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

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Abstract

Research has consistently found that the month of a child's birth is associated with later outcomes involving health, education, and earnings; what drives this association remains unclear. In this paper we consider a new and previously-overlooked explanation—that children born at different times in the year are conceived by women with different socioeconomic characteristics. Using live birth certificate data, we document highly repetitive seasonal changes in the characteristics of women giving birth throughout the year. Children born in the winter are disproportionally born to women who are more likely to be teenagers, and less likely to be married or have a high school degree. These differences are large; for instance, the fraction of children born to teenagers typically declines by 10 percent from January to May. Using census data, we show that these patterns have been present since at least the 1950s. We also show that a parsimonious set of family background characteristics can explain up to 45 percent of the relationship between season of birth and adult schooling and wage outcomes. However, we estimate returns to education using quarter-of-birth as an instrumental variable, and find that the results are robust to the omission of these characteristics.

Introduction

Research 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. Prior explanations for this phenomenon consider social and natural factors (such as compulsory schooling laws, changes in climate, or exposure to illness) that might affect children born in the winter in particular ways. These explanations often implicitly, and in many cases explicitly, assume that children born at different times of the year are initially similar, and that factors intervene after conception or birth to create differences in outcomes.

In this paper we consider an alternative explanation—that children born throughout the year are not similar absent intervening factors but are conceived by women with different socioeconomic characteristics. Using data from live birth certificates, we 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 do look different: they are younger, less educated, and less likely to be white or to be married.

We do not think it is hyperbole to describe the magnitude of these differences as extremely large. For example, we find that the fraction of children born to women without a high school degree decreases by over 2 percentage points—almost a 10 percent effect—between January and 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 of a one-percentage-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 10 percent effect is about as large as the decline in the annual fraction of children born to teenagers observed between 1989 and 2000. These effects are observable for children born throughout the second half of the twentieth century.

We then use census data to see whether or not variation in family background characteristics can account for much of the difference in outcomes typically ascribed to season of birth. Our estimates suggest that a reasonably parsimonious set of family background controls can significantly reduce differences in education and earnings between people born in different quarters of the year. Our controls generally reduce the magnitude of season of birth by 25 to 45 percent; the reduction is statistically significant.

Perhaps the most common use of season-of-birth-related controls in economics is in the returns to education literature, where these controls are used as instrumental variables. While the validity of these instruments has been criticized at least since work by Bound, Jaeger and Baker (1995), they remain in use and we find it instructive to see how sensitive these IV results are to whether family-background controls are included. Perhaps surprisingly, we find that the estimates are almost totally unchanged when family background controls are included; this is due in part to the family controls attenuating both the reduced-form and the first-stage coefficients for season of birth at the same time.

The remainder of the paper is organized as follows. Section 2 provides some background on season of birth and later outcomes. Section 3 examines season of birth and mothers' characteristics. Section 4 looks at how family background controls can explain season of birth relation to later outcomes. Section 5 concludes.

2. Season of Birth and Later Outcomes

Researchers have long recognized that the month of a child's birth is associated with later outcomes such as test performance, wages, and educational attainment.[1] 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.

[1] Examples include Angrist and Krueger (1991 and 1992); Bound and Jaeger (2000); Bound, Jaeger and Baker (1995); Cascio and Lewis (2006); Chamberlain and Imbens (2004); Chernozhukov and Hansen (2006); Chesher (2007); Cruz and Moreira (2005); Donald and Newey (2001); Dufour and Taamouti (2007); Honoré and Hu (2004); Hoogerheide, Kleibergen, and van Dijk (2007); Kleibergen (2002); Plug (2001); Staiger and Stock (1997).

A large body of research outside of economics has proven that season of birth is associated with health outcomes such as developing schizophrenia (Tochigi et al., 2004; Davies et al., 2003; Torrey et al., 1997; and Watson et al., 1984), 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 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 (e.g., are more likely to be schizophrenic, more likely to be very shy, less likely to be lucky) than other children.[2]

It remains unclear why these seasonal relationships exist. Most prior explanations for these relationships 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 laws on wages.

Researchers (especially Bound and Jaeger, 2000; and Bound, Jaeger and Baker, 1995) have cast doubts on Angrist and Krueger's assumption that these laws are the *only* reason schooling and wages change with season of birth. These critics have noted that differences in health outcomes are likely not driven by schooling laws, and that worse outcomes among winter births can be observed in situations

[2] Some of these studies are international in focus. While seasonality in birth has been documented in other countries, international seasonal relationships sometimes differ than 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 US case.

where no schooling laws were in place.[3] Also, researchers have pointed out that other phenomena, such as in-utero exposure to weather (Gortmaker et al., 1997) or illness (Almond, 2006; Sham et al., 1992; Suvisaari et al., 1999) may help to explain why winter births have worse 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 (Tarnowski et al., 1990; Plug, 2001; Williams et al., 1970).

