Relative Wage Changes and Timing of Childbearing

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Abstract: The U.S. wage structure experienced substantial changes due to energy price shocks, increased international competition, and technological change. Katz and Murphy (1992) among others have argued that these changes reflected a rise in demand for more skilled workers and also females. In this paper I use these types of relative wage changes to identify the effects of women's wages and husbands' earnings on fertility. I utilize 1984-2001 Survey of Income and Program Participation panels to construct individual fertility histories and NCHS birth certificate data to construct age-specific and total birth rates. I examine the impact of the woman's wage and husband's earnings (potential husband's earnings in the case of single women) on the timing and spacing and on the age-specific and total birth rates. Since unobserved heterogeneity is a concern in the individual level regressions, I use state, region and education specific wage trends constructed from the CPS Outgoing Rotations data to identify the effect of female wages and male earnings. To identify exogenous variation in the wages I use two types of instruments for the wages: a) group indicators within group-level regressions and b) predicted labor demand shifts. Results using individual and group-level earnings both indicate that a higher female wage leads to delay of first births. A 10% rise in the female wage causes approximately a 0.4 to 1% reduction in the probability of giving first birth over the next month. Male earnings do not play an important role for the timing and only increase the probability of second births among younger women. Results with grouped data on age-specific and total birth rates suggest that higher female wages delay births among the younger women but have no effect on total fertility. These findings are consistent with previous papers, which have found that female wages have greater effect on fertility than male incomes (Heckman and Walker, 1990; Merrigan and St. Pierre, 1998).

Key words: Fertility delay, timing, spacing, grouped data.

JEL: J13, J11

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1. Introduction

Over the past three decades fertility has declined sharply in many developed countries. Among countries in the European Union, total fertility rate has fallen below 2 on average. In Southern Europe, Germany and Austria the fertility rate has fallen even further - to 1.3 and below (Adsera, 2004). A number of papers have explored the possibility that the decline in fertility resulted from persistent high unemployment rates in Europe, particularly among young workers (Adsera, 2004, 2005; Gustafsson, 2001). Regardless of the cause, the decline in fertility and the corresponding aging of the population has become a major policy focus for many developed countries. The reasons for the urgency are several. First, the aging of the population and a shrinking workforce may strain many pension and social security programs. Second, childbearing at older age may increase the risk of health problems for mother and child.

In the U.S. totally fertility declined sharply during the 1960s and the early 1970s but has leveled off since mid-1970s. While total fertility rate has remained relatively steady, the aggregate masks some important underlying trends. For example, the rapid growth of the Hispanic population has shifted composition towards groups with higher levels of fertility. More importantly, there has been a noticeable trend in postponement of births in the U.S., starting in the late 1970s and continuing into 2000s. Mean age at first birth increased from below 22 in 1975 to above 24 in 2000 (Mathews and Hamilton, 2002). Age-specific birth rates[3] decreased among women under age 25, especially among non-Hispanic women, but increased in recent decades among women over age 30, consistent with the delay phenomenon (Martin, 2000; Mullin and Wang, 2002). While

[3] Birth rate hereinafter refers to the number of women giving birth rather than total number of children born, since the latter adds up all children from plural births (twins, triples, etc.), and occurrences of these are hardly explicable by economic variables.

the delay in fertility has not lead to a noticeable decline in total fertility, delay itself could be undesirable from a health perspective. Morgan (1996) suggests that major health issues may arise due to delayed childbearing. These health issues include an increased incidence of health problems for mother and child. Chromosomal abnormalities may occur more frequently among children born to older women (Hook, 1991). Sterility or subfecundity may be another concern because the more women postpone childbearing into their late 20s and 30s, the number who will not be able to bear the children they desire increases substantially (Morgan, 1996; Bongaarts and Potter, 1983).

The classic models of Becker (1960) and Mincer (1963) first explored the connection between women's wage, household income and fertility. In these models, children are durable goods in the utility function of parents. An increase in the woman's wage will raise household income but also the time cost of children, and thereby have offsetting income and substitution effects on the demand for children. An increase in husband's wage will increase the demand for children through the income effect, and this effect will be reinforced if husband's and wife's time are substitutes in household production.

Estimating the causal effect of female wage on fertility is complicated by the endogeneity of wages. That is, factors that affect women's earnings potential could also affect her family formation and fertility decisions. Empirical approaches that do not account for this will yield biased estimates. In this paper I exploit changes in the U.S. wage structure that prevailed during the 1980s and 1990s to identify these effects. Starting in the mid 1970s, and accelerating in the 1980s, the U.S. wage structure experienced substantial changes due to energy price shocks, increased international competition, and technological change (Levy and Murnane, 1992; Klein et al., 2003).

Part of these wage changes has been argued to be due to a secular rise in demand for more skilled workers and females (Katz and Murphy, 1992). A number of papers have utilized the recent changes in wage structure to identify income and substitution effects in labor supply models (Blundell, Duncan, Meghir (1998), Pencavel (1998), Devereux (2004), but to my knowledge, no other paper has looked at the impact of these relative wage changes on fertility or its timing. On the other hand, the use of aggregate level shocks to identify parameters of a dynamic fertility model follow in the tradition of Schultz (1985) and Black, Sanders, and Daniel (1996).

I utilize 1984-2001 Survey of Income and Program Participation panels to construct individual fertility histories. I examine the impact of the woman's wage and husband's income (potential husband's income in the case of single women) on the timing of first and spacing of second births. Since unobserved heterogeneity is a concern in the individual level regressions, I use state and education specific wage trends constructed from the CPS Outgoing Rotations data to identify the effect of female wages and male earnings. Further, to identify exogenous variation in the wages I use two types of instruments: a) group indicators within group-level regressions of age-specific and total fertility rates and b) a measure of region-level labor demand shifts, which is a weighted sum of national industry employment changes (excluding own region employment) projected onto region industrial composition. Results using both individual-level earnings and group-level earnings show that higher female wage leads to delay of first births for women 20-29 years old. Compared to the effect of women's own wage, male earning has a weaker impact on the timing of fertility. Male earning has a significant and positive effect only on the probability of second births for young women in individual-level regressions. These findings are consistent with previous literature on timing and spacing (Heckman and Walker, 1990; Merrigan and St. Pierre, 1998) who found greater effect of female wages than male incomes. The weakness of husband's income effect found here, however, is somewhat at odds with Black, Sanders, and Daniel (1996) who found a substantial income effect of husbands' earnings on fertility.

The remainder of this paper is organized as follows. Section 2 describes aggregate trends in fertility and wages. Section 3 briefly surveys the related literature on fertility and on recent changes in the wage structure. In section 4 I describe a standard life-cycle model of fertility and focus discussion on the impact of female wages and male earnings on the timing of fertility. Section 5 describes the data. Section 6 describes the main empirical results. Section 7 concludes.

2. Aggregate Trends in Fertility and Wages

2a. Patterns of Total Birth Rate and Age-Specific Birth Rates in the U.S.

Figure 1 plots total fertility rate for the 15-44 year old women in the U.S. The data on births come from Natality Data based on birth certificates (NCHS, 1985-2003) and estimates of the population are based on the CPS data. The figure shows that total birth rate has remained steady over the whole period under observation.[4] The total birth rate however masks an important phenomenon which is substantial delay of births. Figure 1 also examines age-specific birth rates in the two 10-year age categories, 20-29 and 30-39 year olds – where most of childbearing takes place. The figure shows that while fertility

[4] Total fertility rate (TFR) can be interpreted as the "total lifetime number of births that would be predicted if a representative woman realized the age-specific fertility rates that prevailed in a particular year" (Hotz et al. (1997)).

has remained relatively constant for women in their 20s, it has been rising among women aged 30-39.

A contributing factor to the steadfast aggregate birth rate is changing composition of the population. Rising share of the Hispanic population has shifted the composition towards groups with high levels of fertility. Figure 2 shows the total birth rate among non-Hispanics. The top panel of figure 2 shows the age-specific and total birth rates for non-Hispanic blacks. There is a slight decline in the total birth rate falling from 2.4 to 2.1 from 1989 to 2003, and birth rate after age 30 is trending up slightly. The delay phenomenon illustrated in the bottom panel of figure 2 is more pronounced among non-Hispanic whites.[5] This group shows substantial decline in birth rates at ages 20-29 and substantial increase at ages 30-39. Since the rising share of Hispanic women will give a misleading picture of group-specific trends, I focus on the results for non-Hispanic population in this paper.

2b. Trends in Wages and Earnings by Education and Gender from 1985 to 2003

Figure 4 describes the evolution of male and female earnings by education in the U.S. A large fraction of these changes were attributed to skill-biased technological changes favoring high-skilled labor. From late 1980s until early 2000s, wages of men with a high school degree or less were falling while men with college degree experienced continuous growth in their wages, which is reflected in their weekly earnings in figure 4. High school dropout women did not experience as large a decline in their wage as their male counterparts. However, inequality increased among women as well. High school

[5] The "other" race category was grouped with whites because their fertility patterns are similar.

graduate and college graduate women gained in wages almost continuously throughout the late 1980s and the 1990s, while high-school dropouts suffered a decline.

3. Related Papers

Heckman and Walker (1990) estimated a semiparametric reduced-form model of fertility using Swedish fertility history data. They found that the female wage delayed time to all conceptions and reduced total conceptions, while male incomes (defined to be zero for unmarried women without a cohabiting male partner) reduced time to conceptions and increased total conceptions. Merrigan and St. Pierre (1998) replicated the Heckman and Walker model on Canadian data using average real wages for women and men, for five different age groups for the period 1954 to 1990. Their estimates were close to those in Heckman and Walker (1990). In both papers the magnitude of female wage effects was bigger than male income effect, and the latter was not robust to inclusion of marital status. Female wages were found to have the biggest effect on delay of first birth. Butz and Ward (1979) modeled the effect of variation in aggregate male incomes and female wages on annual birth rates and found strong negative effect on fertility rates for female wages and positive effect for male incomes. They instrumented wages and incomes with their lagged values. The wage data used in these studies was aggregate, and was grouped only by year, age and sex. Tasiran (1995) replicated the Heckman and Walker model using the U.S. PSID 1985-1988 'Birth History File' and individual-level wages, and found a positive and statistically significant effect of the female wage and negative effect of male income.

Among studies that tried to use exogenous variation to identify the effect of economic factors on fertility dynamics are studies which exploited cross-country labor market differences (Adsera, 2005), Germany reunification (Bhaumik and Nugent, 2005; Kreyenfeld, 2005) and Russian transition (Kohler and Kohler, 2002). Adsera (2005) investigated the relationship between unemployment and the timing and number of children using variation in country-specific institutional arrangements in 13 European Union countries. Her study utilized mostly cross-sectional variation in the aggregate economic conditions. Duclos et al. (2001) used difference-in-differences estimator and fixed-effect regressions to estimate the impact of the 1986-1996 tax and transfer policies on aggregated fertility transition rates for different age groups. The focus of these papers, however, was not the effect of wages and incomes on fertility, but instead, the effect of aggregate economic uncertainty.

Schultz (1985) analyzed the effect of male and female labor market opportunities on fertility in Sweden 1860-1910 using time-series data on fertility in 25 counties. He used the change in the ratio of butter to rye price induced by international trade factors as instruments for the female wage, motivated by the fact that women were dominant in the pre-industrialist dairy production. For male incomes, output market prices, the industrial distribution of employment and the percentage of urban population were used as instruments. He found that a 10% increase in the female-to-male wage ratio explained a quarter of the decline in fertility. While the doubling of real male wages had no effect on completed family size, it did induce earlier marriages and shifted fertility from women over age 30 to women under age 30. Black, Sanders, and Daniel (1996) applied instrumental variable approach to study fertility in Kentucky. They used the world prices for energy and different types of coal interacted with county coal reserves as instruments for male and female wages. They found strong positive effect of higher male wages on fertility rates.

