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CHILD CARE AVAILABILITY AND FIRST-BIRTH TIMING IN NORWAY*

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Abstract

Both sociological and economic theories posit that widely available, high-quality, and affordable child care should have pronatalist effects. Yet to date, the empirical evidence has not consistently supported this hypothesis. We argue that this previous empirical work has been plagued by the inability to control for endogenous placement of day care centers and the possibility that people migrate to take advantage of the availability of child care facilities. Using Norwegian register data and a statistically defensible fixed-effects model, we find strong positive effects of day care availability on the transition to motherhood.

Throughout the industrialized and postindustrialized world, perhaps no other social, economic, or demographic change has had such a widespread influence on fertility as the changes in women's work roles and related increases in educational attainment. At the individual level, the inverse association between fertility and female labor force participation has long been recognized (e.g., Blake 1965). Recently a number of researchers (Ahn and Mira 1999; Bernhardt 1993; Billari and Kohler 2004; Brewster and Rindfuss 2000; Del Boca 2002; Morgan 2003; Pinnelli 1995; Rindfuss, Guzzo, and Morgan 2003; Sundström and Stafford 1992) have noted that, at the macro level, the cross-sectional relationship between female labor force participation and fertility reversed in the mid-1980s from negative to positive:[1] today, the countries with the highest levels of female labor force participation also have the highest fertility levels. Societies' institutional responses to rising

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[1]Using a time-series approach, Kogel (2004) argued that the association between fertility and female labor force participation in Organization for Economic Co-Operation and Development (OECD) countries is still negative, but that there has been a substantial reduction in the strength of this relationship beginning about 1985. Basically, whether one looks at the series of cross-sectional associations or uses a time-series approach, fundamentally similar results are found: at the macro level, the association between fertility and female labor force participation has changed, and the timing of this change can be dated to the mid-1980s.

female labor force participation is likely the major contributor to this reversal (see Morgan and Taylor 2006; Rindfuss et al. 2003). As suggested by feminist observers (e.g., Folbre 1997, 2001), countries that facilitated combining the worker and mother roles have higher fertility *and* higher female labor force participation rates. A major institutional influence is the availability and acceptability of child care centers.[2]

This macro argument has a micro-level complement: other things being equal, one would expect that those living in a place with greater access to child care should have higher fertility levels. Testing this hypothesis at the individual level is extremely difficult given data demands,[3] and as a result, empirical results have been contradictory. In this article, using an appropriate data set from Norway, we test the hypothesis that the availability of high-quality child care has a positive effect on the timing of first births. This child care hypothesis is mentioned, implicitly or explicitly, in virtually all recent discussions of the emergence of very low fertility levels (e.g., Bettio and Villa 1998;Caldwell and Schindlmayr 2003;Gauthier 2002;Kohler, Billari, and Ortega 2002;McDonald 2000). In the remainder of this introduction we detail the child care hypothesis and review tests of it to date.

THE CHILD CARE AND FERTILITY HYPOTHESIS

Arguments about child care availability and fertility have been part of the fertility and population literature for a long time (e.g., Myrdal 1941). There are two separate strands to the argument. One, within sociology, emphasizes role incompatibility. The other, within economics, stresses opportunity costs. We discuss each in turn, noting that they are substantively similar and lead to the same prediction—that is, other things being equal, increases in child care availability should lead to a younger age pattern of fertility and a higher level of overall fertility.

Sociologists have focused on the incompatibility between the mother and worker roles (e.g., Mason and Kuhlthau 1992; McDonald 2000; Morgan 2003; Presser and Baldwin 1980; Rindfuss 1991; Stycos and Weller 1967). In today's developed economies, with few exceptions, jobs and workplace settings do not permit children to be present. Even those who work from home frequently need help with child care, and Gerson and Kraut (1988) found that clerical workers who work at home spend more on child care than those who work at an office. Further, closer supervision of children is needed than was the case in our bucolic past. Thus, even with part-time work, flextime, and shift work, the issue of child care while the mother is working remains. If high-quality child care centers are available at an affordable price, and using child care centers is considered acceptable, then role incompatibility is reduced, leading to higher levels of fertility than would have otherwise occurred, along with a younger age pattern of fertility.

Economists have tended to focus on the opportunity costs associated with the mother staying out of the labor force to bear and raise her children. These opportunity costs include forgone wages while out of the labor force, along with the loss of skill development that can affect wage rates upon reentry into the labor force. The shorter the time out of the labor force, the lower the opportunity costs. Paid child care, to the extent that it is well below the woman's wage rate, would reduce the opportunity costs of childbearing and child rearing. Hence, the

[2]The feminist critique was broader than the need for quality, affordable, and available child care services. It included calls for greater paternal responsibility and sharing of child care provision and a greater value placed on caring for family dependents. See Folbre (2001).

[3]Among the data demands are over-time information on the availability of child care measured at the local level. Also, individual-level data are needed, including a fertility and migration history, with the latter necessary for an accounting of where respondents lived at various times and day care availability in those places.

availability of high-quality child care at affordable prices should be positively associated with fertility and its timing.

In this article, we examine the effect of child care availability on the transition to motherhood. There are two reasons for this focus. First, the importance and distinctiveness of the motherhood transition justifies this focus: motherhood's distinctiveness suggests that day care effects on fertility may vary by parity (Kravdal 1996). Second, the sequential nature of fertility experience allows earlier experiences to affect subsequent ones and makes samples at higher parities highly selective. Thus, understanding the transition to the mother role is key to understanding subsequent transitions.

The transition to motherhood is a key life-course event (e.g., Elder 2003; Mortimer and Shanahan 2003), usually considered more life-changing than having a second or higher-order child (e.g., Rindfuss, Morgan, and Swicegood 1988). The first birth is the entry into the mother role, a role that is time-consuming when the child is young, is long lasting, and competes with other roles that the woman might occupy, such as student or worker. While becoming a mother is normatively supported, there is substantial latitude about its timing. The general expectation is that prospective mothers should be financially and emotionally "ready" for the mother role, and they are expected to time becoming a mother so that it conflicts less with other demanding roles, such as being a student or establishing a career. In general, the higher the expectation a woman has for her education and her occupation, the later the transition. Hence the common pattern in low-fertility settings is an inverse relationship between (a) educational and occupational aspirations and achievement and (b) the timing of first births, coupled with higher rates of voluntary childlessness among those with the highest aspirations and achievement.

Now, how does this pattern intersect with child care? To answer this question from a sociological perspective, it is important to remember that the late teen and early young-adult years are a time when individuals are rapidly acquiring human capital, both in school and subsequently during their early working period. While the pattern varies by occupation and career choice, in general, the rate of acquisition of human capital declines from the early potential childbearing years to later ones, so there may be much to lose by staying at home taking care of children during the early part of the young-adult years.[4] The time and energy demands of acquiring human capital compete with the demands of motherhood if one were to become a mother during these years. Available, affordable, and acceptable child care reduces the conflict between acquiring the necessary human capital (whether it be through formal education, on-the-job training, or short-course technical training) and the demands of motherhood, and thus would permit an earlier assumption of the mother role during the demographically dense years (Rindfuss 1991;Rindfuss, Morgan, and Offutt 1996). Available, affordable, and acceptable child care would also be important at later potential childbearing ages, but the impact is expected to be less. Further, the more the first birth is postponed, the more likely the transition will never occur (e.g., Veevers 1973,1979), thus influencing the number of children women have as well as the timing of the first birth.

The theory of human capital investment (see Becker 1960; Becker and Lewis 1973) also notes that human capital investment is predominantly made at younger ages. Reasons for this include that the opportunity costs of such investments (forgone earnings) are relatively low at younger ages, and the time horizon for realizing the returns is long. Given that the

[4]More specifically, if a woman has a child while she is enrolled in school, she might quit school and not return, with a resulting long-term occupational and wage penalty. A temporary withdrawal from the labor force at an early career stage may also have more harmful long-term consequences than a temporary withdrawal at a later stage. The positive relationship between first-birth rates and accumulated work experience seen in Norway and Sweden suggests that Nordic women believe that it is best to avoid work disruptions early in their careers (Kravdal 1994; Santow and Bracher 2001).

earlier one has children, the longer one can enjoy the utility of them, and given that having available and affordable child care permits mothers to work (e.g., Blau and Robins 1988; Gustafsson and Stafford 1991), economics would also predict that an increase in the availability of child care (or a decrease in its cost) would lead to earlier childbearing, other things being equal. Further, couples may want to synchronize childbearing and childrearing costs with their family income because of a diminishing marginal utility of the income available for nonchild consumption and because borrowing money is costly (e.g., Happel, Hill, and Low 1984). When day care centers are scarce and the mother may have to quit work to take care of the child, there would be an incentive to postpone parenthood until the male partner has a higher income.

While we confine our attention in this article to the first birth, in several ways, there are likely child care links to subsequent childbearing. Postponement of the onset of childbearing is negatively related to completed childbearing (Bumpass and Mburugu 1977; Kohler et al. 2002; Marini and Hodson 1981; Morgan and Rindfuss 1999), and this relationship is likely related to women (and their partners) developing interests while postponing childbearing that compete for the time and attention of the woman even after she has had a first child. In addition to the competing-interests argument, delaying childbearing also increases the probability that decreased fecundity could reduce completed fertility. Further, after having had a child, a woman and her partner are likely to have better knowledge of local child care options. Such knowledge and experience might strengthen the impact of the local child care situation on subsequent fertility. In ongoing work, we are examining the impact of child care availability on completed fertility.