None of these alternative explanations, however, seriously consider the possibility that children born in the winter are different from other children *at conception*, even without subsequent effects of climate or school laws. Moreover, many researchers continue to assume that children conceived throughout the year are indeed initially similar; this is especially true for work estimating the impact of educational attainment on wages or income. This work often uses season of birth as an instrument when comparing different estimation methods; these comparisons necessitate that season of birth is unrelated to wages except through its impact on schooling. Hoogerheide et al. (2007) write, “for quarter of birth to be a valid instrument it should only influence income through its effect on education. This is a plausible assumption, as one’s birthday is unlikely to be correlated with personal attributes other than age at school entry” (pg. 79). Kleibergen (2002) writes, “Quarter of birth related variables can serve as instruments since the quarter of birth is randomly distributed over the population” (pg. 1795). Papers making similar assumptions include Cruz and Moreira (2005), Staiger and Stock (1997), Chamberlain and Imbens (2004), Chernozhukov and Hansen (2006), and Dufour and Taamouti (2007).

We will hypothesize that children born in different seasons are conceived by different groups of women. To be clear, it is possible that this alternative explanation would be a complement, rather than a substitute, for existing explanations of season of birth’s impact on outcomes. We think that intervening

[3] Researchers have also criticized the performance of Angrist and Krueger’s instruments on statistical (rather than conceptual) grounds, showing that the quarter-of-birth instruments are in some specifications weakly significant in the first stage and thus subject to bias. As we show below, this criticism is less relevant here given the specifications we choose in the IV regressions.

phenomena such as schooling laws and exposure to influenza might help explain season of birth's association with later outcomes.

But evidence of this alternative explanation—for instance, evidence showing that children born in the winter are much more likely to be born to teenage mothers or unmarried mothers—would be important both because it may help explain a widely-noted but poorly-understood relationship between outcomes and season of birth, and also because it would present a serious challenge to research assuming that season of birth is randomly distributed across the population. We know of no research using recent U.S. data and no research at all in economics which rigorously investigates the hypothesis that children conceived at different times of year are different.[4] In the next section we provide such an investigation.

Section 3: Season of Birth and Mother's Characteristics

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.

The Natality Detail Files provide information on a number of maternal characteristics, including marital status, age, and education. This dataset is ideal for our study, because the information is taken at birth and therefore will not be affected by later interventions. Thus, any observed differences in the characteristics of mothers with births in different months must be due to pre-birth factors.

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[4] There is a small and inconclusive of research outside of economics which use 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 highly-educated low-density census tracts in Fulton County, Georgia, have less seasonality in conception than other women. Pasamanick et al. (1960) look at births to women 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. 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.

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, and Panel C shows the percent of women giving birth who are white. Panels A, B, and C each depict a clear seasonal pattern that is highly persistent across years. Children born in the winter are more likely to be born to a teenage mother, less likely to be born to a married mother, and more likely to be born to a mother who is not white.[5]

These seasonal trends are strikingly large. For instance, Panel A shows that the fraction 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 married from January to May seen in Panel B is about two percentage points on average. In Panel C, we see that the percent of mothers who are white is about two percentage points higher in May than in January.

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 even within racial groups.

[5] A few colleagues have questioned whether standard errors are needed in the figure since (in theory) we are using the population of births in the United States from 1989 to 2001. While the conceptual need for confidence intervals in the figure may be debatable, from a practical standpoint the confidence intervals are so small as to be indistinguishable from the trends depicted; consequently they are omitted.

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, nearly a 10 percent effect. By way of comparison, this 2 percentage point effect on the fraction of mothers without a high school degree 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 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 on 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 aggregate the data into county/month-of-birth/year-of-birth cells. Using cell *c* as the unit of observation we estimate

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

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 (d) teenage mothers or (e) the average birth-weight, in grams, of children in the cell. The term "month" in equation (1) represents a set of 11 dummy variables for month of birth (with one month omitted), the term θ_y represents a third-order polynomial for birth-month trends, and the term ε_i is noise.

The estimates can be seen in the regression results in Table 1 (with January as the omitted month). Regressions are weighted by cell size and robust standard errors clustered by county are reported in brackets. 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 SES mothers, and the peak is in May. The estimates are similar in magnitude to what one might infer from Figures 1 and 2.

In Table 1, we also explore whether there are seasonal patterns in infant birth weight. We are interested in birth weight because it can be considered a very early "outcome" that is associated with mothers' characteristics, and because research has shown that weight at birth is associated with long term well-being (Behrman and Rosenzweig 2004; Black, Devereux, and Salvanes 2005; Case, Paxson, and Fertig 2005). Birth weight exhibits a seasonal pattern similar to the one for mothers' characteristics: babies born in spring have higher birth weights on average than those born in winter. Infants born in April weigh 23.6 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 the estimated effect of AFDC participation for blacks.

Are these results driven by differences in regional birth patterns throughout the year? Table 2 addresses this question by adding county fixed effects to equation (1). The results are somewhat smaller—especially for the spring months. But from the summer on, the coefficients are close to before and still clearly significant. The patterns here are thus not primarily driven by regional differences in births during the year but instead are mostly driven by within-county changes during the year.

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.

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).[6] 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. As

[6] 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.

before, all regressions include third-order polynomials for birth-quarter trends. For each outcome, the regressions for each census year are run separately.

Table 3 reports results from regressing quarter-of-birth dummies (and time trends) on a number of outcomes; the omitted quarter is the first quarter of the year. Panel A reports the results from a linear probability regression on the likelihood that a child's mother has a high-school degree; all of the coefficients in all of 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 other two 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. These results are also very similar to those found in the Natality Detail Files for 1989-2001; that the census results are slightly smaller in magnitude reflects the fact that using quarters rather than months masks monthly within-quarter variation.