In this paper I take advantage of changes in female and male real earnings that took place over the last two decades in different age and education groups and states in order to identify fertility response to changes in male and female earnings. My approach is similar to those implemented in recent papers on labor supply. Pencavel (1998), Devereux (2004) and Blau, Kahn and Lawrence (2005) used group-specific trends in wages and employment to estimate wife's wage elasticity. Blundell, Duncan, Meghir (1998) used relative wage and tax variation across education and cohort groups to estimate labor supply elasticity.

4. The Life-Cycle Model of Fertility

In general, changes in prices and income over the life cycle may result in changes in the timing of fertility, even if they do not change completed fertility. Below I provide an overview of features of a general dynamic model of fertility used in the theoretical literature. After that I discuss the structure of the solution to parent's optimal childbearing over the life cycle. Finally, I discuss the implications of a general life-cycle model of fertility for the effect of income and wages on the timing of first birth and the spacing between births.

Economic theories of fertility in a life-cycle setting combine features of static models of fertility with various aspects from other dynamic models of behavior, which include models of optimal life-cycle consumption, models of life-cycle labor supply, and investment in human capital. I will focus on the description of a framework that incorporates the effect of husband's income and wife's wage in its solution. Here I basically borrow from the framework and discussion laid out in Hotz et al. (1997). Consider a household which consists of a wife and a husband who make fertility, time and resource allocation decisions over a finite lifetime. The couple makes their choices so as to maximize their utility subject to time and budget constraints[6]. The most general specification of lifetime parental utility takes the form:

$$U = \sum_{t=0}^{T} \beta^t u(c_t, l_t, s_t), \qquad (1)$$

where l_t is mother's non-market and non-child caring (leisure) time at age t, s_t is parental consumption, β is the couple's rate of time preference, and c_t is the flow of child services parents receive at age t from their stock of children, which is governed by child production function:

$$c_t = g(b_0, b_1, ..., b_{t-1}, t_{ct,} x_{ct}), \qquad (2)$$

where $b_\tau = 1$ if the parents gave birth to a child when they were age $\tau(\tau = 0,...,t-1)$ and $b_\tau = 0$ otherwise, and t_{ct} and x_{ct} denote, respectively, the mother's time and a vector of market inputs used in the production of child services. If children do not die before their parents, it follows that the couple's stock of children at age t is given by $n_t = \sum_{\tau=0}^{t-1} b_\tau$. Life-cycle models include constraints on mother's time in each period:

$$l_t + h_t + t_{ct} = 1, \qquad (3)$$

where the amount of time available to mother in each period is normalized to 1 and h_t is the amount of time spent in the labor market. Fathers are usually not assumed to

[6] Constraint not considered here might include natural constraints governing the reproduction and rearing of children.

allocate their time to child rearing, and they only provide for the children through their income.

If the parents start with no assets ($A_0 = 0$) and leave no bequests ($A_T = 0$), and if savings at age *t* are $S_t = A_t - A_{t-1}$, the parents' budget constraint for this period is

$$S_t = Y_{ht} + w_t h_t - s_t - p_{ct}^{/} x_{ct} - \pi_n n_t , \qquad (4)$$

where Y_{ht} denotes husband's income at age *t*, w_t is the wife's market wage rate, p_{ct} is the vector of prices for market inputs to the production of child services and π_n is per-unit non-quality cost of children. The form of the budget constraint facing the couple varies depending on what is assumed about the credit constraints they face. If capital markets are perfectly-imperfect, parents cannot save, which makes $S_t = 0$ in all periods.

In this life-cycle model parents make sequential decisions about childbearing, parental consumption, allocation of mother's time across labor market and childbearing activities so as to maximize equation 1 subject to the constraints implied by equations 2, 3 and 4. In the case of perfectly-imperfect capital markets assumption, the solution to the parents' problem maps parental choice variables at each age to the life-cycle sequences of prices and incomes:

$$b_t = b_t\left(\{p_{c\tau}\}_{\tau=0}^{T}, \{w_\tau\}_{\tau=0}^{T}, \{r_\tau\}_{\tau=0}^{T}, \{Y_{h\tau}\}_{\tau=0}^{T}, A_0 ; \theta\right) \qquad (5)$$

where $\{\cdot_\tau\}_{\tau=0}^{T}$ denotes the sequence of variables over the life time, and θ denotes a vector of the exogenous parental attributes. The life-cycle setting entails several alternative types of price and income effects. Transitory changes in prices or income without changes in parental wealth may shift the timing of births over the life-cycle without necessarily affecting completed fertility. While the births at a given age are a function of sequences of prices, none of these prices correspond to the "price of children" concept from static models of fertility. This is because children are treated as "durable" goods whose "user cost" is a function of the sequences of prices in equation 5.

This life-cycle model of fertility implies that for credit constrained households a temporary increase in the wife's wage would lead to substitute fertility into periods when her own wage is low and husband's income is high, which implies a negative association between women's wage, and positive association between husband's income, and the probability of birth in a given period. This reflects the tendency of women to time fertility into periods where the cost of childbearing is the smallest to them and a consumption smoothing role of husband's income. If the changes in wages and incomes are permanent, then, in addition to the effect on timing described above, there will be an effect on completed fertility.

5. Data Description

Detailed descriptions of the data, the Survey of Income and Program Participation (SIPP) and the Current Population Survey (CPS and CPS-ORG) are in the appendix. I restrict analysis to non-Hispanic population to avoid capturing immigration and assimilation effects of quickly rising Hispanics population in the U.S. I examine separately married and single women since these two groups possess very different characteristics and fertility behavior. Women may choose to remain single over entire life and this will determine their lifetime profile of wages and fertility. On the other hand, marital status, like fertility, may change over time and may be determined jointly with fertility, so I present my regression analysis for the whole sample as well.

In tables 1a and 1b I provide some descriptive statistics for non-Hispanic women who are 16-64 years old in the SIPP panels. Tables 1a and 1b indicate that married women have more schooling than single women. The proportion that did not finish high school is 6.3 percentage points higher among single women than among married women. Approximately 5.5% of single women and 12.7% of married women gave birth during the sample period. Since I need to measure other families' characteristics at the time of conception, the period at risk of giving birth is truncated by 8 months at the beginning of observations for each woman. This reduces the percent of usable birth data to 4.2% and 9.8% of births for single and married women, respectively. Among single women 35.5% are at risk of first conception and among married women, 14.2% are at risk. The number of months that a woman is observed waiting for her first conception is 28.7 among single and 26.5 among married women. It takes single women on average 3.3 more months to conceive for the second time than it takes married women. Age at first birth is 21.4 for single women, and 23.2 for married women. Single women's hourly earnings ($7.6) are slightly smaller than married women's hourly earnings ($8.12). Married women's weekly husbands' earnings are $504 and for single women, I use the 25th percentile CPS male earnings in the same age and education category as a proxy for "potential" husband's earnings. Single women worked on average 38.2 hours per week, married women worked 36 hours and their husbands worked 43.5 hours per week.

Figure 3 presents estimates of age-specific and total fertility rates based on births identified in 1984-2001 SIPP panels. Fertility rate of 20-29 year old women show some evidence of decline after 1990. Fertility in 30-39 age groups exhibits secular increase during the whole period. Apart from being noisier, trends in the SIPP data in birth rates reflect trends based on population estimates in figures 1 and 2.

Figure 5 suggests that aggregate CPS/ORG and SIPP hourly earnings of full-time workers closely track each other. Moreover, the correlation between CPS/ORG and SIPP earnings series for men and for women is high within education groups and states[7].

6. Empirical Results

While equation 5 characterizes the general solution to the parents' fertility choice, it typically cannot be expressed as closed form or particularly manageable function. This complicates the task of devising econometric specifications of the life-cycle models of fertility. Due to the difficulty in bringing theory to the data, most previous papers have used a reduced-form approach in estimation. However, the discussion of the general model is useful in interpreting various types of wage and income effects on fertility and its timing.

6a. Empirical Estimates of Discrete Time Proportional Hazard Model

Discrete time hazard model was chosen to model transition to births in the SIPP data because of the discrete nature of the birth process. The choice of discrete hazard model reflected the nature of these data. When modeling fertility, it would be natural to measure time in terms of number of biological cycles rather than number of calendar months. However, in the absence of information on biological cycles in the SIPP, calendar months were chosen as approximation. Further, absence of information on the exact day and hour of birth implies substantial number of women having exactly same

[7] Estimates available upon request.

number of months to birth (i.e. ties), which may result in numerical problems when estimating continuous time survival models.

Following previous literature estimating the effect of economic variables on the timing of births using proportional hazard models (Newman and McCulloch, 1984; Heckman and Walker, 1990; Adsera, 2005), I begin by estimating proportional hazard models for the timing of first and second births. For a set of women i = 1, 2, .., N who become at risk of birth in month t=0, the discrete time hazard function for ith person in month t=1,2,…,T is assumed to take the proportional hazard form[8]

$$\lambda_{ijst} = 1 - \exp\left[-\exp\left(b_1 w_{ijst}^f + b_2 w_{ijst}^m + b_3 Z_{ijst} + m_j + m_s + m_t + \gamma_0(\mathrm{t})\right)\right]$$

where exp(.) is the exponential function; w_{ijst}^f and w_{ijst}^m are log wages of female and log weekly earnings of her spouse in state s in month t, Z_{ijst} is a vector of covariates summarizing observed differences among families in state s in month t; b_1 , b_2 and b_3 are parameters to be estimated and m_j , m_s , and m_t are the sets of indicator allowing for fixed differences across husbands' and wives' educational levels, states and time, respectively. The duration variable in all estimates is the number of months to birth from either age 16 in the case of the first birth or age at first birth for the second birth. I restricted the age of women to be between 20 and 39 since fertility is most responsive to changes in economic conditions in this age interval. Moreover, 90.3% of all SIPP pregnancies in my sample happen during this interval. The choice of age 30 as a cutoff was based on demographic studies (Martin, 2000; Chen and Morgan, 1991) and aggregate statistics, which indicate declines in first birth rates in the twenties and increases after age 30. Modeling the timing to third birth was not attempted in this paper because most women were only

[8] A complementary log-log model was used as described in Jenkins (1995). This model is referred to as cloglog model. An alternative proportional odds model was estimated using logistic regression, and results were not significantly different.

asked about their first and last child in the SIPP[9] and there were a very low number of third births given in the sample period by women with known dates of second births. $\gamma_0(t)$ is a vector of parameters summarizing the pattern of duration dependence in the monthly hazard, which is common to all women, and is estimated non-parametrically using a set of dummies for all months present in the data. Since my interest does not lie in modeling the life-cycle profile of births, the choice of non-parametric pattern in the duration dependence of hazard allows to completely abstract from the specification of duration dependence. The estimation involves solving for the proportional effects by maximizing the log-likelihood function, which is the same as the log-likelihood for a generalized linear model of the binomial family with complementary log-log link (Jenkins, 1995). Estimates in the tables are presented in hazard ratio form. The risk of births for all women is assumed to end at the age 45. All estimates were obtained using SIPP personal frequency weights and standard errors were estimated using robust variance estimator and clustered by female ID.

Before turning to the results, a number of issues need to be addressed. One major issue in using SIPP panels is how to handle right-censored observations—those women who are not observed giving birth. In theory, hazard model is designed to handle censored observations by only including them in the denominator of the likelihood function. In practice however, due to the shortness of the panels available in the SIPP, too many of the women who are at risk of giving a birth are never observed giving a birth, i.e. right-censored. As many as 96% of single women at risk of first birth and 84% of single women at risk of second birth are censored. The corresponding numbers for married women are smaller, but still quite large: 80% and 66%, respectively. Given the

[9] Because of this the sample was restricted to women who had at most 2 kids.

shortness of the panel—at most four years, the fraction of women who give birth during this time frame is much smaller than the fraction women who give a birth of certain parity. On the other hand, many women would never give birth of certain parity due to exogenous reasons, and these women should never be included in the risk set[10]. In this version I address the issue of having a lot of censored observations on duration by including an indicator for the right-censored observations in the hazard regressions. This effectively means that I estimate the effects only for women who are observed having birth(s) within the SIPP panel windows[11]. One way to interpret the estimates is as "short-run" adjustments in timing of births among women who are 100 percent "at risk". Estimates of the hazard and the group-level regressions in Section 6b should be compared with this in mind since aggregate fertility rates in the grouped regressions include all women, even those who may remain childless through their entire reproductive life.

