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Abstract: Using a hazards framework and panel data from the National Longitudinal Survey of Youth (1979-2004), we analyze the fertility patterns of a recent cohort of U.S. white and African American women. We examine how completed fertility varies by women's education, differentiating between intended and unintended births. We find that the education gradient on fertility comes largely from unintended childbearing, and it is not explained by childbearing desires or opportunity cost, the two common explanations in past literature. Less educated women want no more children than the more educated, so this factor explains none of their higher completed fertility. Less educated women have lower wages, but wages have little of the negative effect on fertility predicted by economic theories of opportunity cost. We propose three other potential mechanisms linking low education and unintended childbearing, focusing on access to contraception and abortion, relational and economic uncertainty, and consistency in the behaviors necessary to avoid unintended pregnancies. Our work highlights the need to incorporate these mechanisms into future research.

In recent decades, education differences in the *timing* and *partnership context* of fertility have widened. College graduates have postponed childbearing, while less educated women continue to have their first birth at relatively young ages (Rindfuss, Morgan, and Offutt 1996). At the same time, all groups of women have postponed marriage. Combined with greater union instability among the less educated, this has led to growing disparities in single parenthood (Ellwood and Jencks 2004). Recent literature on U.S. fertility has focused so intently on timing and context that the question of social class or education differences in *completed* fertility has been eclipsed. In this paper, we return attention to this topic.

U.S. socioeconomic differences in levels of fertility are longstanding, with the poor and less educated generally having more children (Blake 1968; Freedman, Whelpton and Campbell 1959). In the 1960s and 1970s, scholars and policy-makers were concerned about high fertility and the problems of over-population. Many saw technological innovation and access to effective contraception as key to reducing unintended pregnancy and driving down fertility, particularly among those with relatively large families (Ryder 1973b; Westoff 1972; Westoff and Bankole 1996). Indeed, with the diffusion of the birth control pill, the incidence of unintended pregnancy in the U.S. declined in the 1960s and 1970s (Pratt et al. 1984), and fertility dropped to about replacement. But the diffusion did little to level socioeconomic differences in fertility (Sweet and Rindfuss 1983). Further, while all education groups experienced declines in unintended pregnancies up until the 1990s, this decline *reversed* in the mid-1990s for poor and less educated women, increasing the education gradient on unintended fertility (Boonstra et al. 2006; Finer and Henshaw 2006). As of the mid-1990s, age-specific fertility rates were such that a woman with a high school degree or less would be expected to have 2.1 children in her life, while college graduates would have 1.6 (Yang and Morgan 2003). The gradient on unintended fertility is steeper, with a much higher proportion of unintended pregnancies to women with low education, and a lower proportion of the unintended pregnancies resolved with abortion. In 2001, 40 percent of births to women with less than a high school education and 10 percent of those to college graduates were unintended (Finer and Henshaw 2006).

The dominant account of education differences in fertility focuses on opportunity costs; women who can earn higher wages are more strongly motivated to limit their fertility because the income they forego for any time taken out of employment for childrearing is greater. A second, cultural view posits that having children is more socially valued in lower classes, in part because alternative sources of meaning are so scarce (Edin and Kefalas 2005). Both of these views appear more relevant in explaining why women have intended births, but they may also bear on unintended fertility. Given large – and growing – education differences in unintended fertility, we set out to better understand differences in the separate components of intended and unintended births. We assess the extent to which education differences can be explained by opportunity costs and values, using longitudinal data that span the reproductive lives of a recent cohort of U.S. women. Women's education is correlated with their parents' education and income, with their own occupational status and earnings, and with the education and earnings of their partners. Thus it can be seen, broadly, as an indicator of "class" or SES in a heterodox formulation. Our analysis, however, concerns only effects of education, with select models assessing the role of wage in mediating the education-fertility relationship.

Our analysis is the first to examine how education affects both intended and unintended fertility within a multivariate hazard framework. It is also novel in directly examining the extent to which education differentials in completed (intended and unintended) fertility are explained by differences in women's wage rates or the number of children they want. To foreshadow, we will find that education differentials in completed fertility operate largely through unintended births, and that these differentials are not explained by either wage rates or number of children wanted. In discussing our results, we draw on diverse literatures to propose three potential explanations for an education gradient in *unintended* childbearing, focusing on differences in access to contraception and abortion, instability in relationships and economic circumstances, and consistency in the behaviors necessary to avoid unintended pregnancies, or efficacy. We end calling for tests of these mechanisms in future work.

PAST THEORY AND RESEARCH

Opportunity Costs

Among economists, the dominant perspective linking education or other measures of earning power to fertility is sometimes called the Columbia-Chicago cost-of-time view (Becker 1981; Hotz, Klerman, and Willis 1997; Mincer 1963; Pollak and Watkins 1993). In this view, women decide between alternative uses of their time—in childrearing or market work. The higher their potential wage, the higher the opportunity cost to them of having a child, on the assumption that they will reduce their employment for child rearing. Women with higher potential wages will choose fewer children because the "price" of any hours of employment foregone to engage in child care is greater for those who can earn more.

Of course, well educated women typically marry better educated husbands (Schwartz and Mare 2005). In the economic view, men's income increases fertility, just as it increases the purchase of many consumer durables, because it makes having children more affordable (Becker 1960). Indeed, in theory, either men's or women's earning power could have a negative opportunity cost effect (also called a "price effect" by economists) or a positive "income effect" on fertility (Macunovich 1996). However, to the extent that women do most of the child rearing work that involves a reduction of market labor supply, the income effect of men's and the opportunity cost effect of women's earnings are expected to dominate.

Given the centrality of the price-of-time perspective, there is surprisingly little direct empirical evidence from the U.S. to support it. Schultz (1994) used 1980 Census data and showed that women's predicted wage (based on education, demographic, and state policy variables) had a negative effect on their number of children. An earlier time series analysis for 1948-1975 showed that net effects of women's average wages on fertility were negative, an effect that was stronger when the female employment rate was higher, while men's average wages had a net positive effect on fertility (Butz and Ward 1979). Using a similar method with data from 1964-1994, Macunovich (1996) found a negative effect of average female wage on fertility early in the period, but a positive effect in more recent years. She concludes that income effects may now dominate the price effects of women's earnings. Surveying the evidence, Heckman and Walker (1990) conclude that "the main question—do wages and incomes matter?—is still open."[1]

Values—How Many Children are Desired

Demographers have largely been silent on whether education differences in desired family size might explain education differences in fertility. The exception is Judith Blake, a dissenter to the view that contraceptive technology and access drive fertility trends and differentials (Blake 1967; Blake and Das Gupta 1975). She argued for the importance of motivation to have a certain number of children, yet the survey data from the 1930s through the 1960s that she assembled suggested that social classes differed little in the number of children they wanted, with the exception that those at the very bottom wanted slightly more (Blake 1967).

Using rich, qualitative interviews, Edin and Kefalas (2005) argue that, compared to their middle-class counterparts, low-income women place a higher value on having children. They rely in part on the opportunity cost view, reviewed above, pointing out that the jobs poor women might forego for childbearing pay little anyhow. But they also argue that lower class women place a higher absolute value on children, not merely a higher value relative to their other options (Edin and Kefalas 2005:205). They recount poignant testimonials from their sample of poor, unmarried mothers who saw children as the greatest source of meaning in their lives. They further cite survey data showing that those with low education are dramatically more likely to say that people without children live “meaningless lives” (Sayer, Wright, and Edin 2003). Lower education is also correlated with more traditional gender role beliefs, which might be expected to encourage fertility (Kane 1995; Kiecolt and Acock 1988).

