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

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Abstract

Research has found that season of birth is associated with later health and professional outcomes; what drives this association remains unclear. In this paper we consider a new explanation: that children born at different times in the year are conceived by women with different socioeconomic characteristics. We document large seasonal changes in the characteristics of women giving birth throughout the year in the United States. Children born in the winter are disproportionally likely to be born to women who are teenagers, who are unmarried, and who lack a high school degree. We show that controls for family background characteristics can explain up to half of the relationship between season of birth and adult outcomes. Our findings suggest that, though popular, using season of birth as an instrumental variable may produce inconsistent estimates. Finally, we provide evidence that seasonality in maternal characteristics is driven by high-socioeconomic status women disproportionately planning births away from winter.

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"; Kleibergen (2002) writes, "quarter of birth is randomly distributed over the population"; 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.[1]

Yet despite the widespread use of season of birth as an instrumental variable and the assertion 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, less educated, and less likely to be married.

These differences are large. For example, we find that the fraction of children born to women without a high school degree is about 10 percent higher (2 percentage points) in January than in May. By way of comparison, this 2-percentage-point-effect on the fraction of mothers without a high school degree is about ten times larger than the effect from a one-percentage-

[1] Studies using season of birth as an instrumental variable or arguing for its suitability as such include Angrist and Krueger (1991, 1992, 1995, 2001), Staiger and Stock (1997), Levin and Plug (1999), Plug (2001), Adams (2002), Gelbach (2002, 2009), Lemke and Rischall (2003), Chamberlain and Imbens (2004), Hansen, Heckman, and Mullen (2004), Honoré and Hu (2004), Skirbekk, Kohler, and Prskawetz (2004), Chesher (2005), Cruz and Moreira (2005), Imbens and Rosenbaum (2005), Chernozhukov and Hansen (2006), Lefgren and McIntyre (2006), Dufour and Taamouti (2007), Andini (2008), Leigh and Ryan (2008), Angrist and Pischke (2009) and Maurin and Moschion (2009).

point increase in unemployment estimated by Dehejia and Lleras-Muney (2004). We also document a 10 percent decline in the fraction of children born to teenagers from January to May. This effect, which is 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 show similar seasonality in maternal characteristics using the 1960, 1970, and 1980 censuses.

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.

Next, we discuss the implications of this result for research using season of birth as an instrumental variable (IV). 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 IV settings where it has been used. We add controls for family background to IV estimates in a returns-to-schooling regression and find, when the effects of family background are allowed to vary over time, that the inclusion of these controls nearly doubles the estimated return to schooling from the baseline IV estimate; this large change may reflect either an increase or a decrease in the asymptotic bias of the IV estimate. These findings build on past work critiquing the validity of season-of-birth as an instrument, such as Bound, Jaeger, and Baker (1995). However, past work on the validity of this instrument has focused primarily on the instruments being "weak," and as mentioned above many researchers continue to argue that season of birth satisfies relevant exclusion restrictions.[2] The findings here pose a potentially fatal challenge to such arguments. Our findings may also have implications for other work comparing cohorts of children born at certain times of year to those born at other times of year, such as work on school entry dates (e.g., Elder and Lubotsky, 2006), tax-induced timing of births (Dickert-Conlin and Chandra, 1999), and on the fetal origins of adverse health outcomes

[2] 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).

(Winchester, Huskins, and Ying, 2009).

Lastly, 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 the patterns we see. The seasonality we document may thus be driven by wanted births, unwanted births, or some combination of the two.

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. This result 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. Controls for expected weather 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; conditions at conception have almost no explanatory power. 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. These findings may also have implications for studying seasonal fertility outcomes in other countries, where prior work has documented different patterns in countries sharing similar climates. In the conclusion, we discuss how variation in planning births may reconcile differences across countries whose seasonal weather patterns are broadly similar but whose seasonal fertility outcomes are not.

These results raise the question of why there are strong maternal patterns among women wanting to conceive. High-SES women wanting a birth could have more births at certain times of year if they either have stronger preferences for those times or if they are better able to achieve the desired timing. We show using a simple model that both a preference story and a timing-ability story are compatible with our results, but they have opposite implications for seasonality in *correctly timed* births. We provide suggestive evidence from the NSFG that our patterns are driven by high-SES women having stronger preferences for non-winter births.

The remainder of the paper is organized as follows. Section I provides some background on season of birth and later outcomes. Section II examines season of birth and mothers' characteristics using birth certificate and census data. Section III looks at how family background controls can explain season of birth's relation to later outcomes. Section IV examines using season of birth as an instrumental variable, and Section V explores causes for seasonality in maternal characteristics. Section VI concludes.

I. Season of Birth and Later Outcomes

Economists have long recognized that the month of a child's birth is associated with later outcomes such as test performance, wages, and educational attainment.[3] 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 (using metrics such as Armed Forces Qualification Test scores) intellectual ability.

Similarly, a large body of research outside of economics has proven that season of birth is associated with health outcomes such as developing schizophrenia (Watson et al., 1984; Torrey et al., 1997; Davies et al., 2003; and Tochigi et al., 2004), autism (Gillberg, 1990), dyslexia (Livingston et al., 1993), severity of menopausal symptoms (Cagnacci et al., 2006), extreme shyness (Gortmaker et al., 1997), risk for suicide (Rock et al., 2006) and life expectancy among the elderly (Costa and Lahey, 2005; and Doblhammer et al., 2005). Research has even suggested

[3] Examples include Angrist and Krueger (1991 and 1992), Bound, Jaeger and Baker (1995), Staiger and Stock (1997), Bound and Jaeger (2000), Donald and Newey (2001), Plug (2001), Kleibergen (2002), 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).

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). Many (but not all) of these studies find that children born in winter months have worse outcomes than other children.[4]

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. Angrist and Krueger argue that season of birth can therefore be used as an instrumental variable to study the long-term impacts of compulsory schooling on wages.

Researchers have cast doubts on Angrist and Krueger's assumption that these laws are the *only* reason schooling and wages change with season of birth. The best-known critique is by Bound, Jaeger and Baker (1995) (see also Bound and Jaeger, 2000; Cascio and Lewis, 2006; and Dobkin and Ferreira, 2010), who question whether quarter-of-birth dummies are valid instruments. However, Bound, Jaeger and Baker admit that, "we know of no indisputable evidence on the direct effect of quarter of birth on education or earnings," and the great majority of work building on their paper has focused on the concern they raise over weak instruments.

In this paper, we provide strong evidence regarding the relationship between quarter of birth and family background and we show that between-quarter correlations of the type they briefly investigate mask much larger within-quarter correlations. We demonstrate this relationship in birth cohorts from 1943 to 2001; our cohorts overlap with the cohorts considered by Angrist and Krueger (and subsequent work using quarter of birth as an instrument) but include more recent cohorts as well.

But more importantly our paper documents a substantial but previously-undiscovered 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 (further, in Section V we

[4] Some of these studies are international in focus. While relationships between season of birth and later outcomes have been documented in other countries, they sometimes differ from those found in the U.S.; it is unclear what explains these differences (Rosenberg, 1966). As in most prior work, our focus is on the U.S. case; in the conclusions we briefly discuss implications of our work for international research.

discuss the factors that drive this pattern). A relatively small amount of work has considered explanations for this relationship. In addition to the compulsory schooling explanation, researchers have pointed out that phenomena such as in-utero exposure to weather (Gortmaker et al., 1997) or illness (Sham et al., 1992; Suvisaari et al., 1999; Almond, 2006) may help to explain why winter births have worse outcomes. 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. Additionally, children born in the winter are likely to start school at an older age than other students, and this relative age difference may affect (for instance) their likelihood of being diagnosed with debilitating mental or physical conditions (Williams et al., 1970; Tarnowski et al., 1990; Plug, 2001).

There is little work establishing the practical importance of any of these explanations and none of these alternative explanations seriously consider the possibility that children born in the winter are different from other children at conception. Moreover, many researchers continue to assume that children conceived throughout the year are initially similar.[5] We hypothesize 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 the hypothesis that children born at different times of year are different at conception.[6] In the next section we provide such an investigation.

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[5] Almost all of the instrumental-variables research mentioned in the introduction postdates Bound, Jaeger, and Baker (1995). Research has used season-of-birth instruments both to explore substantive outcomes and to evaluate econometric techniques (examples of primarily econometric work include Chamberlain and Imbens, 2004; Cruz and Moreira, 2005; Chernozhukov and Hansen, 2006; Chesher, 2005; and Staiger and Stock, 1997). While the implications of our paper might be viewed differently for "methodological" as opposed to "substantive" use of season-of-birth-based IV, in each case the invalidity of the instrument could lead to problematic results. We discuss in Section IV some of the possible consequences of our findings for work using season of birth as an instrument in a returns-to-education setting.

[6] There is a small and inconclusive body of research which uses mostly small-scale and/or international data to consider whether seasonality of conception differs for certain women. Warren and Tyler (1979) find that women living in certain census tracts in Fulton County, Georgia, have less seasonality in conception than other women. Pasamanick et al. (1960) look at births in Baltimore in the early 1950s and find that high-socioeconomic-status (SES) women have less seasonality in conception. Lam, Miron, and Riley (1994) find that white women in Georgia from 1968 to 1988 have less seasonality in births than nonwhite women; Seiver (1989) has a similar result. Kesterbaum (1987) uses census data to find that for births between 1977 and 1979 there is more seasonality for low

II. 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 mothers' average 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 has less education.[7]

SES women. In contrast, James (1971) examines births in Great Britain and Bobak and Gjonca (2001) look at seasonal conception in the Czech Republic and in both cases they find greater seasonality among higher-SES women. Mitchell et al. (1985) find that seasonal conception patterns varied by profession in nineteenth century Tasmania.

