No child left behind: Subsidized child care and children's long-run outcomes*

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Many developed countries currently consider a move towards subsidized, widely accessible child care or pre-school. However, studies on how large-scale provision of child care affects child development are scarce, and focused on short-run outcomes. We analyze a large-scale expansion of subsidized child care in Norway, addressing the impact on children's long-run outcomes. Our precise and robust difference-in-differences estimates show that subsidized child care had strong positive effects on children's educational attainment and labor market participation, and also reduced welfare dependency. Subsample analyses indicate that girls and children with low educated mothers benefit the most from child care.

Keywords: child care, pre-school, child development, long-run outcomes

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The increased demand for child care associated with the rise of maternal employment is attracting the attention of policy makers and researchers alike. Indeed, access to child care has gone up in many developed countries over the last years (OECD, 2004), and there is a heated debate about a move towards subsidized, widely accessible child care or pre-school, as offered in the Scandinavian countries. For example, the European Union's Presidency formulated in 2002 as a policy goal *"to provide childcare by 2010 to at least 90% of children between 3 years old and the mandatory school age and at least 33% of children under 3 years of age"* (EU, 2002, p. 13). Further, Quebec recently introduced highly subsidized child care, and other Canadian provinces are considering similar policies. In the US, the so-called *'Zero to Five Plan'* of US President Obama aims at making states move towards voluntary universal preschool. At the same time, studies on how large-scale provision of child care affect child development are scarce, focused on short-run outcomes, and the findings are mixed.

This paper investigates the effects on children's *long-run* outcomes of a reform from late 1975 in Norway, which led to a large-scale expansion of subsidized child care. An advantage of our long-run perspective is that we get round the issues of whether short-run impacts of child care persist, and perhaps are amplified, over time. For example, as pointed out by Baker et al. (2008), their findings of a negative short-run impact of child care on children's non-cognitive development, could represent an initial cost of socialization, with little or no long-run consequences. Moreover, if investments in human capital have dynamic complementarities, then even a small learning gain in the short-run may improve the long-run prospects of children considerably (Heckman, 2006). By investigating the effects on adult outcomes of intrinsic importance, we also avoid reliance on test scores and changes in test scores that have no meaningful cardinal scale (Cunha and Heckman, 2008).

We find that subsidized child care had large positive effects on children's adult outcomes, measured in their early 30s. This is true with regard to both education and labor market attachment, as well as welfare dependency. In aggregate terms, the additional 17,500 child care places produced about 6,200 years of education. Consistent with the evidence of higher education and stronger labor market attachment, we also find that children exposed to child care delayed child bearing and family formation as adults. Our subsample analysis indicates that most of the effect on education stems from children with low educated mothers, whereas most of the effect on labor market attachment and earnings relates to girls. This suggests that good access to subsidized child care levels the playing field by increasing intergenerational mobility and closing the gender wage gap.

To address the concern for omitted variables bias, we follow much of the previous literature on child care and child development in using a difference-in-differences (DD) approach. The reform we study assigned responsibility for child care to local governments and increased federal subsidies, which immediately generated large variation in child care coverage for children 3–6 years old, both across time and between municipalities.[1] Our main empirical strategy is the following: We compare the adult outcomes for 3 to 6 year olds before and after the reform, from municipalities where child care expanded a lot and municipalities with little or no increase in child care coverage. As described in detail below, formal child care both before the reform period and during the expansion was severely rationed, with informal care arrangements (such as friends, relatives, and unlicensed care givers) servicing the excess demand. In our analysis, we will focus on years immediately after the reform, when child care coverage increased from 10 to 28 percent, which we argue reflect a slackening of constraints on the supply side, rather than a spike in the local demand. We have found no other reforms or changes taking place in this period, which could confound our estimated child care effects. Nevertheless, to increase our confidence in the empirical strategy, we run a battery of specification checks.

To interpret our findings, we take a close look at a number of possible mechanisms. In line with recent studies from several countries (Lundin et al., 2008; Cascio, 2009b; Havnes and Mogstad, 2009), our results indicate that the new subsidized child care crowds out informal care arrangements, with almost no net increase in maternal labor supply. Hence, our study should be viewed as the consequences of moving children from informal care, rather than from parental care, into formal care of relatively high quality.

The paper proceeds by first discussing our study in relation to previous research on child care and child development. Section II outlines the 1975 reform and the succeeding expansion in child care, before describing the characterizing features of publicly subsidized child care

[1]Throughout this paper, child care coverage rates refer to formal care, including publicly and privately provided child care institutions as well as licensed care givers, all eligible to subsidies from the government.

institutions in Norway in the period of study. Section III outlines the empirical strategies and Section IV presents our data. Section V discusses our main results, whereas Section VI reports the specification checks. Section VII investigates heterogenous responses, before Section VIII focuses on the mechanisms behind our findings. Section IX summarizes and concludes.

I Child care and child development

Recent research from a number of fields suggests that investments in early childhood have high returns, especially for disadvantaged children (Knudsen et al., 2006). Studies in neuroscience and development psychology indicates that learning is easier in early childhood than later in life (Shonkoff and Phillips, 2000). Meanwhile, Becker (1964) argues that returns to investments in early childhood are likely to be high, simply due to the long time to reap rewards. Going one step further, Carneiro and Heckman (2004) argue that investments in human capital have dynamic complementarities, implying that learning begets learning.

On this background, Currie (2001) suggests that governments should aim to equalize initial endowments through early childhood development, rather than compensate for differences in outcomes later in life. The role of governments in facilitating child development is particularly important, both from positions on equity and efficiency, if families under-invest in early childhood due to market failures such as liquidity constraints, information failures, and externalities (Gaviria, 2002; Havnes and Mogstad, 2010).

Child care institutions are important arenas for child development, and expanding child care coverage is an explicit goal in many countries. A number of studies show that early childhood educational programs can generate learning gains in the short-run and, in many cases, improve long-run prospects of children from poor families.[2] While the results are encouraging, the programs evaluated were unusually intensive and involved small numbers of particularly disadvantaged children from a few cities in the US. A major concern is therefore that this evidence may tell us little about the effects of child care systems offered to the entire population (Baker et al., 2008). Nonetheless, it has fueled an increasing interest in large-scale provision of

[2]The Perry Preschool and Abecedarian programs are well-known examples of how preschool services can improve the lives of disadvantaged children. See Barnett (1995) and Karoly et al. (2005) for surveys.

child care as a means of advancing child development.

Our paper contributes to a small but rapidly growing literature on the effects on child development of large-scale, publicly subsidized pre-school or child care programs.[3] So far, the evidence is focused on short-run outcomes, and the findings are mixed. Loeb et al. (2007), for instance, find that pre-primary education in the US is associated with improved reading and mathematics skills at primary school entry. However, Magnuson et al. (2007) suggest that these effects dissipate for most children by the end of first grade. Positive effects of child care on children's short-run outcomes are also found by Gormley and Gayer (2005), Fitzpatrick (2008), Melhuish et al. (2008), and Berlinski et al. (2008, 2009). On the other hand, Baker et al. (2008) analyze the introduction of subsidized, widely accessible child care in Quebec, finding no impact on children's cognitive skills but substantial negative effects on children's non-cognitive development. Bernal (2009) suggests that having a mother that works full-time and uses child care has a small, negative effect on ability test scores. These negative effects echo the results in Herbst and Tekin (2008), while Datta Gupta and Simonsen (2007) find that compared to home care, being enrolled in preschool does not lead to significant differences in child non-cognitive outcomes.

While the evidence on short-run effects of large-scale child care programs is of interest, a crucial question is whether these effects persist, and perhaps are amplified, over time. As noted by Baker et al. (2008), negative short-run effects could reflect that children have difficulties in their first interactions with other children. In that case, child care attendance may expose children to these costs earlier on, so that they are better prepared for attending school. In addition, evidence from early intervention programs targeting particularly disadvantaged children suggests that even though the short-run gains in test-scores tended to dissipate over time, there were strong and persistent impacts on long-run outcomes (Heckman et al., 2006). This paper circumvents these issues by investigating the impact of child care on adult outcomes that are of intrinsic importance. By doing so, we also avoid reliance on test scores and changes in test scores that have no meaningful cardinal scale (see Cunha and Heckman, 2008).

To the best of our knowledge, Cascio (2009a) is the only study looking at the long-run

[3]See Almond and Currie (2010) for a recent review of the literature on child care and child development.

effects of large-scale, publicly subsidized pre-school or child care programs. She uses data from four decennial censuses to study the impact of introducing Kindergarten into public schools in the US. Using a cohort-based design, her baseline specification suggests that white children born after the reform in states that began funding kindergartens, largely in the South, were less likely to drop out of high-school. Yet she finds no effect on several other outcomes, like employment, college attendance, and earnings. Nor does she find any effects for blacks. She interprets the general lack of program effects as a result of (i) the low-intensity nature of the program, (ii) significant crowding out of participation in federally-funded programs, such as Head Start, and (iii) cut-backs in state expenditure on schools to fund kindergartens.

II Background

The child care reform. In the post-WWII years in Norway, the gradual entry on the labor market of particularly married women with children, caused growing demand for out-of-home child care. In a survey from 1968, when child care coverage was less than five percent, about 35% of mothers with 3 to 6 year olds stated demand for formal child care (NOU, 1972). In the same survey, only 34% of the latter group of respondents stated that they were in fact using out-of-home child care on a regular basis. Out of these, just 14 percent were in formal child care, while more than 85 percent were using informal arrangements.[4] The severe rationing of formal child care acted as a background for political progress towards public funding of child care.[5] In 1962, federal subsidies to formal child care were assigned a permanent post on the national budget, and increased over the subsequent ten years from a modest USD 50 per child care place to a maximum of more than USD 1,200 annually.[6] The child care subsidies were contingent on a federally determined maximum price to be paid by the parents, which in 1972 was about USD 215 per month for full time care (NOU, 1972).

In 1972, the Norwegian government presented the Kindergarten White Paper (NOU, 1972),

[4]Relatives stand out as the largest group of informal care givers at 35 percent, followed by play parks at 20 percent, maids at 14 percent, other unlicensed care givers at 10 percent, and finally more irregular arrangements (such as neighbors and friends) at 7 percent (NOU, 1972).

[5]See Leira (1992, ch. 4) and The Norwegian Ministry of Children and Family Affairs (1998) for detailed surveys of the history of Norwegian child care policies since WWII.

[6]Throughout this paper, all monetary figures are in US dollars, and fixed at 2006-level (NOK/USD = 6.5).

proposing radical changes in public child care policies. To (i) create positive arenas for child development, (ii) free labor market reserves among mothers, and (iii) lessen the burden on parents and relieve stress in the home, it was argued that child care should be made universally available. This marked a strong shift in child care policies, from focusing on children with special needs (in particular disabled children and children from disadvantaged families) to a focus on a child care system open to everyone. In June 1975, the Kindergarten Act was passed by the Norwegian parliament with broad bipartisan political support. It assigned the responsibility for child care to local municipalities, but included federal provisions on educational content, group size, staff skill composition, and physical environment. By increasing the level of federal subsidies for both running costs in general and investment costs for newly established institutions, the government aimed at quadrupling the number of child care places to reach a total of 100,000 by 1981.[7] In the years following the reform, the child care expansion was progressively rolled out at a strong pace, with federal funding more than doubling from USD 34.9 million in 1975 to 85.8 million in 1976, before reaching 107.3 million in 1977.[8] This implied an increase in the federal coverage of running costs from about 10% in 1973 to 17,6% in 1976, and further to 30% in 1977. From 1976, newly established child care places received additional federal funds for a period of five years. Municipalities with relatively low child care coverage rates were awarded the highest subsidies.

Altogether, the reform constituted a substantial positive shock to the supply of formal child care, which had been severely constrained by limited public funds. In succeeding years, the previously slow expansion in subsidized child care accelerated rapidly. From a total coverage rate of less than 10% for 3 to 6 year olds in 1975, coverage had shot up above 28% by 1979. Over the period, a total of almost 38,000 child care places were established, more than a doubling from the 1975-level. By contrast, there was almost no child care coverage for 1 and 2 year olds during this period. Figure I draws child care coverage rates in Norway from 1960 to 1996 for 3 to 6 year olds. As is apparent from the figure, there has been strong growth in child

[7]In addition, the price-setting was delegated to local municipalities, abolishing the federally determined maximum parental price for child care subsidies. However, Gulbrandsen et al. (1981) report survey data suggesting that the maximum price to be paid by the parents actually changed little in the years following the reform, and formal child care remained rationed well into the 1990s.

[8]Source: National budgets 1975/76 through 1978/79.

