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ABSTRACT

This paper explores how the wage and career consequences of motherhood differ by skill and timing. Past work has often found smaller or even negligible effects from childbearing for high-skill women, but we find the opposite. Wage trajectories diverge sharply for high scoring women after, but not before, they have children, while there is little change for low-skill women. It appears that the lifetime costs of childbearing, especially early childbearing, are particularly high for skilled women. These differential costs of childbearing may account for the far greater tendency of high-skill women to delay or avoid childbearing altogether.

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High-skill women bear children far later and less often than do low skill women. The former typically postpone childbearing until their late 20s and often well into their 30s. A significant minority seem to never have children at all. Low-skill women typically have children comparatively young, and nearly all have at least one child in their lifetime.[1] Table 1 illustrates just how dramatic this pattern is. Some 64% of high school graduates and nearly 80% of dropouts among women born in the early 1960s, had a child by the time they reached 25. Only 20% of college graduates were in the same position. Even by age 30, 80% of high school grads had given birth while only half of college graduates had. By age 40, a period that nearly marks the end of childbearing opportunities, most college grads have had children, but more than a quarter have none. Aggregate fertility for this college educated group is now well below the 2+ required for stable reproduction.

Interestingly this differential pattern by education, though it has long existed,[2] has become far more dramatic in recent decades. Among the cohort of women born 20 years earlier, the timing of childbearing for high school dropouts was not very different from what it is today, though total fertility was higher. But college graduates behaved quite differently. Nearly 50% of college grads had children by age 25 and only 18% had no children by age 40, even though college graduates in that cohort were a far smaller and more elite group in that earlier cohort.

The obvious question is: what accounts for the dramatic differences in childbearing patterns by skill level? This paper explores whether the career costs, in particular, the wage costs, of childbearing are higher for high-skill women.

There is abundant theoretical and empirical literature exploring the economics and sociology of fertility and family formation. This literature uniformly assumes that the labor market behavior of at least one parent will be affected by the presence of having a child and that expectations regarding labor market outcomes in turn influence family decisions. Yet the true consequences of childbearing on the labor market outcomes of women have received only modest attention, mostly involving studies of the static effect of children on pay of women. Limited attention has been paid to how children might influence longer wage trajectories, and importantly, whether or not the long term labor market consequences of childbearing might vary by the level of human capital or other characteristics of the parent.

[1] See for example Rindfuss et al. (1996) and Ellwood and Jencks (2004).

Using data from the National Longitudinal Survey of Youth 1979 (NLSY79), this paper examines the consequences of childbearing on the wage trajectories of women, and to some degree, men. It explores differentials by skill level of the parent and the timing of first births. Contrary to some recent literature on the costs of children by mother's education level and in support of a few recent studies of the consequences of teen childbearing, our findings strongly indicate that the wage costs of childbearing are vastly higher for high-skill women, that these wage penalties persist over time, and that having children later may reduce, but will not eliminate the significant lifetime costs of childbearing for higher skill women.

Past Literature on the Costs of Childbearing

A number of authors have systematically examined the average impact of children on the wages on women (and men). Waldfogel (1997) and Waldfogel (1998) find that one child reduces a woman's wages by roughly 6% and two by 15% in a fixed effects model, even after controlling for actual work experience. When she controls for part-time work status, the effects drop by a couple of percentage points. Similarly Budig and England (2001) find a 7% wage penalty per child without controlling for actual experience and a 5% penalty after controlling for actual experience in fixed effects models. Earlier work such as Korenman and Neumark (1992) and Jacobson and Levin (1995) found smaller penalties, but this work had fewer controls for unmeasured fixed effects. In general, these authors do not systematically examine whether the effects grow or decline as time passes since the birth, though Waldfogel (1997) finds that first difference models show larger effects as the time between the differences grows. She attributes this not to a growing penalty as parenting progresses but instead worries that ―using short first-differences might underestimate the penalty."[3] Loughren and Zissimopolus (2008) use a first difference model to look at the impact of marriage and childbearing on wage levels and growth and find that a first birth lowers wages by 2.1%, without affecting wage growth, but their specifications do not allow the impact of childbirth on wages to vary with the age of first birth[4].

