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Season of Birth and Later Outcomes: Old Questions, New Answers

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Abstract

Season of birth is associated with later outcomes; what drives this association remains unclear. We consider a new explanation: variation in maternal characteristics. We document large changes in maternal characteristics for births throughout the year; winter births are disproportionally realized by teenagers and the unmarried. Family background controls explain nearly half of season-of-birth's relation to adult outcomes. Seasonality in maternal characteristics is driven by women trying to conceive; we find no seasonality among unwanted births. Prior seasonality-in-fertility research focuses on conditions at conception; here expected conditions at birth drive variation in maternal characteristics while conditions at conception are unimportant.

Research across the social and natural sciences has consistently found that the month of a child's birth is associated with later outcomes involving health, educational attainment, earnings and mortality. Much of this work shows that on average individuals born in the winter have worse outcomes (less schooling, lower wages) than other individuals. What drives this association remains unclear. Some prior work has speculated that this association may be driven by social and natural factors (such as compulsory schooling laws, changes in temperature, or exposure to illness) that could affect children born in the winter in particular ways, but there is no consensus about the importance of these or other explanations.

Moreover, most work has explicitly dismissed the possibility that seasonality in outcomes might reflect inherent differences in personal attributes or family background. For example, Hoogerheide et al. (2007) write, "one's birthday is unlikely to be correlated with personal attributes other than age at school entry"; and in a survey on the returns to schooling literature, Card (1999) concludes that relationships between wages, education, and season of birth "are probably not caused by differences in family background." These claims are often made or implicitly relied upon in the large body of work using season of birth as an instrumental variable.[i]

Yet despite assertions among researchers that family background is unrelated to season of birth, we know of no rigorous investigation of the relation between season of birth and family background. In this paper we undertake such an investigation. Using data from live birth certificates and the census, we first see whether the typical woman giving birth in the winter looks different from the typical woman giving birth at other times of year. We find that women giving birth in the winter look different from other women: they are younger,

[i]Studies using season of birth as an instrumental variable include Angrist and Krueger (1991, 1992, 1995, 2001); Levin and Plug (1999); Plug (2001); Adams (2002); Gelbach (2002); Lemke and Rischall (2003); Hansen, Heckman, and Mullen (2004); Skirbekk, Kohler, and Prskawetz (2004); Lefgren and McIntyre (2006); Andini (2008, 2010); Leigh and Ryan (2008); Angrist and Pischke (2009); Maurin and Moschion (2009); Arkes (2010); Lee and Orazem (2010); and Robertsen (2011).

less educated, and less likely to be married. These differences are large. For example, we document a 10 percent decline in the fraction of children born to teenagers from January to May; this effect, observed every spring, is about as large as the decline in the annual fraction of children born to teenagers observed over the entire 1990s.

We then see whether variation in family background characteristics can account for much of the difference in outcomes typically ascribed to season of birth. Our estimates from census data suggest that a parsimonious set of family background controls can significantly reduce estimated differences in education and earnings between people born in different quarters of the year. Our controls generally reduce the magnitude of the season of birth effect by 25 to 50 percent. Thus the well-known relationship between season of birth and later outcomes is largely driven by differences in fertility patterns across socioeconomic groups, and not merely natural phenomena or schooling laws that intervene after conception. The fact that family background characteristics have strong relations with both season of birth and later outcomes indicates that season of birth will likely fail the exclusion restriction in most instrumental-variables (IV) settings where it has been used. These findings build on past work critiquing the validity of season-of-birth as an instrument, such as Bound, Jaeger, and Baker (1995).[ii] However, past work on the validity of this instrument has focused primarily on the instrument being "weak," and as mentioned above many researchers continue to argue that season of birth satisfies relevant exclusion restrictions.[iii] The findings here pose a potentially fatal challenge to such arguments.

Next, we consider why these seasonal patterns exist. We begin by noting that seasonal factors could affect conceptions both among women trying to conceive and among women who are not trying to conceive. For instance, if high-socioeconomic status (SES) women trying to conceive have stronger preferences for non-winter births or are better at timing births away from winter, this could explain the patterns we see. Alternately, work has shown that weather can affect sexual activity. If changes in weather affect "risky" sexual behavior, and if such effects vary over SES groups, this could also drive these patterns.

Using data from the National Survey of Family Growth (NSFG) we show that seasonal maternal patterns are driven by women wanting a birth; there is no evidence of seasonality in maternal characteristics among unwanted births. In addition to helping explain seasonality in maternal characteristics, this result has a number of other important implications; for example it indicates there is seasonal variation in the wantedness of births within SES and that alternate explanations relating season of birth to later outcomes (such as schooling laws and nutrition) may be even less important than our findings using census data would suggest. That one's birth date is in part the result of a choice made by one's parents also indicates that IV regressions on quarter of birth would likely be problematic even if strong family controls were available.

Furthermore, most prior work discussing seasonality in birth has focused on conditions at conception (such as weather) as key explanatory controls. The fact that our patterns are driven by women wanting a birth indicates that conditions at the anticipated time of birth may play an important role in explaining seasonality in fertility outcomes. We show that controlling for county fixed effects, weather at conception, and expected weather at birth leads to a 50-to-70 percent reduction in seasonal maternal patterns. Surprisingly, conditions at conception have almost no explanatory power here. Instead, controls for expected weather

[ii]Some other work has questioned whether using IV based on season of birth—or even using discontinuity-based methods exploiting exact school entry dates—can provide identification in a returns to education setting; examples include Bound and Jaeger (2000), Cascio and Lewis (2006), and Dobkin and Ferreira (2010).

[iii]We note that almost all of the instrumental-variables research mentioned in this introduction postdates Bound, Jaeger, and Baker (1995).

at birth are the driving force behind this reduction; for many months of the year expected conditions at birth account for essentially all of the observed reduction in the maternal pattern. This indicates that future work on fertility should consider expected conditions at birth, and not just conditions at conception, as a possible determinant of seasonal patterns.

The remainder of the paper is organized as follows. Section I provides evidence relating season of birth and maternal characteristics. Section II considers how this relationship might account for season of birth's impact on later outcomes. Section III explores causes for seasonality in maternal characteristics. Section IV concludes.

I. Season of Birth and Mother's Characteristics

A. Natality Detail Files

In this section we document clear within-year patterns in the characteristics of women giving birth that are persistent throughout the second half of the twentieth century. We first use the Center for Disease Control's Natality Detail Files from 1989 to 2001, which contain data from all live birth certificates in the United States in each year. Below, we perform a similar analysis using decennial census data for 1960, 1970, and 1980, representing births between 1943 and 1980.

In addition to the infant's month of birth, the Natality Detail Files provide information on a number of maternal characteristics, including marital status, age, race, and education. As of 1985, all states report 100% of their birth certificate data, representing over 99% of all births in the United States. We choose 1989 as a starting year because the standard birth certificate was substantially revised in this year. Marital status is first reported directly in 1989, though six states still impute marital status in this year. Only Michigan and New York still impute marital status in 2000, where a woman is considered to be unmarried if paternity acknowledgement was received or the father's name is missing. In 1989, 8.9% of birth certificates do not report mother's education; this number decreases to 1.4% by 2000.

Figure 1 depicts trends in the characteristics of mothers from month to month, for 1989 to 2001. There are approximately 52 million total births used in each picture. Panel A shows the percent of women giving birth each month during this period who are teenagers. Panel B shows the percent of mothers giving birth who are married, Panel C shows the percent of women giving birth who are white, and Panel D shows the percent with a high school degree (defined as twelve or more years of education). All the panels depict a clear seasonal pattern that is highly persistent across years. Children born in the winter are less likely to be born to a married mother and more likely to be born to a mother who is a teenager, who is not white, or who lacks a high school degree.

These seasonal trends are strikingly large. For instance, Panel A shows that the percent of women who are teenagers decreases by about one percentage point between May and January, about a 10 percent effect. By comparison, this is roughly equal to the decline in the *annual* percent of births to teenagers that occurred during the 1990s, which was driven by much-noted declines in the teen birth rate (Ventura, Curtin, and Mathews, 2000; Arias et al., 2003). The increase in percent unmarried between May and January seen in Panel B is about two percentage points on average, which is roughly the same size as the increase in nonmarital childbearing from a one standard deviation increase in monthly welfare benefits in Rosenzweig (1999). In Panels C and D, we see that the percent of mothers who are white or who have a high school degree is about two percentage points higher in May than in January. These magnitudes are 25 and ten times larger, respectively, than those associated with a one-percentage-point increase in the unemployment rate estimated by Dehejia and Lleras-Muney (2004).