[7] A few colleagues have questioned whether standard errors are needed in the figure since the birth certificate data represent virtually the entire population of births in the United States from 1989 to 2001. While the conceptual need for confidence intervals in the figure may be debatable (see Deaton, 1997), from a practical standpoint the confidence intervals are so small as to be indistinguishable from the trends depicted; consequently they are omitted.

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 Panel C, we see that the percent of mothers who are white is about two percentage points higher in May than in January; this effect is about 25 times larger than the increase in births to white mothers associated with a one-percentage-point increase in the unemployment rate (Dehejia and Lleras-Muney, 2004). Panel D shows an increase in average mother's education of 0.15 years every spring; estimates of the effect of one additional year of mother's education on one's own education typically find a smaller effect of about 0.12 (Behrman and Rosenzweig, 2002).

Figure 2 shows a similar pattern in the percent of mothers who have finished the twelfth grade. Here, we plot the data separately by race, to show that these patterns exist within racial groups. While the percent of nonwhite mothers with a high school degree is increasing over this period, both whites and nonwhites giving birth in May are more likely to have graduated high school relative to those giving birth in January. For each group we observe that the magnitude of this difference is about 2 percentage points. By way of comparison, this is almost ten times larger than the effect of a one-percentage-point increase in unemployment estimated by Dehejia and Lleras-Muney (2004).

One might wonder whether this result on high-school degree attainment is mechanically related to the result for teen births in Panel A of Figure 1 since many teenage mothers are not old enough to have completed high school. Interestingly, Panel B of Figure 2 shows that the pattern for percent of mothers without a high school degree is preserved even if one restricts the observations to women giving birth at age 19 or above. While fewer women in this group do not have a high school degree, the effect is very similar even when births to women of high school age are omitted.

To assess the magnitudes of the seasonal trends we collapse the data into county-of-birth/month-of-birth/year-of-birth cells.[8] Using cell *c* as the unit of observation we estimate

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

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_i is noise. Regressions are weighted by cell size and robust standard errors are reported in brackets.

The estimates can be seen in the regression results 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; Black, Devereux, and Salvanes 2005; Case, Paxson, and Fertig 2005; Currie, 2008).

Table 2 presents month dummy variables from regressions on birth weight, fraction low-birth-weight births, and fraction born premature. In all regressions the omitted month is January and all regressions include a third-order polynomial trend for birth month. 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, with early spring and late summer births being less likely to be low-birth-weight and less likely to be 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*

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

We now turn to the decennial census to see if season of birth relates to family characteristics in earlier years and in a different data set. The census data have limitations, including the fact that they represent only a sample of all births and that the most recent usable censuses do not contain month-of-birth information but instead report quarter-of-birth information. However, analyzing census data 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.

Table 3 reports results from regressing a number of outcomes (described below) on quarter-of-birth dummies and a third-order polynomial time trend. The regressions are analogous to equation (1) except that month of birth has been replaced by quarter of birth; the omitted quarter is the first quarter of the year. 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).[9] 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.

Panel A reports results from linear probability regressions on a dummy variable that equals unity if a child's mother has a high-school degree; all but one of the coefficients in the regressions are positive, indicating children born in the second through fourth quarters of the year are more likely to have a mother with a high school degree. For the 1960 regression, a Wald test that the season of birth coefficients are jointly zero is marginally significant, with a *p* value of 0.12. For the remaining regressions in Panel A—and all the other regressions in the table—a test that the birth-quarter coefficients are jointly zero can be rejected at the one percent level. The coefficients are also reasonably large in magnitude; with the second-quarter coefficient representing a little less than 2 percent of the (steadily rising) mean. The results are generally similar across census years; although seasonality (especially for the third and fourth quarters) is more precisely estimated in later years. 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; for Panel A the magnitudes are quite similar. The results suggest that the use of quarterly-level data imposed by the Census masks significant within-quarter variation.

[9] Age in months is available in 1940 and 1950 only for individuals under age 1 at the time of the census, and for individuals under age 5 at the time of the census in 1930 and 1920. Quarter- or month-of-birth information is not available from IPUMS for the 1990 and 2000 censuses.

The dependent variable in Panel B is a dummy for whether a child's mother was married at the time of the census. The coefficients here are very comparable to those in Panel A, showing that children born in the first quarter are more likely to be born to unmarried parents, and that this result grows somewhat stronger over time. Panel C shows that the fraction of children who are white is lower among children born in the first quarter of the year, and the result again gets stronger across census years. For both Panels B and C the estimated effect of being born in the second quarter is about one percent of the mean or less in magnitude. Again, these results underestimate the magnitude of seasonality's relation to family background since the Vital Statistics results in Table 1 show significant variation within birth quarters. When collapsed to the quarter level, the Vital Statistics results are consistent with growing seasonality in maternal characteristics over time. For all the regressions in Panels B and C, a Wald test can reject that the quarter-of-birth coefficients are jointly zero at the one-percent level.

Panel D reports regressions from each census on the likelihood that a child lives in an impoverished household, an outcome that is not directly observable in the Vital Statistics data. For each census it is clear that children born in the first quarter of the year are more likely to live below the poverty line than other children. The effects here are reasonably large, suggesting for each census year a relative increase from the first to the second quarter of the year that is about 4 percent of the mean. As with the prior estimates, the difference between the first and second quarters is the largest, and again a Wald test rejects for each census year that the quarter-of-birth coefficients are jointly zero.

Taken with the Vital Statistics results, Table 3 shows that the relationship between season of birth and family background has persisted for at least the second half of the twentieth century, and the results for the second and third quarter appear in some cases to be stronger in later years. In the next sections we consider how this relationship might account for season-of-birth's impact on later outcomes, and the implications of our finding for past work using quarter of birth as an instrumental variable.

III. 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. In this section we consider to what extent the relationship between season of birth and later outcomes is accounted for by variation in maternal and family background characteristics of children born throughout the year.

As in most prior studies, we use the decennial census for this investigation. In addition to quarter of birth information, the census has information on completed schooling and earnings. However, for our study we also need to observe measures of individuals' family backgrounds. Such information is readily 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 (wage and completed schooling information) 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, where cells are defined by state of birth, year of birth, and quarter of birth. Using 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 the typical conditions for individuals ages 16 and under living with their biological mothers.[10] We then match background information from the 1960 census to information on the outcomes realized as of the 1980 census (the latest available year). This combination of cells across census years is similar in spirit to Angrist and Krueger (1992).[11] While these cohorts are as old as possible while allowing us to measure family characteristics in 1960, there may be a concern that the wage information for younger individuals in these cohorts will not be an accurate reflection of lifetime earnings. Consequently, we further restrict the sample to individuals born in or before 1955 and thus ages 25 to 36 when observed in 1980. Similar results are obtained when using all children 16 and under in the 1960 census and not just those born in or before 1955 (see Buckles and Hungerman, 2008, for these results). Using census data from 1960 (1% IPUMS sample) and 1980 (5% IPUMS sample), we estimate

$$Outcome_c = \alpha_1 + \beta_1 \mathrm{Q} + \gamma_1 \phi_s + \theta_1 \mathrm{Y} + \lambda_1 age + \rho_1 age^2 + \varepsilon_1 \qquad (2)$$

[10] Over 95% of all children in the 1960 census ages 16 and under live with their biological mother. Migration from the household is significantly more evident for individuals ages 17 and over.

[11] 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).

and

$$Outcome_c = \alpha_2 + \beta_2 \mathrm{Q} + \delta \mathrm{X_c} + \gamma_2 \phi_s + \theta_2 \mathrm{Y} + \lambda_2 age + \rho_2 age^2 + \varepsilon_2 \qquad (3)$$

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 (c) the log of average wages [12] for cell *c* or (d) average wages (in levels) for cell *c*.[13] 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.

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, and the fraction of mothers in each cell who are teenagers, who are working, who are married, the fraction white, and the fraction of mothers without a high-school degree. Maternal controls are measures for *c* as of 1960 and family income is for 1959.[14]

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 in equation (2). Alternatively, if one of these conditions is met, then 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

[12] Using the average of logged wages, instead of the log of average wages, produces similar results to those shown.
[13] Wages are constructed as total individual pre-tax wage and salary income in the past year over weeks worked in the past year. As wages are measured in only one year, there is no need to adjust for inflation.
[14] 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 the wage regressions.

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 4. Regressions are weighted by cell size.[15] The first pair of columns estimate (2) and (3) where 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).[16] 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.[17]

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. 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 pairs of columns look at logged wages and wages in levels. The logged wage regressions 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. The results are similar when looking at wages in levels, where for two quarter-of-birth coefficients the result is essentially eliminated after controlling for

[15] 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.

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

[17] The family background coefficients are not reported here for brevity but generally accord with intuition.

family characteristics. (The average weekly wage in the sample is about \$330, so the implied proportional effect for the average individual in the last two columns is comparable to the proportional effects found using the log of wages.) 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.[18]

IV. Implications for Quarter of Birth as an Instrumental Variable

Season of birth is often used 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.

In this section we explore the sensitivity of using quarter of birth as an instrumental variable in a returns-to-education setting. In theory, the effect of the exclusion-restriction failure on the IV estimates is hard to predict and would depend upon the sign and magnitude of the omitted variable bias in the first-stage and the reduced-form estimates, as well as the extent to which these biases are addressed by our controls. As discussed in Buckles and Hungerman (2008), it is possible that adding controls for family background reduces the bias in both the first-stage and reduced-form regressions, but that the asymptotic bias of the IV estimator stays the same or increases. Thus, while it will be interesting to see how IV results change when family controls are added, there is little to guide us in terms of predicting how the IV estimates should

[18] 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 4, 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).

change. Moreover, even if the IV results were unchanged with the addition of family backgrounds, this would not imply that the instrument is uncorrelated with unobservables; it could instead indicate that the instrument is correlated with unobservables in both the first-stage and reduced-form equations.