Recently a few authors have begun investigating whether these impacts differ by level of skill or schooling. Amuedo-Dorantes and Kimmel (2005), Taniguchi (1999), and Todd (2001)

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[2] See Goldin (2004) for a historical discussion of birth and work patterns of college educated women over the twentieth century.

[3] Waldfogel (1997, p. 213)

[4] Kalist (2007) looks at the earnings, golf scores, and rankings of players competing in the Ladies Professional Golf Association. Including fixed effects and using an approach similar to the approach we follow, he graphs the earnings

all report that the wage penalty declines with schooling. Indeed Amuedo-Dorantes and Kimmel (2005) report that the college educated get a wage *boost* from childbearing using a fixed effects model with additional sample selection corrections. Budig and England (2001) argue that there is ~~"n~~o clear evidence that more skilled or committed women experience higher penalties."[5] Recently Anderson et al. (2002), Anderson et al. (2003) report finding a u-shaped penalty with respect to education: only middle education women experience a penalty—high and low education women show little effect. Loughren and Zissimopoulus (2008) cannot reject the hypothesis that the effects of childbirth on wages for women with less than 12 years of education, 12 years of education, between 12 and 15 years of education, and more than 15 years of education are the same (and equal to 0).

These findings are somewhat surprising for both theoretical and empirical reasons. First, one of the most plausible explanations for a motherhood penalty, even after controlling for experience, is that mothers who are facing increased demands at home reduce the intensity of work effort or choose somewhat less pressured positions. While there is no theoretical reason why such increased commitment conflicts should disproportionately affect high-skill women, one might expect that the most lucrative careers, those with the steepest wage profiles, would require a special degree of commitment. Second, there are a host of more popular books all produced by thoughtful and careful authors, including Crittenden (2002), Hewlett (2002), Maushart (2000), and Williams (2001) that all argue that professional women face sizable career costs and difficult tradeoffs in deciding to become a mother.

Still, the frequent finding of limited costs to motherhood for high-skill women could be accurate. Perhaps college educated women can afford to pay for child care and other supports that help them reduce their career costs. The need to be able to pay for such supports could also help explain the delays and avoidance of childbearing. And recall that these results are net of any losses due to reduced experience, so the claim in the literature is not that educated women do not lose earnings as a result of becoming mothers, but rather that reduced work experience accounts for those earnings reductions. Nevertheless, we find a very different result than this earlier literature, so the question of why these results differ needs to be addressed.

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and performance trends for these women before and after birth; after steadily improving in the 5 years prior to birth, the performance of these LPGA players worsened in each year after childbirth.

[5] Budig and England (2001, p. 219)

Finally there is a literature exploring whether the timing of childbearing (i.e. the age of first birth) affects the penalties of childbearing. The most prominent portion of this work focuses exclusively on whether teenage childbearing harms the prospects of women disproportionately more than childbearing at later years. Until recently, most observers would have said that adolescent childbearing was clearly more costly in terms of the mother's future earnings. But some recent literature, nicely summarized in Cherlin (2001), has suggested that disadvantaged women who postpone childbearing until they are in their twenties often fare no better than those who have children in their late teens. Geronimus and Korenman (1992) find that when sisters are compared, the differences between teen parents and older parents drop by as much as two-thirds. Hotz et al.(1997), Hotz et al. (2005) compare women who miscarried with women who did not and find virtually no impact of teen childbearing (although their estimates are not very precise). Hoffman (1998) also raises legitimate questions about this prior literature.

Literature on the effect of timing more broadly is far more limited. Amuedo-Dorantes and Kimmel (2005) report that college educated women who delay childbearing earn 43% *more* than childless women, and 21% more than earlier childbearers. Taniguchi (1999) reports that delay reduces the cost of childbearing. More recently, Miller (2008) and Herr (2007) have used instrumental variables approaches to conclude that delayed childbearing significantly reduced the costs of childbearing and that delay was particularly beneficial for more educated women.

Taken together this literature seems to suggest that there are costs to childbearing above and beyond the effects of reduced work hours, but that it is hard to determine whether such effects differ by education or whether timing matters much. Moreover, the literature is puzzling in its variety of findings since most studies use data from the NLSY79 or the NLSY68.