To assess the magnitudes of the seasonal trends we collapse the data into county-of-birth/month-of-birth/year-of-birth cells.[iv] Using cell c as the unit of observation we estimate

$$Outcome_c = \alpha + \text{month}^* \beta + \theta_y + \varepsilon_c \quad (1)$$

where $Outcome_c$ is the fraction of children in the cell born to (a) married mothers (b) white mothers (c) mothers with a high-school degree or (d) teenage mothers. The term “month” in equation (1) represents a set of 11 dummy variables for month of birth (with January omitted). The term θ_y represents a third-order polynomial for birth-month trends, which is included to capture broad trends in the dependent variable occurring over this time. The term ε_c is noise. Regressions are weighted by cell size and robust standard errors are reported in brackets.

The estimates can be seen in Table 1. Not surprisingly, the set of month dummies is highly significant in all regressions. For each of the four outcomes, January is the month with the lowest maternal SES, and the peak is in May.

The Natality Detail Files also include information on measures of health outcomes such as birth weight and gestation. It will be useful to examine these measures as they are strongly related both to family background (cf. Forssas et al., 1999; Thorngren-Jerneck and Herbst, 2001) and to later outcomes linked to season of birth (Behrman and Rosenzweig, 2004; Case, Fertig, and Paxson, 2005; Black, Devereux, and Salvanes, 2007; Currie, 2009). Therefore Table 1 also presents month dummy variables from regressions on birth weight, fraction low-birth-weight births, and fraction born premature, using the same specification as in equation (1). The results show that children born in December and January have lower average birth weights than other children; the highest average birth weights are in the spring. Infants born in April weigh 23.3 grams more on average than those born in January; this effect is three-fourths the size of the effect of AFDC participation on poor whites estimated by Currie and Cole (1993) and is larger than their estimated effect for blacks. The results for low-birth-weight and for prematurity also show seasonality; early spring and late summer births are less likely to be low-birth-weight or premature. The differences are statistically and economically significant. Thus, the data show seasonal variation in child health outcomes in addition to variation in maternal characteristics.

B. Decennial Census

We now conduct a similar exercise using the 1960, 1970, and 1980 decennial censuses, which will allow us to verify how persistent the relationship between season-of-birth and family background is over time. The analysis is also pertinent since census data will be used in the following section. The results are in Table 2. The regressions are analogous to equation (1) except that month of birth has been replaced by quarter of birth, which is the birth date measure in the most recent usable censuses. The omitted quarter is the first quarter of the year.[v] These results using births from 1943 to 1980 are similar to those from the 1989–2001 Natality Detail Files discussed above. Children born in the second through fourth quarters of the year are more likely to have a mother who has a high school degree, is married, is white, and who does not live in poverty. For marital status and race, the patterns get stronger over time. For comparison with the Natality Detail Files for 1989–2001, in the last column we estimate a birth-quarter version of equation (1) for the birth certificate data;

[iv]The data are collapsed for computational tractability. Estimation at the individual level produces nearly identical results.

[v]We use IPUMS data from 1960 (1% sample), 1970 (the 1% Form 1 and 1% Form 2 state, metro, and neighborhood samples) and 1980 (5% sample). In each census year, the unit of observation is the child and our sample consists of children ages 16 and under living with their biological mothers. For each outcome, the regressions for each census year are run separately.

the magnitudes are quite similar. The results suggest that the use of quarterly-level data imposed by the Census masks significant within-quarter variation. In the next section we consider how the relationship between season of birth and family background documented in Tables 1 and 2 might account for season-of-birth's impact on later outcomes, and discuss the implications of our finding for past work using quarter of birth as an instrumental variable.

II. Implications for Later Outcomes

The striking patterns of seasonal birth characteristics are important in their own right, but they also may have implications for past work on seasonality of birth and later outcomes. Economists have long recognized that the month of a child's birth is associated with outcomes such as test performance, wages, and educational attainment.[vi] These studies overwhelmingly show that children born in the winter months (or in the first quarter of the year) have relatively low educational attainment, wages, and intellectual ability. Similarly, a large body of research outside of economics has proven that season of birth is associated with health outcomes including schizophrenia, autism, dyslexia, extreme shyness, risk for suicide, and life expectancy among the elderly (Tochigi et al., 2004; Gillberg, 1990; Livingston et al., 1993; Gortmaker et al., 1997; Rock et al., 2006; Doblhammer et al., 2005). Research has even suggested an association between season of birth and self-reported "luckiness" (Chotai and Wiseman, 2005) and season of birth and the likelihood of being left-handed (Martin and Jones, 1999).[vii]

It remains unclear why these seasonal relationships exist. Prior explanations involve social and natural phenomena that intervene after conception or birth to create differences in outcomes. This type of explanation was notably considered by Angrist and Krueger (1991), who posit that compulsory schooling laws intervene to create different outcomes for children. Since children born in the winter are likely to be older when they begin school, they will have attained less schooling on average than other children when they reach an age where they can legally drop out. Other explanations for why winter births have worse outcomes include differences in relative ages when starting school (Tarnowski et al., 1990; Plug, 2001) or in-utero exposure to weather (Gortmaker et al., 1997) or illness (Sham et al., 1992; Almond, 2006). The "fetal origins hypothesis" (Barker, 2001) contends that nutrient deprivation at various stages of fetal development may be linked to adult diseases; if nutritional intake is seasonal, this could explain seasonal variation in health outcomes.

We hypothesize that seasonal variation in outcomes may be driven by the fact that children born in different seasons are not initially similar but rather are conceived by different groups of women. It is certainly possible that this hypothesis would be a complement, rather than a substitute, to existing explanations of season of birth's impact on outcomes. We think that intervening phenomena such as schooling laws and exposure to influenza might help explain season of birth's association with later outcomes. But we know of no research using recent U.S. data which rigorously investigates whether differences in family background for children born at different times of year can explain seasonality in outcomes.[viii]

[vi]Examples include Angrist and Krueger (1991 and 1992); Bound, Jaeger, and Baker (1995); Staiger and Stock (1997); Bound and Jaeger (2000); Plug (2001); Chamberlain and Imbens (2004); Honoré and Hu (2004); Cruz and Moreira (2005); Cascio and Lewis (2006); Chernozhukov and Hansen (2006); Chesher (2007); Dufour and Taamouti (2007); Hoogerheide, Kleibergen, and van Dijk (2007).

[vii]Many (but not all) of these studies find that children born in winter months have worse outcomes than other children. Some of these studies are international in focus; as in most prior work, our focus is on the U.S. case. In the conclusion we briefly discuss implications of our work for international research.

We use the decennial census for this investigation. In addition to quarter of birth information, the census has information on outcomes such as completed schooling and earnings. However, for our study we need both information on outcomes and information on family background. Family background information is available for individuals living at home with their parents when the census is completed, but most such individuals are children for whom the outcomes of interest are not available. For most adults in the census information on family background is limited.

To confront this problem, we combine information on cells of individuals across multiple census years; in the census data we define cells by state of birth, year of birth, and quarter of birth. From the 1960 census (the earliest census usable for this investigation since quarter-of-birth information is not readily available for the 1920–1950 censuses), we gather information on average family background characteristics for cells of individuals ages 16 and under living with their biological mothers.[ix] From the 1980 census (the latest available year), we take information on average outcomes for each cell. We then match the cell's family background information to the cell's outcomes; this approach is similar in spirit to that of Angrist and Krueger (1992).[x] Using cohorts of individuals ages 16 and under as of 1960 allows us to accurately measure family characteristics, but there may be a concern that 1980 wage information for the younger individuals in these cohorts will not be an accurate reflection of lifetime earnings. Consequently, we restrict the sample to individuals who are ages 25 to 36 when observed in 1980, omitting those ages 20 to 24 (that is, those aged four and under in 1960). Similar results are obtained when using all children 16 and under in the 1960 census, however.[xi]

Using census data from 1960 (1% IPUMS sample) and 1980 (5% IPUMS sample), we estimate

$$Outcome_c=\alpha_1+Q\beta_1+\varphi_s\gamma_1+Y\theta_1+age\lambda_1+age^2\rho_1+\varepsilon_1 \quad (2)$$

and

$$Outcome_c=\alpha_2+Q\beta_2+X_c\delta+\varphi_s\gamma_2+Y\theta_2+age\lambda_2+age^2\rho_2+\varepsilon_2 \quad (3)$$

where the dependent variable $Outcome_c$ is either (a) the average years of school obtained by individuals in cell c (b) the percent of individuals in c without a high-school degree or (c) the average of log wages for cell c. The term Q represents a set of quarter-of-birth dummies (with one quarter omitted), ϕ_s is a set of state-of-birth dummies, Y is a set of year dummies, and age and age^2 are linear and quadratic controls for age (measured in birth quarters). The numerical subscripts index the coefficients and error terms in the two equations. Regressions are weighted by cell size.[xii]

[viii]There is a small and inconclusive body of research which uses selected subsamples of the U.S. population or international data to consider whether seasonality of conception differs for certain women, but none of this work considers later outcomes. Examples include Pasamanick et al. (1960); Warren and Tyler (1979); Kesterbaum (1987); Seiver (1989); Lam, Miron, and Riley (1994); James (1971); Bobak and Gjonca (2001); and Mitchell et al. (1985).