Education Differences in Unintended Fertility

The contraceptive revolution has brought the U.S. nowhere near abolishing unintended pregnancy (Boonstra et al. 2006:26; Finer and Henshaw 2006). To assess whether pregnancies were intended, demographers generally use survey questions that classify pregnancies as unintended if women say that, at the time they conceived, they never wanted to become pregnant again, or that they wanted to become pregnant at some point in the future, but not yet. The first is called “unwanted” and the second “mistimed,” and both are seen as unintended. This classification scheme is imperfect, ignoring the possibility of a continuum with shades of ambivalence in the middle (Bachrach and Newcomer 1999; Edin et al. 2007), and ignoring men’s views, but it does allow us to compare educational subgroups.

As noted, recent data show large and increasing differences by education and income in unintended fertility (Boonstra et al. 2006; Finer and Henshaw 2006). At a proximate level, these disparities arise from less consistent and effective contraception (or abortion) by those with less education and income, even when they are not trying to get pregnant (Boonstra et al. 2006: Chapter 5; Brown and Eisenberg 1995; Silverman, Torres, and Forrest 1987). But studies on this topic are based largely on bivariate associations, so the less proximate mechanisms remain mysterious. At first glance, high levels of unintended fertility seem inconsistent with explanations of education differences based on opportunity cost and values, as these emphasize women's motivations in getting pregnant. That is, in a rational choice framework, either preferences (childbearing desires) or pecuniary incentives (based on opportunity cost) should affect whether one wants a pregnancy, and thus affect decisions about whether or not to have unprotected sex, practice contraception, or abort, finally affecting intended pregnancies. Indeed, economists interpret behavior as "revealed preferences." Yet, insofar as contraception and abortion have monetary and psychic costs, those whose values or opportunity costs give them only weak motivation to avoid childbirth may not to be as vigilant about birth control, leading to some unintended pregnancies as well (Hotz et al. 1997; Miller 1986). Thus we expect values and opportunity costs to have *some* effect on unintended fertility, but we expect both to have larger effects on intended than unintended fertility.

RESEARCH STRATEGY AND HYPOTHESES

We rely on longitudinal data rich in individual characteristics and changes in characteristics over time, allowing us to directly test two key explanations of education differences in completed fertility: the value women place on children and opportunity costs. If education affects fertility through values, then we would expect to find that a young woman's desired number of children accounts for some of the education/fertility relationship. If the lower fertility of more educated women is because of their higher earning potential, and, hence, higher opportunity cost of time spent in childrearing, then we would expect wages to explain the education/fertility relationship. Overall, we expect education differences in childbearing desires and opportunity costs to operate on intended and unintended fertility, but especially on intended.

We derive estimates of completed cohort fertility by education from discrete-time multinomial hazard models of intended, mistimed, and unwanted births. Our models include births of all parities and incorporate interactions between age and education to account for the older age schedule of childbearing among more highly educated women (Martin 2000; Rindfuss, Morgan, and Offutt 1996). All analyses are run separately for white and African American women, since both the timing and number of births vary by race (Morgan 1996; Yang and Morgan 2003). We outline our approach in greater detail below.

DATA AND METHODS

National Longitudinal Survey of Youth 1979 (NSLY79)

We use the NLSY79, a national probability sample of individuals born between 1957 and 1964 and living in the U.S. in 1979 (U.S. Bureau of Labor Statistics 2006). The sample includes just under 4,000 white and black women ages 14-21 in 1979.[2] Interviews were conducted every year from 1979 to 1994 and then every other year until 2004 (the survey is ongoing), when sample members were 39-46, or 42 years old on average. Personal interviews were conducted in all survey years except 1987, in which a limited phone interview was done. Attempts were made each year to interview all sample members, regardless of whether they missed prior surveys. Retention rates for the NLSY79 have been relatively high: they exceeded 90 percent through the early 1990s and were 77 percent in 2004. Sample weights are used to adjust for differential attrition over time.

Measures

Intended and unintended fertility. Birth histories provide dates of birth for all children born to the respondent. Key to our analysis, the NLSY79 also includes information about the intention status of each pregnancy, based on a series of questions pertaining to contraceptive use and feelings at the time of pregnancy. After a pregnancy, women are first asked whether they were contracepting at the time they became pregnant and, if not, whether it was because they wanted to become pregnant. They are then asked: “Just before you became pregnant the (first, second, third, etc.) time, did you want to become pregnant when you did?” Births are *intended* if a woman reported not using contraception because she wanted to get pregnant or said, irrespective of contraceptive use, that she wanted to get pregnant or felt indifferent about getting pregnant at that time. Births are *mistimed* if a woman said she did not want a(nother) baby at the time she got pregnant, but did want a(nother) baby at some time in the future,[3] and they are *unwanted* if she said she did not want a(nother) baby at any time in the future. Although the quality of retrospective reports of pregnancy intendedness has been debated (Ryder 1973; Trussell, Vaughan, and Stanford 1999; Westoff and Bankole 1996; Williams, Abma, and Piccinino 1999), there is ample evidence of its validity. For example, a high proportion of couples who report wanting no more children choose sterilization soon after their last wanted birth (Bumpass 1987). Also, reported birth intendedness is associated with child outcomes later in life (Barber et al. 1999; Baydar 1995; Brown and Eisenberg 1995; Crissey 2005; but see Joyce, Kaestner, and Korenman 2000). In an analysis using multiple controls and sibling fixed effects, Barber and East (forthcoming) show that while the effects of a mistimed birth on child outcomes are smaller than those of an unwanted birth, both have negative effects, suggesting that the joint category of "unintended" has some utility.

In the NLSY79, questions about pregnancy intentions are generally asked over a one or two-year interval.[4] We were unable to match pregnancy intentions to 8% of recorded births (due to either item nonresponse on the intendedness questions or missing reports on pregnancies corresponding to birth dates in the fertility history). Missing information on intention status is fairly evenly distributed across education groups. In descriptive tabulations, we allocate missing information on intention status based on observed race- and education-specific proportions intended, mistimed, and unwanted. We exclude these births from our hazard analyses. We model one birth per year, and thus further exclude all but the first birth to occur in any year of age (resulting in a loss of 1% of births in our sample). Finally, to allow for lagged predictors in our models, we exclude births prior to 1981, meaning that we exclude all births to women under age 16 (another 1% of births in our sample). See model and sample details, below.

Explanatory variables. In descriptive tabulations, we rely on years of final completed education, as measured at the last survey. Years of education are categorized into four groups: less than 12, 12, 13-15, and 16 or more. For normative schooling transitions, these correspond to high school drop out, high school graduate, some college, and college graduate, respectively, and we will typically refer to them as such. In our main analyses, we use time-varying predicted years of final completed education. As births may truncate educational experiences, the idea is to create a measure of education that will allow us to examine the effects of education on births without incorporating the effects of births on education. We lag these two years from the time a birth is assessed to ensure that they precede conception. Predicted values are generated by regressing final completed years of schooling on completed education to date; school enrollment; educational expectations at first interview; the Arms Forces Qualifying Test (AFQT), a measure of cognitive ability or skill level administered in 1981; family background, including mother's and father's education, parental income in 1979, and family structure at age 14.[5] Regressions are run separately by five-year age groups and race, and thus are fully interacted along these dimensions. Quantitative predictions are categorized to correspond to less than high school (<12), high school (12-12.99), some college (13-15.99), and college (16+). One limitation of our analysis is the possibility that the estimated effects of (predicted) education are subjected to omitted variable bias, reflecting in part other characteristics of women not in our models that cause both their educational attainment and their tendency to have unplanned births.

Our measure of childbearing desires comes from a question asked in 1979, in the first year of the survey: "How many children do you want to have?" Respondents were in their late adolescence, on average, at the time of the question. For most, this question was asked prior to first birth.[6] Distinct from childbearing ideals or expectations, this item comes closest to tapping unconstrained fertility preferences.

