The Effects of Motherhood Timing on Career Path

September 2009

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Abstract

This paper estimates the causal effects of motherhood timing on female career path, using national panel data from the NLSY79 and biological fertility shocks to instrument for the age at which a woman bears her first child. Motherhood delay leads to a substantial increase in earnings of 9 percent per year of delay, a smaller increase in wage rates of 3 percent, and an increase in hours worked of 6 percent. Supporting a human capital story, the postponement premium is largest for college-educated women and those in professional and managerial occupations. Family leave laws do not significantly influence the premium. Panel estimation reveals evidence of both fixed wage penalties and lower returns to experience for mothers: a "mommy track" is the likely channel for the timing effect.

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1. Introduction

American women have achieved dramatic labor market gains over the past several decades (Blau, 1998). At the same time, they have increasingly delayed the onset of motherhood (Chen and Morgan, 1991). The time-series association between fertility delay and women's career success (Caucutt et al., 2002) is echoed in cross-sectional comparisons (Hofferth, 1984). For women in the National Longitudinal Survey of Youth 1979 cohort, the positive correlations between age at first birth (A1B) and various measures of career outcomes, such as earnings, hours and wage rates, remain evident even after adjusting for education, race, ability and birth year.[1] In the absence of further evidence, however, it is difficult to know whether these correlations flow from the effect of motherhood timing on earnings, the effect of anticipated earnings on timing, or from the influence of some outside factor, such as ambition or productivity, on both. The first channel is a key element in the calculus of optimal motherhood timing, as it may offset some of the costs associated with delay, such as fewer years of motherhood, increased health risks (Alonzo, 2002) and reduced opportunity for childbearing (Maranto, 1995).[2]

This paper attempts to isolate the effects of motherhood delay on career outcomes by exploiting three biological fertility shocks as instrumental variables (IVs) for age at first birth: (1) whether first pregnancy ended in miscarriage, (2) whether conception of the first child occurred while using contraception, and (3) elapsed time from first conception attempt to first birth. After finding a positive career impact from motherhood delay, the paper then explores heterogeneous effects and uses panel data to investigate the underlying channel, finding evidence for motherhood penalties in both wage levels and returns to experience.

Researchers face a substantial challenge in identifying the causal effects of fertility on career outcomes, as work and family choices may be undertaken jointly and with foresight. Previous studies have associated motherhood delay with career benefits after controlling for observable maternal characteristics in cross-sectional data (Hofferth, 1984) or for individual

[1] Appendix Figures 1 to 5 of this paper depict the relationships in age-earnings, age-hours and wage profiles for women grouped by A1B. Mothers with A1B in the 30 to 34 range outperform the two groups of earlier mothers (A1B 20 to 24 and A1B 25 to 30) by the age of 35. Although they are more pronounced at age 35, the differences in wage rates and wage growth rates are apparent even prior to childbearing, and childless women have lower wage rates than later mothers.

[2] Hewlett (2002) describes some personal costs of motherhood delay, and argues that many women with successful careers have come to regret the family sacrifices they made to further their professional goals.

fixed effects in panel data (Taniguchi, 1999). Although fixed effects and controls for pre-motherhood wage trajectories can alleviate the omitted variable bias, potential biases remain from reverse causality (fertility decisions occurring in response to current or anticipated changes in career outcomes) and joint determination (where fertility and work choices are made simultaneously).[3] Some studies employ socioeconomic background and beliefs as instrumental variables,[4] but deemphasize their IV estimates as imprecise (Blackburn et al., 1993) or unstable (Chandler et al., 1994). More problematic, however, is the potential endogeneity of the instruments and their direct effects on career outcomes.

Since no single approach is ideal, it is important for researchers to accumulate evidence using various empirical strategies. The IV strategy in this paper, similar in spirit to the natural experiment in Hotz et al. (1999), exploits biological variation to identify the effects of fertility delay. Instrumental variables strategies using biological variation from miscarriages (Hotz, Mullin and Sanders, 1997) and age at menarche (Klepinger, Lundberg and Plotnick, 1999), as well as regional policy variation in welfare generosity and access to family planning services, have yielded important insights into the effects of teenage motherhood on human capital and career outcomes. The shocks used in this paper are selected for their potential to shift motherhood timing for women in their twenties and early thirties. These shocks drive a wedge between a woman's actual and preferred age at first birth. They are plausibly exogenous to earnings potential and largely unanticipated.[5]

The first contribution of this paper is new causal evidence that fertility delay after adolescence improves career outcomes. A year of delayed motherhood is found to increase career earnings by 9%, work experience by 9%, and average wage rates by 3%. The effects are heterogeneous across women; those with college degrees and in professional and managerial occupations receive the greatest career returns to delay. Post-motherhood wages are also shown to vary with motherhood timing.

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[3] Section 6 of this paper employs a more flexible specification of the panel estimation strategy, including fixed effects in both wage levels and their growth rates, and using instrumental variables constructed from biological fertility shocks.

[4] Blackburn et al. (1993) uses number of siblings, parental education, mother's labor force participation, and family disruption. Chandler et al. (1994) uses religion and number of siblings. Amuedo-Dorantes and Kimmel (2003) uses parental education and family disruption.

[5] Further evidence supporting the validity of the instruments is presented in Sections 3, 4 and 5. These instrumental variables are used in Miller (2009) to estimate the impact of motherhood delay on children's test scores. Similar variation in fecundity is exploited in Cristia (2008), who finds substantial labor supply effects from motherhood on a sub-sample of women who sought fertility assistance in National Survey of Family Growth.

The second contribution stems from the exploration of the underlying mechanism of the timing effect. Motherhood delay can influence a woman's career by altering the timing of her work experience. Mincer and Polacheck (1974) and Mincer and Ofek (1982) provide early evidence that human capital depreciates during career interruptions for women in the NLS68. Depreciation increases with education, and is larger for women with more years of experience at the time of their interruption.[6] The finding is confirmed by Baum (2002), who reports evidence of skill depreciation for women in the NLSY79, but only among those who switched employers following their interruption. Albrecht et al. (1999) replicates the result with Swedish data, adding that interruption type, and not simply duration, affects the wage penalty. Light and Ureta (1995) argues that differences in the timing of work experience account for as much as 12% of the gender wage gap. These consistent findings of depreciation suggest an important potential channel for the causal effect of motherhood delay.

Using the exogenous variation in motherhood timing produced by the biological fertility shocks, I generate estimates of the impact of motherhood on wage levels and wage growth, controlling for individual fixed effects. These results contribute to the literature documenting differences in pay and hours between mothers and childless women, known as the "family gap" (e.g., Cramer, 1980; Korenman and Neumark, 1992; Browning, 1992; Joshi et al., 1998; Waldfogel, 1998; Dankmeyer, 1996; Budig and England, 2001; Simonsen and Skipper, 2003). Previous studies generally assume that motherhood is exogeneous or employ family background proxies as instrumental variables. Unfortunately, as above, there are few *a priori* reasons to assume that family background should be excluded from the wage equation. Two convincing quasi-experimental papers in the literature exploit biological fertility events association with twins (Bronars and Grogger, 1994) and sex ratios (Angrist and Evans, 1998) to produce exogenous variation in the number of children. However, while these methods are useful for measuring the impact of additional children, they cannot be used to identify the effects of delaying or avoiding childbearing altogether. Using biological fertility shocks, this paper finds motherhood reduces both the level and slope of the wage profile, with smaller a penalty for older

[6] This may be caused by women who expect larger depreciation costs choosing to delay motherhood.

mothers.[7] This supports a "mommy track" as the channel for the effect of motherhood timing, which may be either voluntary or externally imposed.

The focus of this paper is limited to the career impact of motherhood timing. The paper does not measure the individual or social costs and benefits associated with motherhood delay outside of the labor market, such as the role of increased motherhood delay in reducing total period fertility rates (Gustafsson, 2001) or the impact of late motherhood on health outcomes. An analysis of optimal motherhood timing would need to incorporate such considerations as well.

2. Conceptual Framework

This section presents five benchmark cases to illustrate the potential career effects of delayed motherhood. It motivates the choice of outcome variables and the panel data investigation of the impact of motherhood on wages, but is not meant as a complete model of optimal fertility timing. The optimal A1B for each woman will depend on these career concerns as well as the full set of utility, health and other costs associated with fertility delay. To isolate the career effect of timing, I focus on scenarios in which the motherhood career interruption is of a fixed duration. For this thought experiment, I consider a hypothetical woman who has a single child, works full-time until motherhood, takes a year out of the labor force to care for her infant, and then returns to full-time employment until retirement.

In Case 1, future income is discounted there is no wage growth. The labor market cost of motherhood arises solely from foregone wages, the present value of which decreases with delay. In Case 2, annual wage growth exceeds the discount rate, making early motherhood less costly. Steeper wage profiles lead to larger financial benefits from early motherhood. Since wages grow independently of experience, terminal wage rates are invariant to fertility timing.

In the next three cases, wages rise with age because of increased worker productivity from on-the-job training and investment in human capital. The cost of a work interruption is the sum of the foregone wages and the foregone human capital accumulation:

$$\text{Foregone Wages} + \text{Foregone Human Capital} = \int_{A1B}^{A1B+Break} w(t)e^{-rt}dt + \int_{A1B+Break}^{T} [w(t) - w(t, A1B)]e^{-rt}dt$$

[7] The wage effects found in the panel estimates in Section 6 have been confirmed in later papers such as Ellwood et al. (2004) and Loughran and Zissimopoulos (2009) using OLS models. Both papers extend the analysis to consider career outcomes for men and Loughran and Zissimopoulos (2009) also estimates the effects of marriage timing.

where Break is the duration of the labor force interruption, T is age at retirement, *w(t)* are wages for a childless woman, and *w(t,A1B)* are wages for a mother who bore her child at age A1B. Having discussed discounting, I set the discount factor *r* to zero for the remaining cases. The first term increases in A1B, capturing the effect of wage growth in raising foregone wages for later interruptions. The second term deceases in A1B, as the foregone human capital investment is accrued over fewer years for later interruptions.

The equation describes Case 3, illustrated in Figure 1, where wages increase with experience, and there are no additional costs to motherhood. The "childless" line represents the uninterrupted wage profile. The effects of motherhood are shown, for A1B=23 and A1B=30, as: (1) zero earnings during the interruption and (2) a return to pre-motherhood wage level after the break. The effect of an interruption is a horizontal shifting of the profile for the duration of the break. The terminal wage profile and total career earnings (area under the wage profile) are the same for all mothers, irrespective of timing. The geometric intuition for this is that an interruption is a slice and shift of the profile - the location of the slice has no effect on the total area. The total cost of any interruption is the value of earnings during the terminal period (equal in duration to the interruption) on the uninterrupted profile. This equivalence does not depend on a particular functional form for the relationship between wages and experience.[8]

Case 4 introduces a fixed cost, which can be a motherhood penalty (lower productivity or employer response to a negative signal) or depreciation of human capital during career interruptions. This is depicted in Figure 2 by a vertical downward shift in the mother's wage profile. Here, lifetime earnings increase with delay, since later mothers work more years on the higher (un-depreciated) profile. Of course, human capital depreciation may take a form other than a fixed linear drop. Depreciation can increase with experience, if the loss of capital is proportional to the amount accumulated, or decrease, if women with more seniority are better able to protect their human capital assets. In these situations, terminal wage rates will also vary with motherhood timing.