[ix]Over 95% of all children in the 1960 census live with their biological mother.

[x]One may wonder whether the use of aggregated data will affect this analysis. The facts that seasonal variation in maternal background is similar both within and across time and place and that our OLS results on aggregate data resemble results on individual-level data suggest that aggregation will not significantly impact the analysis. However, if our family background controls are proxies for other relevant controls (such as ability), and if the covariance between our controls and the omitted controls is weaker at the cohort level than at the individual level, it is possible our approach understates the ability of family background to explain seasonality in outcomes. For related work on aggregation bias, see Geronimus, Bound, and Neidert (1996), Dickens and Ross (1984), and especially Hanusheck, Rivkin, and Taylor (1996).

[xi]See Buckles and Hungerman (2008) for these results.

The difference between (2) and (3) is that the latter includes the matrix X_c which contains controls for family background characteristics. These family-background controls include cell averages for mother's education, mother's age at birth, and family income as a percent of the poverty line, the fraction white, and the fraction of mothers in each cell who are teenagers, who are working, who are married, and who are without a high school degree. Maternal controls are measures for c as of 1960 and family income is for 1959 .[xiii]

For both equations (2) and (3), the coefficients for the quarter-of-birth dummies report the difference in the likelihood of a given outcome occurring for a child born in each quarter relative to the omitted quarter. We can test whether background characteristics drive these seasonal relationships by comparing the quarter-of-birth coefficients in (2) and (3). There are two conditions under which adding controls for family characteristics would not change the estimates of the quarter-of-birth coefficients β: if family characteristics are orthogonal to quarter of birth, or if they have no direct impact on the outcomes (that is, the δ coefficients in equation (3) are zero). If neither condition is satisfied, excluding maternal characteristics will lead to inconsistent estimates of β_1 Alternatively, if these conditions are met, then in equation (2). equation (2) is correctly specified and estimates of (2) will be not only consistent but will also be efficient, since they would exclude the superfluous variables added into equation (3). A Hausman test can thus be performed to test the null hypothesis that $\beta_1 = \beta_2$.

A drawback of the traditional Hausman test is that it imposes that the covariance between the coefficients in the two models is zero. A more general version of the Hausman-style test can be conducted by "stacking" the census data on top of itself and estimating both equations (2) and (3) simultaneously using Seemingly Unrelated Regression estimation. This allows for a more robust estimation of a variance-covariance matrix between coefficients in the two models; based on this variance-covariance matrix, it is straightforward to test whether the quarter-of-birth coefficients from the two models are the same.

Results from estimating (2) and (3) are shown in Table 3. In the first pair of columns, the outcome of interest is years of completed schooling. The first column shows that, as expected, children born in the second through fourth quarters of the year obtain more school on average than other children; these results are similar in magnitude to those shown in Angrist and Krueger (1991).[xiv] However, column 2 shows that these effects are made significantly smaller by adding controls for family characteristics; the decline in the estimates ranges from 25 percent to 40 percent. A Wald test rejects that the coefficients are the same in each column.[xv]

The next two columns look at the fraction of men in a cell who have not completed high school. The first set of results is again similar in magnitude to estimates from past work and suggests that those born in the first quarter of the year are more likely to drop out.

[xii]Cell size is taken from the 1980 census. The correlation between cell sizes in the two census years is over 0.99 and using either year to weight the data gives similar estimates. The education regressions weight by total individuals in a cell; the wage regressions weight by total individuals reporting positive earnings in a cell. The regressions have 2,596 cells; for the wage regressions there are 927,954 individuals and for the education regressions there are 1,090,826 individuals.

[xiii]We have also considered adding more flexible controls for family background. Adding in interactions and logged values of the family controls modestly increases the effect of the controls on the birth-quarter coefficients, especially for wage regressions.

[xiv]See the second line of Table 1 in their paper for the most comparable regression (although note they exclude the fourth-quarter dummy).

[xv]A referee noted that these estimates may suffer from finite sampling error (cf. Deaton, 1985), which would likely bias the quarter-of-birth coefficients away from zero after background controls are added. We explored a corrective procedure for this problem proposed in Devereux (2007) and based on a program authored by Aliaksandr Amialchuk. Controlling for sampling bias typically generated point estimates that were similar but often even closer to zero for the quarter-of-birth coefficients compared to those in Table 3. However, the results were often imprecise and Wald tests would typically fail to reject that bias-corrected estimates were different from any of the relevant Table 3 estimates.

Controlling for family background again significantly reduces these estimates for all three quarter-of-birth dummies; the changes are economically and statistically significant. The last two columns look at logged wages. The results are comparable to the estimates in Angrist and Krueger (1991), finding about a 1-percent difference in wages for those born in the first quarter to others. Again, adding family background controls significantly weakens the magnitude of this effect. In all cases the null hypothesis that $\beta_1 = \beta_2$ can be rejected at the one -percent level.

It is interesting to note that, while the magnitude of the effect is much smaller, season of birth is sometimes still predictive even after family background controls are included, especially in later quarters. The persistence and magnitude of seasonality in later quarters may be partly driven by our use of cohort-level data and the parsimonious set of family-background characteristics available from the census. This persistence is also likely driven by the various other explanatory phenomena put forward by past work, including compulsory schooling laws. But clearly variation in family background plays a crucial role in explaining differences in outcomes for those born at different times of year.[xvi]

One important implication of the results in this section concerns the use of season of birth to instrument for schooling in a returns-to-education setting. This depends upon season of birth satisfying an exclusion restriction requiring that season of birth affects earnings only through its effect on education. The fact that family background characteristics have strong relations with both season of birth and later outcomes (including education and earnings) indicates that season of birth will likely fail this exclusion restriction.[xvii]

III. Explaining Seasonality in Maternal Characteristics

In the previous sections, we have documented a substantial but not well-known pattern in maternal characteristics that goes significantly beyond past critiques of season-of-birth towards explaining *why* quarter of birth is related to later outcomes. One might wonder why these striking patterns in maternal characteristics exist. As a starting point, Figure 2 shows the mean residuals each month from regressions of logged births per day for (a) married women and (b) single women.[xviii] The regressions, based on the Natality Detail Files from 1989–2001, include a third-order polynomial trend in months. To better capture seasonal variation in conceptions, we have estimated the month of conception using gestational age (in weeks) and then imputed month of birth assuming a 40 week gestation. Thus the upper row of month labels indicate the expected month of birth at conception; the lower row of month labels in parentheses are the typical month of conception for a given month of birth.

There are two noticeable features in Figure 2. The first is the drop in births to single women between February and June, and the second is the decline in births to married women in the winter (December/January). Together, these create the large differences in the average characteristics of mothers giving birth in the first and second quarter seen earlier.[xix]

[xvi]We have also explored the extent to which the seasonal variation in infant health is driven by variation in maternal SES in the Natality Detail Files. Similar to Table 3, the month coefficients for birth weight (for example) fall by 21 to 52% when controls for maternal education, marital status, age, and race are added (and the results are similar regardless of whether aggregated or individual-level data are used).