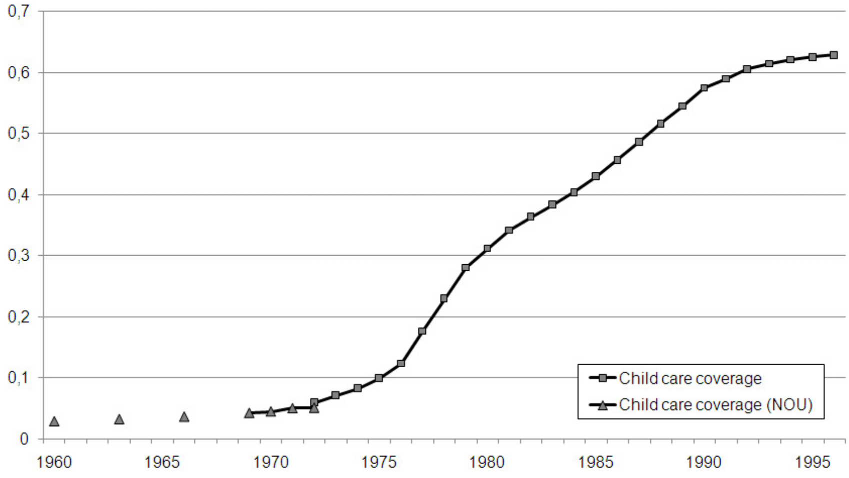

Figure I: Child care coverage rate in Norway 1960–1996 for children 3–6 years old.
Sources: Administrative data for 1972–1996. Data for 1960–1972 from NOU (1972), Table II.1.

care coverage rates since 1975, particularly in the early years. In our analysis, we will focus on the early expansion, which likely reflects the abrupt slackening of constraints on the supply side caused by the reform, rather than a spike in the local demand.

We might worry about confounding the estimated child care effects with other reforms or changes taking place in the same period. However, we have found no significant reforms or breaks in trends that could be of concern for our estimations. An extension in maternity leave implemented in 1977 did not affect the children in our sample directly, but could potentially influence family size, which could in turn matter for child development. However, the reform was nationwide, and should be controlled for by cohort fixed-effects. In addition, our rich set of controls may pick up potentially remaining effects of this policy change.[9]

Importantly, there were no significant changes in the Norwegian educational policies affecting the cohorts of children we consider. On the contrary, Norway was known for its unified public school system based on a common national curriculum, rooted in a principle of equal rights to high-quality education, regardless of social and economic background or residency. This is mirrored in very similar expenditure levels per student across municipalities and virtually no private schools.[10]

The Organization of Formal Child Care. To interpret our results, we must understand the

[9]Furthermore, there is no evidence of a break in the fertility trends around the time of the maternity leave reform. Nor is there any sign of an increase in fertility rates in municipalities where child care expanded a lot, compared to municipalities with little or no increase in child care coverage.

[10]See Telhaug et al. (2006) and Volckmar (2008) for an in-depth discussion of the Norwegian educational system since the 1950s.

Table I: Child care institutions by ownership structure

	1975	**1977**	**1979**	**1981**
Private (%)	28.4	26.7	26.3	21.9
Municipality (%)	48.6	45.4	46.9	51.2
Church (%)	7.3	8.0	8.6	8.6
Cooperatives (%)	5.6	8.2	9.7	10.0
No. of child care institutions	880	1,469	2,294	2,754
No. of children in child care (3–6 y.o.)	25,536	43,239	63,218	73,152
Coverage rate (3–6 y.o., %)	10.0	17.6	28.1	34.2

Notes: Private ownership indicates ownership by a private firm, organisation or foundation. Cooperatives are parental or residential. Categories not reported are ownership by state, regions and other.

type of care we are studying. The Ministry of Consumer Affairs and Administration was responsible for overall regulation of formal child care. Specifically, the Kindergarten Act regulated the authorization, operation and supervision of formal child care institutions. The act defined formal child care institutions as care and educationally oriented enterprises for pre-school children, where an educated preschool teacher was responsible for the education. Formal child care institutions were run either by the municipalities or by firms, public institutions or private organisations, under the approval and monitoring of local authorities in the municipality. Table I reports child care institutions by owner biannually from 1975 through 1981, and shows the strong growth in municipal and cooperative child care centers. Over the period, the share of private centers decreased from 28.4 to 21.9 percent, driven almost entirely by a decline in the share of centers run by private organizations.

Regardless of ownership, formal child care institutions were required to satisfy federal provisions on educational content and activities, group size, staff skill composition and physical environment. The Kindergarten Act specified regulations, and guidelines were formulated for activities and content. To be eligible for subsidies, institutions were obliged to meet the requirements and follow the guidelines. To secure opportunities for parental involvement and promote cooperation between staff and parents, the Kindergarten Act required that every institution must have a parent council and a coordinating committee. Local authorities were required by law to monitor the fulfillment of these federal provisions.

As discussed above, formal child care institutions were financed jointly by the federal government, municipalities and parents. All approved institutions received subsidies for running and establishment costs from the federal government. Subsidies were determined on the basis of the number and age of children, and the amount of time they spend in formal child care. In general, formal child care institutions were open during normal working hours. All children were eligible, and open slots were in general allocated according to length of time on the waiting list and age. Only under special circumstances could a child gain priority on the waiting list.

Every formal child care institution had to be run by an educated pre-school teacher responsible for day-to-day management. The pre-school teacher education is a college degree, including supervised practice in a formal child care institution. Through his or her position and training, this head teacher was responsible for ensuring satisfactory planning, observation, collaboration and evaluation of the work. The head teacher was also in charge of staff guidance, as well as collaboration with parents and local authorities, such as health stations, child welfare services and educational/psychological services. In addition, formal child care institutions were required to have at least one educated pre-school teacher per 16 children aged 3–6. Teachers typically worked closely with one or two assistants, and were responsible for the educational programmes in separate groups and for day-to-day interaction with parents. There were no educational requirements for assistants.

In terms of educational content, a social pedagogy tradition dominated the child care practices, according to which children where supposed to develop social, language and physical skills mainly through play and informal learning.[11] The informal learning was typically carried out in the context of day-to-day social interaction between children and staff, in addition to specific activities for different age groups.

Overall, formal child care in Norway (along with other Nordic countries) was characterized by relatively high expenditure levels per child compared to large-scale programs in other countries. For example, the average yearly expenditure for a slot in formal child care was ap-

[11]The social pedagogy tradition to early education has been especially influential in the Nordic countries and Central-Europe. In contrast, a so-called pre-primary pedagogic approach to early education has dominated many English and French-speaking countries, favoring formal learning processes to meet explicit standards for what children should know and be able to do before they start school.

proximately USD 6,600.[12] This is, for instance, substantially higher than the expenditures for the Head Start Program in the US aimed at low-income families, which cost around USD 5,000 per year (Currie, 2001). The high expenditure levels were mirrored in fairly extensive requirements to qualifications of child care staff and physical environment, as well as a relatively low number of children per staff. For example, the average staff-child ratio was about 1:8 in 1977. In comparison, in the US and Canada, the corresponding ratio is 1:12, in Spain 1:13, and France 1:19 (see Datta Gupta and Simonsen, 2007).

III Identification strategy

To estimate the effect of the expansion of subsidized child care on children's outcomes we apply a DD approach, exploiting that the supply shocks to formal child care were larger in some areas than others. Below, we will first describe our main empirical strategy, before discussing alternative specifications addressing potential threats to identification.

Main empirical strategy. Our main empirical strategy is the following: We compare the adult outcome of interest for 3 to 6 year olds before and after the reform, from municipalities where child care expanded a lot (i.e. the treatment group) and municipalities with little or no increase in child care coverage (i.e. the comparison group).

The child care expansion started in 1976, affecting the *post-reform cohorts* born 1973–1976 with full force, and to a lesser extent the *phase-in cohorts* born 1970–1972. The *pre-reform cohorts* consist of children born in the period 1967–1969. We consider the period 1976–1979 as the child care *expansion period*. Starting in 1976 gives the municipalities some time to plan and react to the policy change. Also, 1976–1979 was the period with the largest growth in child care coverage. In the robustness analysis, we make sure that our results are robust to changes in the exact choice of expansion period.

To define the treatment and comparison group, we order municipalities according to the percentage point increase in child care coverage rates from 1976 to 1979. We then separate the sample at the median, the upper half constituting the *treatment municipalities* and the lower half

[12]Estimated annual budgetary cost per child care place from NOU (1972) is about USD 5,400 per child 3–6 years old. In addition, investment costs are estimated at about USD 12,000 per child care place, adding USD 1,200 to the annual cost if written down over ten years.

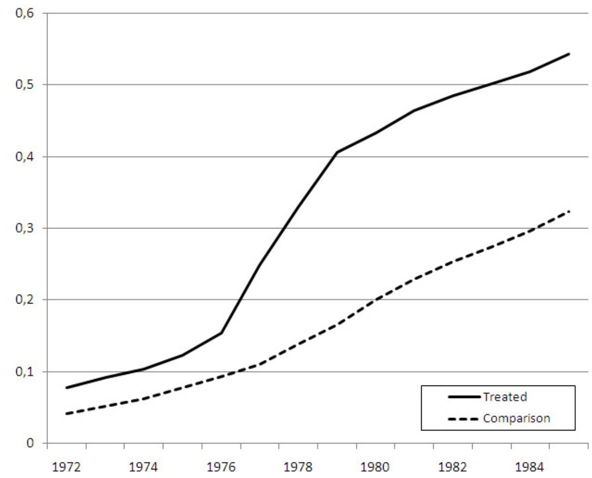

Figure II: Child care coverage rates 1972–1985 for 3–6 year olds in treatment and comparison municipalities.
Notes: Treatment (comparison) municipalities are above (below) the median in child care coverage growth from 1976 to 1979.

the *comparison municipalities*. Figure II shows child care coverage before and after the 1975 reform in treatment and comparison municipalities (weighted by population size). The graphs move almost in parallel before the reform, while child care coverage in treatment municipalities kinks heavily after the reform. This illustrates that our study compares municipalities that differ distinctly in terms of changes in child care coverage within a narrow time frame. In the robustness analysis, we take several steps to ensure that our results are robust to the exact child care coverage cut-off, defining treatment and comparison municipalities.

Our main regression model, estimated by OLS over the sample of children born during the period 1967–1976, can be defined as

$$Y_{ijt} = \psi_t + \gamma_1 Treat_i + \gamma_2(Treat_i \cdot Phasein_t) + \theta(Treat_i \cdot Post_t) + X'_{ijt}\beta + \epsilon_{ijt} \qquad (1)$$

where Y is the outcome of interest measured in 2006, i indexes child, j indexes family, and t indexes the year the child turns 3 years old. The vector of covariates X includes dummy variables for parent's birth cohort, their education when the child is 2 years old, their age at first birth, the number of older siblings (also capturing birth order) and relocation between municipalities within treatment/comparison area, the child's sex and immigrant status, as well as municipality-specific fixed effects. The dummy variable $Treat_i$ is equal to 1 if child i lives in the treatment area, $Phasein_t$ and $Post_t$ are dummy variables equal to 1 when $t \in [1973, 1975]$

and $t \in [1976, 1979]$ respectively, while ψ_t are cohort-specific fixed effects.[13]

The parameter of interest, θ, captures the average causal effect on children who reside in the treatment area in the post-reform period, of additional child care slots following the reform in the treatment municipalities compared to the comparison municipalities. There are two types of averaging underlying this average causal effect. First, there is averaging over the impacts on children from different municipalities in the treatment area. And second, there is averaging across the marginal effects of the additional child care slots.

Like in Baker et al. (2008), we will interpret θ as an intention-to-treat effect (ITT), since our regression model estimates the reduced form impacts on all children from post-reform cohorts who reside in the treatment area. An advantage of the ITT parameter is that it captures the full reform impact of changes in both formal and informal care arrangements, as well as any peer effects on children who were not attending child care. However, since this parameter averages the reform effects over all children in the municipality, it reflects poorly the *size* of the child care expansion. To arrive at the treatment-on-the-treated (TT) effect, we follow Baker et al. and scale the ITT parameter with the probability of treatment. Specifically, we divide the ITT parameter with the increase in child care coverage following the reform in the treatment group relative to the comparison group. For example, $TT = ITT/0.1785$ in our baseline specification. The TT parameter gives us the effect of child care exposure – per child care place – on children born in post-reform cohorts who reside in the treatment area. In our main results, we report both the ITT and the TT estimates.

The DD approach controls for unobserved differences between children born in different years as well as between children from treatment and comparison municipalities. The identifying assumption is that the change in the outcome of interest for 3 to 6 year olds before and after the reform would have been the same in the treatment municipalities as in the comparison municipalities, in the absence of the reform. A concern could be that the time trend in children's outcomes differs by, say, parent's education, while there are systematic differences

[13] Some of the outcomes of interest are limited dependent variables. In these cases, our linear probability model will be the best least-squares approximation of the true conditional expectation function. As noted by Angrist (2001), if there are no covariates or they are discrete, as in our case, linear models are no less appropriate for limited dependent variables than for other types of dependent variables. In any case, we have checked that our results are robust to alternative approximations of the conditional expectation function, estimating Logit and Probit models.

in parental education between treatment and comparison municipalities. To address such concerns for omitted variables bias, we estimate equation (1) with and without the set of controls X.

Because we also control for municipality-specific fixed effects, it is not necessary that the child care expansion is unrelated to municipality characteristics. It is useful, however, to understand the determinants of the expansion across municipalities. In Section IV, we investigate this closely, finding that the characteristics of treatment and comparison municipalities are fairly similar in terms of political and demographic composition as well as local government expenditure and income. A notable exception is that the expansion was strongest in municipalities with the lowest ratio of formal child care coverage to employment rate of mothers with children in child care age. This conforms well to intuition, since federal subsidy rates were higher for municipalities with low child care coverage prior to the reform, but also because the local political pressure for expansion of formal care is likely to be stronger in areas where child care was severely rationed.