The most direct indicator of opportunity cost is the cost of one's time, or hourly wage rate. Our primary measure of opportunity cost is the natural log of current hourly wage in 2004 dollars. In models including current wage, we also control for whether the respondent is currently in paid employment (wages are set to the mean for those not employed for pay[7]). Wages and employment (like education) are time-varying and lagged two years. Although wages may be seen as the most direct measure of opportunity cost, they may be a poor indicator of long-term economic potential (e.g., Xie et al. 2003). We thus examine an alternative measure, which is the natural log of the running average of past wage observations (in constant 2004 dollars). This smoothes out fluctuations in wages and (arguably) provides a better indicator of long-term potential. When we include this indicator in our models, we control for the proportion of all years a woman has spent in paid employment since the start of the survey and flag the cases with no paid work experience.

In addition to these key variables, all models control for school enrollment. Some models include time-varying and lagged indicators of marital status and the natural log of spouse employment income. Specifically, these variables are used to test the sensitivity of our results to the potentially confounding effects of spouse income on the relationship between fertility and own wage. Descriptive statistics of explanatory variables are shown in Appendix Table 1.

Discrete-Time Hazard Models

We examine the education gradient on fertility in a discrete-time hazard framework, which allows us to incorporate influences of multiple explanatory variables that vary over time. We use multinomial logistic regression to examine the relative risks of an intended, mistimed, or unwanted birth of any order, at a given age, relative to having no birth. Suppressing individual subscripts, our full model can be written:

$$(1)\quad \log [P_{j(t)}/(1 - P_{j(t)})] = \alpha_{1j} + \alpha_{2j}\text{age}(t) + \alpha_{3j}\text{age}^2(t) + \boldsymbol{\beta_{1j}\text{Ed}(t\text{-}2)} + \boldsymbol{\gamma_{1j}\text{Ed}(t\text{-}2) \text{ x } \text{age}(t)} + \boldsymbol{\gamma_{2j}\text{Ed}(t\text{-}2) \text{ x } \text{age}^2(t)} + \beta_{2j}\text{Enrollment}(t\text{-}2) + \beta_{3j}\text{Desires} + \beta_{4j}\ln(\text{Wages})(t\text{-}2) + \beta_{5j}\text{Employment}(t\text{-}2)$$

where the logit or log odds of a birth is an additive function of covariates, *j* indexes births according to intention status, and *t* indexes person-years of age from 16-46. The parameters α_{1j} to α_{3j} represent the baseline hazard, or the value of the log odds of a birth of intention status *j* at age *t* when all other covariates are zero. $\boldsymbol{\beta_{1j}}$ is a vector of coefficients for the main effect of predicted education, indicated by high school, some college, and college dummies, all relative to less than a high school degree. $\boldsymbol{\gamma_{1j}}$ and $\boldsymbol{\gamma_{2j}}$ are vectors of coefficients for the interactions between education dummies and age and age-squared; these allow education's effect on fertility to vary by age, accounting for the older ages at which more educated women, and in particular, college graduates, have their children. β_{2j} to β_{5j} represent the estimated effects of school enrollment, childbearing desires, wages, and employment, respectively. Childbearing desires are measured only once, at the time of first survey, but all other substantive predictors vary over time and are lagged two years from the assessment of a birth.

A simple transformation of the logit (equation 1) makes it possible to estimate the probability of a birth of intention status *j* at age *t*:

(2) $P_{j(t)} = 1/(1 + e^{-logit})$

Applying this transformation and filling in parameter estimates from our models, we generate age-specific probabilities of intended, mistimed, and unwanted childbearing. We sum these probabilities over ages 16-46 to generate a measure of completed cohort fertility by intention status *j*,[8] which can be written:

(3) $CCF_j = \sum_{t=16}^{46} P_{j(t)}$

Sample

We include all person-years in which there is information on births, intention status, and explanatory variables. For example, if sample members drop out of the survey for a year or more but re-enter, we include all person-years for which there is data. When data are incomplete, we either exclude the affected interval (this is always the case when birth intention status is missing) or use a simple imputation strategy to fill in the gaps. In years in which the survey is not administered (odd years between 1994 and 2004) and in cases of noninterview and item nonresponse, we impute missing values on time-varying explanatory variables by assigning values from the past year.[9] Because complete, retrospective fertility histories are collected (including on birth intention status), this method allows us to retain most of the off-survey years.

As noted, time-varying explanatory variables are lagged two years from the interval in which a birth is assessed to ensure that their measurement precedes conception. We left-censor births prior to 1981 to allow for this lag, and consequently begin our analysis of births at age 16 (the youngest respondents are 14 in 1979, and thus 16 in 1981). Left censoring does not bias our results, although it may affect the precision of estimates for the youngest age groups, where sample sizes are relatively small.[10]

There are 2,543 white and 1,446 black women in our initial sample. We exclude a small number of cases (N=26) due to missing values on childbearing desires. We construct a person-year-of-age file for 3,963 individuals with data spanning 1981-2004, yielding 95,112 records. We drop 611 person-years due to missing information on the planning status of births. We censor 8,375 person-years due to attrition from the survey and 9,545 due to missing information on time-varying variables, leaving a final sample of 76,581 person-years (48,930 for whites and 27,651 for blacks). Overall, 3,934 women contribute an average of 19.5 years exposure and a total of 4,939 births.

RESULTS

Table 1 shows the average number of intended, mistimed, and unwanted births to white and black women in 2004, at the last year of the survey. Recall that respondents are 39-46 years old at last interview, i.e., at or very near the end of their childbearing years. These are thus reasonable estimates of completed childbearing for this cohort. The first column of Table 1 shows average fertility levels pooled across education groups. As is well documented (Yang and Morgan 2003), whites have lower overall fertility than blacks, although neither group is far from replacement level, with whites averaging 1.85 children per woman and blacks, 2.18. The share of all births that are unintended differs by race, with 23% (i.e., .42/1.85) mistimed and 4% unwanted among whites, compared to 38% and 18%, respectively, among blacks.

-- Table 1: Observed Completed Fertility by Education --

Columns 2-5 (Table 1) show average completed fertility by years of completed education in 2004; the final column shows the ratio of total fertility for the lowest to highest education group, providing a measure of the education gradient on fertility. Summing over all births, white high school dropouts have 1.28 times as many children as their college educated peers, and black high school dropouts have 2.22 times as many. This overall fertility gradient, however, masks substantial variation by the intention status of births. For both race groups, the education gradient on fertility increases substantially for mistimed and unwanted births, relative to intended births. Indeed, white high school dropouts have virtually the same number of intended births as college graduates, but they have 2.39 times as many mistimed and 3.30 times as many unwanted births. For blacks, the ratios for high school dropouts compared to college graduates are 1.49, 2.39, and 6.24 for intended, mistimed, and unwanted births, respectively. Clearly, education differences in fertility are driven largely by differences in unintended childbearing.

Next we examine education gradients in a multivariate framework. All subsequent analyses rely on our measure of predicted ultimate education. Table 2 reports estimates of total intended, mistimed, and unwanted births by education derived from our multinomial hazard models. These are obtained by generating age-specific predicted birth probabilities from model coefficients for each of our four education groups, holding all other variables at their (weighted) mean values, and then summing birth probabilities from age 16 to 46.[11] Model 1 is our most basic model, which includes a quadratic function of age, predicted education, predicted education by age and age-squared, and school enrollment status. Model 2 adds the respondent's early report of her desired fertility, and Model 3 adds our most direct indicator of opportunity cost, hourly wage. Results for whites and blacks are shown separately, and the complete models are reported in Appendix Table 2.