Table 5 shows regressions using quarter-of-birth variables as IVs in a returns-to-education regression.[19] The dependent variable in the first four regressions is the log of average cell wages; the last four regressions use average wages in levels. The regressions use the same sample and controls as the regressions in Table 4. Following Angrist and Krueger (1991), we show results using quarter of birth interacted with year-of-birth dummies as instruments.

The first two regressions show a return to education of 8.9 and 10.7 percent, respectively, estimates close to those in Angrist and Krueger (1991).[20] Columns 3 and 4 add in the controls for family background. Column 4 allows these controls to vary by cohort age; this may matter if the impact of family background on education and earnings varies by age or over time.[21] The estimates display sensitivity to the addition of family controls, with a 48 percent increase in the coefficient between columns 1 and 3 (from 0.089 to 0.132) and an 82 percent increase between columns 1 and 4 (0.089 to 0.162).[22] The 16.2 percent return to education in column 4 is strikingly larger than the baseline estimate. It is also larger than the baseline OLS estimate of 0.089 [0.003], countering a key finding from Angrist and Krueger that OLS and IV regressions yield qualitatively similar results. One explanation for this change is that the OLS is negatively biased and our family controls allow for an unbiased IV regression that produces an accurate estimate of the returns to schooling. Alternately, it could be that OLS is positively biased (the standard intuition) and that our IV regressions with family controls amplify this bias. This amplification could occur, for instance, if the family controls reduce the association between the

[19] Consistent with past work, the first stage F-statistics for the instruments in Table 5 are often small (e.g., less than 2), which likely biases IV towards OLS.

[20] Our specification in column 1 is most similar to Angrist and Krueger (1991) Table VI, column 4, where their 2SLS estimate of the return to education is 0.0948 (se = 0.0223). This baseline result is also comparable to most of the 2SLS results they produce for all of their cohorts, including those born in the 1920s and 1930s.

[21] A Wald test rejects at the one-percent level the hypothesis that, for each background control, the background-by-year coefficients for the regression in column 4 are equal across years. This indicates that the predictive effect of these controls varies over time. This fits with evidence on maternal background for our cohorts in Table 2 of Card (1999), as well as more general discussions in (for instance) Haveman and Wolfe (1995), Taubman and Wales (1973), Galindo-Rueda and Vignoles (2005), and Riphahn and Schieferdecker (2008).

[22] The results in Table 5 are similar to those in Table 6 of Buckles and Hungerman (2008). The minor differences between the two are the result of limiting the sample in the current version to cohorts born through 1955, since more recent cohorts' earnings in 1980 may not be a good predictor of lifetime earnings.

instruments and the unobservables more in the first-stage regression than in the structural regression (cf. Buckles and Hungerman, 2008). The last four regressions in Table 5 use wages in levels; this alternative specification produces very similar results. Overall, the results in Sections II, III, and IV suggest that the use of season of birth as an instrumental variable is problematic.

V. Explaining Seasonality in Maternal Characteristics

The results of this paper show that mothers who are younger, unmarried, nonwhite, and less educated are disproportionately more likely to give birth in winter months than higher SES women. One might wonder why these striking patterns in maternal characteristics exist. As a starting point, Figure 3 shows the mean residuals each month from regressions of logged births per day for (a) married women and (b) single women.[23] 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. 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.

There are two noticeable features in Figure 3. 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.[24]

[23] 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).

[24] 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.

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. The seasonality we document may thus be driven by wanted births, unwanted births, or some combination of the two.

We can investigate this 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, if they stopped using birth control before the pregnancy, and if the reason for stopping contraception was to become pregnant. There are 35,792 pregnancies ending in a live birth in the data.

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

$$married = \alpha + \beta * \text{month}*want + \delta * \text{month}*notwant + \gamma * want + \theta_y + \varepsilon \,, \tag{4}$$

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 *notwant* is a dummy that equals unity if a birth is reported as not wanted.[25] 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. Below we consider an alternate definition of wantedness. The term θ_y includes a third-order monthly time trend and dummies for interview year.

Table 6 reports marginal effects from Probit regression estimates of equation (4). (Linear

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

probability estimates are similar.) Robust standard errors, clustered by respondent, are in brackets. 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. The December coefficient is a bit larger than expected, but for each coefficient in Table 6 the 95% confidence interval includes the corresponding value found in Table 1 and in most cases the coefficients in Table 6 are less than a standard-error away from the Table 1 values (although this is partly driven by the lack of precision for some of the Table 6 estimates). Overall the seasonal patterns in NSFG appear to be reasonably close to the patterns found in the Natality Detail Files.

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. We address this concern in two ways. First, in the last three columns of Table 6 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 from this specification 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 ($p = 0.002$).

Second, we redo the estimates in Table 6 using an alternative definition of wantedness based on women's use of contraception. The NSFG asks respondents whether they used or stopped using contraception before becoming pregnant; women who stopped or never used birth control were then asked why. Based on these questions, we define a birth as wanted if the mother was not contracepting at the time of conception and if the mother stated that she was not contracepting because she wanted to become pregnant. All other births—about 12,000 births or a third of the data—are defined as not wanted.

Table 7 reports results from this alternate measure of wantedness; the results are the same as before. In fact, Table 7 shows that the patterns in maternal characteristics are not only driven by women who describe their births as "wanted," but more specifically are driven by women who are actively trying to conceive.

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.[26] Second, this result indicates there is seasonal variation in the wantedness of births within SES.[27] 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, exogeneity issues aside, seasonality in wantedness is an interesting and potentially important new factor when considering the relationship between season of birth and later outcomes. Our work in Section III 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 III indicate.

Fourth, most prior work discussing seasonality in birth has focused on conditions at conception (such as weather) as potentially important explanatory controls. The results here suggest that in addition to conditions at conception, it may be that expected conditions at the *anticipated time of birth* will play a key role in explaining seasonality in fertility outcomes.

For example, Lam and Miron (1996) show that extreme heat may reduce conceptions, in part because heat reduces sperm count and sperm motility (the relationship between

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

[27] 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 Tables 6 and 7 only document a relative increase in wanted births.

meteorological phenomenon at conception and seasonal fertility has been considered in a number of other studies; examples include Rodgers, Harris, and Vickers,1992; Bronson, 1995 and 2004; Seiver, 1985; Leppäluoto et al., 2003; Wood et al., 2006; Wehr, 2001; and Pharm et al., 2004). Low SES individuals may be more exposed to temperature extremes, and work has shown that temperature may have larger effects on the health outcomes of low SES populations than others.[28] If low SES women or their partners are more responsive to summer heat than other women, this may help explain the "spring dip" in low-SES births in Figure 3 (nine months after the hottest months of summer). But if seasonality in maternal characteristics is driven by wanted or planned births, then the expected conditions at birth may play a salient role in explaining seasonal patterns in maternal characteristics.

To consider these alternate explanatory channels, we investigate whether the coefficients in Table 1 are significantly affected when we add controls for weather, where we control for weather not only at the estimated time of conception, but also at expected time of birth. Our measure of expected weather at birth is weather 3 months prior to the estimated month of conception.[29] For the regressions, county and month of conception are estimated using gestation and county of residence, and are matched to weather data at the county-month level. Weather data are from the National Climatic Data Center and include controls for mean temperature, mean maximum and minimum temperature, number of days over 90 degrees Fahrenheit, and the degree departure from normal temperature over the estimated month of conception.[30] 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.[31] The inclusion of these effects also allows the weather controls to be identified by seasonal meteorological

[28] For instance, Curriero et al. (2002), O'Neal, Zanobettir and Schwartz (2003), and Schwartz (2005) all find evidence that the relationship between mortality and extreme temperatures may be greater for low SES individuals.

[29] Thus for a woman trying to conceive 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. We have used alternate methods of constructing expected conditions, including simply using the actual conditions at birth; alternate approaches give similar results.

[30] 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.

[31] Dehejia and Lleras-Muney (2004) show that married 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 married 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.

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.[33] The results of the decomposition are in Table 8. First, we show the coefficients from a regression of month of birth on the fraction of mothers married, using birth certificate data from 1989-2001 (replicating the first column in Table 1).[34] 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

[33]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.

[34] 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). In most cases, the missing data is for degree departure from normal temperature. Also, because the additional controls in Table 8 vary at the county level, we now cluster the residuals by county.

weather at birth.[35,36] These results are depicted graphically in Figure 4, which shows the effects of adding our various controls on the month of birth coefficients. The top line shows the month coefficients from column 1 of Table 8. The next line shows the coefficients after adding county fixed effects (i.e., the line subtracts column 4 in Table 8 from column 1). The third line shows the month coefficients once fixed effects and weather at conception are both controlled for, and the final line shows the month coefficients once all controls are included. (As these effects are estimated using Gelbach's decomposition, they are order invariant.)

Taken together, the results in Tables 6, 7, and 8 indicate that our seasonal patterns are driven by wanted births and that high-SES women are especially likely to realize wanted births away from the winter. As discussed above, this has a number of important implications. But one might wonder *why* high-SES women are especially likely to plan births seasonally. This could be driven by preferences (high-SES mothers have stronger preferences against births in the winter, for instance because it is more difficult to get time off of work in the winter) or by timing ability (no one wants the winter but high-SES women are better at getting their preferred timing). While either a preference story or a timing-ability story could explain the patterns we document here, they can in fact be distinguished, as they have opposite predictions for seasonality for correctly timed births.

To see this, consider a simple model of women planning a birth. There are two times of the year, Winter (e.g., October through March), and the rest of the year, which we call Summer. Suppose there are two groups of women, high types and low types, and for simplicity normalize the population of each group of women to unity. For women in group *i*, the fraction who want a birth in the Summer is θ^i. We will assume that most women do not want a Winter birth, so that $\theta^i > ½$. This is consistent with data in Rogers and Udry (1988), which finds that the vast majority of women name a "best month for birth" other than winter months. The iid likelihood that a woman has a correctly timed birth (i.e., a birth at her preferred time of year) is denoted α^i. We will also assume that women planning births are at least slightly more likely to get their

[35] 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.