Another issue is how to treat marital status. The hazard model is estimated under the assumption of conditional independence of duration to birth on censoring after controlling for observables (Wooldridge, 2002). Given the differences in average times at risk of becoming pregnant between single and married women in tables 1a and 1b, it is reasonable to expect average time to birth to change with the change in marital status, which would violate conditional independence assumption and lead to biased estimates in the regression on combined sample. On the other hand, marriage is endogenous to women's wage and male incomes so that I may be closing off a potentially powerful channel by examining married women separately. I address these issues by estimating the

[10] In total, 18% of women aged 40-44 years old were childless in 1994 (Hotz et al. 1997).

[11] The non-censored sample contains disproportionally more high-fertility women who are more likely to give a birth in a given time. Also, due to the shortness of the panel, only short-time adjustments to earnings shocks can be measured. Essentially, dummying out censored observations has a trade-off between controlling for non-fecund women and making the sample selected. Ideally, if the right-censoring were completely random, only the intercept in the proportional hazards model would be changed, but not the slope coefficients.

model for married women separately, and for married and single combined[12]. This raises the issue of what is a reasonable estimate of "potential" husband's earnings for single women. Assuming that single women are disadvantaged in the marriage market, I use the 25th percentile of earnings for men in the same age and education category living in the same state as an approximation for the "potential" husband's earnings.

Results using individual-level earnings

Table 2 presents estimates of hazard ratios of transition to first birth using individual earnings for all non-Hispanic women and for married non-Hispanic women separately.[13] Since I am using individual-level wage data in tables 2 and 3, I include only working women with observed wages in these tables. In addition to the women's education, race, immigrant status, I also control for state fixed effects, aggregate time effects, and husband's education dummies. Table 2 shows that a higher female wage significantly reduces the hazard of first birth. The coefficient .964 observed in column (2) for married women, for example, suggests that a 10% increase in the woman's wage relative to average reduces the probability of first conception over the next month by nearly 0.4 percent. The coefficient on male earnings is positive (i.e. greater than 1) but not significant. The coefficient on female wage remains significant and smaller than 1 (thus implying a negative effect) when I pool married and single women. The bottom panel shows the results for older women, women who are 30-39 who are at risk to give first

[12] Due to low number of observations I do not estimate models separately for single women.

[13] The estimates of the effect of independent variables on the timing of births are presented in the tables in terms of the hazard ratios. For earnings expressed in logs, the estimates can be interpreted as the proportional change in hazard rate when log earnings increase by 1, or earnings increase by approximately 100%. For example, the coefficient of 0.7 on log women's earnings can be interpreted as a 3% decrease in the hazard rate of having a conception over the next month if the wage increases by 10% above the average wage in her state-educational group. Regressions include the whole set of dummies for states, education and aggregate time dummies. The effects measure deviations from the mean.

birth. The estimates for these older women are comparable to those of younger women above, though less precise.

Table 3 shows the hazard ratios for second births using individual-level earnings data. For second births, the woman's wage effect is not significant, and may even turn positive (i.e. hazard ratio greater than 1) for 30-39 year old women. However, male earnings have a positive effect for women who are 20-29 years old. For example, among married women, the coefficient 1.05 suggests that a 10% higher wage relative to average leads to a 0.5 percent increase in the hazard of having a second birth at the age 20-29. The effect of female wage is positive for the older women and the effect of male earnings is negative although most estimates are not significant. This result can be interpreted in terms of income effect dominating substitution effect in the case of female wage; and in terms of quantity-quality tradeoff where parents prefer to invest extra income in the existing child rather than having extra child in case of male earning.

Results using average CPS earnings

Using individual earnings to measure the effect on the timing of births is subject to the usual endogeneity problems. One might think of many unmeasured characteristics simultaneously affecting individual earnings and fertility patterns. If these unobservables remained fixed over time, one could implement linear fixed-effects estimator to remove their influence from the parameter estimates. However, this approach is problematic in non-linear estimators since estimation of fixed effects will produce biases in the parameters of interest, and the bias is more severe if the number of periods under observation is small (Neyman and Scott, 1948). Time-variant individual-level unobserved heterogeneity in earnings and fertility patterns presents another difficulty when using individual earnings in the estimations. Using aggregate fluctuations in the earnings of the group of people who are very similar in observable characteristics to the sample individual allows abstracting from the problem of individual-level heterogeneity. Since I argue that education-specific trends in earnings were fueled by the exogenous technological progress occurring during the 1980s and the 1990s, the estimates on average CPS earnings can be considered as truly exogenous variation in individual earnings.

Tables 4 and 5 present the estimates of wage and income effects on the hazard of first and second births using group-level earnings data. Table 4 refers to the hazard ratio of first births while table 5 refers to second births. The top panel refers to young women aged 20-29 while the bottom panel refers to older women, 30-39. To the extent that delay is important, I expect to find stronger effects for younger women and weaker effects for older women. For example, women who are at risk of having first births at ages 30-39 are women who have already delayed and they may be less responsive to economic variables due to biological constraints. The results are further broken down by the working status of the woman. Butz and Ward (1979) argued that the effect of female market wages on fertility is a weighted average of the effect on working women and non-working women who switch to working, with the effect on non-working being smaller than the effect on women who were already working.

Table 4 shows that the female wage and male earnings effects estimated from group-level earnings data are consistent with the results using individual-level earnings data. Among young working women (columns (2) and (4) in the top panel), the coefficient on female wage is 0.909 among all women and 0.896 among married women. These coefficients suggest that a 10% increase in female wage relative to average reduces the hazard of first birth by about 1 percent. The effects are larger than when individual-level earnings are used. However, the coefficients are only marginally significant (i.e. significant at the 10% level).

Table 5 presents the results using group-level earnings data for the hazard ratio of second birth. The positive impact of male earnings for young women remains but the effect is only significant in the specifications where single and married women are combined. This suggests that most of this effect may be operating through differences in marital status[14]. The bottom of table 5 shows the hazard ratios for women 30-39. Consistent with the estimates using individual-level earnings data, the effect of female wage is positive and the effect of male earnings is negative although again, most of the coefficients are not statistically significant.

To summarize, both the individual-level earnings data and the aggregate level earnings data point to a consistent negative effect of the female wage on the probability of first birth. This negative effect exists for both married women and single and married women combined. Consistent with Butz and Ward argument, the effects for non-working and working women combined are smaller than for working women, and they are not statistically significant. Comparing the individual earnings results to the results with average earnings, the size of the estimates is bigger with the average earnings while the coefficients are also less tightly estimated. Compared to the effect of female wage, the effect of male earnings on the hazard of first birth is never significant at conventional levels. The coefficients on male earnings are positive but not significant using individual level earnings data and are negative (also not significant) using the aggregate level

[14] Marital status indicator was not included in these specifications, as marital status is assumed to be endogenous.

earnings data. Male earnings are found to have a positive effect only on the hazard of second births for women who are 20-29 years old.

6b. Empirical Estimates Using Period Age-Specific and Total Fertility Rates

The proportional hazard models estimated in the previous section exploited panel data to examine the impact of wage and income changes on short-run adjustments in the timing of births. In this section, I explore the impact of wage and income changes on total and age-specific fertility rates which may include both a delay and a permanent reduction in fertility. I implement a grouping estimator to model the effect of average earnings trends by region and education on aggregate trends in the U.S. period age-specific and total fertility rates. I use grouped data to specify the following fertility rate regression:

$$F_{aert} = a_0 + a_1 w^f_{aert} + a_2 w^m_{aert} + a_3 B_{aert} + v^m_a + v^f_e + v^m_e + v_r + v_t + \varepsilon_{aert}$$

where *f* refers to female, m refers to male, *e* refers to one of the three education levels (high-school dropouts, high-school graduates, college), *a* refers to one of the six 5-year age groups of females (15-19, 20-24,…, 40-44) or one of the five 10-year age groups of males (10-19, 20-29 …, 50-59), *r* refers to one of the 9 U.S. Census regions[15] and *t* refers to year. The dependent variable refers to the natural log of yearly age-specific birth rate per 1000 women in one of the six 5-year age groups or the natural log of yearly total fertility rate of 15-44 years old women calculated from the U.S. birth certificates data (National Center for Health Statistics, 1982-1994) on the number of women giving birth and the CPS data on the number of women. Examining age-specific fertility rates (ASFR) will allow me to address the issue of timing and delay. To the extent that

[15] Grouping by regions instead of states was done to increase number of observations per group and reduce measurement error in the estimated group means.

increases in the wage reduce fertility rates of young women, but not so much the total fertility rate, this may suggest that delay is the main fertility outcome.

Earning variables in fertility regressions may be endogenous because of measurement error and unobserved characteristics that are correlated with earnings and fertility. Estimating regressions on grouped data represents an instrumental variables approach where the instruments are group indicators. This instrumental variables approach can be shown to be equivalent to grouping the data and regressing the group means of fertility rates on group means of the right hand side variables using weighted least squares (Angrist, 1991).

I match the number of women giving birth and the total number of women in month *t* with the information on the group characteristics and earnings at the time of conception, which I assume to be 9 months prior to birth. For example, average 1982 earnings are matched to total births from October, 1982 to September, 1983. Again, I restrict analysis to non-Hispanic women. I categorize the data on birth rates of married couples into discrete groups, which are interactions of husband and wife age and education and region of residence. Single women are assumed to have perfect assortative mating and their potential husbands are assumed to have same age and education as single women. The variables w^{f}_{aert} and w^{m}_{aert} refer to CPS yearly averages of log hourly full-time wages of females and log weekly full-time earnings of males in age group *a*, education group *e* in region *r* in year *t*. In order to increase the number of observations for calculating group average earnings, I compute average earnings of all males and all females from the CPS/ORG irrespectively of their marital status and characteristics of the spouse, and then merge average earnings to the data on birth rates by age and education of male and female, region and year.

All regressions of age-specific birth rates include fixed effects for female and male education levels, male age groups, regions and years. The vector of male age group fixed effects v_a^m controls for age profile of male earnings in the regressions with age-specific birth rates. The vectors of education fixed effects v_e^f, v_e^m allow for intrinsic differences in fertility across education categories because of unobserved attributes such as career-orientation, desired number of children, etc. Vector of region-specific effects v_r controls for fixed differences in fertility rates across U.S. regions. Vector of year dummies v_t allows for aggregate shifts of fertility over time as a result of changes in attitudes, social norms and economy-wide factors that affect women's preferences for fertility and national trend in the female labor force participation. All regressions include proportion of Blacks in each group B_{aert} as a control, since there are differences in marital fertility between races. This specification identifies the effects of earning changes using education- and region- specific trends in aggregate earnings of males and females over time. In a separate specification I report estimates that include region-specific linear time trends, which would control for region-specific time-varying unobserved factors correlated with wages, such as government programs introduced in specific regions or differences in regional trends in female labor supply. I restrict period of estimation to 1982-1993. CPS/ORG weekly earnings and hours are available in every month from 1982 on and birth certificates have father's education in them until 1994. In all analyses, the group means are weighted by the total number of women in each group calculated from the number of women in each group in the sample and the CPS personal weights.

Table 6a presents the estimates of elasticities of yearly age-specific and total fertility rates with respect to hourly female and weekly male full-time earnings while controlling for education, region, year and male age fixed effects from weighted least squares regression using grouped data. Panel A shows that among married, the effect of female wage on fertility is negative for all age groups except 40-44 year olds, but is strongly negative and significant only for women 20-24 and 25-29 years old (-2.102 and -1.206). The elasticity of the total fertility rate with respect to woman's wage is negative, but insignificant. Male earnings do not have a significant effect on the age-specific or the total fertility rates, except for the negative effect on teenage women.