A final generalization allows for a reduction in returns to work experience following the birth of a child, depicted in Figure 3 as a flattening of the wage profile following motherhood. Mothers may experience reduced opportunities for training and promotion, and find themselves

[8] It does depend on the assumptions of no discounting and that the duration of the break is independent of the timing of motherhood. If women work part-time after childbearing, the exact equivalence only holds in the case of linear returns to experience.

relegated to a "mommy track."[9] Alternatively, mothers may choose to invest less in developing new skills at work, and consequently gain less on-the-job training. When early career contests conflict with early motherhood, women may benefit professionally from delayed motherhood.[10] In these situations, women who defer motherhood receive higher earnings and higher terminal wages.

These five stylized cases provide a conceptual framework for the empirical analysis by identifying the potential channels through which motherhood timing can affect wages and earnings. Motherhood delay improves career earnings if there is a fixed cost of motherhood, a reduction in returns to experience, or discounting greater than secular wage growth. Delay leads to higher post-motherhood wage rates only if depreciation costs are decreasing with experience, or if mothers experience a slope decline. The empirical approach in Section 5 relaxes certain simplifying assumptions by allowing multiple children, interruptions of varying duration, and part-time work. These labor supply effects of motherhood timing are accommodated by providing two specifications: one that controls for work experience, and one in which the total impact of delay is defined to include variation in experience.[11]

3. Identification using Biological Fertility Shocks

The first step in identifying the effect of motherhood timing on earnings is to control for observable factors associated with both, which can be accomplished through a variety of empirical methods. One approach is to estimate the parameters in the following equation:

$$\mathrm{Ln}(Y_i) = \beta_0 + \beta_{A1B}\mathrm{A1B}_i + \beta_X X_i + \varepsilon_i \qquad (1)$$

where Y_i represents earnings, wage rate or hours worked, $\mathrm{A1B}_i$ is age at first birth, X_i are the control variables, and ε_i is a random error term. The parameter of primary interest is β_{A1B}, the effect of fertility delay on the outcome variable.

OLS and other techniques produce biased estimates if A1B is correlated with the error term ε_i as a result of an omitted variable or reverse causality. Women may have differing

[9] For an anecdotal account from the legal profession, see Mumford's experience of motherhood as an associate at a major New York City law firm in *American Lawyer* (2003).

[10] This may be especially relevant for women working in academia (Mason and Goulden, 2002), law, medicine, and accounting.

[11] In this case, the thought experiment must be altered to allow for the duration of the break to vary with motherhood timing, as a result of some optimization that depends on age at first birth. The "total effect of delay" is defined to include the effect of increased labor supply.

priorities regarding family and career that lead some women to both invest less effort at work and to begin childbearing sooner. Common unobserved social group factors that affect both family and career choices can produce a spurious correlation, and women with higher earnings potential may choose to delay motherhood to reduce the financial penalty. Controlling for pre-motherhood trends in wages can alleviate some of the bias from omitted variables, but will not address the general problem of endogeneity: bias will remain if motherhood decisions respond to actual or *anticipated* career outcomes or if family and career choices are jointly determined. If later mothers face higher potential earnings profiles than earlier mothers, OLS will overstate the earnings benefits of deferred motherhood. Alternatively, if women with higher earnings potential are the ones who have children sooner, OLS will underestimate the effect of delay.[12]

This paper exploits three biological fertility shocks as instrumental variables (IV) to obtain consistent estimates of β_{A1B}:

- an indicator for first pregnancy ending in miscarriage or stillbirth
- an indicator for "accidental" first pregnancy occurring while using contraception, and
- the lag in years from first attempt to conceive to first birth.

These variables capture random and unanticipated factors that drive a wedge between the actual timing of motherhood and optimal or expected timing. A1B is modeled as the product of the woman's desired timing and a series of random shocks, including the instruments, represented in the first stage regression:

$$\text{A1B}_i = \gamma_0 + \gamma_M \text{Miscarriage}_i + \gamma_C \text{Contraception}_i + \gamma_L \text{YearsToBirth}_i + \gamma_X \text{X}_i + \upsilon_i \qquad (2)$$

where X_i contains the controls from the wage equation. Miscarriage and extended time to conception measure unpredictable events that increase A1B, while accidental pregnancy captures a similar failure of contraception that decreases it. The validity of the instruments depends on their predictive power in the first stage regression and their lack of correlation with the error term ε_i from Equation 1. The former can be tested directly, and the strength of the instruments is shown in Section 5.2, but the exclusion restriction cannot. Instead, I draw on a combination of biological evidence and indirect statistical tests to support their exogeneity.

Female fecundity is known to decline with age, leading to the concern that miscarriage and time to conception are correlated with age at first pregnancy attempt. However, the fecundity

[12] This would happen, for example, if women who are more successful and energetic in their careers are also more "successful and energetic" in their personal lives, marrying and bearing children sooner.

drop is highly nonlinear, and is apparent primarily for pregnancies after age 33, the highest age at first birth included in the sample.[13] Another concern is that miscarriage may be associated with risky behavior. Medical evidence does not support a strong impact of behavioral factors on miscarriage risk. Rather, over 85% of miscarriages occur within the first trimester of pregnancy and over 90% are caused by genetic defects or other anomalies that prevent the fetus from developing properly (Merck, 1999). Miscarriage has been associated with some extreme behaviors such heavy alcohol use or drug addiction.[14] Fortunately, these factors are observed and controls are included in the regressions. While miscarriage may adversely affect a woman's psychological state, this is more likely for recurrent miscarriage, which affects less than 1% of women (Regan, 2001). Nevertheless, if miscarriage is negative related to unobserved maternal health, the estimated effect of delay will be downward biased.

One concern regarding the "accidental" pregnancy variable is that women may differ in contraceptive practices in ways that are correlated with earnings. In addition to including an indicator for any contraceptive use prior to first birth, I also show (in Section 5.2, Table 3) that the effect of accidental pregnancy is not explained by differences in *type* of contraception used. Although a source of bias remains if the consistency with which a woman used contraception (the intensive margin) is correlated with her productivity at work, conditional on covariates, this seems less likely in light of the fact that the extensive margin (on type of contraception) was not a driving source of the variation. Time to conception may also be correlated with unobserved productivity at work, but the more plausible direction for the bias is negative, against finding an effect of A1B.[15]

A final concern that applies to all of the instruments (Ellwood et al., 2004) is that the data may be contaminated by systematic misreporting of contraceptive use or miscarriage among women who intentionally aborted. Systematic misreporting, if present, would likely be related to a woman's religious beliefs and attitude towards abortion, contraception and motherhood. A spurious correlation would exist between religion and the measured instrumental variables, while

[13] One way to address the concern of declining fecundity is to estimate the effect of delay separately women who had their first child within different age ranges. Due to the small sample size, this exercise does not produce precise estimation results.

[14] Regan (2001) describes miscarriage risks associated with heavy alcohol use, drug use (cocaine, marijuana and cannabinoid compounds), and regular smoking during pregnancy.

[15] Negative bias would occur if women who are better organized at work are able to conceive more quickly, or if women with higher unobserved productivity choose to target their A1B timing more precisely and use contraception until they desire motherhood.

the true instruments would be uncorrelated. In fact, neither a woman's religious affiliation (at birth or in the present), nor her frequency of attendance at religious services, has any statistical power in predicting any of the three instruments. In regressions of each of the instruments on the basic controls and indicators for religion, the religion variables were statistically insignificant, both individually and jointly, which implies that misreporting is unsystematic in relation to beliefs.

In Appendix Table 1, I report the results of a more detailed exploration of the relationships between the instrumental variables and other observable background factors reported in the dataset. Each estimate in the table is from a separate regression using the main cross-sectional estimation sample, in which the only controls are the variable listed for that row and a constant term. The first column shows significant relationships between A1B and background factors that predict wages. Women with higher age-adjusted AFQT scores, who lived in homes (at age 14) with magazines, newspapers or a library card, whose parents had completed more years of schooling and who have fewer siblings tend to become mothers at later ages. In contrast with these strong associations for the endogenous variable of interest, the associations with the IVs reported in the later columns are, for the most part, economically and statistically insignificant. The few relationships that are statistically significant at the 10 percent level (1 out of every 13 tests) likely reflect random chance. If anything, these statistically significant associations suggest a *negative* correlation between predicted A1B and wages, which, if present, would bias the IV estimates downwards. The only significant predictor of miscarriage is the absence of magazines at home; the statistically significant predictor for an unintended first pregnancy is having a father with more schooling; and the significant predictor for duration of the time to conception is the absence of newspapers at home.

I conducted two additional indirect tests of the instruments. The first exploits the fact that, with three IVs for a single endogenous variable, the system is over-identified. The implied restrictions are tested in Section 5.2; exogeneity of the IVs is not rejected, and the three instruments produce similar estimates for β_{A1B}. Section 5.2 also contains a falsification test further supporting the validity of the instruments. The variables are shown to predict wages following motherhood, but not preceding it.

An additional complicating factor in our interpretation of the β_{A1B} estimate stems from the fact that motherhood delay may have heterogeneous career effects for different women. In

particular, IV estimates are a local average treatment effect (Imbens and Angrist, 1994), weighted across women by their fertility timing response to the instruments. Fortunately, some of the most important factors leading to heterogeneity are observable, and can be studied directly. Therefore, in Section 5.2, I estimate the effects separately by education level, occupation type, employment sector, spousal income and family leave regime.

4. Data Description

The data are from the NLSY79 public use files (1979-2000).[16] The survey is uniquely suited to this exercise as it combines detailed labor market information with pregnancy, childbirth and contraceptive use histories. The analysis excludes the military, minority and disadvantaged white over-samples;[17] the remaining respondents constitute a nationally representative sample of over 6,000 women, who were between the ages of 14 and 22 at their first interviews. After 1979, they were interviewed annually until 1994, and then biennially.

To measure the effects of motherhood timing, the sample is restricted to women with at least one child born during the survey period. To focus on potential trade-offs between work and family, the sample is further limited to those who had their first birth between the ages of 21 and 33, and between the years 1983 and 2000, excluding teen mothers (studied in Hotz et al., 1999). However, the results unchanged if younger mothers are included.[18] Several other restrictions are motivated by data limitations: earnings observations are available for all birth year cohorts for the age range from 21 to 34, and contraceptive information starts in 1982. The number of potential observations is about 1500 women, but the actual number is lower due to missing variables and attrition.[19] Summary statistics are reported for the main variables in Table 1.