Panel B considers the fraction of children whose mothers were 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 in this case the results are consistent for all quarters and for all census years. For both Panels B and C the estimated effects are about one percent of the mean or less in magnitude; although again these results may underestimate the magnitude of seasonality's relation to family background since the Vital Statistics results show significant variation within birth quarters. 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 in an impoverished household 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, although the results for the second and third quarter appear in some cases to be stronger in later years. In the next section 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.

Section 4. Implications for Estimating Returns to Educations

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.

We then consider whether variation in the types of children born throughout the year would bias returns-to-education estimates that use quarter of birth as an instrument. A number of other papers have questioned the validity of quarter of birth as an instrument in this context, especially Bound and Jaeger (2000) and Bound, Jaeger and Baker, (1995). However, we feel testing quarter of birth as an instrument in this context is worthwhile for at least two reasons. First, despite over a decade of criticism quarter of birth remains a popular instrument in this literature, as discussed in Section 2. Second, we will not simply provide evidence that quarter of birth likely fails the exclusion restriction, but we will also directly test to what extent this violation appears to bias estimates of returns to education.

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. Additionally, adults living at home with their parents are a highly-selected (and for our purposes unappealing) group. For most adults in the census information on family background characteristics is very limited.

To confront this problem, we combine information on cohorts of individuals across multiple census years, where cohorts 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, as mentioned earlier, 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.[7] We then match this information to information on the outcomes realized by cohorts as of the 1980 census (the latest available year), when individuals are ages 20 to 36. This combination of cohorts across census years is similar in spirit to Angrist and Krueger (1992).[8] Following prior work, we restrict our attention to cohorts of males.

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

$$Outcome_c = \alpha + \beta \mathrm{Q} + \gamma \phi_{\mathrm{s}} + \lambda_1 age + \lambda_1 age^2 + \varepsilon \qquad (2)$$

and

$$Outcome_c = \alpha + \beta \mathrm{Q} + \delta \mathrm{X}_{\mathrm{c}} + \gamma \phi_{\mathrm{s}} + \lambda_1 age + \lambda_1 age^2 + \varepsilon \qquad (3)$$

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[7] 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.

[8] One might wonder how best to interpret results from younger individuals for whom wage information may not perfectly predict lifetime income. The cohorts used here are as old as possible while still allowing us to measure family characteristics in 1960. The results shown below are similar if the sample is restricted to (for instance) cohorts born in 1955 or earlier and thus observed in 1980 when schooling has largely been completed. For our youngest cohort—those born in 1960—87 percent of individuals on average report positive earnings in 1980, suggesting that our sample sizes are sufficiently large for including all available cohorts.

where the dependent variable $Outcome_c$ is either (a) the average years of school obtained by individuals in cohort *c* (b) the percent of individuals in *c* without a high-school degree (c) the log of average wages [9] for cohort *c* or (d) average wages (in levels) for cohort *c*.[10] 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, and *age* and age^2 are linear and quadratic controls for age (measured in birth quarters).

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 average mother's education in cohort *c*, fraction of mothers in *c* without a high-school degree, average mother's age at birth in *c*, fraction of mothers in *c* giving birth as teenagers, fraction of mothers in *c* working, fraction of mothers in *c* married, percent white in *c*, and average family income as a percent of the poverty line in *c*. Maternal controls are measures for *c* as of 1960 and family income is for 1959.

For both equations (2) and (3), the coefficient for quarter of birth *q*, which we can denote β_q, reports the difference in the likelihood of a given outcome occurring for a child born in quarter *q* relative to the omitted quarter. By adding family-background controls in equation (3), we can see whether seasonal patterns in outcomes are partly explicable by variation in family characteristics between those born at different times of year.

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 in equation (2). Alternatively, if one of these

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[9] Using the average of logged wages, instead of the log of average wages, produces similar results to those shown below.

[10] 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.

conditions is met, then equation (2) is correctly specified and estimates of (2) will not only be consistent but will also be efficient, since they would exclude the superfluous variables added into equation (3). A Hausman test can thus be performed to compare the models estimated by (2) and (3) to see whether the coefficients on quarter of birth estimated in the two models are statistically significantly different from each other.

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. The regressions are for cohorts of males ages 16 and under as of the 1960 census. Regressions are weighted by cohort size.[11] All regressions include state of birth and age controls.[12] 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 the canonical paper by Angrist and Krueger (1991).[13] However, column 2 shows that these effects are made significantly smaller by adding

[11] Cohort size is taken from the 1980 census. The correlation between cohort sizes in the two census years is over 0.99 and using either year to weight the data gives (not surprisingly) very similar estimates. The education regressions weight by total individuals in a cohort; the wages/earnings regressions weight by total individuals reporting positive earnings in a cohort. The regressions on wages have 3,463 cohorts totaling 1,295,279 individuals; the regressions on education have 3,463 cohorts totaling 1,459,473 individuals.
[12] Omitting these controls does not qualitatively alter the results.
[13] See the second line of Table I in their paper for the most comparable regression (although note they exclude the fourth-quarter dummy).

controls for family characteristics; the decline in the estimates ranges from 23 percent to nearly 50 percent. A Wald test strongly rejects that the coefficients are the same in each column.[14]

The next two columns look at the fraction of individuals in a cohort who have not completed high school. The first set of results is again similar in magnitude to estimates from past work and again 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 even more dramatic when looking at wages in levels, where for two quarter-of-birth coefficients the result is essentially eliminated by controlling for family characteristics. (The average wage in the sample is about $300, 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 columns 4 and 5.) The differences in coefficients are in all cases significant at the one-percent level.