Panels B-E present results for the single women under different assumptions about potential husband's earning. If potential husbands or current partners of single women earn at the 25th percentile in their age-education group (Panel B), as it was assumed in the hazard regressions above, then the estimated elasticity of fertility rates with respect to woman's wage are negative and big for the 15-29 year olds and for the total fertility rate, and positive for older women, but they never attain significance at the 5% level. The effects of potential husband's earning are smaller and frequently negative, but never statistically significant. The lack of statistical significance here may be explained by much smaller number of groups for the single women, which makes it harder to detect the effects. In Panel C single women's husbands are assumed to earn 0, which has been a tradition in the previous fertility literature. In this case the effects of woman's wage are comparable to the effects in Panel B, and the effect for ages 20-24 (-2.122) becomes statistically significant and comparable to the effect for the married women. Husband's earning effects are relatively small and always insignificant and are not reported. Panels D and E report estimates under alternative assumptions that single women's potential husbands earn at the mean and the median. The estimates are generally comparable across panels A-E: there is a negative effect of woman's wage on fertility of the younger women with the biggest effect for the 20-24 year olds and positive, but insignificant effect for the 40-44 year olds. The effects for the 20-29 year old married women are much more pronounced than for the single women of same age. On the contrary, male earnings do not have a significant effect on fertility. Also, total fertility rate is not significantly affected by female or male earnings.

Table 6b contains results from the same type of regressions as in Table 6a, but with region-specific linear time trends added to the set of regressors. While I no longer utilize plausibly exogenous wage variation contained in these trends, I am now able to control for the variation in various region-specific programs that might affect families within a certain socio-economic and demographic strata. As one would expect, the magnitude and statistical significance of the coefficients on married women's earnings have generally decreased with the effect of female wage on birth rates among 20-24 year olds decreasing to about half of the original size and the statistical significance of the effect for the 25-29 year olds disappearing. However, the effect for the 40-44 year olds became significant at the 5% level. Also, while decreasing in size, the negative effects for the 20-24 year old single women are now everywhere estimated with less than 1% probability of incorrectly rejecting the hypothesis of no effect and the positive effects for the 40-44 year olds are now statistically significant at the 10% level. Husband's earning effects remain unimportant. Compared to Table 6a, estimates in Table 6b present better fit for the data as reflected in the improved R^2, suggesting joint significance of the included regional trends.

Overall, female wages exert a depressing effect on birthrates of women younger than 39 and positive effect on birth rates of women aged 40-44. The effect on the younger women is, however, not spread evenly over the childbearing years, being mostly pronounced among the 20-24 year old women. On the other hand, total fertility rate does not seem to be significantly affected by female wage. If these estimates measure the real wage effect on the life-cycle process of births, then higher female wages decelerate entry into childbearing but do not significantly raise completed fertility, as approximated here by the period total fertility rate. A rise in female wages does, however, lengthen the period between generations and thereby decrease slightly the rate of population growth. In addition, married women are found to be more responsive to the variation in female wage compared to single women. In contrast, male earnings do not seem to affect either timing or completed fertility in the recent decades. These findings corroborate my previous results for the effect of male and female earnings on the timing of births.

6c. Empirical Estimates Using Period Age-Specific and Total Fertility Rates and Wages Instrumented with Measured Labor Demand Shocks.

An alternative set of estimates in Table 6c presents the effects of wages on aggregate birth rates using measured labor demand shocks used as instruments for the wages. As mentioned in the previous section, trends in the labor supply within regions might be contaminating the estimated effect of wages on birth rates in Tables 6a and 6b. In this section I use region-level measured labor demand shifts to estimate exogenous effect of wage trends on birth rates. Following the approach used in Juhn (1994) and Autor and Duggan (2003), I exploit cross-region differences in industrial composition and national-level changes in employment to predict regional employment growth. In particular, I calculate the predicted log employment change $\hat{\eta}_{jt}$ for each region *j* between year *t-1* and *t* as

$$\hat{\eta}_{jt} = \sum_k \gamma_{jkt-1} \eta_{-jkt} \, ,$$

where η_{-jkt} is the log change in two-digit (12 total, defined as in Juhn (1994)) industry *k*'s employment share nationally and γ_{jkt-1} is the share of region *j* employment in industry *k* in year *t-1*. Taking logs from the changes in industry employment shares makes this demand index strictly a relative measure. The subscript $-j$ in η_{-jkt} indicates that each region's industry k employment is excluded in calculating the national employment share change. Autor and Duggan (2003) find that including own-state employment substantially increased the predictive power of the employment projections in their DI application regressions, which raised a concern about a potential mechanical relationship.

This methodology predicts what each region's change in employment would be if industry level employment changes occurred uniformly across regions and region-level industrial composition was fixed in the short term. Regions that had a relatively large share of workers in declining industries will have predicted employment declines, while states that differentially employed workers in growing industries will have predicted increases. Provided that national industry growth rates, excluding own region industry employment, are uncorrelated with region-level labor supply shocks, this approach will identify plausibly exogenous variation in region employment and wages.

Separate demand indices were constructed for male and female employment changes and for each education and age group by using the corresponding shares of employment in each group in place of γ_{jkt-1}. However, changes in the shares of employment were measured only at the year-region-industry levels and were uniform for each sex-education-age group within each year-region-industry group. This implicitly assumes that changes in employment were the same across all sex-education-age groups within each year-region-industry group. Further, employment was measured in efficiency units after multiplying the number of employed people in each group by the average group real wage for the period 1982-1993.

Table 6c presents 2SLS counterpart of regressions in Table 6a, where male and female wages are instrumented with the predicted demand shocks. First stage regressions for married and single women's wages precede the birth rate regressions. The coefficients in most cases are not statistically significant, which might reflect weak predictive power of instruments in the first stage. Given weak first stage results, further work is needed to justify using observable labor demand shifts as instruments for female and male earnings.

7. Conclusion

In this paper I examined the impact of women's wage and husband's earnings on both the timing of fertility and total fertility. Using the data from 1984-2001 SIPP panels, I find evidence of these effects using variation in both individual-level earnings data and aggregate-level earnings data. I find a consistently negative effect of the female wage on the timing of first births, suggesting that better market opportunities lead to delay of childbearing, even controlling for education. In contrast to the effect of female wage, I do not find a robust effect of male earnings on the timing of first births. I find a positive impact of male earnings only on the hazard of second births for young women.

I also examine the impact of female wage and male earnings on age-specific and total fertility rates using birth certificates data (NCHS, 1982-1994). I find evidence that a higher female wage leads to delay but does not reduce total fertility. Male earnings do not have a substantial impact on fertility rates once I control for female wage. These findings are consistent with previous papers which have found that female wages have greater influence on fertility than male incomes (Heckman and Walker, 1990; Merrigan and St. Pierre, 1998). Absence of significant income effect of male earnings is, however, at odds with much of the previous studies.

This paper extends the methodology used in the previous empirical research on the impact of earnings on the timing and spacing of births. I utilize variation in the aggregate earnings that was argued to be influenced by aggregate demand conditions to identify causal effect of earnings on fertility. Using individual-level data helps to concentrate on the timing aspect of fertility and to control for a richer set of confounding factors. The panel aspect of the data allows measuring individual characteristics at the time of conceptions, which increases precision of estimated effects compared to estimates obtained from routinely used cross-sectional fertility recalls that usually lack retrospective information on many variables.

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Table 1a. SIPP Descriptive Statistics: Non-Hispanic Single Women

	Non-missing observations	Mean	Std Dev
Percent women HSD	363282	18.415	38.76
Percent women HSG	363282	36.248	48.072
Percent women SC	363282	26.364	44.061
Percent women CG	363282	18.973	39.208
Age of woman	363282	38.157	13.632
Percent of right-censored time at risk of 1st birth	150690	95.873	19.892
Percent of right-censored time at risk of 2nd birth	69796	83.87	36.781
Percent with used births	363282	4.138	19.916
Percent of total births	363282	5.457	22.714
Percent at risk of 1st pregnancy	363282	35.461	47.84
Percent at risk of 2nd pregnancy	363282	15.995	36.656
Time being at risk of 1st pregnancy, months	150690	28.74	7.499
Time being at risk of 2nd pregnancy, months	69796	27.608	9.236
Age at first birth	189652	21.382	4.679
Percent non-Hispanic Black	363282	23.951	42.679
Percent non-Hispanic White	363282	72.381	44.711
Percent non-Hispanic other race	363282	3.668	18.797
Percent immigrant	363282	4.931	21.652
Percent of working women	363282	69.567	46.013
Female hourly earnings	254643	7.576	4.819
Female hours worked per week	259406	38.206	9.511
Full-time female hourly earnings, CPS, by state	355545	6.639	2.165
Full-time male weekly earnings, CPS, by state	359338	317.882	120.115
Full-time female hourly earnings, CPS, by region	362935	6.555	1.993
Full-time male weekly earnings, CPS, by region	362986	332.88	117.034

Note: SIPP panels 1984-2001. HSD - high-school dropout, HSG – high-school graduate, SC – some college, COL – college degree or more. Women ages 16-64. Age at second birth was calculated only for births within sample period. Immigrant is a family dummy equal to 1 if either of spouses was born abroad. CPS male earnings for single women were taken from the 25th percentile of the distribution of weekly male earnings within same state-age-education group as woman.

Table 1b. SIPP Descriptive Statistics: Non-Hispanic Married Women

	Non-missing observations	Mean	Std Dev
Percent women HSD	682412	12.142	32.662
Percent women HSG	682412	38.829	48.736
Percent women SC	682412	26.12	43.929
Percent women CG	682412	22.909	42.025
Percent men HSD	682412	14.443	35.152
Percent men HSG	682412	32.958	47.006
Percent men SC	682412	24.507	43.013
Percent men CG	682412	28.049	44.924
Age of woman	682412	41.375	11.672
Age of spouse	682412	44.077	12.564
Percent of right-censored time at risk of 1st birth	123672	80.139	39.896
Percent of right-censored time at risk of 2nd birth	144839	66.019	47.365
Percent with used births	682412	9.751	29.665
Percent of total births	682412	12.657	33.249
Percent at risk of 1st pregnancy	682412	14.18	34.885
Percent at risk of 2nd pregnancy	682412	15.886	36.554
Time being at risk of 1st pregnancy, months	123672	26.448	9.533
Time being at risk of 2nd pregnancy, months	144839	24.192	10.579
Age at first birth	545723	23.162	4.941
Percent non-Hispanic Black	682412	7.699	26.658
Percent non-Hispanic White	682412	87.976	32.524
Percent non-Hispanic other race	682412	4.325	20.341
Percent immigrant	682412	7.116	25.709
Percent of working women	682412	64.717	47.785
Female hourly earnings	431624	8.118	5.195
Male weekly earnings	464998	503.701	326.157
Female hours worked per week	442461	35.919	10.93
Male hours worked per week	470957	43.48	10.003
Full-time female hourly earnings, CPS, by state	675285	7.069	2.19
Full-time male weekly earnings, CPS, by state	666718	429.764	148.721
Full-time female hourly earnings, CPS, by region	682279	7.021	1.993
Full-time male weekly earnings, CPS, by region	670687	425.21	133.518

Note: SIPP panels 1984-2001. HSD - high-school dropout, HSG – high-school graduate, SC – some college, COL – college degree or more. Women ages 16-64. Age at second birth was calculated only for births within sample period. Immigrant is a family dummy equal to 1 if either of spouses was born abroad. CPS male earnings for single women were taken from the 25^{th} percentile of the distribution of weekly male earnings within same state-age-education group as woman.