[xvii]Angrist and Krueger argue for the validity of their approach by noting that seasonal patterns are smaller for college graduation and insignificant and wrong-signed for Masters and PhDs. Such arguments do not gainsay the damaging implications of Figure 1 in this paper. Additionally, their college patterns actually align well with the family background patterns shown here. Further, their post-college outcomes involve a small minority of the population (as little as three percent) and it is entirely possible the patterns we find are driven by those outside of this select group (especially since our patterns are stronger for more recent years). We discuss the role of different SES groups in driving our patterns more in Section III.

xviii: For what follows we have also considered other measures of SES. Such results are generally similar to those shown here and so we focus on married versus single births for ease of exposition. That single mothers have lower SES than other mothers is well known; see for instance the comparison of single mothers to married mothers in Meyer and Sullivan (2003).

Why might high-SES women have fewer births in winter and more in the spring? We first note that seasonal factors could affect conceptions both among women who are and are not trying to conceive. For instance, if high-SES women trying to conceive have stronger preferences for non-winter births or are better at timing births away from winter, this could explain the patterns we see. Alternately, work has shown that seasonal phenomena such as weather can affect sexual activity (some of this work is summarized in Macdowall et al., 2008). If changes in weather affect "risky" sexual behavior, and if such effects vary over SES groups, this could also drive these patterns.

We investigate whether the seasonality we document is driven by wanted or unwanted births using National Survey of Family Growth (NSFG) data from 1988, 1995, and 2002. The NSFG is a nationally representative survey of women 15 to 44 years of age, with complete pregnancy histories for each woman surveyed. We observe the month of birth for each pregnancy and the marital status of the mother at the time of birth. Women are also asked whether they wanted the pregnancy. There are 35,792 pregnancies ending in a live birth in the data; each such pregnancy will be a unit of observation in this analysis.

To investigate whether our patterns are driven by wanted or unwanted births, we estimate

$$married = \alpha + \mathrm{month}^{*} want^{*} \beta + \mathrm{month}^{*} notwant^{*} \delta + want^{*} \gamma + \theta_y + \varepsilon, \quad (4)$$

where *married* is a dummy variable for whether a child's mother is married, the vector "month" is a set of 11 month-of-birth dummies (with January as the omitted month), the dummy variable *want* equals unity if a birth is reported as wanted, and the variable *not want* is a dummy that equals unity if a birth is reported as not wanted.[xx] Wantedness is determined in response to the question, "Right before you became pregnant, did you yourself want to have a baby at any time in the future?" The birth is recorded as unwanted if the response is "unwanted," "didn't care/indifferent," or "don't know/not sure." About 87% of births are reported as "wanted" by this definition (and thus there are over 4,500 unwanted births); 56% of unwanted births are to married women. The term θ_y includes a third-order monthly time trend and dummies for interview year.

Table 4 reports marginal effects from Probit regression estimates of equation (4). (Linear probability estimates are similar.) The first column reports a regression using a single set of month dummies for all births, omitting the dummies for wantedness and their interactions with the month dummies. The coefficients depicted are similar to the monthly patterns documented in Table 1, with January having fewer births to married women than other months and the peak in married months coming in late spring and early summer.

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[xix]We have also considered whether the patterns seen reflect differential patterns in conception outcomes besides live birth, such as ectopic pregnancy or abortion. Exploring these factors is made difficult by "inadequacies in the reporting of all end products of conception" and "the difficulty in estimating the precise time when conception occurs" (Petersen and Alexander, 1992). However, Warren, Gwinn, and Rubin (1986) find no significant seasonal pattern in induced or spontaneous abortions or in ectopic pregnancies once seasonality in conceptions is controlled for. Additionally, Parnell and Rodgers (1998) state that "it is clearly not the case that abortion patterns contribute to the birth seasonality" and Stupp and Warren (1994) conclude that "seasonality of each pregnancy outcome can best be understood by understanding the seasonality of conception for all pregnancies." Further, Petersen and Alexander (1992) find little variation in the percent of adolescent pregnancies conceived over the year which end in induced abortion, except for a decline in this percent for conceptions in early autumn. But even if such a decline were particular to adolescents, it would likely work against the seasonal patterns we find here; Parnell and Rodgers (1998) also argue that abortion use may actually lead to underestimates of the importance of seasonality inferred from studying live births. This suggests that while other pregnancy outcomes may play some role in our results, given data limitations it is reasonable to focus on live births.

[xx]Using other measures of SES in these regressions instead of marital status yields frequently similar but occasionally less precise results.

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Columns 2 and 3 report the results from estimating equation (4)—thus both columns are from a single regression (the coefficient for the uninteracted wantedness dummy is given below the table). Clearly, the seasonal pattern in births is driven by wanted births; the coefficients here are larger and more statistically significant than the estimates in column 1. Column 3 shows that the coefficients among unwanted births are all insignificant and in fact most of them are wrong-signed. A test that the coefficients in column 2 equal those in column 3 is rejected, with a *p*-value of 0.036. Seasonality here appears to be driven by wanted births; there is no evidence of seasonality among unwanted births.

Although we observe over 4,500 unwanted births, one might be concerned that the insignificant coefficients in column 3 are driven by small sample size. To address this concern, in the last three columns of Table 4 we repeat our two regressions, but group months into "month pairs" using a single dummy to identify births in March and April, and so on (January and February are the two omitted months). The results are similar to before: again, the seasonal pattern is clearly found among wanted births and clearly absent among unwanted births. A test that the coefficients in column 5 equal those in column 6 is again rejected ($p = 0.002$).[xxi]

Beyond helping to explain the patterns in our paper, there are at least four noteworthy implications of this finding. First, this result is compatible with a story where women time births for certain seasons, and thus may help to explain the fact that our seasonality results sometimes appear stronger in more recent years than they do in the 1950s and 1960s, when women's ability to use contraception to control fertility was more limited.[xxii] Second, this result indicates there is seasonal variation in the wantedness of births within SES.[xxiii] As child wantedness may itself impact later outcomes, the patterns documented here pose a severe problem for research using season of birth as a source of exogenous variation even if strong family controls are available. Third, seasonality in wantedness is a potentially important new factor when considering the relationship between season of birth and later outcomes. Our work in Section II shows that family controls can explain up to half of the relationship between season of birth and outcomes; the fact that variation in wantedness *within SES* may play a role suggests that other explanations (like schooling laws and nutrition) may be even less important than the results in Section II indicate.

Fourth, most prior work discussing seasonality in birth has focused on conditions at conception (such as weather) as explanatory controls.[xxiv] But if seasonality in maternal characteristics is driven by wanted or planned births, then expected conditions at the *anticipated time of birth* may play a key role in explaining seasonal patterns in maternal characteristics. To consider this possibility, we investigate whether the coefficients in Table 1 are significantly affected when we add controls for weather at both the estimated time of conception and at the expected time of birth.

[xxi]We have also redone the estimates in Table 4 using an alternative definition of wantedness where a birth is classified as wanted if the mother was not contracepting at the time of conception *and* her stated reason is that she wanted to become pregnant. All other births—about 12,000 births or a third of the data—are defined as not wanted. Results using this definition are the same as before, showing that the patterns in maternal characteristics are not only driven by women who describe their births as "wanted" but specifically by women who are actively trying to conceive.

[xxii]This may also help explain why Card (1999) fails to find seasonal variation in maternal education in the 1940 census.

xxiii: To see this, suppose instead that the fraction of wanted births was constant throughout the year for each SES group. Then an increase in the fraction of births to high-SES women would be driven by a relative increase in total births to high-SES women—which, by assumption, would necessarily include a relative increase in *both* wanted and unwanted births to high SES women. Yet Table 4 only documents a relative increase in wanted births.

[xxiv]For example, Lam and Miron (1996) show that extreme heat may reduce conceptions, in part because heat reduces sperm count and sperm motility. Other studies considering the relationship between meteorological phenomenon at conception and seasonal fertility include Rodgers, Harris, and Vickers (1992); Bronson (1995 and 2004); Seiver (1985); Wood et al. (2006); and Wehr (2001).

For this exercise, we match county-month level weather data from the National Climatic Data Center to the estimated county and month of conception.[xxv] Our measure of expected weather at birth is weather 3 months prior to the estimated month of conception.[xxvi] The regressions also include county fixed effects since the geographic distribution of births may vary across the year and such cross-sectional variation may contribute to seasonality.[xxvii] The inclusion of these effects also allows the weather controls to be identified by seasonal meteorological changes across time within counties. The results of this type of accounting exercise can be substantially affected by the order in which the covariates are added. Therefore, we follow the corrective procedure in Gelbach (2009) for decomposing the change in the coefficients in Table 1. Essentially, Gelbach's method decomposes the sample omitted variable bias into components that are estimated conditionally on all covariates, making the order of addition irrelevant.[xxviii]

The results of the decomposition are in Table 5. First, we show the coefficients from a regression of fraction of mothers married on month of birth, using birth certificate data from 1989–2001 (replicating the first column in Table 1).[xxix] Column 2 shows the coefficients after adding the full set of controls, and in column 3, we see the difference (original minus full). Our set of controls reduces the seasonal pattern in maternal characteristics; the reduction is both economically and statistically significant. These coefficients typically explain about half or more of the pattern, and for the summer months the pattern is completely eliminated.