[16] Confidential Geocode data are used for state level identification of family leave regime. Additional testing using data through 2006 confirms the main findings for career outcomes between ages 21 and 40. Unfortunately, attrition from the survey after 2000 substantially reduces the sample size for the analysis using later years of data.

[17] The military and the economically disadvantaged samples were survey until 1983 and 1988, respectively.

[18] For example, if the IV regression on total earnings from Table 2 is repeated on the expanded sample, the A1B coefficient is 0.088 with a standard error of 0.024. The IV estimate for average wages is 0.031 (s.e. of 0.012) and for total hours of experience is 0.038 (s.e. of 0.015).

[19] For the main cross-sectional sample of career outcomes, earnings between ages 21 and 34 are known for 3,043 women. Of those, 492 were childless by 2000 and 135 had missing fertility information. The age at first birth restrictions eliminate another 719 women with teenage pregnancy leading to first birth and 126 women with first birth after age 33. The year at first birth restriction drops another 226 women. Missing values for the instrumental variables and substance abuse controls are responsible for the remaining sample loss and variation. The cross-sectional sample for terminal wages rates and the panel sample of wages and wage growth are restricted to working women who report wages at the relevant ages.

The dependant variables used in the empirical analysis are, in logarithmic form: total career earnings (between ages 21 and 34), total career hours worked, average career wage rates, hourly wage rates for each age between 21 and 34, and changes in hourly wage rates. For the period in which the survey is biennial, wage rates and income for missing years are imputed with linear interpolation.[20] Earnings are aggregated using inflation rates from the Bureau of Labor Statistics calculation of the Consumer Price Index. Results are not sensitive to using the Federal Reserve discount rates or Treasury bill 1-year risk-free rates instead. For robustness, career earnings are also calculated over expanded windows: from age 21 to 35, 21 to 36, and on, until 21 to 42. The longer windows include smaller samples of women, but yield substantially similar results for career effects. Nevertheless, one should remain cautious about extrapolating the findings to motherhood delays past age 33. Hours worked each year are computed using actual hours reported in the work history files (covering all weeks from 1979 to 2000), and averaged over missing weeks within a year. Periods of non-participation are included as zeros; they do not induce sample selectivity for career earnings. The average wage rate is obtained by dividing career earnings from 21 to 34 by the number of hours worked during that period.[21] The measure used for working women's wage rates is the self-reported hourly wage paid at the primary job.

The basic controls are: birth year cohort, race, education level, and ability. Date of birth and date of interview are reported in the survey. Race is summarized by indicator variables for Black and Hispanic, with the omitted category consisting primarily of White. Educational attainment is represented by dummy variables for High School Diploma and College Degree. The omitted category is high school dropouts, and College Degree also includes women with advanced and professional degrees. Armed Forces Qualification Test (AFQT) percentile score achieved in the 1980 test measures aptitude.[22]

For the exploration of heterogeneous effects, each woman is assigned to an occupation group based on predominant and most recent employment, among the categories: professional, technical and managerial; sales and service; clerical and kindred; and crafts, operatives, laborers, farmers and foremen. The family leave regime indicator is set to 1 for women who bore their

[20] Results are unchanged if wages in missing years (1994, 1996 and 1998) are omitted or if the sample is limited to women aged 18 and older in 1979. The estimated effect of A1B on career earnings are 10% and 12% respectively, each statistically significant at 1%.
[21] This is equivalent to a weighted average, by hours worked at that rate, of all wage rates received during the period.
[22] NLSY79 respondents were not rewarded based on their test performance. As a result, scores most likely reflect some combination of ability and motivation, both of which matter for labor market outcomes (Segal, 2006).

first child in a state-year with a mandated protection rule. This applies to all women after the 1993 federal Family and Medical Leave Act, which provides up to twelve weeks of unpaid leave following the birth of a child (or adoption, or a medical condition affecting oneself or a family member), as long as the employer size and previous tenure conditions are met.[23] In addition, several states passed laws offering comparable protection prior to 1993.[24] When state laws are used for interactions, state fixed effects are included as controls, and identification is provided by state law changes during the sample period.

Survey questions about pregnancy outcomes provide data for the first IV, an indicator for miscarriage at first pregnancy. For women whose first pregnancy ended in abortion, the outcome of the first non-aborted pregnancy is used.[25] The indicator variable for contraceptive use at the time of pregnancy is taken from a combination of two questions asked after each live birth. The first asks if the woman had used contraception prior to the pregnancy, and the second, if she had ceased contraceptive use prior to conception. The indicator is intended to identify women who first became pregnant "accidentally" and in spite of precautionary efforts.

The third instrument, time to first conception, is constructed using biennial information on contraceptive use. Among women who report prior contraceptive use, a woman's first conception attempt is defined to start in the first year that she reports sexual activity and no contraceptive use. The lag is defined as the number of years between the woman's first attempt and the birth of her first child. For women who never report using contraception or whose first attempt is identified after their first birth (about half the sample), the variable is set to zero, making it a nonlinear function of A1B and observed pregnancy attempt.[26] A companion dummy variable for "reported contraception prior to first birth" is included to remove the potential bias from contraceptive use. To account for maternal behaviors associated with an increased risk of

[23] Ruhm (1997) outlines the provisions, coverage, and consequences of the Family and Medical Leave Act.
[24] Waldfogel (1999) is the source for state maternity leave rules.
[25] Hotz et al. (1997) estimates bounds on the effects of teen motherhood that incorporates potential contamination of the miscarriage natural experiment from misreporting, and from latent abortion-types who miscarry. These bounds confirm the qualitative instrumental variables findings. The problem of latent abortion-types in the miscarriage group should be smaller in this paper, because the present sample is limited to women who become mothers after age 20, and abortion rates are substantially lower (less than half as high) for these later pregnancies.
[26] This instrument may be susceptible to measurement error, especially in the date of the initial pregnancy attempt. It is reassuring to note that the main results are largely unchanged when it is removed: the effect of delay on career earnings and wages increases to 0.16 (s.e. of 0.023) and 0.07 (s.e. of 0.038), respectively, and the effect on hours decreases to 0.03 (s.e. of 0.04).

miscarriage, control variables are added for substance use during first (or earliest reported) pregnancy, such as smoking cigarettes, drinking alcohol, and using marijuana or cocaine.

Since the key variables are based on self-reported miscarriage and contraceptive use, there is a danger that systematic misreporting could compromise the identification strategy. Hence, it is worthwhile to confirm their validity with outside sources. The incidence of miscarriage at first pregnancy in my sample is 14%, which is in line with Regan (2001), who cites medical estimates of about 15% of all recognized pregnancies ending in miscarriage. While a direct comparison of contraceptive use variables is not available, some general features of the NLSY match data from independent sources such as the Alan Guttmacher Institute and the National Surveys of Family Growth (NSFG). The most popular type of contraception for childless women is the birth control pill, although the pill has been somewhat replaced by condoms for women in more recent cohorts.

The dummy variable for unintended pregnancy has a mean of about 0.32 in my sample. This may seem high when compared with failure rates of properly used contraceptive methods, but is lower than the NSFG reported rates of unintended pregnancy (57% of pregnancies in 1987).[27] The NLSY79 also asks if pregnancies were "desired." Women in the sample are more than willing to report that their children were not planned; in fact, the fraction of children who are "intended" is quite low. For example, consider responses to the 1982 question, "Just before you became pregnant the first time, did you want to become pregnant when you did?" 11.6% of respondents said "Yes," 4.2% said "Didn't matter," 63.8% said "No, not at that time," and 20.5% said "No, not at all." These comparisons provide some corroboration for the accuracy of reported miscarriage and contraceptive use in the NLSY79, and support the claim that biological shocks and unplanned conception play important roles in the fertility outcomes of US women.

5. Effects of Motherhood Delay on Earnings, Hours Worked, and Wage Rates

5.1 Empirical Framework

The empirical analysis begins with direct estimation of the effects of motherhood delay on career outcomes. The unit of observation is an individual woman, and the baseline model takes the form of Equation 1. This model is estimated in OLS, and then IV, using the biological fertility shocks.

[27] One difference in rates of accidental pregnancy and accidental live birth is abortion. Another difference between the NSFG estimate and my sample is that I exclude teenage mothers.

The dependent variables are selected to evaluate separately the effects of fertility timing on career earnings, career wage rates, career labor force experience and terminal wage rates, as motivated in Section 2. Due to data constraints, "career" outcomes are aggregated over the age range from 21 to 34, and "terminal" wages are at age 34, which allows the same age range to be used for all women from different birth year cohorts.[28]

Heterogeneity is explored by adding interaction terms between age at first birth and education level, occupation type, employment sector, spouse earnings, and family leave regime. In these specifications, the additional instrumental variables are the interactions of the original instruments with the exogenous variables of interest. My hypothesis regarding family leave is that it can reduce the returns to deferred motherhood by alleviating the career-motherhood conflict. If the motherhood penalty is concentrated among women who are not able to return to the pre-interruption jobs (Baum, 2002), then job protection should reduce the costs of interruption. It should have the largest effect on the most vulnerable workers, young women with brief tenures. A negative A1B·FamilyLeave coefficient would indicate that delay is less important under Family Leave and support the hypothesis that mandated protection is an effective substitute for seniority in preserving women's career opportunities after motherhood.

5.2 Results

Table 2 reports results for career outcomes: delayed motherhood leads to higher earnings and wage rates and more hours worked. OLS and IV estimates attribute a nearly 10% increase in earnings to each year of fertility deferment. Women with higher AFQT scores and greater educational attainment receive higher earnings, and minority women earn more than white non-Hispanic women, conditional on covariates.[29] The birth cohort variable is defined as age in 1979. The negative coefficients imply that later-born women had higher earnings and labor force participation, which reflects a mix of differences across cohorts in labor supply (due to preferences or unobserved ability) as well as secular differences in labor demand (from macroeconomic forces).

[28] Results are robust to expanding the career window as far as the maximum observed age of 42 or to including additional data from later surveys and estimating career outcomes between ages 21 and 40 for women of all birth year cohorts.

[29] This is an interesting reversal of the unconditional relationships in the data, where Black and Hispanic women report lower earnings. The main source of the change is the inclusion of the AFQT control variable, but the sample restriction (to exclude teen mothers) also plays a role.

Later mothers tend to have fewer children by the age of 34 than earlier mothers. To account for this potential difference in labor supply, number of children is added as a control in column 3, reducing the coefficient on A1B to about 6%. To account for potential endogeneity in the number of children, I estimate an additional model using the main biological shocks as instruments for both motherhood timing and number of children. This produces a non-significant negative effect for number of children, and a coefficient estimate of 0.061 (standard error of 0.037) for A1B. The lack of separate instruments for number of children limits the power of this specification, however. Since the effect of child quantity is not well-identified, it is excluded from the regressions that follow, which decompose the total effects into hours and wage rate contributions.