Table 2. Hazard Ratios- First Birth Using Individual Earnings of Working Individuals

	All women (1)	Married women (2)
A. Women ages 20-29		
Log(female wage)	0.963***	0.964***
	(-3.237)	(-2.787)
Log(male earnings)	1.015	1.017
	(1.230)	(1.371)
Subjects	22942	20126
Failures	2001	1755
Log likelihood	-115835078	-99332146
B. Women ages 30-39		
Log(female wage)	0.969*	0.959**
	(-1.923)	(-2.522)
Log(male earnings)	1.028	1.028
	(1.357)	(1.352)
Subjects	13657	12475
Failures	869	794
Log likelihood	-50849833	-46690717

Note: Estimated hazard ratios from Cloglog Proportional Hazard Model. T-statistics in parentheses. Month, state and spouse education dummies are included. Each model includes a dummy for right-censored observations. For first births, exposure starts at age 16 and for second it starts at the time of first birth. The period of estimation is 1984-2003. All time-varying RHS variables are measured 9 months before birth.
* p<0.1, ** p<0.05, *** p<0.01

Table 3. Hazard Ratios- Second Birth Using Individual Earnings of Working Women

	All women (1)	**Married women** (2)
A. Women ages 20-29		
Age at first birth	0.999	0.997
	(-0.155)	(-0.895)
Log(female wage)	0.977	0.975
	(-1.292)	(-1.350)
Log(male earnings)	1.061***	1.050**
	(3.051)	(2.468)
Subjects	11124	9458
Failures	1798	1529
Log likelihood	-71817978	-63621431
B. Women ages 30-39		
Age at first birth	1.005	1.006*
	(1.603)	(1.903)
Log(female wage)	1.011	1.009
	(0.621)	(0.449)
Log(male earnings)	0.984	0.977
	(-0.906)	(-1.246)
Subjects	12620	11676
Failures	1283	1187
Log likelihood	-56785770	-52934241

Note: Estimated hazard ratios from Cloglog Proportional Hazard Model. T-statistics in parentheses. Wave, state and spouse education dummies are included. Each model includes a dummy for right-censored observations. For first births, exposure starts at age 16 and for second it starts at the time of first birth. The period of estimation is 1984-2003. All time-varying RHS variables are measured 9 months before birth.
* p<0.1, ** p<0.05, *** p<0.01

Table 4. Hazard Ratios- First Birth Using Average CPS Earnings

	All women		Married women	
	All women	**Working women**	**All women**	**Working women**
	(1)	(2)	(3)	(4)
A. Women ages 20-29				
Log(female wage)	0.939	0.909*	0.940	0.896*
	(-1.120)	(-1.651)	(-1.010)	(-1.843)
Log(male earnings)	0.974	0.995	0.973	0.988
	(-0.831)	(-0.113)	(-0.725)	(-0.263)
Subjects	24223	21255	20574	18410
Failures	1803	1582	1532	1371
Log likelihood	-94825270	-79205937	-77541600	-67014541
B. Women ages 30-39				
Log(female wage)	0.913	0.896*	0.917	0.899
	(-1.482)	(-1.656)	(-1.311)	(-1.481)
Log(male earnings)	1.040	1.077	1.043	1.074
	(0.842)	(1.352)	(0.860)	(1.227)
Subjects	15570	13657	14252	12474
Failures	991	869	907	794
Log likelihood	-58714605	-50848111	-53907645	-46693535

Note: Estimated hazard ratios from Cloglog Proportional Hazard Model. T-statistics in parentheses. Wave, state and spouse education dummies are included. Each model includes a dummy for right-censored observations. For first births, exposure starts at age 16 and for second it starts at the time of first birth. The period of estimation is 1984-2003. All time-varying RHS variables are measured 9 months before birth.
* p<0.1, ** p<0.05, *** p<0.01

Table 5 Hazard Ratios- Second Births Using Average CPS Earnings

	All women		Married women	
	All women	**Working women**	**All women**	**Working women**
	(1)	(2)	(3)	(4)
A. Women ages 20-29				
Age at first birth	1.001	1.004	1.000	1.002
	(0.427)	(0.975)	(0.007)	(0.521)
Log(female wage)	0.970	0.942	0.956	0.914
	(-0.495)	(-0.678)	(-0.691)	(-1.014)
Log(male earnings)	1.132***	1.181***	1.028	1.059
	(2.885)	(3.065)	(0.608)	(1.007)
Subjects	17860	11275	14599	9442
Failures	2654	1675	2169	1403
Log likelihood	-86753813	-52160206	-74824121	-46260993
B. Women ages 30-39				
Age at first birth	1.000	1.005*	1.001	1.006**
	(0.023)	(1.647)	(0.493)	(2.013)
Log(female wage)	1.127*	1.108	1.116	1.120
	(1.814)	(1.278)	(1.605)	(1.334)
Log(male earnings)	0.954	0.931	0.939	0.905**
	(-1.139)	(-1.449)	(-1.475)	(-1.988)
Subjects	18668	12620	17273	11676
Failures	1897	1283	1755	1187
Log likelihood	-86770350	-56740462	-81017071	-52885911

Note: Estimated hazard ratios from Cloglog Proportional Hazard Model. T-statistics in parentheses. Wave, state and spouse education dummies are included. Each model includes a dummy for right-censored observations. For first births, exposure starts at age 16 and for second it starts at the time of first birth. The period of estimation is 1984-2003. All time-varying RHS variables are measured 9 months before birth.
* p<0.1, ** p<0.05, *** p<0.01

Table 6a. Estimates from weighted least squares regression of log age-specific and log total fertility rates on log female and male earnings and main fixed effects.

	Age 15-19	Age 20-24	Age 25-29	Age 30-34	Age 35-39	Age 40-44	TFR
			A. Married women				
Log(female wage)	-0.71	-2.102***	-1.206**	-0.054	-0.829	0.892*	-1.046
	(0.473)	(0.614)	(0.513)	(0.689)	(0.629)	(0.458)	(0.841)
Log(male earning)	-0.939**	-0.512	0.021	-0.065	0.647	0.203	0.076
	(0.381)	(0.395)	(0.571)	(0.550)	(0.664)	(0.527)	(0.737)
Adj. R-Square	0.79	0.692	0.677	0.732	0.766	0.73	0.669
N. of groups	977	2222	2790	3188	3173	2889	972
			B. Single women, potential husband's wage at the 25th percentile				
Log(female wage)	-0.304	-2.060*	-0.954	0.911	0.325	1.681	-0.541*
	(0.252)	(0.990)	(0.821)	(1.151)	(0.481)	(0.963)	(0.274)
Log(male earning)	0.475	-0.098	-0.19	-0.721	-0.74	-0.459	0.317
	(0.802)	(0.255)	(0.415)	(0.754)	(0.632)	(0.745)	(0.724)
Adj. R-Square	0.837	0.921	0.879	0.772	0.675	0.613	0.868
N. of groups	242	324	324	324	324	324	324
			C. Single women, potential husband's wage at zero				
Log(female wage)	-0.202	-2.122**	-1.066	0.426	-0.087	1.466	-0.278
	(0.274)	(0.878)	(0.733)	(0.755)	(0.443)	(0.854)	(0.611)
Adj. R-Square	0.835	0.921	0.879	0.771	0.673	0.613	0.867
N. of groups	242	324	324	324	324	324	324
			D. Single women, potential husband's wage at the mean				
Log(female wage)	-0.265	-1.692**	-0.56	0.053	-0.452	1.467	-0.504
	(0.212)	(0.720)	(0.588)	(0.734)	(0.542)	(0.893)	(0.597)
Adj. R-Square	0.837	0.921	0.879	0.772	0.674	0.613	0.868
N. of groups	242	324	324	324	324	324	324
			E. Single women, potential husband's wage at the median				
Log(female wage)	-0.334	-1.347**	-0.11	0.01	-0.467	1.546	-0.234
	(0.195)	(0.493)	(0.464)	(0.643)	(0.535)	(1.037)	(0.738)
Adj. R-Square	0.839	0.923	0.881	0.772	0.674	0.613	0.867
N. of groups	242	324	324	324	324	324	324

Note: Heteroscedasticity-robust, clustered by region standard errors are in parentheses. The period of estimation is 1982-1993. “TFR” - Total fertility rate. All regressions include sets of fixed effects for female and male education, region, year and % of Blacks in each group. Observations are weighted by CPS person weights.
* p<0.1, ** p<0.05, *** p<0.01

Table 6b. Estimates from weighted least squares regression of log age-specific and log total fertility rates on log female and male earnings, main fixed effects and region-specific linear time trends.

	Age 15-19	Age 20-24	Age 25-29	Age 30-34	Age 35-39	Age 40-44	TFR
				A. Married women			
Log(female wage)	-0.189	-0.889***	-0.348	-0.098	-0.827	0.626**	-0.48
	(0.337)	(0.252)	(0.192)	(0.298)	(0.602)	(0.242)	(0.377)
Log(male earning)	-0.072	-0.213	0.26	0.027	0.533	0.217	0.321
	(0.625)	(0.205)	(0.237)	(0.291)	(0.408)	(0.333)	(0.514)
Adj. R-Square	0.837	0.819	0.842	0.869	0.884	0.858	0.838
N. of groups	977	2222	2790	3188	3173	2889	972
		B. Single women, potential husband's wage at the 25th percentile					
Log(female wage)	-0.012	-0.670***	-0.133	0.323	-0.18	1.152*	0.225
	(0.153)	(0.137)	(0.232)	(0.605)	(0.412)	(0.564)	(0.423)
Log(male earning)	0.15	-0.062	0.208	-0.54	-0.751	-0.378	-0.384
	(0.323)	(0.228)	(0.361)	(0.671)	(0.477)	(0.448)	(0.478)
Adj. R-Square	0.951	0.973	0.963	0.924	0.898	0.861	0.958
N. of groups	242	324	324	324	324	324	324
		C. Single women, potential husband's wage at zero					
Log(female wage)	0.009	-0.690***	-0.051	-0.008	-0.56	1.000*	-0.051
	(0.155)	(0.099)	(0.227)	(0.299)	(0.569)	(0.441)	(0.267)
Adj. R-Square	0.951	0.973	0.963	0.923	0.896	0.861	0.958
N. of groups	242	324	324	324	324	324	324
		D. Single women, potential husband's wage at the mean					
Log(female wage)	0.002	-0.552***	-0.13	-0.274	-0.727	0.919*	-0.478
	(0.150)	(0.099)	(0.209)	(0.414)	(0.630)	(0.422)	(0.423)
Adj. R-Square	0.951	0.973	0.963	0.923	0.896	0.861	0.958
N. of groups	242	324	324	324	324	324	324
		E. Single women, potential husband's wage at the median					
Log(female wage)	-0.02	-0.605***	-0.016	-0.358	-0.833	0.930*	-0.299
	(0.139)	(0.096)	(0.211)	(0.322)	(0.725)	(0.482)	(0.404)
Adj. R-Square	0.951	0.973	0.963	0.924	0.897	0.861	0.958
N. of groups	242	324	324	324	324	324	324

Note: Heteroscedasticity-robust, clustered by region standard errors are in parentheses. The period of estimation is 1982-1993. "TFR" - Total fertility rate. All regressions include sets of fixed effects for female and male education, region, year, region-specific linear time trends and % of Blacks in each group. Observations are weighted by CPS person weights.