Model 1 serves as a baseline estimate of the association between education and fertility. The education gradient derived from our model (as measured by the ratio of total fertility for the least to highest educated) is *less than one* on intended births to white women. That is, white college graduates are predicted to have somewhat higher completed intended fertility than their less educated peers. White high school dropouts are predicted to have 3.02 times as many mistimed births and 6.68 times as many unwanted births as white college graduates. Results are similar for blacks, although the education gradient is always greater than one, i.e., high school dropouts have higher fertility than college graduates, irrespective of intention status: 1.36 times as many intended, 1.69 times as many mistimed, and 7.33 times as many unwanted.

-- Table 2: Predicted Completed Fertility by Education --

Model 2 additionally controls for fertility desires, measured in the first year of the survey. If education differences in fertility desires were an important source of the education/fertility gradient, we should see a lessened education gradient after desires are controlled. But Model 2 (Table 2) shows that the differentials are virtually identical with and without controlling for fertility desires. The reason that early childbearing desires do not explain any of the education gradient on fertility is that, despite the fact that desires are modestly related to fertility (Appendix Table 2), they are hardly correlated with education. For all education groups and both races, the median and modal desire is 2 children. What education differences there are in desires show college graduates wanting somewhat *more* children (Appendix Table 1a). Those white women who go on to be college graduates initially desired an average of 2.61 children, compared to 2.40 among those who drop out of high school; the analogous numbers for blacks are 2.68 for those who become college graduates and 2.02 for those who drop out of high school.

Model 3 adds indicators of opportunity cost. Our most straightforward measure of opportunity cost is the natural log of the respondent's hourly wage two survey years before the hazard of a birth is assessed. Since women who are not employed have no wage, we set their wage to the mean and control for paid employment status. We examine change in the education gradient that results from adding these labor force variables to assess whether education differences in fertility arise from the way education affects the opportunity cost to women (and their families) of any time they take out of the labor market for child rearing. If wages deter fertility, the education gradient on predicted fertility derived from Model 3 should be lower relative to that from Model 2 (Table 2), i.e., we would expect to see a reduction in the ratio of fertility of the bottom to the top education group. In fact, we find no change in the education gradient for whites (the ratio of completed fertility for white high school dropouts to college graduates remains constant at 1.29 across Models 2 and 3), and we find a very small decline for blacks (the ratio decreases from 1.88 to 1.83).

Why do wages fail to explain the education gradient on fertility? Of course wages vary by education, but they are, surprisingly, only weakly associated with fertility. Model 3 estimates in Appendix Table 2 reveal that wage coefficients are not always statistically significant, nor are they consistently negative, as predicted by economic theories of opportunity cost. Indeed, under a rational choice model, the negative effect of women's wages should be largest on intended fertility, but here we find *positive* associations. We explore the relationship between fertility and wages in greater detail in Table 3. This table shows the predicted value of fertility, varying wages under alternative model specifications. To generate predictions, we hold education constant at the level of high school graduation and set school enrollment, childbearing desires, and employment status to their weighted mean values. The first set of results is based on Model 3 (which uses current wage to measure opportunity costs). Among whites, increasing wages from the 25th to the 75th percentile *increases* intended fertility by an estimated .14 births (from 1.22 to 1.36). Wage effects on intended fertility among blacks are in the same direction and are of a similar magnitude, but miss statistical significance. Higher wages are associated with reduced unintended fertility, although changes are statistically significant only in the case of black mistimed fertility, with an increase from the 25th to the 75th percentile of wage reducing mistimed fertility by .09 births. In all cases, the ratio of fertility at the 25th percentile of wage to fertility at the 75th percentile of wage is close to one.

-- Table 3: Predicted Completed Fertility by Wages --

Next we performed sensitivity tests to see if alternative procedures would support predictions about opportunity costs. In Model 4 (Table 3), we include the same labor force variables as Model 3, i.e., current wage and employment status, but we also control for marital status and spouse employment income (all lagged by two years). Women with high earnings tend to marry men with high earnings and, in economic theory, men's wages are presumed to have an income effect, whereas women's are thought to have a price effect. Thus, without controlling for spouse income, we might mask any true effect of women's wage on fertility. Results based on Model 4 suggest that this is not the case. Women's own wages have little effect, whether or not we control spouse income.[12] Controlling spouse income, we find the same small, positive coefficients of own wage on intended fertility (statistically significant for whites) and negative coefficients on unintended fertility (statistically significant for black mistimed births).

Of course, wages fluctuate, and women's current wages may be imperfect predictors of their future wages, particularly if more education gives women access to jobs with much on-the-job training that offer lower starting wages but steeper wage trajectories. We thus try another sensitivity test, based on the idea that an average wage across past years in which the woman worked may be a better indicator of her potential wage. Instead of current wage, Model 5 (Table 3) includes the natural log of average past wage (in constant dollars) for all years. And instead of current employment, we control for the proportion of all past survey years that the respondent reports a wage and flag cases with no paid work experience (as in previous models, labor force indicators are lagged two years). Here again, we see little change in predicted fertility varying average past wage from the 25th to the 75th percentile and holding all other variables constant. Average past wage has the same positive effect on white intended fertility as current wage; none of the other changes (for whites or blacks) are statistically significant.[13] Adding controls for marital status and spouse income in Model 6 (Table 3) does not alter these findings. In sum, our results suggest that wages do little to deter fertility; indeed, they have a small, positive effect on intended fertility. As far as we can discern by our measures, opportunity cost does not explain the association between education and fertility.[14]

We briefly comment on racial differences here, although our focus is on education differences within racial groups. Overall, black women average higher fertility than white women (2.18 compared to 1.85 children). However, highlighting the importance of separate analyses by race, Table 1 shows that race and education interact, such that among the least educated women, blacks have substantially higher fertility (3.26 versus 2.06), whereas among college graduates the difference reverses, with black women averaging fewer (1.47 versus 1.61). The strong education gradient on unintended births that we emphasize here holds for both races, but even more strongly for blacks. The other striking race difference is that, within each education level, a higher proportion of black than white births are unintended (mistimed or unwanted, but especially the latter). These differences may reflect the large racial differences in union status across the education distribution; black women are much less likely to be married, and married women are much more likely to intend their births.

SUMMARY, CONCLUSIONS, AND POLICY IMPLICATIONS

Using panel data from the NLSY79, we assessed the effect of women's education on whether she had no birth relative to the competing hazards of intended, mistimed, and unwanted births. If the mother reported, after the birth, that at the time she got pregnant she wanted another child someday in the future but not right then, the birth was classified mistimed; if she said she never wanted another child, it was considered unwanted. These categories have in common that they are unintended at the time they occur. We wanted a measure of the effects of education on births not contaminated by effects of births on education. To try to get at this, we lagged our explanatory variables two years behind the wave in which the birth was reported and relied on predicted education (based on socioeconomic background, test scores, educational aspirations, and education to date). Predicted education also captures the effects of a young woman's realistic knowledge that she is likely to go to college on whether she has a baby in her early years.

We find that (predicted) education has a negative effect on completed fertility for both black and white women. Our basic model predicts that white women who do not complete high school will have 1.88 children, on average, and those who complete college will have 1.48; for blacks, the analogous decrease across education categories is from 2.39 to 1.31 (Table 2, Model 1). These figures include intended and unintended births, and obscure the fact that what education mainly deters is unintended births. Indeed, our key finding is that the education gradient on unintended fertility, and especially on unwanted fertility, is much steeper than on intended fertility. The least educated white women are predicted to have .86 times as many intended, 3.02 times as many mistimed, and 6.68 times as many unwanted births as their counterparts who have graduated from college (Table 2, Model 1). The least educated African American women are predicted to have 1.36 times as many intended, 1.69 times as many mistimed, and 7.33 times as many unwanted births as their counterparts who have graduated from college (Table 2, Model 1). In sum, the completed fertility gradient comes largely (and in the case of whites, entirely) from the tendency of less educated women to have more *unintended* births.