PREVIOUS EMPIRICAL EVIDENCE

Despite the intuitive appeal of the child care hypothesis, the empirical evidence for a positive effect is mixed. It is a very difficult hypothesis to test due to the data requirements to allow a methodologically defensible test. There have been several U.S. studies. Presser and Baldwin (1980) found that expected fertility was lower among mothers who reported having felt that inadequate child care had constrained their work activity. Lehrer and Kawasaki (1985) found slightly lower intended fertility among women who had to rely on nonrelatives for child care than among those whose children were cared for by relatives. Blau and Robins (1989) also found support for the child care hypothesis. On the other hand, Mason and Kuhlthau (1992) concluded that policies to increase the supply of child care "… would affect their fertility relatively little" (p. 540).

From the perspective of examining the availability of child care on fertility, all of the U.S. studies have drawbacks. None included measures of the actual availability of child care providers in the respondents' localities. Two studies (Mason and Kuhlthau 1992; Presser and Baldwin 1980) relied on perceptional data; respondents were asked if they thought that the lack of satisfactory child care led them to reduce their childbearing. Blau and Robins (1989) used information on child care costs reported by respondents and aggregated up to the 20 communities in their sample. Lehrer and Kawasaki (1985) examined the relationship between their respondents' current child care arrangements and future childbearing intentions. Three of the studies (Lehrer and Kawasaki 1985; Mason and Kuhlthau 1992; Presser and Baldwin 1980) were restricted to women who already were mothers and hence provided no information on the critical transition into motherhood.

Several European studies had measures of child care availability. Two found that the availability of informal child care through the child's grandparents increases the likelihood of childbearing (Del Boca 2002; Hank and Kreyenfeld 2003), but only Del Boca (2002) found a marginally significant, positive effect of the availability of public day care centers.

A Norwegian study demonstrated weak effects of the proportion of children in public day care on third births (with some indication that the effects were stronger for the better educated), though only in the presence of controls for region of residence, distance from a large town, and occupational structure (Kravdal 1996). Hank and Kreyenfeld (2003), looking at the effect of the availability of child care spaces for children ages 3–6 on first births in Western Germany, and Andersson, Duvander, and Hank (2004), looking at the effect of different indicators of public child care (the proportion of children enrolled in day care, the child-to-staff ratio, and the prices of day care) on second and third births in Sweden, did not find consistently significant effects in the theoretically expected direction. With the exception of Kravdal (1996), none of the studies had data on more than three years of day care availability, and all assumed that the provision of child care is exogenous. This assumption that child care is exogenous is especially questionable. Unobserved local factors that affect fertility decisions are also likely to affect the local availability of day care centers. We return to this issue later.

Here we move beyond previous research by using more appropriate data and methods that acknowledge the endogeneity of child care availability, and we find the expected strong, positive effect of the availability of organized child care on the transition to motherhood. The data set is large (N = 175,722), longitudinal, not restricted to married women, and includes time-varying measures of child care availability. Our data and methods allow us to take into account the factors governing the placement and growth of child care centers. Further, we demonstrate that not taking these factors into account leads to biased and incorrect results. Our results are robust to a variety of model specifications.

THE NORWEGIAN SETTING

The data for this article are from Norway, a country of approximately 4.6 million people. Norway is one of the Scandinavian countries that Esping-Anderson (1990, 1999) classified as *Social Democratic*, promoting equality and socializing family costs. The cornerstone of the Social Democratic welfare state is universalism—the goal is to raise everyone to a comfortable living standard rather than some minimum standard (cf. Rønsen 2004). As such, Norway has a long history of generous social welfare policies. Since 1956, Norway has been extending its parental leave, which now totals 52 weeks at 80% pay or 42 weeks at 100% pay. Four weeks of the leave are reserved for fathers both to strengthen the father's relationship with his child and to signal the need for fathers to be involved in child rearing. Also, since 1946, family allowances have been provided to families caring for children under 18, with extra benefits for single mothers. The family allowance is meant to facilitate equitable redistribution of income between families with and without children.

Norway has moderately high fertility. Its 2005 total fertility rate (TFR) was 1.84 (Statistics Norway 2007). After a decline from 2.98 to 1.66 between 1965 and 1983, an upturn occurred in the last half the 1980s. Since 1990, Norway's TFR has fluctuated within a relatively narrow band. Age at first birth has increased among more recent cohorts. Half the women born between 1935 and 1950 had become mothers by age 23; for the 1970 cohort, the median age at first birth was 27.6 (Rønsen 2004).

Norway has a mixture of both public and private child care centers, with funding from parents, local municipalities, and the national government. Public day care centers are owned and run by local municipalities. Private day care centers are nonprofit and are typically started in response to a lack of sufficient openings in public day care centers. At the end of the study period, both public and private centers received state subsidies of about $500 per month per child served, which amounts to a little more than half the cost of providing child care to one child (Håkonsen et al. 2003). Some public centers are also subsidized by the municipality, depending on local economic resources and political climate. In addition, families with low income pay a reduced price. It is also important to note that the child care system is designed to accommodate working parents, and centers are open until the time when most parents return from work.

The political motivation for public transfers to child care centers is to facilitate female employment, which is positive from a gender-equality perspective as well as beneficial for both the family's and country's economy. Further, anecdotal evidence suggests that it is widely believed that the socialization and education children receive in day care centers is beneficial.

During the end of the period studied, Norway was divided into 435 municipalities, which are the smallest units of governmental and administrative authority. These municipalities are diverse on a wide variety of dimensions. The largest is Oslo, with over 500,000 residents; some of the smaller municipalities have fewer than 500 residents. Two-thirds of municipalities are along a coastline, and only one-fourth contain or are near a major city. In 1998, the municipality average gross income for individuals 17 and older ranged from 164,900 NOK ($21,985) to 341,600 NOK ($45,550). The principal economic activities vary substantially across municipalities. Some are farming or fishing communities. Manufacturing plants are sometimes located in predominantly rural areas. There are idiosyncratic reasons why some municipalities might have more resources than others. For example, some municipalities derive significant income from the production of hydroelectric power.

Norway's educational system, especially the postsecondary component, is an open one, such that it is possible to drop out for a while and then return. Home ownership is common (approximately 80% of households own their residences). Governmental authorities have taken a number of actions to promote widespread home ownership, including income tax deductions for mortgage interest, subsidized loans for those who build small- to medium-sized residences, and initiatives to construct low-cost housing. Unemployment among young adults is relatively low. Cohabitation, nonmarital fertility, and divorce are common.

DATA

The individual data for this article have been constructed by extracting data from three different sources maintained by Statistics Norway: the Norwegian Central Population Register (Lunde et al. 1980), other Norwegian registers, and the most recent censuses. Subsequently, the data have been linked together by means of a personal identification number assigned to all individuals who have lived in Norway after 1960.[5] For each woman,[6] there is information on time of death or emigration (if any), country of birth, community of residence at any time after 1964, and dates of births of all children. Educational level and enrollment are available as of October 1 for 1980 onward, with the exception of 1983–1984. Hospitals are required to report births and deaths, and schools report on enrollment and exams. Individuals report on changes of residence, and it is in their own interest to do so, not only because they have a legal responsibility, but also to make sure that benefits from public sources come to the right place, to obtain rights as a local resident (including access to child care), and because the tax authorities would find out anyhow.

[5]In order to maintain confidentiality, Statistics Norway removed all identifiers, including place identifiers, after construction of the data file.

[6]We limit our analyses to women because the theoretical arguments involve the mother and worker roles. Also, most empirical analyses of fertility have focused on women, and hence our understanding of the fertility process from the perspective of women is better developed than that from the perspective of men.

The Norwegian population registration system is considered to be of extremely high quality. It has been used continuously for 40 years in public administration, and few errors have been reported (personal communication, Kåre Vassenden). It is constantly being updated and cross-checked against other Norwegian data systems. Indeed, the Norwegian register data are considered to be of such high quality that they will be used instead of conducting censuses in the future. For our purposes, the only known weakness of the data is that, for the years examined here, the registration system does not record the dorm or apartment residences for college students who live apart from their parents; rather, they are recorded as living in their parents' residence.

We obtain data on child care coverage and other municipality characteristics from the Municipality Database operated by the Norwegian Social Science Data Services. The child care data begin in 1973, which influenced our choice of cohorts to analyze. To avoid selectivity problems associated with women having children prior to our estimation period, we wanted to choose cohorts that reached age 15 in 1973 or later. This allows us to start our discrete-time hazard analyses at the beginning of the childbearing years for each woman and to eliminate problems caused by left censoring. The 1957 birth cohort was age 15 at the beginning of 1973. We also wanted to follow cohorts through at least age 35 in order to capture the most important parts of the first-birth process, and 1998 is the last year for which we have fertility data from the population registration system. The 1962 cohort was age 35 at the beginning of 1998. Hence, we examine six birth cohorts, 1957 to 1962. This provides us with a large sample[7] (N = 175,722) for whom we have fertility and other bio-graphical data from age 15 through 35.

For a number of reasons, we exclude from our analysis women who were born outside Norway. They are a very heterogenous group, ranging from Asian immigrants to children born to Norwegian parents who were working abroad. While we know where they were born, the diversity is too large to control effectively, and we have inadequate information about them while they lived abroad. Finally, they represent only a very small fraction of those born between 1957 and 1962.

VARIABLE MEASUREMENT

Theories about child care and fertility (e.g., Andersson et al. 2004; Rindfuss and Brewster 1996) include four dimensions of child care: availability, quality, cost, and acceptability. From the Municipality Database, we have an annual, municipality-level measure of child care availability beginning in 1973, but we do not have measures of the other three dimensions. Before describing our operationalization of availability, we briefly discuss why not having the other three dimensions is less problematic for Norway in the last quarter of the twentieth century than it might be in other settings.