If labor force participation is not affected by fertility timing, as in the fixed-interruption cases presented in the conceptual framework, total earnings and average wages should be equivalent outcome measures. However, the IV results in column 7 indicate that each year of motherhood delay leads to 6% more hours of work between ages 21 and 34. This is not solely an artifact of the early cutoff age (when late mothers may not have completed their interruptions), as it persists when the cutoff is relaxed past age 34. Instead, the effect likely reflects differences in the labor market and childcare options available to new mothers of different ages.[30] Because of the systematic differences in hours worked, the results for earnings confound several factors. Early mothers earn less, but also consume more leisure and home time. For this reason, it may be easier to interpret results for the average wage rate. The IV estimate in column 5 shows a 3% increase in wages rates per year of deferment, with a standard error of 0.010.

Additional regressions including controls for marital history and birth year dummies are not reported in the table, but show similar patterns. Over 90% of the women in the sample were married prior to their first birth and 63% were never divorced. After controlling for these two factors, the IV point estimate of A1B for earnings increases to 12% and average wage rate to 3.6%, both significant at 1%. An artifact of the data selection rule is that women from earlier birth cohorts are over-represented among the older mothers. To ensure that the A1B results are not driven by non-linearity in the effect of birth year, regressions are run using birth year

[30] This is confirmed in separate Probit and IV-Probit results, where A1B is found to be a positive predictor of working full time in the year, 2 years and 3 years following first birth.

dummies, again leading to substantially similar results: A1B coefficients of 10.2% for earnings and 3.3% for wages, significant at 1%.

Table 3 reports estimation of the first stage regression (Equation 2). Coefficients on the instrumental variables listed in the first column are reasonable in both sign and magnitude: miscarriage defers first birth by about 6 months, and unintended pregnancy hastens motherhood by about 8 months. A year of conception lag is associated with 9 months of delay, yielding an average delay of 1 year and 2 months at the sample mean of 1.5. Typically, only one of the IVs applies to a given woman, so the instruments are causing about 6 months to a year of fertility shifts. These are small, but not trivial, effects that provide identification in the second stage, where the A1B coefficient should be interpreted as the average effect of a year of motherhood delay due to biological fertility shocks.[31] The second column shows estimates from the first stage regression including controls for type of contraception, and the third column includes these controls and additional interaction terms between contraception type and failed contraception (the estimate for failed contraception in the third column is for the omitted group of women who report using an "other" form of contraception).[32] The instruments are powerful predictors of A1B. They are individually statistically significant, and the F-statistic on their joint significance is over 100 in the main specification. The partial R-squared of the instruments is 0.25.

Results from tests of the over-identifying restrictions are reported in the final rows of Table 2. Exogeneity is never rejected at conventional significance levels. In a related exercise, the regressions are repeated omitting one of the instruments in turn. Coefficient estimates remain qualitatively identical and statistically indistinguishable. For example, the coefficients on A1B in the baseline log-earnings equations are 0.09, 0.10 and 0.16 when the instruments are omitted for failed contraception, miscarriage at first pregnancy, and time to conception, respectively.

Regressions that allow for heterogeneous effects of A1B are reported in Table 4. Columns 1 and 2 show OLS and IV results by occupation type. Motherhood delay has the largest, and most statistically significant, benefit on the average wage rates of women in professional or managerial occupations (IV coefficient of 0.047), followed by those in clerical

[31] Since shocks occur during the entire period, the estimate captures the average effect of a delay across women of different ages. Regressions were run using polynomial terms in A1B to consider non-linearity in the effect of delay by A1B, but coefficients were imprecisely estimated.

[32] As a robustness exercise, the main regressions on career and terminal wage outcomes were repeated with the full set of indicators for contraceptive type. In each case, the A1B point estimate became slightly larger and remained statistically significant. Hence, the results are not being driven by differences in the type of contraception used.

occupations (coefficient of 0.022). Sales and service workers have negligible returns of less than 1% per year of delay, and women in the crafts and manual labor have marginally significant negative returns of about 10%. The A1B·(Prof/manager) is significantly different from A1B·(Craft/labor) and A1B·(Sales/service), although it is not significantly different from A1B·Clerical. By contrast, the OLS model yields significant A1B coefficients for women in professional/managerial, clerical, and sales/service occupations, with the first term actually lower than the IV estimate. In contrast with the results in Table 2, where OLS and IV estimates are nearly identical, this table suggests a more complex story for selection into later motherhood by potential earnings: there appears to be negative selection within professional and managerial occupations but positive selection for the other women. Column 4 contains IV estimates by education level, where college educated women experience a substantial wage benefit from motherhood delay (of 5% per year of delay) that is significantly larger than the benefit to women with less formal education (at the 5% significance level). In a similar pattern to the split by occupation, selection into later motherhood appears to be negative for college graduates but positive for other women.

The results by education and occupation indicate that motherhood delay is most important for women working in careers characterized by ongoing human capital accumulation and costs for interruptions. These results also help reconcile the main finding of this paper with the finding in Hotz et al. (1999) that avoiding teen motherhood does not improve labor market outcomes. As discussed in that paper, teen mothers tend to be women with lower expected earnings, regardless of childbearing status, and with educational and occupational characteristics that make them less likely to benefit from motherhood delay.

The IV interaction term for government employment (in Column 6) is negative, but not statistically different from zero.[33] Family leave regime influences neither wage rates nor returns to delay. The OLS coefficient on A1B·FamilyLeave is -0.003 with a standard error of 0.009, and the IV estimate is –0.010 with a standard error of 0.020.[34] The coefficients on FamilyLeave

[33] Public sector jobs have been associated with smaller motherhood wage penalties in Europe (Nielson et al. 2004). If this is true in the US as well, the impact of A1B should be smaller for government workers, which is consistent with the negative point estimate. In an unreported regression, the interaction of A1B with spouse earnings is also negative and statistically insignificant.

[34] In another specification, state laws were ignored, and family leave was presented by an indicator for first birth occurring after the Family and Medical Leave Act (FMLA). The coefficient on A1B*(FMLA) is 0.018 in OLS and 0.037 in IV.

range from –1.11 (standard error of 1.20) to 0.44 (standard error of 0.53). The direct effects of family leave and interactions with A1B are imprecisely estimated and qualitatively inconsistent, providing no supporting evidence for the role of mandated leave in mitigating the conflict between career and early motherhood.

As discussed in Section 2 (Case 4, Figure 2), in the special case where the only effect of motherhood is a fixed cost of human capital depreciation, fertility timing affects career earnings and average wages, but not terminal wage rates. Table 5 contains results from estimating Equation 1 with wages at age 34, representing terminal motherhood wage rates, as the dependent variable. The OLS coefficient on A1B in column 1 implies that a year of fertility deferment is associated with a 4% increase in wages, statistically significant at the 1% level. The baseline IV estimate in Column 2 is lower, around 3% and is less precisely estimated (significant at 5%). Columns 3 and 4 show how the A1B coefficient is affected by the inclusion of experience hours as a control, first as an endogenous variable using the core instrumental variables, and then as an exogenous control. The measure of experience is the fraction of hours worked from age 21 as a share of full-time full-year employment. The first change increases the estimate of the A1B coefficient to 4%, while the second reduces it to 2%, and renders it statistically insignificant.

The final column shows how a Heckman two-step correction for sample selectivity affects the results. In this case, the probability of working at age 34, and of generating a wage rate observation, was predicted in a Probit model using all of the controls from the main regression, and the additional participation instruments of marital status and spouse earnings. Additional variations are estimated, such as treating A1B as endogenous in the first step, and including higher order polynomial terms of the Inverse Mills Ratio in the second step. In the first stage, A1B does not significantly affect participation at age 34. Across specifications, the A1B coefficient on wages is consistently positive and around 3.5%. The positive effect of delay on terminal wage rates suggests that depreciation costs are lower for older mothers or that women experience a reduction in wage growth following motherhood.

In an additional test of the exclusion restrictions, a similar IV specification is used to measure the impact of A1B on wages *prior* to motherhood (wages at ages 22 and 25 for women who become mothers after ages 22 and 25, respectively). The coefficients are negative (-0.01 and -0.026) and statistically insignificant (standard errors 0.011 and 0.025). This falsification test

provides further support for the claim that the instruments are not operating through a correlation with fixed omitted factors.

6. Effects of Motherhood on Wages and Wage Growth

6.1 Empirical Framework

After estimating the effects of motherhood timing on career and terminal wages using cross-sectional data, I estimate the effects of motherhood itself on wages using a panel dataset to uncover the underlying channel for the timing effects. As motivated in the conceptual framework, I investigate the presence of human capital depreciation and other fixed motherhood penalties, as well as a reduction in returns to experience for mothers.

The first panel regressions have log-wages as the dependent variable and include controls for individual fixed effects, age, motherhood status, and years since first birth. While more flexible representations are possible, the small sample size forces some parameterization to increase precision. The first specification includes only linear terms for wage growth, and the second adds quadratic terms:

$$\text{Ln}(\text{Wage}_{it})=\beta_0+\beta_1\text{Age}_{it}+\beta_2\text{Age}_{it}^2+\beta_3\text{Mother}_{it}+\beta_4\text{Mother}_{it}\bullet(\text{Age}_{it}\text{-A1B}_i)+\alpha_i+\varepsilon_{it} \quad (3)$$

where (Age_{it}-A1B_i) measures years since first birth, and α_i captures individual fixed effects. Instrumental variables estimation of this equation uses the predicted value from the first stage regression (Equation 2) with the original instruments ($\widehat{A1B}_i$) to create instruments for the motherhood indicator and interactions with years since first birth.[35] The new instruments are:

- $1(Age_{it} \geq \widehat{A1B_i})$, an indicator for Age being greater than predicted A1B, and
- $1(Age_{it} \geq \widehat{A1B_i})\bullet(Age_{it} - \widehat{A1B_i})$, the indicator interacted with predicted years since first birth.

In a third specification, the effect of motherhood is allowed to vary with the timing of first birth. The regression equation is:

[35] The original instrumental variables are fixed for a given woman, and cannot be used in a panel framework with individual fixed effects. Instead, they are interacted with the time-varying exogenous variable *Age* to produce new instruments. The new instruments are created by combining variables that are assumed to be exogenous in the wages equation (the original instruments and age). If the initial instruments are exogenous, it follows that the new instruments are exogenous as well. The exact form of the new instruments corresponds to the form of the endogenous variables.

$$\text{Ln}(\text{Wage}_{it}) = \beta_0 + \beta_1 \text{Age}_{it} + \beta_2 \text{Mother}_{it} + \sum_{h=1}^{2} \beta_{3,h} \text{Mother}_{it} \bullet (\text{Age}_{it} - \text{A1B}_i) \bullet \text{l}(\text{A1B}_i \in \text{FBCat}_h) + \alpha_i + \varepsilon_{it} \quad (4)$$

where the two FB Categories are: A1B between 21 and 27 and A1B between 28 and 33.[36]

While the first panel approach is informative, it has the unattractive property of constraining wage growth to be identical for all women. In order to relax that assumption, and allow for greater flexibility and heterogeneity in underlying wage profiles, I explore the effects of motherhood on wage growth rather than levels. The unit of observation remains a person-age, but the outcome variable is now the change in log wages over the next three years.[37] I consider the following specification:

$$\Delta\text{Ln}(\text{Wage}_{it}) = \beta_0 + \beta_1 \text{FirstBirth}_{it} + \beta_2 \text{FirstBirth}_{it} \cdot (\text{A1B-20})_i + \beta_3 \text{X}_i + \delta_t + \varepsilon_{it} \quad (5)$$

where X_i contains the basic control variables in Table 2, and δ_t are fixed effects for age to capture nonlinear wage growth. The dummy for FirstBirth is set to one in the year of the first birth. β_1 is a measure of the effect of motherhood on wage growth, and β_2 captures how this effect varies by age at first birth. A negative coefficient on β_1 indicates a temporary reduction in earnings growth associated with motherhood, while a positive coefficient on β_2 indicates that the reduction is smaller for older mothers. To account for changes in work hours surrounding first birth timing, specifications are also estimated controlling for hours worked during the period.

