerbare kjennetegn som er felles for Troms og varierer over tid (tidsfasteffekter), og uobserverbare kjennetegn som varierer mellom kommuner men er konstante over tid (kommunefasteffekter). Vi finner at en reduksjon i den direkte kostnaden ved å ha barn økte fruktbarheten blant kvinner tidlig i 20årene. Paritetsspesifikke modeller viser at det hovedsakelig er sannsynligheten for å få et første barn som blir påvirket. Effekten er konsentrert blant ugifte kvinner, en gruppe som består av både samboere og enslige. Funnet er robust til modellering av regionale trender i fruktbarhet. Det er også en antydning til at sannsynligheten for å få et tredje barn øker på grunn av reformene, men dette funnet er ikke robust på tvers av spesifikasjoner.

Lavere skatt vil kunne øke arbeidstilbudet, og slik påvirke fruktbarhet indirekte. Empiriske undersøkelser tyder på at funnene våre ikke er drevet av endringer i arbeidstilbudet.

1 Introduction

Western high-fertility countries typically have a "package" of policies that facilitate childbearing, each often quite costly. For voters, politicians and policy makers who face both low fertility and increasingly severe budget constraints, an exact measure of the pronatalist effects of each policy is crucial. In this study, we assess how changes in income and the direct costs of a child affects fertility the Nordic context, taking Norway as an empirical example.

According to the microeconomic theory of fertility, a reduction in the cost of a child should lead to an increase in fertility (Becker 1960). Kindergartens, which reduce the *indirect* or time cost of children, contribute to the relativly high fertility in Norway (Rindfuss et al. 2010). However, effects of the *direct* or pecuniary cost of childrearing on fertility in the Nordic countries has been explored less. Furthermore, couples or women are expected to respond to an increase in household income either by investing more in each child (Becker and Lewis 1974) or by having more children (Becker 1960). As decisions of work and fertility are jointly determined (see e.g. Francesconi (2002)), estimating such *income effects* from observational data is challenging. For causal estimation, exogenous changes in household income – through increase in cash allowances or tax breaks – is required.

A handful of previous studies have used individual-level data to identify effects of changes in universal child benefits on fertility.[1] Milligan (2005) utilizes a regional increase (and subsequent revocation) of cash transfers to parents in Quebec, using the rest of Canada as a control group in a difference-in-difference design. Cohen et al. (2013) uses parity-specific changes in the level of child benefits in Israel to identify income and price effects on fertility. Both these studies mainly utilize variation in the benefit levels for the *third* child, and are hence unable to give information on the income and cost effects for lower parities. Furthermore, as relatively young women rarely are on the margin to have

[1] There is also a relatively large literature on extensions and revocations of welfare benefits for the low-income population in the US. Studies with a plausibly causal design tend to find no or very modest income and price effects in this population (see e.g. Dyer and Fairlie (2004); Kearney (2004); Fairlie and London (1997); Joyce et al. (2004); Wallace (2009))

a third child, these studies are unable to give an accurate description of how responses to changes in income and the cost of children varies with age.

In this study, we utilize a regional reform implemented in parts of Northern Norway in 1989-1990 to estimate price and income effects on fertility. When fully implemented, the reform increased the universal child allowance by about 3600 NOK yearly, about 575 1990-USD.[2] As the additional allowance was provided also for children already born, this reform induces an income effect among mothers, as well as a reduction in the direct cost of a child at all parities. Furthermore, regional tax breaks implied an exogenous increase in household income in the reform region, providing an additional opportunity to test for income effects on fertility.

We identify effects by comparing women who resided in Northern Troms (reform region) to women who resided in bordering municipalities in Southern Troms (non-reform region). Our analysis is based on detailed, high quality data on fertility, education, income and marital status for the full female population in this region, drawn from various administrative registers. To avoid bias from selective migration, place of residence and all potentially endogenous covariates are measured prior to the reform.

Taking the number of children as the main dependent variable, we estimate reform effects both in a standard difference-in-difference setup, and by parametric modeling of region- and age-specific trends in fertility. In both these specifications, we find that the reform increased fertility among unmarried women in their early 20s. This finding is robust to a battery of robustness checks. We also find some evidence of effects on higher parities, although this is somewhat less robust. The strongest effects are found for the transition to parenthood. In contrast to mothers, there is no pure "income effect" in this group – the income of childless women increase only if they have a first child. Hence, the pattern in effects by parity indicates that the effects mainly are due to a reduction in the direct cost of a child.

Our results indicate that the direct cost of childbearing is among the drivers of fertility postponement in Norway. Relatively small reductions in the direct cost of a child

[2] Using an exchange rate of 6.25 NOK per USD. All conversion rates are obtained from `http://www.norges-bank.no/en/Statistics/exchange_rates/currency/USD/`.

could be expected to translate into a lower mean age at birth. As the increase is concentrated among unmarried mothers, it seems likely that a reduction in the price of a child would lead to a larger proportion of non-union childbearing and/or a larger proportion of children born in more fragile unions.[3]

The remainder of the paper is organized as follows. In Section 2 we outline a theoretical framework, discuss relevant studies, and state expectations of effects. Section 3 describes the relevant reform details. Section 4 presents the identification strategy and the data used in the analysis. The main results are presented in Section 5, robustness checks in Section 6. A concluding discussion is given in Section 7.

2 Theoretical and empirical framework

According to the microeconomic theory of fertility, the demand for children will increase if the cost of raising a child falls.[4] The direct cost of a child – which is the concern of this paper – consists of expenses to clothes, food, equipment and housing, as well as schooling and health care. Governments can reduce the direct cost of raising a child by cash transfers, tax breaks and housing subsidies, and by providing high-quality public health care and schooling (Gauthier 2007). The indirect cost of childrearing equals the earnings loss due to childrearing – including immediate loss due to fewer hours worked, and long-term effects of human capital depreciation (Walker 1995).

The simplest microeconomic model of fertility predicted a positive *income effect* on fertility, i.e. that family size increases in household income, all else equal (Becker 1960). This was later refined to an assumption that the *spending* on children increases in income, but that parents respond to income increases mainly by investing more in each child (Becker and Lewis 1974).[5] Despite the simplicity of the theoretical model of income and fertility, it has proven hard to test empirically. Mainly, higher earnings also makes it

[3]On average, cohabiters consistently have higher dissolution rates that married couples, even when comparing couples with children (Lyngstad and Jalovaara 2010)

[4]In this framework, it is assumed that the demand for children translates directly into actual fertility, i.e. that there are no regulation costs (c.f. Easterlin and Crimmins (1985)). For our purpose, this assumption is a reasonable heuristic, as the reform is unlikely to change the regulation costs of fertility.

[5]In other words, “child quality” is assumed to be a close substitute for “child quantity”.

more costly to take time off to care for a child, making for a negative *substitution effect* of (women's) wages on fertility.[6] This counteracting mechanisms complicates identifying a (potentially) positive income effect.

Variation in money transfers to parents – in the form of cash or tax breaks – provides excellent opportunities to investigate fertility effects of changes in income and the direct cost of children, as there are no complications arising from substitution. Earlier studies based mainly on time series data find weak or no effects (for studies on cash transfers see Ermisch (1988); Walker (1995); Zhang et al. (1994); Gauthier and Hatzius (1997); Kalwij (2010), for an overview of the effects on welfare see Moffitt et al. (1998)). Similarly, while Whittington et al. (1990) finds large fertility responses to tax breaks for parents analyzing time series data, Crump et al. (2011) find substantially smaller and less robust effect using the same data set. However, studies based on time series data are prone to omitted variable bias from correlated trends in benefits levels and fertility that differ by country or region.

Our study contributes to a small but growing body of individual-level studies using exogenous variation in income and the price of children to test predictions from the microeconomic theory of fertility. The most obviously comparable study is Milligan's (2005) comparison of fertility development in Quebec with the fertility development in the rest of Canada. Those findings suggest a quite large effect of benefits on fertility, particularly at second and third parities. Despite the regional variation in child benefits, some serious challenges to identification are apparent. First, the increased cash allowance was introduced partly as a *response* to a regional fertility decline, making the reform introduction potentially endogenous to the outcome (Besley and Case 2000). Furthermore, Milligan (2005) finds some empirical evidence of regional trends in fertility, but has insufficient data to model these. Due to data limitations, these estimates are also vulnerable to upwards bias from selective in-migration of women with higher latent fertility.

Cohen et al. (2013) utilize parity-specific differences and variation in the level of Israeli child benefits over time, finding that while a reduction of the price of the marginal child

[6] Earnings may also be correlated by unobservable personal characteristics that can affect the propensity for childbearing.

has a substantial effect on fertility, the magnitude of the income effect is negligible. The study utilizes (arguably) unexpected increases and revocations of child benefits, which also vary by parity, to estimate effects on fertility. Despite a sound identification strategy, it leaves several questions to be explored. First, Israel has strong pro-natalist sentiments, and estimates may not generalize to less pro-natalist cultures. As the benefits were targeted at increasing fertility, reform effects could be mediated through changes in norms and values regarding childbearing (Jagannathan et al. 2010), making them less informative of the fertility effects of changes in income and costs of children.[7] Finally, as Cohen et al. (2013) mainly utilizes variation in the benefits for the third child, meaningful comparisons of effects across parities are unattainable.

Reductions in the direct cost of children and income increases could affect both *whether* and *when* women have children. Having children shifts some resources from own consumption to spending on children, which is expectedly preferable at higher ages when earnings are higher (see e.g. Happel et al. (1984)).[8] Hence, both reductions in the direct cost of children and income increases should, all else equal, be more important at lower ages. Effects mainly at lower ages, and/or at lower parities, indicate that the reform mainly induces a tempo shift.[9]

While previous study has found that the timing of children is more sensitive to policy changes than completed fertility (Gauthier 2007), there are also compelling reasons that the reform could affect family size. Most importantly, the cash transfers reduce the cost of having a(nother) child relative to the price of all other goods – i.e. both own consumption and child quality. While the income increase induced by the reform could be invested in either child quality or increases in family size, the relatively lower price of the marginal child should also shift this spending towards an additional child. Effects at higher ages – where the potential for postponement or recuperation is small – are indicative of quantum

[7] Jagannathan et al. (2010) finds that reducing in welfare benefits to large families affects fertility through normative pressure rather than economic incentives (see also Gauthier (2007)).

[8] This holds if one cannot borrow freely against the future, and the marginal utility of consumption is diminishing.

[9] This interpretation relies on the assumption that women have a target family size, so that the reform-induced increase in fertility at lower ages will lead to a relative decline in fertility among the treated women at higher ages. Our experimental setup does not allow us to compare the *completed* fertility of treated and untreated women.

effects. In the treated birth cohorts, almost half of the women who become mothers have two children, while about a third go on to have three or more children.[10] Hence, effects at the propensity to have three or more children are indicative of quantum effects.

2.1 Disentangling mechanisms: Subpopulation analysis

Estimating effects in separate subpopulations allows for a better understanding of the mechanisms linking the direct costs of a child to the demand for fertility. First, reform effects could vary by marital status. In our sample, we are able to distinguish between married women and unmarried (i.e. cohabiting, single and divorced) women. As living with a partner before having a child has a practical advantage and is to some extent normatively expected (Thornton and Young-DeMarco 2001), women who are married can respond more quickly to changes in the economic incentives of childbearing. On the other hand, married women on average have better-earnings partners and hence higher household income also compared to cohabiters (Texmon 1999; Petersen et al. 2011). Hence, if the *relative* size of the income increase and price reduction is most important for the reform effects, we expect to see the largest effects among unmarried women.