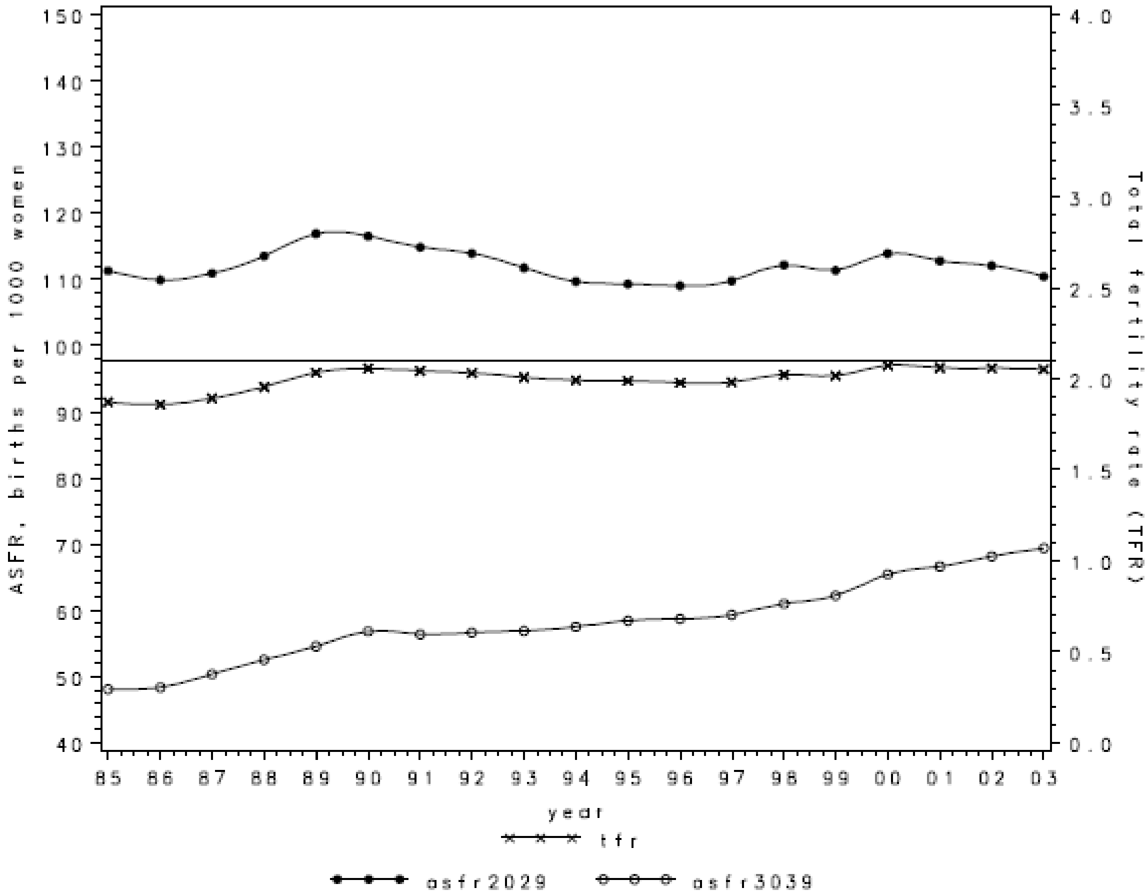
* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 6c. Estimates from weighted least squares regression of log age-specific and log total fertility rates on log female and male earnings instrumented with predicted employment changes.

	Age 15-19	Age 20-24	Age 25-29	Age 30-34	Age 35-39	Age 40-44	TFR
			A. First stage for female wage, married women				
Female emp.	-9.814	-11.171	-12.677	-8.854	-8.969	-13.159	-1.564
	(46.988)	(15.606)	(7.509)	(8.819)	(9.821)	(13.207)	(1.333)
Male emp.	-8.519***	-2.479**	0.296	1.358	-1.214	1.489	-0.286
	(1.274)	(0.925)	(1.800)	(2.697)	(1.233)	(1.045)	(0.329)
Adj. R-Square	0.704	0.905	0.942	0.957	0.961	0.95	0.972
N. of groups	977	2222	2790	3188	3173	2889	972
			A. Married women (2SLS regressions)				
Log(Fem. wage)	-3.007	-7.787	-4.728	12.427	-7.719	7.065	-0.479
	(3.406)	(26.383)	(6.278)	(13.193)	(6.645)	(8.199)	(11.612)
Log(Male wage)	0.329	1.764	1.331	-5.107	2.874	-2.148	-0.942
	(0.942)	(11.311)	(2.216)	(5.361)	(2.224)	(2.559)	(5.769)
Adj. R-Square	0.695	0.618	0.641	0.35	0.656	0.635	0.672
N. of groups	977	2222	2790	3188	3173	2889	972
			B. First stage for female wage, single women				
Female emp.	23.732	-3.96	-7.089	-14.12	-10.892	-12.835	-0.946
	(56.276)	(16.753)	(10.929)	(11.491)	(9.951)	(14.265)	(1.946)
Male emp.	-8.914	-4.624**	-2.345	2.879	0.829	2.603	-0.605
	(39.049)	(1.666)	(4.124)	(3.925)	(1.949)	(1.843)	(0.527)
Adj. R-Square	0.647	0.922	0.953	0.958	0.962	0.952	0.964
N. of groups	242	324	324	324	324	324	324
			B. Single women, potential husband's wage at the mean (2SLS regressions)				
Log(Fem. wage)	-18.147	-2.416	-4.973	-2.72	-2.899	5.344	5.228
	(62.075)	(3.947)	(7.569)	(20.201)	(4.956)	(4.284)	(11.782)
Log(Male wage)	4.266	-0.106	1.983	2.29	2.161	-2.854	-4.855
	(14.714)	(2.450)	(4.668)	(14.207)	(3.500)	(2.990)	(9.295)
Adj. R-Square	.	0.92	0.859	0.758	0.668	0.574	0.811
N. of groups	242	324	324	324	324	324	324

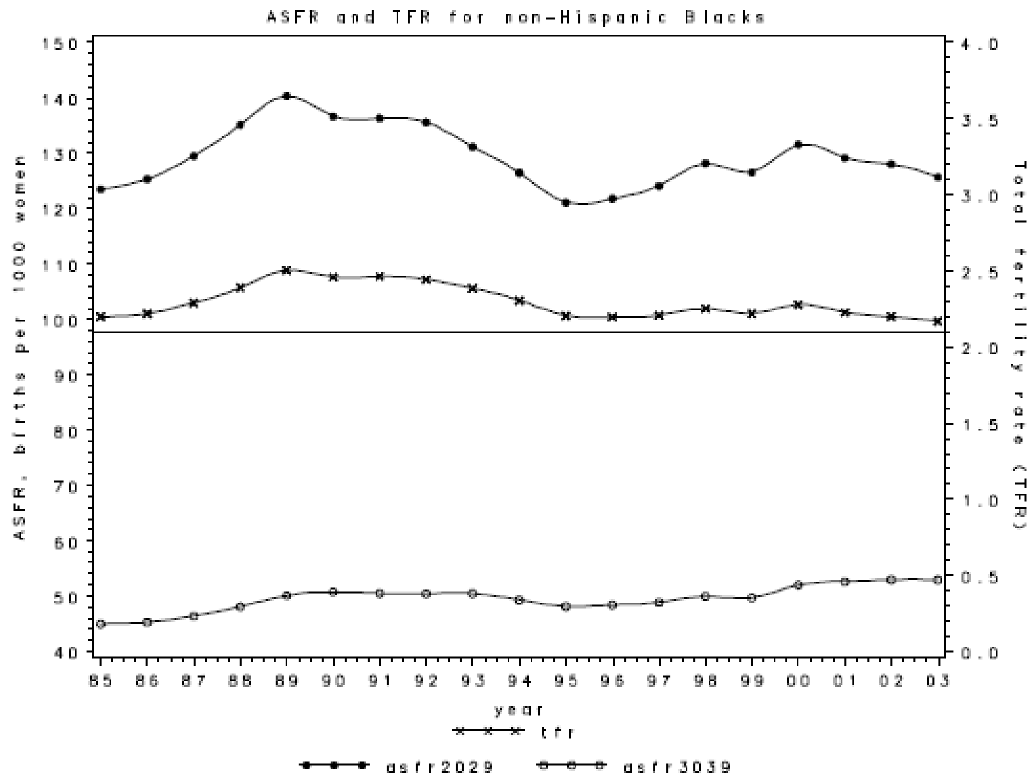
Note: Heteroscedasticity-robust, clustered by region standard errors are in parentheses. The period of estimation is 1982-1993. “TFR” - Total fertility rate. All regressions include sets of fixed effects for female and male education, region, year, and % of Blacks in each group. Observations are weighted by CPS person weights. Female and male wages instrumented with predicted employment changes for each sex.
* p<0.1, ** p<0.05, *** p<0.01

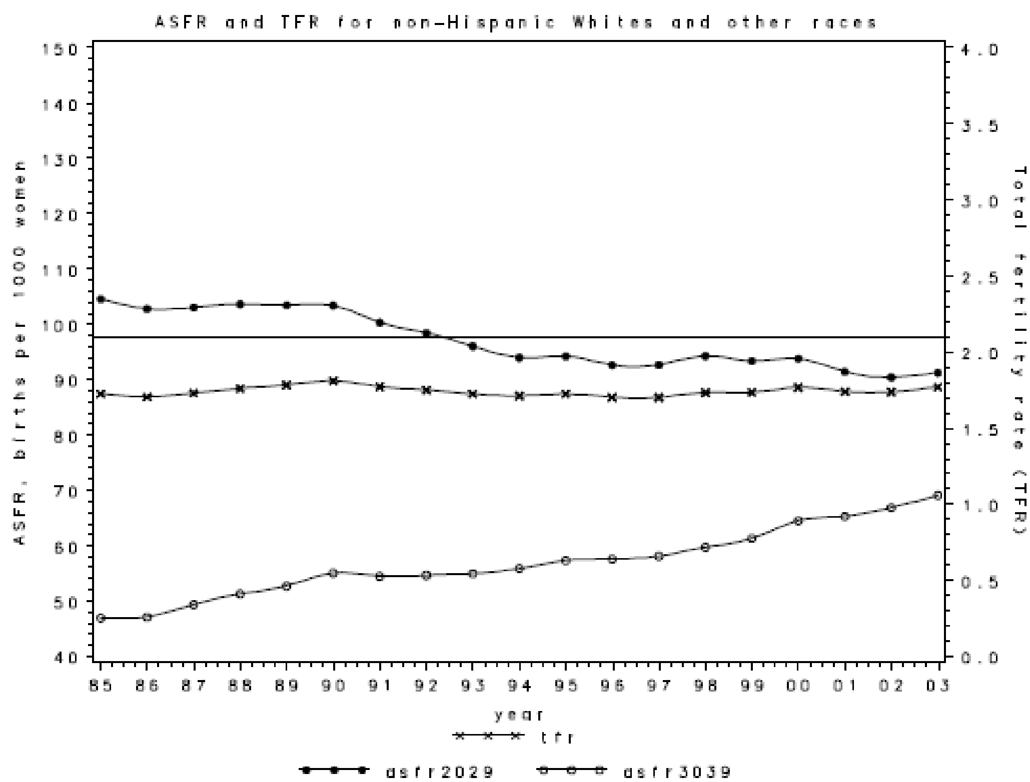
Figure 1. U.S. age-specific and total fertility rates, 1985-2003.



Source: U.S. Birth certificates data (NCHS), CPS data; author's estimates; "tfr" is total fertility rate of 15-44 y.o., "asfr2029" is age-specific fertility rate for 20-29 y.o., "asfr" is age-specific fertility rate for 30-39 y.o.