Theoretical Models—Impacts of Childbearing on Wage Profiles and Impact of Childrearing Costs on the Timing and Incidence of Childbearing

The well-established finding that mothers on average have lower wages than non-mothers even after controlling for fixed effects might be explained in several ways, ranging from discrimination to unmeasured differences not captured by fixed effects. Becker (1985), Becker (1991) offers a logical theoretical starting point for the issue. He separates time and effort expended in market work activities and home activities. He assumes utility depends on ―home goods" produced inside of the home using time, effort and market goods. For purposes of this exposition, it is just as easy to enter these inputs directly in an indirect utility function:

Thus

$U = U(C, t_H, e_H, Y)$

Where

C = number of children

t_H = time spent at home

e_H = effort expended at home

Y = income (used to purchase goods used in home production or consumed directly)

Any time or energy not spent at home is assumed to be expended on market work. Income comes from earnings and outside income. Wages are a function of effort and human capital. Thus utility is maximized subject to the following constraint:

$Y = W(e_M) \cdot t_M + V$
where
$W()$ = wage/salary
V = outside income
Total time $T = t_M + t_H$, Total effort $E = e_M + e_H$ C
It is easily shown that for any given number of children, maximizing utility implies that

(1) $\frac{\partial U}{\partial t_H} = \frac{\partial U}{\partial Y} \cdot W$

(2) $\frac{\partial U}{\partial e_H} = \frac{\partial U}{\partial Y} \cdot \frac{\partial W}{\partial e_M} \cdot [t_M]$

onditions (1) and (2) yield the very sensible conditions that the marginal utility of time and effort in market work and home production will be equalized.

Potential parents consider maximized utility with and without children in deciding whether or not to have them. In terms of time and effort devoted to market work, increasing the number of children from 0 to 1 would seem to imply that the marginal utility from time and energy spent at home would rise. Absent a change in the marginal utility of income, work and effort outside the home would surely decline. But the marginal utility of income would likely also rise due to the need for increased food, housing diapers, child care, and the like (a point not emphasized by Becker). As a result, whether time or energy outside of home declines depends on the relative changes in marginal utility of income and the changes in the marginal productivity of time and energy spent in the home.

What this might imply for women with different skill levels is uncertain. For women with little or no outside income (such as single parents who do not want to go onto welfare), the increased need for food and housing might dominate the pressures to spend time and energy on nurturing, so effort and time spent on market work might actually increase. Higher skill women, and those with other sources of income might see less of a change in the utility of income and might be more inclined to cut back on time and energy devoted to market work. On the other hand, higher skill women might also be in jobs where the impact of effort on wages is greater, so they might seek to reduce effort somewhat less.

If parents, particularly mothers, do reduce market effort, the Becker model implies that their wages per hour worked will fall. Thus the apparent effect of children on pay could be traced to a form of omitted variable bias caused by the fact that effort cannot be measured in the wage equation.

This sort of model focuses our attention on the question of whether parents adjust their market activities in unmeasured ways that affect their wages. But is it plausible that changes in effort associated with childbearing would immediately and instantaneously lead to declines in wages? Becker argues rightly that in some cases women will be inclined to change jobs to ones that are less effort intensive and thus see immediate wage declines. But many women also return to their same employer following childbirth. It is far less reasonable to assume that employers would cut the pay of new parents—indeed it may be illegal to do so.

An alternative story, and one quite consistent with the popular literature on this topic, is that wage declines do not occur instantaneously after childbirth, but rather that wage growth is heavily dependent on perceived effort expended. Promotions may go to people who are devoted to the job, who rearrange schedules to deal with immediate crises at work, who seem focused almost entirely on work. Parents, and probably disproportionately mothers, could face conflicting commitments and thus see far slower wage growth. Thus a more plausible account of the effect of childbearing on wages may be that wage *growth*, not current pay, is dependent on effort. And if actual effort is hard to monitor, employers may rightly or wrongly perceive mothers as less committed to their jobs and move them off –the fast track.”