Both previous research and theory points to the expectation that effects will be concentrated among women with lower educational attainment. On average, women with higher education have higher earnings (and, for married women, higher earning spouses). Hence, the *indirect* cost of childbearing will be larger among highly educated women, while higher household income is expected to make the demand for children less sensitive to changes in the direct cost of children.[11]

With respect to (earned) income, predictions are more involved. At first glance, as lower earnings strongly proxies lower household income (also through earnings homogamy) one would expect the strongest effects among low-earning women. Somewhat anti-intuitively, previous studies have found strongest effects of cash transfers among high-income households (Milligan 2005; Cohen et al. 2013). Cohen et al. (2013) argue

[10]Source: Statistics Norway StatBank `https://www.ssb.no/en/statistikkbanken`, Table 05769 *Number of children distributed by age and cohorts of births (per cent).*

[11]Assuming diminishing marginal utility of consumption, see Happel et al. (1984).

that this could result from non-linearities in the quality-quantity interactions. [12]

Unlike Milligan (2005), we use a measure of women's earnings that is exogenous to the reform. However, particularly when looking at the propensity to have a first child, earnings level does not capture economic resources only. Relatively high earnings may signalize that one has gained a foothold in the labor market, which may correlate with being at a life course stage where one is at the margin to have a first child (Bergnehr (2008), Hart (2015), Pedersen (2014)). Women who are at the margin of having a (first) child may be more readily influenced by relatively small changes in income or the direct cost of a child.

3 Reform details

Our sources of exogenous variation is two regional reforms, implemented in 1989-90, that substantially improved the economic conditions of families and individuals in Northern Troms and Finnmark. Importantly, the reforms were *not* a response to a regional fertility decline. They were targeted at recruiting and retaining high-skilled labor to the region and to improving the labor market for low-skilled workers.[13] As such, this reform is not a "family policy" aiming at increasing fertility. This has two important advantages. First, regional reforms that in part are a response to fertility decline (such as the reform studied by Milligan (2005)) indicate that regional trends in fertility may differ in the first place, raising concern on whether the identifying assumptions required in a difference-in-difference design holds. Second, Jagannathan et al. (2010) show that changes in cash incentives motivated by changing fertility behavior may affect fertility mainly through changes in norms and values. Hence, compared to reforms that are targeted at increasing (or reducing) fertility (such as Cohen et al. (2013)), the effects of this reform are considerably more likely to be mediated by changes in economic circumstances only.

This section give a detailed picture of how the increased child benefits (Section 3.1)

[12] Households with income around the median will invest additional income in the children they already have. At some point, the marginal utility of further increases in child quality are very small, and households with very high incomes may respond to an income increase by increasing family size.

[13] Norway has a long tradition - with considerable political and public opinion support - of "district policies" aimed at maintaining population levels in remote parts of the country.

and the tax deductions (Section 3.2) changed the economic conditions of women in the control region. In Section 3.3, we discuss other changes that could have affected fertility differently in the reform and control region, and how we handle these.

3.1 The child benefit reform

Table 2 shows the benefit level by year over time in the reform and control regions. Starting 1 January 1989, universal child care benefits were increased with 2400 NOK yearly in the reform region, an increase amounting to roughly \$ 347 (in 1989 dollars, 6.9 USD/NOK).[14] In 1990, additional benefits in the reform region increased to NOK 3600 or \$ 575. Due to inflation, the real value of the benefits declined slightly during the 1990s despite a small nominal increase in 1991. Neither the general child benefits (for the whole country), nor the additional benefits introduced for Finnmark and North Troms are means-tested.

Anticipatory effects are very unlikely. In August 1988, Prime Minister Gro Harlem Brundtland announced the intention to increase the cash allowance in Finnmark and North Troms, and to generally strengthen the "district policies" targeted at Northern Norway. The final decision was made by the Norwegian Parliament 20th December 1988. The child benefits was paid from January 1st the next year. The consequences of this reform were easy to grasp, potentially making for stronger and more immediate effects (Gauthier 2007).

An estimate of the direct costs of childrearing in the child's first year of living for Norway in 1989 is given in the first column of Table 1. The estimate accounts for the cost of clothes, food, health care, toys and equipment during the first year of living. [15] The estimated cost of a child is then compared to the cash allowance for a first child in the reform region (column 4) and in the rest of Norway (column 6).[16] As the table shows,

[14] Conversion rates obtained from `http://www.norges-bank.no/en/Statistics/exchange_rates/currency/USD/`.

[15] Health expenses for mother and child are low, as they are mainly covered through the public health care system.

[16] While the additional benefit per child in the reform region is independent of family size, the additional "base" cash allowance (given in both the reform and treatment region) is slightly higher for higher parities. For simplicity, the first child is displayed here.

Table 1: The cash allowance and the monetary cost of a first child.

	Cost of child[a]	Size of child benefit[b]					
		Treatment region		Control region		Difference	
	NOK	NOK	%	NOK	%	NOK	%
1989	19 320	10 236	53%	7 836	41 %	2 400	12%
1990	21 112	12 348	58%	8 848	41%	3 600	17%

[a] Estimates of cost of living from March 1989 made by the National Institute of Consumer Research (`http://www.sifo.no/files/standardbudsjett1989mar.pdf`). The sum includes expenses to food, clothes, health, toys, and various equipment. Increases in various household expenses, amounting to approximately 100 NOK per month (depending on household size), are not included. The budget does not account for increases in housing cost driven by an additional child. SIFO budgets for 1990 are not available, the 1989 estimate is adjusted upwards for a 4,1% price increase to give the 1990 estimates (`https://www.ssb.no/en/kp`).
[b] Source: NOU 1996:p. 134 and 436.

Table 2: Income increase induced by the regional increase in cash allowance by year and number of children.

Year	Childless	1 child	2 children	3 children	4 children	5 children
1989	0	2400	4800	7200	9600	12000
1990	0	3600	7200	10800	14400	18000
1991-1997	0	3792	7584	11376	15168	18960

Note: Source Norwegian Ministry of Regions & Municipalities (2004, p. 134 and 436). Not adjusted for price increase.

even absent the regional reform, the Norwegian child allowance reduced the immediate direct cost of a child with 41%. When the reform was fully implemented, women in the reform region received a cash allowance that should cover 58% of the expenses in the child's first year of life. The reduction in the price of a child is 17 percentage points larger in the reform region than in the rest of the country.[17] If the demand for children is sensitive to the direct cost of a child, we expect positive effects in our sample.

As the cash allowance was given also for children already born, it also implied a substantial income increase for women with (many) children. Table 2 shows the increase in household income induced by the reform, by parity and year. The child cash allowance increases linearly in number of children. In relative terms, the income increase is likely to be even larger for women with several children, as these on average will have lower

[17] As the costs of raising a child increases with the child's age, the proportion covered by the child allowance will decrease over time (`http://www.sifo.no/files/standardbudsjett1989mar.pdf`). However, as earnings also increase with the parent's age, the direct costs of childrearing in the near future may still be crucial for fertility decisions.

Age group	Mean earnings	[95% C.I]	Allowance as prop.
15-19	16184	[14968,17400]	0.17
20-24	63831	[61035,66626]	0.04
25-29	77506	[73303,81709]	0.03
30-34	81926	[77582,86271]	0.02
35-39	85797	[81621,89972]	0.02
Total	58736	[57133,60340]	0.07

Table 3: Mean earnings among women in the reform region prior to the reform (1988). Separate estimates by age group.

earnings. For a mother with two children, the additional child allowance for the two children already born and the marginal (potential next) child would cover about 75% of the cost of the marginal child.

The size of the cash allowance is economically meaningful also compared to mean earnings among women in the reform region. For women in their 20s and 30s in the reform region, the *additional* cash allowance amounted to about 2-4 per cent of the yearly earnings (Table 3). Among adult women, the value of the cash allowance relative to earnings is highest for women in their early 20s (who have the lowest earnings), and lowest for women in their 30s. For teenagers, who rarely have employment as their main activity, the increase in cash allowance is equal to more than a fifth of yearly income.

The increased cash transfer can increase fertility through both income and price effects: It reduces the price of the marginal (potential next) child, as the direct costs of this child will be partly offset by the increase in child allowances. Additionally, for families with children, the reform increases household income, making for a possible positive income effect on the propensity to have another child.

3.2 Tax deductions

From 1 January 1990, substantial regional (income) tax deductions for all taxpayers were implemented in the reform region, increasing net wages in Nord-Troms and Finnmark.[18] Tax deductions were adjusted promptly by the tax authorities, giving obvious and imme-

[18]Prior to 1990, individual taxpayers in the region had enjoyed a slightly higher general tax deduction than taxpayers in the rest of the country, but the 1990 increased those tax breaks/deductions considerably. See `http://www.regjeringen.no/en/dep/krd/Subjects/rural-and-regional-policy/virkeomrader-retningslinjer-og-regler/action-zone-in-finnmark-and-nord-troms.html?id=527171`

diate effects on paychecks for salaried employees. Additionally, (private sector) employers in the region were exempt from (mandatory) employer contributions to the national social security system, reducing the cost of labor for employers, potentially increasing demand for labor. Through higher wages and lower unemployment, the economic situation in the reform region was to be improved.

The income tax breaks consisted of two parts, an increase in the general (lump-sum) deduction available for all taxpayers and lower (marginal) tax rates for incomes at higher levels. The general deduction was increased by 10 000 NOK per individual, and twice this amount for single parents.[19] The deduction was later increased to 15 000 NOK, again with twice the amount available for single parents.[20] As the tax deduction is given to single parents independent of their number of children, it may affect fertility of single employed women through two separate mechanisms. For childless single women, the additional tax deduction implies a reduction of the direct cost of the first child. For single mothers, the tax deduction increases household income, potentially giving a positive income effect on fertility.

Furthermore, two changes in the tax rate were implemented: The "base tax", applying to taxable income at all levels, was set 3.5 percentage points below the rate for the rest of Norway. Additionally, the increase in tax for higher tax brackets ("toppskatt" or surtax) was reduced with 4 percentage points in the reform region, compared to the rest of Norway.[21] While both the "base tax" and the surtax fluctuated over time, the difference between the tax levels in the reform and control regions remained relatively stable (Angell et al. 2012, p.140).

The reform could influence female labor supply through two channels. First, if the reform increases fertility, it will expectedly also reduce female labor supply (Angrist and Evans 1996; Cools and Strøm 2014). This does not threaten the validity of our results. More worrisome, the tax reform could increase female labor supply directly. If

[19] Ministry of Finance: Ot.prp.no 32, 1989-1990.

[20] Norwegian Ministry of Regions & Municipalities (2004, p.9). The deduction was 10 000/15 000 NOK in Tax Class I, and 20 000/30 000 NOK in Tax Class II. Single parents ("enslige forsørgere") are classified as Tax Class II.

[21] This applies to Innslagspunkt I, i.e. the lowest of the top tax brackets (Norwegian Ministry of Regions & Municipalities 2004, p. 9)

the reform causes a shift hours from unpaid to paid work, this indicates a strengthened substitution effect. In presence such an effect, estimates of price and income effects will be biased towards zero, and hard to interpret. We test for effects on women's labor supply by estimating difference-in-difference models taking earnings and the propensity to be employed (have taxable earnings) as the dependent variables in Section 6.4. Reassuringly, there is no clear indication that the reforms have increased female labor supply through channels other than increased fertility.