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

The magnitudes of the changes in Table 4 suggest that instrumental-variable estimates using quarter of birth could potentially be very sensitive to specification. Table 5 shows 2SLS estimates regressing average cohort's wages (in levels and logs) on education, where education is instrumented by

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

quarter of birth.[15] The table also shows the *F*-statistics from tests that the excluded instruments are jointly zero in the first stage. The results of these *F* tests are fairly close the thresholds often used for identifying weak instruments (Stock, Wright, and Yogo, 2002) but are not clearly problematic.

Interestingly, the results are almost unchanged regardless of whether or not family background controls are added. The estimates using logged average wage are extremely close and in both cases are about 0.17; a reasonably large estimate. The estimates using wages in levels imply a proportionate response of about 14 percent at the mean, which is similar and slightly smaller. But in both cases the results are essentially identical regardless of whether or not family background controls are included.

Why are the results so insensitive to these controls? Looking back to Table 4, one can regard the regressions on years of schooling as the first-stage regression and the regressions on wages as the reduced form. It is clear from the table that the inclusion of family background controls weakens quarter-of-birth coefficients in both cases. One can conceptualize the IV coefficients as being determined by the ratio of the reduced-form and first-stage coefficients, so that reductions in both may lead to little change for the second-stage coefficients. The results here thus suggest that while maternal and other family characteristics can explain much of the relationship between season of birth and later outcomes, work on the returns to education may not be seriously biased from failing to control for family background. Whether and to what extent other confounding phenomena must be accounted for we leave to future research.

Conclusions

Research throughout the social and natural sciences has demonstrated an association between the month of a child's birth and a variety of later outcomes. In this paper, we document large and regular seasonal changes in the socioeconomic characteristics of women giving birth and we show that these seasonal changes can account for a large portion of the relationship between season of birth and other

[15] It is common in the literature to use quarter of birth*year of birth dummies as instruments. Doing so does not change the essential finding of the table; we use simpler estimates since they facilitate direct comparison to the results in Table 4 (which shows first-stage and reduced-form estimates).

outcomes. However, our estimates of the return to education that use quarter-of-birth as an instrument are practically invariant to the inclusion of family background characteristics.

These results raise a large number of unanswered questions. First, why are different women likely to give birth (and conceive) at different times of year? We have conducted preliminary work showing that regional differences in conception patterns and changes in weather (which is often considered an important factor in research on seasonality of conception, cf. Lam and Miron, 1996) cannot explain these patterns. This result is suggested by Table 2—adding county-level fixed effects did not significantly attenuate season of birth patterns. A second hypothesis is that high socioeconomic status women either have stronger preferences for births in the spring and summer, or perhaps are better at timing births than are other women. These and other explanations are left to ongoing and future research.

These results also raise the question of whether the apparent failure of quarter-of-birth instruments in satisfying the exclusion restriction has practical importance. We have proven in this paper that quarter-of-birth is associated with a host of family background characteristics, despite recent and explicit claims to the contrary. But instrumental variable regressions are unchanged regardless of whether observables for family background are included. Whether unobservable differences in family background drive IV estimates, or whether the residual effect of season of birth on education is suitable for identification, remains unanswered.