Do Childbearing Desires Explain Education Differences in Fertility?

We found that controlling for how many children women said they wanted at the first wave of the survey (when they were 14-21) does nothing to reduce the educational differentials. This is because less educated women do not aspire to more children. At the median, all eight race-by-education groups want exactly two. Qualitative accounts depict children as the main source of meaning for less educated women (Edin and Kefalas 2005), whereas they are one among a number of meaning-making projects for the better educated. Desires for children *relative to other pursuits* may explain education differences in fertility; education differences in early reports of how many children women want, however, do not.

Do Opportunity Costs Explain Education Differences in Fertility?

Economists argue that having a child has higher opportunity costs for well educated women because they have access to jobs with higher earnings, so they have more to lose from any scaling back of employment they do for childbearing—whether dropping out entirely for a time, or reducing their hours of paid work. We noted that even the past literature, with its mixed results, gives us reason to doubt this explanation. Some authors have cautioned that the theory does not clearly predict that high wages of women will deter fertility because the income effect (such that earning more makes raising children more affordable) might override the opportunity cost effect (Macunovich 1996), especially as purchasable substitutes for mothers' time are more available (Joshi 2002). Moreover, the theory seems to apply most clearly to intended fertility, whereas the educational gradient comes largely from unintended births.

Our study casts further doubt on the opportunity cost explanation of fertility differentials. Education does, of course, raise women's wages. However, when we enter wage rate as a covariate in our hazard models, it explains little to none of the educational gradient on fertility (Table 2, moving from Model 2 to 3). The reason that wage does not explain education effects is that wage effects on fertility are small, even if we control for husband's employment income (and marital status) (Table 3). Further, these effects are *positive* on intended fertility, opposite the opportunity cost prediction. One might criticize using current wages to test opportunity costs; wages fluctuate a good deal for young people, while the opportunity cost calculations envisioned by the economic model are based on long-term earning power. Thus, as a sensitivity test, we created an alternative measure of earning power—the average of all past years' wage rates (in constant dollars). This variable did no better than current wage at explaining education effects on fertility. Thus, we doubt that the higher opportunity cost faced by more educated women is why they are having fewer children.

What Explains the Education Differences in Unintended Fertility?

We have shown that neither wanting more children (they do not) nor lower wages (they affect fertility little) explains the higher fertility of less educated women, so what does? Because the higher fertility of the less educated comes mainly from unintended fertility, the question is really why the less educated have more unintended births of both the mistimed and unwanted variety. We propose three possibilities: socioeconomic differences in access to contraception or abortion, unstable relationships, and efficacy. We call for further research to shed light on unintended fertility – one of the "persistent empirical puzzles" in the social demography of fertility (Bachrach et al. 2007:2).

Access to contraception. The simplest hypothesis is that unintended fertility is higher among the less educated because they do not know about or cannot afford or access contraception or abortion. Those with low education and income often have no private health insurance,[15] and while most clinics accept Medicaid and many offer sliding scales, many private physicians do not accept Medicaid patients (Silverman et al. 1987). One survey of low income, fertile, sexually active women found that about a quarter of those not wanting to get pregnant were nonetheless not contracepting. But only about 9% of nonusers mentioned a reason that could be construed as about cost or availability of services (Silverman et al. 1987). A recent qualitative study corroborates this; unmarried low-income parents were asked if they had ever been in a situation where they wanted to use birth control but did not because they could not afford it. None of those with unintended pregnancies said that money had ever been a barrier to contraceptive use (Edin et al. 2007).

There is stronger evidence that access limits use of abortion. The political climate has become increasingly unfavorable to abortion since about 1980, just the period in which NLSY79 data were collected, and many women have to travel a considerable distance to obtain one (Boonstra et al. 2006). Medicaid has not paid for abortions for decades except in states that contribute their own funds for what the federal government will not cover as a matter of policy (Boonstra et al. 2006). One quasi-experimental study gained analytical leverage from the fact that North Carolina provided funding for abortions for poor women, but did so each year only until funds ran out. The study assessed whether the dates of funding cutoff, which varied by year, corresponded with trends in abortions and births among poor women, concluding that not funding abortions for poor women decreases abortions and increases birth (Morgan and Parnell 2002).

Relational and economic uncertainty. Most demographic thinking into the 1970s assumed that childbearing largely took place within long-term, stable marriages, so that the two important questions were how children were spaced, and at what number the couple would stop. Today, the relationship context is more complicated, such that an unintended birth may mean that a woman wanted a child but *not with this partner* (Zabin et al. 2000). Fertility and marriage have decoupled, with 1 in 3 births now to unmarried women (Hamilton, Martin, and Ventura 2006), and rates much higher for less educated women. While nonmarital births are often to cohabiting couples, these couples have high rates of separation (England and Edin 2007: Chapter 1; Wu and Musick 2008). Women with less education are increasingly less likely to marry (Goldstein and Kenney 2001) and more likely to divorce (Martin 2006) than those with higher education, and thus are now much less likely to be making childbearing decisions within a continuous relationship. Increased inequality and the decline of men's earnings at the bottom (Morris and Western 1999) have also increased the economic instability in which partnerships exist.

Recent qualitative studies suggest that this climate of economic and relational instability may create more ambivalence about pregnancies, leading to more pregnancies that are reported as unintended. Edin and Kefalas' (2005) study, mentioned earlier, found that poor women see the ideal context for childbearing as an economically stable married couple with a house. But when neither economic stability nor the house seem within reach anytime in the foreseeable future, valuing children greatly, they sometimes "roll the dice" and stop contraception, leading to conceptions which are not explicitly planned, but often welcomed. In another qualitative study using in-depth interviews of a subsample of unmarried couples from the Fragile Families survey, Edin et al. (2007) classified parents' nonmarital births by intendedness, finding that the more serious and stable the relationship was, the further the pregnancy was along the continuum from unplanned to ambivalent. (Few were explicitly planned.) Thus a subset of the nonmarital births they analyzed seem to fit the mold suggested by Edin and Kefalas.

Anthropologist Johnson-Hanks (2005) argues that people facing great uncertainty do not set fertility targets in the way presumed by family planning programs. The women in Cameroon that she studied could not tell her how many children they wanted because the answer depended on particular – and unpredictable – configurations of relationships, income, and kin support. Yet they did not act randomly; she characterizes their decisions as "judicious opportunism," seizing good-enough opportunities for family building when they arose.

These qualitative studies suggest that the relational and economic instability now faced by less educated women often creates ambivalence about the appropriateness of childbearing, leading at times to couples opening themselves to pregnancy in less than ideal circumstances. Some of these pregnancies will be classified as "unintended" by the usual survey methods, and, indeed, they are not explicitly intended, even though they may be at least ambivalently wanted at the time of conception. Of course, if such ambivalent pregnancies were the main explanation of the education gradient in births classified in surveys as unintended, it would call into question the measurement of intendedness that we (and other demographers) have used. We believe that this conclusion is premature, but call for more qualitative studies that probe the intentionality around conception.