Still the puzzle in fertility patterns that most interests us is why more educated women postpone childbearing more than less educated ones. Becker's static model offers little help with that question. Placing this Becker model in a life cycle context creates a remarkably complex dynamic programming model, especially if one allows related but separate choices over the timing of marriage and fertility, along with human capital decisions and choices over effort. To even begin to make the problem tractable, rather stringent assumptions are necessary. Mullin and Wang (2002) offer among the most complete and complex models (although they ignore marriage). Within their model, children create an automatic temporary reduction in productivity in both the labor market and in investments for human capital. Age of first birth rises with the ―productivity loss from children" and the ability of the mother. The model has not been implemented empirically, but it points to the need to investigate the extent to which labor market losses associated with children might vary with skill or ability. Hotz and Miller (1988) offer the most widely cited version of an empirical model with life-cycle fertility, but they do not allow for career costs of childbearing (even due to reduced accumulation of experience) or distinctions by level of skill.

Still, even without a dynamic programming model, we can gain insights into the potential costs of childbearing on careers. Only a modest extension of the basic model described above leads to the straightforward notion that both wages and wage growth might depend on labor market experience in addition to effort. Then the effect of children on career is likely to differ for high- and low-skill parents because they may be in jobs that differ in their sensitivity to effort and because they might make different choices regarding their work effort upon childbearing. Consider a 3 period model for women. Each period is long enough to bear and raise a child. Women may choose to have their child in the first or the second period but not both, and they are not able to bear children in the third period when they are no longer fertile. To keep things simple, we focus on only one child, and we assume women work full-time in periods when they are not rearing a child. In the periods when women do rear children, they work reduced hours, and experience some permanent wage decline due to a persistent decline in returns to experience. Note that one could model the wage penalty as a one-time only cost, but if a woman works for an extended period at a lower paying job while childrearing, it seems unlikely that she could simply return to the occupation and wages she would have had if she had never had children, once her children are grown.

Assume

W_1 = full-time pay rate for woman in year 1

g = growth rate for a full-time woman who does not rear a child in period (returns to experience)

α = the share of full-time hours worked by a woman who rears a child in period

p = wage penalty for a woman who rears a child in that period

k = full-time equivalent growth rate of pay for a woman who rears a child in period

Let us compare the earnings of women who have a child in period 1, period 2 or not at all.

Earnings for Women by Period and Timing of Childbirth

	No Child at All	Rear Child In Period 1	Rear Child in Period 2
Period 1	W_1	$W_1(1-p)\alpha$	W_1
Period 2	$W_1(1+g)$	$W_1(1-p)(1+\alpha k)$	$W_1(1+g)(1-p)\alpha$
Period 3	$W_1(1+g)(1+g)$	$W_1(1-p)(1+\alpha k)(1+g)$	$W_1(1+g)(1-p)(1+\alpha k)$

The woman who has no children earns full-time pay of W_1 in period 1. If she remains childless in period 2, her pay rises by g and she continues to work full time, so she earns $W_1(1+g)$ and this pattern of growth repeats into period 3. By contrast, a woman who has a child in the first period faces a wage reduction of p and works only α share of full time work and thus earns $W_1(1-p)\alpha$. In the next period this woman returns to working full time, but her wage grows less than it would have otherwise, by αk, because she worked part-time and raised a child in the previous period and because the growth rate in pay for such women is k. For the final period, her pay once again rises by g as she reared her child in period 1.

The Earnings Costs of Early Childbearing

Consider the difference in lifetime wage earnings between the woman who rears her child in period 2 versus period 1. Note that third period earnings are the same for both women. In effect, the early childbearers catch up to the later ones. But there are differences in the first two periods. Discounting the second year and subtracting yields the gain from waiting until the second period:

$$\frac{rW_1[1-\alpha+\alpha p]}{1+r} + \frac{pW}{1+r} + \frac{W(1-p)\alpha(g-k)}{1+r}$$

All three elements have a straightforward interpretation. The first term is simply the gain achieved by postponing the pay lost due to child rearing one period into the future. The childbearing cost in the period of childbearing is the same proportion of income regardless of when it occurs. Postponing it by a year lowers its present value. The second term is the result of the fact that we assume the reduction of pay which occurs in the first year persists permanently. Thus having children one period earlier creates one extra period of reduced pay. If this loss was offset in future years, the term might go away. The final term captures the effect of differential growth for women with and without children.