Although municipality-specific fixed effects picks up the direct effects of pre-determined factors of the municipalities, like differences in rationing of formal child care prior to the reform, we may worry about the determinants of the child care expansion being systematically related to underlying trends in child outcome. And even though the DD approach controls for unobserved differences both between children born in different years as well as between children from treatment and comparison municipalities, there could be changes over time in the *differences* in the unobservable characteristics of children from the two groups. As always in policy evaluation using non-experimental data we cannot completely guard against such omitted variables bias. Yet to increase the confidence in our identification strategy, we run a battery of specification checks.

Alternative specifications. To investigate the assumption of a common time trend between the treatment and comparison group in the absence of the reform, we perform two different placebo-tests. In the first placebo-test, we pretend that the child care expansion took place in the pre-reform period. The second placebo-test exploits that taller adults have, on average, higher education and earn more than other workers, and that genetic factors are the primary determinant of variation in adult height in developed countries. Significant effects in the placebo tests would therefore suggest that our estimated child care effects reflect differential time trends, rather than true policy impacts.

To allow treatment and comparison areas to follow different trends due to, say, differences in child care rationing prior the reform, we further estimate equation (1) with municipality-specific time trends. We also interact the cohort fixed-effects with pre-reform municipality characteristics, allowing for differential inter-cohort time trends across different municipalities. To make sure that our results are not driven by secular changes between urban and rural areas coinciding with the child care reform, we drop the three big cities from our analysis. Further, we add family-specific fixed effects, limiting the comparison to siblings before and after the reform that have the same family background but experience different exposure to child care. In addition, we take several steps to address the concern for selective migration of families into treatment and comparison municipalities.

As discussed above, under the common trend assumption our DD estimator captures the average causal effect of additional child care slots following the reform in treatment relative to comparison municipalities. When drawing histograms by treatment status of the distributions of municipalities by child care coverage rate in 1976 and 1979, we see a fairly good coherence in coverage rates before the reform, and a striking difference after the reform.[14] It is also evident that treatment intensity varies within the two groups of municipalities. In the robustness analysis, we therefore consider variations in treatment intensity by changing the child care coverage cut-off defining treatment and comparison municipalities. In addition, we follow Berlinski et al. (2009) in regressing child outcome on child care coverage rate in each municipality, controlling for cohort and municipality fixed-effects, as well as a set of controls. This regression model, estimated by OLS over the sample of children born during the period 1967–1976, restricts the marginal effects of additional child care slots to be constant, and can be defined as

$$Y_{ijt} = \delta_t + \zeta CC_{it} + X'_{ijt}\varphi + \epsilon_{ijt} \tag{2}$$

[14]See Figure A1 reported in the Appendix.

where CC_{it} is the average child care rate in the municipality of child i from the year t when the child turns 3 years old until, but not including, year $t + 4$ when he or she turns 7 and starts primary school.

IV Data

Our data is based on administrative registers from Statistics Norway covering the entire resident population of Norway from 1967–2006. The data contains unique individual identifiers that allow us to match parents to their children. As we observe each child's date of birth, we are able to construct birth cohort indicators for every child in each family. The family and demographic files are merged through the unique child identifier with a wide range of his or her adult outcomes measured in 2006, including educational attainment, earnings, welfare dependency, household type and composition, and height. The information on educational attainment is based on annual reports from Norwegian educational establishments, whereas the income and welfare data are collected from tax records and other administrative registers. The household information is from the Central Population Register, which is updated annually by the local population registries and verified by the Norwegian Tax Authority. Unlike the other outcome measures, adult height is collected from military records and is only available for males, since military service is compulsory for men only. Before entering military service, medical and psychological suitability is assessed, including a measurement of height.[15]

Importantly, we also have administrative register data on all formal child care institutions and their location from 1972, reported directly from the institutions to Statistics Norway. All licensed care givers are required to report annually the number of children in child care by age. Merging this data with the demographic files containing information about the total number of children according to age and residency, we construct a time series of annual child care coverage (by age of child) in each of the 414 municipalities. The coverage and reliability of Norwegian register data is considered to be exceptional, as documented by the fact that they

[15]Eide et al. (2005) examine patterns of missing data in military records for males from the 1967-1987 cohorts. Of those, 1.2% died before 1 year and 0.9% died between 1 year of age and registering with the military at about age 18. About 1% of the sample of eligible men had emigrated before age 18, and 1.4% of the men were exempted because they were permanently disabled. An additional 6.2% are missing for a variety of reasons including foreign citizenship and missing observations.

received the highest rating in a data quality assessment conducted by Atkinson et al. (1995).

We start with the entire population of children born 1967–1976, alive and resident in Norway in 2006. This sample consists of 575,300 children, spanning these 10 birth cohorts. The choice of cohorts serves three purposes. Since our outcomes are measured in 2006, treated children are 30–33 years old at the time of measurement, which should be suitable when assessing children's adult outcomes (see e.g. Haider and Solon, 2006). Second, since treatment and comparison groups are defined by the expansion in child care from 1976 to 1979, the regional and time variation between the two groups breaks down as we move away from 1979. Indeed, the coverage rates do converge slowly after 1979. Finally, to ensure comparability of children before and after the reform, we don't want the cohorts to be too far apart.

We restrict the sample to children whose mothers were married at the end of 1975, which makes up about 92% of the above sample. The reason for this choice is that our family data does not allow us to distinguish between cohabitants and single parents in these years. As parents' family formation may be endogenous to the reform, we only condition on pre-reform marital status. To avoid migration induced by the child care reform, we also exclude children from families that move between treatment and comparison municipalities during the expansion period, which makes up less than 5% of the above sample. Finally, we exclude a handful of children whose mother had a birth before she was aged 16 or after she was 49. Rather than dropping observations where information on parents' education is missing, we include a separate category for missing values. The education of the parents is measured when the child is 2 years old. The number of older siblings relates to children born to each mother. The final sample used in the estimations consists of 499,026 children from 318,367 families, which makes up about 87 percent of the children from each cohort.

When interpreting our results, it is necessary to have these sample selection criteria in mind. We focus on children of married mothers. Thus, our results do not speak to the literature on early childhood educational programs targeting special groups like children of single mothers, but these are not the central focus of the current policy debate. To arrive at the TT parameter, we assume equal take up of child care among children included in and excluded from our sample. In particular, we assume that children of single and cohabitant parents are as likely to take up the new child care places as children of married mothers. If the take-up rate is higher (lower) for children excluded from our sample, the TT parameter will be downward (upward) biased. Unfortunately, we do not have data on child care use by child and parental characteristics.

The adult outcomes are defined as follows. Years of education of the child is the number of completed years of education in 2006. Attended college means having at least 13 years of education, while high-school dropout is having no more than 11 years of education. To measure labor market attachment and welfare dependency, we rely on the basic amount thresholds of the Norwegian Social Insurance Scheme (used to define labor market status, determining eligibility for unemployment benefits as well as disability and old age pension). In 2006, one basic amount is about USD 10,500. To account for non-linearities in the effects on earnings, we use four different earnings measures. An individual is defined as a low earner if he or she earns less than two basic amounts (also including zero earnings), whereas an average earner has at least four basic amounts in earnings. High and top earners are defined as having at least eight and twelve basic amounts, respectively. Our earnings measure includes wages and income from self-employment. A person is defined as being on welfare if he or she receives more than one basic amount in public cash transfers. Individuals are defined as single with no child if they are neither married/cohabitant or single parent. A person is defined as a single parent if he or she is single and the primary care giver to a child, whereas individuals are defined as parent if they are in a couple with children or are single parents. Finally, adult height is reported in centimeters, measured after the children turn eighteen, and for the great majority before their twentieth birthday. In developed countries, adult height is typically attained before age 18 for boys (Case and Paxson, 2008).

Descriptive statistics. Table II shows means for our dependent variables. As is evident from the table, there are modest or no differences in the outcomes of the treatment and comparison group for pre-reform cohorts. The phase-in cohorts diverge slightly in most variables, while post-reform cohorts show distinct differences. In a DD framework, this pattern is suggestive of significant effects of child care on children's outcomes. When graphing the means of all outcomes by child cohort in treatment and comparison municipalities, we see a good coherence between the time trends of the groups before the reform, and a substantial change in the relative

Table II: Descriptive statistics: Outcome variables

	– Level – **Treated** Pre-reform	– Differences – **Treated – Comparison** Pre-reform	Phase-in	Post-reform
Years of education	12.65 [2.56]	0.0435	0.0627	0.1180
Attended college	0.3740 [0.4839]	0.0074	0.0138	0.0231
High school dropout	0.2625 [0.4400]	-0.0010	-0.0031	-0.0101
Low earner	0.1546 [0.3616]	-0.0019	-0.0031	-0.0068
Average earner	0.6929 [0.4613]	0.0067	0.0076	0.0172
High earner	0.1620 [0.3684]	0.0145	0.0149	0.0104
Top earner	0.0417 [0.1999]	0.0066	0.0049	0.0033
On welfare	0.1624 [0.3688]	-0.0104	-0.0131	-0.0193
Parent	0.8082 [0.3937]	-0.0113	-0.0214	-0.0304
Single, no child	0.1396 [0.3466]	0.0073	0.0112	0.0160
Single, parent	0.0838 [0.2770]	-0.0030	-0.0010	-0.0037
Height (boys only)	179.94 [6.4394]	0.1658	0.0773	0.1305
No. of children (level)		77,933 – 87,832	74,182 – 83,621	84,052 – 91,406

Notes: Pre-reform cohorts are born 1967–1969, phase-in cohorts are born 1970–1972, and post-reform cohorts are born 1973–1976. Treatment (comparison) municipalities are above (below) the median in child care coverage growth from 1976 to 1979. Outcomes are defined in Section IV. Standard deviations are in brackets.

outcomes after the reform.[16] The last row of Table II shows means for height, our placebo outcome. We see immediately that differences in all periods are very small, and never more than 2.5 percent of the standard deviation.

Our DD approach identifies the effects of child care by comparing the change in the outcome of interest before and after the reform of children residing in treatment and comparison areas. Substantial changes over time in the *differences* in the observable characteristics of the two groups might suggest unobserved compositional changes, calling our empirical strategy into question. Table III shows means of our control variables for characteristics of the child and the parents. It turns out that the treatment and comparison groups have fairly similar characteristics. More importantly, there appears to be small, and generally insignificant, changes over time in the relative characteristics of the two groups.

A concern in applying linear regressions is lack of overlap in the covariate distribution. As emphasized by Imbens and Wooldridge (2009), this can be assessed by the (scale-invariant) normalized difference measure. For each covariate, the normalized difference is defined as the

[16]See Figures A3-A5 reported in the Appendix.

Table III: Descriptive statistics: Control variables

	– Level – **Treated** Pre-reform	– Differences – **Treated – Comparison** Pre-reform	Phase-in	Post-reform
Male	0.5069	-0.0014	0.0036	0.0017
	[0.5000]	{-0.0020}	{0.0051}	{0.0024}
No. of older siblings	2.1319	-0.0818	-0.0736	-0.1118
	[1.2343]	{-0.0456}	{-0.0432}	{-0.0718}
Mother's age at	23.3286	0.5671	0.5916	0.6472
first birth	[4.0432]	{0.1021}	{0.1119}	{0.1223}
Father's age at	26.5592	0.4936	0.4867	0.5444
first birth	[5.2946]	{0.0675}	{0.0705}	{0.0823}
Mother's education when	9.6618	0.2805	0.2817	0.3072
child 2 y.o.	[2.0739]	{0.0987}	{0.0992}	{0.1066}
Father's education when	10.3715	0.3730	0.3787	0.4044
child 2 y.o.	[2.8162]	{0.0971}	{0.0995}	{0.1065}
Immigrant	0.0566	0.0110	0.0165	0.0162
	[0.2311]	{0.0355}	{0.0535}	{0.0534}
Relocated	0.0358	-0.0016	0.0021	0.0070
	[0.1858]	{-0.0061}	{0.0061}	{0.0172}
No. of children (level)		77,933 – 87,832	74,182 – 83,621	84,052 – 91,406

Notes: Pre-reform cohorts are born 1967–1969, phase in-cohorts are born 1970–1972, and post-reform cohorts are born 1973–1976. Treatment (comparison) municipalities are above (below) the median in child care coverage growth from 1976 to 1979. Control variables are defined in Section IV. Standard deviations are in square brackets, and normalized differences are in curly brackets.

difference in averages by treatment status, scaled by the square root of the sum of variances. Imbens and Wooldridge suggest as a rule of thumb that linear regression methods tend to be sensitive to the functional form assumption if the normalized difference exceeds one quarter. Table III displays normalized differences for our controls in curly brackets, indicating that lack of overlap should be of little concern for the estimated effects.

As discussed above, because we control for municipality-specific fixed effects, it is not necessary that the child care expansion is unrelated to municipality characteristics. However, if determinants of the expansion are systematically related to underlying trends in children's potential outcomes, we may be worried about differences in the characteristics of treatment and comparison municipalities. For example, if expansive municipalities are aiming at counteracting a particularly negative trend in child development, or if they are taking some of the child care investment funds from other policies affecting child development, then our estimates will be biased downwards. Similarly, if municipalities expand in order to stimulate a particularly positive trend or if expansive municipalities also invest in other means of stimulating child development, then our estimates will be biased upwards. It is useful, therefore, to understand the determinants of the expansion across municipalities.