3.3 Other relevant regional changes

In addition to the reforms mentioned above, starting in 1988, individuals had their (public) student loans reduced by 10% (up to a limit of NOK 15.000) for each year residing and working in Northern Troms and Finnmark. This policy also aimed to retain and recruit highly educated individuals to the region. Hence, confounding effects from this change are likely handled by measuring place of residence prior to the reform. We check whether selective migration took place in Section 6.3. When the (long term) cost of education is reduced, some women may be induced to pursue higher education, which will again likely lead to a postponement of childbearing (Lappegård and Rønsen 2005). We test for the presence of such an "enrollment effect" by estimating the effect of the reform on educational enrollment, using same difference-in-difference approach (Section 6.4).[22]

4 Methods and Data

4.1 Identification strategy

Our starting point for identification of reform effects is a standard difference-in-difference-model, which pertains to a Linear Probability Model (LPM) including fixed effects (i.e. a set of dummy variables) for region and year. The fixed effects net out change over time

[22] Strictly speaking, the reduced student loan repayments increases disposable income both in the short and long run. However, the increase in household income in the short run is likely too small to affect fertility decisions. While the increase in life time disposable income are of a larger magnitude, and could affect fertility if individuals are perfectly rational and forward-looking, we expect that consequences in the far future that are relatively hard to grasp to be less important for fertility decisions.

that is shared across regions, and any region specific factors that affects fertility but are constant over time. The main DD-specification takes the following form:

$$\begin{aligned} Y_{i,t} =& \alpha + \beta_{Ref_{i,t}} X_{Ref_{i,t}} + \beta_{Age_{i,t}} X_{Age_{i,t}} + \beta_{AgeSq_{i,t}} (X_{Age_{i,t}} \times X_{Age_{i,t}}) + \\ & \sum_{m=1}^{m} \beta_{Muni_{i,t}} X_{Muni_{i,t}} + \sum_{y=1984}^{1997} \beta_{Year_{i,t}} X_{Year_{i,t}} + \varepsilon \end{aligned} \tag{1}$$

X_{Ref} is a dummy which takes 1 for years in the reform period for individuals who lived in the reform region in the year prior to the reform. β_{Ref} captures the effect of the reform on the outcome Y, given that the identifying assumption holds. X_{Year} are dummies for year (year fixed effects), X_{Muni} are dummies for municipality (municipality fixed effects). We include a linear and quadratic term for age. As reform effects are estimated separately by 5-year age group, this is equivalent to a quadratic spline with five year knots.

If the trends in fertility across the region are similar (absent the reform), Equation 1 will identify the reform effects. Figure 2a indicates that when the fertility of all age groups is considered jointly, the similar trend assumption holds. However, when fertility trends are broken down by age group (Figure 3), some tendency of regional trends appear. To rule out that such regional trends are driving the results, we also estimate a Regional Trend Specification. For these models, the identifying assumption is that trends are modeled correctly, and hence netted out fully. As shown in Figure 3, the trends in age specific fertility are non-linear. A curvilinear time trend (dotted lines) fits the trends well, and this specification is included in the models. The time trend is estimated separately by region and 5-year age group. When the model is estimated separately by 5-year age group, this gives the following specification:[23]

[23] An alternative to the curvilinear trends would be a cubic spline model. As shown in Supplementary Material, Figure S.1, such a specification is immensely flexible, following the trends in fertility in the reform and control regions very closely. Hence, one risks netting out region specific changes in fertility that happened after the reform only – i.e. the reform effects the DD-model is intended to capture. To avoid controlling out the very variation we aim to capture, we prefer the quadratic trend model.

$$
\begin{aligned}
Y_{i,t} =&\alpha + \beta_{Ref_{i,t}} X_{Ref_{i,t}} + \beta_{Age_{i,t}} X_{Age_{i,t}} + \beta_{AgeSq_{i,t}} (X_{Age_{i,t}} \times X_{Age_{i,t}}) + \\
&\sum_{m=1}^{m} \beta_{Muni_{i,t}} X_{Muni_{i,t}} + \beta_{LnReform} (X_{Year} \times X_{RefReg}) + \beta_{LnControl} (X_{Year}) + \\
&\beta_{SqReform} (X_{Year} \times X_{Year} \times X_{RefReg}) + \beta_{SqControl} (X_{Year} \times X_{Year}) + \varepsilon
\end{aligned} \tag{2}
$$

Where X_{RefReg} is an indicator for living in the reform region, X_{Year} is a numeric variable for calendar year. $\beta_{LnReform}$ and $\beta_{SqReform}$ are the estimated linear and quadratic trends for the reform region, and $\beta_{LnControl}$ and $\beta_{SqControl}$ are the estimated linear and quadratic trends for the control region. Standard errors are robust with clustering on municipality, in line with recommendation for analysis of the type we perform here (Angrist and Pischke 2009). Municipality of residence is measured yearly, but no later than in the year before reform implementation to avoid endogeneity issues.

4.2 Data and study sample

Our data comes from various register sources with information on the full population of Norway. The data from different registers are merged using a unique person identifier (PIN). Data from the population registers include information on date of birth (for both mothers and children) as well as municipality of residence as January 1st each year. Information on educational enrollment and completion was obtained from the National Educational Database (NUDB).

Due to a tax reform implemented in 1983 that affected the treatment and reform region differently, our observation window starts in 1984. Our observation window is limited upwards by the introduction of the cash-for-care reform in 1998, which could change the economic incentives of childbearing and confound the effects of our reform (Andersen et al. 2015). Our data set is restricted to women who lived in the treatment- or control region for at least one year in the time period 1984-1988.[24] Based on this

[24]This restriction is due to the requirement that place of residence is to be measured prior to the reform. Up to 1988, women can exit and (re)enter the sample by moving. For the years after the reform, the sample is fixed based on residence in 1988.

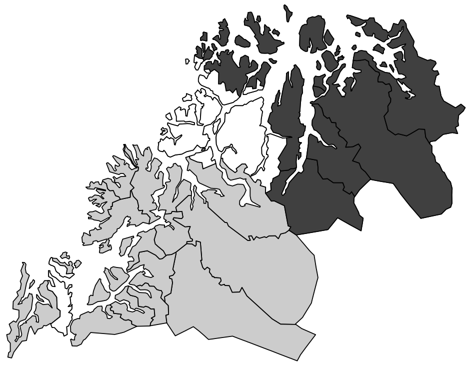


Figure 1: Map of Troms with municipality borders.

Treatment municipalities in dark grey, control municipalities in light grey, cities omitted from the control region in white.

sample, we construct a data set of person years. Person years for women younger than 15 or older than 39 are dropped. The latter restriction is due to the comparatively low fecundability of women above age 39, which may hinder them in responding to fertility incentives, and bias our estimates towards null.

For reform and control groups to be comparable, we utilize the fact that the reform was implemented in the northern, but not the southern, part of Troms. The seven reform municipalities in Northern Troms hence constitute our reform or treatment region.[25] Our control region are municipalities in Southern Troms. The treatment region has no larger cites, while the cites Tromsø and Harstad are located in Southern Troms. As fertility trends are found to vary on a rural/urban axis (Kulu et al. 2007), we exclude the municipalities Tromsø and Harstad from our sample.[26] We still refer to the control region as Southern Troms for brevity.

[25]These are Kvernangen, Skjervøy, Nordreisa, Kåfjord, Lyngen, Storfjord and Balsfjord.

[26]When Tromsø and Harstad are included in the control region, trends are no longer similar in the treatment and control region (results available upon request).

4.3 Variables

Dependent variables

Our main dependent variable is number of children, derived from monthly updated data on children ever born. For each year, we include children already born, as well as conceptions that year leading to live births. Dates of conception are imputed as the birth date minus the average length of pregnancy. Based on the number of children, we also construct three dummy variables for parity specific analysis, for having at least 1, 2 and 3 children in the given year.

We also estimate the difference-in-difference models taking educational enrollment and earnings as dependent variables (see below for specifications).

Reform variables

Our independent variable of interest is a dummy for "Reform" - indicating that the individual lived in the reform region in the reform period. As the reform may induce selective migration, we use place of residence in 1988 as a proxy for current place of residence in the reform (post 1988) period. This gives Intention To Treat (ITT) estimates. This exogenous measure of region of residence is also used in the construction of the trend variables and for clustering. To investigate whether the endogeneity of place of residence does affect the results, we also estimate a model where place of residence is genuinely time varying for all years.

Controls

All models include municipality fixed effects (i.e. dummies for municipality of residence) and either year fixed effects, or a (set of) trend variables, as outlined above. Age is captured by a linear and a quadratic term.

A quasi-experimental modeling strategy often does not, strictly speaking, require the inclusion of further (individual) characteristics for identification of reform effects (Angrist and Pischke 2009). However, inclusion of covariates may serve as a useful robustness check, and also reduce unexplained variance and hence sharpen the precision of our

Table 4: Balancing tests on observable characteristics.

	Control region		Reform region	
	Mean	[CI]	Mean	[CI]
Number of children	1.157	[1.150,1.165]	1.140	[1.129,1.150]
Age	27.008	[26.966,27.051]	26.465	[26.408,26.522]
Prop. married	0.339	[0.337,0.342]	0.310	[0.306,0.313]
Earned income	76 220	[75 797,76 643]	71 352	[70.816,71 888]
Prop. enrolled in education	0.176	[0.174,0.178]	0.186	[0.183,0.189]
Prop. with higher education	0.310	[0.308,0.313]	0.283	[0.279,0.287]
Prop. with basic education only	0.050	[0.049,0.051]	0.073	[0.071,0.075]
Number of women aged 15-39 in municipality	15306	[15244,15368]	10505	[10474,10537]

Note: Sample is women aged 15-39 who lived in the Treatment or Control region in Troms in the period 1984-1997.

estimates. As all covariates observed in our data set (except age) can themselves be affected by the reform, they must be measured prior to the reform (i.e. in 1988 at the latest). We therefore allow covariates to vary with time, up to 1988, from which their value is held fixed. To avoid excessive lags after the reform, the models with covariates are estimated on data up to 1991 only. We also utilize the set of exogenous covariates to estimate separate models by subsample.

We include the following covariates. First, a set of dummies for educational attainment, distinguishing between mandatory (primary and lower secondary), high school and higher education. Missing education is included as a separate category. We also include a dummy variable for being enrolled in education, set to 1 if the individual is registered as enrolled in education for at least 4 months the current year. As a proxy for union status, we include a dummy variable for being registered as married in the current year.[27] Finally, household income is proxied by earned income in thousand NOKs.

5 Main results

5.1 Descriptive results

Regional trends in fertility and related outcomes

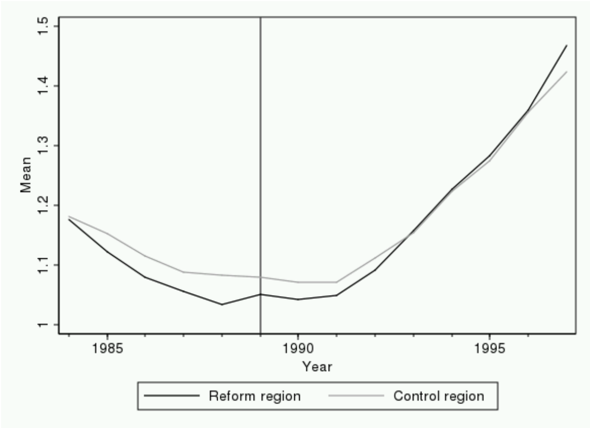
Balancing tests of the comparability of the reform and treatment region are shown in Table 4. The table shows the means and confidence intervals of number of children, earned income, and municipality size, as well as the proportion married, with higher education, with lower eduction, and enrolled in education – all estimated separately for the reform and control regions. All outcomes are measured in 1987. There are no significant differences in number of children by region of residence. There are small, but statistically significant differences in age, earned income, educational enrollment and attainment, and municipality size. However, as long as the trends in these outcomes are similar in the reform and control region, they will be netted out in a difference-in-difference design.