Several features are worthy of note. First the gains to postponing are increased as the base wage rises, as the immediate wage penalty from childbearing rises, and the *difference* between the child and no child growth rate increases. Note that the absolute growth rate in pay itself is irrelevant. This somewhat counterintuitive result can be understood as follows. A woman with a high growth rate who postpones childbearing will have a higher second period wage, but that gain will be offset by the fact that she will then suffer a higher second period loss when she has children. If her wages grow more slowly as the result of having children, postponing that loss in growth results in a higher present value of earnings. Note also that the impact of a higher α (a higher level of work when rearing children) is ambiguous. It lowers the earnings cost of childbearing since less time is taken off from work, and thus lowers the discounting benefits of postponing childbearing. But it also heightens any impact of a reduced wage growth rate. As women spend more and more time in work, the impact of any differential growth rate will become a larger and larger share of the costs of early childbearing.

To determine whether or not early childbearing makes sense, women will need to compare the added lifetime earnings costs to the other benefits (and costs) of earlier childbearing. Early childrearers have more years to spend with their children and will be younger when they become grandparents. But they will also have less leisure time early in life, less lifetime earnings, and so forth.

The Earnings Costs of Childbearing Versus Having No Children

Simple inspection of the first and last columns of the table above reveal that the cost of childbearing versus avoidance is likely to be much larger than the cost of early versus late childbearing. Even those women who have children late still lose the earnings while out of the labor force, still face a long term wage penalty, and still experience a year of slower growth in this model. In addition, the growth rate of earnings, g, does enter directly. Thus, in this model, the level of wages and the returns to experience influences the costs of having children. By contrast, the timing of childbearing is influenced by the level of wages and the relative returns to experience between mothers and non-mothers.

Empirical Strategies

In determining what the impact of childbearing is on pay, our biggest challenge is endogeneity. An important part of the motivation for this work is the finding that more skilled women delay childbearing more than less skilled women and theory suggests this may be related to the costs of childbearing. By definition then, childbearing and its timing are likely to be related to labor market performance.

Unobserved heterogeneity is a conceptually distinct challenge. The characteristics of men and women that might make them less appealing to potential partners and reduce their likelihood of becoming parents, might also make them less successful in the labor market. These hampering characteristics might influence both their level of wages at any given time and also their rate of wage growth.

There are two strategies that might offer a convincing way of discerning the effect of childbearing on work and careers. The first involves finding appropriate instruments for childbearing that are not directly correlated with work and wages. The second is to exploit the patterns and trends in the longitudinal data itself, looking for clear changes in trajectories that follow, but do not precede the birth or conception of a child.

<u>The difficult search for instruments</u>—The ideal instrument would exogenously influence childbearing without any direct impact on earnings. But good instruments are hard to come by. Having a child is probably the most enduring and life altering event for most people, and thus potential parents will likely work very hard to control it. Moreover, the very people for whom the event would have the greatest impact are precisely the group who would have the strongest incentive to control fertility effectively.

Perhaps the most clever and compelling instrument is the incidence of miscarriage, prominently used by Hotz, McElroy and Sanders (1997, 2005) in their work on the impact of teen childbearing on wages, education, and other outcomes. If miscarriages are largely exogenous, women who have a miscarriage should, on average, have children later than those who do not.

Still, there are several important limitations to this instrument. First, reported miscarriages are uncommon. In the NLSY79, just over 10% of women report having a miscarriage with their first pregnancy. And a miscarriage usually results only in modest delay, not avoidance altogether. At least 80% of women reporting a first miscarriage are later observed to have a child, and the average delay between miscarriage and childbirth is 2 years. Thus, this instrument could at best be used to explore the impact of a two year delay on wages, and its power will be weak given its low incidence. Second and more worrisome is the fact that health and other behavioral characteristics of mothers who miscarry may be worse than those who do not. These health limitations may in turn lead such women to have lower pay. Alternatively, mothers who miscarry may live in communities which have unobservable differences which influence both the likelihood of miscarriages and wages; Fletcher and Wolfe (2008) find that the probability of having a miscarriage for teens is correlated both with community level factors and behaviors including smoking, drinking and using drugs during pregnancy. Furthermore, they suggest that early miscarriages are unlikely to be a valid comparison group for live births because many of those who had early miscarriages would have gone on to have abortions given their higher SES status and other observable characteristics. Other researchers using the NLSY79 have also found that the rate of miscarriage varies considerably with predetermined characteristics, including marital status and age -- suggesting that there are many unobservable characteristics which are likely to be correlated with having a miscarriage and also wages (Herr 2007)[6]. Finally, there is evidence that reported miscarriages are not exogenous. In our analyses of the NLSY79, we found that the reported rate of miscarriage was 17% among women who had reported in the previous survey that they did not expect to become pregnant within the next couple of years and who actively tried to prevent pregnancy through contraception. The rate was just 11% among women who reported that they expected to become pregnant and were not using