Consider quality first. In Norway, minimum quality standards are set by the national government rather than by local municipalities, and these minimum standards are very high. While some child care centers might exceed these national standards, the variation across municipalities is minimal and thus unlikely to affect our results. A similar argument applies to cost. The central Norwegian government provides approximately a 50% subsidy for child care costs at all child care centers. Municipalities pay additional subsidies, especially to low-income families. There is some variation in municipality subsidies, but by and large the cost

[7]Technically, of course, it is not a sample. We have data for all Norwegian women born in Norway between 1957 and 1962 and resident in Norway at least some time between their 15th and 35th years. Thus, reports of statistical significance need to be considered as indicators of magnitudes of effects.

of child care differs little across Norway (Rauan 2006). Hence, not having cost in the model is unlikely to affect our results.

With respect to the acceptability of the use of child care centers, we are unaware of Norwegian attitudinal evidence across municipalities and time that would speak to this issue. Anecdotal evidence suggests a high level of acceptance of using child care centers and that such acceptance has existed for a long time. The Norwegian central government, democratically elected, has long supported subsidized child care centers. In short, it would appear that there has been widespread acceptance of child care centers.

Our measure of availability is the percentage of preschool-age children in day care centers by municipality and year. As such, this is literally a measure of utilization rather than capacity. We would argue that throughout the period under investigation, the demand for organized child care has exceeded the supply, and hence our variable measures both capacity and utilization. The first piece of evidence is anecdotal: one frequently hears about a shortage of day care slots, but the same is not true about unused day care facilities. A recent report by a (private) consulting organization (Asplan-Viak 2005) showed that there still is an undersupply of day care slots in many municipalities, and it was surely even more pronounced during the period we examine. Another piece of evidence involves the growth of private day care centers. Public day care is provided by municipalities. If residents are not satisfied with the local availability of day care, they can organize a private, nonprofit day care center. These private centers need to be approved by local authorities and have to operate at the same high standards as public centers. They receive the same subsidies from the national government as the public centers. Beginning in 1989, the Municipality Database broke down the number of children in public and in private day care centers. In the larger municipalities, the proportion of children in private day care centers relative to those in public day care centers increased between 1989 and 1998, suggesting that demand for day care was not being met by public facilities. In the very small municipalities, there may have been some overcapacity at times just after the opening of a new child care center. But in the larger municipalities, where most Norwegian women live, utilization and capacity were likely the same, and thus our measure of availability is a reasonable one.

Figure 1 shows the growth in child care availability over the period examined. It shows the first, second, and third quartiles for the percentage of children aged 0–6 in day care.[8] The cohorts we examine experienced steady growth in the availability of child care, quite low when these cohorts were age 15 but with the median approaching 50% in child care centers as these women aged into their 30s.[9]

Woman's age is included as a time-varying variable. Given the nonlinear relationship between age and the timing of first births, we use the following age categories: 15–19, 20–24, 25–29, and 30–35. Consistent with results from the United States (Rindfuss et al. 1988), we anticipated that the effects of our other variables would vary by age, and hence we include age interactions. The arguments are a straightforward variation of the classic

[8]The Municipality Database also had available the percentage of children aged 0–2 and 3–6 in day care centers. We experimented with using these disaggregated variables. The results were substantively similar to the 0–6 results. The 0–2 results were not as strong as the 3–6 or 0–6 results, most likely related to the generous maternity leave benefits.

[9]There is a noticeable decline in 1997, which probably is the result of two factors. First, in 1997, the age at entry into school was changed from 7 to 6, and so 6-year-olds who formerly were in child care were then in school. Second, prior to 1998, the national government paid child care centers (public and private) a monthly subsidy per child served. Some argued that this was unfair to those who did not have children in child care centers. So in 1998, the policy was changed such that all parents of children aged 1, and later extended to those with children age 2 (infants are typically at home with a parent because of the one-year parental leave), who were not using a day care center received a monthly allowance of about $500. Some parents might have decided to keep their children at home and use the $500 in other ways. It is quite possible that for 1997, capacity might have been greater than utilization. However, this should not present any problem for our analyses because 1996 is the last year of child care availability that we use.

catching-up/selection issue. For example, consider father's education. The higher the father's education, the higher the educational and occupational aspirations he would have for his daughter. Further, the higher his education, the more material and social resources he could bring to assist in his daughter's education and early work experiences. The better-educated fathers would also likely encourage their daughters to postpone fertility during their teen years as well as during their early 20s. On the other hand, it is a reasonable expectation that even well-educated fathers want to become grandfathers sometime. So, by the time their daughters are in their late 20s, well-educated fathers are likely to switch from discouraging motherhood to encouraging it. Analogous arguments apply to mother's education.[10]

Or consider the possibility of a woman's mother helping with child care. As a woman ages, her mother also ages, which could mean the maternal grandmother is available as a potential caregiver if she retires, but could also mean she is less likely to have the health and energy to care for her grandchild.

Even though we have only six, single-year birth cohorts, we include dummy variables for cohort to control for possible cohort trends. We ran the analyses with and without the cohort controls, and the substantive results are unchanged.

Two aspects of the woman's education are included: enrollment and attainment. Their relevance to the first-birth process is straightforward. It is difficult to assume child-rearing responsibilities while enrolled in school. Attainment is related to the economic resources the woman is likely to have and to the likelihood that she will be in a career-type job to which she will want to return. Attainment is measured in five categories: 9 or fewer years (compulsory level), 10 years (i.e., some secondary schooling), high school or full vocational schooling, at least one year of college, and a bachelor's degree or higher. Annual data on both attainment and enrollment are available except for 1983–1984 and for years prior to 1980. Attainment and enrollment for these years were filled in by using a hot-deck-type procedure that relies on information from neighboring cohorts (see Weisberg 2005 for a general discussion of the hot-deck approach). The details are described in Appendix A.

Table 1 shows attainment and enrollment status at ages 15–30 by five-year increments.[11] Enrollment declines as age increases, which is as expected. (Note that enrollment data are not collected until age 16, so the number for age 15 is blank in Table 1. It is safe to assume that enrollment at age 15 is essentially universal.) Even at age 30, 6% are enrolled. This reflects the high educational attainment of the population and the open nature of the Norwegian educational system.[12] At age 15, educational attainment for virtually all women is at the compulsory level; then as they age, some women move into the higher educational categories.

Some might argue that the educational process is endogenous to fertility. To see if our child care and other results were sensitive to including education, we ran the analyses with and without these education variables. Both sets of analyses produced similar results for the day

[10]These arguments about parents' education affecting the timing of the first birth do not preclude additional reasons why parents' education might be related to the timing or number of children a woman has, such as shared genes, a shared environment, or an income effect.

[11]When members of the analysis cohorts were living abroad, information on education was not collected for them. The same is true for the municipality variables. When they return, fertility information is filled in, so we have complete information on their first-birth timing. The proportion abroad is 1% or less in any given year. We include a dummy variable to control for those who spent some time outside Norway. We also ran our main model excluding those who went abroad and returned, and our substantive findings were not affected. If they moved abroad and never returned, they are censored the year they left Norway.

[12]It should be noted that the 5%–6% enrolled in their 30s is not due to 5%–6% of these women being continuously enrolled in school. Rather, women move in and out of school.

care variables that are the primary focus of this article. Here we present the results with enrollment and attainment.

We use three variables measuring characteristics of the woman's mother and father: father's education credentials, mother's education credentials, and whether the mother is alive and living near the woman. Parent's education credentials, coded in the same five categories as the woman's educational attainment, proxy for the woman's socioeconomic background. The mother's current status (alive and lives in the woman's municipality, alive and lives in a different municipality, dead or living abroad) is a measure of potential child care help from the woman's mother. Clearly, this time-varying control variable is not an ideal indicator of the extent to which the woman might think she could obtain some child care help from her mother. For example, we do not know the mother's health, nor do we know whether the mother herself is working. Further for confidentiality reasons, if a mother does not live in the same municipality as her daughter, we do not know in which municipality the mother lives. If the mother lives in a different municipality, it could be a neighboring one or it could be quite distant. Hence our general expectation is that if the mother is in the same municipality, the woman is more likely to have a first birth and at a younger age.

The links between the woman and her parents were established in the 1970 census, using relationship data. Our cohorts were aged 8–13 then, and most were living with their parents at the time of the 1970 census. But if the woman was not living with her parents in 1970, no link between her population register ID and her parents' IDs was made, resulting in missing data for the parental variables. Parental data are missing for slightly less than a quarter of the women, and it is most common for the older cohorts. Missing data on the parental variables is also likely to be related to other factors that lead women to leave the parental home at a relatively early age, and these factors, in turn, are likely related to the first-birth process. Hence, we did not want to exclude women with missing parental information. Our procedure makes missing parental information the omitted category for all three sets of parental variables. Thus, for example, the interpretation of the coefficients for mother living in the same municipality in the tables in Appendix B is the difference in the birth probability between those with missing parental information and those whose mothers live in their municipality.

Table 2 shows parental education distributions. These are time-invariant variables. That parents have lower educational attainment than their daughters (cf. Table 1) reflects the increases in educational attainment that Norway experienced in the second half of the twentieth century. Also note that there are more fathers with degrees (corresponding to BA or MA) than with only one or two years of college education.

As the study cohorts age, they are less likely to live in the same municipality as their mothers (see mother's status in Table 1). This reflects the general movement by young people out of rural Norway that was still underway during the last quarter of the twentieth century.