The treatment and comparison municipalities are quite well spread out over Norway, covering urban and rural municipalities.[17] In our baseline specification, five of the ten largest cities – by the number of children in our sample – are defined as treatment municipalities (Oslo, Bergen, Stavanger, Bærum and Fredrikstad), while the others are defined as comparison municipalities (Trondheim, Kristiansand, Tromsœ, Skien and Drammen). Furthermore, there appears to be no substantial differences in terms of local government expenditure per capita, in total or on primary school in particular.[18] This is most likely because of strict federal provisions for minimum standards of different local public services, and considerable ear-marked grants-in-aid from the central government. The same holds for local government income, consisting largely of grants-in-aid from the central government, local income taxes, and user fees. This comes as no surprise, as the federal government determines the tax rate and the tax base of the income tax. Also, the federal government used equalization transfers to redistribute income from rich to poor municipalities, such that local differences in revenues are largely offset (Løken, 2010). Interestingly, there are no noticeable differences in the share of female voters between the municipalities of the treatment and comparison area, nor is there significant disparity in the socialist shares of voters. This conforms well to the fact that there was broad bipartisan support for child care expansion in Norway in the 1970s. Further, we do not find any substantial differences in population size or the population shares of neither 0 to 6 year olds, nor females of fecund age, 19–35 or 36–55 years old.

There are, however, some notable differences between treatment and comparison municipalities. Most importantly, the ratio of child care coverage to employment rate of mothers of 3–6 year olds pre-reform, is substantially lower in treatment than in comparison municipali-

[17] See Figure A2 reported in the Appendix.

[18] Table A1, reported in the Appendix, displays characteristics of the municipalities in 1976, in the treatment and comparison area. In addition, when examining the pre-reform trends in time-varying municipality characteristics (such as municipal expenditures, primary school expenditures, tax income, average education, labor force participation, family patterns), we find a good coherence between the treatment and the comparison municipalities. As expected, we find a good coherence between the time trends of the treatment and the comparison municipalities.

ties. In treatment municipalities, there is on average more than four employed mothers for each child care place, while the same ratio is less than three-to-one in comparison municipalities. This is not surprising, since federal subsidy rates were higher for municipalities with low child care coverage prior to the reform, but also because local political pressure for expansion of formal care is likely to be stronger in areas where child care was severely rationed. Two of the variables indicating rurality also have a small positive relationship with the child care expansion (average distance to zone center and ear marks per capita). This might be due to the discreteness of child care expansion; Establishing a typical child care institution increases the child care coverage rate more in smaller than in larger municipalities. In Norway, there was a very slow process of urbanization until the mid 1980s (Berg, 2005), which implies that rurality status is likely to be more or less constant during the period we consider, and should, therefore, be picked up by the municipality-specific fixed effects.[19]

V Main results

Table IV shows our main results based on equation (1), both per child in the treatment area denoted by ITT (column 3), and per child care place denoted by TT (column 2). We focus on the estimated effects per child care place, since these reflect the size of the child care expansion. To address concerns for selection bias, we estimate equation (1) with and without the set of controls capturing important child and parental characteristics, as well as municipality-specific fixed effects; Estimates are quite similar across the different specifications and qualitatively the same. We use as our baseline specifications the estimations including controls and municipality-specific fixed effects. Nearly all these estimates are significant at the one percent level. The reform effects for the phase-in cohorts are relatively small and mostly insignificant.

Education. In light of the recent focus on dynamic complementarities in learning, a compelling question is how subsidized child care affects children's educational attainment. Starting with these estimations, panel A of Table IV shows immediately the profound consequences of

[19]We have also regressed the change in the municipality's child care coverage between 1976 and 1979 on the characteristics of the municipalities. Consistent with the descriptive statistics, there is little evidence of systematic relationships between the child care expansion and the characteristics of the municipalities. A notable exception is the ratio of child care to maternal employment rate prior to the reform. In addition, we have examined the pre-reform trends in time-varying municipality characteristics.

subsidized child care. Our estimated TT effect shows an additional .35 years of education per child care place, corresponding to an ITT effect of .06 years per child in the treatment area. This implies that by facilitating the supply of an additional 17,500 child care places, local governments were able to produce about 6,200 years of education.

A vast literature suggests that the return to education is non-linear, with relatively high returns to high school and college completion.[20] We estimate that subsidized child care decreases the probability of dropping out of high school by nearly 6 percentage points, while increasing the probability of attending college by almost 7 percentage points.

Earnings and welfare dependency. Two other important dimensions for evaluating the impact of public policy on long-run prospects of children are earnings and welfare dependency. In panel B of Table IV, we report the estimated effects of the child care expansion on the probability of being a low, average, high and top earner, as well as the probability of being welfare dependent. We find that the reform reduced the chances of having little or no earnings: Per child care place, the expansion in subsidized child care is estimated to decrease the probability of being a low earner by about 3.6 percentage points. In comparison, the probability of having at least average earnings increased by 5.1 percentage points.[21] Meanwhile, the effect on high and top earners go in the opposite direction, decreasing the probability by 3.4 and 2.2 percentage points respectively. It should be noted, however, that the latter results are not robust to two of the many specification tests presented below. With this caveat in mind, the counteracting effects on the top and the bottom of the earnings distribution suggest that child care has an equalizing effect. This conforms with previous studies suggesting systematic heterogeneity in the sign and magnitude of the effects of out-of-home child care: Children from disadvantaged families are expected to benefit the most from a subsidized child care system, whereas it could be less important or even detrimental for children from families with monetary and human capital to facilitate alternative arenas for child development of high quality (see Almond and Currie, 2002).

For welfare dependency, we find results mirroring those for low and average earnings.

[20] See for example Trostel (2005) for cross-country evidence on non-linearity in the return to education.

[21] We have also estimated the effect on the probability of having very low or no earnings (under one basic amount) to be a decrease of about 6 percentage points (significant at the one percent level).

Table IV: Main results

	(1) TT	(2) ITT	(3) SE(ITT)	(4) Mean	(5) Controls
A. Educational attainment					
Years of education	0.4129***	0.0737***	0.0174	12.66	No
	0.3523***	0.0629***	0.0155		Yes
Attended college	0.0868***	0.0155***	0.0034	0.3764	No
	0.0685***	0.0122***	0.0031		Yes
High school dropout	-0.0498***	-0.0089***	0.0029	0.2618	No
	-0.0584***	-0.0104***	0.0028		Yes
B. Earnings and welfare dependency					
Low earner	-0.0281**	-0.0050**	0.0025	0.1552	No
	-0.0359***	-0.0064***	0.0025		Yes
Average earner	0.0596***	0.0106***	0.0032	0.6931	No
	0.0514***	0.0092***	0.0031		Yes
High earner	-0.0219**	-0.0039**	0.0023	0.1628	No
	-0.0337***	-0.0060***	0.0022		Yes
Top earner	-0.0183***	-0.0033***	0.0011	0.0422	No
	-0.0220***	-0.0039***	0.0011		Yes
On welfare	-0.0496***	-0.0089***	0.0025	0.1632	No
	-0.0511***	-0.0091***	0.0025		Yes
C. Family formation					
Parent	-0.1029***	-0.0184***	0.0030	0.8083	No
	-0.0799***	-0.0143***	0.0029		Yes
Single, no child	0.0472***	0.0084***	0.0026	0.1398	No
	0.0347***	0.0062***	0.0025		Yes
Single, parent	-0.0036	-0.0007	0.0018	0.084	No
	-0.0025	-0.0004	0.0017		Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (one-tailed)

Notes: Estimations are based on OLS on equation (1), with and without municipal-specific fixed effects and the controls listed in Table III. The outcome variables are defined in Section IV. The sample consists of 499,026 children from birth cohorts 1967–1976. ITT/TT = .1785 (i.e. the increase in child care coverage following the reform in the treatment group relative to the comparison group). Mean refers to the pre-reform means in the treatment group. Standard errors are robust to within family clustering and heteroskedasticity.

Specifically, the expansion in child care caused a reduction in the probability of being on welfare by almost 5 percentage points, which is a very large effect when compared to the pre-reform mean of about 16 percent.

Family formation. Panel C of Table IV displays estimates of the effects of the child care expansion on family formation. We find that the children exposed to the reform are about 8 percentage points less likely to have a child, and almost 3.5 percentage points more likely to be single with no child. There is no effect on the probability of being a single parent. These findings align well with our results on education and earnings: When young adults undertake more education, child bearing and cohabitation are delayed (Heck et al., 1997; Buckles, 2008). Whether these effects persist is an open question, but delayed child birth is often associated with a decrease in the probability of a third or fourth child, since fecundity weakens when the female ages beyond 30 (Van Noord-Zaadstra et al., 1991). In end effect, the child care expansion could therefore turn out to lower the fertility rates and reduce the family size of the exposed children.

VI Specification checks

This section reports results from a battery of specification tests. When considering the effect of child care on children's educational attainment, the estimates are fairly similar across the different specifications and qualitatively the same. This also holds true for most of the other outcomes, including low earner, average earner, on welfare, parent, and single no child. In comparison, the results for high earner and top earner are sensitive to two of the ten specification tests, and should therefore be interpreted with more caution.

Time trend. Our DD approach identifies the child care effects from the assumption of a common time trend in the treatment and comparison group in the absence of the reform. A concern is that our positive effects may reflect differential time trends in the outcomes of interest between the treatment and comparison municipalities, rather than a true policy impact. When examining this graphically, we find that the pre-reform trends are quite similar for the treatment and comparison group, but that there is a striking change in their relative outcomes after the reform.[22] Consistent with this evidence, we find no effect of a placebo test, pretending that the child care reform took place in the pre-reform period.[23] Specifically, we add interaction terms between treatment status and cohort dummies for children born in 1968 and 1969 (with the 1967-cohort as the omitted category) to equation (1). The placebo test turns on the estimated treatment effects for cohorts born in 1968 and 1969, relative to 1967. Differential secular time trends in treatment and comparison municipalities, should cause these effects to be significantly different from zero. The results show that none of the treatment effects for cohorts born in 1968 and 1969, are significant even at the 10%-level.

Although it is reassuring to find that the trends are not systematically deviating in the pre-reform period, we may worry about breaks in the underlying trends coinciding with the reform. If we could find a variable that is strongly correlated with our outcomes of interest, but not affected by the child care reform, then we could tackle this by performing a placebo test within the reform period. Our second placebo test therefore exploits variation in adult height. Height should be a promising candidate for two reasons. First, a large number of twin and adoptive studies have shown that genetic factors are the overwhelming determinant of variation in height within developed countries. For example, Silventoinen et al. (2003) report heritability estimates around 0.9 for Norwegian males born between 1967 and 1978, implying that within this population about 90% of the variance in adult height can be accounted for by the variance of genes.

Second, it has long been recognized that taller adults have, on average, higher education and earnings. This also holds true in our sample where the correlation between height and our outcomes of interest are always highly significant: For example, a one standard deviation increase in height is associated with an increase in education by .29 years and college attendance by 5 percentage points, while lowering dropout rates by 3.6 percentage points.[24] Case and Paxson (2008) offer an explanation: On average, taller people take higher education and earn more because they are smarter. As early as age 3 – before child care has had a chance to play

[22] See Figures A3–A5 reported in the Appendix.

[23] See Table A2 reported in the Appendix.

[24] As shown in Table A5 in the Appendix, the correlations between height and our other outcomes are -0.0135, 0.0196, 0.0261 and 0.0091 for low, average, high and top earner, -0.0134 for welfare dependency, and finally 0.0137 for parent, -0.0038 for single parent, -0.0141 for single, no child.

a role – and throughout childhood, they find that taller children perform significantly better on cognitive tests. Moreover, they demonstrate that the correlation between height in childhood and adulthood is very high, so that tall children are much more likely to become tall adults.

A significant effect of the child care reform on children's adult height would raise concern that effects on other outcomes reflect omitted variables bias, like unobserved heterogeneity in innate ability, rather than true policy impacts. However, in line with the descriptive evidence for height in Table II, we find no effect of the child care reform, when estimating equation (1) with children's adult height as the dependent variable.[25] Since this test is performed only for boys, it should be noted that we in the subsample analysis find large positive effects of the child care reform for boys on, for instance, education.

To make sure that results are not driven by secular changes between urban and rural areas coinciding with the reform, we further drop the three big cities (Oslo, Bergen, and Trondheim) from our analysis. Column (3) in Table V reports estimation results excluding these cities, whereas Column (1) repeats our baseline results for comparison. The fact that our estimates vary little between the specifications increases our confidence in the empirical strategy.[26]

Following Duflo (2001), we allow for differential inter-cohort time trends across municipalities in the DD estimation. Specifically, we interact the cohort fixed effects with a large set of pre-reform municipality characteristics, such as male and female education and labor supply, primary school expenditures, and population density.[27] In doing so, we allow for different underlying trends in children's potential outcomes, depending on the pre-reform characteristics of the municipality. Column (8) in Table V reports estimates from this specification, which conform well to the results from the baseline specification. The estimated effects are higher for some outcomes, like the educational outcomes, and lower for others, such as welfare dependency. The exceptions are the results for high earner and top earner, where the estimates with interactions between cohort-fixed effects and municipality characteristics become imprecise and close to zero.

[25] Specifically, our main specification produces a TT estimate of -0.151 with a standard error of 0.352, less than 3% of a standard deviation, and pointing in the opposite direction if we are concerned about a positive bias in the results. This corresponds to an ITT effect smaller than -.027 per child in the treatment area.

[26] We have also dropped both the six and the ten largest cities from our analysis, yielding very similar results.

[27] The characteristics are listed in Table A1 in the Appendix, and discussed in Section IV.