Figure 2 provides a test of whether the regional trend assumption holds, looking at number of children, proportion married, earnings and educational enrollment and attainment. The figure shows the means of these outcomes, estimated separately for the reform region (Northern Troms, black) and the control region (Southern Troms, grey).[28] The trends are strikingly similar across regions, with two possible exceptions: The proportion with higher education seems to increase somewhat faster in the treatment than the control region after the reform, most likely indicating selective migration (Figure 2d). Earned income seems to be increasing at a slightly faster pace in the control region than in the reform region, also before the reform (Figure 2e).

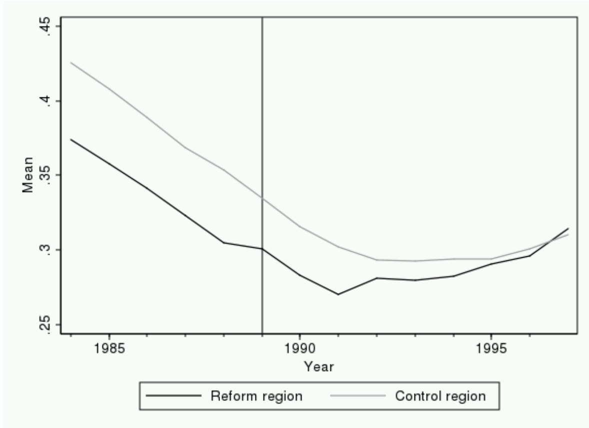
Figure 3 shows the fertility trends estimates separately for five-year age groups, as an additional test of the regional trend assumption. The figure gives some evidence of regional age-specific trends, casting some doubt on the identifying assumption required

[27]While an indicator of cohabiting unions would certainly be of interest, this is not available for the full study sample in the years of interest.

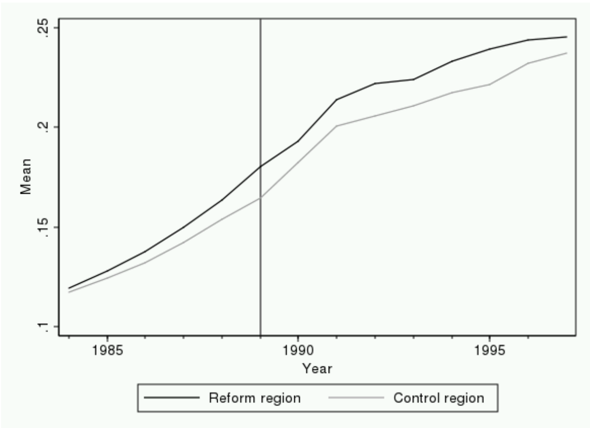
[28]As the mean is averaged over the ages 15-39, the mean number of children is significantly lower than completed fertility for the cohorts included.



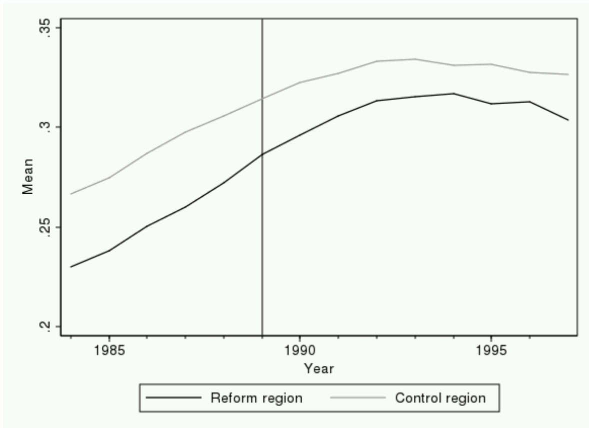
(a) Number of children.



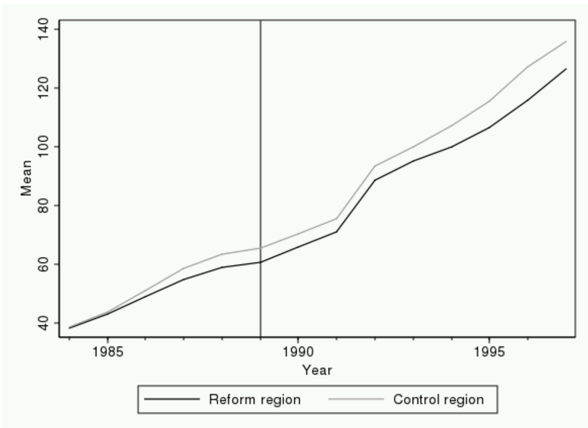
(b) Prop. married



(c) Prop. enrolled in education



(d) Prop. with higher educaction



(e) Earnings

Figure 2: Trends in number of children, proportion married, education and earnings by region.

Note: Calculations are simple means, without correction for age structure. Sample is women aged 15-39 who lived in the Treatment (black) or Control (grey) region in Troms county at some time point in the period 1984-1997. Information on region of residence is updated yearly throughout the period, and hence potentially endogenous in the post-reform period.

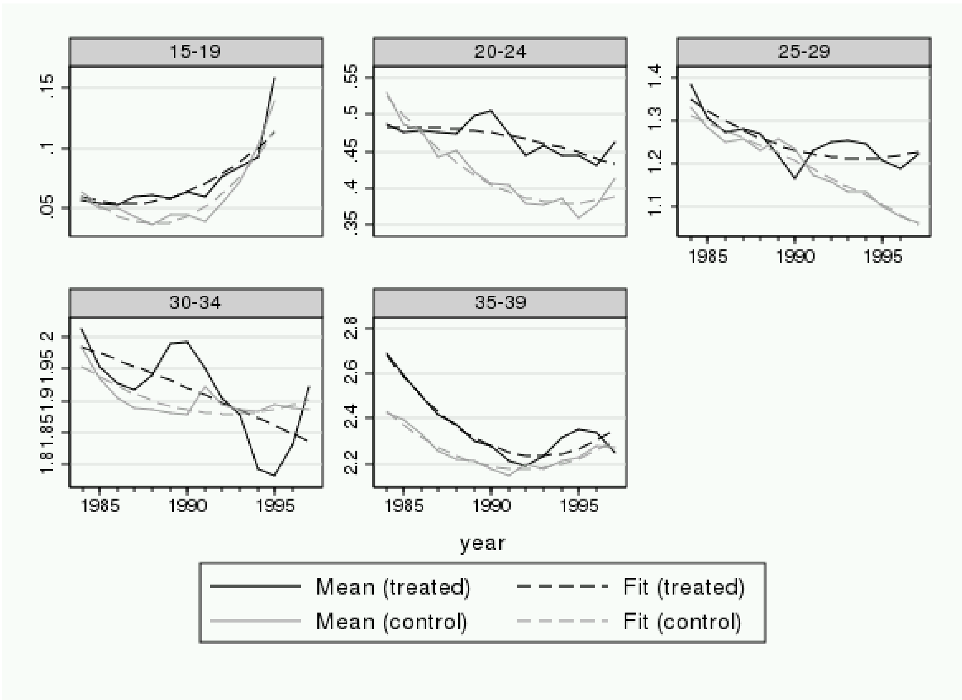


Figure 3: Trends in number of children by age group and region.

Note: The full lines show simple means, while the dotted lines are predictions from OLS regression of number of children on age and age squared for the given subsample. Sample is women aged 15-39 who lived in the Treatment (black) or Control (grey) region in Troms county at some time point in the period 1984-1997. Information on region of residence is updated yearly.

in Eq. 1. However, with the exception of women aged 30-34, these trends are captured well by a quadratic trend (the dotted lines in Figure 3). While the trend model nets out bias from regional trends efficiently, the trends are estimated also using post-reform variation in the outcome. Hence, these run the risk of netting out some of the variation in the outcome *generated* by the reform. Aware of the limitations of each specification, we present our main results both in a standard difference-in-difference specification (Eq. 1) and a regional trend specification (Eq. 2).

The descriptive statistics indicate that the reform did facilitate family formation: After the reform, it seems that fertility falls slower, and increases faster, in the reform than in the control region (Figure 2a). Furthermore, the proportion married seems to increase faster in the reform than the control region after the reform (Figure 2b). However, these aggregate estimates are based on *actual* region of residence, and therefore prone to bias from selective migration. Also, region-specific changes in the age structure could drive trend deviations. Hence, multivariate estimations are required to test whether a reform effect is present.

5.2 Main regression results

This section presents the main results, as estimated by LPM. As fertility responses and regional trends likely vary considerably by age, all multivariate results are estimated separately by age group. Women in the main childbearing years will more often be on the margin to have a(nother) child, and may thus be more likely to respond to economic incentives. On the other hand, women in their 20s have significantly lower earnings (Table 3) and may hence be more responsive to economic incentives.

Table 5: Reform effects on number of children. Separate models by age group.

	15-19		20-24		25-29		30-34		35-39	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Reform effects										
Number of ch.	0.011	-0.002	0.109***	0.046**	0.098	0.000	0.024	0.131***	-0.120	-0.000
	[-0.025,0.047]	[-0.018,0.014]	[0.052,0.166]	[0.013,0.079]	[-0.037,0.233]	[-0.109,0.110]	[-0.056,0.104]	[0.077,0.186]	[-0.250,0.010]	[-0.054,0.054]
Prob. of having >= 1 ch.	0.014	0.004	0.072**	0.049***	0.032	0.009	-0.013	0.017	0.001	-0.003
	[-0.020,0.047]	[-0.013,0.021]	[0.032,0.111]	[0.022,0.076]	[-0.020,0.085]	[-0.030,0.047]	[-0.033,0.008]	[-0.005,0.038]	[-0.026,0.028]	[-0.021,0.015]
Prob. of having >= 2 ch.	-0.002	-0.006*	0.043***	0.001	0.045	-0.013	-0.001	0.045*	-0.013	0.003
	[-0.008,0.003]	[-0.011,-0.001]	[0.021,0.066]	[-0.015,0.017]	[-0.013,0.103]	[-0.057,0.032]	[-0.042,0.039]	[0.011,0.079]	[-0.057,0.032]	[-0.023,0.030]
Prob. of having >= 3 ch.	0.000	0.000	-0.004	-0.003	0.010	-0.002	0.023	0.057***	-0.044	-0.011
	[0.000,0.000]	[0.000,0.000]	[-0.011,0.002]	[-0.011,0.005]	[-0.025,0.045]	[-0.045,0.040]	[-0.012,0.057]	[0.031,0.082]	[-0.090,0.002]	[-0.034,0.012]
Endog. reform variable										
Number of ch.	0.016	0.002	0.077*	-0.030	0.074	0.006	-0.002	0.018	-0.091	0.059
	[-0.014,0.045]	[-0.007,0.012]	[0.017,0.137]	[-0.086,0.026]	[-0.026,0.175]	[-0.077,0.090]	[-0.093,0.089]	[-0.095,0.130]	[-0.215,0.034]	[-0.060,0.178]
Prob. of having >= 1 ch.	0.017	0.005	0.055*	-0.001	0.016	-0.004	-0.012	-0.000	-0.000	-0.004
	[-0.011,0.044]	[-0.006,0.015]	[0.014,0.096]	[-0.029,0.028]	[-0.030,0.062]	[-0.034,0.027]	[-0.037,0.013]	[-0.027,0.027]	[-0.027,0.027]	[-0.027,0.020]
N	20425	20425	29450	29450	27251	27251	26256	26256	28768	28768
Placebo reform										
Number of ch.	0.0114	-0.00288	0.0413	0.0481	0.00970	-0.142*	-0.00660	-0.0483	-0.0805*	-0.0567
	(0.00876)	(0.00533)	(0.0204)	(0.0357)	(0.0335)	(0.0506)	(0.0368)	(0.0534)	(0.0336)	(0.0496)
Prob. of having >= 1 ch.	0.0106	-0.00338	0.0191	0.0207	-0.00229	-0.0784**	0.00731	-0.0116	-0.00125	-0.0107
	(0.00909)	(0.00556)	(0.0146)	(0.0210)	(0.0122)	(0.0250)	(0.0107)	(0.0200)	(0.00793)	(0.0121)
N	11783	11783	11108	11108	9000	9000	9364	9364	9775	9775

95% confidence intervals in brackets

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Estimates from Linear Probability models. All models include controls for municipality fixed effects, age, and age squared. Standard errors are clustered at the municipality level. Odd numbered columns include controls for year FE, even numbered columns controls for regional trends (linear and squared). The sample is women aged 15-39 who lived in the Treatment or Control region in Troms county at some time point in the period 1984-1988, and who were in the age range 15-39 at some point in the period 1984-1997 (1984-1988 for the placebo models).