In addition to child care, we control for one other municipality variable: the level of female unemployment. The expectation is that high levels of unemployment among women would be pronatalist, particularly when women are in their 20s. Sociologists have argued that unemployment for some women is an opportunity to have a child (Rosenfeld 1996; Wenk and Rosenfeld 1992); economists have argued that periods of high unemployment reduce the opportunity costs of childbearing (Butz and Ward 1979). We do not have the standard measure of unemployment rates. Instead, for each municipality, we divide the number of women reported to be unemployed by the total number of women aged 16–66. Overall, the levels of female unemployment, averaged across all municipalities, are quite low, ranging from 0.3% in 1973 to 3.1% in 1995.

Some important aspects of the first-birth process are not included in our models. Chief among these are the woman's labor force and marriage/cohabitation experiences. With the exception of information from the 1970, 1980, and 1990 censuses, data on labor force activity and cohabitation status is not available in the Norwegian population register. Hence, our model should be considered a reduced-form model in which these important variables have been replaced by their exogenous determinants. This, of course, means that we cannot follow the pathways through which our set of exogenous variables affects fertility. Instead, we measure total effects of each variable.

METHODS

We estimate a discrete-time hazard model for the timing of the first birth. The statistical specification of the model is

$$\ln\left[\frac{P(B_{tij}=1|B_{t-1,ij}=0)}{P(B_{tij}=0|B_{t-1,ij}=0)}\right]=X_{t-2,ij}\beta+Z_{ij}\alpha+M_{t-2,j}\gamma+\mu_j+\varepsilon_{ij}, \tag{1}$$

where the dependent variable is the log odds that respondent i (i = 1, 2, …, N_j) from municipality j (j = 1, 2, …, C) had a birth at age t (t = 15, 16, …, 35) given that a birth had not occurred at an earlier age. The X's represent time-varying characteristics of the woman, such as her enrollment status. The time-varying variables are lagged two years. This allows for a 9-month gestation, 5-month average waiting time to conception (e.g., Bongaarts and Potter 1983), and an average birth occurring in the middle of the calendar year, which are the units used in the discrete-time hazards models. Obviously, woman's age is a time-varying variable, but we do not lag it because it makes no difference for this variable. We allow age to have interactive effects with all the other variables in the model. These interactions are not represented in Eq. (1) because they would simply clutter the notation but would not add additional estimation difficulties that need to be discussed.

The Z's represent fixed variables associated with women, such as the education category for her mother and father. The M's represent the two time-varying municipality-level variables: day care availability and female unemployment rate. For our municipality variables, if the woman migrated, we use the municipality where she resided for the year indexed by the dependent variable for fertility and then lag the value back two years. This means that she might have been living in some other location when the child was actually conceived. The argument is that people anticipate migrations by months and make childbearing decisions accordingly. To check whether this assumption might affect our results, we also ran models in which we used the municipality where she was actually living two years prior to the year indexed by the dependent variable for fertility; the results were substantively identical.

We specify two components for the error term. The μ's represent unobserved, fixed, community-level variables that affect the timing of births, while the ε's represent unobserved fixed variables at the individual level. The presence of the time-invariant ε's means that multiple observations on the same woman are correlated with each other and, since we are estimating a discrete time hazard, the later observations on the same woman are subject to selectivity bias. Because we estimate a reduced-form model, we assume that the ε's are uncorrelated with the observed variables. We discuss this further below.

The presence of the time-invariant μ's implies that observations for women within the same community are correlated with one another. However, it is also possible that the μ's are correlated with some of the observed variables in the model. In particular, a frequent argument is that a government policy variable could be correlated with unobserved characteristics of the municipality that also affect fertility. If this is the case, one would expect the estimated effect of the day care variable to be biased. In addition, the estimated impact of other variables that are correlated with day care availability could also be biased. Abundant examples in the literature demonstrate this type of bias; see Todd (2006) for a recent review. Rosenzweig and Wolpin (1986) demonstrated that the impact of public programs in the Philippines was seriously biased due to unobserved characteristics of the program distribution mechanism being correlated with health and fertility outcome variables. Pitt, Rosenzweig, and Gibbons (1993) and Gertler and Molyneaux (1994) described the implementation of the Indonesian family planning program and showed that ignoring the nonrandom nature of its implementation produces seriously biased results. All three articles used fixed-effects methods to obtain statistically correct measures of program impact. In a very different setting, Angeles, Guilkey, and Mroz (1998) also showed that naive methods (that is, those that ignore the endogeneity of family-planning clinic placement) yield biased results for the impact of family-planning programs on fertility in Tanzania. Angeles et al. compared the results of a fixed-effects estimator to a random-effects estimator that explicitly modeled the facility placement process (family-planning facilities in their case) and used a full-information maximum likelihood procedure to control for endogenous placement. The two methods gave very similar results.

An additional reason to expect bias from simple methods is that there is a great deal of migration across municipalities in Norway for women in the age range under scrutiny. Obviously, migration decisions are made for a wide variety of reasons. Schultz (1988) noted that "public sector programs, such as those in education or child health, cannot be evaluated in terms of their regional 'effect' on schooling or mortality of children unless these program evaluation studies explicitly model who migrates to benefit from the programs and how the migrants differ." In a study in Colombia, Rosenzweig and Wolpin (1988) used a fixed-effects estimator to control for endogenous migration and found that simple methods overstate program effects on child health.

Our approach in this article is to take advantage of the very large sample size and use a fixed-effects procedure to correct for possible bias due to selective migration and the potential endogeneity of the day care availability variable. This means that we do not explicitly model the migration decision, as suggested by Schultz (1988), nor do we model the availability of day care. Although such a structural approach would be more efficient if good identifying variables were available, the fixed-effects approach requires fewer assumptions, and the loss in efficiency is mitigated by the extremely large size of our sample. Thus, we include municipality dummy variables. In earlier work, we used a full set of fixed effects (434 fixed effects represented by dummy variables), but many of the municipalities are very small, making measurement over time on the municipal variables volatile. In the fixed-effects model discussed here, we use 99 fixed effects for the largest 99 municipalities (accounting for 74% of the exposure). Results remain the same if the full set of fixed effects is used, but some municipality-level variables show levels and changes that are not believable.

Of course, the fixed-effects procedure that we use will not control for correlation between the day care availability variable and time-varying unobservable variables. In the next section, we discuss the results of several tests that gauge the robustness of our results. These tests include estimating the model cohort-by-cohort and allowing the municipality fixed effects to vary to some degree through time. Another possibility would be to use a fixed-effects instrumental variables procedure (for details, see Cameron and Trivedi 2005). What would be needed is a set of identifying variables that affect the placement of day care facilities over time and that do not have a direct effect on fertility. Cameron and Trivedi suggested using time-varying exogenous variables from earlier periods. Unfortunately, we do not have such variables in our data set.

We also estimate a model without fixed effects, a “naive” model, to show what the results might be without taking into account the potential endogeneity of the day care availability variable.

The fact that we are estimating a hazard model is an additional estimation concern. It is well known that for both continuous- and discrete-time hazard models, the presence of unobserved heterogeneity can result in biased parameter estimates (Heckman and Singer 1984; or see Wooldridge 2002 for a textbook discussion). However, since we are estimating only a single birth interval, unobserved heterogeneity cannot be separately identified from the functional form specification of the model (see, e.g., Wooldridge 2002:703–706). We tried adding normally distributed heterogeneity to models with and without municipality fixed effects. In both cases, a likelihood ratio test of the null hypothesis that the heterogeneity distribution parameter was 0 could not be rejected at any standard level of significance (the *p* value was almost equal to 1). We also tried a Heckman-Singer (Heckman and Singer 1984) semiparametric estimator, similar to that used by Angeles et al. (1998), and again we found no evidence of unobserved heterogeneity.

Thus, our estimation approach is a straightforward discrete-time hazard analysis.[13] Robust coefficient standard errors are estimated by using the Ecker-Huber-White sandwich estimator (see, e.g., Angeles, Guilkey, and Mroz 2005).

RESULTS

The models we estimate are complex because age effects vary by most other covariates. As a result, the model coefficients are difficult to interpret because one must simultaneously consider the main effect of a variable and the interactive effects of that variable with age (e.g., Stolzenberg 1979). To present our results in a more intuitive manner, we show expected first-birth odds by age group and the various substantive covariates. In Appendix B, we describe how these expected first-birth odds were calculated. Also, Appendix B shows the estimated coefficients for the naive and fixed-effects models. The coefficients for the municipality dummy variables in the fixed-effects model are not shown in Appendix B but are available from the authors upon request.

We begin with the result of primary interest. Does the availability of day care affect fertility? The results are graphed in Figure 2, with the fixed-effects first-birth odds in the top panel and comparable odds from the naive model in the bottom panel. The top panel of Table 3 shows the expected odds that are graphed in Figure 2. The fixed-effects and naive models give radically different results. The naive model results suggest that greater availability of day care is associated with lower levels of fertility, which, of course, is contrary to theoretical expectations from both sociology and economics. The fixed-effects model, on the other hand, yields the theoretically expected results: at every age, greater

[13]Consistent with standard practice, the data are organized with periods of exposure as the units of analysis. The period of exposure is usually one year (12 months) but can be shorter if exposure is truncated by an event (e.g., a birth or a migration). We assign weights to exposure periods that reflect their duration. For instance, if a woman has a first birth at midyear, then she would be exposed to the risk of a first birth in only the first six months of this year. Thus the weight for this six-month period of exposure would be 0.5. A year-long period of exposure is weighted 1.0. This strategy provides a more precise estimate of exposure, the denominator of our risk measure.

availability of day care in the woman's municipality is associated with earlier childbearing. For reasons we describe in the next section, we choose the fixed-effects model as the preferred model. The implied estimates indicate substantial pronatalist effects of child care availability.