Efficacy. Another possible explanation for education disparities in unintended fertility is that those with less education are less apt to believe that their own actions can have effects, and also have less of the self-regulation needed to engage in the sometimes onerous behaviors that further one's long-term goals. Applied to contraception, these two factors lead to less consistency in contraception and more unintended pregnancies. Mirowsky and Ross (2007) show that those with more education perceive themselves to have more control over their lives. They theorize that a greater subjective sense of control is built up from experiences with challenges that one can master, more autonomous jobs, the control that having money brings, and better health. The disadvantages of the childhood and adult circumstances of women with little education may mitigate against a sense of control. Perceived lack of control may in turn be a self-fulfilling prophesy, discouraging one to undertake even actions that would pay off. For example, Ross and Mirowsky (2003) show that those with less education are less likely to engage in health-promoting behaviors (e.g., exercising or refraining from smoking), and that these create some of the education differentials in health. In the qualitative interview study of low income unmarried parents described above, Edin et al. (2007) concluded that about a quarter of the pregnancies they analyzed were cases in which there was not even an ambivalent desire to have a child at the time of conception, yet the couple was not contracepting; the authors saw these cases as indicative of lack of efficacy, since behavior was not aligned with goals.

Something akin to this explanation of education or class differentials in fertility can be found in decades-old qualitative research by Rainwater (1960), which found many working class and poor couples with more children than they wanted. Operating in an era before the birth control pill, and before sterilization became common, the main means of birth control were condoms and the diaphragm. Most people in all classes knew about available methods, and money to buy them was not mentioned as the limitation. Nor did opportunity costs seem a factor as few women thought of employment as an option unless income was desperately needed. But education related to consistency in the use of these contraceptives. Some of the education differences in contraceptive use leading to unintended pregnancies, reviewed above, may be cases where individuals do not believe they can control their fertility and/or are lacking the self-regulation to consistently engage in the hassle of contraception, even in situations where they do not want a pregnancy.[16]

Implications for Public Policy

The issue of high unintended fertility concentrated among disadvantaged populations is more pronounced in the U.S. than other affluent nations (Brown and Eisenberg 1995: Chapter 2). No doubt related to its high level of unintended childbearing, the U.S. has a relatively high rate of early and nonunion fertility (Kiernan 2002; Heuveline, Timberlake, and Furstenberg 2003), also concentrated among the disadvantaged (Ellwood and Jencks 2004). Despite the dramatic reduction in U.S. teen fertility (Hamilton, Martin, and Ventura 2006), far from converging with other nations, there are signs that education and income inequalities in U.S. family patterns are growing. For example, education differences in divorce rates have widened (Martin 2006; Raley and Bumpass 2003), as have differences by education and income in the proportion of births that are unintended (Boonstra et al. 2006; Finer and Henshaw 2006).

Several decades ago, demographers saw the high rates of unintended fertility in the U.S. and elsewhere in the context of fears about world overpopulation. They envisioned that contraceptive availability could eliminate most unintended births and usher in zero population growth. Instead, the U.S. got to about zero population growth even while the proportion of unintended births remained high, especially for those with low education (Finer and Henshaw 2006). Some women are having larger families than they intended. But, overall, U.S. women have .33 fewer births at the end of their childbearing years than their stated expectations early in life (Quesnel-Vallee and Morgan 2003). Our Table 1 shows that of all the eight race-by-education groups, only black women in the lowest education group have fertility appreciably above the replacement level of 2.1. U.S. fertility among this most recent cohort to reach the end of their childbearing years would fall well below replacement without unintended childbearing.

Well educated women are relatively successful at avoiding unintended fertility, but they often have fewer children than they wanted early in life. Our analysis suggests that it is not their high wages leading to their low fertility, nor does it seem to be a lack of stable, economically secure marriages. College graduates have adopted a cultural schema that demands intensive mothering (Hays 1996; Lareau 2003), thus many wait until they are established in careers and marriages before having children. We suspect that it is their late start at marriage and fertility, as well as the difficulty they experience when trying to combine paid work and parenting, that leads many of them to end up with fewer children than desired. The lack of male participation in parenting and limited parenting accommodations by employers may discourage the transition to motherhood and the decision to have a second child.

Women with less education are much less likely to put off childbearing and are more likely to have unintended pregnancies. Their fertility, however, averages only slightly above replacement; most women in this group are not having more children than they wanted, more typically, they are having them at times and in situations that they view as less than ideal. Our analysis casts doubt on the usual explanations of the fertility-education relationship. Less educated women do not start out wanting more children than those who get more education, nor do their lower wage levels explain much of their higher unintended fertility. Since our analysis was unable to explain educational differentials in unintended fertility, we could only speculate on their causes. We have suggested three mechanisms that deserve further study – that lack of economic and geographical access to abortion is a barrier, that the difficult economic circumstances common among less educated women lead them to open themselves to pregnancies that they ambivalently want when the right circumstances seem unlikely, and that these same difficult circumstances erode the sense of control and self-regulation needed for contraceptive efficacy when women are clear that they do not want a conception. Assessing the merits of these hypothesized explanations will help to inform policies aimed at reducing unintended fertility.

NOTES

1. These authors provide evidence that Swedish women in cohorts with higher average earnings have lower fertility, but they lacked individual measures of earnings, and the cohort effects may confound other downward cohort influences on fertility with wage effects, as the authors admit.

2. This number excludes the military, poor white, and Hispanic oversamples. The full military sample was dropped from the NLSY79 in 1985; the poor white oversample was dropped in 1991. Oversamples of African Americans provide an adequate number of cases for the separate analysis of this group, but sample sizes are too limited to extend our analysis beyond blacks and whites. We exclude men, as we know them to misreport fertility, particularly nonmarital fertility (Rendall et al. 1999).

3. Given this definition, pregnancies that came later than the woman wanted are not classified as mistimed, but rather as intended, so long as she reports that she still wanted to get pregnant at the time of conception. Unlike some other surveys, the NLSY79 does not explicitly ask if the pregnancy came too soon, about the right time, or too late.

4. The intendedness questions are asked first in 1982 about all prior pregnancies. Questions are repeated yearly from 1982 to 1986; after 1986, they are asked every other year.

5. Missing values on quantitative variables are set to the mean and flagged, and indicators are included for missing values on categorical predictors.

6. About 15% of the sample had already had a child by 1979, when NLSY79 members were between 14 and 21 years of age, and their stated desires may have been affected by prior fertility. Our results were not sensitive to whether these women were included in our analyses.

7. In all analyses of women's wages, we set non-working women's wages to the weighted mean and include a control for whether or not employed. Because of this control, the wage coefficient does not vary depending on the particular treatment of missing data, that is, by whether we replace missing data with the mean, 0, or some other value. The coefficient on the employment dummy used to factor out the missing cases on wage *do* change depending on what values missing scores are assigned, but we do not interpret the meaning of the employment coefficient.

8. Our predictions underestimate total fertility by about 10%. Recall that our hazard analysis excludes all but the first birth per year of age (loss of 1% of births in our sample), births to women under 16 (loss of 1%), and births with missing information on intention status (loss of 8%).

9. We exclude person-year observations when valid data are not available from the prior year, and we flag imputed values in our analyses. We impute values for 25% of the person-years; most imputations (about 20%) are done to fill in data for non-survey years. We examined various imputation strategies, and our results were not sensitive to the number of years we looked back to fill in missing values, nor were they sensitive to how imputed values were flagged.

10. This is true at the upper tails of the age distribution, as well. Namely, while we can estimate age-specific birth probabilities for women up to and including 46, these estimates are based on fewer observations than estimates from ages that all sample members pass through during the survey period, i.e., 23-39.

11. We tested an alternative method of generating predictions, setting controls to woman-specific values, taking a weighted average of predictions across women by age and education group, and then summing weighted means over all ages for each education group. Results were similar whether we followed this approach or set controls to their weighted mean values, as described in the text.

12. The estimated effect of spouse income is positive and statistically significant on intended births among whites and blacks (as predicted by economic theory), but it is not statistically significantly related to unintended fertility (results available upon request).