Table 5 shows the main results estimates separately by age group, using year FE (odd-numbered columns) and regional trends (even-numbered columns). We find significant and substantial reform effects among women aged 20-24, across specifications. For women in this age group, the reform causes a 10 percentage point increase in the (yearly) probability of conceiving a child, as estimated in the standard DD-specification. In the (more conservative) regional trend specification, the reform effect is halved, but still statistically significant. There is also a tendency of effects among women in their early 30s, but this effect is not robust across specifications, and should be interpreted with some caution.

For teenagers (aged 15-19) the reform effect is quite precisely estimated to zero across specifications. Hence, in this context, it seems that teenage fertility is not sensitive to economic incentives. Teenage fertility is very low in our sample (see e.g Figure 3), and it seems likely that there is simply no latent demand for children among teenagers that makes them respond to economic incentives. In this age group, one would, if anything, expect a fertility response to be mediated by lower abortion rates. Aggregate data on the percentage of births ending in abortion by county gives no indication that the reform affected the overall propensity to terminate pregnancies (Supplementary Material, Figure S.2).

Parity specific effects

As outlined in Section 2, the mechanisms driving the reform effects depend on parity. Among childless women, the reform work only by reducing the direct cost of the first child. For mothers, there is an additional income effect from the increase in cash allowance from children already born. To explore parity-specific effects, we also estimate effects of the reform on the probability of having (conceived) at least 1, 2 or 3 children (Table 5). We also estimate models for higher parity transitions, but these models yield no significant results (available upon request).

The parity-specific models show that the strongest and most robust effects for the transition to parenthood, concentrated among women in their early 20s (Table 5, column 3 and 4). In this group, the propensity to enter parenthood increases by 5-7 percentage points (depending on specification) due to the reform. In the DD-specification, we also find effect on the propensity to have a second child among women in their early 20s, but this effect is netted out when the regional trends are included. The effects among women aged 30-34 (column 7 and 8) are found at the propensity to have at least two and at least three children, but are significant in the Regional Trend model only.

Effects are strongest for the propensity to enter parenthood, which is the only parity for which there is no income effect due to the cash benefits (Table 2). This indicates that our results are driven by reduction in direct costs of children (present at all parities) rather than income effect.[29] Importantly, while parents could also choose to spend the additional income on "child quality", no such close substitutes for having children are available for childless women.

The effects on the propensity to have a third child among women in their early 30s may very well be a quantum effect, indicating that the reform shifted some women from a completed family size of two children to a completed family size of three. However, as these results are sensitive to specification, we are cautious in our conclusions.

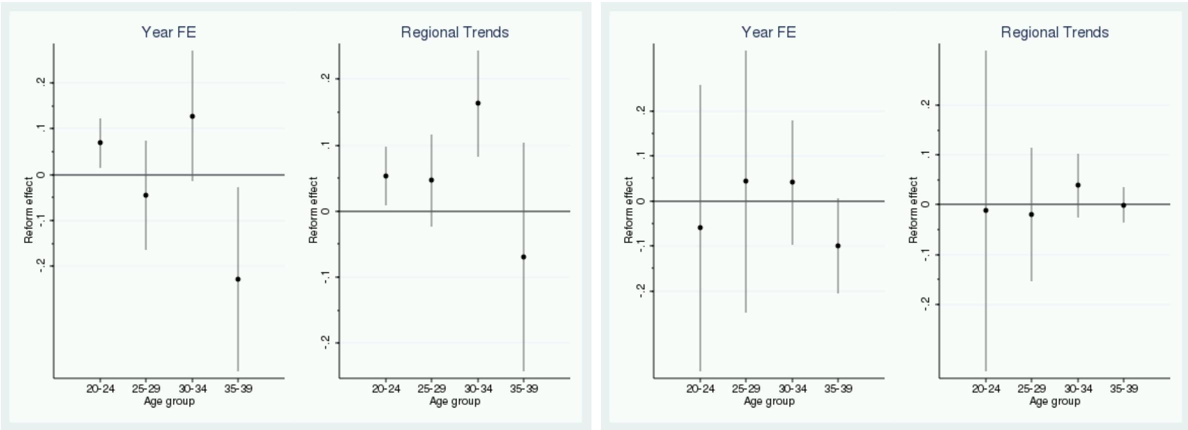
5.3 Subgroup analysis

To investigate which mechanisms that are likely to drive our results, we also estimate effects for different subpopulations. For reasons outlined in Section 4.3, we restrict the time series to the period 1984-1991. Models are again estimated by age, and we present results both using a standard DD-model and a Regional Trend model.

Effects by marital status

As most women prefer to have and raise a child together with a partner (Hobcraft and Kiernan 1995; Thornton and Young-DeMarco 2001), women who are living alone will expectedly respond slower to changes in income or the cost of children. On the other hand, the cash transfer implies a relatively larger change in economic circumstances for

[29] Importantly, the income effect present for mothers only is due to cash transfers, which are given irrespective of hours worked. Hence, downward bias from substitution cannot explain the parity-specific findings.



(a) Unmarried women.

(b) Married women.

Figure 4: Reform effects on number of children by marital status and age.

Note: Sample is women who lived in the Treatment or Control region in Troms county at some time point in the period 1984-1988, and who were in the age range 20-39 at some point in the period 1984-1991. Information on region of residence and marital status is updated yearly up to 1988, after which region of residence in 1988 is used as an (exogenous) proxy. Estimates from Linear Probability models. All models include controls for municipality fixed effects, age, and age squared. Standard errors are clustered at the municipality level.

women who are living alone or cohabiting (see Section 2.1), and may hence generate larger responses in this group. Furthermore, as outlined in Section 3.2, single parents have an additional lump-sum tax deduction, which further reduces the cost of having a first child outside a union, and induces an additional income increase for single mothers. Hence, the design of the reform points toward finding the strongest effects among unmarried women.

Figure 4 shows the reform effects estimated separately for unmarried (4a) and married (4b) women. For married women, the effects are close to zero. Hence, in accordance with expectations, the reform effects previously found are concentrated solely among unmarried women. Parity specific models (not shown) indicate that the effects among unmarried women are concentrated at the transition to parenthood.

While the cash incentives clearly increases non-marital childbearing, it is not obvious that this means an increase in non-union childbearing. At the time of the reform, as much as 70 per cent of the non-marital births in the reform region were to women who were cohabiting with the child's father at the time of the birth (Appendix Figure A.1). While register data on cohabitation were unavailable in Norway at the time of the reform, we explore whether the reform affects the propensity to form and formalize unions in Section

6.4.

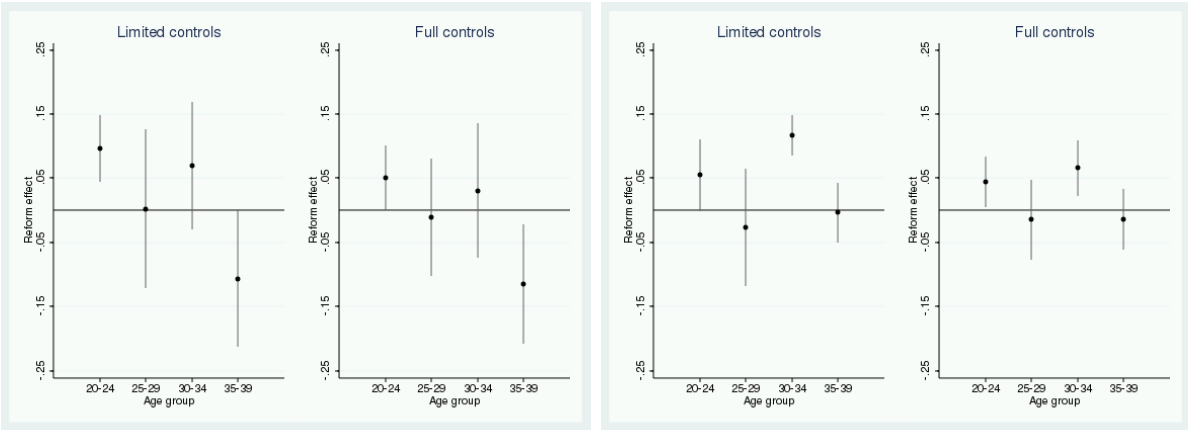
Effects by socioeconomic resources

We have also estimated effects separately by socioeconomic resources, that is, highest completed educational attainment and earnings quintile, both measured before the reform. Somewhat surprisingly, we find no clear pattern of effects by socioeconomic status. For educational attainment (Supplementary Material, Table S.1), most significant effects are found among women with medium education, but this is also by far the largest educational group, making for a substantially better test strength.

Results estimated separately by earnings quartile are also shown in Table S.1. Again, no clear pattern by earnings level emerges. As sample size becomes small, precision decreases, and the estimates are rarely significant. For the highest quartile (Q4), significant effects are found in both directions indicating that the identifying assumption does not hold in this specification and subsample.

6 Robustness checks

This section presents results from a variety of robustness checks, each performed to investigate whether our identifying assumptions hold, and the results presented above have a causal interpretation. We present two traditional robustness checks – inclusion of exogenous covariates and a placebo test. We also investigate how the results change if place of residence was measured yearly also after the reform. Finally, we explore whether the reform affected earnings and educational enrollment, which would make for a theoretically less clean interpretation of the reform effects. In sum, these tests indicate that our identifying assumption holds, and that we are unlikely to capture fertility effects driven by changes in women's labor supply or educational enrollment.



(a) Year and municipality FE.

(b) Regional trend model.

Figure 5: Robustness check: Controls for sociodemographic characteristics.

Note: Estimates from Linear Probability models. All models include controls for municipality fixed effects, age, and age squared. Standard errors are clustered at the municipality level. Sample is women who lived in the Treatment or Control region in Troms at some point in the time span 1984-1988, and who were in the age range 20-39 at some point in the period 1984-1991. All covariates are updated yearly up to 1988. After 1988, information from 1988 is used as an exogenous proxy for all covariates except age.

6.1 Controls for sociodemographic characteristics

If our design captures a true natural experiment, inclusion of (exogenous) covariates will not alter the estimates. As a robustness check, we therefore present a model that control for educational enrollment and attainment, earned income, and marital status.[30] The covariates are time varying, but not updated after 1988 to avoid endogeneity issues . For these lagged covariates to have a reasonable interpretation also in the post-reform period, we restrict the observation period to 1984-1991. The estimates in Figure 5 compares the main model estimated on this shorter panel ("Limited controls") and the estimates after inclusion of the exogenous controls outlined above ("Full controls"). The model is estimated both in the traditional DD-specification (Eq. 1) and in the Regional Trend specification (Eq. 2). All models take number of children conceived as the dependent variable, and are estimated separately by age group for comparability with the main results above.

In general, the models with limited controls replicate the results from the full period

[30]While we cannot be certain that inclusion of further characteristics, unobservable to us, would yield similar results, the inclusion of observed covariates is the only feasible test.