Turning to columns 4, 5, and 6 we can see which sets of controls are responsible for the change in the month coefficients. For the early months all three sets of controls are important, but from late spring onwards it is clear that expected weather at birth dominates the decomposition. For most months expected weather at birth plays a larger role than fixed effects and weather at conception combined, and for later months in the year the difference is especially large. Indeed, from September onwards the effect of weather at conception—perhaps the single most-studied determinant of seasonal fertility outcomes—is wrong-signed and frequently insignificant, while the decomposition is almost entirely determined by our measure of expected weather at birth.[xxx],[xxxi] These results are depicted graphically in

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[xxv]We are able to match mother's county of residence to county-level weather data for 455 counties accounting for 73% of the sample. Where the mother's county of residence is not large enough to be uniquely identified in the birth certificate data, we use weather conditions for the state capital or (in cases where weather information for the capital is unavailable) the most populous city in the state. Results omitting these unidentified counties from the regressions are very similar to the results shown here. The weather controls included are listed below Table 5.

[xxvi]Thus for a woman who conceives in October (whose baby is expected to be born in July), we use the conditions in the most recent July to represent expectations of conditions at birth. Alternate methods of constructing expected conditions, including simply using the actual conditions at birth, give similar results.

xxvii: Dehejia and Lleras-Muney (2004) show that high-SES women are more likely to conceive when unemployment is higher. In the U.S., unemployment rates fluctuate seasonally with a peak in unemployment in the first quarter on average, which could help explain the observed birth patterns (in particular, the secondary fall peak in births to high-SES women). However, we investigated unemployment as an explanatory control and found it had little effect on our seasonal patterns; we omit it here for brevity.

xxviii: More specifically, consider a regression $y = X_1\beta_1 + X_2\beta_2 + \varepsilon$ that omits the matrix of regressors X_2; the omitted variables bias for β_1 is then $(X_1' X_1)^{-1} X_1' X_2\beta_2$. (Here, X_1 is a set of month of birth dummies and X_2 includes county dummies and controls for weather.) Gelbach decomposes the contribution to this bias from covariate k in X_2 as $(X_1' X_1)^{-1} X_1' X_{2k}\beta_{2k}$, where X_{2k} is column k in X_2 and β_{2k} is the associated coefficient for X_{2k} in the regression on y. This decomposition is conditioned on all other covariates and thus is invariant to the order in which covariates are considered. The decomposition sums up over k to the full omitted variable bias, and Gelbach shows that under reasonable conditions asymptotic estimation of the covariance matrix for the terms in the decomposition is obtainable. Aggregating the decomposition over a set of k covariates (e.g., all county dummies) is straightforward and described in his paper; see his appendix for covariance estimation formulas.

[xxix]These results vary very slightly from those in Table 1 because the sample here omits observations with missing weather or county of residence data (2.7% of the sample). Also, because the additional controls in Table 5 vary at the county level, we now cluster the residuals by county.

[xxx]We have also considered including controls for expected weather at other points in the pregnancy (for example, at 3 and 6 months gestation). These sets of controls do not have a practically or statistically significant effect on the birth month coefficients.

Figure 3, which shows the effects of adding our various controls on the month of birth coefficients. As these effects are estimated using Gelbach's decomposition, they are order invariant.

The dominance of expected conditions in Table 5 and Figure 3 is surprising. As mentioned above, prior work on seasonality has focused on how meteorological conditions at conception might drive seasonal fertility outcomes by affecting sperm motility, hormone production, male and female fecundability, coital frequency, and behavioral changes in the type or riskiness of sexual activity. But our results show that expectations about future conditions are much more important than any of these phenomena in accounting for the seasonal patterns considered here. Our work indicates a possible explanation for why researchers find important seasonal variation in fertility outcomes even after controlling for weather at conception (cf. Lam and Miron, 1996) and why seasonal patterns differ across countries sharing similar seasons; we discuss this more in the conclusions.

IV. Conclusions

Research throughout the social and natural sciences has demonstrated an association between the month of a child's birth and a variety of later outcomes, including health, education, and earnings. Past explanations of this relationship have been limited to factors that intervene after conception, such as compulsory schooling laws or seasonal exposure to disease and nutrition. In this paper, we consider the possibility that individuals born at different times of year are born to mothers with significantly different characteristics. Using birth certificate data and census data, we document large and regular seasonal changes in the socioeconomic characteristics of women giving birth. Women giving birth in winter are more likely to be teenagers and less likely to be married or to have a high school degree. These effects are large in magnitude and are observable for children born throughout the second half of the twentieth century. We show that these seasonal changes can account for a large portion of the poorly understood relationship between season of birth and other outcomes.

These results suggest that future researchers should use caution when considering season of birth as an instrument. While concerns on the instrument have been raised before, it remains in common use. Further, while Bound, Jaeger, and Baker "know of no indisputable evidence" on the direct effect of quarter of birth on education or earnings, they point out that "even a small direct association between quarter of birth and wages is likely to badly bias the estimated coefficient on education." Here we provide evidence for such a worrisome association. Future work comparing the outcomes of children born at different times of year —either as the independent variable of interest or for identification—should consider the large and persistent trends documented here. Further, in Section III we provide evidence that one's birth date is in part the result of a choice made by one's parents, suggesting that such comparisons would likely be problematic even if strong family controls were available.

While our focus is on US births, our findings may have implications for work on seasonal patterns internationally. As noted earlier, our work indicates a possible explanation for why researchers find important seasonal variation in fertility outcomes even after controlling for weather at conception (cf. Lam and Miron, 1996) and why seasonal patterns in outcomes differ across countries sharing similar seasonality. For instance, Germany and Spain are both located in the Northern Hemisphere and have similar changes in seasons during the year

[xxxi]One might be concerned that the inability of weather at conception to explain the seasonal pattern is somehow driven by collinearity with expected weather at birth, despite the precision of the estimates. When we perform the Gelbach decomposition excluding either controls for weather at conception or expected weather at birth, the results confirm the differential explanatory power of the controls in Table 5.

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(though different climates). But research has found better health outcomes for Spanish men born in June or July (Banegas et al., 2001, cf. also Reher and Gimeno, 2006) while documenting better health outcomes for German men born in the late fall and winter (Lerchl, 2004; Doblhammer, Scholz, and Maier, 2005). If these outcomes are driven by wanted births, then international variation in preferences for when to have a birth could help explain them. In fact, Basso et al., (1995) provide evidence that Germany and Spain have opposite patterns in seasonal planning of births, with the plurality of women in Spain first stopping contraception in the hopes of conceiving between July and September (which would typically yield a birth in late spring or early summer of the following year) while the plurality of German women planning a pregnancy stop contracepting between January and March. A thorough investigation of this topic would require a rigorous analysis relating contraception stoppage to the timing of pregnancy outcomes (or the use of a direct measure of preferences in birth timing), and large international data with information on time of birth and family background. Addressing these needs is a challenge we leave for future work.

