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 indicate that learning can be more effective in early childhood than later in life (Shonkoff and Phillips, 2000). Meanwhile, classical economic theory predicts that returns to investments in early childhood are likely to be high, simply due to the long time to reap rewards (Becker, 1964). Going one step further, the economic model of skill formation developed by Carneiro and Heckman (2004) implies that investments in human capital have dynamic complementarities, suggesting 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. Economic theory implies that government’s role in facilitating child development is particularly strong, 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 (see e.g. Gaviria, 2002).

Child care institutions are important arenas for child development, and expanding child care coverage is an explicit goal in many countries. A number of randomized experiments 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. 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.,

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1 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.
2008). Nonetheless, it has fuelled an increasing interest in economics about large-scale provision of child care as a means of advancing child development.

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. 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. In particular, 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). In Mogstad and Havnes (2011), we side step the question of whether the short run effects persist, by investigating how the large-scale expansion of subsidized child care in Norway in the late 1970s affected children’s long-run outcomes.

The purpose of this article is to summarize our analysis. The empirical results are interesting in their own right, but also serve to illustrate how economists apply statistical methods to analyze the effects of child care on child development based on observational data (that is, data not generated by a randomized experiment). But first, we will briefly discuss the sense in which randomized experiments of the sort used in medical research provide an ideal benchmark for the quantitative analysis of child care and child development.

**THE EXPERIMENTAL IDEAL**

The experimental ideal can be organized around three questions, concerning the causal relationship of interest, the ideal experiment, and the identification strategy.²

The first question is: **What is the causal relationship of interest?** Although purely descriptive research has an important role to play, we believe that the most interesting research in social science is about questions of cause and effect, such as the effect of child care on child development. A causal relationship is useful for making predictions about the consequences of changing circumstances or policies; it tells us what would happen in alternative (or “counterfactual”) worlds.

The second question is concerned with **the experiment that could ideally be used to capture the causal relationship of interest.** To investigate the relationship between child development and child

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² This discussion builds on Angrist and Pischke (2009), who also provide an introduction to quantitative
care, ideally we would conduct a randomized experiment where a restricted number of child care slots were handed out in a lottery. In this case, we could compare the development of children that are randomly in child care with children that are randomly not in child care, and infer the effect of child care directly. However, ideal experiments are often difficult, practically or ethically, to implement. Still hypothetical experiments are often worth contemplating because they help us pick fruitful research topics, and helps you formulate causal questions precisely. An ideal experiment also highlights the forces you would like to manipulate and the factors you would like to hold constant. For example, questions about the causal effect of gender seem to be very difficult to give a meaningful answer, because gender is hard to manipulate in isolation (“imagine your chromosomes were switched at birth”). If you cannot devise an experiment that answer your question in a world where anything goes, then the odds of generating useful results with a modest budget and observational data seem pretty slim.

The last question is: **What is your identification strategy?** The term identification strategy refers to the manner in which a researcher uses observational data to approximate a randomized experiment. There is, for instance, every reason to suspect that parents who send their children to child care are more career oriented and have higher education, and that their children would develop differently in any case. Consequently, simply comparing the development of children in child care with children not in child care based on observational data would most likely produce a biased estimate of the effect of child care. Credible identification strategies address this so-called selection bias, so that the groups being compared become truly comparable.

**ANALYSIS USING OBSERVATIONAL DATA: AN EXAMPLE**

Havnes and Mogstad (2011) investigate how the large-scale expansion of subsidized child care in Norway in the late 1970s affected children’s long-run outcomes. Since we use observational data, we employ a statistical method to take into account differences between children and their parents that are both observed and unobserved to the researcher. Specifically, we follow much of the recent economic literature on child care and child development in using a difference-in-differences (DD) approach.³

*The child care expansion.* In June 1975, the Norwegian parliament assigned the responsibility for child care to local municipalities, but under federal provisions on educational content, group size, staff skill composition, and physical environment. The government aimed at quadrupling the number of child care places to reach a total of 100,000 by 1981, and federal funding more than doubled from USD 34.9 million in 1975 to 85.8 million in 1976, reaching 107.3 million in 1977.⁴ This implied an increase in the federal coverage of running costs from about 10 percent in 1973 to 17.6 percent in 1976, and further to 30 percent in 1977.

Altogether, the reform constituted a substantial positive shock to the supply of subsidized child care, which had been severely constrained by limited public funds. In succeeding years, the previously

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³ See for instance the study by Baker et al. (2008) of a reform in Quebec that introduced large subsidies for child care available to the general population.