(Table 5) fairly closely. For women aged 20-24, the reform effect is significant at 10 percent level in the DD-specification, and like in the main model, is approximately halved in the Regional Trend Specification (the loss of significance in this specification is unsurprising due to the loss of power). In general, the addition of control attenuates the estimates somewhat, but not dramatically, towards zero. The change is smallest in the Regional Trend specification, indicating that this model provide the soundest identification.

6.2 Placebo reforms

If the identifying assumption holds, estimating "reform effects" in years before the reform will not yield significant results. To test this, we estimate effects of a "placebo reform" in 1985. The setup for the placebo effect is identical to the one used for main results. We measure place of residence in the year prior to the reform, and create a "reform variable" which is the interaction between living in the reform region the year prior to the reform and the year being 1985 or later. To avoid using any variation in fertility generated by the reform, we exclude all post-treatment years (1988 onwards). The placebo reform (Table 5, lower panel) is estimated in the same specifications as the main model (Table 5, upper panel).

Reassuringly, the effects of the placebo are close to zero, and rarely significant. While we only show effects for number of children and the propensity to have at least one child, no effects are found at higher parity transitions. For all age groups except for women in their early 30s, the zero effects are quite precisely estimated across specifications. For women in their early 30s, there are some significant negative effects in the regional trend model, indicating that the trend model is unable to net out regional trends properly in this age group. This falls in line with the strongly non-linear fertility trends in this age groups (Figure 3), indicating that the validity of the effects in this age group is somewhat compromised in the Regional Trend model.

In sum, the Regional Trend model performs better in the robustness checks than the standard DD-specification in all age groups but 30-34.

6.3 Migration to and from the reform region

The regional reform package we study had the explicit aim of slowing migration from the region and motivating (high-skilled) persons to (re-)settle there. Furthermore, as the reform lowered the cost of a child, it could motivate women who intend to have a child in the near future to move into the region – or to not move from the region. To investigate whether the reform did invoke selective migration with respect to fertility, we compare our main results using an exogenous reform covariate (Table 5, upper panel) with results based on time-varying residence status (Table 5, middle panel).

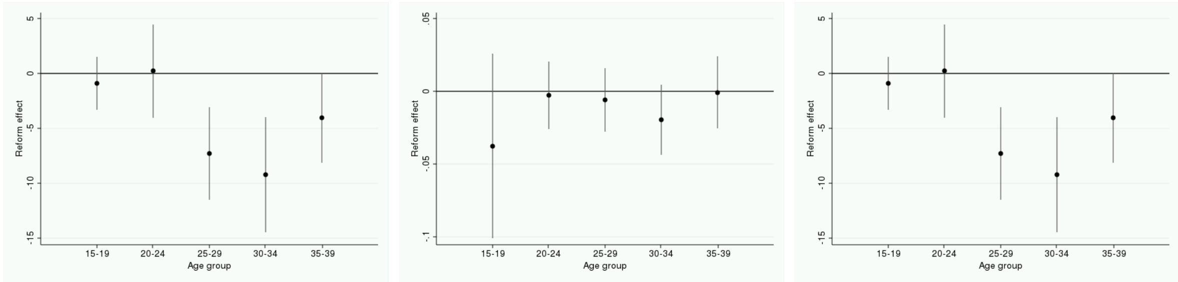
When allowing for post-reform migration, the estimates in the Year FE model are somewhat attenuated, and no significant effects emerge in the Regional Trends model. This gives some indication that reform induced migration biases the estimates toward zero, i.e. that the reforms induced in-migration of women who did not intend to have a child in the near future. This falls well in line with the intent to recruit/retain highly educated people, an interpretation corroborated by the increased proportion of highly educated women in the region (Figure 2d).

The fact that this reform did induce selective migration underlines the need of using an exogenous measure for treatment. Particularly, regional reforms targeted at changing fertility (such as the one analyzed by Milligan (2005)) may very well induce in-migration of individuals with above-average latent fertility.

6.4 Effects on other outcomes

Earned income and educational enrollment

As outlined in Section 3, a potential complication with our design is that the reform affected not only the economic circumstances of families, but also the incentives to work and enroll in education. Increased educational enrollment or increased female labor supply might in itself reduce fertility, biasing the effect of economic circumstances on fertility towards zero. As an indirect test for such bias, we estimate reform effects on earned income and educational enrollment. For these outcomes, age-specific trends in the outcome



(a) Earnings in 1000 NOK. (b) Prop. with earnings. (c) Prop. enrolled in educ.

Figure 6: Reform effects on earnings and educational enrollment.

Estimates from Linear Probability models. All models include controls for municipality fixed effects, age, and age squared. Standard errors are clustered at the municipality level. When earnings is the dependent variable (Figure 6a and 6b), the sample is women who lived in the Treatment or Control region in Troms at some point in the time span 1984-1988, and who were in the age range 20-39 at some point in the period 1984-1997. For educational enrollment as the dependent variable (Figure 6c), women who lived in the Treatment or Control region in Troms at some point in the time span 1984-1987, and who were in the age range 20-39 at some point in the period 1984-1997, constitute the sample.

are similar across region prior to the reform (Supplementary Material, Figures S.3 and S.4b), justifying the estimation of a standard DD-equation (Eq. 1). For educational enrollment, the reform is defined as happening in 1988 (and, accordingly, municipality of residence is measured no later than 1987), but otherwise the design is identical.

We estimate effects for two outcomes capturing female labor supply: The probability of having earned income in a given year, and yearly earnings in 1000 NOK.[31] Two mechanisms could drive reform effects on earned income. First, if the reform increases fertility, this is likely to translate into somewhat lower labor supply, concentrated at the same ages at the fertility response. Second, the tax deductions could increase labor supply directly, leading to positive effects across age.

Our results, displayed in Figure 6, show no effects on the probability of working (i.e. the extensive margin), and significant negative effects on total earnings. In combination, this indicates a reduction in hours worked among those employed (the intensive margin) due to the reform. The negative effects are found at ages where there are positive effects on fertility. Hence, both the direction of the effects, and the distribution of effects by age, indicate that these effects are mediated by changes in fertility.[32] This interpretation

[31]Year fixed effects makes adjustment for price increase superfluous.

[32]In theory, negative effects could also be an *income effect on labor supply* due to the tax reform: As the tax reform increases hourly wages, some women may choose spend some of this additional income to "buy time" – i.e. work less. If this mechanisms was driving the results, we would expect negative effects

is further strengthened by the observation that the proportion stay-at-home-mothers was relatively low, while the proportion mothers working part-time was substantial at time of the study (Norwegian Ministry of Children & Families 1996, Ch. 3). Hence, a reduction in earnings at the intensive margin is a plausible response to increases in family size. Our results give no indication that reform effects are biased downwards as women substitute time towards market work due to tax deductions.

There is no indication that the reform increased educational enrollment among women (Figure 6c). While the reduction in student loans did reduce the life time cost of education, this change did not seem to have influenced the choices of the women in Troms. One potential explanation for this lack of effect is the student loan reduction mainly improved economic circumstances years into the future, and that implications of this change was non-obvious (Gauthier 2007).

Effects on proportion married

As shown in Figure 4, the reform effects were concentrated among women who were unmarried at the time of the reform. This raises the question of whether the reform increases non-marital (and possibly also non-union) childbearing, or whether it ups the pace for forming or formalizing a union. In this section, we explore reform effects on the proportion married.

Appendix Figure A.2 shows the estimated reform effects on the propensity to be married, in both the fixed effects and regional trend specification. Due to some evidence of region-specific trends in the proportion married (Supplementary Material, Figure S.4a), the latter is more robust. In our preferred specification, the reform does not affect the proportion married. Hence, while the reform shifts some women into parenthood, this is not paralleled by an increase in the proportion women living in a formalized union.

across all ages.

7 Concluding discussion

In this paper, we have utilized a regional reform to estimate the effect of economic circumstances on fertility behavior. The regional design makes for plausible identification of causal effects, as we can compare people who lived in the same county at the same time, but where exposed to different economic policies. The treatment and control regions consist of municipalities comparable on observable characteristics, and display very similar trends in fertility prior to the reform.

It seems clear that the reform induced some women to have a first (and perhaps second) child earlier than they would otherwise have chosen to. A reduction in the mean age at birth has a lasting impact on population structure (Goldstein et al. 2003). Our results also indicate that the reform increased the third birth probability among women in their early 30s, and reduces the labor supply slightly in the same group. This indicates that lowering the direct cost of a child could increase completed family size in Norway. While this result is unsurprising, we are not aware that any other study with a plausible causal design has identified this effect in the Nordic context.

We find no indication that the additional income effect among mothers – induced by the increase in cash allowance for children already born – translates into stronger effects at higher parities. This points toward that the reduction in the cost of a child, which applies to all parities, drives our results.

The subpopulation analysis (Section 5.3) reveals that the effects are concentrated among unmarried women. The reduction in the cost of children was largest in this group, in part because women in the treatment group who had a first child without living with a partner received a substantial additional tax break on top of the increased child cash allowance and income tax reductions (Section 3.2). Our results indicate that reductions in the cost of a child seem to change the order of life course transitions, shifting some births from higher ages and formalized unions to lower ages, where unions are less likely to be formalized, or even formed. To the extent that stability of parental unions have benefits for adults and children, a shift of births from more stable to less stable union contexts will have less favorable consequences.[33]

Among the women shifted into early parenthood due to the cash transfers, we find little indication of reductions in labor supply (Figure 6). Hence, our results do not raise concerns that increases in cash transfers to mothers will weaken young mothers' commitment to the "dual strategy" of work and motherhood (Ellingsæter and Rønsen 1996). For women in their 30s who are induced by the reform to have a third child, a different pattern emerges: here, our results indicate that some women prefer the combination of a larger family and shorter work hours when the direct cost of raising a child is lowered.

The combination of a clean quasi-experimental design and access to population data of high quality corroborates the robustness of our results, giving our study credibility when compared to previous similar setups. As our reform is not targeted at increasing fertility, effects mediated through mechanisms other than changes in costs or income are very unlikely. The combination of a regional reform and extremely detailed data allows us to construct an *a priori* plausible control group, and for extensive (indirect) testing of the identifying assumption. When these extremely detailed tests give any reason for concern, we are able to net out age-specific regional trends in fertility by parametric modeling. The plausibility of our identifying assumption is also bolstered by the estimates being relatively unchanged when exogenous covariates are included, and by a placebo test yielding zero effects. Furthermore, the availability of exogenous covariates allows us to estimate effects in subsamples without endogenous conditioning, which was neither feasible for Milligan (2005) nor Cohen et al. (2013).

However, some concerns with our analysis should be mentioned. First, while the theoretical expectations of how the tax benefits and cash allowances interact can be outlined, studying each of these in isolation would make for a theoretically cleaner interpretation of the results. As it is, we are able to test that the reform did not affect women's labor supply through other channels than increasing fertility (i.e., that the substitution effect does not bias our estimates downwards). However, due to the combination of effects,

[33]Though the majority of the non-union births are to cohabiting women (Appendix Figure A.1b), these unions are significantly less stable than marital unions: Using data from Norway in the reform period, Kravdal (1997) shows that many Norwegian cohabiting women are "[w]anting a child without a firm commitment to the partner" (see also Vinberg et al. (2015)).

we abstain from calculating price- and income elasticities, which again complicates comparison of reform effects across contexts. Furthermore, as our main effects are identified among unmarried women, it would be of both theoretical interest and policy relevance to investigate whether the effects are concentrated among non-union births, or births to cohabiting women. Unfortunately, limitations of register data on cohabitation do not permit such investigations.[34]

As for all quasi-experimental studies, the external validity of estimates remains a concern. Our external validity is strengthened by the finding of similar effects on third births in both Canada (Milligan 2005) and Israel (Cohen et al. 2013). If anything, our context should facilitate relatively weak effects of reductions of the direct costs of a child. With publicly covered high-quality schooling (through university) and nearly free public high-quality health care, as well as relatively low housing prices, the direct cost of a child in Troms in around 1990 was relatively low compared to most other regions in today's Western world. Hence, our results indicate that the demand for children would be sensitive to the direct cost of childrearing in almost any Western context today.