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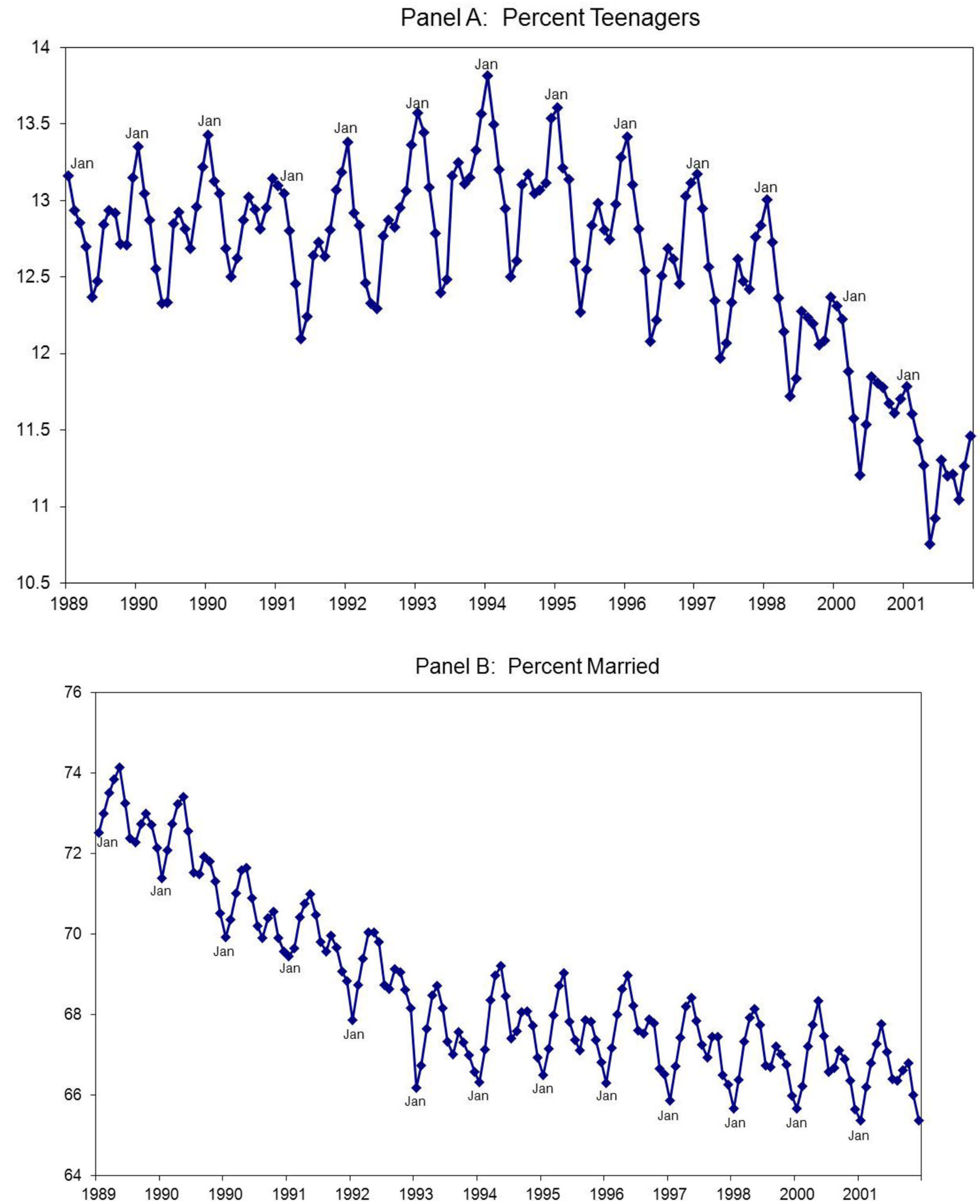
Panel A: Percent Teenagers
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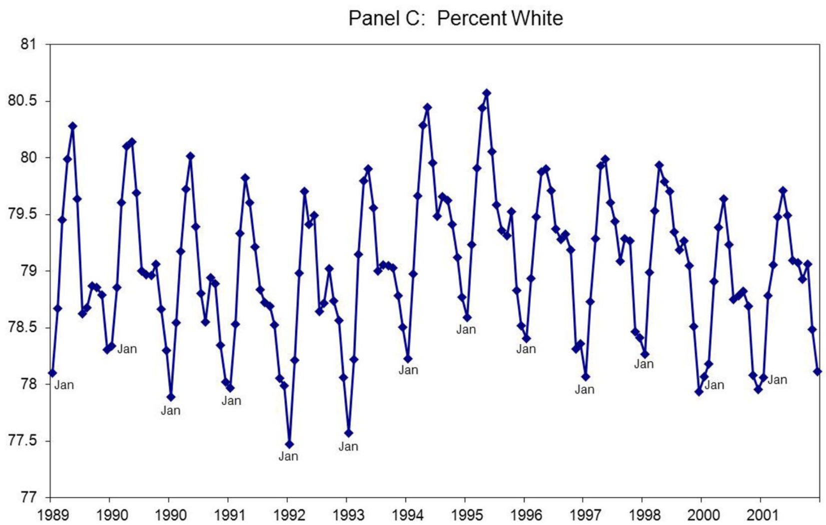

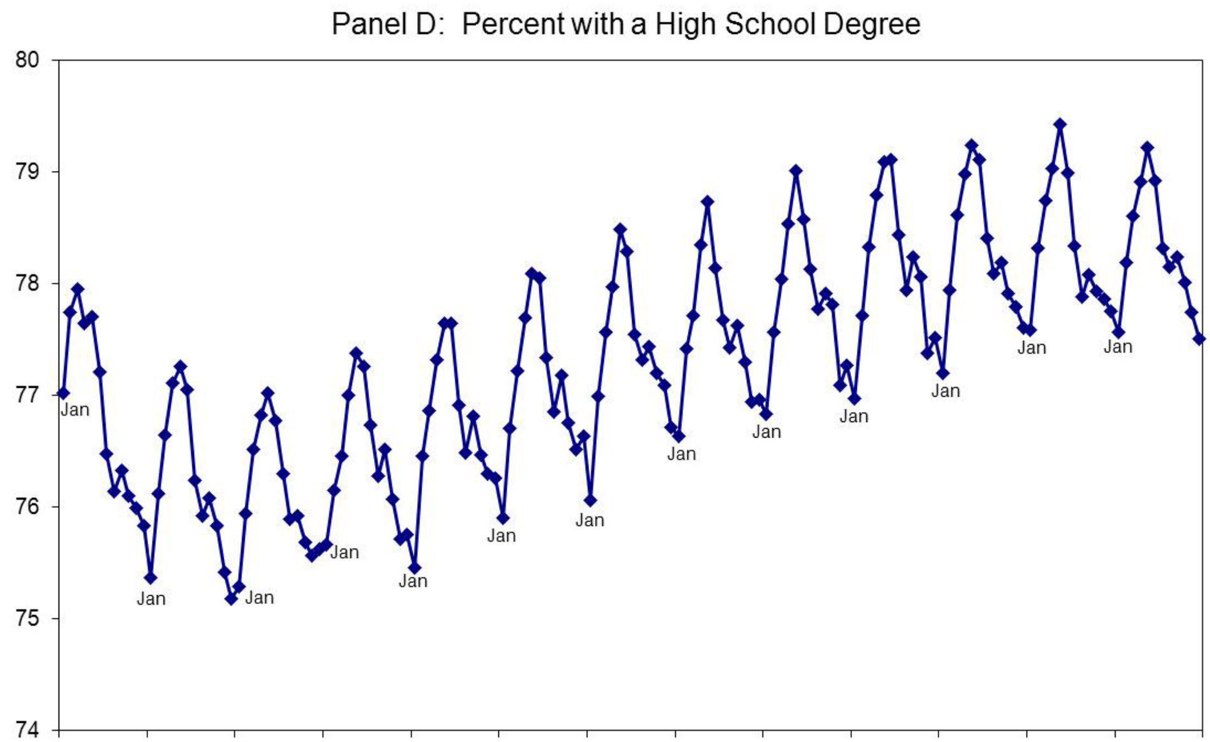

Figure 1.

Maternal Characteristics by Month, Natality Files, 1989–2001

Notes: The sample for each figure includes all births in the Natality Detail Files from 1989–2001, for 52,041,054 observations.

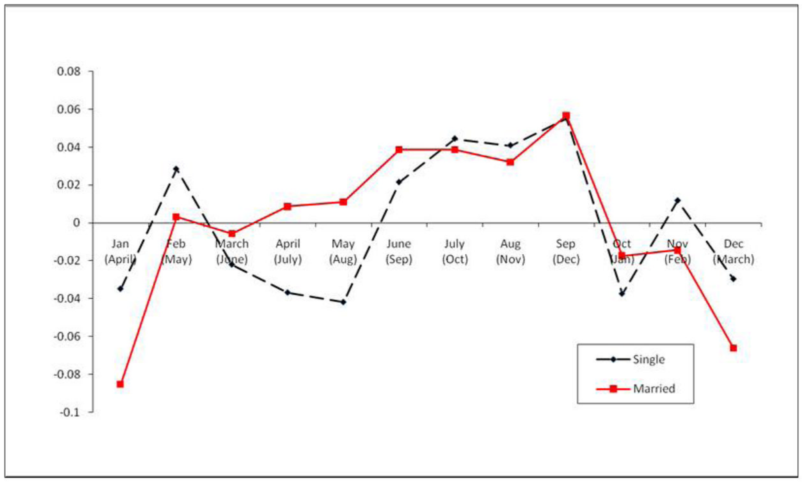

Figure 2.