Using the change in log-wages as the dependent variable makes the model comparable to a model of log-wages in levels with individual fixed effects that remove average earnings for each woman. The specification in Equation 5 is more flexible, though, since it allows wage growth to vary with individual factors, and for the impact of first birth to vary with its timing. In a related specification, individual fixed effects α_i are included to absorb personal idiosyncrasies in the wage profile not accounted for by education, race, aptitude and birth year cohort. In addition, to allow for a lasting effect of motherhood on wage growth, I estimate the equation:

$$\Delta\text{Ln}(\text{Wage}_{i,t,t+3}) = \beta_0 + \beta_1 \text{FirstBirth}_{it} + \beta_2 \text{FirstBirth}_{it} \cdot (\text{A1B-20})_i + \beta_3 \text{Mother}_{it} + \delta_t + \varepsilon_{it} \quad (6)$$

[36] Results are similar when 3 categories are used for first birth from 21 to 24, 25 to 29 and 30 to 33.
[37] Using two-year and four-year intervals yielded similar but smaller coefficients.

which contains an additional term for motherhood, with and without individual fixed effects α_i. Controls for factors that affect wage growth improves upon previous panel studies, but the problem of endogenous fertility remains. Hence, Equation 6 is estimated with the IVs:

- $1(Age_{it} \geq \widehat{A1B_i})$,
- $1(Age_{it} + 1 \geq \widehat{A1B_i} \geq Age_{it} - 1)$, and
- $1(Age_{it} + 1 \geq \widehat{A1B_i} \geq Age_{it} - 1) \bullet (\widehat{A1B_i} - 20)$.

6.2 Results

Table 6 contains results from the three primary specifications for log-wages: linear, quadratic (Equation 3), and quadratic wage growth with costs of motherhood that vary with first birth timing (Equation 4). Standard errors for OLS models are robust and clustered at the individual women level. For the IV estimates, standard errors are estimated by bootstrapping with 1,000 replications using clustered sampling at the individual woman level.

Mother and *Mother·YearsSinceFirstBirth* have large and significant negative effects across all specifications, indicating that mothers suffer both fixed wage penalties and lower wage growth. Figure 4 plots simulated wage profiles, based on the IV coefficients of the linear model in column 4. The solid line represents the wage profile for a childless woman, while the dashed and dotted lines show the expected profile for that same woman, with A1B=25 and A1B=30. The key elements apparent in the figures are that women: (1) return to work at lower wages, and (2) experience lower growth in wages, following first birth. Together, these factors produce the career and terminal wage benefits from motherhood delay estimated in Section 5.

When the changes in wage growth for mothers are allowed to vary by first birth timing, the penalties are larger for younger mothers. In the OLS model (column 3), the flattening of the wage profile is significant for both earlier (21≤A1B<27) and later (28≤A1B<34) mothers, but more important for the former group. In the IV model in column 6, the flattening of the profile is only present for earlier mothers.

The results from the growth model are in Table 7. The first column corresponds to Equation 5, which constrains mothers to be on the same wage profile as non-mothers, but with the *FirstBirth* variable, allows for a temporary reduction in wage growth in the period immediately following initial fertility. This variable is analogous to the *Mother* variable in the

levels model in the previous table that captured permanently lower wages following first birth. In fact, the coefficient estimates are quite similar, on the order of a 10 percent reduction in wages. This reduction in wages corresponds to the fixed motherhood penalty in Cases 3 and 4 of Section 2, and reflects penalties associated with career interruptions as well as foregone human capital investment during the labor market absence. The positive (1% in OLS and 2% in IV) coefficients on *FirstBirth*·(A1B-20) indicate these costs are lower for older mothers.

In columns 3-4 and 7-8, a *Mother* indicator is included to accommodate a permanent change in the *slope* of the wage profile following motherhood. This corresponds to Cases 4 and 5 of Section 2. The coefficient is only significantly different from zero in the OLS models, but the negative point estimates across specifications does suggest a potential flattening of wage profiles for mothers, consistent with the negative *Mother·YearsSinceFirstBirth* coefficients in the previous table. Adding the *Mother* variable does not significantly change the *FirstBirth* or *FirstBirth*·(A1B-20) coefficients. The IV estimates for the effects of motherhood on wage growth, in columns 4 and 8, although very imprecisely estimated, do tend to reproduce the qualitative features of the OLS models and provide some corroboration. The lack of precision may not be surprising considering that the dependent variable measures changes over time while the variation in the biological shocks is entirely cross-sectional across women.

These panel data estimates demonstrate a potential mechanism for the positive career and terminal wage effects of motherhood delay estimated in Section 5 and shown in Tables 2 and 5. In the main IV panel estimates in Table 6, column 4, motherhood is associated with a downward shift of the wage profile by 10.7% and a flattening of its slope by 3.4%. The vertical shift on its own implies no difference in terminal wage rates for women who delay motherhood, but it does imply an increase in total career wages (over a 14 year career) of 0.7% per year of delay. The flattening of the profile implies an additional percentage increase in career wages of 3.4**NYDelay**(0.5**NYDeay* + *NYPost*)/14 for a delay of *NYDelay* years with *NYPost* years of work remaining after the later interruption. For a typical woman in the sample with A1B of 26, the two effects together would imply that a 1 year delay would increase the average career wage rate by about 2.5% which is quite comparable to the 3% estimated career wage effect in Table 2, column 5. The same women, by delaying her shift to a flatter wage profile by an additional year, would also experience a predicted increase in her terminal wage rate of 3.4% from a year of motherhood delay. This is consistent with the cross-sectional estimate of a 3% gain in terminal

wage rate, reported in Table 5, column 2. This last exercise demonstrates the consistency of the cross-sectional and panel data estimates. It shows that the overall effects of motherhood timing in the previous section can be explained by the effects of motherhood on wage profiles found in this section.

7. Conclusion

This paper presents causal evidence that delayed motherhood increases both hours worked and wages, and hence career earnings, as well as post-motherhood wage rates. The direct panel evidence indicates a likely channel for the effect: a "mommy track." Women experience reduced earnings around the time of first birth, together with a flattening of the wage profile following motherhood.

Given the available evidence, it is impossible to determine precisely where such effects originate. On the supply side, mothers may reduce their hours in the labor market and invest less in skill development. From the demand side, employers may offer mothers fewer training and advancement opportunities. The two are likely interconnected. The importance of wage growth and, by implication, human capital investment, is echoed in the heterogeneity of the fertility timing effect: fertility delay improves career outcomes only for women with college degrees and those in professional or managerial occupations. As expected, women in careers with greater wage growth are the ones who gain financially from delaying motherhood.

The results indicate that women can achieve higher earnings by delaying motherhood during their twenties and early thirties. On the one hand, this suggests a way for women to reduce the career penalties from motherhood, which take the form of lower wages relative to men and childless women. Motherhood remains an obstacle to women's economic equality with men (Fuchs, 1988); its deferral may constitute an important mechanism for reducing that inequality. On the other hand, if one considers equality the benchmark, the financial rewards to delay represent an effective penalty for early motherhood. Viewed in this light, the findings of this paper elucidate a key component of the work-family conflict, by gauging the tradeoffs between early motherhood and career attainment. Family and children's advocates (Crittenden, 2001; Atkinson, 2003) argue that such tradeoffs signal a need for increased government intervention to support working mothers. Proposed remedies include mandating paid maternity leave, extending protected leave to all workers, and providing universal access to quality and

affordable childcare services. Although the Family and Medical Leave Act did not mediate the timing trade-off, a more extensive policy might.

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Table 1: Summary Statistics

Variable	Observations	Mean	Std. Dev.
Age at first birth (A1B)	1030	26.814	3.347
Age at first pregnancy	1020	24.597	4.112
First pregnancy ended in miscarriage	1030	0.135	0.342
Lag before first birth (years)	1030	1.459	2.449
Contraception at first pregnancy	1030	0.316	0.465
Positive lag before first birth	1030	0.499	0.500
Never divorced or separated	1030	0.626	0.484
Married before first birth	955	0.923	0.268
NPV earnings 21-34 (in 1999$)	1030	322,868	206,270
NPV earnings 21-40 (in 1999$)	893	471,482	308,805
Average weekly hours (age 21-34)	1030	26.12	10.76
Average weekly hours (age 21-40)	1015	26.48	11.39
Hourly wage rate at 22 (in 1999$)	2264	5.46	3.12
Hourly wage rate at 25 (in 1999$)	2244	6.91	6.14
Hourly wage rate at 34 (in 1999$)	1187	9.30	8.32
Number of children	1030	2.211	0.903
Number of children desired (in 1979)	1029	2.617	1.536
Number of children expected (in 1979)	1024	2.549	1.460
Black	1030	0.188	0.391
Hispanic	1030	0.150	0.357
No high school diploma	1030	0.039	0.193
High school diploma	1030	0.494	0.500
College degree or higher	1030	0.467	0.499
Alcohol during pregnancy	1030	0.537	0.499
Smoking during pregnancy	1030	0.238	0.426
Cocaine during pregnancy	1030	0.008	0.088
Marijuana during pregnancy	1030	0.025	0.157

Note: The summary statistics are for women in the NLSY 1979, who report at least one child by 2000, with a first birth between the ages of 21 and 33, and during the period from 1983-2000. For hourly wage rates, the sample includes all working women with reported wage rates at that age and with non-missing information on the instrumental variables and control variables. For the remaining variables, the sample is all women with a complete work profile between ages 21 and 34 and complete information on the instrumental variables and controls (corresponding to the main cross-sectional estimation sample used in Table 2). Some variables not used in regressions have fewer than 1030 observations due to missing values.