13. In results not shown, we tried another variation. We constructed a (CPI-adjusted) average of a woman's wage in all survey years in which she was employed, including those *after* the birth. While clearly an extreme variation, the merit of the measure is that women who are on a steep wage trajectory (and know that early in their careers) are "credited" for their higher later earnings even in their early years. Thus, if women with high expected earnings have fewer children because of the high *anticipated* rather than current opportunity costs, adding this measure to our regressions should have explained a share of the education gradient on fertility. But it did not: its estimated effect on intended fertility was close to zero and statistically insignificant.

14. Another version of the opportunity cost argument is that welfare systems create incentives for fertility among low educated women who qualify for assistance. The fact that it is unintended – not intended – fertility that declines with education makes this hypothesis less compelling.

15. Future research could make use of measures of health insurance in the NLSY79 to test the importance of insurance to unintended births.

16. The NLSY79 contains a measure of how much mastery individuals feel over their environment. In results not shown, we included this in models, and did not find it to mediate the education effect. While further research is needed, this may indicate that believing in one's control is not sufficient—that self-regulation is also critical.

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Appendix 1a. Time-Invariant Sample Characteristics

	Whites	Blacks
Childbearing desires in 1979 -- all women	2.57	2.33
Childbearing desires in 1979 -- by final education		
<HS	2.40	2.02
HS	2.53	2.26
Some college	2.64	2.34
College	2.61	2.68
Variables in predicted education regressions		
Educational expectations in 1979	13.98	13.94
Standardized scores on AFQT	0.55	-0.54
Mother's education	11.96	10.81
Father's education	12.18	10.40
Parents' income in 1979	24,159	12,203
Lived with both parents until age 14	0.80	0.49
Total sample size pooled over person-years	48,930	27,651

Notes: Means are weighted and *N* 's are unweighted. Characteristics are shown for our analysis sample, poled over all person-years.

Appendix 1b. Time-Varying Sample Characteristics at Three Observation Points and Over All Years

	Whites				Blacks			
	1981	1991	2001	All Years	1981	1991	2001	All Years
Completed education to date								
<HS	0.59	0.08	0.06	0.13	0.68	0.15	0.11	0.20
HS	0.28	0.44	0.40	0.42	0.22	0.43	0.42	0.41
Some college	0.12	0.23	0.24	0.24	0.10	0.29	0.32	0.29
College	0.01	0.25	0.30	0.21	0.00	0.12	0.15	0.10
Predicted completed education								
<HS	0.09	0.07	0.06	0.07	0.16	0.15	0.11	0.14
HS	0.19	0.33	0.39	0.32	0.24	0.38	0.42	0.34
Some college	0.53	0.33	0.25	0.34	0.54	0.33	0.32	0.38
College	0.19	0.27	0.30	0.27	0.07	0.14	0.15	0.14
Age	19.29	29.52	39.55	30.24	19.14	29.53	39.46	30.32
Currently enrolled in school	0.65	0.08	0.07	0.17	0.68	0.06	0.05	0.15
Current wage (if employed)	8.46	13.82	15.91	12.98	9.16	11.20	12.30	11.19
Ln of current wage (if employed)	1.97	2.42	2.56	2.36	2.06	2.28	2.37	2.25
Currently employed	0.62	0.88	0.86	0.85	0.35	0.80	0.85	0.74
Average past wage (when employed)	8.46	9.66	9.14	9.07	9.16	8.43	7.43	8.03
Ln of average past wage (when employed)	1.97	2.18	2.12	2.10	2.06	2.03	1.89	1.96
Proportion years employed from survey start	0.62	0.85	0.86	0.83	0.35	0.70	0.73	0.67
Never employed	0.38	0.01	0.00	0.05	0.65	0.05	0.02	0.13
Spouse wage income (if married)	27,242	49,543	57,091	46,592	22,847	33,114	39,427	32,697
Ln of spouse wage income (if married)	9.79	10.29	10.55	10.31	9.52	9.39	9.97	9.57
No spouse income (if married)	0.03	0.11	0.08	0.10	0.06	0.25	0.12	0.20
Married/spouse present	0.13	0.62	0.72	0.55	0.05	0.30	0.31	0.25
N	1,909	2,243	1,855	48,930	1,040	1,277	1,052	27,651

Notes: Means are weighted and *N*'s are unweighted. Characteristics are shown for our analysis sample, including all valid person-years in the years indicated. All wages/income are in constant 2004 dollars.

Appendix 2a. Multinomial Logistic Regression Models 1-3 of Intended, Mistimed and Unwanted Births -- Whites

	M1			M2			M3		
	Intended	Mistimed	Unwanted	Intended	Mistimed	Unwanted	Intended	Mistimed	Unwanted
Age	0.19	0.04	0.33	0.18	0.04	0.34	0.18	0.06	0.38
Age-squared	-0.01 *	0.00	-0.01	-0.01 *	0.00	-0.01	-0.01 *	0.00	-0.01
Predicted education (<HS omitted)									
HS	-6.86 *	-5.68 *	-4.70	-7.08 *	-5.87 *	-4.52	-6.95 *	-5.78 *	-4.25
Some college	-11.38 *	-9.00 *	-7.03	-11.54 *	-9.16 *	-6.86	-11.28 *	-9.26 *	-6.65
College	-26.10 *	-19.89 *	-10.47	-26.29 *	-20.07 *	-10.21	-25.71 *	-20.03 *	-9.94
HS X age	0.47 *	0.41	0.23	0.49 *	0.42	0.22	0.48 *	0.42	0.21
HS X age-sq	-0.01 *	-0.01	0.00	-0.01 *	-0.01	0.00	-0.01 *	-0.01	0.00
Some college X age	0.75 *	0.62 *	0.31	0.76 *	0.63 *	0.30	0.74 *	0.64 *	0.30
Some college X age-sq	-0.01 *	-0.01 *	0.00	-0.01 *	-0.01 *	0.00	-0.01 *	-0.01 *	0.00
College X age	1.62 *	1.22 *	0.46	1.64 *	1.23 *	0.45	1.60 *	1.24 *	0.44
College X age-sq	-0.02 *	-0.02 *	-0.01	-0.02 *	-0.02 *	-0.01	-0.02 *	-0.02 *	-0.01
Currently enrolled in school	-0.58 *	-0.08	0.35	-0.58 *	-0.08	0.35	-0.57 *	-0.13	0.30
Childbearing desires				0.09 *	0.07 *	-0.11	0.09 *	0.07 *	-0.11
Ln of current wage							0.19 *	-0.10	-0.07
Currently employed							-0.09	-0.29 *	-0.44 *
Imputation flag	0.14	0.12	-0.01	0.13	0.11	-0.01	0.11	0.06	0.15
Intercept	-3.54	-2.31	-8.08 *	-3.61	-2.38	-7.97 *	-3.94 *	-2.31	-8.13 *

Notes: An imputation flag is included for person-years in which any time-varying variable is filled in with data from the previous year.
** P<.05*

Appendix 2b. Multinomial Logistic Regression Models 1-3 of Intended, Mistimed and Unwanted Births -- Blacks