[6] Herr (2007) follows a similar approach to that of Miller (2008); she instruments for age of first birth using fertility shocks including miscarriage and contraceptive failure – the limitations of which are described in the text.

contraception at the time they became pregnant. Perhaps unwanted pregnancies are more stressful, resulting in more miscarriages, or perhaps women knowingly misreport such events (for example, by calling an abortion a miscarriage, or being more willing to report a miscarriage of an unwanted child). Regardless, there are good reasons to believe that this instrument is correlated with the perceived costs of childbearing.

An alternative methodology is to use ―undesired" or ―unexpected" pregnancies. For example, Miller (2008) proposes using as an instrument whether the woman reports that she was using contraception at the time of conception. Unfortunately this instrument has more serious problems. Only 14% of women who became pregnant reported that they were using contraception at the time of conception (presumably proving that contraception mostly works). Moreover, contraceptive use (and thus the odds of failure) will vary with the woman's information, her own sense of control, the age of the partners, the availability and willingness to have an abortion, and most importantly, the perceived cost of childbearing. If contraceptive use varies in ways correlated with market outcomes, or the competence and knowledge of the woman, pregnancies that are the result of contraceptive failure will be correlated with these factors as well.

Another problem with this instrument is the obvious question as to whether such self-reported, retrospective answers can be trusted. Women might overreport contraceptive use because they are embarrassed by their pregnancies, or underreport it so as not to admit the child was unwanted. The NLSY also includes prospective questions about when people expect to become pregnant. These have the advantage that they are asked prior to pregnancy. Revealingly, of those who reported that they became pregnant while using contraception, nearly half had previously reported they had expected to be pregnant in the subsequent two years. One might count as truly ―undesired" pregnancies only those where the woman was using contraception and did not expect to become pregnant in the next two years at the time of pregnancy. But only about 8% of first pregnancies fit these criteria, so the power of such a test would be low.

Finally, many unwanted pregnancies end in abortion, not childbearing, further weakening the instrument. Of the 8% of reported pregnancies fitting the criteria above, 38% were reported to have been aborted and another 17% ended in miscarriage,[7] leaving just 3-4% of all first pregnancies as unwanted, unexpected, and resulting in a birth.[89]

In this work, we tried several other potential instruments, including mothers' age of first birth, type of contraception, expected age of pregnancy when age 18, and the like[10]. Overall, the estimates they produced were generally in the ―right" direction, and sometimes significant. But these instruments were also unstable and thus unsatisfactory, especially when we sought to decompose effects by subgroups.

<u>Tracking trajectories</u>—In the absence of credible instruments, we instead exploit the inherent advantages of longitudinal data. The NLSY79 tracked women who were aged 14 to 22 in 1979 and has followed them more or less continuously ever since. We use the available data from 1979 to 2006, so the ending ages are 41 to 49. The vast majority of respondents were childless at the start of the survey, and most were parents by the end. As a result, the obvious starting point for this work, examining the changes in wage trajectories as respondents become parents, offers a surprisingly powerful methodology for examining the impact of childbearing on pay.

While previous studies have allowed for fixed effects, they have not been able to identify different wage trajectories. Fixed effects are essentially before-after strategies that control for any unchanging and unmeasured differences across individuals. But women will also differ in

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[7] By contrast only 26% of pregnancies ended in abortion or miscarriage among women who claimed they were using contraception, but who had reported they expected to have a child within the next couple of years, very similar to the 21% termination pattern among those who reported they had not been using contraception.

[8] In addition, nearly half of all abortions reported in establishment data are not reported in individual survey data such as the NLSY.