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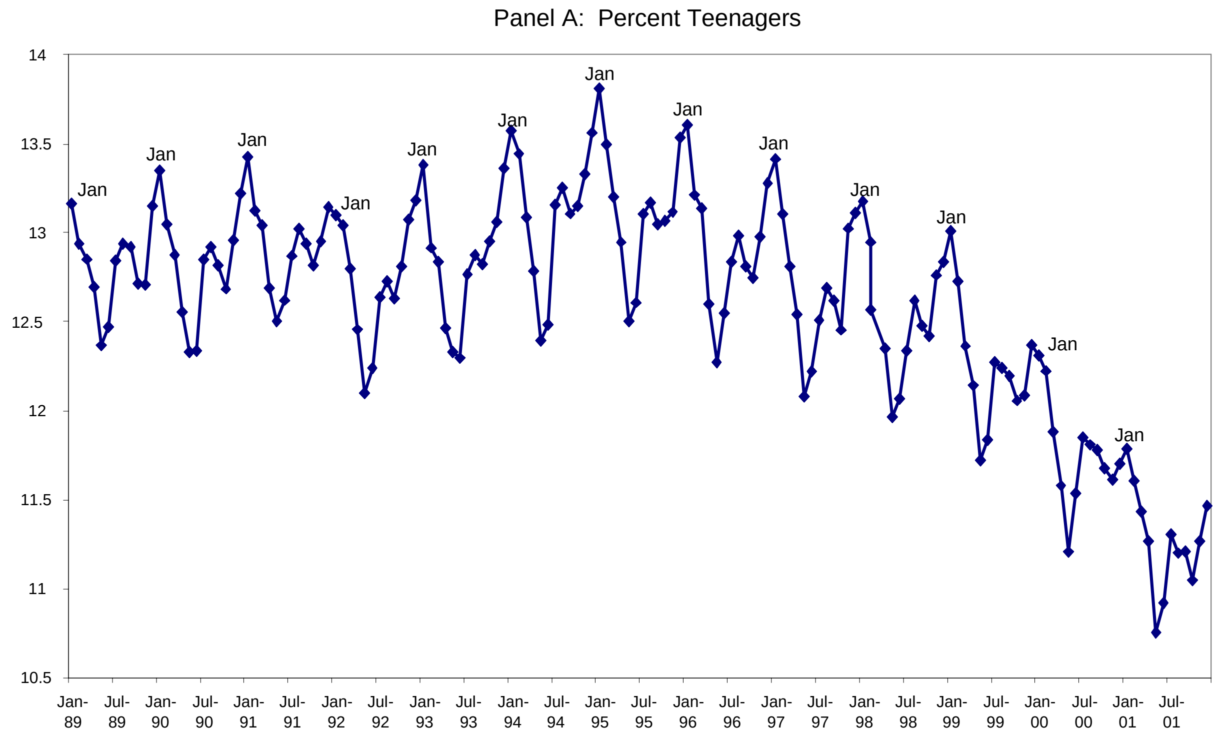
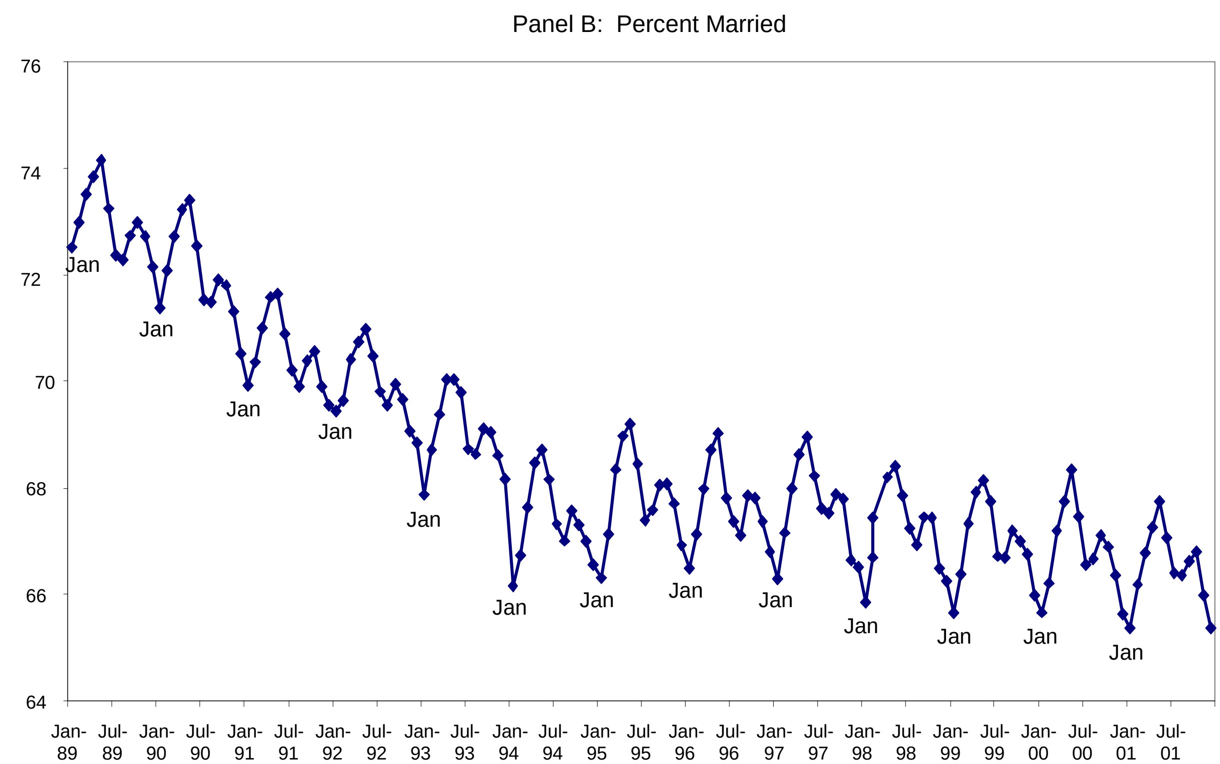
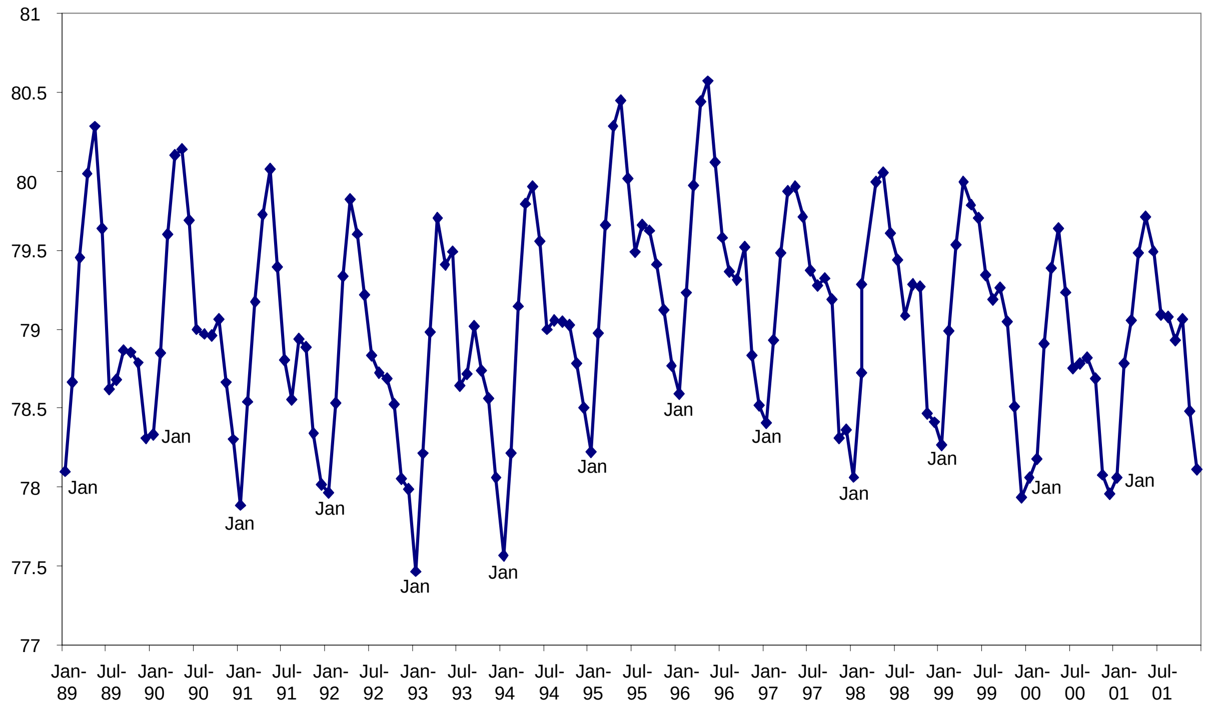
Figure 1: Percent of Women Giving Birth Each Month with a Given Characteristic: Natality Detail Files, 1989-2001