Besley and Burgess (2004) show that allowing for differential time trends between areas in a DD regression may kill otherwise significant and large treatment effects. Column (5) in Table V reports estimation results where we have added municipality-specific time trends to our baseline specification. The idea is to use the pre-reform data to extrapolate the time trend of each municipality into the post-reform period. This allows treatment and comparison municipalities to follow different secular trends in a limited but potentially revealing way. As expected, estimates are less precise, as we now exploit deviations from preexisting municipal trends to pin down the child care effects. However, it's heartening to find that the results in general support the picture from our baseline specification; The estimates are higher for some outcomes, like years of education, and lower for others, such as high-school dropout. The exceptions are the results for high earner and top earner, where the estimates are substantially affected by the inclusion of municipality-specific time trends.

Clustering. To account for the fact that the variation we use to estimate the child care effects is at the municipality-period level, Column (2) in Table V reports results from our baseline specification, clustering the standard errors at the municipality–period level. This allows for shocks common to children who are born in the same period and live in the same municipality. We find that accounting for dependence within municipality–period groups does not increase our standard errors much, and the significance levels of the results are very similar.[28]

Selective migration. Though location decisions based on unobservable characteristics may affect our estimates, the direction of the bias is not obvious.[29] On the one hand, education-oriented or labor market attached parents may be more attracted to municipalities with high child care coverage rates. On the other hand, parents with children who need special attention or supervision may also be attracted to such municipalities. Though recent empirical work finds little support for Tiebout sorting across states or municipalities according to public good provision like school quality,[30] we take several steps to avoid that selective migration of families

[28]Bertrand et al. (2004) show that the standard errors in DD regressions may be misstated if there is serial correlation in the municipality-period shocks. We reduce this problem considerably by collapsing the time-series dimension into three periods: pre-reform, phase-in, and post-reform.

[29]Note that families living on the municipal borders could not take advantage of the child care expansion in neighboring municipalities without relocating, since eligibility was based on municipality of residency.

[30]See e.g. Rhode and Strumpf (2003) who find little support for Tiebout sorting across municipalities and counties using about 150 years of data.

into treatment and comparison municipalities confounds our results.

To address concern for in-migration, we excluded in our main analysis children from families that move between treatment and comparison municipalities during the expansion period. In addition, we control for relocation between municipalities within the treatment/comparison area; We have also performed all estimations excluding families that relocate, and the results are unchanged. However, one could argue that even the sample of stayers is selective, as out-migration could be endogenous to the child care expansion. To address this issue, we follow Hægeland et al. (2008) in using children's municipality of birth to determine whether they belong to treatment or comparison municipalities. Column (4) in Table V shows that the effects of the child care expansion is robust to using municipality of birth to determine treatment status. This finding conforms well with the results from Hægeland et al. (2008), which suggest that school quality matters little, if anything, for location decisions in Norway.

Alternative treatment definitions. In our baseline specification, we define the treatment and comparison areas by ordering municipalities according to the increase in child care coverage rate in the period 1976–1979, and then separating them at the median. Below, we make sure that our results are not artifacts of this choice of treatment definition.

In Column (6), we use the same expansion period, 1976–1979, but divide the sample at the 33rd and 67th percentiles of child care growth. Municipalities below the lower threshold are in the comparison group, while those above the upper threshold are in the treatment group. Children from municipalities in between the thresholds are excluded from the sample used for estimation. In Column (7), we define the treatment and comparison according to the median child care growth, but alter the expansion period to 1977–1979. To be consistent with this new definition of the expansion period, the 1970 cohort is now defined as a pre-reform instead of a phase-in cohort. Our findings show that the child care effects are similar across treatment definitions: The estimated effects are generally statistically significant at conventional levels and not significantly different from our baseline specification.[31]

To get round the issue of how to draw the line between treatment and comparison munic-

[31] We have also considered expansion periods 1976–1978, 1977–1980, and 1978–1980. For all expansion periods, we have used both thresholds: the median and 33rd vs. 67th percentile. The results confirm the picture presented above.

ipalities we estimate equation (2), where child outcome is regressed on child care coverage in each municipality, controlling for cohort and municipality-specific fixed effects as well as a set of controls. Column (9) reports estimates from this regression. In line with the results from the baseline specification, the findings suggest that child care had positive effects on educational attainment, labor market attachment, and welfare dependency. Although not significantly different, the point estimates for educational attainment are considerably lower with the specification that is linear in child care coverage, relative to our baseline specification. For instance, whereas the baseline specification estimates the reform effects per child care place to be .35 years of education, the linear specification suggests that creating another child care slot increases years of education of a child by .15. It should be noted, however, that relaxing the assumption of constant marginal effects by adding a quadratic term in child care coverage to equation (2), gives results for educational attainment that are very similar to our baseline specification. Specifically, our quadratic specification predicts that the rise in child care coverage from 1976 to 1979 of 17.85 percentage points led to increases per child care place of .39 for years of education and 6.5 percentage points for college attendance, whereas high-school drop-out rates reduce by 6 percentage points (all significant at a one percent level).

Family-specific fixed effects. In this final specification test, we take advantage of the fact that we can link all children to their parents through unique individual identifiers. This allows us to identify siblings, and add family-specific fixed effects controlling for unobserved time-invariant family characteristics. The reform effects are then identified only from comparisons of the outcomes of siblings from the pre-reform and the post-reform cohorts, who have the same family background but experience different exposure to child care. A notable feature is that these children have a sibling that is at least three years apart. This implies that children born late among the post-reform cohorts, when the child care coverage rate was highest, will be undersampled.[32] Consequently, to compare the family-fixed effect results with our main results would be inappropriate. Instead, we restrict our sample to children from families with siblings from at least two of the three groups of cohorts; pre-reform, phase-in, and postreform.[33] Next,

[32]Notice that a child born in 1976, say, must have a sibling at least 7 years his senior (born in 1969, and therefore in the pre-reform cohorts) to be included in this sample.

[33]To gain efficiency, we include the phase-in cohorts, but they will not contribute to identifying the child care effects.

Table V: Robustness

	(1) **Baseline**	(2) **Cluster**	(3) **No cities**	(4) **Selective migration**	(5) **Mun. trend**	(6) **1976–79 33rd/67th**	(7) **1977–79 50th/50th**	(8) **Flexible trends**	(9) **Linear in child care**
Years of education	0.3523***	0.3523***	0.3738***	0.3378***	0.4207*	0.2988***	0.5577***	0.4151***	0.1521***
	(0.0871)	(0.0985)	(0.0953)	(0.0894)	(0.2582)	(0.0755)	(0.1326)	(0.0908)	(0.0593)
Attended college	0.0685***	0.0685***	0.0685***	0.0563***	0.1000**	0.0482***	0.1061***	0.0780***	0.0203**
	(0.0172)	(0.0180)	(0.0190)	(0.0177)	(0.0514)	(0.0151)	(0.0263)	(0.0181)	(0.0120)
High school dropout	-0.0584***	-0.0584***	-0.0694***	-0.0551***	-0.0282	-0.0544***	-0.0902***	-0.0742***	-0.0342***
	(0.0155)	(0.0183)	(0.0172)	(0.0159)	(0.0456)	(0.0135)	(0.0236)	(0.0163)	(0.0104)
Low earner	-0.0359***	-0.0359***	-0.0317**	-0.0200*	-0.0327	-0.0202**	-0.0338*	-0.0259**	-0.0226***
	(0.0139)	(0.0147)	(0.0152)	(0.0143)	(0.0415)	(0.0120)	(0.0212)	(0.0145)	(0.0096)
Average earner	0.0514***	0.0514***	0.0572***	0.0302**	0.061	0.0427***	0.0510**	0.0457***	0.0384***
	(0.0171)	(0.0213)	(0.0189)	(0.0175)	(0.0511)	(0.0150)	(0.0260)	(0.0180)	(0.0119)
High earner	-0.0337***	-0.0337***	-0.0223**	-0.0403***	0.0006	-0.0199**	-0.0536***	-0.0022	-0.0328***
	(0.0124)	(0.0125)	(0.0136)	(0.0127)	(0.0369)	(0.0108)	(0.0189)	(0.0128)	(0.0083)
Top earner	-0.0220***	-0.0220***	-0.0117**	-0.0209***	0.0342**	-0.0125**	-0.0317***	-0.0018	-0.0177***
	(0.0064)	(0.0083)	(0.0069)	(0.0066)	(0.0187)	(0.0056)	(0.0097)	(0.0064)	(0.0041)
On welfare	-0.0511***	-0.0511***	-0.0390***	-0.0231*	-0.0697**	-0.0300***	-0.0581***	-0.0329**	-0.0369***
	(0.0137)	(0.0162)	(0.0151)	(0.0141)	(0.0410)	(0.0118)	(0.0210)	(0.0144)	(0.0092)
Parent	-0.0799***	-0.0799***	-0.0595***	-0.0537***	-0.0456	-0.0354***	-0.0938***	-0.0231*	-0.0605***
	(0.0165)	(0.0240)	(0.0182)	(0.0169)	(0.0501)	(0.0146)	(0.0251)	(0.0172)	(0.0119)
Single, no child	0.0347***	0.0347***	0.0359**	0.0252**	0.0552*	0.0223**	0.0359**	0.0161	0.0227**
	(0.0142)	(0.0131)	(0.0156)	(0.0146)	(0.0429)	(0.0124)	(0.0216)	(0.0148)	(0.0102)
Single, parent	-0.0025	-0.0025	0.0028	-0.0086	-0.0353	0.0135*	0.0024	-0.0012	0.0011
	(0.0097)	(0.0085)	(0.0107)	(0.0100)	(0.0283)	(0.0084)	(0.0148)	(0.0102)	(0.0064)
ITT/TT	0.1785	0.1785	0.1785	0.1785	0.1785	0.2946	0.1171	0.1785	–
No. of children	499,026	499,026	420,054	483,394	499,026	259,685	499,026	496,256	499,026

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (one-tailed)

Notes: Each entry reports the treatment effect per child care place (TT parameter); ITT/TT is defined by the increase in child care coverage following the reform in the treatment group relative to the comparison group in the estimation sample. Outcomes are defined in Section IV. In column 1-8, estimations are based on OLS on equation (1), including all controls from Table III and municipal-specific fixed effects. Column 1 repeats our baseline estimates. In column 2, standard errors are clustered to account for serial dependence of the errors within municipality–period groups. In column 3, the three largest cities are dropped. In column 4, treatment is defined by the child's birth municipality (children with missing values dropped), while column 5 includes a linear municipality-specific trend. In column 6, treatment municipalities are above the 67th percentile in child care coverage growth, while comparison municipalities are below the 33rd. In column 7, the expansion period is 1977–1979. In column 8, the cohort fixed effects are interacted with pre-reform municipality characteristics listed in Table A1. In column 9, estimates are based on equation 2. Standard errors in parentheses are robust to within family clustering and heteroskedasticity.

we estimate equation (1) on this subsample, with and without family-specific fixed effects. We find that the reform effects are, as expected, generally smaller and less precisely estimated in this subsample.[34] More importantly, comparing the results with and without family-specific fixed effects increases our confidence in the empirical strategy, since the point estimates are quite similar, and never significantly different.

VII Heterogeneous effects of child care

Our main results show strong positive effects of child care on children's long-run outcomes, raising educational attainment, strengthening labor market attachment, and lifting people out of welfare dependency. However, estimating the average effect for the treatment group as a whole may conceal important differences in the consequences of the reform across subgroups. For instance, in their review of child care and child development Almond and Currie (2010) suggest that girls and children with low educated parents benefit most from child care attendance. To address this question, we estimate our model for different subsamples.

In Table VI, we report results from subsamples divided by child sex (columns 2–3) and by maternal education (columns 4–5). Column 1 repeats our baseline estimates for ease of comparison. Considering first the estimated effects by child sex, we find that most, if not all, of the reduction in the probability of being low and average earners relates to girls. This indicates that child care may contribute to closing the gender wage gap. Interestingly, results also reveal that it is mostly girls who delay child bearing and family formation as adults when exposed to child care. When it comes to mother's education, we find that most of the benefits associated with subsidized child care relate to children of low educated mothers. The child care reform could therefore be expected to increase intergenerational mobility.[35]

As in Baker et al. (2008), interpreting the differences in our estimates across subpopulations can be difficult because there could both be differences in child care take-up and heterogenous impacts of uptake. Unfortunately, we do not have data on child care use by child and parental

[34] Results are reported in Table A3 in the Appendix.

[35] We have also estimated the model separately by number of siblings, parents' age, mother's labor market attachment, and father's education. We generally find small differences across the subsamples, although the estimates are often more imprecise.