[9] Miller (2003) uses yet another instrument for age of first birth model: ―the lag in years from the first attempt to conceive to first birth." But the NLSY has no direct question about when people sought to become pregnant. Instead Miller looks at the time between the first unprotected intercourse and the first birth. Aside from the fact that few people are actually attempting to get pregnant with their first unprotected intercourse, there are severe econometric issues associated with subtracting age of first unprotected intercourse from age of first birth to instrument for age of first birth. Since nearly everyone has at least one unprotected intercourse early, this seems hardly an instrument at all. Not surprisingly the t-statistic of this variable in the first stage regression is nearly 20.

[10] Recent work by Bailey (2006) uses state variation in access to the birth control pill for minors between 1960 and 1976 as a source of variation in age of first birth and finds that increasing access to the pill increased the number of women in the paid labor force and raised the annual number of hours worked up until several years after the age of emancipation. She hypothesizes that the mechanism through which access to the bill affected labor force participation was by changing women's lifetime career paths – evidenced by increased educational enrollment at age 21-22 and 23-24. Unfortunately, this is not a potential instrument for us because only a small proportion of our respondents would have been affected by these law changes (as they were 14-22 in 1979). Other instruments used in the recent literature include state laws which mandated insurance coverage for infertility payments (Buckles 2007). Estimates of the impact of such policies on the wages and labor force participation of women are difficult to

their wage trajectories. Moreover, unanticipated success or failure in the labor market may also influence childbearing may also so. Thus fixed effects will not fully solve the endogeneity problem. Fortunately with longitudinal data, trajectories can themselves be observed and one can look for anomalous breaks in patterns that differ with the timing of childbirth. Exploiting the basic insight of Sims (1972), one can look for evidence of causality by noting whether wage changes preceded or followed the childbearing.

Wage Trajectories by Timing of Childbearing and Skill Level

Figures 1 through 4 show the age-wage trajectories of women by the timing of their first birth. Figure 1 shows the pattern for all women. Inspection suggests that women who bear children later seem to be on different trajectories. But close inspection appears to reveal a discernable change in the income trends for each group when they reach the age when they give birth. Intriguingly, women who never have a child are often on a lower trajectory than women who later do. This becomes far clearer when we break women into skill categories.

In Figures 2 through 4, all women are broken into thirds based on their performance on the Armed Forces Qualification Test (AFQT) that was administered to NLSY respondents. We split the sample by AFQT rather than education to reduce endogeneity that would be present since early childbearing clearly can and does influence education.[11] There are several striking features of the figures.

First and as one might expect, the trajectories of the lower skill women are considerably flatter than those of higher skill women. Far more importantly, the trajectories of low scoring women do not change very noticeably after they have children. Women who remain childless or have children late may do somewhat better, but the differences appear quite modest.

In contrast, for women in the top third, wage trajectories seem to shift rather dramatically after they have children. Wages of women rise sharply and largely in unison in the period prior to their having children, but at almost precisely the moment they bear children, their wage profiles flatten out. For example the wages of women who have a child between the ages of 27 and 29 seem to rise almost exactly with women who have children later. Then exactly at age 28, their wages flatten sharply. This certainly provides prima face evidence that childbearing has a

interpret, however, because such policies may affect the wages and employment of women directly by increasing the costs of health insurance for them.

large impact on wages, and that the impact of childbearing is greatest for women that have the strongest skills. Note also that higher skill women who remain childless are not at the upper edge of the earnings envelope, even in the period prior to or after the time when their peers have children. On average, these women clearly are not similar to those who do indeed bear children.

These graphs provide strong visual evidence that childbearing may indeed have a powerful effect on wage trajectories and that there are sharp differences in these impacts by skill group. The graph for high-skill women also helps explain a puzzling finding in the literature, namely that high-skill women apparently are not harmed much by having children, especially if they have children late. The graph suggests the finding is an artifact of uncaptured differences between high-skill women who have children late and those who do not have children at all. Figure 4 makes clear that higher skill women who bear children comparatively late do indeed have higher pay than women who remain childless—but that advantage begins well before the women have children and it diminishes sharply after they become mothers.