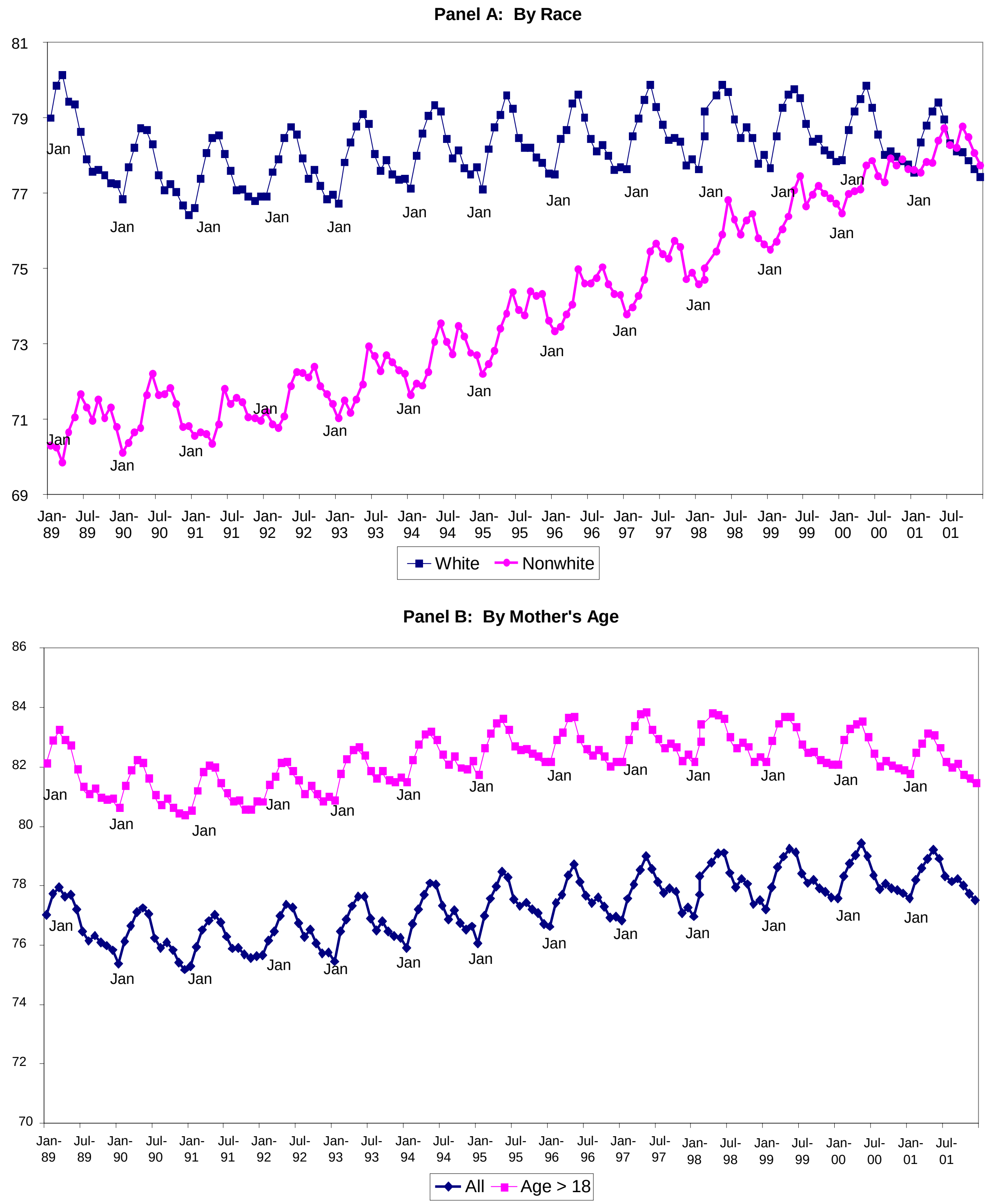


Panel C: Percent White



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 Detail Files, 1989-2001



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.