Table 2: Effects of Motherhood Timing on Career Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	IV	IV	OLS	IV	OLS	IV
Dependent Variable	*Ln(Earnings)*			*Ln(WageRate)*		*Ln(Hours)*	
A1B	0.0963**	0.0878**	0.0610**	0.0319**	0.0306**	0.0644**	0.0572**
	[0.0098]	[0.020]	[0.022]	[0.0052]	[0.010]	[0.0070]	[0.014]
Birth cohort	-0.0658**	-0.0623**	-0.0533**	-0.0259**	-0.0254**	-0.0399**	-0.0370**
	[0.015]	[0.016]	[0.016]	[0.0078]	[0.0086]	[0.011]	[0.012]
HS diploma	1.123**	1.127**	1.156**	0.237**	0.238**	0.886**	0.889**
	[0.16]	[0.16]	[0.15]	[0.083]	[0.082]	[0.11]	[0.11]
College or higher	1.258**	1.276**	1.308**	0.379**	0.382**	0.879**	0.894**
	[0.17]	[0.17]	[0.17]	[0.089]	[0.091]	[0.12]	[0.12]
Black	0.180*	0.174*	0.154+	0.0735	0.0726	0.106+	0.102
	[0.087]	[0.087]	[0.086]	[0.046]	[0.046]	[0.062]	[0.062]
Hispanic	0.301**	0.297**	0.300**	0.138**	0.138**	0.163*	0.159*
	[0.089]	[0.089]	[0.087]	[0.047]	[0.047]	[0.063]	[0.063]
AFQT percentile	0.827**	0.836**	0.935**	0.578**	0.579**	0.249*	0.257*
	[0.15]	[0.15]	[0.15]	[0.079]	[0.079]	[0.11]	[0.11]
Report contraception	-0.105+	-0.0933	-0.0666	-0.0293	-0.0274	-0.0756+	-0.0659
	[0.060]	[0.064]	[0.064]	[0.032]	[0.034]	[0.043]	[0.046]
Smoking	-0.144*	-0.146*	-0.172*	-0.0712+	-0.0715+	-0.0731	-0.0747
	[0.073]	[0.073]	[0.072]	[0.039]	[0.038]	[0.052]	[0.052]
Alcohol	0.135*	0.135*	0.127*	0.0805*	0.0805*	0.0549	0.0546
	[0.063]	[0.062]	[0.061]	[0.033]	[0.033]	[0.045]	[0.044]
Marijuana	-0.131	-0.13	-0.196	-0.179+	-0.179+	0.0479	0.0488
	[0.20]	[0.20]	[0.20]	[0.11]	[0.10]	[0.14]	[0.14]
Cocaine	-0.584+	-0.589+	-0.517	-0.163	-0.164	-0.420+	-0.425+
	[0.35]	[0.35]	[0.35]	[0.19]	[0.19]	[0.25]	[0.25]
# Children by age 34			-0.210**				
			[0.040]				
Observations	1030	1030	1030	1030	1030	1030	1030
R-Squared	0.27	0.27	0.29	0.24	0.24	0.18	0.18
	Over-identification test of instrumental variables						
Sargan statistic		1.767	0.511		1.277		1.053
P-value		0.413	0.775		0.528		0.591

Note: Dependent variables are calculated over the age range from 21 to 34, adjusting dollars for inflation. The sample includes all women with full experience profiles, who had their first child between the ages of 21 and 33, and the years 1983-2000. Standard errors in brackets.
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 3: Impact of Biological Shocks on Motherhood Timing

	(1)	(2)	(3)
First pregnancy ended in stillbirth or miscarriage	0.563*	0.440+	0.577*
	[0.24]	[0.26]	[0.24]
Years to conception of first child	0.743**	0.620**	0.746**
	[0.043]	[0.046]	[0.044]
First pregnancy while using contraception	-0.644**	-0.696**	-0.502*
	[0.18]	[0.19]	[0.25]
Control for contraception type	N	Y	Y
Interact failed contraception with contraception type	N	N	Y
Observations	1030	1026	1026
R-squared	0.42	0.32	0.43
F-test of joint significance of instruments	110.58	68.18	20.93
P-value for test of joint significance	0.000	0.000	0.000
Partial R-squared (for instruments)	0.25	0.25	0.25

Note: Dependent variable is age at first birth (A1B). Coefficients not reported for birth year, race, education level, AFQT, contraception use prior to motherhood, and substance use. The sample in column 1 is the same as in Table 2. Columns 2 and 3 are missing 4 observations due to missing contraception information. Standard errors in brackets. * significant at 5%; ** significant at 1%.

Table 4: Heterogeneous Effects of Fertility Timing on Career Wage Rates

	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV
A1B·(Prof/manager)	0.027	0.047				
	[0.008]**	[0.019]*				
A1B·Clerical	0.022	0.022				
	[0.007]**	[0.016]				
A1B·(Craft/labor)	0.016	-0.098				
	[0.018]	[0.055]+				
A1B·(Sales/service)	0.044	0.008				
	[0.009]**	[0.021]				
A1B			0.032**	0.002	0.033**	0.027*
			[0.006]	[0.014]	[0.005]	[0.011]
A1B·(College grad)			-0.001	0.051*		
			[0.009]	[0.023]		
A1B·Government					-0.018	-0.012
					[0.016]	[0.050]
Observations	1105	1016	1105	1016	1105	1016
R-squared	0.27		0.25		0.25	

Note: Dependent variable is ln(Average wage rate, age 21-34). Coefficients not reported for birth year, race, education level, AFQT, contraception use prior to motherhood, and substance use. Occupation type is suppressed from columns 1-2 and employment sector from columns 5-6. Samples include all women who had their first child between the ages of 21 and 33, and the years 1983-2000. Standard errors in brackets. + significant at 10%; * significant at 5%; ** significant at 1%.

Table 5: Effects of Motherhood Timing on Terminal Wage Rates

Dependent variable: Ln(WageRate$_{i,34}$)

	(1) OLS	(2) IV	(3) IV	(4) IV	(5) IV
A1B	0.0386**	0.0296*	0.0424+	0.0171	0.038+
	[0.0064]	[0.013]	[0.024]	[0.013]	[0.020]
Birth cohort	-0.0777**	-0.0735**	-0.0826**	-0.0662**	0.119
	[0.0088]	[0.010]	[0.017]	[0.010]	[0.218]
HS diploma	-0.022	-0.019	0.0711	-0.0977	0.354
	[0.10]	[0.10]	[0.17]	[0.097]	[0.226]
College or higher	0.213*	0.229*	0.320+	0.14	0.114
	[0.11]	[0.11]	[0.18]	[0.11]	[0.092]
Black	0.0819	0.0802	0.11	0.047	0.178*
	[0.053]	[0.053]	[0.075]	[0.051]	[0.079]
Hispanic	0.127*	0.124*	0.148+	0.0812	0.006**
	[0.053]	[0.053]	[0.077]	[0.052]	[0.001]
AFQT percentile	0.598**	0.610**	0.735**	0.494**	-0.064**
	[0.095]	[0.097]	[0.21]	[0.093]	[0.015]
Report contraception	-0.0703+	-0.0563	-0.0765	-0.0415	-0.13+
	[0.039]	[0.043]	[0.055]	[0.042]	[0.075]
Smoking	-0.0871+	-0.0903+	-0.0878+	-0.0831+	0.043
	[0.048]	[0.048]	[0.053]	[0.046]	[0.059]
Alcohol	0.0671	0.0674+	0.0863+	0.0526	0.021
	[0.041]	[0.041]	[0.052]	[0.039]	[0.192]
Marijuana	-0.125	-0.125	-0.167	-0.0958	0.091
	[0.12]	[0.12]	[0.14]	[0.12]	[0.405]
Cocaine	-0.00896	-8.4E-05	-0.101	0.0938	-0.106+
	[0.21]	[0.21]	[0.27]	[0.20]	[0.063]
Experience share			-0.811	0.741**	
			[1.15]	[0.087]	
Inverse Mills Ratio					0.701**
					[0.201]
Observations	1187	1187	1175	1175	674

Note: In column 3, actual experience between ages 21 and 34 (reported as a fraction of hours worked relative to full-time full-year employment) is included as an additional endogeneous variable (using the same instrumental variables), while in column 4 experience is as treated as exogenous. Regression 5 uses a Heckman two-step procedure to correct for sample selectivity. Instruments for labor force participation are current spouse or partner earnings and marital status.
Standard errors in brackets. + significant at 10%; * significant at 5%; ** significant at 1%

Table 6: Effects of Motherhood on Wage Profiles

	(1) OLS	(2) OLS	(3) OLS	(4) IV	(5) IV	(6) IV
Mother	-0.104**	-0.105**	-0.107**	-0.245**	-0.218**	-0.265**
	[0.015]	[0.015]	[0.015]	[0.077]	[0.073]	[0.0898]
Mother·YearsSinceFirstBirth	-0.0389**	-0.0341**		-0.0690**	-0.0753**	
	[0.0038]	[0.0044]		[0.007]	[0.0104]	
Mother·YearsSinceFirstBirth·1(20<A1B≤27)			-0.0335**			-0.0632**
			[0.0045]			[0.018]
Mother·YearsSinceFirstBirth·1(27<A1B≤33)			-0.0300**			-0.00725
			[0.0095]			[0.082]
Age	0.0625**	0.114**	0.118**	0.0907**	0.0564*	0.144
	[0.0031]	[0.017]	[0.019]	[0.0093]	[0.026]	[0.106]
Age2		-0.000974**	-0.00107**		0.000644	-0.0011
		[0.00033]	[0.00038]		[0.00051]	[0.0021]
Observations	15784	15784	15784	13837	13837	13837
Number of women	1488	1488	1488	1284	1284	1284

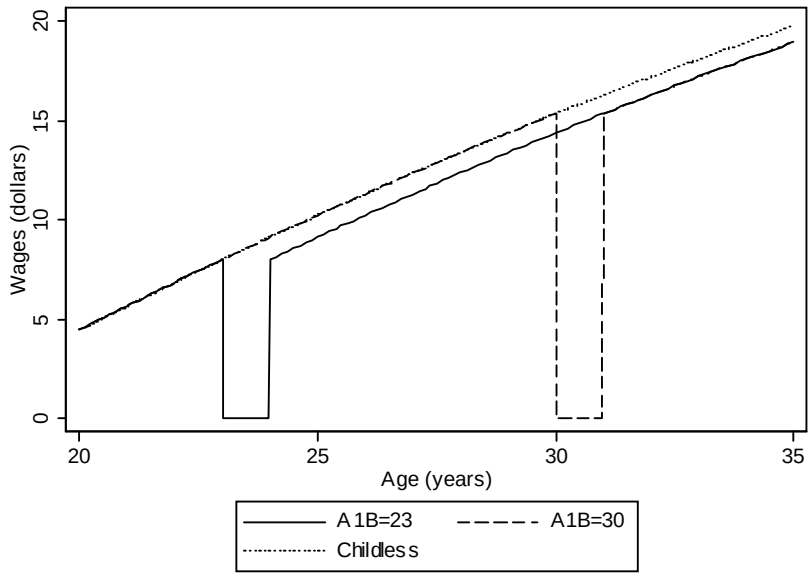
Note: The dependent variable is ln(wage rate). The longitudinal sample includes working women between the ages of 21 and 34 (who report wage rates), who have their first child between the ages of 21 and 33 and years 1983-2000. The unit of observation is an individual-year. Standard errors in brackets. OLS errors are clustered at the individual woman level. IV standard errors calculated by block bootstrapping at the individual level with 1,000 replications. + significant at 10%; * significant at 5%; ** significant at 1%