Note that the effects of increased availability of day care in the fixed-effects model are larger at the younger ages. This is consistent with arguments we made at the beginning of this article. The younger ages are when people are trying to finish their education, start their work lives, and finish the maturation process. The greater availability of day care, especially heavily subsidized day care like that in Norway, should have the largest impact at the youngest ages.

We stress that the difference between the fixed-effects model and the naive model and the positive effect in the fixed-effects model are very robust across different specifications of the naive and fixed-effects models. We ran models (a) including and excluding the woman's educational enrollment and attainment, (b) including and excluding cohort dummy variables, and (c) including and excluding those who lived for some time outside Norway. We also estimated the model separately for each cohort, and the pattern of results was the same: negative and significant day care effects for models without the municipality fixed effects, and positive and significant day care effects for models with the fixed effects. The magnitudes of the day care effects were also similar for the separate cohorts, with a slight downward trend in the size of the coefficients for later cohorts. We ran versions of the model to examine the determinants of a birth in one chosen year, conditional on a birth not having occurred until that year. In spite of the obvious selectivity problems, we found the same general pattern of results: negative day care effects for models without fixed effects and positive day care effects for models with fixed effects. Finally, we ran models in which we created time-varying municipality dummy variables for four periods: the 1970s, 1980–1984, 1985–1989, and the 1990s. This allows for the possibility that the unobservable municipality-level variables vary over time. Again, the pattern of results was the same: increased day care availability is associated with an earlier transition to motherhood.[14]

The second day care variable is whether the woman's mother lives in the same municipality. This was the best measure available to indicate the potential of the woman's mother for providing help with child care, and we expected a positive effect. Instead, we find a negative effect at all ages for both the naive and fixed-effects models. This negative effect is likely the result of our measure not picking up the availability of the woman's mother for potential help with child care. Remember that this measure does not include information on the grandmother's health or work status or precise information on travel time between the woman's and her mother's residence. Further, it might be picking up some aspects of the woman's marriage/cohabitation status. If she is neither married nor cohabiting, she may be more likely to be living in her parents' house and hence in the same municipality.

We now turn to the results for several variables in the model that are not the primary focus of our article. We do so because they have substantive import in their own right. But, more importantly, these effects were theoretically expected and are consistent with those found in other studies—both in Norway and in the United States. Thus, these results provide confidence in the model and data we used. Further, for these other variables, the naive and fixed-effects models produce identical results. Put differently, the day care center

[14]The estimated positive impact of the day care variable is attenuated when time-varying dummy variables are included. This was expected since, at the limit, dummy variables for each year-by-municipality exposure fully account for any observed variation by day care level and thus do not allow for estimation of a day care effect.

availability variable is the only one that produces different results between the naive and fixed-effects models.

The middle panel of Table 3 shows the expected odds for both the naive and fixed-effects models for father's education, and Figure 3 graphs the expected first-birth odds for the two extreme categories of father's education. Overall, the odds of giving birth as a teenager are very low in Norway. Women whose fathers had more education are less likely to have a child as a teenager compared with those whose fathers had less education. By the time the women reach their late 20s, the differences by parental education fade. Then, in the 30s, expected first-birth probabilities are slightly higher for daughters of better-educated fathers. This overall timing pattern is consistent with theoretical expectations. Well-educated parents would encourage their daughters to delay parenthood, but they also would encourage their daughters to eventually become parents. The results are similar for the fixed-effects model and the naive model, as illustrated in Figure 4, which shows the odds ratios for both the naive and the fixed-effects models of a first birth for those whose fathers had a compulsory education or less compared with those whose fathers had 15 or more years of education. The plotted value for age group 15–19 (approximately 3) implies that women with the least-educated fathers were three times more likely to have a first birth during the teen years than were those with the most-educated fathers. Effects at the older ages decline.

Note that the lines for the fixed-effects and naive models (in Figure 4) are virtually indistinguishable from each other. This result is plausible since parents' education is assumed to be exogenous with respect to daughter's first-birth timing, and thus the naive model should produce unbiased results for parent's education. The pattern of effects for mother's education is essentially identical to those for father's education, and so we do not show them here. The similarity of mother's and father's education effects has two interesting implications. First, the effect of parental (father's and mother's) education is roughly twice that shown in Table 3 because the effects of father's and mother's education are additive. Second, the fact that the effects of parental education do not vary by gender is perhaps to be expected in a gender-egalitarian society such as Norway.

Net of the powerful effects of parents' education, we also find large effects of the woman's school enrollment (see bottom panel of Table 3). Teens not enrolled in school are about three times more likely to become parents than those enrolled. After the teen years, the effects of not being enrolled (i.e., the ratio of not enrolled to enrolled) diminish, but for every age group, those not enrolled are more likely to become parents than those enrolled. This is the theoretically expected pattern. Furthermore, the results from the naive and fixed-effects models are indistinguishable.

Caution needs to be exercised when interpreting the effects of educational attainment at the youngest ages because there is little, if any, attainment variation at these ages. Refer back to Table 1. At age 15, all but 1% are at the compulsory level, and no one has reached the higher education levels. This is a simple reflection of the time it takes to move through the educational system, but it also makes it difficult to interpret the attainment results. For this reason, we do not show the attainment results. But the pattern is clear: more-educated women are less likely to have children in their teens and early 20s, and those with more education "catch up" with higher rates from the mid-20s through the 30s (see Kravdal 1994; for similar U.S. results, see Rindfuss et al. 1996). Again, comparable results are found in the naive and fixed effects models.

The last control is a municipality-level variable: female unemployment. We examined expected first-birth odds at a variety of plausible female unemployment levels (not shown). Again, we reach the same conclusion with the naive and fixed-effects models. The effects of female unemployment levels in the municipality vary substantially with age. At the youngest ages, higher levels of female unemployment are associated with higher odds of having a first birth, and at the older ages, the pattern is reversed. Remembering that locally high unemployment can trigger out-migration, especially among young adults, we expect that those who stay may have lower career and material aspirations. Their reaction to high local, and presumably temporary, female unemployment levels is to have their first child earlier. Women who are still childless in their 30s, on the other hand, may have stronger roots in the community and after repeated postponement of the first child, may be considering not having a child given high local female unemployment.

DISCUSSION AND CONCLUSION

Across a variety of specifications, the behavior of individual-level variables (e.g., parents' education) is essentially identical in the naive and fixed-effects models. Further, the results of these control variables were as theoretically expected and as have been found in prior studies in various low-fertility countries. The naive and fixed-effects models also agree on the effects of female unemployment rates, which is an attribute of the municipality. Taken together, this evidence indicates that our data and modeling strategy are producing expected and interpretable results.

The naive and fixed-effects models differ substantially, however, with respect to the effects of our key variable of interest, the availability of child care. In the naive model, the effects of child care availability are negative, strong, and statistically significant. In the fixed-effects model, the effects of child care availability are positive, strong, and statistically significant. This difference with respect to the effects of child care between the naive and fixed-effects model is robust across a variety of model specifications. We argue that the fixed-effects model is the preferred model for the Norwegian setting and that this is likely true in other settings as well.

The first argument for the fixed-effects results is that they are consistent with predictions derived from sociological and economic theories. Increased availability of day care has been widely expected to lead to an earlier timing of the transition to motherhood and higher overall fertility levels. Here we examined the timing of the first birth and found that increased child care availability is related to higher probabilities of making the transition to motherhood at every age. Prior empirical tests for the child care hypothesis were inconclusive at best and tended to find results that are the opposite of theoretical expectations, but these studies failed to control for local factors that affect fertility and the supply of child care.

Properly specified models must allow for the process by which child care facilities are established and expanded. Consider research that examined the effects of family planning programs in developing countries. The early research tended to show that communities with a family-planning program had higher levels of fertility—an association the opposite of what had been expected. Then, in a seminal paper that used data from the Philippines, Rosenzweig and Wolpin (1986) showed that family-planning programs were initiated earliest in the poorest areas with the worst health conditions. When the endogeneity of program placement was taken into account, the programs had the expected effects. In the Philippines, decisions about program placement were made centrally, in Manila.

In Norway, decisions about the establishment and expansion of child care facilities are made locally, in each municipality. But we argue that there can be characteristics of local areas that increase the likelihood of earlier and expanded child care facilities as well as low fertility. For instance, some places might have more employment opportunities for women, and these opportunities could depress fertility. Arguments about competing roles and opportunity costs predict this association. The incompatibility of work and fertility could provide incentives for local social movements to initiate or expand day care. Once day care is in place, fertility rises. This scenario is entirely consistent with our theoretical arguments, and proper specification requires that the initial association of day care and low fertility be modeled. Admittedly, it would be preferable to have direct measurement of the hypothesized variables, but these data are unavailable. Thus, we model these effects as unobserved fixed effects.

Conversely, the assumption of the naive model that the emergence of day care centers was random or at least unrelated to local fertility levels seems implausible. What we know about the provision of day care in Norway is inconsistent with this assumption. The national government has encouraged growth in the availability of child care centers, and the trend in the increase in private centers discussed earlier suggests that local demand for centers is an important factor.

Further, the fixed-effects model assumes stability in unobservable factors over a two-decade time frame. This specification is consistent with the character of locales. Some municipalities are more influenced by religious ideas, and hence traditional family values, than others, as indicated by the percentage voting for the Christian Democratic Party and church statistics on attendance. There are also regional differences in the proportion supporting socialist parties. Likewise, the growth of administrative and service jobs is consistently greater in some municipalities. Similarly, some communities consistently have higher per capita tax revenues than others. So again, the fixed-effects model has assumptions that are plausible in the Norwegian setting, although it is important to remember that the fixed-effects model does not control for unmeasured time-varying variables.