Our findings contribute to the understanding of how fertility behavior is influenced by income and the direct cost of children. While previous studies have demonstrated convincingly that the low indirect costs of childbearing matters for the high fertility in the Nordic countries (Rindfuss et al. 2010), knowledge of the importance of the direct cost of childbearing and household income been more scarce. Using quasi-experimental data, we show that fertility in Norway increases when the direct cost of a child falls. This indicates that the low direct cost of raising a child contributes to the high fertility in the Nordic countries.

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[34]Unsurprisingly, we know of no survey data set on births to cohabiters that would allow us to both zoom in sufficiently to get a plausible control group, and to retain a sample size sufficient to identify effects of a meaningful size.

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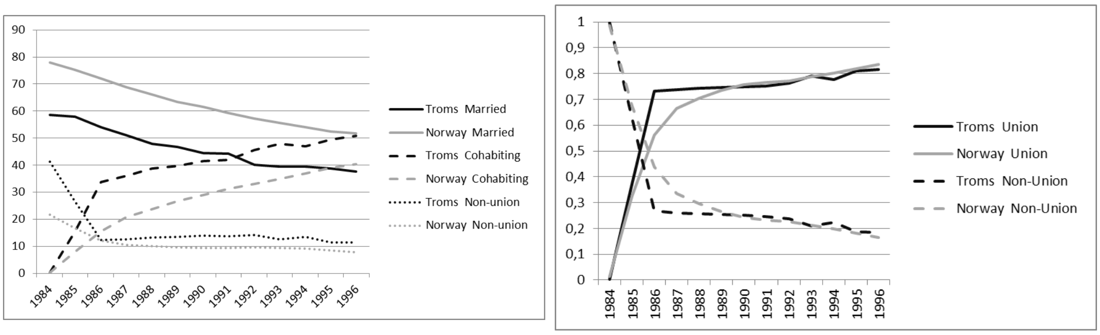
Appendix



(a) All births by union type. Troms and Norway.

(b) Non-marital births by union type. Troms and Norway.

Figure A.1: Births by union type and region

Source (both panels): Data from Medical Birth Register, accessed online at `http://statistikkbank.fhi.no/mfr/`. *F4b, Live births by mothers' union status.* Own calculations in left panel.
The rapid increase in the proportion births to cohabiting women 1984-87 is mainly due to a correction of underreporting of births cohabiters in this period.

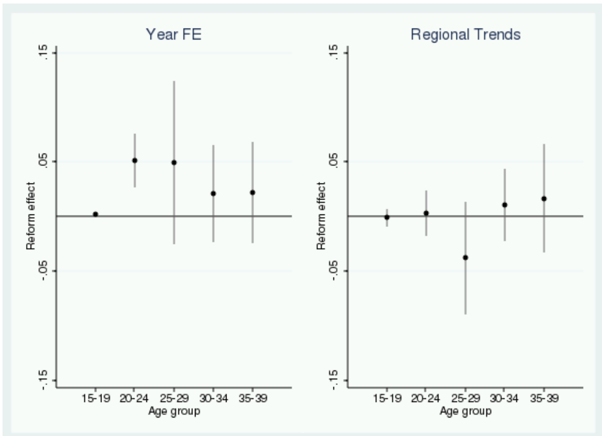


Figure A.2: Reform effects on the propensity to be married.

Note: Estimates from Linear Probability models. All models include controls for municipality fixed effects, age, and age squared. Standard errors are clustered at the municipality level. Sample is women who lived in the Treatment or Control region in Troms at some point in the time span 1984-1988, and who were in the age range 15-39 at some point in the period 1984-1997.

Supplementary Material

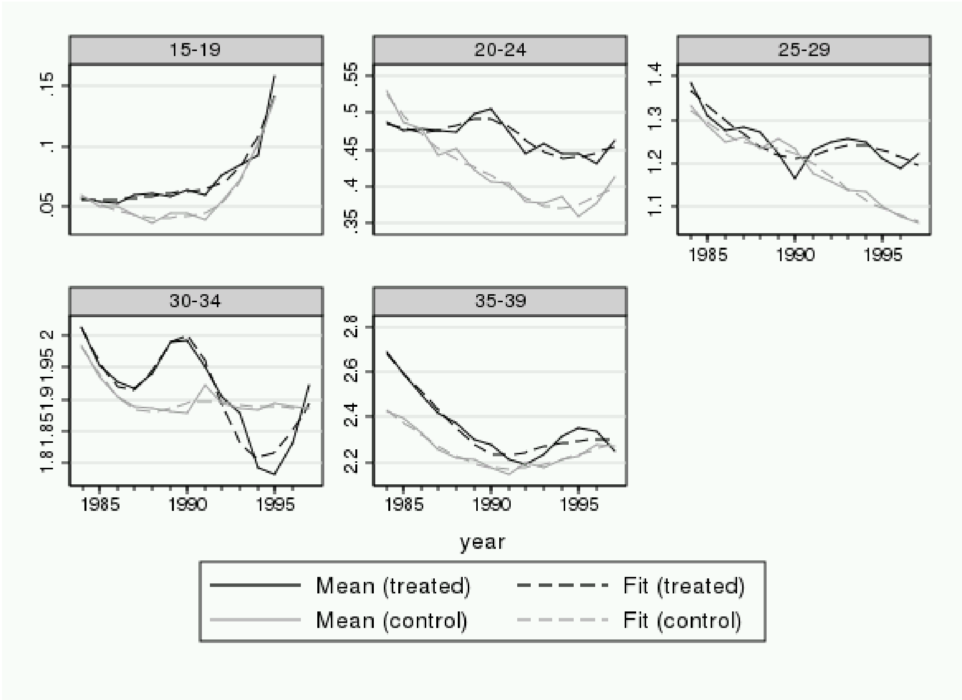


Figure S.1: Trends in number of children by age group and region. Cubic fit.

Note: The full lines show simple means, while the dotted lines are predictions from OLS regression of number of children on age, age squared and age cubed for the given subsample. Sample is women aged 15-39 who lived in the Treatment (black) or Control (grey) region in Troms county at some time point in the period 1984-1997. Information on region of residence is updated yearly.

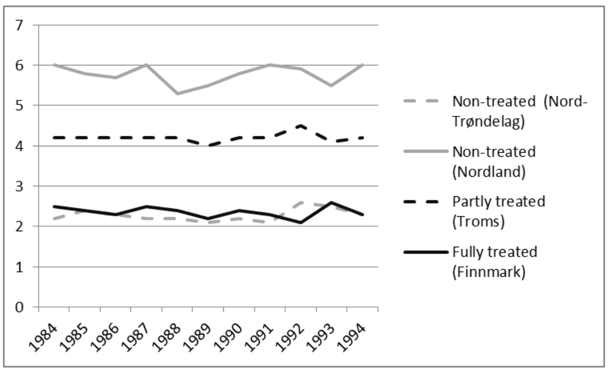


Figure S.2: Induced abortion as percentage of live births. By region and year.
Source: The Medical Birth and Abortion Register, accessed online at `http://statistikkbank.fhi.no/mfr/`. *Induced abortions as percentage of all births by region ("fylke").*

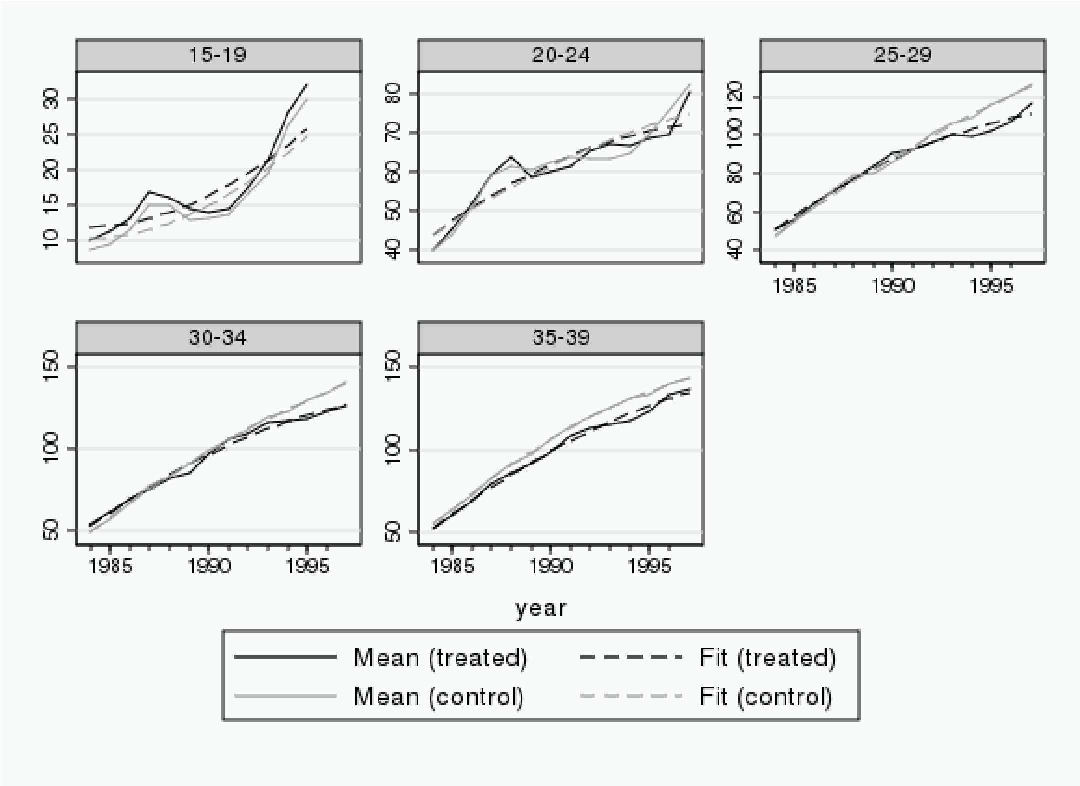
Table S.1: Reform effects by age and educational attainment (upper panel) and age and earnings quintile (lower panel). Linear probability models.