Table 7: Effects of Motherhood on Wage Growth

	(1) OLS	(2) OLS	(3) OLS	(4) IV	(5) OLS	(6) OLS	(7) OLS	(8) IV
Individual fixed effects	N	N	N	N	Y	Y	Y	Y
FirstBirth	-0.141**	-0.101**	-0.109**	-0.982	-0.120**	-0.0923*	-0.102*	-1.642
	[0.039]	[0.039]	[0.040]	[1.68]	[0.040]	[0.040]	[0.040]	[132.17]
(A1B-20)·FirstBirth	0.0131*	0.00950+	0.00960+	0.0208	0.0104*	0.00899+	0.00928+	0.141
	[0.0051]	[0.0052]	[0.0052]	[0.184]	[0.0052]	[0.0053]	[0.0052]	[18.67]
Mother			-0.0436**	-0.0773			-0.0538*	-0.245
			[0.015]	[0.069]			[0.022]	[12.72]
Birth Cohort	-0.0252**	-0.0238**	-0.0262**	-0.0264**				
	[0.0018]	[0.0018]	[0.0018]	[0.0046]				
HS diploma	0.0538**	0.022	0.0474**	0.0383				
	[0.015]	[0.015]	[0.015]	[0.037]				
College or higher	0.0043	-0.0215	0.00241	-0.00309				
	[0.014]	[0.014]	[0.014]	[0.020]				
AFQT percentile	0.0689**	0.0459*	0.0654**	0.0585*				
	[0.018]	[0.018]	[0.018]	[0.029]				
Black	0.0412**	0.0332**	0.0427**	0.0442**				
	[0.0095]	[0.0095]	[0.0095]	[0.012]				
Hispanic	0.0238*	0.0162+	0.0245*	0.0244*				
	[0.010]	[0.0097]	[0.010]	[0.011]				
Hours worked		0.00103**				0.00123**		
		[0.00011]				[0.00020]		
Observations	18216	18215	18216	18216	18216	18215	18216	18216
Number of women	1280	1280	1280	1280	1280	1280	1280	1280

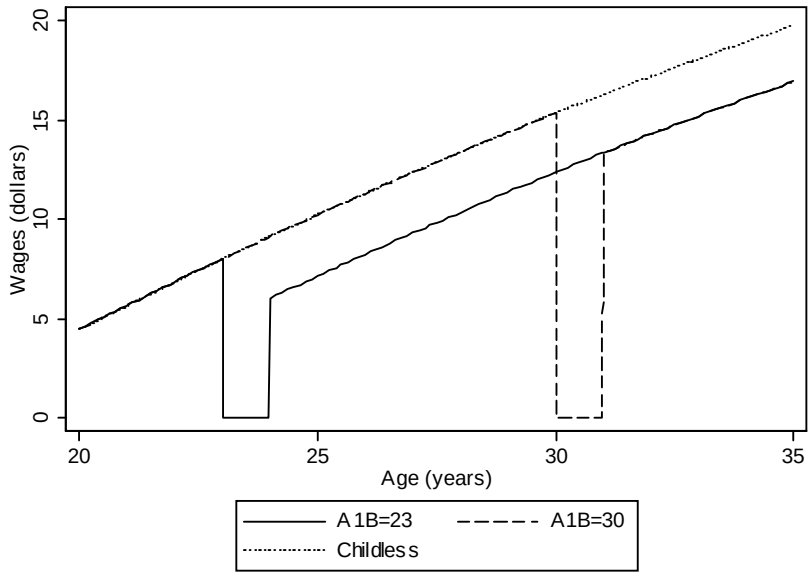
Note: The dependent variable is the change in ln(wage rate) over a 3 year period = ln(wage $\text{rate}_{i,t+3}$)-ln(wage $\text{rate}_{i,t}$). All regressions include a full set of age fixed effects. The longitudinal sample includes working women between the ages of 21 and 34 (who report wage rates in years *t* and *t+3*), who have their first child between the ages of 21 and 33 and years 1983-2000. FirstBirth is a dummy variable set to one if *t* is the year of first birth. Standard errors in brackets. OLS errors are clustered at the individual woman level. IV standard errors calculated by block bootstrapping at the individual level with 1,000 replications. + significant at 10%; * significant at 5%; ** significant at 1%

Figure 1: Returns to Experience



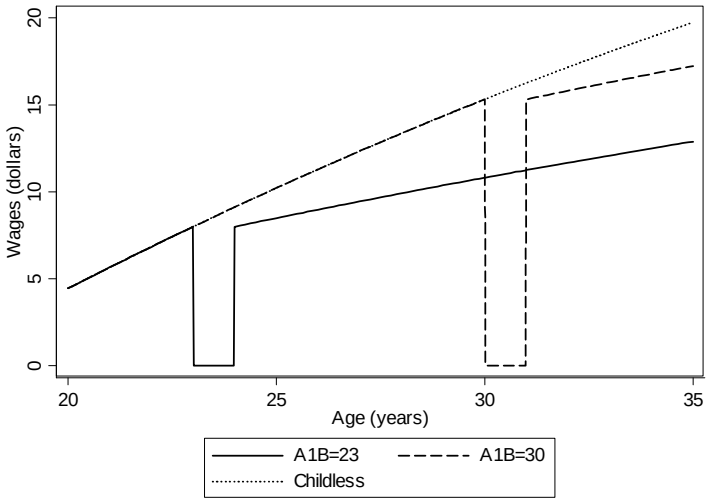
Note: Figure illustrates the effect of a one-year career interruption associated with motherhood at ages 23 and 30, relative to the uninterrupted profile, for a hypothetical woman in Case 3: returns to experience and no additional costs associated with motherhood.

Figure 2: Fixed Costs of Motherhood



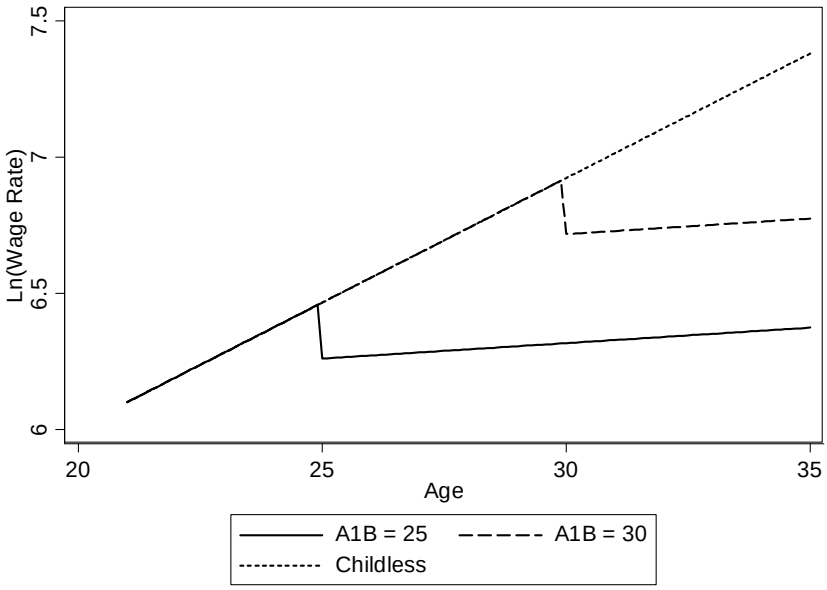
Note: Figure illustrates the effect of a one-year career interruption associated with motherhood at ages 23 and 30, relative to the uninterrupted profile, for a hypothetical woman in Case 4: returns to experience and a fixed cost of motherhood or depreciation of human capital.

Figure 3: The "Mommy Track"



Note: Figure illustrates the effect of a one-year career interruption associated with motherhood at ages 23 and 30, relative to the uninterrupted profile, for a hypothetical woman in Case 5: returns to experience, with a reduction in returns to experience for mothers.

Figure 4: Simulated Wage profiles by Age at First Birth



Note: The figure depicts three potential wage profiles, for a given woman, who either has her first child at age 25, at age 30 or after age 35. The profiles are simulated using the IV results reported in Table 6, column 5.

Appendix

Table 1: Correlations of A1B and the Fertility Shocks with Background and Ability

	Age at first birth (A1B)	First pregnancy ended in miscarriage	First pregnancy while using contraception	Years to conception of first child	Obs.
AFQT	2.892**	-0.0731	0.0655	0.0615	1030
	[0.429]	[0.0447]	[0.0609]	[0.321]	
Born in US	-0.106	-0.00316	0.0786	-0.116	1030
	[0.453]	[0.0462]	[0.0628]	[0.331]	
Lived in a city (age 14)	0.131	-0.0174	0.0408	-0.0411	1026
	[0.260]	[0.0265]	[0.0360]	[0.190]	
Magazine at home (age 14)	0.947**	-0.0525*	0.00294	-0.129	1023
	[0.227]	[0.0233]	[0.0317]	[0.168]	
Newspapers at home (age 14)	1.065**	-0.0427	0.0374	-0.329+	1026
	[0.265]	[0.0273]	[0.0370]	[0.195]	
Library card at home (age 14)	0.557*	-0.0245	0.0546	0.00975	1024
	[0.252]	[0.0258]	[0.0349]	[0.185]	
Live with both parents (age 14)	-0.288	0.00445	-0.0272	0.132	1030
	[0.209]	[0.0213]	[0.0290]	[0.153]	
Mother born in US	-0.0171	0.0233	0.0233	-0.3	1028
	[0.345]	[0.0352]	[0.0479]	[0.252]	
Mother highest grade completed	0.216**	-0.00548	0.00636	-0.0111	1001
	[0.0355]	[0.00369]	[0.00504]	[0.0266]	
Father born in US	-0.125	0.00248	0.00955	-0.253	1023
	[0.356]	[0.0363]	[0.0494]	[0.261]	
Father highest grade completed	0.191**	0.000367	0.00749+	0.0246	955
	[0.0280]	[0.00293]	[0.00400]	[0.0214]	
Number of siblings	-0.134**	0.00341	0.00491	-0.0218	1030
	[0.0447]	[0.00459]	[0.00624]	[0.0329]	
Number of children ideal	-0.13	0.00533	0.00463	0.101	1028
	[0.0840]	[0.00860]	[0.0117]	[0.0615]	

Note: Each coefficient reported in the table is from a separate regression of the dependant variable listed in the column heading on the control for that row and a constant term. Standard errors are in brackets. The AFQT measure is adjusted for age at the time of the test. The estimation sample is based on the cross-sectional sample of 1030 women used in Table 2. The actual sample sizes are smaller due to missing values for the controls. These are reported in the final column. + significant at 10%; * significant at 5%; ** significant at 1%.