In addition to these unobserved fixed effects, women make migration decisions that might be affected by the supply of child care, and migration is common. For the cohorts of women examined here, between ages 15 and 30, only 27% had no moves across municipality boundaries, and approximately one-third had three or more moves. Among those who had a first birth, a third moved in the 24 months preceding the first birth.

Might availability of child care be a factor in deciding to move and the choice of destination? We know of no empirical literature on this issue, but we would argue that it is possible, indeed likely. There is a long literature in the United States on the value households implicitly place on school quality, using a hedonic estimation approach (Black 1999; Bogart and Cromwell 1997; Brasington 1999; Downes and Zabel 2002; Hayes and Taylor 1996). While this work contains arguments about the actual magnitude of the increase in property values if the public schools are of high quality, that a positive effect on property values exists is not in dispute. By extension, it seems plausible that potential parents might make location decisions based on the supply of child care, though it might not be the only factor involved in the migration decision, or even the most important. On average, women in our cohorts move from municipalities with lower levels of child care to those with higher levels, which may be related to their intention to have a child in the near future. The differences are not huge, but they are in the expected direction, suggesting the possibility that the migration could be endogenous to fertility. This endogeneity is, of course, corrected in the fixed-effects model (Rosenzweig and Wolpin 1986).

Further, in considering the possible influence of child care availability as one factor that might influence a destination choice, remember that the move could be of short distance. Norway is a relatively small country divided into 435 municipalities. One could move from one municipality to another and still be in the same labor market.

Some have asked whether the causal direction might be reversed, that is, whether higher fertility leads to greater availability of child care. Specifically in Norway, do municipalities with higher fertility at time t generate a larger *proportion* of children in day care at time t + 1? We examined this question with aggregate data for municipalities, and the answer is *no*: higher fertility (municipal-level TFR) is not positively associated with the percentage of children enrolled in day care (results not shown here). So on its face, this claim is weak. Further, although such questions regarding reverse causality are commonly asked, in this case, greater specificity of the proposed mechanisms is necessary before we can evaluate such arguments and assess whether they compete with the ones we propose. We have considered a range of such explanations and find none to be compelling. As an example, one might argue that our preferred model results can be interpreted as follows: the increased odds of a first birth in municipality m_i in year t caused greater availability of child care in m_i in year $t – 2$. We structured our analysis so that such claims run counter to an expected causal ordering. But one might still argue that those *anticipating a child* (or their intimates) could have lobbied successfully for their local governments to increase day care availability in anticipation of their having a child. While this scenario requires high levels of individual agency (perhaps unbelievably high) and very responsive local governments (again perhaps unimaginatively responsive), this could be a causal mechanism. But this mechanism is entirely consistent with our arguments: in response to citizens' request for relief from the competing demands of family and work (or education), local institutions respond with subsidized child care. As a result, fertility increases (just as we have argued). In fact, this scenario challenges our explanation only if women's fertility behavior is completely insensitive to local government responsiveness. That is, women's fertility timing is not affected by the increased day care availability they or their intimates sought. To believe that day care availability is an important enough issue to produce effective mobilization for day care but not important enough to affect women's fertility timing requires an unappealingly tortured logic.

To conclude, the fixed-effects model is the preferred model; the availability of high-quality, affordable child care leads to higher rates of transition into motherhood. The effects are substantively large and are consistent with intuitively appealing theoretical arguments. These results are based on the experience of six Norwegian cohorts, but we know of no reason why they would not be generalizable to other times and places. Thus, we suggest that some of the pessimism regarding public policy's inability to encourage earlier and higher fertility (e.g., Demeny 1986, 2003) may be unwarranted. The Norwegian experience suggests that widely available, affordable, and high-quality child care can encourage increased fertility.

APPENDIX A. IMPUTING EDUCATION BEFORE 1980 AND FOR 1983–1984

In the Norwegian registration data, the educational enrollment and attainment data are available for 1980–1982 and for 1985 onward, but this information is not available prior to 1980 and for 1983–1984. Because we need to follow women from the beginning of their reproductive years, it was necessary to impute the educational variables for the years it was missing. To do so, we used a modified version of the U.S. Census Bureau's traditional hot-deck procedure. We begin by describing the procedure for imputing 1983–1984.

Consider the 1962 cohort first. It is missing data on enrollment and attainment in 1983–1984, which roughly corresponds to ages 20 and 21. For every member of this cohort, we have information on their enrollment and attainment in 1982 and 1985, at ages 19 and 22. The nearest cohort for which we have complete information on ages 19–22 is the 1965 cohort, which we refer to as the "companion" cohort. We randomly sorted members of the 1962 and 1965 cohorts. Then, using four pieces of information for person k in the 1962 cohort (enrollment status at age 19, attainment status at age 19, enrollment status at age 22, and attainment status at age 22) we searched for the first person in the 1965 cohort who had the same values for these four variables. Then that person's enrollment and attainment values were used to fill in enrollment and attainment for ages 20 and 21 for cohort member *k*. Multiple copies of the 1965 cohort were used to provide sufficient matches for the 1962 cohort based on the enrollment status at age 19, attainment status at age 19, enrollment status at age 22, and attainment status at age 22. A similar procedure was used for the other cohorts except that different companion cohorts were used as follows:

Actual Cohort	Companion Cohort
1962	1965
1961	1964
1960	1963
1959	1962
1958	1961
1957	1960

A similar procedure was used for imputing enrollment and attainment values prior to 1980 except that the 1964 cohort was used as the companion cohort for all cohorts from 1957 through 1962. We used 1964 because it is the nearest cohort that could provide education values for any of the ages missing for the 1957–1962 cohorts. For each member of the 1957–1962 cohorts, we used her actual enrollment and attainment data for 1980 to match to a woman in the 1964 cohort at the corresponding age.

APPENDIX B. CALCULATING EXPECTED FIRST-BIRTH ODDS FROM THE FIXED-EFFECTS AND NAIVE MODELS

Calculation of expected values requires assumed values on all independent variables. In the article, we frequently display the odds of a first birth across categories of a selected variable, with all other variables set to their mean values. Expected odds shown in the text were calculated from expected logits from the relevant model. Expected logits are [ln[(predicted births) / (predicted years at risk with no birth)]]. Expected logits were exponentiated to produce odds. Effects of particular contrasts are estimated by the ratio of expected odds.

Appendix Table B1 shows estimated effects from the fixed-effects model (the municipality fixed effects themselves are available from the authors). Appendix Table B2 shows parallel estimates from the naive model.

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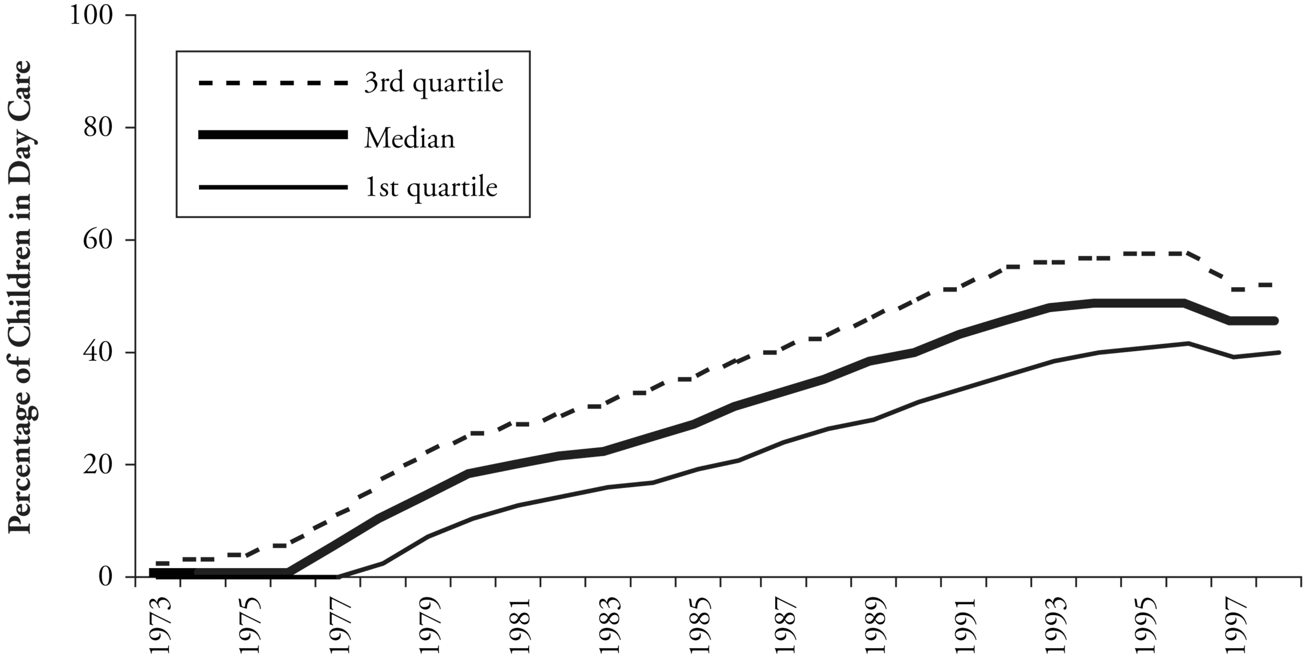