⁴ Source: National budgets 1975/76 through 1978/79. Amounts are reported in 2006-USD, with USD/NOK = 6.5.
slow expansion in subsidized child care accelerated rapidly. From a total child coverage rate of less than 10 percent for 3 to 6 year olds in 1975, coverage had shot up above 28 percent by 1979. Over the period, a total of almost 38,000 child care places were established, more than a doubling from the 1975-level. 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.

The 1975-reform quickly generated large variation in child care coverage for children 3–6 years old, both across time and between municipalities. Figure 1 shows average child care coverage before and after the 1975 reform in 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 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.

**Figure 1. Child care coverage rates 1972–1985 for 3–6 year olds in treatment and comparison municipalities. Note: Treatment (comparison) municipalities are above (below) the median in child care coverage growth from 1976 to 1979.**

The identification strategy. Since we use observational data, our identification strategy needs to address that children living in municipalities with good access to child care may be inherently different and would have different outcomes in the absence of the reform. Roughly speaking, our identification strategy is the following: We compare the adult outcome of interest for 3 to 6 year olds before and after the reform, from treatment municipalities and comparison municipalities. In doing

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5 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.
so, we are able to control for unobserved differences between children born in different years as well as between children from different areas. The identifying assumption is that 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. We take several steps to examine the identifying assumption. First, we make sure our estimates are robust to inclusion and exclusion of a large set of controls capturing important child and parental characteristics, as well as municipality-specific fixed effects. Second, we run a battery of specification checks, testing or relaxing the assumption of a common time trend between the treatment and comparison municipalities.

Our identification strategy and main results on education are illustrated in Figure 2. Panel (a) graphs children’s years of education by birth cohort, separately for the treatment group and the comparison group. The child care expansion started in 1976, affecting children born between 1973 and 1976 with full force. The reform effect per child in the treatment group is given by the change in years of education for 3 to 6 year olds before and after the reform, in the treatment group relative to the comparison group. Panel (b) takes the size of the child care expansion into account, showing the predicted reform effects per child care place based on our DD approach (without controls). Figure 2 shows a good coherence between the time trends of the groups before the reform, and a striking change in the relative outcomes after the reform. Consistent with this evidence, our baseline DD estimations (with controls) suggest that the reform caused .35 years of education per child care place (cf. Panel (b)), corresponding to an effect of .06 years per child in the treatment municipalities (cf. Panel (a)).

Figure 2. Unconditional cohort means for years of education in 2006 by treatment and comparison group for cohorts born 1967-1976, and predicted reform effect per child care place. Note: Treatment (comparison) municipalities are above (below) the median in child care coverage growth from 1976 to 1979.
Main results. The results in Havnes and Mogstad (2011) show that child care had strong positive effects on children’s educational attainment and labor market participation, and also reduced welfare dependency. In light of the recent focus on dynamic complementarities in learning, a compelling question is how subsidized child care affects children's educational attainment. We estimate 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, which amounts to around .35 years of education per newly established child care place. In addition, our estimates suggest 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. When breaking down the effects according to observable characteristics, we find that most of the effect on education stems from children of low-educated mothers. Consistent with these effects on education, we also find that girls who gained access to child care postponed child bearing and family formation as adults.

Mechanisms. Finally, we take a close look at what mechanisms may be driving our findings. In particular, a crucial point in interpreting our estimates is the counterfactual mode of care, i.e. the type of care the children would be exposed to absent the reform. Following Blau and Currie (2006), consider the following three combinations of mother’s work and child care decision: not working and maternal care, working and informal care, and working and subsidized care. If the reform led to a shift from parental to subsidized child care, we would expect it to affect maternal employment rates also. A shift from parental care to subsidized 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).6

To learn about the counterfactual mode of care, we estimate the effect of the child care reform on full-time and part-time work of married mothers. To this end, we use a similar DD approach as above, comparing the growth rate in employment of mothers with the youngest child aged 3 to 6 years depending on the municipal expansion in child care coverage. 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. Specifically, we find that the child care expansion caused an increase of about 0.06 percentage points in maternal employment per percentage point increase in the child care coverage rate. The difference between the increase in maternal employment and the rise in child care coverage suggests a 94 percent crowding out of informal care arrangements by the new subsidized child care slots. This finding of weak effect of child care on maternal employment, and a significant crowding out, is in line with studies from several other countries, including Sweden (Lundin et al. 2008) and the United States (Cascio 2009).

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6 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 subsidized care did not increase much over the period 1973–1985.