	M1			M2			M3		
	Intended	Mistimed	Unwanted	Intended	Mistimed	Unwanted	Intended	Mistimed	Unwanted
Age	0.06	0.25	0.01	0.06	0.25	0.01	0.06	0.25	0.03
Age-squared	0.00	-0.01 *	0.00	0.00	-0.01 *	0.00	0.00 *	-0.01 *	0.00
Predicted education (<HS omitted)									
HS	-8.24 *	1.65	-8.54 *	-8.30 *	1.57	-8.51 *	-8.31	1.39	-8.80 *
Some college	-9.03 *	-1.45	-4.43	-9.11 *	-1.55	-4.38	-9.06	-1.87	-4.67
College	-16.97 *	-8.10	-5.66	-17.01 *	-8.21	-5.62	-16.60	-8.72	-5.83
HS X age	0.60 *	-0.18	0.58 *	0.60 *	-0.18	0.57 *	0.60	-0.16	0.60 *
HS X age-sq	-0.01 *	0.00	-0.01 *	-0.01 *	0.00	-0.01 *	-0.01 *	0.00	-0.01 *
Some college X age	0.60 *	0.01	0.23	0.60 *	0.02	0.23	0.60	0.04	0.26
Some college X age-sq	-0.01 *	0.00	0.00	-0.01 *	0.00	0.00	-0.01 *	0.00	0.00
College X age	1.06 *	0.52	0.20	1.06 *	0.53	0.20	1.03	0.57	0.23
College X age-sq	-0.02 *	-0.01	0.00	-0.02 *	-0.01	0.00	-0.02 *	-0.01	0.00
Currently enrolled in school	-0.26 *	-0.27	-0.01	-0.26 *	-0.26	-0.01	-0.25	-0.29 *	-0.06
Childbearing desires				0.06 *	0.05 *	-0.03	0.06 *	0.05 *	-0.03
Ln of current wage							0.19	-0.25 *	-0.16
Currently employed							-0.04	-0.10	-0.26 *
Imputation flag	0.18	-0.08	-0.11	0.18	-0.07	-0.11	0.32	-0.02	0.04
Intercept	-1.95	-3.98	-2.19	-2.01	-4.01	-2.16	-2.47	-3.52	-2.04

Notes: An imputation flag is included for person-years in which any time-varying variable is filled in with data from the previous year.
** P<.05*

Table 1. Observed Completed Fertility by Education

	All	<HS	HS	Some College	College	Ratio of <HS to College
Whites						
Intended Births	1.36	1.38	1.36	1.35	1.34	**1.03**
Mistimed Births	0.42	0.56	0.55	0.43	0.23	**2.39**
Unwanted Births	0.08	0.13	0.10	0.07	0.04	**3.30**
Total Births	1.85	2.06	2.01	1.86	1.61	**1.28**
N	2,029	123	819	490	597	
Blacks						
Intended Births	0.94	1.27	0.96	0.86	0.85	**1.49**
Mistimed Births	0.83	1.15	0.93	0.78	0.48	**2.39**
Unwanted Births	0.40	0.84	0.41	0.39	0.14	**6.24**
Total Births	2.18	3.26	2.30	2.04	1.47	**2.22**
N	1,197	116	504	391	186	

Notes: Means are weighted and *N*'s are unweighted. Sample includes women at last interview, in 2004, ages 39-46. Missing data on birth intention status is imputed based on the observed proportion of intended, mistimed, and unwanted by education and race.

Table 2. Predicted Completed Fertility by Education

	<HS	HS	Some College	College	Ratio of <HS to College
Whites					
Model 1					
Intended Births	1.07	1.24	1.15	1.25	**0.86**
Mistimed Births	0.62	0.46	0.39	0.21	**3.02**
Unwanted Births	0.19	0.08	0.07	0.03	**6.68**
Total Births	1.88	1.78	1.60	1.48	**1.27**
Model 2: Model 1 + desires					
Intended Births	1.08	1.24	1.14	1.24	**0.87**
Mistimed Births	0.63	0.46	0.39	0.20	**3.07**
Unwanted Births	0.18	0.08	0.07	0.03	**6.52**
Total Births	1.89	1.78	1.59	1.47	**1.29**
Model 3: Model 2 + wage					
Intended Births	1.12	1.28	1.14	1.17	**0.96**
Mistimed Births	0.55	0.43	0.38	0.21	**2.55**
Unwanted Births	0.16	0.08	0.07	0.03	**5.43**
Total Births	1.83	1.78	1.58	1.41	**1.29**
Blacks					
Model 1					
Intended Births	1.03	0.76	0.73	0.76	**1.36**
Mistimed Births	0.80	0.85	0.69	0.47	**1.69**
Unwanted Births	0.56	0.29	0.34	0.08	**7.33**
Total Births	2.39	1.90	1.75	1.31	**1.83**
Model 2: Model 1 + desires					
Intended Births	1.05	0.77	0.72	0.74	**1.42**
Mistimed Births	0.82	0.86	0.68	0.47	**1.75**
Unwanted Births	0.55	0.29	0.34	0.08	**7.15**
Total Births	2.42	1.92	1.74	1.28	**1.88**
Model 3: Model 2 + wage					
Intended Births	1.10	0.80	0.73	0.71	**1.56**
Mistimed Births	0.74	0.80	0.66	0.48	**1.54**
Unwanted Births	0.48	0.28	0.34	0.08	**5.77**
Total Births	2.32	1.88	1.74	1.27	**1.83**

Notes: Predictions are derived from (weighted) multinomial logistic regression models. Total fertility is estimated as the sum of age-specific predicted birth probabilities varying education while holding all other predictors at their weighted mean values. Model 1 includes age, age-squared, age by predicted education interactions, and school enrollment; Model 2 adds childbearing desires; Model 3 adds ln of current wage and employment status.

Table 3. Predicted Completed Fertility by Wages Under Alternative Model Specifications

	Intended	Mistimed	Unwanted
Whites			
Model 3. Model 2 + current wage			
25th pctile wage ($7.85)	1.22	0.44	0.08
75th pctile wage ($14.30)	1.36	0.41	0.08
Ratio of 25th to 75th pctile	0.90	1.07	1.05
Model 4. Model 3 + spouse income			
25th pctile wage ($7.85)	1.26	0.46	0.08
75th pctile wage ($14.30)	1.43	0.44	0.08
Ratio of 25th to 75th pctile	0.88	1.06	1.05
Model 5. Model 2 + average past wage			
25th pctile wage ($6.11)	1.22	0.44	0.08
75th pctile wage ($10.70)	1.35	0.44	0.07
Ratio of 25th to 75th pctile	0.91	1.01	1.09
Model 6. Model 5 + spouse income			
25th pctile wage ($6.11)	1.27	0.47	0.08
75th pctile wage ($10.70)	1.40	0.47	0.07
Ratio of 25th to 75th pctile	0.90	1.00	1.10
Blacks			
Model 3. Model 2 + current wage			
25th pctile wage ($6.88)	0.77	0.85	0.29
75th pctile wage ($11.36)	0.84	0.76	0.27
Ratio of 25th to 75th pctile	0.91	1.12	1.08
Model 4. Model 3 + spouse income			
25th pctile wage ($6.88)	0.79	0.85	0.27
75th pctile wage ($11.36)	0.86	0.75	0.26
Ratio of 25th to 75th pctile	0.92	1.12	1.07
Model 5. Model 2 + average past wage			
25th pctile wage ($5.90)	0.77	0.88	0.27
75th pctile wage ($8.94)	0.80	0.84	0.27
Ratio of 25th to 75th pctile	0.97	1.04	1.00
Model 6. Model 5 + spouse income			
25th pctile wage ($5.90)	0.79	0.87	0.26
75th pctile wage ($8.94)	0.81	0.84	0.26
Ratio of 25th to 75th pctile	0.98	1.05	0.99

Notes: Predictions are derived from (weighted) multinomial logistic regression models. Total fertility is estimated as the sum of age-specific predicted birth probabilities varying wages while holding education at the level of high school graduation and all other predictors at their weighted mean values. Model 2 includes age, age-squared, age by predicted education interactions, school enrollment, and childbearing desires; Model 3 adds ln of current wage and employment status; Model 4 adds marital status and ln of spouse income. Starting again with Model 2, Model 5 adds ln of average past wage, proportion years employed, and whether never employed; Model 6 adds marital status and ln of spouse income.