Table VI: Subsample results by gender and mother's education

	(1)	(2)	(3)	(4)	(5)
		– Child sex –		**– Mother's education –**	
	Full sample	Boys	Girls	High school	Not high school
Years of education	0.3523***	0.3801***	0.3208***	0.1161	0.4188***
	(0.0871)	(0.1204)	(0.1251)	(0.2102)	(0.0960)
Attended college	0.0685***	0.0690***	0.0676***	0.0184	0.0779***
	(0.0172)	(0.0235)	(0.0251)	(0.0392)	(0.0194)
High school dropout	-0.0584***	-0.0696***	-0.0452**	-0.0118	-0.0712***
	(0.0155)	(0.0219)	(0.0218)	(0.0251)	(0.0185)
Low earner	-0.0359***	-0.0019	-0.0696***	-0.0479**	-0.0386***
	(0.0139)	(0.0170)	(0.0222)	(0.0291)	(0.0159)
Average earner	0.0514***	0.0175	0.0844***	0.0314	0.0648***
	(0.0171)	(0.0210)	(0.0269)	(0.0370)	(0.0194)
High earner	-0.0337***	-0.0256	-0.0441***	-0.0451*	-0.0153
	(0.0124)	(0.0216)	(0.0113)	(0.0336)	(0.0132)
Top earner	-0.0220***	-0.0227**	-0.0222***	-0.0320*	-0.0108**
	(0.0064)	(0.0116)	(0.0049)	(0.0198)	(0.0064)
On welfare	-0.0511***	-0.0352**	-0.0657***	-0.0694***	-0.0468***
	(0.0137)	(0.0156)	(0.0228)	(0.0274)	(0.0159)
Parent	-0.0799***	-0.0487**	-0.1141***	-0.0702**	-0.0618***
	(0.0165)	(0.0248)	(0.0216)	(0.0380)	(0.0185)
Single, no child	0.0347***	0.0171	0.0532***	0.0129	0.0315**
	(0.0142)	(0.0225)	(0.0172)	(0.0332)	(0.0158)
Single, parent	-0.0025	0.012	-0.0175	-0.0344**	0.0065
	(0.0097)	(0.0099)	(0.0168)	(0.0203)	(0.0112)
No. of children	499,026	253,752	245,274	101,879	397,147

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (one-tailed)
Estimations are based on OLS on equation (1) separately for each subsample, with controls listed in Table III and municipal-specific fixed effects. Each entry reports the treatment effect per child care place (TT parameter) for the subsample assuming equal take-up of the child care places created by the reform; ITT/TT = .1785 (i.e. the increase in child care coverage following the reform in the treatment group relative to the comparison group). Outcomes are defined in Section IV. Standard errors in parentheses are robust to within family clustering and heteroskedasticity.

characteristics. The results in this section therefore assume the same take-up rate across sub-populations. This is admittedly a strong assumption, which we should have in mind when interpreting the results. However, for child's sex the assumption is less controversial as child care institutions sought to balance the composition of children. When considering mother's education, it is perhaps most likely that we underestimate the reform effects on children with low educated mothers, since high educated married mothers are more attached to the labor market and should therefore be more likely to use child care.

VIII Mechanisms

Parental or informal care. When interpreting our estimated effects of subsidized child care, a crucial point is the counterfactual mode of care, i.e. the type of care the children would be exposed to absent the reform. There are two distinct counterfactual modes of care to formal child care. The first is parental care, while the second is informal care, including relatives, unlicensed care givers, and other irregular care givers such as friends and neighbors. A shift from parental care to formal child care could affect children differently than a shift from informal care, which is likely to be of inferior quality (see e.g. Datta Gupta and Simonsen, 2007).

If we knew the effect of the child care expansion on maternal employment, we could hone in on the counterfactual mode of care. Following Blau and Currie (2006), consider the following three combination of mother's work and child care decision: not working and maternal care, working and informal care, and working and formal care. If the new subsidized formal child care led to a shift from parental to formal care, we would expect it to affect maternal employment rates also.[36]

Havnes and Mogstad (2009) estimate the effect of the child care reform on full-time and part-time work of married mothers. To this end, they use a DD approach, comparing the growth rate in employment of mothers with the youngest child aged 3 to 6 years living in municipalities where child care coverage expanded a lot (i.e. the treatment group), with the growth rate for mothers with the youngest child of the same age who live in municipalities with little or no

[36] It is possible that non-working mothers were taking up some of the new care child care slots. However, survey results reported in Leira (1992) suggests that the number of non-working mothers using formal child care did not increase much over the period 1973–1985

increase in child care (i.e. the comparison group). The analysis provides robust evidence that the new subsidized child care crowds out informal care arrangements, with almost no net increase in total care use or maternal labor supply.[37]

As our sample differs somewhat from theirs, we re-examine the effect on maternal employment.[38] The precise DD results in Table VII conform to Havnes and Mogstad (2009). The .1785 increase in child care coverage is estimated to have caused less than 1 percentage point increase in maternal employment. This implies a .04 percentage point increase in maternal employment per percentage point increase in the child care coverage rate, which in turn suggests that the new child care slots were associated with a 96% crowding out of informal care. We have also performed the DD regressions separately by education and age, which barely moves the estimates. Consequently, our positive results of child care on children's outcomes should be interpreted as reflecting a shift mostly from informal rather than parental care.

The shift from informal to formal care seems relevant for debates about subsidized child care also in other countries. Cascio (2009b) and Lundin, Mørk and Øckert (2008) find no effect on maternal labor supply for married mothers in the US and Sweden, respectively, from increased access to subsidized child care. Also, while Gelbach (2002), Berlinski and Galiani (2007) and Baker et al. (2008) find positive effects on maternal labor supply, they all find considerable crowding out of informal care arrangements.

Other possible mechanisms. A concern for the interpretation of the estimated reform effect is that the child care expansion could be associated with changes in the quality of formal care. If the quality of child care institutions improved in the treatment municipalities relative to the comparison municipalities, we could potentially overstate the impact of the new child care

[37] A battery of specification checks support their results, including a placebo reform, the inclusion and exclusion of a rich set of controls, a triple-difference approach using mothers of 7–10 year olds as a second comparison group, and the inclusion of individual-specific and municipality-specific fixed effects.

[38] We estimate the following regression model by OLS, with controls listed in Table III and municipality-specific fixed effects:

$$Y_{jt} = \gamma_0 + \gamma_1 Treat_j + \gamma_2(Treat_j \times Phasein_t) + \theta(Treat_j \times Post_t) + \psi_1 Phasein_t + \psi_2 Post_t + X'_{jt}\beta + \epsilon_{jt},$$

where Y_{jt} is equal to 1 if mother j works when her child is between 3 and 6 years old (and 0 otherwise). The dummy variable $Treat_j$ is equal to 1 if mother j lives in a treatment municipality, whereas $Phasein_t$ and $Post_t$ are dummy variables equal to 1 when the observation is from phase-in cohorts and post-reform cohorts, respectively. If a mother has more than one child from either the pre-reform, the phase-in or the post-reform cohorts, we consider the youngest child only.

Table VII: Mechanisms: Family size, mother's education and maternal employment

	(1) TT	(2) ITT	(3) SE(ITT)	(4) Mean
Family size	0.1003***	0.0179***	0.0060	2.995
Mother's education	-0.0051	-0.0009	0.0061	10.15
Maternal employment				
– Low earner	-0.0431***	-0.0077***	0.0025	0.1190
– Average earner	0.0443***	0.0079***	0.0015	0.0373

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (one-tailed)

Notes: The sample consists of 318,367 mothers of the 499,026 children from cohorts born in 1967–1976. ITT/TT = .1785 (i.e. the increase in child care coverage following the reform in the treatment group relative to the comparison group). Standard errors are clustered on the mother. *Maternal employment:* Maternal employment status is determined based on average earnings over the years the child is between 3 and 6 years old. Estimations are based on OLS on the equation in footnote 38. *Family size and mother's education:* Estimations are based on OLS on equation (1), with controls listed in Table III and municipal-specific fixed effects. Mother's education is measured when the child is 16 years old. Family size is measured in 2006. Standard errors are robust to within family clustering and heteroskedasticity.

Table VIII: Descriptive statistics: Quality of child care

	1975	**1977**	**1979**	**1981**
Treatment				
Children/teacher	16.67	18.78	19.18	18.12
Children/employee	6.80	8.48	9.43	8.99
No. of institutions	520	988	1,582	1,794
Comparison				
Children/teacher	18.82	17.40	17.69	17.52
Children/employee	7.49	7.49	8.13	8.32
No. of institutions	360	481	712	960

places. However, there is no evidence of such quality changes over this period. Considering Table VIII, it seems that, if anything, the quality of formal child care may have deteriorated somewhat in the treatment group compared to the comparison group: From 1975 to 1979, the number of children per teacher increased somewhat in the treatment group from just under 17 to about 19 children, while the opposite was true in the comparison group, with a decrease from about 19 to just over 17 children per teacher. The same holds true for the number of children per employee, where the difference between the groups shifted about two points in favor of the comparison group from 1975 to 1979. Unfortunately, we do not have data on other aspects of child care quality. In any case, Blau (1999b) shows that child care characteristics, such as group size, staff-child ratio, and employee training, have little association with child development.

The fact that the reform had little impact on maternal employment also means that it is unlikely that increased family income is the driving factor behind the positive effects. The child care subsidies could be interpreted as a modest increase in family income, depending on the costs associated with alternative modes of care. However, Gulbrandsen et al. (1982) report small differences in the price of formal child care and unlicensed care givers in Norway. In any case, Løken (2010) finds little, if any, causal effect of family income on children's outcomes in Norway in the 1970s, mirroring evidence from the US (Blau, 1999a).

Another possible mechanism behind our results is that the access to child care made it easier for mothers to undertake education, which may spill over to child outcomes. We examine this mechanism by estimating the baseline specification of equation (1), with mother's years of education when the child is 16 years old as the dependent variable. As shown in Table VII, the reform had no impact on mother's education. In any case, Black et al. (2005b) find little evidence of a causal relationship between parents' education and children's education in Norway. Consequently, we can rule out intergenerational transmission of education as an important mechanism behind the positive effects of the child care reform.

Finally, we consider the impact of the child care expansion on family size. The well-known quantity-quality model of fertility introduced by Becker and Lewis (1973) suggests that greater family size negatively affects parents' investments in child development through resource dilution. If increased access to child care promotes larger families, then the reform may reduce children's human capital, offsetting the positive effects of actual child care attendance. To investigate this, we estimate the baseline specification of equation (1), replacing child outcome with completed family size (measured in 2006) as the dependent variable. Table VII shows that the child care expansion caused a modest .1 increase in family size per child care place. Moreover, Black et al. (2005a), using data from Norway, suggest no causal effect of family size on children's adult outcomes. This implies that changes in family size are of little concern for the interpretation of our results.

IX Conclusion

There is a heated debate in many developed countries about a move towards subsidized, widely accessible child care or pre-school. This controversy has been fueled by a number of studies showing that early educational programs can generate learning gains in the short-run and, in many cases, improve the long-run prospects of children from poor families (see e.g. Karoly et al., 2005). While the results from these studies are encouraging, the programs evaluated were unusually intensive and involve small numbers of particularly disadvantaged children from a few cities in the US. A major concern is therefore that this evidence may tell us little about the effects of child care arrangements offered to the entire population (Baker et al., 2008).

This paper has examined the effects on children's long-run outcomes of a reform from late 1975 in Norway, which led to a large-scale expansion of subsidized child care. Our precise and robust difference-in-difference estimates show that child care exposure improves long-run prospects of children considerably, both educational attainment, labor market attachment, and welfare dependency. In aggregate terms, the additional 17,500 child care places produced 6,200 years of education. The child care expansion also raised the chances of completing high school and attending college, in orders of magnitude similar to the black–white race gaps in the US. Consistent with the evidence of higher education and stronger labor market attachment, we also find that children exposed to child care delayed child bearing and family formation as adults. Our subsample analysis indicates that most the effect on education stems from children with low educated mothers, whereas most of the effect on earnings relates to girls. This suggests that good access to subsidized child care levels the playing field by increasing intergenerational mobility and closing the gender wage gap.

Whether these positive findings for Norway would extend to other countries is an open question. For example, the majority of child care interventions in the US are employment based, requiring explicitly that parents must be employed to be eligible to child care. However, the Scandinavian countries were the first to introduce subsidized child care on a large scale, and their experience is currently a unique source of information about its long-run consequences. At the very least, our study serves as an example of a large-scale early intervention that improved the trajectories of children. Nevertheless, in interpreting our study, it is important to keep in mind that our findings are likely to reflect the effects of moving children from informal care, rather than parental care, into formal care of relatively high quality. In comparison, other studies, such as Baker et al. (2008), might be more relevant when discussing the consequences of moving children from parental care to formal child care. And further, Cascio (2009a) may be informative of the impacts of low-quality child care that crowds out participation in more intensive programs and is funded by cut-backs on school expenditures.