Figure 1: Earnings Profiles by Age at First Birth

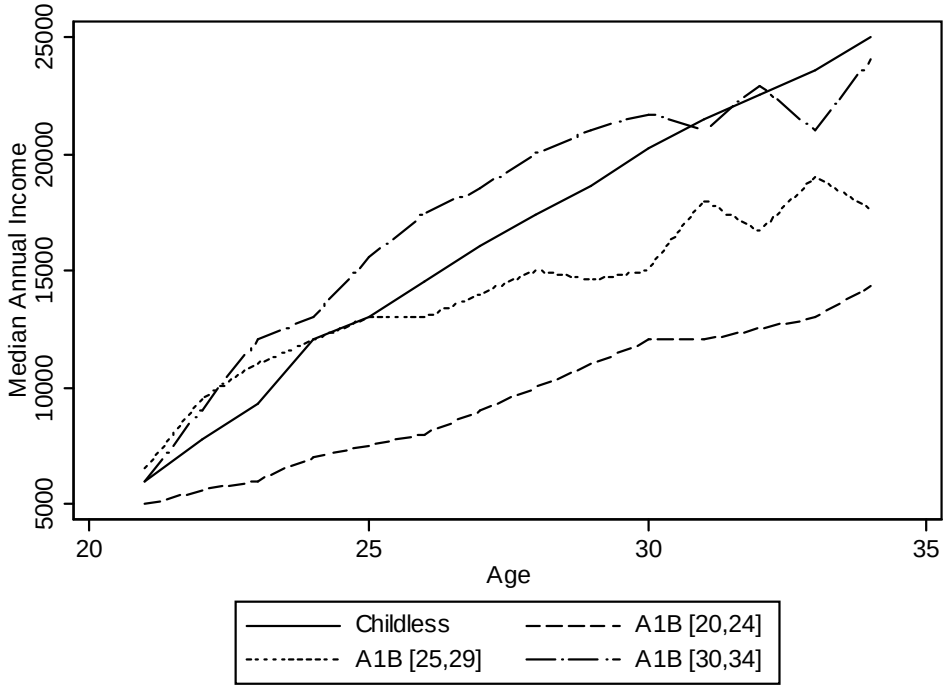
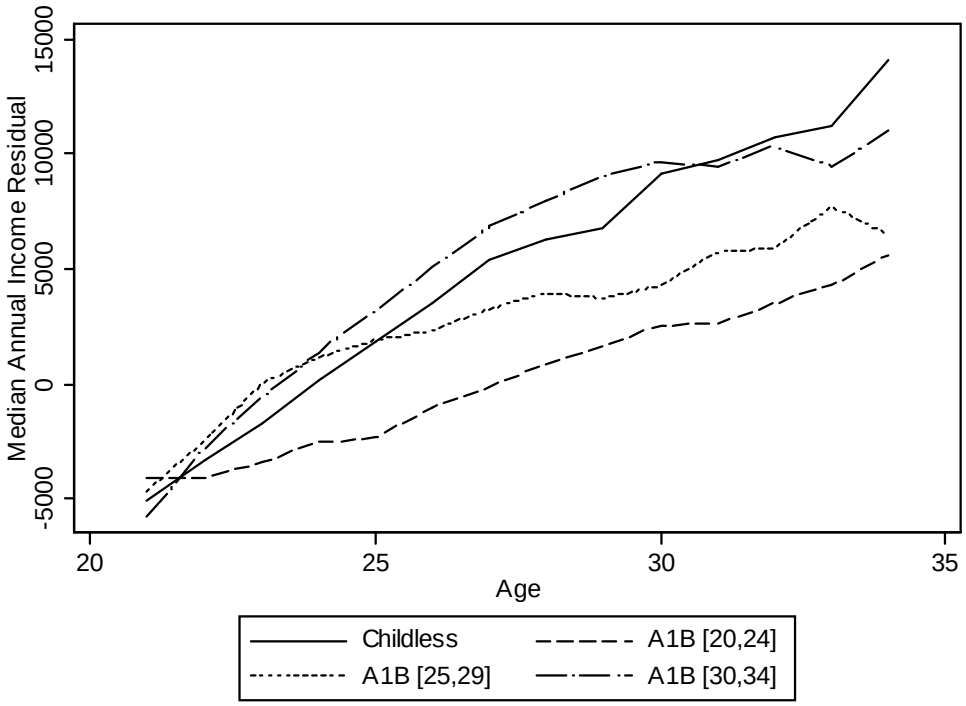


Figure 2: Earnings Residual Profiles by Age at First Birth



Note: Residuals are the difference between actual and predicted log-earnings from a linear regression with education, race, ability and birth year cohort as controls.

Figure 3: Hours Worked Profiles by Age at First Birth

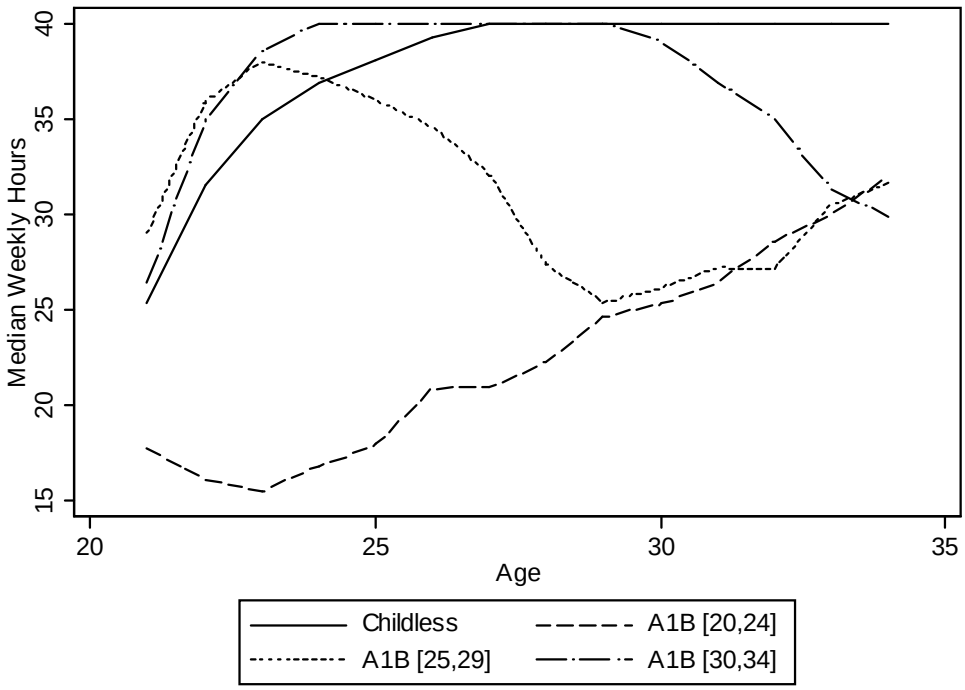


Figure 4: Wage Rate Profiles by Age at First Birth

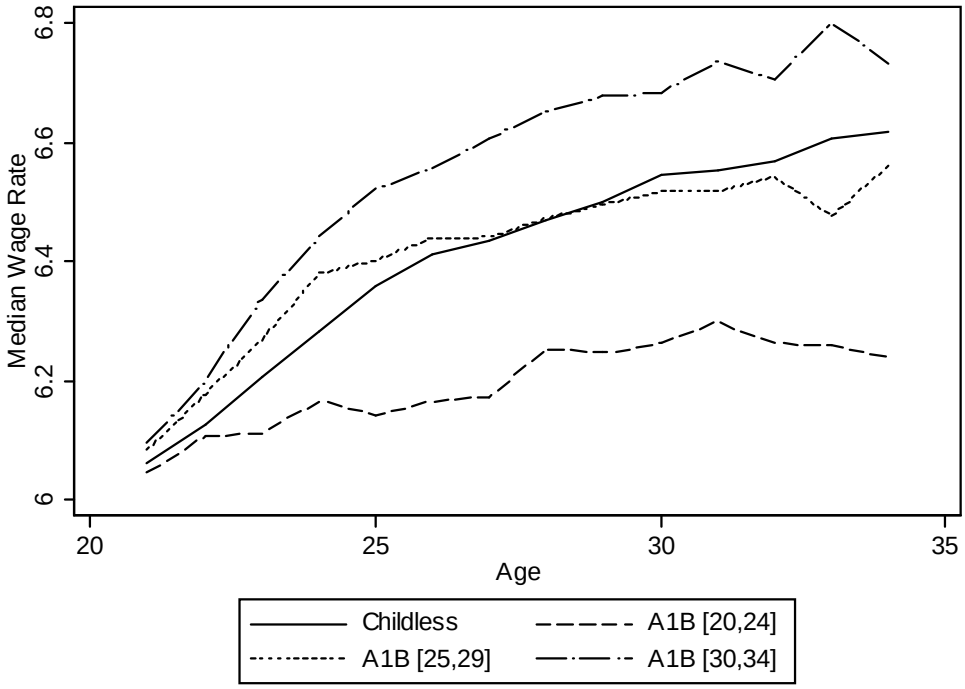
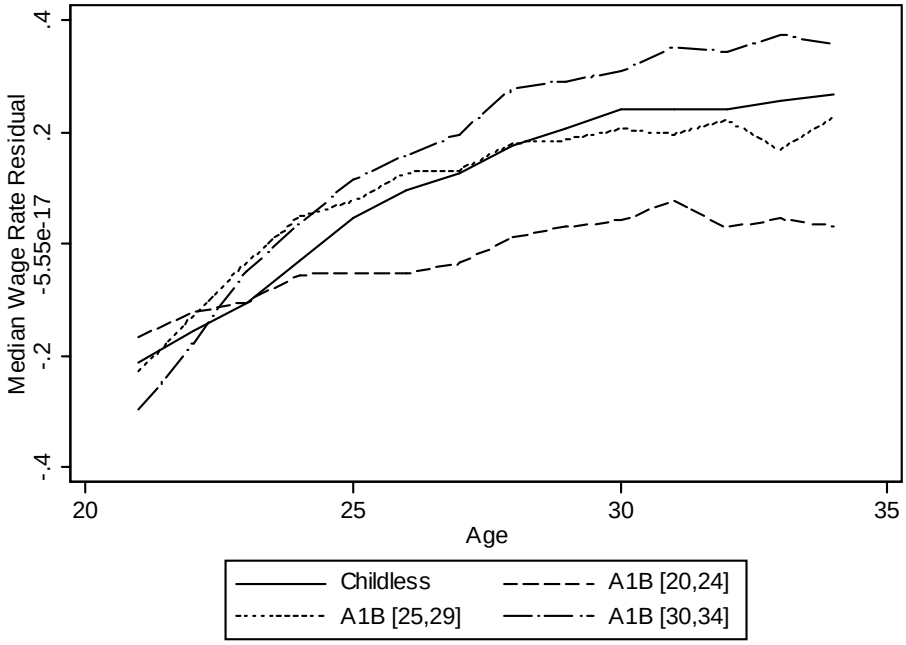


Figure 5: Wage Rate Residual Profiles by Age at First Birth



Note: Residuals are the difference between actual and predicted log-wage rate from a linear regression with education, race, ability and birth year cohort as controls.