Births Per Day

Notes: Figure shows the mean residuals each month from regressions of logged births per day on a third-order month-of-birth trend. Data are from the Natality Detail Files, 1989–2001. The upper row of month labels are month of birth; the lower row of month labels in parentheses are the typical month of conception for a given month of birth.

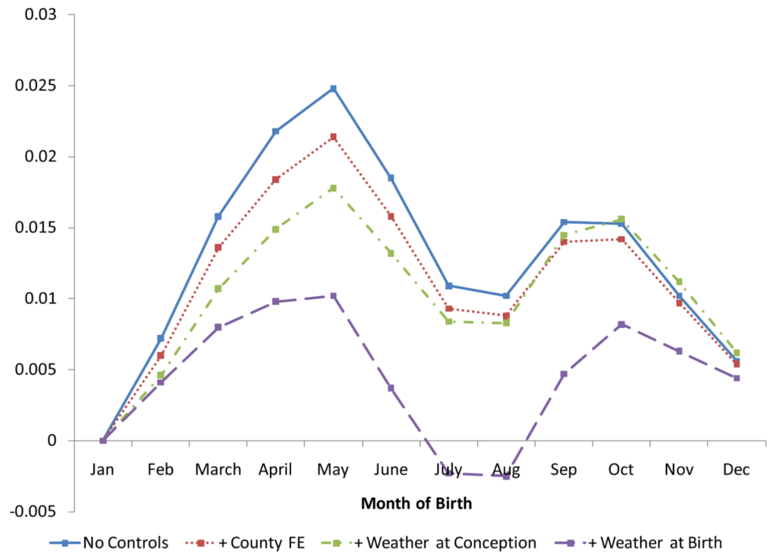

Figure 3.
Decomposition of Effect of Additional Covariates (Fraction of Mothers Married)
Notes: Figure is based on results of Gelbach decomposition in Table 5; see Table 5 for details of sample and estimation. The vertical axis gives the coefficient on the month dummies after adding the indicated controls. January is the omitted month.

Table 1

Mother and Infant Characteristics by Month: Natality Files, 1989–2001

	Mother Characteristics				Infant Characteristics		
	Married	**White**	**HS Degree**	**Teenager**	**Birth Weight in Grams**	**Low Birth Weight**	**Preterm**
February	0.0070 [0.0001]	0.0060 [0.0001]	0.0073 [0.0001]	−0.0024 [0.0000]	12.30 [0.05]	−0.0033 [0.0000]	−0.0032 [0.0000]
March	0.0155 [0.0001]	0.0127 [0.0001]	0.0122 [0.0001]	−0.0045 [0.0000]	19.06 [0.05]	−0.0049 [0.0000]	−0.0047 [0.0000]
April	0.0219 [0.0001]	0.0181 [0.0001]	0.0163 [0.0001]	−0.0074 [0.0000]	23.34 [0.05]	−0.0045 [0.0000]	−0.0050 [0.0000]
May	0.0250 [0.0001]	0.0189 [0.0001]	0.0195 [0.0001]	−0.0107 [0.0000]	20.43 [0.05]	−0.0036 [0.0000]	−0.0016 [0.0000]
June	0.0185 [0.0001]	0.0153 [0.0001]	0.0174 [0.0001]	−0.0093 [0.0000]	12.77 [0.05]	−0.0018 [0.0000]	0.0012 [0.0000]
July	0.0109 [0.0001]	0.0102 [0.0001]	0.0103 [0.0001]	−0.0053 [0.0000]	7.71 [0.05]	−0.0019 [0.0000]	−0.0023 [0.0000]
August	0.0102 [0.0001]	0.0096 [0.0001]	0.0068 [0.0001]	−0.0043 [0.0000]	9.07 [0.05]	−0.0029 [0.0000]	−0.0081 [0.0000]
September	0.0154 [0.0001]	0.0103 [0.0001]	0.0088 [0.0001]	−0.0050 [0.0000]	13.38 [0.05]	−0.0050 [0.0000]	−0.0157 [0.0000]
October	0.0154 [0.0001]	0.0098 [0.0001]	0.0055 [0.0001]	−0.0054 [0.0000]	8.18 [0.05]	−0.0020 [0.0000]	−0.0031 [0.0000]
November	0.0103 [0.0001]	0.0050 [0.0001]	0.0032 [0.0001]	−0.0035 [0.0000]	7.86 [0.05]	−0.0019 [0.0000]	−0.0061 [0.0000]
December	0.0056 [0.0001]	0.0021 [0.0001]	0.0025 [0.0001]	−0.0011 [0.0000]	−0.87 [0.05]	−0.0005 [0.0000]	−0.0032 [0.0000]
Constant	0.7280 [0.0001]	0.7818 [0.0001]	0.7666 [0.0001]	0.1331 [0.0000]	3333.70 [0.05]	0.0728 [0.0000]	0.1084 [0.0000]
F-stat	20,172	6,001	17,364	22,390	52,044	33,125	162,802

Notes: Robust standard errors in brackets. Each column is a separate regression, where the data were collapsed into county-month-year cells. The data were then weighted by cell size; there are 50,660,895 observations for the high school degree regression and for all others there are 52,041,052. Low birth weight is defined as <2500 grams, and preterm is defined as gestation of less than 37 weeks. The omitted month is January. All regressions include third-order polynomials for birth-month trends. The F statistic tests whether the coefficients for all of the month dummies are jointly zero; the 1% critical value for the F-test is 2.25.

Table 2

Season of Birth and Family Background: Results from the Census

Panel A: Regression on Dummy for Mother having a High School Degree

	1960 Census	1970 Census	1980 Census	1989–01 Natality
Second Birth Quarter	0.0098 [0.0019]	0.0126 [0.0007]	0.0101 [0.0008]	0.0105 [0.0002]
Third Birth Quarter	−0.0024 [0.0018]	0.0025 [0.0007]	0.0001 [0.0008]	0.0015 [0.0002]
Fourth Birth Quarter	0.0002 [0.0019]	0.0045 [0.0007]	0.0003 [0.0008]	−0.0034 [0.0002]
Mean of Dep. Var.	0.513	0.619	0.731	0.773

Panel B: Regression on Dummy for having a Married Mother

	1960 Census	1970 Census	1980 Census	1989–01 Natality
Second Birth Quarter	0.0023 [0.0011]	0.0048 [0.0005]	0.0068 [0.0007]	0.0142 [0.0002]
Third Birth Quarter	0.0003 [0.0010]	0.0024 [0.0005]	0.0028 [0.0007]	0.0046 [0.0002]
Fourth Birth Quarter	0.0006 [0.0023]	0.0032 [0.0005]	0.0036 [0.0007]	0.0029 [0.0002]
Mean of Dep. Var.	0.916	0.873	0.815	0.687

Panel C: Regression on Dummy for White

	1960 Census	1970 Census	1980 Census	1989–01 Natality
Second Birth Quarter	0.0064 [0.0013]	0.0083 [0.0005]	0.0092 [0.0007]	0.0111 [0.0002]
Third Birth Quarter	0.0032 [0.0012]	0.0018 [0.0005]	0.0007 [0.0006]	0.0037 [0.0002]
Fourth Birth Quarter	0.0037 [0.0012]	0.0048 [0.0005]	0.0018 [0.0007]	−0.0007 [0.0002]
Mean of Dep. Var.	0.876	0.858	0.827	0.791

Panel D: Regression on Dummy for Living in an Impoverished Household

	1960 Census	1970 Census	1980 Census
Second Birth Quarter	−0.0101 [0.0017]	−0.0058 [0.0005]	−0.0058 [0.0006]
Third Birth Quarter	−0.0049 [0.0016]	−0.0019 [0.0005]	−0.0005 [0.0006]
Fourth Birth Quarter	−0.0069 [0.0016]	−0.0041 [0.0005]	−0.0028 [0.0006]
Mean of Dep. Var.	0.257	0.156	0.162

Notes: Robust standard errors in brackets. In each panel, each column is a separate linear-probability regression. The sample for each census year includes all children ages 16 and under living with their biological mother. There are 578,773 observations in 1960; 3,674,887 obs. in 1970; and 2,766,118 obs. in 1980. All regressions include third-order polynomials for birth-quarter trends. In the last column of Panels A–C, the birth certificate data is collapsed to the birth quarter level for comparison. For all regressions except the first regression in Panel A, a Wald test that the quarter-of-birth coefficients jointly equal zero can be rejected at the one-percent level.