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Appendix to Havnes and Mogstad (2010): "No child left behind: Subsidized child care and children's long-run outcomes"

Table A1: Descriptive statistics for treatment and comparison municipalities in 1976

	Treatment		**Comparison**	
Child care/maternal employment rate	0.2471	[0.4596]	0.3542	[0.6213]
Child care coverage rate	0.0534	[0.0899]	0.0695	[0.0968]
Years of education, males	9.2256	[0.5514]	9.2174	[0.4699]
–, females	8.8198	[0.3820]	8.7672	[0.3313]
Earnings, males	0.3917	[0.0762]	0.4081	[0.0734]
–, females	0.1080	[0.0349]	0.1121	[0.0321]
Labor force part., males	0.8324	[0.0591]	0.8367	[0.0665]
–, females	0.2919	[0.0844]	0.2997	[0.0813]
–, mothers of 3–6 year olds	0.1903	[0.0753]	0.1953	[0.0710]
Expenditure (2006-USD/capita)				
Total	959.65	[291.05]	909.72	[229.55]
Primary school	241.78	[107.40]	223.40	[90.83]
Revenue (2006-USD/capita)				
Ear marks, total	569.54	[217.95]	546.49	[174.11]
–, primary school	87.78	[33.31]	87.45	[36.12]
Fees, total	124.65	[90.03]	105.02	[63.80]
–, primary school	0.86	[1.38]	0.97	[1.80]
Taxes	379.00	[105.00]	379.09	[114.75]
Geography				
In densely populated areas	0.4049	[0.2915]	0.4827	[0.2979]
Ave. distance to zone center	0.8876	[0.7789]	0.7732	[0.6788]
– to neighboring center	3.7768	[2.6130]	3.4297	[2.8039]
Population				
Total	9846	[36400]	9476	[13267]
Married	0.4664	[0.0277]	0.4618	[0.0346]
Divorced	0.0144	[0.0081]	0.0153	[0.0080]
Immigrant	0.0098	[0.0096]	0.0095	[0.0086]
0 to 6 years old	0.1077	[0.0177]	0.1141	[0.0177]
7 to 10 years old	0.0673	[0.0099]	0.0708	[0.0097]
11 to 18 years old	0.1293	[0.0127]	0.1314	[0.0133]
Females: 19 to 35 years old	0.1021	[0.0187]	0.1082	[0.0170]
–: 36 to 55 years old	0.1027	[0.0101]	0.1019	[0.0095]
Males: 19 to 35 years old	0.1175	[0.0152]	0.1227	[0.0137]
–: 36 to 55 years old	0.1096	[0.0091]	0.1077	[0.0092]
Politics				
Registered voters	6480	[26654]	5863	[8488]
–, female	0.4896	[0.0169]	0.4928	[0.0167]
Election participation	0.7243	[0.0587]	0.7093	[0.0563]
–, females	0.7094	[0.0666]	0.6962	[0.0632]
Female elected representatives	0.1521	[0.0807]	0.1394	[0.0635]
Socialist vote share	0.3864	[0.1654]	0.4031	[0.1651]
Socialist mayor	0.3140	[0.4652]	0.3671	[0.4832]
Female mayor	0.0097	[0.0981]	0.0145	[0.1198]

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (one-tailed)

Notes: Columns 1–4 report means and standard deviations across treatment and comparison municipalities, not weighted by population size. Earnings denotes pensionable income in NOK 100,000. Socialist parties are defined as RV, SV and DNA. Densely populated areas are contiguous zones of at least 200 people where the distance between houses is generally less than 50 meters (400 meters if separated by e.g. parks, rivers or industrial zones). Average distance to zone center is the mean predicted travel distance in km from a citizen's home to the most populous census area in a contiguous zone of more than 2,000 people within the municipality. Distance to neighboring center is the mean travel distance from the center of a census area to the closest center in another census area within the same economic zone. Standard deviations are in brackets.

Table A2: Robustness: Placebo reform.

	(1) TT 1968	(2) TT 1969	(3) TT *Phase-in*	(4) TT *Post*	(5) Mean
A. Educational attainment					
Years of education	-0.0009	-0.0506	0.0913	0.3349***	12.66
	(0.1520)	(0.1512)	(0.1243)	(0.1238)	
Attended college	0.0057	-0.0261	0.0268	0.0616***	0.3764
	(0.0295)	(0.0294)	(0.0243)	(0.0243)	
High school dropout	-0.0167	-0.0031	-0.0236	-0.0650***	0.2618
	(0.0284)	(0.0281)	(0.0231)	(0.0228)	
B. Earnings and welfare dependency					
Low earner	-0.0013	-0.0155	-0.0148	-0.0416**	0.1552
	(0.0241)	(0.0240)	(0.0198)	(0.0198)	
Average earner	-0.0087	0.025	0.0041	0.0570***	0.6931
	(0.0295)	(0.0294)	(0.0243)	(0.0243)	
High earner	0.0156	0.0274	0.0097	-0.0192	0.1628
	(0.0233)	(0.0231)	(0.0190)	(0.0185)	
Top earner	-0.0133	-0.0058	-0.0177**	-0.0284***	0.0422
	(0.0130)	(0.0127)	(0.0104)	(0.0099)	
On welfare	0.0264	0.0258	0.0022	-0.0336**	0.1632
	(0.0246)	(0.0244)	(0.0202)	(0.0199)	
C. Family formation					
Parent	0.0161	-0.0093	-0.0421**	-0.0777***	0.8083
	(0.0256)	(0.0259)	(0.0216)	(0.0221)	
Single, no child	-0.0188	-0.0229	0.0012	0.0207	0.1398
	(0.0228)	(0.0229)	(0.0191)	(0.0194)	
Single, parent	0.0167	0.0138	0.0235*	0.0078	0.0840
	(0.0189)	(0.0186)	(0.0153)	(0.0149)	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (one-tailed)

Notes: Estimations are based on OLS on equation (1), including all controls from Table III and municipal-specific fixed effects. The 1967-cohort is the omitted reference category, while children born in 1968 and 1969 are placebo cohorts. The outcome variables are defined in Section IV. Reported treatment effects are per child care place (TT parameter); ITT/TT = .1785 (i.e. the increase in child care coverage following the reform in the treatment group relative to the comparison group). The sample consists of 499,026 children from the birth cohorts 1967–1976. Phase-in-cohorts are born 1970–1972, and post-reform cohorts are born 1973–1976. Standard errors in parentheses are robust to within family clustering and heteroskedasticity.

Table A3: Family-specific fixed effects: Education and earnings

	(1) TT	(2) ITT	(3) SE(ITT)	(4) Controls	(5) Fam. FE
Years of education	0.1177	0.021	0.0233	No	No
	0.1505	0.0269	0.0212	Yes	No
	0.1523	0.0272	0.0321	Yes	Yes
Attended college	0.0326	0.0058	0.0046	No	No
	0.0375*	0.0067*	0.0042	Yes	No
	0.0506*	0.0090*	0.0065	Yes	Yes
High school dropout	-0.0098	-0.0018	0.004	No	No
	-0.0173	-0.0031	0.0039	Yes	No
	-0.0122	-0.0022	0.0062	Yes	Yes
Low earner	-0.0136	-0.0024	0.0035	No	No
	-0.0128	-0.0023	0.0035	Yes	No
	-0.0103	-0.0018	0.0057	Yes	Yes
Average earner	0.0512**	0.0091**	0.0045	No	No
	0.0399**	0.0071**	0.0043	Yes	No
	0.0348	0.0062	0.007	Yes	Yes
High earner	-0.0058	-0.001	0.0032	No	No
	-0.0118	-0.0021	0.0031	Yes	No
	-0.0117	-0.0021	0.005	Yes	Yes
Top earner	-0.0268***	-0.0048***	0.0016	No	No
	-0.0279***	-0.0050***	0.0016	Yes	No
	-0.0241*	-0.0043*	0.0026	Yes	Yes
On welfare	-0.0476***	-0.0085***	0.0036	No	No
	-0.0461***	-0.0082***	0.0035	Yes	No
	-0.0186	-0.0033	0.0057	Yes	Yes
Parent	-0.0849***	-0.0152***	0.0042	No	No
	-0.0746***	-0.0133***	0.0041	Yes	No
	-0.0657**	-0.0117**	0.0067	Yes	Yes
Single, no child	0.0335**	0.0060**	0.0036	No	No
	0.0267*	0.0048*	0.0035	Yes	No
	0.0450*	0.0080*	0.0058	Yes	Yes
Single, parent	0.0013	0.0002	0.0025	No	No
	0.0037	0.0007	0.0025	Yes	No
	0.0017	0.0003	0.0041	Yes	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (one-tailed)

Notes: Estimations are based on OLS on equation (1), with and without controls and family-specific fixed effects. The controls are listed in Table III. The outcomes variable are defined in Section IV. The sample consists of 286,835 children from birth cohorts 1967–1976, who belong to families with siblings from at least two of the three groups of cohorts: pre-reform, phase-in group, and post-reform. Mean refers to the pre-reform means in the treatment group. ITT/TT = .1785 (i.e. the increase in child care coverage following the reform in the treatment group relative to the comparison group). Standard errors are robust to within family clustering and heteroskedasticity.

Table A4: Control variables.

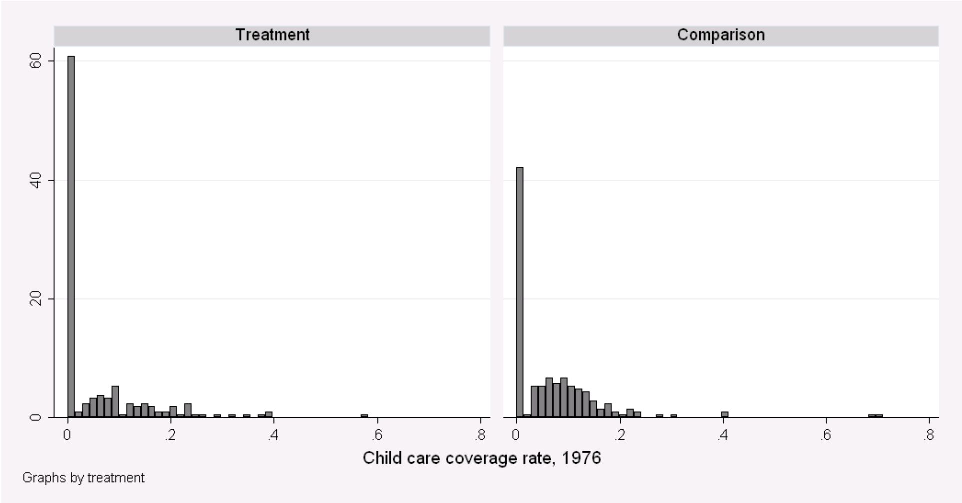
	(1) TT	(2) ITT	(3) SE(ITT)	(4) Mean
Male	0.0187	0.0033	0.0034	0.507
No. of older siblings	-0.0052	-0.0009	0.0052	2.133
Mother's age at first birth	0.0455	0.0081	0.0128	23.33
Father's age at first birth	0.011	0.0020	0.0149	26.56
Mother's edu. when child 2 y.o.	-0.1047*	-0.0187*	0.0118	9.662
Father's edu. when child 2 y.o.	-0.1624*	-0.0290*	0.0159	10.37
Immigrant	0.0106	0.0019	0.0015	0.0566
Relocated	0.0337***	0.0060***	0.0017	0.0358

Estimations are based on OLS on equation (1), with controls listed in Table III and municipal-specific fixed effects, excluding the dependent variable from the set of controls. ITT/TT = .1785 (i.e. the increase in child care coverage following the reform in the treatment group relative to the comparison group). Mean refers to the pre-reform means in the treatment group. Standard errors are robust to within family clustering and heteroskedasticity.

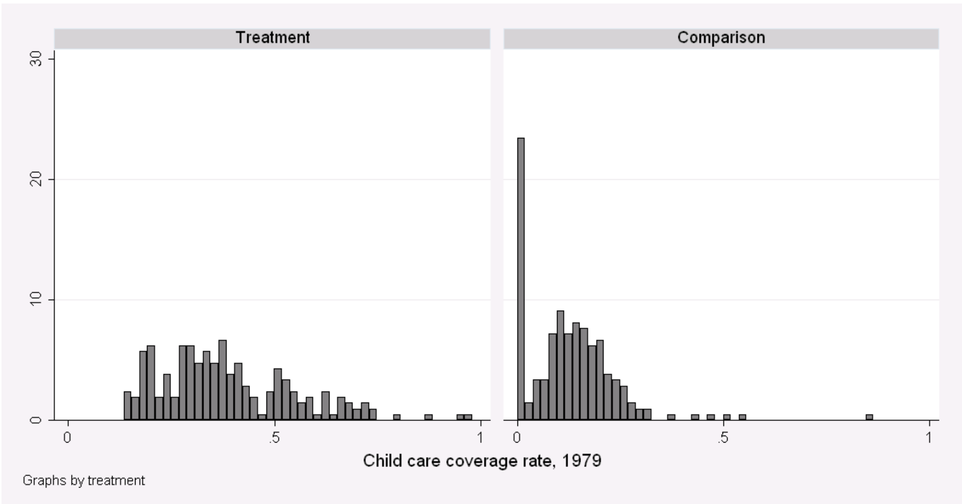
Table A5: Correlation between child long-run outcomes and height (standardized coefficients).

Years of education	0.2923	(59.78)
High school dropout	-0.0364	(-43.61)
College education	0.0495	(52.28)
Low earner	-0.0135	(-22.09)
Average earner	0.0196	(25.67)
High earner	0.0261	(31.74)
Top earner	0.0091	(20.96)
On welfare	-0.0134	(-23.81)
Parent	0.0137	(14.45)
Single, no child	-0.0141	(-17.02)
Single parent	-0.0038	(-10.67)

Standardized coefficients from separate bivariate regressions of each outcome on height on the full sample for which height is available, totalling 246,516 boys. t-values are in parenthesis.



(a) 1976



(b) 1979

Figure A1: Distribution of treatment and comparison municipalities by child care coverage rate for 3 to 6 year olds in 1976 (top panel) and 1979 (bottom panel).

Notes: Pre-reform cohorts are born 1967–1969, phase-in-cohorts are born 1970–1972, and post-reform cohorts are born 1973–1976. Treatment (comparison) municipalities are above (below) the median in child care coverage growth from 1976 to 1979.