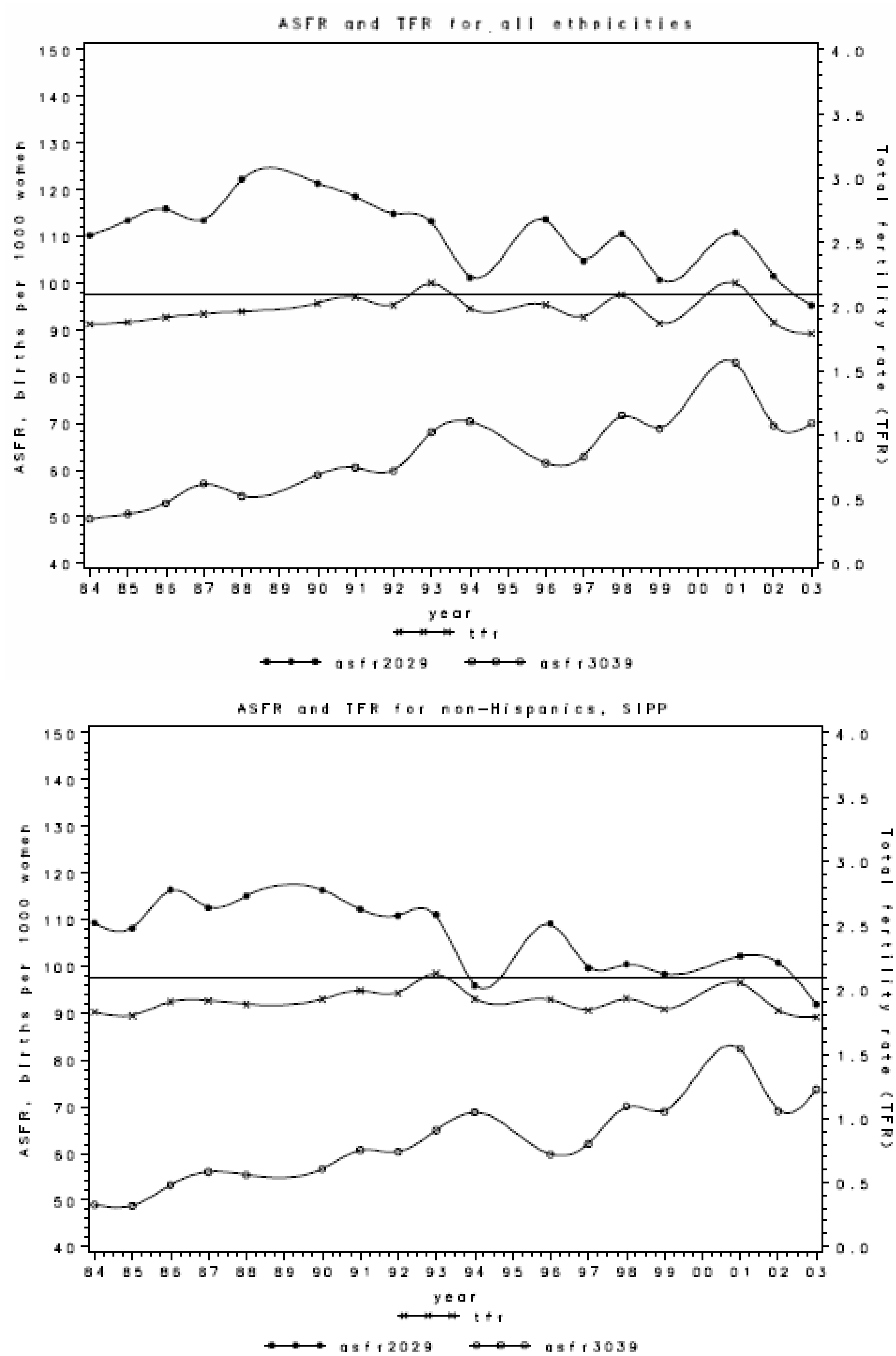
Figure 2. U.S. age-specific fertility rates by ethnicity.





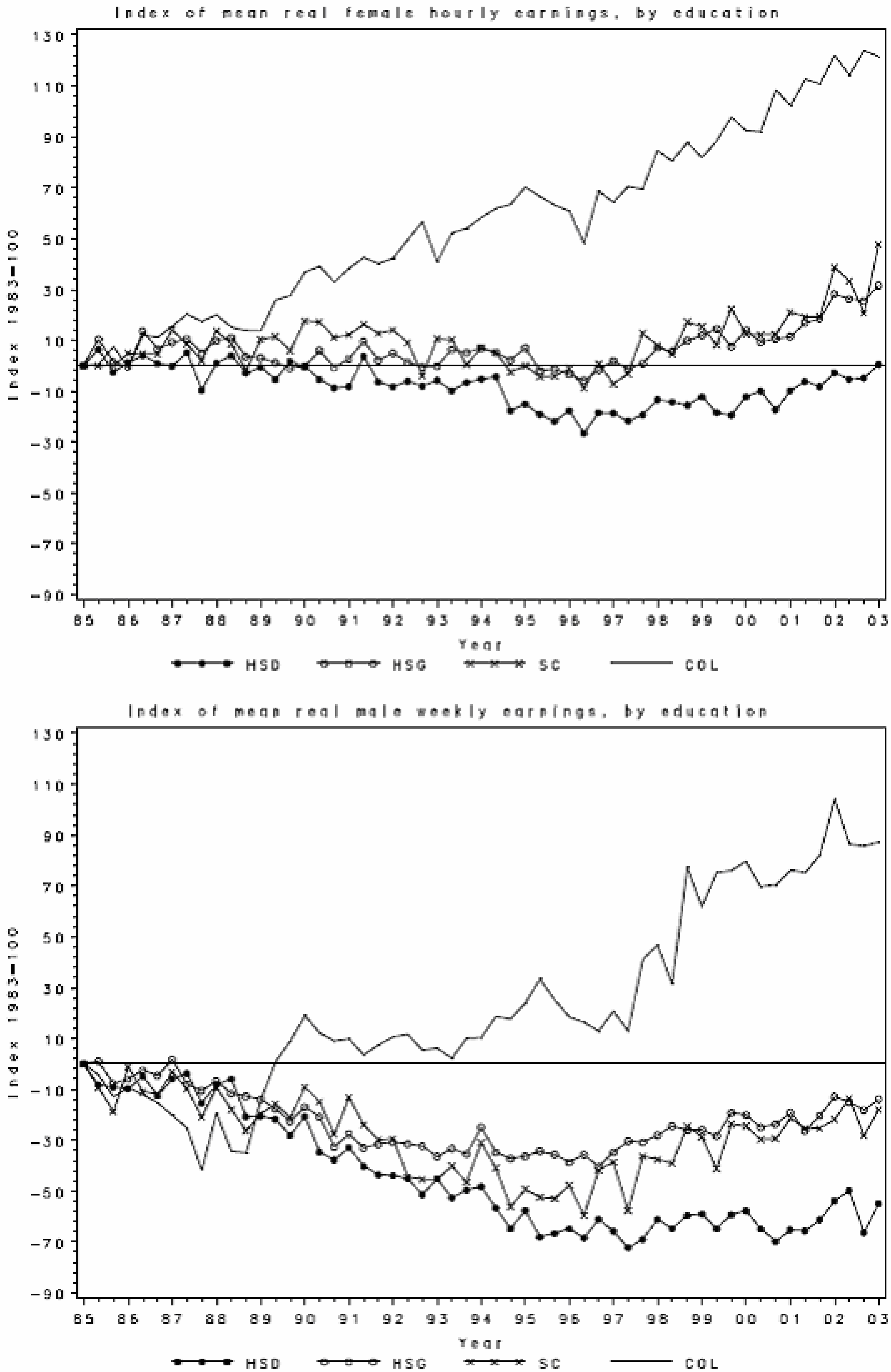
Source: U.S. Birth certificates data (NCHS), CPS data; author's estimates; "tfr" is total fertility rate of 15-44 y.o., "asfr2029" is age-specific fertility rate for 20-29 y.o., "asfr" is age-specific fertility rate for 30-39 y.o.

Figure 3. Estimates of U.S. age-specific fertility rates from SIPP, 1984-2003.



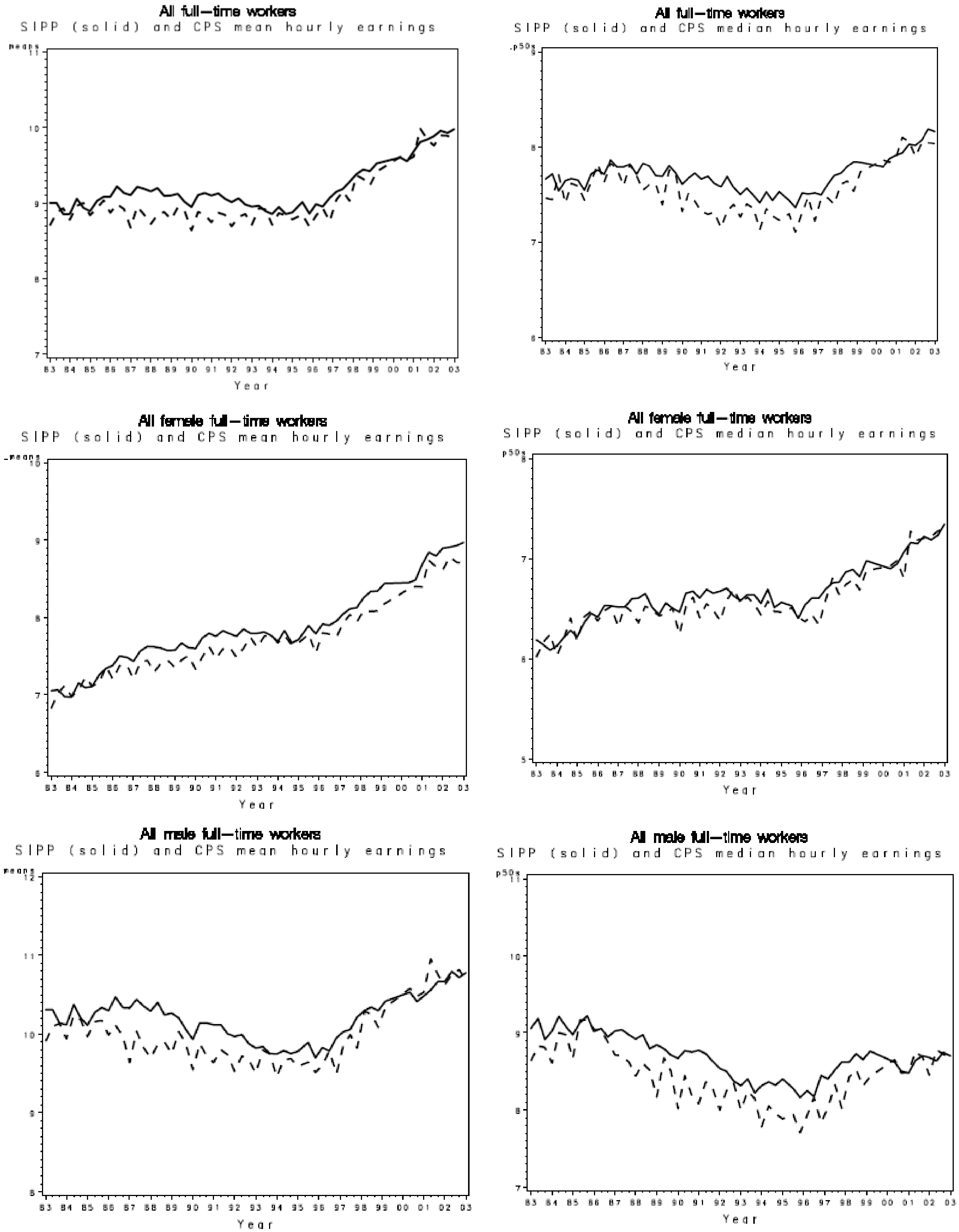
Source: Author's estimates using SIPP (2006); "tfr" is total fertility rate of 15-44 y.o., "asfr2029" is age-specific fertility rate for 20-29 y.o., "asfr" is age-specific fertility rate for 30-39 y.o.

Figure 4. Evolution of male and female earnings by education in the U.S. 1985-2003.



Source: CPS/ORG (2004), author's estimates.

Figure 5. Comparison of mean and median wage series from CPS/ORG and SIPP.



Source: Author’s estimates using CPS/ORG (2004) and SIPP (2006).

Data Appendix

Survey of Income and Program Participation (SIPP)

The SIPP (SIPP, 2006) was designed to provide accurate and comprehensive information about the income and program participation of individuals and households in the United States, and about the principal determinants of income and program participation. The survey design is a continuous series of national panels, with sample size ranging from approximately 14,000 to 36,700 interviewed households. The duration of each panel ranges from 2 ½ years to 4 years. It samples the U.S. civilian noninstitutionalized population. A new cohort is introduced each year, forming a new panel. A 4-year 1996 panel was introduced in April 1996; and a 3-year panel was introduced in February 2001. The SIPP content is built around a "core" of labor force, program participation, and income questions designed to measure the economic situation of persons in the United States. It interviews households every four months, asks retrospective questions on a monthly basis, and follows households for up to 48 months. In this study, the 1984-1988, 1990-1993, 1996 and 2001 SIPP panels are used[16].

The SIPP does have a few drawbacks. Among the most widely mentioned, the data are subject to a "seam bias" problem caused by the fact that individuals are more likely to report changes in their labor force characteristics or program participation between interview reference periods than within interview reference periods. Another potential problem is that beginning with the 1996 survey the SIPP no longer collected overlapping panels. Instead, there is a single panel from 1996 through 2000 and another from 2001 through 2003. This makes it possible that there could be breaks in 1996 and 2001 caused by the introduction of the new panel, which will be difficult to control for. However, the Census provides weights designed to make the survey sample representative of the U.S. population and these weights should help to smooth through panel breaks and account for attrition. Also, the states of Maine, Vermont, Iowa, North Dakota, South Dakota, Wyoming, Montana, Idaho, and Alaska cannot be separately identified in all the years in the SIPP. I eliminate individuals from these states in all years. According to Aaronson and Pingle (2006) they comprise of about 3 percent of the potential sample. For the final sample, I also exclude observations in which spouses are in the military (7103 obs.), women who are self-employed (98713 obs.) or currently at school (79030 obs.).