Table 1: Mother's and Infant's Characteristics by Birth Month: Natality Detail Files, 1989-2001

	Fraction of Moms Married	Fraction of Moms White	Fraction Moms w/HS Degree	Fraction Moms Teenagers	Child Birth Weight
February	0.0070	0.0060	0.0073	-0.0024	12.3031
	[0.0001]	[0.0001]	[0.0001]	[0.000]	[0.049]
March	0.0155	0.0127	0.0122	-0.0045	19.0414
	[0.0001]	[0.0001]	[0.0001]	[0.000]	[0.047]
April	0.0219	0.0181	0.0163	-0.0074	23.6027
	[0.0001]	[0.0001]	[0.0001]	[0.000]	[0.049]
May	0.0250	0.0189	0.0195	-0.0107	21.0483
	[0.0001]	[0.0001]	[0.0001]	[0.000]	[0.049]
June	0.0185	0.0153	0.0174	-0.0093	13.3318
	[0.0001]	[0.0001]	[0.0001]	[0.000]	[0.049]
July	0.0109	0.0102	0.0103	-0.0053	8.1252
	[0.0001]	[0.0001]	[0.0001]	[0.000]	[0.048]
August	0.0102	0.0096	0.0068	-0.0043	9.0711
	[0.0001]	[0.0001]	[0.0001]	[0.000]	[0.047]
September	0.0154	0.0103	0.0088	-0.0050	13.2300
	[0.0001]	[0.0001]	[0.0001]	[0.000]	[0.047]
October	0.0154	0.0098	0.0055	-0.0054	7.9639
	[0.0001]	[0.0001]	[0.0001]	[0.000]	[0.048]
November	0.0103	0.0050	0.0032	-0.0035	8.1081
	[0.0001]	[0.0001]	[0.0001]	[0.000]	[0.049]
December	0.0056	0.0021	0.0025	-0.0011	0.0704
	[0.0001]	[0.0001]	[0.0001]	[0.000]	[0.048]
Constant	0.7280	0.7818	0.7666	0.1331	3349.6
	[0.0001]	[0.0001]	[0.0001]	[0.000]	[0.067]
F-stat For Month Dummies	20172.35	6001.24	17363.73	22390.43	46930.04
Observations	52,041,054	52,041,054	50,660,895	52,041,054	52,158,278

Robust standard errors in brackets. Each column is a separate regression, where the data was collapsed into county-month-year cells. The data were then weighted by cell size, where the number of observations represented 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 2: Mother's and Infant's Characteristics by Birth Month, with County Fixed Effects: Natality Detail Files, 1989-2001

	Fraction of Moms Married	Fraction of Moms White	Fraction Moms w/HS Degree	Fraction Moms Teenagers	Child Birth Weight
February	0.0060	0.0045	0.0058	-0.0019	11.5531
	[0.0004]	[0.0004]	[0.0008]	[0.0002]	[0.571]
March	0.0133	0.0097	0.0098	-0.0034	17.3872
	[0.0008]	[0.0007]	[0.0007]	[0.0003]	[0.608]
April	0.0186	0.0137	0.0130	-0.0058	21.2914
	[0.0009]	[0.0010]	[0.0006]	[0.0003]	[0.735]
May	0.0216	0.0143	0.0160	-0.0091	18.6718
	[0.0009]	[0.0010]	[0.0006]	[0.0003]	[0.884]
June	0.0160	0.0115	0.0148	-0.0082	11.4768
	[0.0009]	[0.0007]	[0.0006]	[0.0004]	[1.004]
July	0.0095	0.0075	0.0089	-0.0048	7.0626
	[0.0007]	[0.0005]	[0.0005]	[0.0003]	[0.827]
August	0.0089	0.0070	0.0064	-0.0041	8.2014
	[0.0007]	[0.0005]	[0.0005]	[0.0003]	[0.857]
September	0.0141	0.0079	0.0087	-0.0048	12.3796
	[0.0008]	[0.0006]	[0.0006]	[0.0003]	[0.742]
October	0.0143	0.0078	0.0056	-0.0051	7.2738
	[0.0007]	[0.0006]	[0.0006]	[0.0003]	[0.783]
November	0.0097	0.0040	0.0038	-0.0035	7.8371
	[0.0006]	[0.0005]	[0.0007]	[0.0003]	[0.698]
December	0.0055	0.0017	0.0032	-0.0014	0.1362
	[0.0005]	[0.0004]	[0.0006]	[0.0003]	[0.664]
Constant	0.7334	0.7887	0.7672	0.1321	3351.2
	[0.0019]	[0.0020]	[0.0023]	[0.0008]	[1.618]
F-stat For Month Dummies	101.50	31.09	128.59	93.98	188.17
Observations	52,041,054	52,041,054	50,660,895	52,041,054	52,158,278