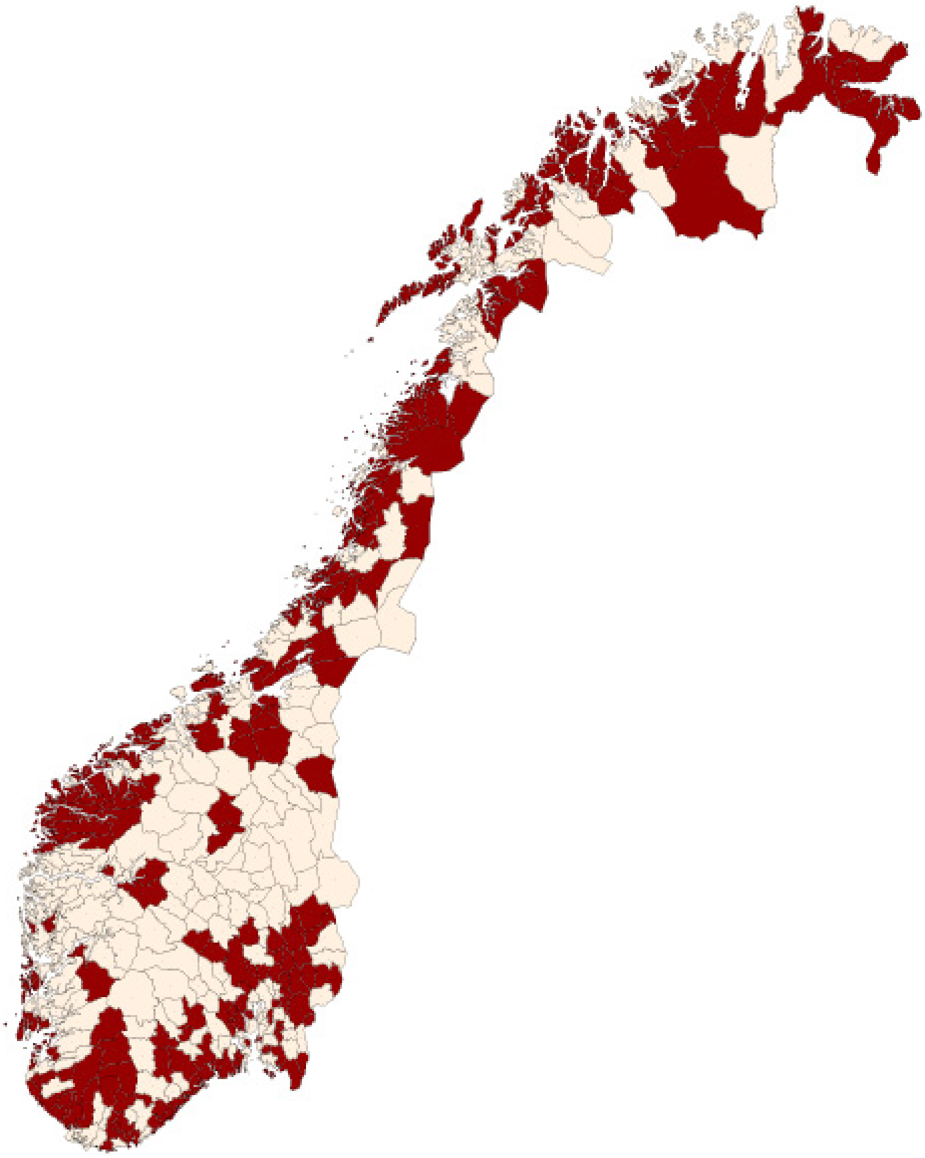
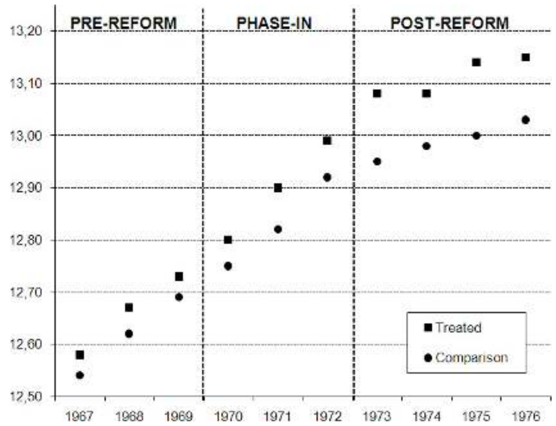
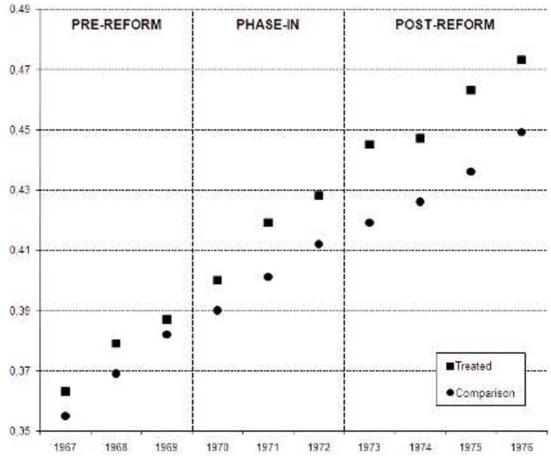


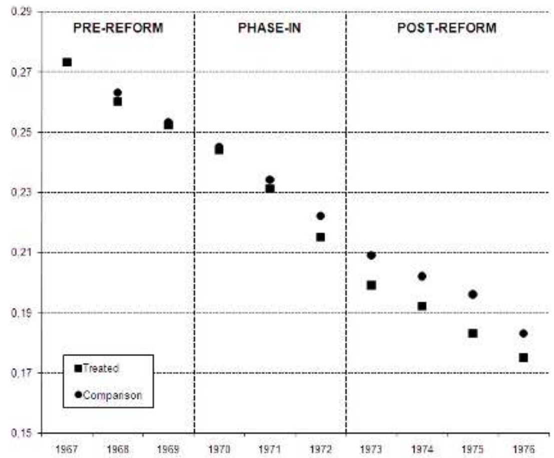
Figure A2: Geographic location of treatment (white) and comparison (dark) municipalities



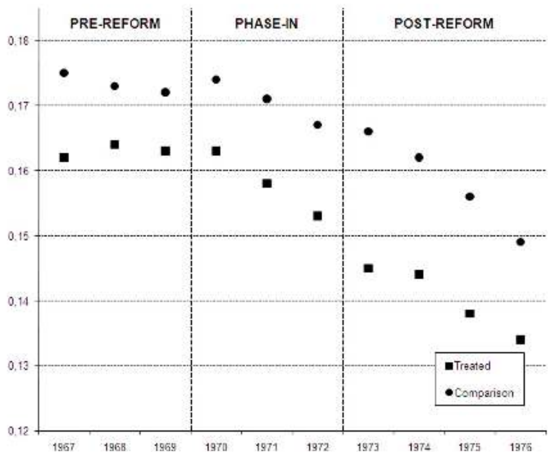
(a) Years of education



(b) Attended college

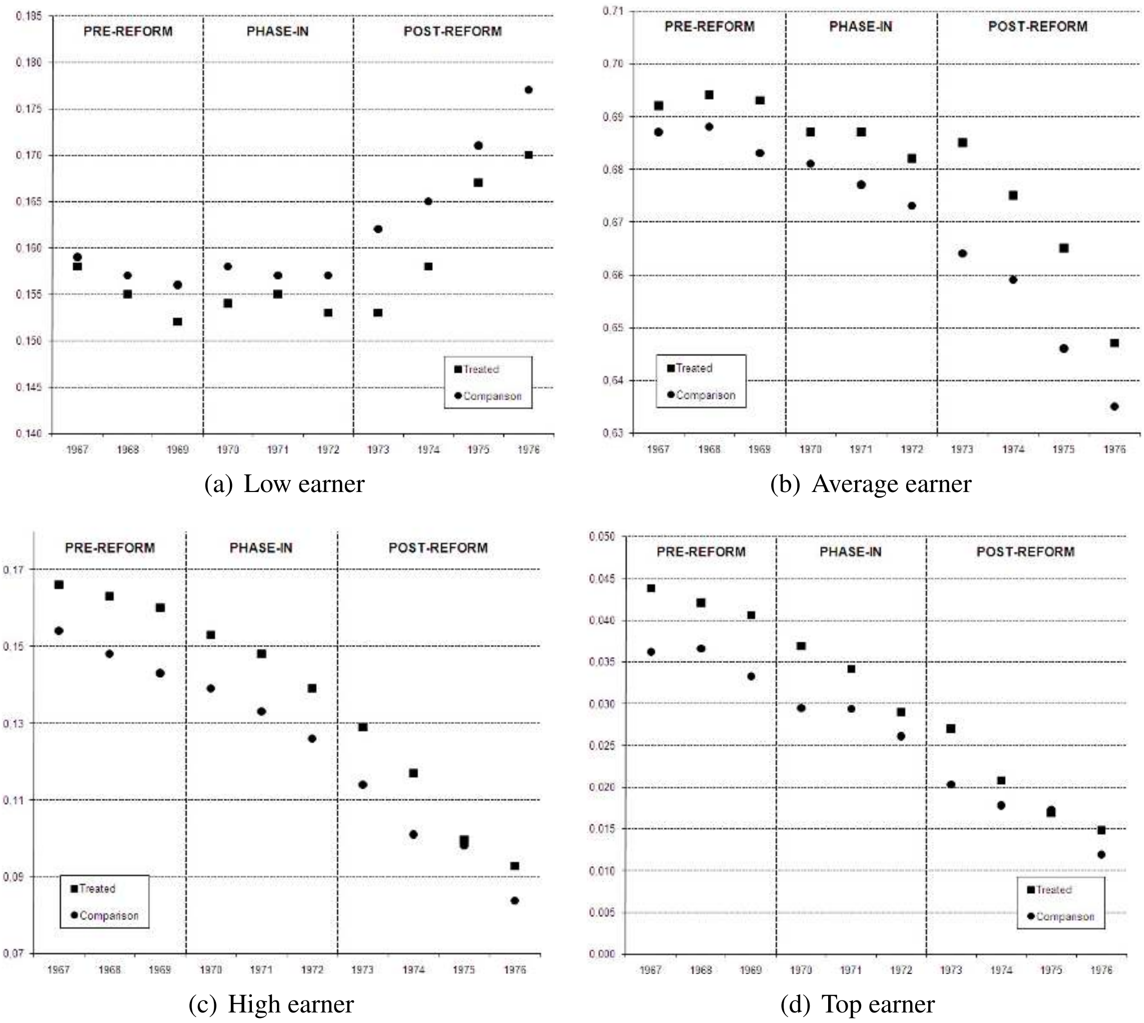


(c) High school dropout



(d) On welfare

Figure A3: Unconditional cohort means for education and welfare dependency for cohorts born 1967–1976 by treatment and comparison group



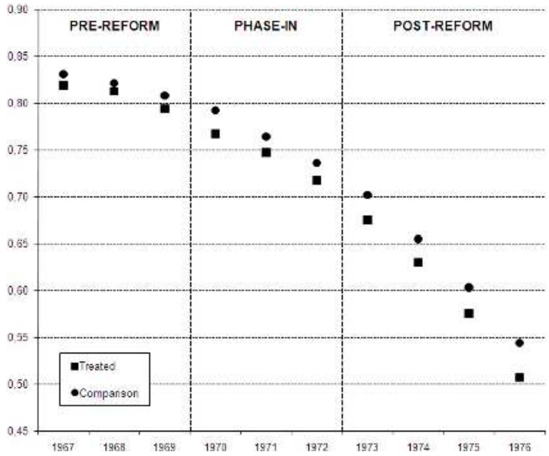
(a) Low earner

(b) Average earner

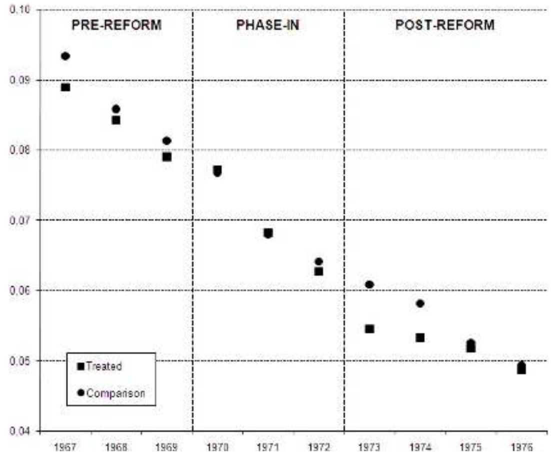
(c) High earner

(d) Top earner

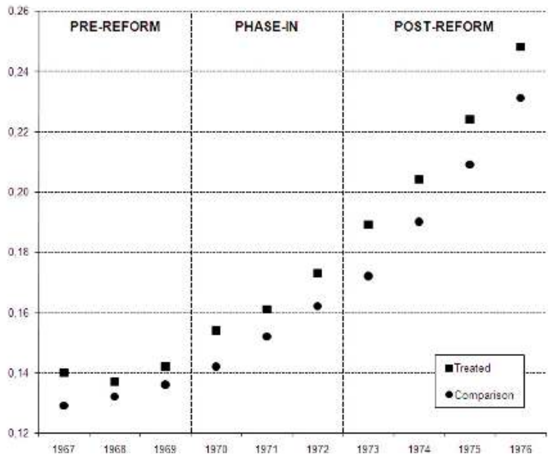
Figure A4: Unconditional cohort means for earnings for cohorts born 1967–1976 by treatment and comparison group



(a) Parent



(b) Single, parent



(c) Single, no child

Figure A5: Unconditional cohort means for cohorts born 1967–1976 by treatment and comparison group