Figure 1.
Percentage of Children Aged 0–6 in Day Care: 1973–1998

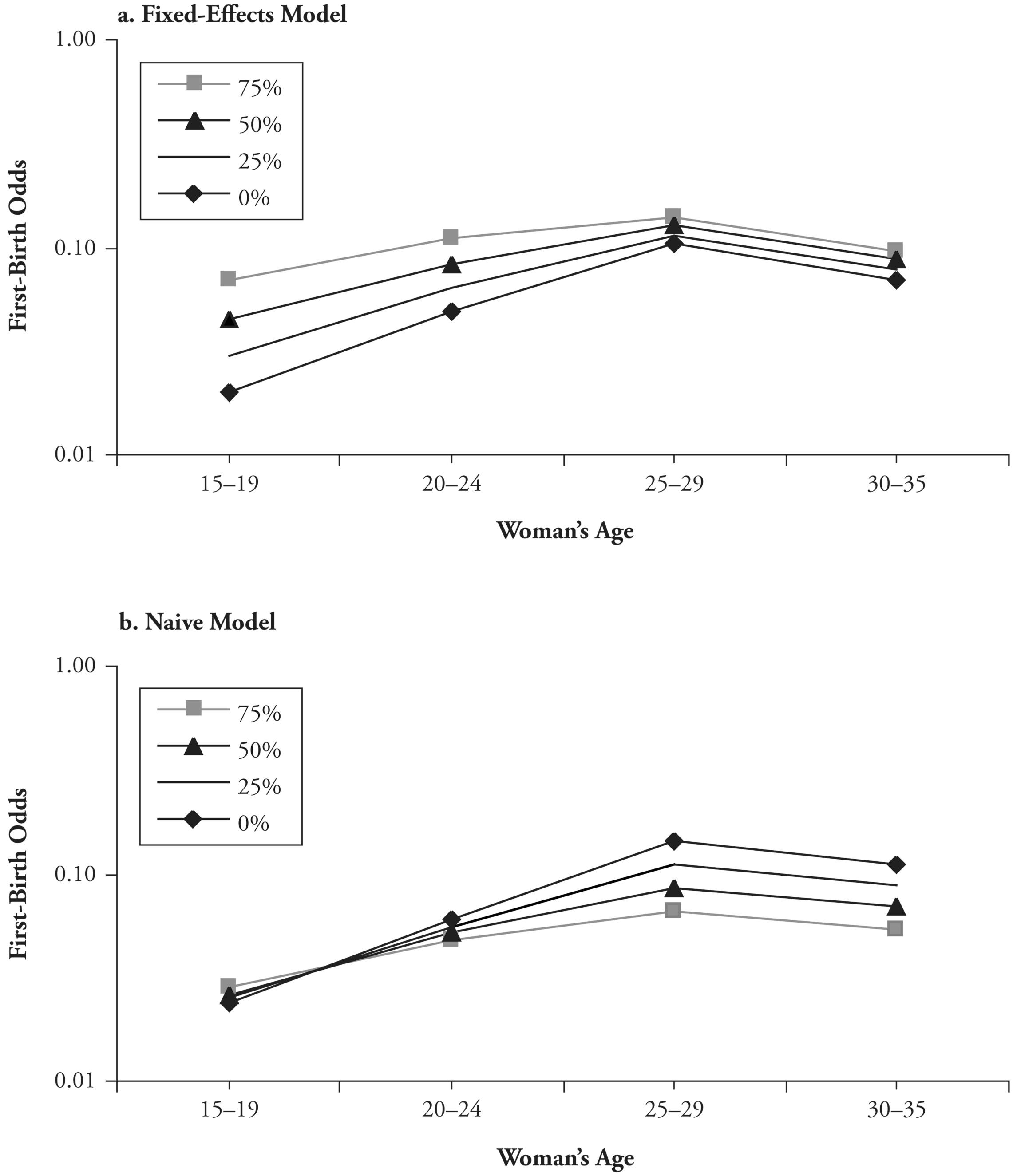

Figure 2.
Expected First-Birth Odds With Various Day Care Levels, by Age: Fixed-Effects and Naive Models

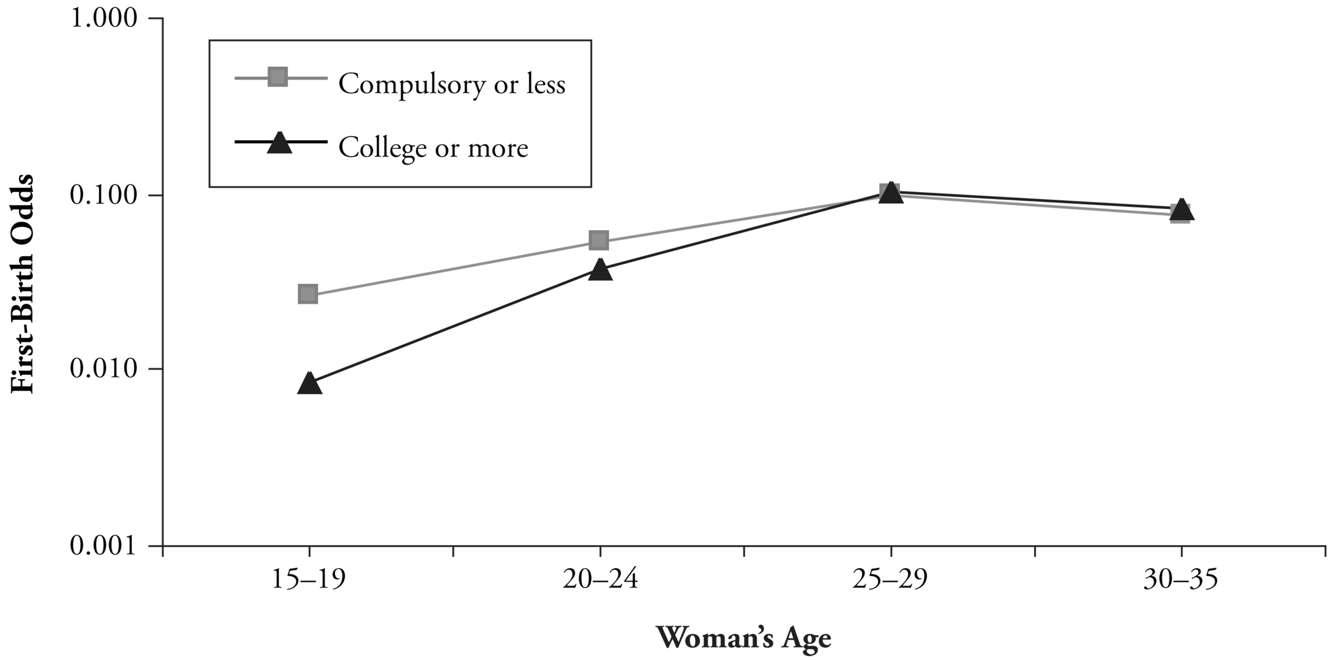

Figure 3.
Expected First-Birth Odds, by Father's Education and Woman's Age: Fixed-Effects Model

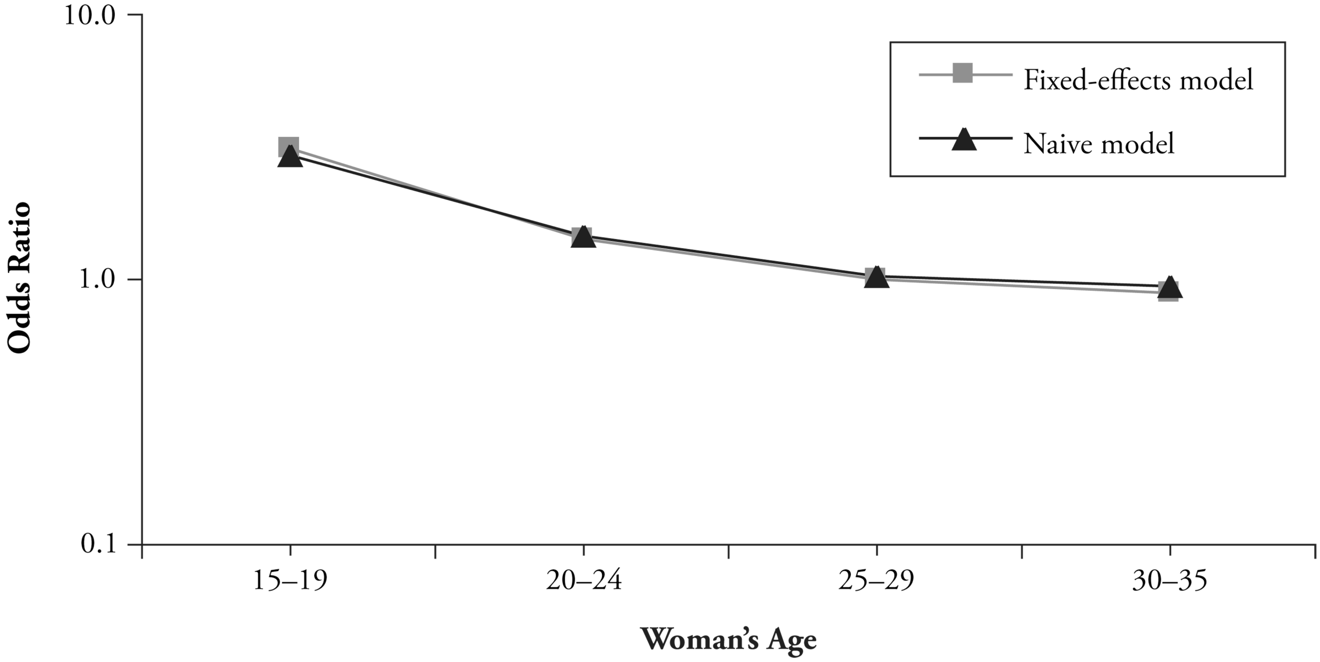

Figure 4.
Odds Ratios of a First Birth for Father's Education of Compulsory or Less to 15 or More Years, by Woman's Age: Fixed-Effects and Naive and Models

Table 1

Women's Educational Attainment, School Enrollment, and Mother's Location, by Age

Variable	Age 15	Age 20	Age 25	Age 30
Educational Attainment				
Compulsory or less	99	16	14	12
10 years	1	36	34	32
High school or vocational school	0	37	29	28
Some college	0	10	14	11
College or more	0	0	9	16
Data are missing	0	1	1	1
Total[a]	100	100	100	100
Percentage Enrolled[b]	—	23	10	6
Mother's Location				
Same municipality	75	57	39	35
Different municipality	1	19	35	37
Dead or abroad	1	2	3	5
Data are missing	23	23	24	24
Total[a]	100	100	100	100

[a]Totals may not sum to 100 because of rounding error.