[36] 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 8.

preferred time of year than they are to mistime their birth, so that α^i > ½.[37]

The number of Summer births for type *i* women will thus be $\theta^i\alpha^i + (1-\theta^i)(1-\alpha^i)$; the first term reflects correctly timed births intended for Summer and the second term reflects mistimed births intended for Winter. The number of Winter births is $(1-\theta^i)\alpha^i + \theta^i(1-\alpha^i)$. It is easy to show that the number of Summer births is increasing in both θ^i and α^i. High types may thus have more births in the Summer if they have a greater preference for the Summer, so that their θ^i is greater (preference story), or if they have a greater ability to correctly time their births, so that their α^i is greater (timing-ability story).

However, the preference and timing-ability stories have opposite implications for seasonality in correctly timed births. The fraction of Summer births for type i that are correctly timed is given by $S^i = \dfrac{\theta^i\alpha^i}{\theta^i\alpha^i + (1-\theta^i)(1-\alpha^i)}$, and the fraction of correctly timed Winter births is given by $W^i = \dfrac{(1-\theta^i)\alpha^i}{(1-\theta^i)\alpha^i + \theta^i(1-\alpha^i)}$. Let D^i be the seasonal variation in correctly timed births; $D^i = S^i - W^i > 0$, where the inequality can be trivially established. An increase in D^i thus denotes greater seasonality in correctly timed births, and a decrease denotes less seasonality.

It is straightforward to show that $\dfrac{\partial D^i}{\partial \theta^i} > 0$ but $\dfrac{\partial D^i}{\partial \alpha^i} < 0$. To see the intuition for the derivative with respect to θ^i, consider an extreme case where θ^i is set to unity and all type *i* women want Summer births. Then type *i* women will have more births in the Summer, but *none* of their Winter births are correctly timed and *all* of their Summer births are correctly timed: there would be extreme seasonality in correctly timed births. Next, consider an extreme case where $\alpha^i = 1$, so that all type i women get the timing they want. Then type i women will again have more births in the Summer, but there will be *no* seasonal variation in the fraction of correctly timed births; all births are correctly timed throughout the year. Thus greater high-type seasonal variation in the fraction of correctly timed births supports a preference story, and less high-type seasonal variation in correctly timed births supports a timing-ability story.

[37] In this simple framework, this assumption merely imposes that the chances of a correctly timed birth are slightly higher than if timing were entirely random. This assumption is also consistent with the NSFG data, which shows that among planned births the great majority of women (about 80%) report a correctly timed birth; this high number is similar both for births to married women and births to unmarried women. One may wonder how the NSFG's use of timing compares to the model's; we discuss this more below.

We can provide suggestive evidence of seasonality in correctly timed births using the NSFG. In addition to asking whether births were wanted, the NSFG asks women whether births were correctly timed. One concern with this test is that many women reporting an incorrectly timed birth may be referring to inter-year timing, whereas our model is based on intra-year timing. So long as notions of inter-year timing among wanted births do not vary by season, this should not bias the coefficients. Furthermore, about 35% of women reporting a wanted-but-mistimed birth state that the birth was mistimed by less than 12 months. However, we might still interpret results of the NSFG on timing as suggestive given this possible discrepancy in the definition of "timing" between the data and our model.[39] The equation we will estimate is:

$$timing = \alpha + \beta * \text{month*}married + \delta * \text{month*}notmarried + \gamma * married + \theta_y + \varepsilon \,, \qquad (5)$$

where *timing* is a dummy variable that equals unity if a birth is reported by the mother as correctly timed, "month" is again a vector of dummies for month of birth, *married* is a dummy that equals unity for a married mother, *notmarried* is a dummy that equals unity for an unmarried mother, and as before the NSFG regressions will include a third-order month trend and dummies for year of interview.[40] Following the model the regressions will be restricted to wanted births as defined in Table 6.[41] The coefficients β and δ reflect how the likelihood a birth is reported as correctly timed varies during the year; larger values of these coefficients correspond to greater differences in seasonality of correctly-timed births (in the context of the model, a larger D^i).

Table 9 reports marginal effects from a Probit estimation of equation (5). (Linear probability models yield similar estimates.) Although many of the coefficients are not statistically significant, the results in columns 1 and 2 indicate greater seasonality in correctly timed births among married women (a test that the married coefficients equal the unmarried coefficients is rejected at a marginally significant level, $\chi^2[11] = 17.07$, p = 0.106). Turning to the last two columns, which report a regression where months are paired together, again the

[39] Dropping women who report births mistimed by a year or more produces similar estimates. However, not all women reporting a mistimed birth are asked about the amount by which the birth is mistimed; we thus report regressions including all mistimed births.

[40] The NSFG also asks women whether their partner reports a birth as correctly timed. Use of that measure of timing produces similar results.

[41] Using the alternate definition of wanted, with its smaller sample size, leads to less precise results. Given that some women may want a pregnancy but may use contraception to help time the pregnancy correctly (and these women would be excluded from the analysis under our second definition of wantedness), our initial broader definition of wantedness not only allows a greater sample size but also fits better with the model. Using all births (wanted and unwanted) produces estimates similar to those shown here.

estimates show greater seasonality in correctly timed births. We can reject the hypothesis that the married coefficients equal the unmarried coefficients, $\chi^2[5]=10.42$, $p = 0.064$.

The results from Table 9 might be regarded as suggestive, as the notion of timing in the NSFG may be viewed by many respondents as inter-year and the model is based on an intra-year notion of timing. However, the facts that high-SES women have fewer planned births in the winter, have fertility outcomes more responsive to anticipated weather conditions at birth than conditions at conception, and are less likely to report that a wanted winter birth was correctly timed, are all consistent with a story where high-SES women have stronger preferences for non-winter births than do other women.

VI. Conclusion

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."[42] Here we provide evidence for such a problematic association.

These results may also have implications for work comparing cohorts born in certain

[42] This concern is also discussed by Conley, Hansen, and Rossi (2008).

times of year to other cohorts, such as work on age at school entry or on the fetal origins of health outcomes. Our results indicate the potential utility of regression-discontinuity-based approaches (e.g., Dobkin and Ferreira, 2010) for studying school cut-off dates and educational outcomes. Further, as discussed in Section 2, our results do not preclude direct effects of school cut-off dates, compulsory schooling laws, in-utero exposure to certain elements, or other seasonal factors on later outcomes. But 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.