	20-24		25-29		30-34		35-39	
Results by educational attainment								
Lower education								
Reform effects	-0.040	-0.007	0.221	-0.055	0.136	0.171	-0.253	-0.009
	[-0.276,0.196]	[-0.237,0.223]	[-0.142,0.583]	[-0.220,0.111]	[-0.228,0.501]	[-0.066,0.407]	[-0.720,0.215]	[-0.235,0.218]
N	1252	1252	1167	1167	841	841	669	669
Medium education								
Reform effect	0.110**	0.082	-0.044	-0.031	0.103*	0.106**	-0.134*	0.010
	[0.045,0.176]	[-0.030,0.195]	[-0.191,0.103]	[-0.149,0.088]	[0.004,0.202]	[0.046,0.166]	[-0.252,-0.015]	[-0.052,0.072]
N	11691	11691	10056	10056	12059	12059	14486	14486
Higher education								
Reform effects from 89	0.060	0.039	-0.026	-0.004	0.132	0.154	-0.049	-0.111
	[-0.004,0.123]	[-0.048,0.127]	[-0.195,0.142]	[-0.184,0.175]	[-0.099,0.362]	[-0.020,0.329]	[-0.298,0.200]	[-0.268,0.045]
N	5567	5567	3606	3606	3132	3132	2494	2494
Results by earnings quartile								
1st quartile								
Reform effects	0.013	-0.116	-0.080	-0.003	0.217	0.263**	-0.119	0.112
	[-0.104,0.129]	[-0.234,0.002]	[-0.308,0.149]	[-0.230,0.224]	[-0.056,0.489]	[0.083,0.443]	[-0.403,0.165]	[-0.145,0.368]
N	3362	3362	2963	2963	3187	3187	3070	3070
2nd quartile								
Reform effects	0.090*	0.011	0.136	-0.084	0.072	0.057	0.029	-0.023
	[0.010,0.169]	[-0.064,0.085]	[-0.088,0.361]	[-0.290,0.122]	[-0.256,0.401]	[-0.180,0.293]	[-0.220,0.278]	[-0.226,0.179]
N	6727	6727	3220	3220	2906	2906	2875	2875
3rd quartile								
Reform effects	0.084	-0.022	0.042	-0.001	0.102	-0.045	-0.103	0.030
	[-0.007,0.174]	[-0.109,0.065]	[-0.140,0.223]	[-0.100,0.098]	[-0.039,0.243]	[-0.191,0.100]	[-0.212,0.007]	[-0.092,0.152]
N	5859	5859	4449	4449	4745	4745	5480	5480
4th quartile Reform effects from 89	0.109	0.126*	-0.086	-0.070*	-0.019	0.178***	-0.208*	-0.111**
	[-0.078,0.297]	[0.013,0.239]	[-0.230,0.058]	[-0.139,-0.001]	[-0.147,0.109]	[0.103,0.254]	[-0.381,-0.034]	[-0.179,-0.043]
N	2538	2538	4162	4162	5154	5154	6164	6164

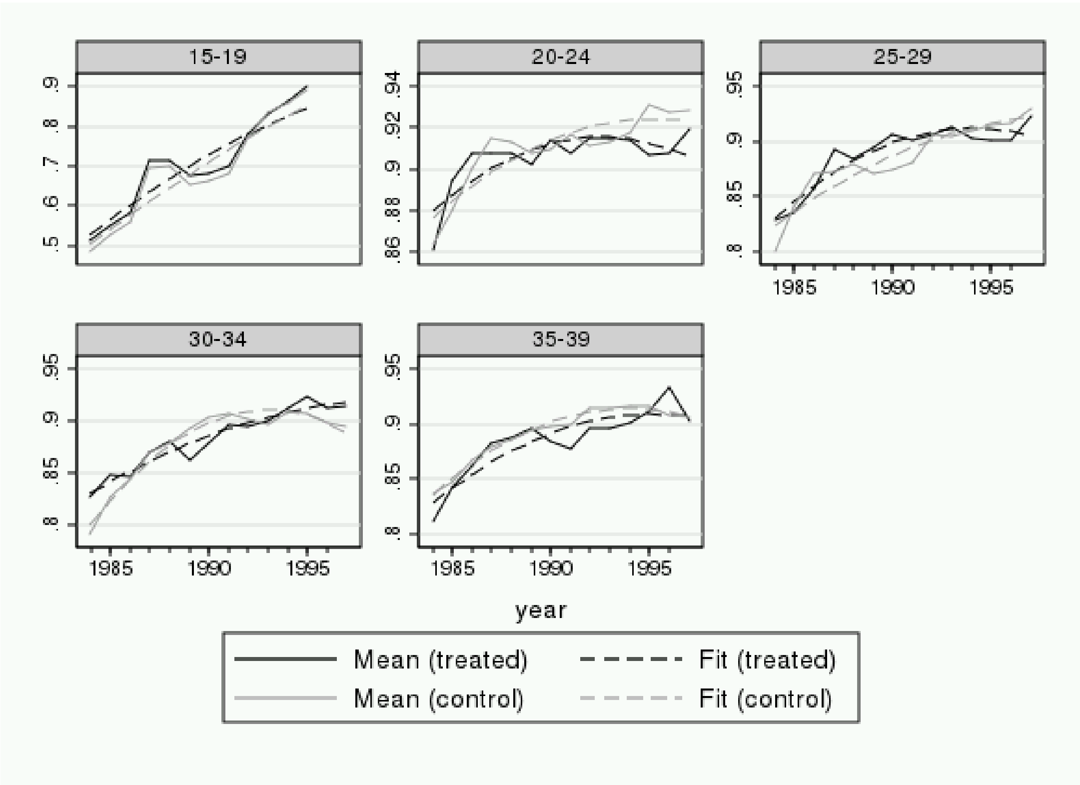
95% confidence intervals in brackets

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Estimates from Linear Probability models. All models include controls for municipality fixed effects, age, and age squared. Standard errors are clustered at the municipality level. Missing earnings are set to zero, and hence included in the lowest quintile. Sample is women who lived in the Treatment or Control region in Troms county at some time point in the period 1984-1988, and who were in the age range 15-39 at some point in the period 1984-1991. Information on region of residence and earnings is updated yearly up to 1988, after information from in 1988 is used as an (exogenous) proxy.



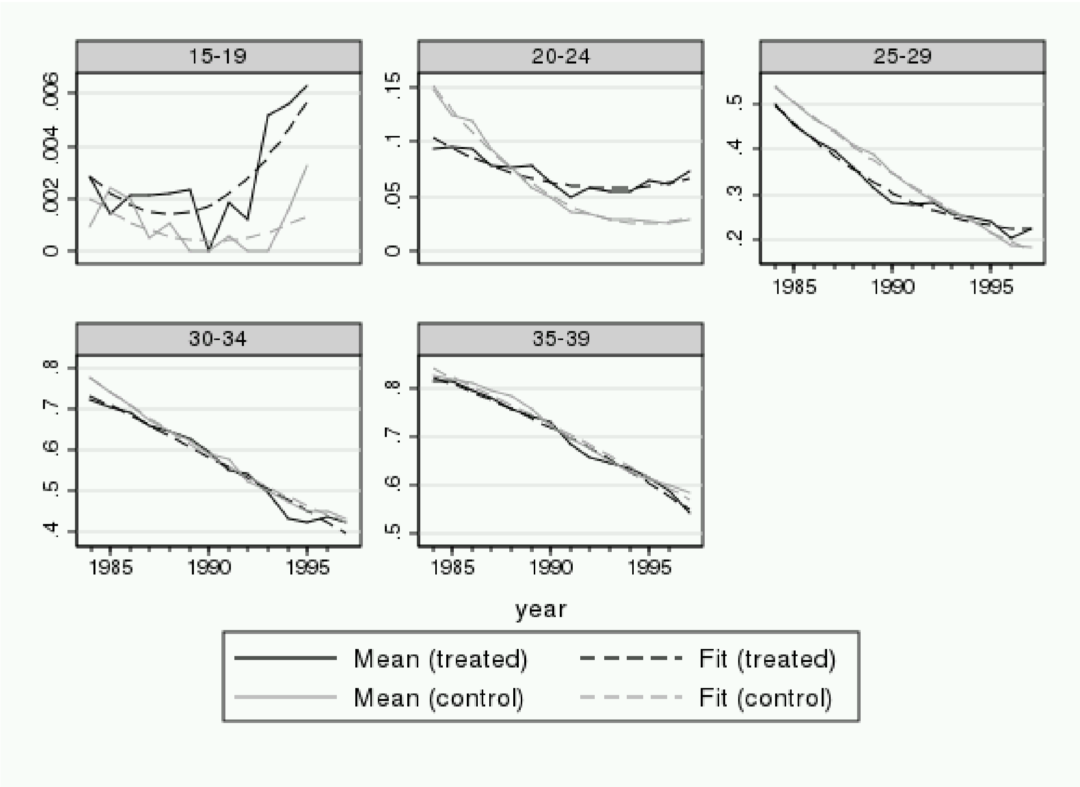
(a) Earnings in 1000 NOK. Missing earnings are set to 0.



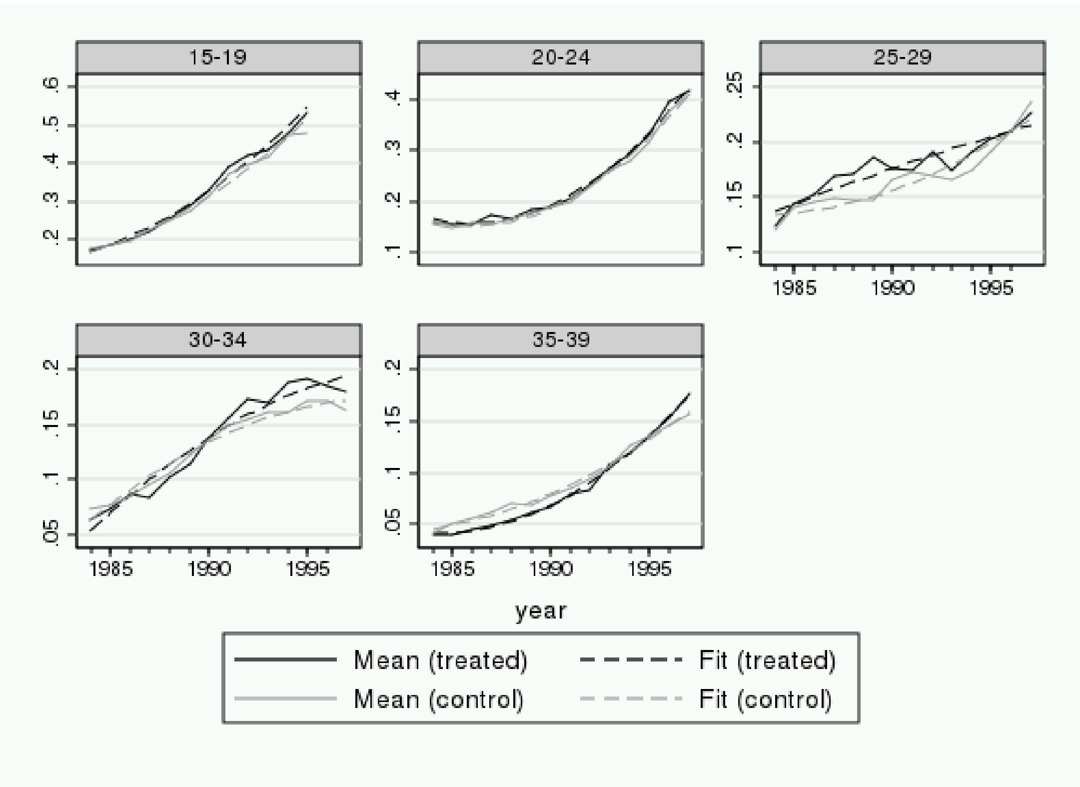
(b) Proportion with earned income.

Figure S.3: Trends in earnings by age group and region.

Note: The full lines show simple means, while the dotted lines are predictions from OLS regression of number of children on age and age squared for the given subsample. Women aged 15-39 who lived in the Treatment (black) or Control (grey) region in Troms county at some time point in the period 1984-1997 are included in the sample. Information on region of residence is updated yearly.



(a) Prop. married



(b) Prop. enrolled in higher education.

Figure S.4: Trends in proportion married (upper panel) and proportion with higher education (lower panel) by age group and region. Dotted lines show a quadratic trend predicted from OLS regression.

Note: The full lines show simple means, while the dotted lines are predictions from OLS regression of number of children on age and age squared for the given subsample. Sample is women aged 15-39 who lived in the Treatment (black) or Control (grey) region in Troms county at some time point in the period 1984-1997. Information on region of residence is updated yearly.

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ISSN: 1892-753X

Design: Siri Boquist

Statistisk sentralbyrå
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