[b]Enrollment data are not collected until age 16.

Table 2

Parents' Educational Distribution

Educational Level	Father	Mother
Compulsory or less	42	52
10 years	11	13
High school or vocational school	11	6
Some college	5	3
College or more	6	2
Data are missing	24	24
Total[a]	100	100

[a]Totals may not sum to 100 because of rounding error.

Table 3

Expected Odds of a First Birth for Selected Factors and Woman's Age: Naive and Fixed-Effects Models

	Woman's Age			
Variable	**15–19**	**20–24**	**25–29**	**30–35**
Percentage in Day Care				
Naive model				
0	0.024	0.061	0.144	0.110
25	0.025	0.056	0.111	0.087
50	0.026	0.052	0.085	0.066
75	0.028	0.048	0.066	0.054
Fixed-effects model				
0	0.020	0.049	0.104	0.070
25	0.031	0.064	0.115	0.078
50	0.045	0.084	0.127	0.087
75	0.069	0.110	0.140	0.097
Father's Education				
Naive model				
Compulsory or less	0.024	0.062	0.124	0.106
10 years	0.017	0.058	0.127	0.111
High school or vocational school	0.016	0.059	0.132	0.112
Some college	0.010	0.053	0.124	0.119
College or more	0.008	0.043	0.122	0.113
Fixed-effects model				
Compulsory or less	0.026	0.055	0.101	0.074
10 years	0.018	0.051	0.104	0.080
High school or vocational school	0.017	0.052	0.109	0.082
Some college	0.011	0.047	0.102	0.088
College or more	0.008	0.038	0.101	0.084
Enrollment Status				
Naive model				
Enrolled	0.016	0.048	0.099	0.087
Not enrolled	0.055	0.078	0.144	0.111
Fixed-effects model				
Enrolled	0.018	0.034	0.066	0.059
Not enrolled	0.060	0.054	0.094	0.069

Appendix Table B1

Estimated Effects and Standard Errors: Fixed-Effects Preferred Model

	Main Effect		Interactions					
			15–19		20–24		25–29	
Variable	Coefficient	*SE*	Coefficient	*SE*	Coefficient	*SE*	Coefficient	*SE*
Age (ref. = 30–35)								
15–19	0.782	0.092						
20–24	1.759	0.092						
25–29	1.603	0.100						
Cohort (ref. = 1957)								
1958	0.032	0.027	−0.030	0.033	0.078	0.031	0.041	0.032
1959	0.135	0.027	−0.135	0.034	0.101	0.032	−0.037	0.033
1960	0.178	0.028	−0.157	0.035	0.148	0.033	−0.088	0.033
1961	0.198	0.029	−0.198	0.036	0.158	0.033	−0.132	0.034
1962	0.201	0.029	−0.149	0.037	0.158	0.034	−0.133	0.034
% Aged 0–6 in								
Day Care	0.426	0.074	1.248	0.101	0.637	0.079	−0.028	0.081
Enrolled in School								
(ref. = no)	−0.213	0.026	−0.990	0.029	−0.274	0.029	−0.149	0.030
Education (ref. = compulsory or less)								
10 years	0.221	0.038	0.109	0.041	−0.232	0.041	−0.043	0.044
High school or vocational school	0.502	0.037	−0.437	0.056	−0.842	0.040	−0.186	0.043
Some college	0.670	0.040			−0.928	0.045	−0.265	0.046
College or more	0.769	0.038			−0.578	0.061	−0.298	0.045
Father's Education								
(ref. = none)								
Compulsory	0.048	0.056	−0.670	0.065	−0.981	0.060	−0.580	0.063
10 years	0.116	0.058	−1.060	0.071	−1.126	0.063	−0.615	0.066
High school or vocational school	0.147	0.058	−1.147	0.072	−1.130	0.063	−0.600	0.066
Some college	0.212	0.061	−1.686	0.091	−1.300	0.068	−0.729	0.070

Variable	Main Effect Coefficient	Main Effect *SE*	Interactions 15–19 Coefficient	15–19 *SE*	20–24 Coefficient	20–24 *SE*	25–29 Coefficient	25–29 *SE*
College or more	0.171	0.061	−1.868	0.098	−1.459	0.069	−0.698	0.070
Mother’s Education								
(ref. = none)								
Compulsory	−0.134	0.153	0.486	0.237	0.777	0.195	0.458	0.189
10 years	−0.093	0.154	0.156	0.239	0.676	0.196	0.455	0.19
High school or vocational school	−0.162	0.155	−0.036	0.243	0.676	0.198	0.450	0.191
Some college	−0.092	0.156	−0.633	0.260	0.379	0.201	0.400	0.193
College or more	−0.063	0.158	−0.497	0.269	0.320	0.204	0.323	0.195
Mother’s Location (ref. = no information)								
Dead or abroad	−0.093	0.164	−1.038	0.249	−1.775	0.206	−1.048	0.200
Same municipality	−0.073	0.162	−1.240	0.242	−1.897	0.202	−1.115	0.198
Different municipality	0.151	0.162	−0.963	0.244	−1.629	0.202	−1.070	0.198
Respondent Living								
Abroad (ref. = living in Norway)	−0.156	0.066	−0.082	0.145	−0.198	0.097	−0.114	0.083
% Female								
Unemployment	−0.159	0.008	0.461	0.013	0.356	0.010	0.148	0.010
Constant	−2.48	0.091						

Note: Fixed-effects coefficients are not shown, but are available from authors upon request.

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Appendix Table B2

Estimated Effects and Standard Errors: Naive Model

	Main Effect		Interactions					
			15–19		20–24		25–29	
Variable	**Coefficient**	***SE***	**Coefficient**	***SE***	**Coefficient**	***SE***	**Coefficient**	***SE***
Age (ref. = 30–34)								
15–19	0.544	0.092						
20–24	1.583	0.092						
25–29	1.547	0.100						
Cohort (ref. = 1957)								
1958	0.040	0.027	−0.025	0.033	0.109	0.031	0.045	0.032
1959	0.151	0.027	−0.113	0.034	0.158	0.032	−0.027	0.033
1960	0.197	0.028	−0.103	0.035	0.223	0.033	−0.063	0.033
1961	0.231	0.029	−0.116	0.036	0.235	0.033	−0.103	0.034
1962	0.245	0.029	−0.040	0.037	0.239	0.034	−0.100	0.034
% Aged 0–6 in								
Day Care	−0.945	0.068	1.125	0.099	0.641	0.079	−0.103	0.080
Enrolled in School								
(ref. = no)	−0.235	0.026	−0.989	0.029	−0.258	0.029	−0.139	0.030
Education (ref. = compulsory or less)								
10 years	0.234	0.038	0.134	0.041	−0.228	0.041	−0.047	0.044
High school or vocational school	0.504	0.037	−0.383	0.056	−0.825	0.040	−0.183	0.043
Some college	0.631	0.039			−0.856	0.045	−0.234	0.046
College ore more	0.765	0.038			−0.511	0.061	−0.279	0.045
Father's Education								
(ref. = none)								
Compulsory	0.078	0.056	−0.699	0.065	–1.000	0.060	−0.595	0.063
10 years	0.127	0.058	−1.070	0.071	–1.128	0.063	−0.615	0.066
High school or vocational school	0.132	0.058	−1.132	0.072	–1.110	0.063	−0.584	0.066

			Interactions					
	Main Effect		15–19		20–24		25–29	
Variable	Coefficient	*SE*	Coefficient	*SE*	Coefficient	*SE*	Coefficient	*SE*
Some college	0.193	0.061	−1.669	0.091	–1.280	0.068	−0.712	0.070
College or more	0.143	0.061	−1.847	0.098	–1.432	0.069	−0.675	0.069
Mother’s Education								
(ref. = none)								
Compulsory	–0.118	0.153	0.494	0.238	0.771	0.195	0.476	0.189
10 years	–0.100	0.154	0.183	0.239	0.685	0.196	0.485	0.190
High school or vocational school	−0.176	0.155	−0.008	0.244	0.689	0.198	0.483	0.191
Some college	−0.110	0.156	−0.614	0.260	0.390	0.201	0.437	0.193
College or more	−0.085	0.158	−0.462	0.270	0.337	0.204	0.362	0.195
Mother’s Location								
(ref. = no information)								
Dead or abroad	−0.099	0.164	−1.060	0.249	−1.772	0.206	−1.070	0.200
Same municipality	−0.064	0.162	−1.277	0.243	−1.912	0.202	−1.147	0.198
Different municipality	0.128	0.162	−0.967	0.245	−1.608	0.202	−1.076	0.198
Respondent Living								
Abroad (ref. = living in Norway)	−0.251	0.066	0.015	0.145	−0.155	0.097	−0.126	0.083
% Female								
Unemployment	−0.118	0.008	0.431	0.012	0.335	0.010	0.147	0.010
Constant	−2.283	0.090						