While our focus is on US births, our findings may have implications for work on seasonal patterns internationally. As noted in Section II, variation in outcomes by season of birth have been noted in many countries, but patterns in outcomes sometimes diverge between countries sharing similar seasons. For instance, both Germany and Spain are located in the Northern Hemisphere (and in Europe), 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 high-SES women in other countries have especially strong preferences or timing ability, then international variation in preferences for when to have a birth could help explain these differences in fertility outcomes. 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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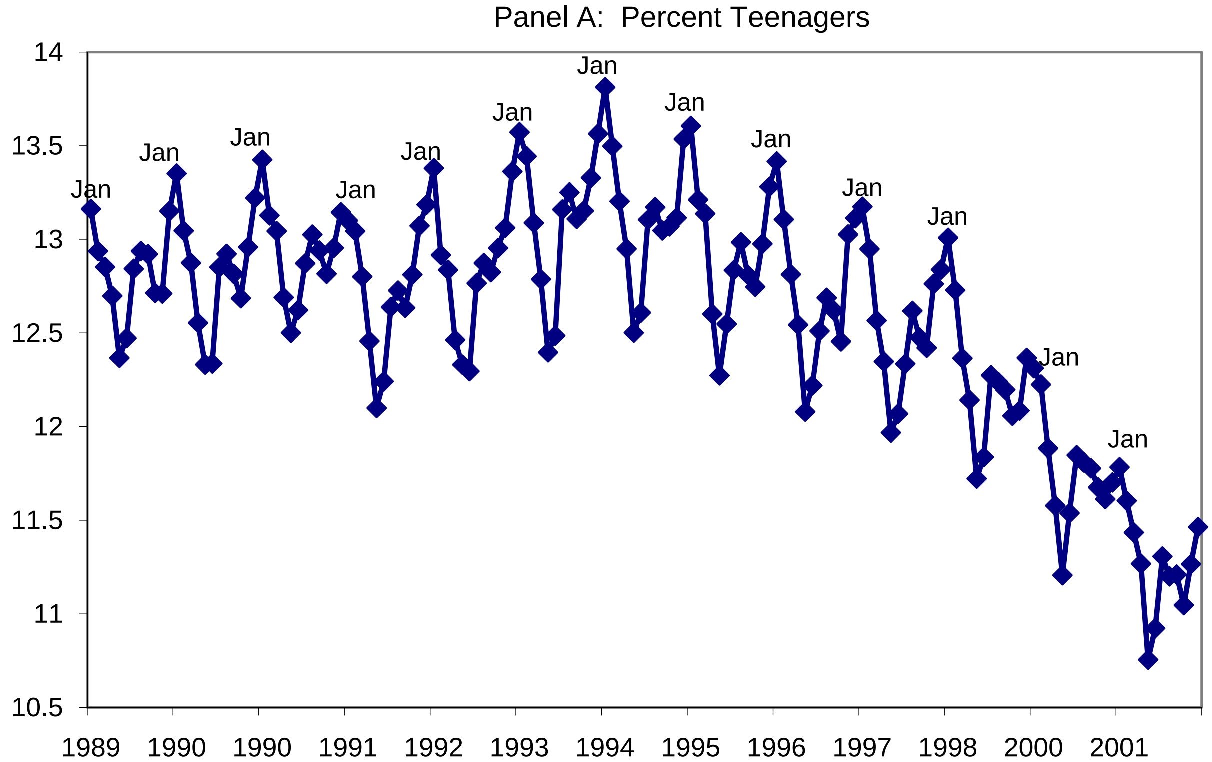
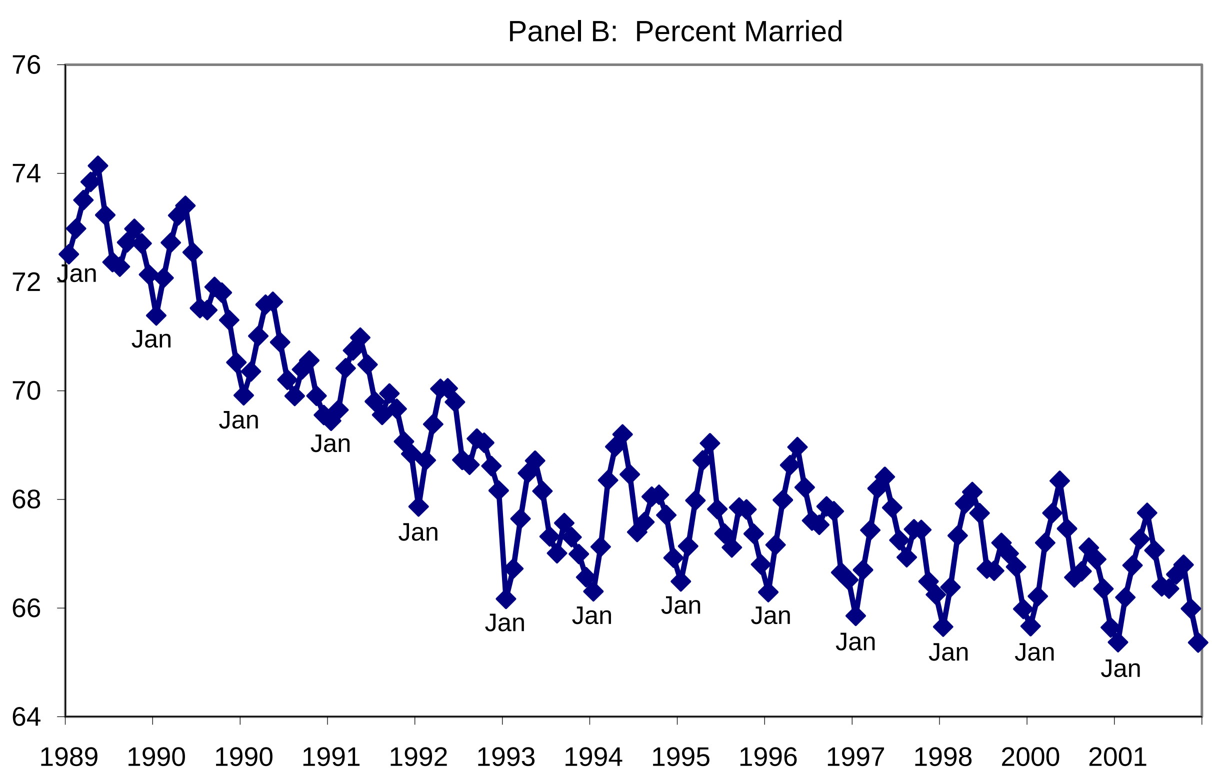
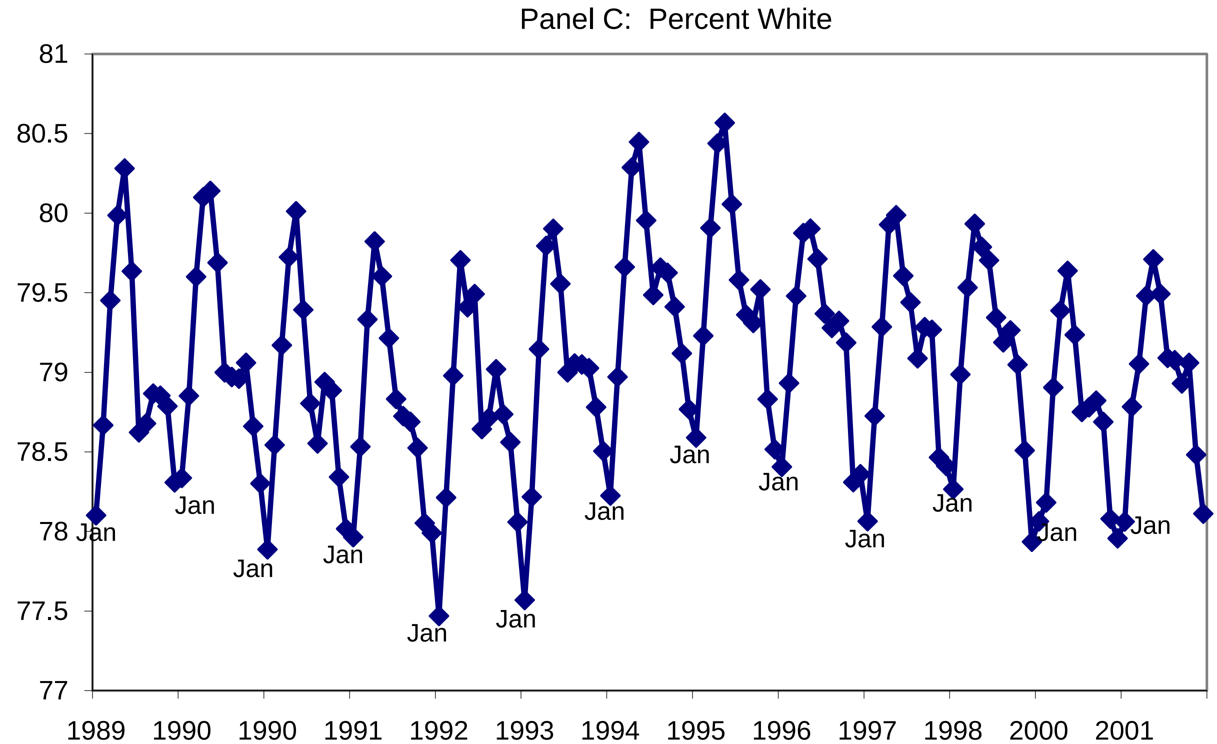
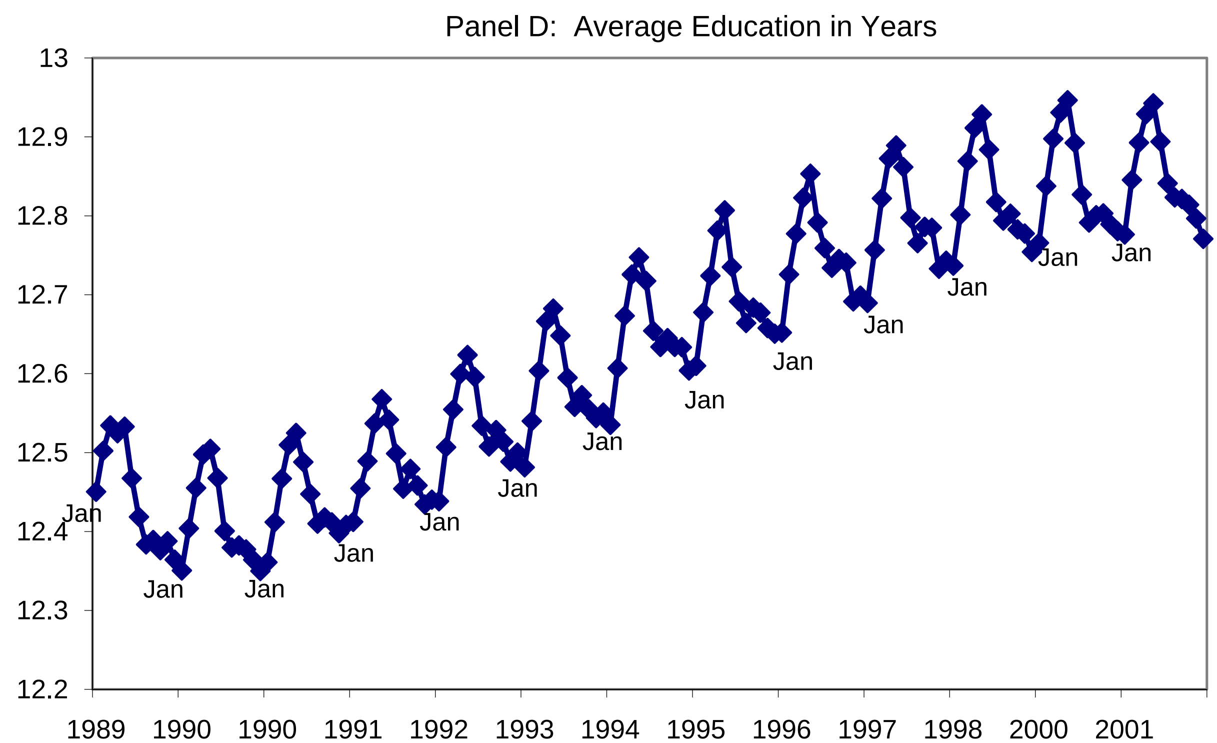
FIGURE 1. MATERNAL CHARACTERISTICS BY MONTH, NATALITY FILES, 1989-2001