Table 3

Maternal Characteristics and Education and Wage Outcomes: Results from the Census

	Years of Schooling		Percent Dropouts		Wages, Logged	
Second Birth Quarter	0.037 [0.013]	0.024 [0.011]	−0.123 [0.127]	−0.004 [0.117]	0.006 [0.0026]	0.004 [0.003]
Third Birth Quarter	0.055 [0.012]	0.041 [0.011]	−0.828 [0.124]	−0.680 [0.114]	0.015 [0.003]	0.013 [0.003]
Fourth Birth Quarter	0.062 [0.013]	0.037 [0.011]	−0.868 [0.132]	−0.630 [0.112]	0.011 [0.003]	0.0065 [0.0024]
Wald Test that Birth-Quarter Coefficients Are the Same	$\chi^2[3]= 30.91$		$\chi^2[3]= 32.67$		$\chi^2[3]= 32.08$	
Family Characteristics?	No	Yes	No	Yes	No	Yes
R-Squared	0.904	0.914	0.906	0.913	0.948	0.951
Age Controls?	Yes	Yes	Yes	Yes	Yes	Yes
State Dummies?	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies?	Yes	Yes	Yes	Yes	Yes	Yes
Weights?	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in brackets. Regressions are for cohorts of males born between 1943 and 1955 and age 16 or under in the 1960 census. Cells are defined by state of birth, year of birth, and quarter of birth. For all cases, the Wald test that birth-quarter coefficients are equal can be rejected at the one-percent level. Family characteristics include controls for average mother’s education, fraction of mothers without a high-school degree, average mother’s age at birth, fraction of mothers giving birth as teenagers, fraction of mothers working, fraction of mothers married, the fraction white, and average cell family income as a percent of the poverty line. The maternal characteristics and income controls are taken from the 1960 census and outcomes are taken from the 1980 census. The wage regressions weight by total individuals reporting positive earnings in a cell; the education regressions weight by total individuals in a cell. The regressions have 2,596 cells; for the wage regressions there are 927,954 individuals and for the education regressions there are 1,090,826 individuals. Wages are pre-tax wage and salary income over weeks worked. Logged wages reports the average of log wages in the cell; using the log of averaged wages produces qualitatively similar estimates. The age controls measure age in birth quarters.

Table 4

Fraction of Mothers Married in the NSFG By Wantedness of Birth

	All Births	Wanted Births	Unwanted Births	All Births	Wanted Births	Unwanted Births
February	0.0122 [0.0139]	0.00906 [0.0147]	0.00805 [0.0415]	-	-	-
March	0.0185 [0.0142]	0.0250 [0.0139]	−0.0749 [0.0511]	0.0187 [0.0101]	0.0252 [0.0102]	−0.0624 [0.0362]
April	0.0304 [0.0140]	0.0337 [0.0149]	−0.0415 [0.0463]			
May	0.0187 [0.0135]	0.021 [0.0141]	−0.0131 [0.0399]	0.0173 [0.0098]	0.0210 [0.0103]	−0.014 [0.0295]
June	0.0269 [0.0133]	0.0292 [0.0143]	−0.0075 [0.0387]			
July	0.0147 [0.0145]	0.00985 [0.0156]	0.0024 [0.0398]	0.0151 [0.0108]	0.0151 [0.0113]	−0.0098 [0.0305]
Aug	0.0269 [0.0136]	0.0288 [0.0141]	−0.0136 [0.0408]			
September	0.00111 [0.0146]	0.0001 [0.0155]	0.0019 [0.0417]	0.0091 [0.0104]	0.0078 [0.0109]	0.0153 [0.0292]
October	0.0285 [0.0138]	0.0240 [0.0145]	0.0364 [0.0368]			
November	0.0262 [0.0137]	0.0227 [0.0146]	−0.00352 [0.0419]	0.0184 [0.00969]	0.0187 [0.0102]	−0.0172 [0.0300]
December	0.0221 [0.0128]	0.0232 [0.0135]	−0.022 [0.0424]			

Notes: Observations: 35,382. Robust standard errors, clustered by respondent, in brackets. The coefficients reported are marginal effects from a Probit regression on a dummy for whether a birth occurred to a married mother. The coefficients in columns 2 and 3 are all from a single regression (that also includes a non-interacted dummy for whether a birth was wanted; the marginal effect of this variable is 0.216 [0.0374]). Columns 5 and 6 are also from a single regression; the marginal effect for a wanted birth for this regression is 0.217 [0.0282]. A birth is defined as wanted if the woman responded that she wanted a birth at any time in the future. The likelihood-ratio test that the coefficients in column 2 equal the coefficients in column 3 is rejected; $\chi^2[11] = 20.77$, $p = 0.0358$; the same test for columns 5 and 6 yields $\chi^2[5] = 18.68$, $p = 0.002$. Regressions include a 3rd-order monthly trend and a dummy for interview year.

Table 5

Decomposition of Effect of Additional Covariates (Fraction of Mothers Married)

				Decomposition of Change in Coefficients From Three Added Sets of Controls:		
	Original Estimate	**Full Estimate**	**Change (Orig.-Full)**	**County FEs**	**Weather at Conception**	**Est. Weather at Birth**
February	0.0072 [0.0005]	0.0041 [0.0008]	0.0031 [0.0005]	0.0012 [0.0002]	0.0014 [0.0004]	0.0005 [0.0001]
March	0.0158 [0.0009]	0.0080 [0.0016]	0.0078 [0.0011]	0.0022 [0.0003]	0.0029 [0.0009]	0.0027 [0.0004]
April	0.0218 [0.0011]	0.0098 [0.0021]	0.0119 [0.0016]	0.0034 [0.0004]	0.0035 [0.0012]	0.0051 [0.0006]
May	0.0248 [0.0010]	0.0103 [0.0022]	0.0145 [0.0018]	0.0034 [0.0004]	0.0036 [0.0012]	0.0076 [0.0009]
June	0.0185 [0.0010]	0.0036 [0.0021]	0.0148 [0.0018]	0.0027 [0.0003]	0.0026 [0.0009]	0.0095 [0.0012]
July	0.0109 [0.0007]	−0.0023 [0.0018]	0.0132 [0.0016]	0.0016 [0.0002]	0.0009 [0.0004]	0.0107 [0.0014]
August	0.0102 [0.0008]	−0.0025 [0.0015]	0.0126 [0.0014]	0.0014 [0.0003]	0.0005 [0.0002]	0.0108 [0.0014]
Sept.	0.0154 [0.0010]	0.0046 [0.0012]	0.0108 [0.0013]	0.0014 [0.0003]	−0.0005 [0.0006]	0.0098 [0.0012]
October	0.0153 [0.0009]	0.0082 [0.0010]	0.0071 [0.0011]	0.0011 [0.0003]	−0.0014 [0.0009]	0.0074 [0.0009]
Nov.	0.0102 [0.0008]	0.0063 [0.0008]	0.0039 [0.0009]	0.0005 [0.0002]	−0.0015 [0.0008]	0.0049 [0.0006]
Dec.	0.0056 [0.0006]	0.0045 [0.0006]	0.0011 [0.0004]	0.0002 [0.0002]	−0.0008 [0.0004]	0.0018 [0.0002]

Notes: Standard errors are clustered at the county level and are in brackets. Sample includes 49,843,781 births; results vary slightly from Table 1 because observations missing weather or county of residence were omitted. Column 1 is a regression of the fraction of children born to married mothers on a time trend and set of month dummies. Column 2 adds three sets of covariates: (a) county fixed effects, (b) weather controls at conception and (c) estimated weather controls at birth. We estimate weather at birth using the weather in the county of residence 3 months prior to conception. Alternate methods of estimating weather at birth (including using actual weather at birth) produce similar results. Column 3 is the change in the coefficients from column 1 to 2. Columns 4–6 decompose column 3, showing the change in the coefficients attributable to each of the three sets of controls. County fixed effects are for county of residence, weather at conception is based on estimated county and month of conception. Weather controls include mean temperature, mean maximum and minimum temperature, days above 90 degrees, and degree departure from normal temperature.