Robust standard errors brackets. Each column is a separate regression, where the data was collapsed into county-month-year cells. The data were then weighted by cell size, where the number of observations represented is shown in the table. The omitted month is January. All regressions include third-order polynomials for birth-month trends. Regressions also include county-specific fixed effects, where counties are identified by the FIPS code in the birth certificate data for counties over 100,000 in population. Individuals living in smaller counties are aggregated by state. 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
Second Birth Quarter	0.0098	0.0126	0.0101
	[0.0019]	[0.0007]	[0.0008]
Third Birth Quarter	-0.0024	0.0025	0.0001
	[0.0018]	[0.0007]	[0.0008]
Fourth Birth Quarter	0.0002	0.0045	0.0003
	[0.0019]	[0.0007]	[0.0008]
Mean of Dep. Var.	0.513	0.619	0.731

Panel B: Regression on Dummy for having a Married Mother

	1960 Census	1970 Census	1980 Census
Second Birth Quarter	0.0023	0.0048	0.0068
	[0.0011]	[0.0005]	[0.0007]
Third Birth Quarter	0.0003	0.0024	0.0028
	[0.0010]	[0.0005]	[0.0007]
Fourth Birth Quarter	0.0006	0.0032	0.0036
	0.0023	[0.0005]	[0.0007]
Mean of Dep. Var.	0.916	0.873	0.815

Panel C: Regression on Dummy for White

	1960 Census	1970 Census	1980 Census
Second Birth Quarter	0.0064	0.0083	0.0092
	[0.0013]	[0.0005]	[0.0007]
Third Birth Quarter	0.0032	0.0018	0.0007
	[0.0012]	[0.0005]	[0.0006]
Fourth Birth Quarter	0.0037	0.0048	0.0018
	[0.0012]	[0.0005]	[0.0007]
Mean of Dep. Var.	0.876	0.858	0.827

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

Robust standard errors in brackets. In each panel, each column is a separate linear-probability regression. The coefficients shown are for dummy variables denoting a child's quarter-of-birth, a dummy for first-quarter-of-birth is omitted. The sample for each census year includes all children ages 16 and under living with their biological mother. The 1960 regressions have 578,773 observations, the 1970 regressions have 3,674,887 observations, and the 1980 regressions have 2,766,118 observations. All regressions include third-order polynomials for birth-quarter trends. 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 (see text).

Table 4: Maternal Characteristics and Outcomes 1980 Censuses

	Years of Schooling		Percent Dropouts		Wages, Logged		Wages, in Levels	
Second Birth Quarter	0.051	0.032	-0.348	-0.191	0.0060	0.0035	1.002	0.022
	[0.013]	[0.011]	[0.121]	[0.110]	[0.004]	[0.003]	[1.066]	[0.973]
Third Birth Quarter	0.07	0.054	-0.948	-0.808	0.0127	0.010	3.415	2.473
	[0.012]	[0.010]	[0.117]	[0.106]	[0.004]	[0.003]	[1.090]	[0.986]
Fourth Birth Quarter	0.064	0.034	-0.844	-0.601	0.010	0.0041	2.521	0.523
	[0.013]	[0.011]	[0.122]	[0.111]	[0.004]	[0.004]	[1.053]	[0.965]
Wald Test that Birth-Quarter Coefficients Are the Same	$\chi^2[3] = 31.91$		$\chi^2[3] = 32.85$		$\chi^2[3] = 40.31$		$\chi^2[3] = 30.06$	
Family Characteristics?	No	Yes	No	Yes	No	Yes	No	Yes
R-Squared	0.904	0.92	0.887	0.899	0.949	0.952	0.929	0.935
Age Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weights?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in brackets. Regressions come from cohorts of males born between 1944 and 1960. Cohorts 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 mother's giving birth as teenagers, fraction of mothers working, fraction of mothers married, fraction cohort white, and average cohort 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 education regressions weight by total individuals in a cohort; the wages/earnings regressions weight by total individuals reporting positive earnings in a cohort. The regressions on wages have 3,463 cohorts totaling 1,295,279 individuals; the regressions on education have 3,463 cohorts totaling 1,459,473 individuals. Wages are pre-tax wage and salary income over weeks worked. Logged wages reports the log of average wages in the cohort; using the average of logged wages produces qualitatively similar estimates. The age control measures age in birth quarters.

Table 5: IV Regressions on Returns to Education: 1980 Census

	Wages, Logs		Wages, Levels	
Years of Education	0.169	0.178	43.74	41.86
	[0.047]	[0.065]	[12.485]	[17.315]
First Stage F-Statistic [p value]	12.26	9.06	12.26	9.06
	[0.0001]	[0.0001]	[0.0001]	[0.0001]
Family Characteristics?	No	Yes	No	Yes
Age Controls?	Yes	Yes	Yes	Yes
State Controls?	Yes	Yes	Yes	Yes
Weights?	Yes	Yes	Yes	Yes

Robust standard errors in brackets. Regressions are 2SLS and come from cohorts of males born between 1944 and 1960; see Table 4 for a description of family characteristics. The instruments are a set of quarter-of-birth dummies, with the first quarter excluded. Using year-of-birth-by-quarter-of-birth as the instrument set produces qualitatively similar results. Wages are pre-tax wage and salary income over weeks worked. Logged wages reports the log of average wages in the cohort; using the average of logged wages produces qualitatively similar estimates. The average wage in the sample is $291. The first stage regressions are the same for columns 1 and 3 and for columns 2 and 4.