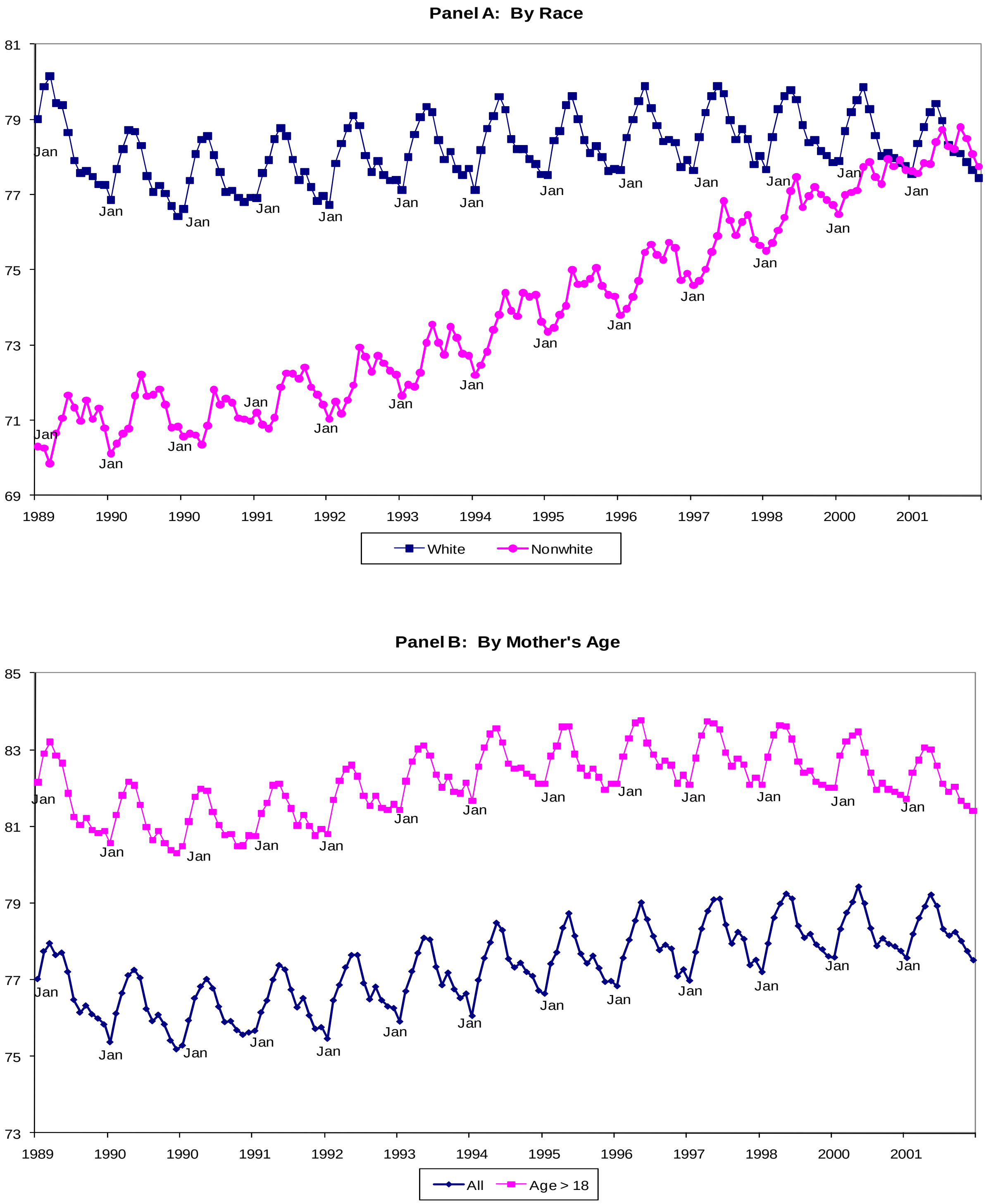






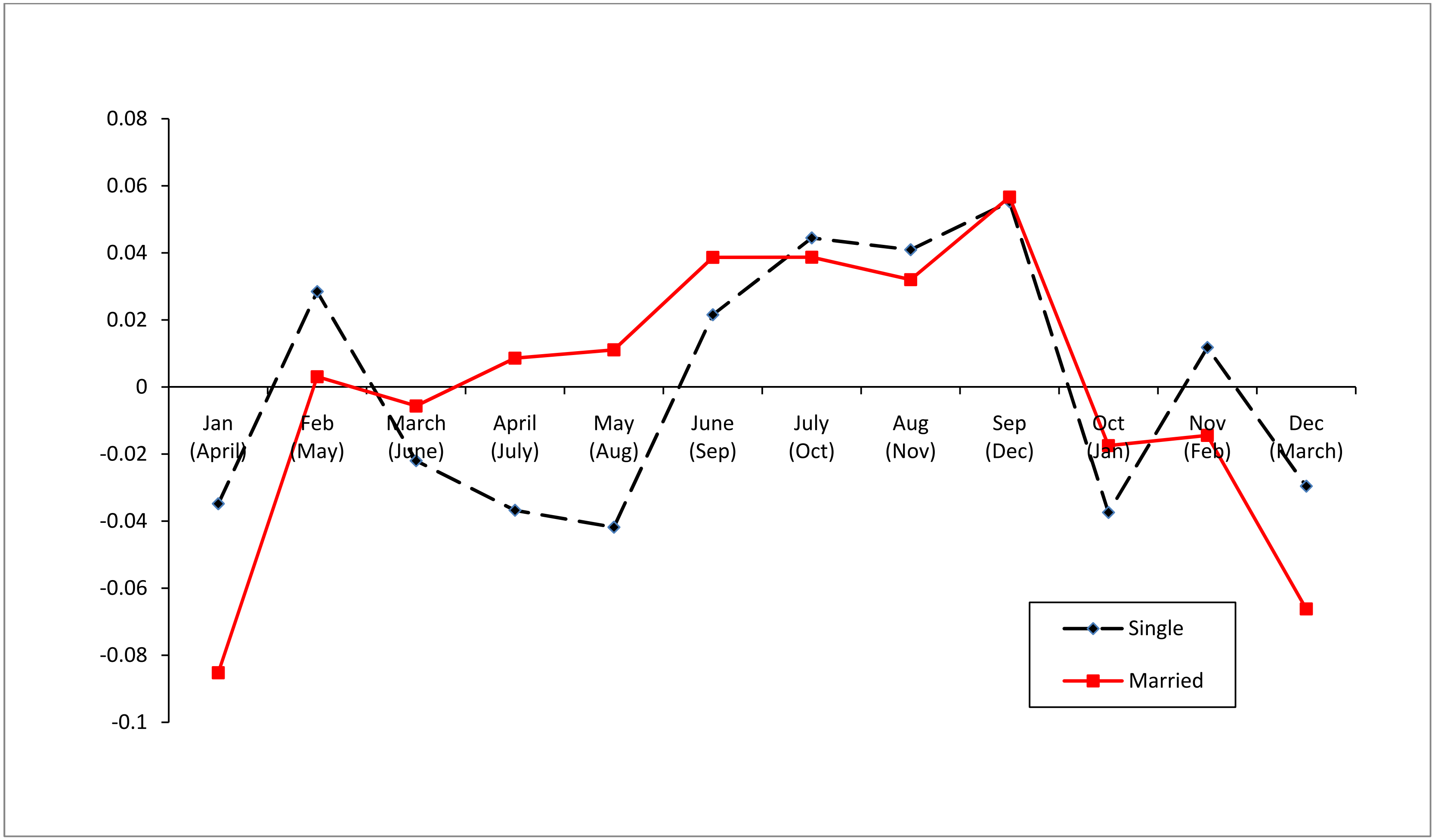
Notes: The sample for each figure includes all births in the Natality Detail Files from 1989-2001, for 52,041,054 observations.

FIGURE 2. PERCENT OF WOMEN GIVING BIRTH EACH MONTH WHO HAVE A HIGH SCHOOL DEGREE, NATALITY FILES, 1989-2001



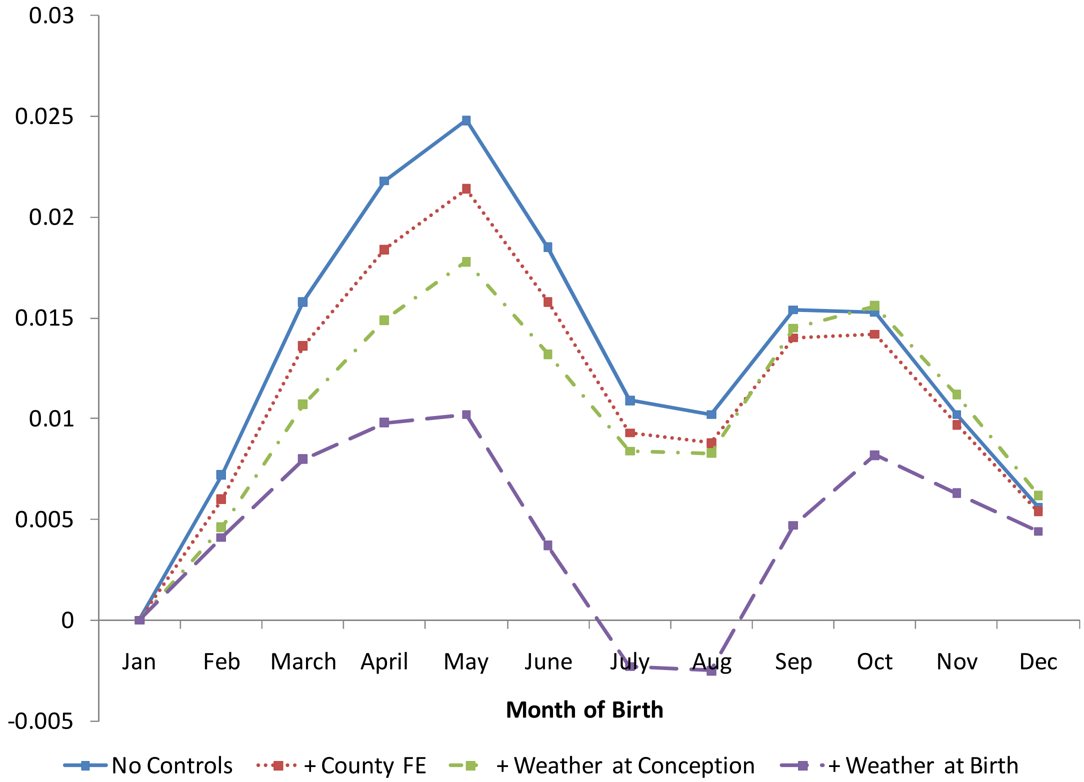
Notes: The sample for each figure includes all births in the Natality Detail Files with mother's education reported, from 1989-2001, for 50,660,895 observations. There are 46,524,641 births to women over 18.