Construction of Pregnancies

I use methodology similar to Yelowitz (2002) to identify infants in a family. The SIPP does not ask about pregnancy, so the only way to tell whether a woman was pregnant is by the presence of an infant in the household. In this paper I link the baby to the mother. In the 1984-1993 panels, the SIPP asks for the parent's/guardian's line number, not the mother's line number. The SIPP also asks for the spouse's line number. The 1996 and 2001 panels separately ask for the mother and father's line number. Most infants have a parent's line number that points to a woman, and a small number have a parent's line number that points to a man. When the parent's line number points to a man and he is married, I substitute the spouse's line number. A small number of infants have more than one parent/guardian who is female. I exclude infants who appear to have more

[16] 1989 SIPP panel was excluded from the analysis because there was no fertility supplement available for this panel.

than one female parent/guardian, as well as infants who have zero female parents/guardians. Information about the date of birth of children was then used to determine in which month a woman gave birth. Pregnancy was assumed to have started 9 months before giving a birth. In order to assign birth order, I use fertility topical modules that are usually conducted in the second interview that ask about the number of children ever born to a woman. I use information on the total number of children at home from the core SIPP content if fertility topical module has missing information on the number of live births. Table 1a in the Appendix presents the number of women with identified one, two and three births in each SIPP panel. The number of one and two births in each panel ranges from 870 and 55 in 1988 panel to 3190 and 667 in 1996 panel, respectively. The number of women who gave birth to three children is negligible.

Wage Data From the Current Population Survey Outgoing Rotation Groups (CPS/ORG)

Since 1979, the Current Population Survey has asked employed adults in one-quarter of the survey's monthly sample – a group referred to as the "Outgoing Rotation Group" (ORG) – to answer a detailed set of questions about their earnings from work. The respondents to the wage question are referred to as the outgoing rotation group because CPS participants are in the survey four consecutive months, out of the survey for eight consecutive months, and then enter the survey again for four consecutive months. Respondents answer earnings-related questions in their fourth and eighth months in the survey[17]. Several features of the survey and changes over the years in the survey design, however, make it difficult to create a consistent hourly wage series from the raw survey responses (Schmitt, 2003).

The wage data used in my analysis comes from the CPS/ORG for years 1982 to 2003. My sample selection decisions for the construction of CPS/ORG wages closely follow Autor, Katz and Kearney (2005). All samples include full-time (at least 35 hrs/week) wage/salary workers ages 16 to 64. Earnings are weighted by CPS sampling weights. In all years, hourly earnings are reported hourly earnings for those paid by the hour and usual weekly earnings divided by hours worked last week for non-hourly workers. I impute missing hours worked last week with the usual hours per week. Autor, Katz and Kearney (2005) report that using imputed usual weekly hours in place of hours last week in all years 1973 – 2003 has little impact on the resulting earnings series. Top-coded earnings observations are multiplied by 1.5. Full-time earnings of below $67/week in 1982$ ($112/week in 2000$) and hourly earners of below $1.675/hour in 1982 dollars ($2.80/hour in 2000$) are dropped, as are hourly wages exceeding 1/35th the top-coded value of weekly earnings. All earnings numbers are deflated by monthly consumer price index (all urban consumers, U.S. city average, base period: 1982-84) deflator[18].

Construction of average earnings series

Male weekly and female hourly full-time CPS earnings were averaged by month, state of residence and 4 educational levels (high-school dropouts, high-school graduates, some college and college degree). I use CPS/ORG earnings to construct grouped earnings series for my analysis mainly because of higher quality and larger sample for the

[17] For extensive information on the CPS, see the Bureau of Labor Statistics CPS homepage: www.bls.census.gov/cps/.

[18] Source: http://www.econstats.com/BLS/blsn_m1.htm. Last accessed 09/19/2006.

CPS/ORG earnings data. However, there might be some discrepancy between the CPS and SIPP earnings data. One possibility for discrepancy between constructed SIPP and CPS/ORG hourly earnings series is that the BLS reports CPS hourly earnings for hourly workers excluding overtime, tips, and commissions, while, the BLS reports SIPP weekly earnings including overtime, tips, and commissions. Figure 1 in the Appendix might reflect this, as SIPP average and median earnings series lie above the CPS/ORG series. Also, SIPP wages were taken from the sample which oversamples low-income households, while the provided weights are intended to make the sample representative of the U.S. population. Due to the problem of large attrition (SIPP, 2006), these weights might do a poor job of making certain parts of sample representative. Also, average year of SIPP data contains about 3 times fewer observations than a corresponding year of CPS/ORG data. Thus, using grouped CPS/ORG earnings data will reduce potential measurement error in the earnings series and provide with a more precise estimate of how actual labor markets performed.

Table 1b in the Appendix presents count of CPS earners in each year for the periods available in the SIPP panels. After averaging these observations into cells defined by 42 states of residence of families, 256 months, 4 educational levels, genders and 6 10-year age groups (10-19,…,60-69) I obtain 117,245 cells with positive number of observations. Given the 2 133 658 total observations these cells on average provide 18 observations per cell. I further restrict cell count to at least 30 observations to increase precision of my earnings measures.

Construction of Education and Immigrant Status and Race/Ethnicity Variables

To attain comparable educational categories across the redefinition of Census Bureau's education variable introduced in 1992 in the CPS and in the 1996 SIPP panel, I use the method proposed by Jaeger (1997). In samples coded with the pre-1992 education question, I defined high school dropouts as those with fewer than 12 years of completed schooling; high school graduates as those having 12 years of completed schooling; some college attendees as those with any schooling beyond 12 years (including an incomplete 13th year) and fewer than 16 completed years; college graduates as those with 16 and more years of completed schooling. In the samples coded with the revised education question, I define high school dropouts as those with fewer than 12 years of completed schooling; high school graduates as those with either 12 completed years of schooling and/or a high school diploma or G.E.D.; some college as those attending some college or holding an Associate's Degree; college as those with a baccalaureate degree and post-baccalaureate degree.

Immigrant status was assigned to a married couple if either of the spouses was born abroad. Race and ethnicity indicators were combined into 4 groups: Hispanics (any race), non-Hispanic Blacks, non-Hispanic Whites and non-Hispanics of other races. Hispanic ethnicity was assigned to a married couple if either of the spouses was Hispanic. Black race was assigned using same logic.

Appendix Table 1a. Number of Live Births in SIPP Panels

SIPP panel	One birth	Two births	Three births
1984	1734	202	23
1985	1118	112	5
1986	901	78	6
1987	954	88	3
1988	870	55	7
1990	1927	242	23
1991	1205	139	15
1992	1721	223	25
1993	1197	597	94
1996	3190	667	95
2001	3083	651	72

Source: SIPP (2006), author's estimates.

Appendix Table 1b. Number of Observations with Non-Missing Earnings in CPS/ORG the periods available in the SIPP.

Survey year	Number of observations
1984	108199
1985	111864
1986	111593
1987	109074
1988	107612
1989	110402
1990	115504
1991	112124
1992	109147
1993	111674
1994	108543
1995	92081
1996	96534
1997	99793
1998	98631
1999	103191
2000	106304
2001	109417
2002	116448
2003	95523

Source: CPS/ORG (2004), author's estimates.

Appendix Table 2. Imputation of inconsistent dates of birth in the 1986-1993 SIPP panels.

	Panel 86	Panel 87	Panel 88	Panel 90	Panel 91	Panel 92	Panel 93
1. % with inconsistent month or year of birth, all	30.49	30.64	29.6	31.42	32.17	32.57	32.63
2. % with inconsistent month of birth, all	3.5	4.23	3.81	4.43	4.7	5.08	5.39
3. % with inconsistent month of birth (after step 1)	2.02	2.45	2.25	2.45	2.64	2.86	3.08
4. Among individuals with consistent and imputed month of birth:							
4.1 % with inconsistent year of birth	28.49	28.25	27.3	28.94	29.56	29.75	29.61
4.2 % with inconsistent year of birth (after step 2)	8.37	7.57	7.16	9.76	10.33	10.64	10.91
4.3 % with inconsistent year of birth (after step 3)	2.11	2.16	1.89	2.51	2.51	2.46	2.51
4.4 % with year of birth different from longitudinal file (after step 3)	8.43	8.77	8.15	11.05	10.5	10.72	10.21
4.5 Total number of individuals	35042	35055	34988	67445	43002	59814	60461
5. % with inconsistent month or year of birth after imputations, all	4.09	4.55	4.09	4.9	5.08	5.25	5.52
6. % with year of birth from longitudinal file equal to the last observation	99.87	99.54	99.56	96.68	96.87	86.36	96.11
7. % with imputed month of birth from longitudinal file equal to the last observation	100	100	100	99.61	99.57	98.39	99.36
8. Total number of individuals in the panel	35764	35935	35792	69136	44166	61576	62383

Notes: In 1986-1993 panels, 29.6%-32.6% of people reported inconsistent year of birth (YOB), and 3.5%-5.39% reported inconsistent month of birth at least once (in 1984-85, 1996 panels, only about 1% of people report inconsistent year or month of birth). Since the focus of this paper is the timing of births, it is essential that the year and month of birth are known precisely. Having approximately a third of the sample with inconsistently reported date of birth would make these data much less reliable.

To deal with this problem I follow 3 steps:
Step 1. I impute month of birth that was reported in at least 80% of cases by each individual during a panel. Such imputation reduces the percentage of people with inconsistent month of birth from about 4% to about 2.5%.
Step 2. A closer look at the mis-reporting of YOB reveals that much of it has a consistent pattern. About two-thirds of individuals with inconsistent YOB in every panel 1986-1993 have their YOB in each of the 4 months preceding interview month being exactly 1 year above YOB in all other months. All of these individuals only have 2 different values of reported YOB and all of them have birthday in the month of interview that immediately follows the 4 months where they over-report their YOB. Moreover, the pattern of inconsistency in the YOB is the same in the reported current age (variable AGE) and in the reported YOB (variable BRTHYR). These same people have their current age in the 4 months before interview 2 years below their age in the month of their birth. Yet the relationship between these people's reported AGE and YOB is still holds:
YOB=(Current calendar year) - (Current age) if (Current calendar month)>=(Month of birth) and
YOB=(Current calendar year) - (Current age + 1) if (Current calendar month)<(Month of birth),
which suggests that one of the variables was used to assign the other. Such pattern of inconsistency in the data could arise if, for example, it was assumed for all people whose birthday is in the month of interview, that their new birthday has already happened some time earlier this month, and then 1 was subtracted from their reported age at the time of interview to assign their age in the 4 months preceding the interview month, so that it reflects their previous age. As a result, if days of birth are distributed uniformly over a calendar month, and months of birth are distributed uniformly over a year, then about 17% of all birth years would be mis-recorded, since approximately 75% of interviews happen by the middle of the month and average interview happens on the 10th. According to the table, the actual percentage of inconsistent YOBs following this pattern is about 20%. The fact that current reported age and YOB only change between waves makes it easy to fix these inconsistencies.
Step 3. Additional 7% of the total sample having inconsistent years of birth had only 2 values of reported YOB. For them, I imputed YOB being the value reported in at least 2/3 of the cases for each individual. This left only about 2.5% of the total sample having inconsistent records on the YOB or 4-5% having inconsistent YOB or month of birth. These people were excluded from further analysis.

The Census Bureau does provide a single value of the pre-edited year and month of birth for each individual in their longitudinal files for each panel (variables U_BRTHYR and U_BRTHMN). However, comparison with the core SIPP files suggests that in almost all cases the Census Bureau set the value of the date of birth equal to the last reported value within a panel. Such an imputation method seems to bear a greater measurement error than the method described above. According to the table, in 8-11% cases Census years of birth from longitudinal files were different from mine.