FIGURE 3. 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 4. DECOMPOSITION OF EFFECT OF ADDITIONAL COVARIATES (FRACTION OF MOTHERS MARRIED)



Notes: Figure is based on results of Gelbach decomposition in Table 8; see Table 8 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'S CHARACTERISTICS BY MONTH: NATALITY FILES, 1989-2001

	Fraction of Moms Married	**Fraction of Moms White**	**Fraction Moms w/HS Degree**	**Fraction Moms Teenagers**
February	0.0070	0.0060	0.0073	-0.0024
	[0.0001]	[0.0001]	[0.0001]	[0.000]
March	0.0155	0.0127	0.0122	-0.0045
	[0.0001]	[0.0001]	[0.0001]	[0.000]
April	0.0219	0.0181	0.0163	-0.0074
	[0.0001]	[0.0001]	[0.0001]	[0.000]
May	0.0250	0.0189	0.0195	-0.0107
	[0.0001]	[0.0001]	[0.0001]	[0.000]
June	0.0185	0.0153	0.0174	-0.0093
	[0.0001]	[0.0001]	[0.0001]	[0.000]
July	0.0109	0.0102	0.0103	-0.0053
	[0.0001]	[0.0001]	[0.0001]	[0.000]
August	0.0102	0.0096	0.0068	-0.0043
	[0.0001]	[0.0001]	[0.0001]	[0.000]
September	0.0154	0.0103	0.0088	-0.0050
	[0.0001]	[0.0001]	[0.0001]	[0.000]
October	0.0154	0.0098	0.0055	-0.0054
	[0.0001]	[0.0001]	[0.0001]	[0.000]
November	0.0103	0.0050	0.0032	-0.0035
	[0.0001]	[0.0001]	[0.0001]	[0.000]
December	0.0056	0.0021	0.0025	-0.0011
	[0.0001]	[0.0001]	[0.0001]	[0.000]
Constant	0.7280	0.7818	0.7666	0.1331
	[0.0001]	[0.0001]	[0.0001]	[0.000]
F-stat for Month Dummies	20,172.35	6,001.24	17,363.73	22,390.43
Observations	52,041,054	52,041,054	50,660,895	52,041,054

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; the total number of observations is shown in the table. 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. INFANT HEALTH OUTCOMES BY BIRTH MONTH: NATALITY FILES, 1989-2001

	Birth Weight	**Low Birth Weight**	**Preterm**
February	12.2982	-0.0033	-0.0033
	[0.4240]	[0.0002]	[0.0002]
March	19.0584	-0.0049	-0.0048
	[0.4135]	[0.0002]	[0.0002]
April	23.3403	-0.0045	-0.0051
	[0.4197]	[0.0002]	[0.0002]
May	20.4308	-0.0036	-0.0016
	[0.4151]	[0.0002]	[0.0002]
June	12.768	-0.0018	0.0011
	[0.4167]	[0.0002]	[0.0002]
July	7.7108	-0.0019	-0.0024
	[0.4097]	[0.0002]	[0.0002]
August	9.0698	-0.0029	-0.0083]
	[0.4084]	[0.0002]	[0.0002]
September	13.3838	-0.0050	-0.0160
	[0.4100]	[0.0002]	[0.0002]
October	8.1768	-0.0020	-0.0033
	[0.4145]	[0.0002]	[0.0002]
November	7.8571	-0.0019	-0.0063
	[0.4208]	[0.0002]	[0.0002]
December	-0.8636	-0.0005	-0.0033
	[0.4169]	[0.0002]	[0.0002]
Constant	3333.704	0.0728	0.1102
	[0.4358]	[0.0002]	[0.0002]
F-stat for Month Dummies	643.78	167.16	928.64
Observations	51,981,365	51,981,365	51,498,912

Notes: Robust standard errors in brackets. Birth weight is measured in grams, low birth weight is defined as <2500 grams, and preterm is defined as gestation of less than 37 weeks. Each column is a separate regression, where the data were collapsed into county-month-year cells. The data were then weighted by cell size; the number of observations is shown in the table. The omitted month is January. All regressions include third-order polynomials for birth-month trends. The 1% critical value for the F-test is 2.25.

TABLE 3. 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	0.0126	0.0101	0.0105
	[0.0019]	[0.0007]	[0.0008]	[0.0002]
Third Birth Quarter	-0.0024	0.0025	0.0001	0.0015
	[0.0018]	[0.0007]	[0.0008]	[0.0002]
Fourth Birth Quarter	0.0002	0.0045	0.0003	-0.0034
	[0.0019]	[0.0007]	[0.0008]	[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	0.0048	0.0068	0.0142
	[0.0011]	[0.0005]	[0.0007]	[0.0002]
Third Birth Quarter	0.0003	0.0024	0.0028	0.0046
	[0.0010]	[0.0005]	[0.0007]	[0.0002]
Fourth Birth Quarter	0.0006	0.0032	0.0036	0.0029
	[0.0023]	[0.0005]	[0.0007]	[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	0.0083	0.0092	0.0111
	[0.0013]	[0.0005]	[0.0007]	[0.0002]
Third Birth Quarter	0.0032	0.0018	0.0007	0.0037
	[0.0012]	[0.0005]	[0.0006]	[0.0002]
Fourth Birth Quarter	0.0037	0.0048	0.0018	-0.0007
	[0.0012]	[0.0005]	[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	-0.0058	-0.0058
	[0.0017]	[0.0005]	[0.0006]
Third Birth Quarter	-0.0049	-0.0019	-0.0005
	[0.0016]	[0.0005]	[0.0006]
Fourth Birth Quarter	-0.0069	-0.0041	-0.0028
	[0.0016]	[0.0005]	[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 4. MATERNAL CHARACTERISTICS AND EDUCATION AND WAGE OUTCOMES: RESULTS FROM THE CENSUS

	Years of Schooling		Percent Dropouts		Wages, Logged		Wages, in Levels	
Second Birth Quarter	0.037	0.024	-0.123	-0.004	0.002	0.001	0.855	0.280
	[0.013]	[0.011]	[0.127]	[0.117]	[0.003]	[0.003]	[1.169]	[1.113]
Third Birth Quarter	0.055	0.041	-0.828	-0.680	0.012	0.010	4.184	3.365
	[0.012]	[0.011]	[0.124]	[0.114]	[0.003]	[0.003]	[1.223]	[1.167]
Fourth Birth Quarter	0.062	0.037	-0.868	-0.630	0.008	0.004	2.607	1.208
	[0.013]	[0.011]	[0.132]	[0.112]	[0.003]	[0.003]	[1.158]	[1.131]
Wald Test that Birth-Quarter Coefficients Are the Same	$\chi^2[3] = 30.91$		$\chi^2[3] = 32.67$		$\chi^2[3] = 18.89$		$\chi^2[3] = 16.28$	
Family Characteristics?	No	Yes	No	Yes	No	Yes	No	Yes
R-Squared	0.904	0.914	0.906	0.913	0.910	0.913	0.876	0.881
Age Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weights?	Yes	Yes	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, 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 log of average wages in the cell; using the average of logged wages produces qualitatively similar estimates. The age controls measure age in birth quarters.

TABLE 5. IV ESTIMATES OF THE RETURN TO EDUCATION: RESULTS FROM THE CENSUS

	Dependent Variable is Logged Wages				Dependent Variable is Wages in Levels			
	Baseline	w/State Dummies	w/Family Controls	w/Family Controls*Year	Baseline	w/State Dummies	w/Family Controls	w/Family Controls*Year
Years of Education	0.089	0.107	0.132	0.162	31.21	37.124	46.24	61.23
	[0.048]	[0.036]	[0.041]	[0.044]	[16.41]	[12.83]	[13.82]	[15.32]
Family*Year Controls?	No	No	No	Yes	No	No	No	Yes
Family Controls?	No	No	Yes	Yes	No	No	Yes	Yes
State Dummies?	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Age Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Instruments	YOB*QOB	YOB*QOB	YOB*QOB	YOB*QOB	YOB*QOB	YOB*QOB	YOB*QOB	YOB*QOB
Weights?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in brackets. The maternal characteristics and income controls are taken from the 1960 census; education and wage outcomes are taken from the 1980 census. Regressions are from cohorts of males born between 1943 and 1955 and age 16 or under in the 1960 census; the instruments are quarter of birth dummies interacted with year-of-birth dummies. The coefficients on years of education from the OLS estimates of the specifications in columns 1 and 5 are 0.088 [0.002] and 29.9 [0.87], respectively; for columns 4 and 8 the OLS estimates are 0.041 [0.007] and 16.6 [3.14]. Observations are state-of-birth/quarter-of-birth/year-of-birth cells and regressions weight by total individuals reporting positive earnings in a cell. The regressions have 2,596 cells totaling 927,954 individuals. The dependent variable in the first four regressions is the log of average wages in a cell; in the last four regressions it is the average of cell wages in levels. Wages are pre-tax wage and salary income over weeks worked. The years of education regressor is average number of years of completed education in a cell. The age controls include linear and quadratic trends and measure age in birth quarters. The year dummies are year-of-birth dummies. Family controls 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, fraction white, and average cell family income as a percent of the poverty line.

TABLE 6. FRACTION OF MOTHERS MARRIED IN THE NSFG BY WANTEDNESS OF BIRTH

<table>
<tr><th></th><th>All Births</th><th>Wanted Births</th><th>Unwanted Births</th><th>All Births</th><th>Wanted Births</th><th>Unwanted Births</th></tr>
<tr><td>February</td><td>0.0122
[0.0139]</td><td>0.00906
[0.0147]</td><td>0.00805
[0.0415]</td><td>-</td><td>-</td><td>-</td></tr>
<tr><td>March</td><td>0.0185
[0.0142]</td><td>0.0250
[0.0139]</td><td>-0.0749
[0.0511]</td><td rowspan="2">0.0187
[0.0101]</td><td rowspan="2">0.0252
[0.0102]</td><td rowspan="2">-0.0624
[0.0362]</td></tr>
<tr><td>April</td><td>0.0304
[0.0140]</td><td>0.0337
[0.0149]</td><td>-0.0415
[0.0463]</td></tr>
<tr><td>May</td><td>0.0187
[0.0135]</td><td>0.021
[0.0141]</td><td>-0.0131
[0.0399]</td><td rowspan="2">0.0173
[0.0098]</td><td rowspan="2">0.0210
[0.0103]</td><td rowspan="2">-0.014
[0.0295]</td></tr>
<tr><td>June</td><td>0.0269
[0.0133]</td><td>0.0292
[0.0143]</td><td>-0.0075
[0.0387]</td></tr>
<tr><td>July</td><td>0.0147
[0.0145]</td><td>0.00985
[0.0156]</td><td>0.0024
[0.0398]</td><td rowspan="2">0.0151
[0.0108]</td><td rowspan="2">0.0151
[0.0113]</td><td rowspan="2">-0.0098
[0.0305]</td></tr>
<tr><td>Aug</td><td>0.0269
[0.0136]</td><td>0.0288
[0.0141]</td><td>-0.0136
[0.0408]</td></tr>
<tr><td>September</td><td>0.00111
[0.0146]</td><td>0.0001
[0.0155]</td><td>0.0019
[0.0417]</td><td rowspan="2">0.0091
[0.0104]</td><td rowspan="2">0.0078
[0.0109]</td><td rowspan="2">0.0153
[0.0292]</td></tr>
<tr><td>October</td><td>0.0285
[0.0138]</td><td>0.0240
[0.0145]</td><td>0.0364
[0.0368]</td></tr>
<tr><td>November</td><td>0.0262
[0.0137]</td><td>0.0227
[0.0146]</td><td>-0.00352
[0.0419]</td><td rowspan="2">0.0184
[0.00969]</td><td rowspan="2">0.0187
[0.0102]</td><td rowspan="2">-0.0172
[0.0300]</td></tr>
<tr><td>December</td><td>0.0221
[0.0128]</td><td>0.0232
[0.0135]</td><td>-0.022
[0.0424]</td></tr>
</table>

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 7. FRACTION OF MOTHERS MARRIED IN THE NSFG BY WANTEDNESS OF BIRTH—ALTERNATE DEFINITION OF WANTEDNESS

<table>
<tr><th></th><th>All Births</th><th>Wanted Births</th><th>Unwanted Births</th><th>All Births</th><th>Wanted Births</th><th>Unwanted Births</th></tr>
<tr><td>February</td><td>0.0122
[0.0139]</td><td>0.0105
[0.0174]</td><td>0.0117
[0.0228]</td><td>-</td><td>-</td><td>-</td></tr>
<tr><td>March</td><td>0.0185
[0.0142]</td><td>0.0292
[0.0162]</td><td>-0.0157
[0.0260]</td><td rowspan="2">0.0187
[0.0101]</td><td rowspan="2">0.0305
[0.0120]</td><td rowspan="2">-0.0178
[0.0180]</td></tr>
<tr><td>April</td><td>0.0304
[0.0141]</td><td>0.0412
[0.0175]</td><td>-0.0079
[0.0237]</td></tr>
<tr><td>May</td><td>0.0183
[0.0135]</td><td>0.0267
[0.0165]</td><td>-0.0057
[0.0229]</td><td rowspan="2">0.0168
[0.00976]</td><td rowspan="2">0.0218
[0.0120]</td><td rowspan="2">-0.00182
[0.0163]</td></tr>
<tr><td>June</td><td>0.0264
[0.0133]</td><td>0.0267
[0.0169]</td><td>0.013
[0.0216]</td></tr>
<tr><td>July</td><td>0.0144
[0.0145]</td><td>0.0106
[0.0186]</td><td>0.0072
[0.0224]</td><td rowspan="2">0.0149
[0.0108]</td><td rowspan="2">0.0194
[0.0135]</td><td rowspan="2">-0.00249
[0.0163]</td></tr>
<tr><td>Aug</td><td>0.0268
[0.0136]</td><td>0.0382
[0.0165]</td><td>-0.0004
[0.0224]</td></tr>
<tr><td>September</td><td>0.0011
[0.0146]</td><td>0.0006
[0.0185]</td><td>0.0008
[0.0238]</td><td rowspan="2">0.0091
[0.0104]</td><td rowspan="2">0.0078
[0.0129]</td><td rowspan="2">0.0101
[0.0164]</td></tr>
<tr><td>October</td><td>0.0285
[0.0138]</td><td>0.0248
[0.0172]</td><td>0.0304
[0.0216]</td></tr>
<tr><td>November</td><td>0.0259
[0.0137]</td><td>0.0255
[0.0170]</td><td>0.0129
[0.0226]</td><td rowspan="2">0.0182
[0.00970]</td><td rowspan="2">0.0207
[0.0119]</td><td rowspan="2">0.0032
[0.0160]</td></tr>
<tr><td>December</td><td>0.0220
[0.0128]</td><td>0.0257
[0.0157]</td><td>0.00493
[0.0224]</td></tr>
</table>

Notes: Observations: 35,395. 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 0.159 [0.0232]). Columns 5 and 6 are also from a single regression, the marginal effect for a wanted birth for this regression is 0.158 [0.0168]. A birth is defined as wanted if the woman was not using contraception at the time of conception and her stated reason for doing so was that she wanted to get pregnant. The likelihood-ratio test that the coefficients in column 2 equal the coefficients in column 3 is not rejected; $\chi^2[11]=15.17$, p = 0.175; the same test for columns 5 and 6 yields $\chi^2[5]=11.82$, p = 0.0373. Regressions include a 3rd-order monthly trend and a dummy for interview year.

TABLE 8. 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	0.0041	0.0031	0.0012	0.0014	0.0005
	[0.0005]	[0.0008]	[0.0005]	[0.0002]	[0.0004]	[0.0001]
March	0.0158	0.0080	0.0078	0.0022	0.0029	0.0027
	[0.0009]	[0.0016]	[0.0011]	[0.0003]	[0.0009]	[0.0004]
April	0.0218	0.0098	0.0119	0.0034	0.0035	0.0051
	[0.0011]	[0.0021]	[0.0016]	[0.0004]	[0.0012]	[0.0006]
May	0.0248	0.0103	0.0145	0.0034	0.0036	0.0076
	[0.0010]	[0.0022]	[0.0018]	[0.0004]	[0.0012]	[0.0009]
June	0.0185	0.0036	0.0148	0.0027	0.0026	0.0095
	[0.0010]	[0.0021]	[0.0018]	[0.0003]	[0.0009]	[0.0012]
July	0.0109	-0.0023	0.0132	0.0016	0.0009	0.0107
	[0.0007]	[0.0018]	[0.0016]	[0.0002]	[0.0004]	[0.0014]
August	0.0102	-0.0025	0.0126	0.0014	0.0005	0.0108
	[0.0008]	[0.0015]	[0.0014]	[0.0003]	[0.0002]	[0.0014]
Sept.	0.0154	0.0046	0.0108	0.0014	-0.0005	0.0098
	[0.0010]	[0.0012]	[0.0013]	[0.0003]	[0.0006]	[0.0012]
October	0.0153	0.0082	0.0071	0.0011	-0.0014	0.0074
	[0.0009]	[0.0010]	[0.0011]	[0.0003]	[0.0009]	[0.0009]
Nov.	0.0102	0.0063	0.0039	0.0005	-0.0015	0.0049
	[0.0008]	[0.0008]	[0.0009]	[0.0002]	[0.0008]	[0.0006]
Dec.	0.0056	0.0045	0.0011	0.0002	-0.0008	0.0018
	[0.0006]	[0.0006]	[0.0004]	[0.0002]	[0.0004]	[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.

TABLE 9. FRACTION OF WANTED BIRTHS CORRECTLY TIMED BY MARITAL STATUS

	Married	Unmarried	Married	Unmarried
February	-0.00783 [0.0212]	-0.0222 [0.0317]	-	-
March	0.0297 [0.0200]	-0.0384 [0.0353]	0.0276 [0.0150]	-0.0071 [0.0239]
April	0.0176 [0.0216]	0.00286 [0.0337]		
May	0.0391 [0.0196]	0.0384 [0.0306]	0.0406 [0.0142]	0.0219 [0.0229]
June	0.0345 [0.0200]	-0.0148 [0.0348]		
July	0.0343 [0.0201]	-0.0332 [0.0354]	0.0271 [0.0154]	-0.0153 [0.0237]
Aug	0.0125 [0.0217]	-0.0185 [0.0317]		
September	-0.0144 [0.0203]	-0.0129 [0.0335]	-0.0134 [0.0144]	0.0017 [0.0238]
October	-0.0201 [0.0209]	-0.00486 [0.0337]		
November	-0.0115 [0.0212]	-0.0183 [0.0348]	-0.0017 [0.0147]	0.004 [0.0238]
December	0.00001 [0.0201]	0.00412 [0.0327]		

Notes: Observations: 30,787. 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 was correctly timed. The coefficients in columns 1 and 2 are all from a single regression (that also includes a non-interacted dummy for whether a birth was to a married mother; the marginal effect of this variable is 0.23 [0.027]). Columns 3 and 4 are also from a single regression, the marginal effect for a married birth for this regression is 0.24 [0.0194]). The likelihood-ratio test that the coefficients in column 1 equal the coefficients in column 2 is rejected; $\chi^2[11]=17.07$, p = 0.106; the same test for columns 3 and 4 yields $\chi^2[5]=10.42$, p = 0.064. Regressions include a 3rd-order monthly trend and a dummy for interview year.