We now turn to our statistical work. In this work, we have chosen to focus exclusively on women who eventually have children since our work clearly illustrates that childless women often have different wage profiles than those who become mothers even prior to childbearing. In addition, since we would like some wage observations before childbearing, and because we want to limit the endogeneity of schooling, test scores and childbearing, we look only at women who had children after age 21, after the period when the AFQT was administered and after most schooling is completed. This limitation seems reasonable for middle and upper scoring women who rarely have children prior to age 21, but it could be problematic for women in the bottom group who often have children as teens. We explore this issue below, but generally find that limiting this sample to women who had children after 21 had no significant effects on the results for any of the groups.

In all of our estimations, we controlled for fixed effects. We focus primarily on the impact of first children on wages, though we include variables for additional children. And we are interested in whether any effects of childbearing grow or diminish as time since the birth passes. Thus, we include dummy variables indicating the women had her first child within the past 0 to 4 years, 5 to 9 years ago, or greater than 10 years previously.

[11] The AFQT also has an element of endogeneity and measurement error since it can be somewhat influenced by education and because it is administered when the women entered the survey. Participant ages varied at the time of

Table 2 reveals that even after controlling for fixed effects, the sharp differences in the apparent effects of childbearing remain. Initially, we control only for “potential experience” (age-schooling-5), to capture the full apparent change in wages as women age. This model is comparable to the formulation of the graphs, except that we allow for fixed effects. For all three score groups, wage growth of women slows considerably after having children, and the pay penalty grows as time since the birth passes. Moreover, the effects on more skilled women are significantly greater. This group has a much sharper upward wage trajectory independent of children, but seems to suffer the largest relative declines in wages as a result of childbearing. While low scoring women have wages that are 12% lower than their counterparts after 10 years, high scoring women have pay that is 35% lower.

Some decline in pay among mothers, or more precisely the slowing of pay growth, would be expected even if motherhood had no impact on wages *per se*. Mothers usually spend less time in the labor market than non-mothers and thus accumulate less work experience.

The models in Table 3 control for actual labor market experience and thus remove the effect on pay of any reductions in hours worked. Including actual experience sharply reduces the coefficient on motherhood for all skill levels. For low-AFQT women, the remaining impact of childbearing moves to “just” 6 to 7% and the coefficient is insignificant after 5 years. Importantly, the negative impact for this group, net of lost work experience, does not appear to grow with time. Thus, low scoring women appear to face a one-time, permanent fall in pay of perhaps 6%, above and beyond any reduction in pay traceable to lost work experience.

In contrast, high scoring women show a net 8% reduction in pay during the first 5 years after giving birth, and that penalty grows to 24% in the decade after birth, even after controlling for actual experience. One might have expected some catch up in later years, but we see the opposite here. Moreover, women in our sample are 41 to 49 in the final sample year, so it seems reasonable to expect that pay recovery would be visible by that time if there were any.

These results control for fixed effects. The possibility remains, however, that women choose to have children at a time when they see their wages flattening. Table 4 tests for this possibility by examining whether wages fell in the period prior to the birth of the child. This is actually a simplified type of causality test—exploring which came first: lower pay or childbearing. Strikingly, wages in the years just prior to birth were insignificantly different from

entry from 14 to 21. The scores used in our analysis were normalized by age.

the baseline for all three groups. Indeed for two of the three groups the point estimates are positive. We see no evidence here of a wage dip before childbearing, only after it.

Finally, even within AFQT groupings, it is plausible that women with steeper trajectories have children later. Since we are conditioning on women having children at some stage, it is not clear how such a correlation would influence the results. We cannot realistically allow for each individual to have their own trajectory. But we did one test that allowed for an interaction between age of first birth, and both experience and experience squared. These interaction terms were individually and jointly insignificant in the lower two AFQT groups and had no influence on those results. There was some evidence that among high AFQT women, those who had children at later ages had steeper trajectories. However, the estimated impact of childbearing was largely unchanged and statistically indistinguishable from the previous estimates, though the estimated negative impact after 10 years leveled off at 22% rather than rising to 24%.

The estimated 20% to 24% reduction in pay for high-scoring mothers after 10 years is dramatically higher than the results found elsewhere in the literature and discussed above. For example Waldfogel (1997) and Budig and England (2001) report reductions net of actual experience in fixed effects models of closer to 7 to 8% and the latter find little variation by education or skill level. As noted above, some authors even find positive effects of childbearing for more skilled women.

The differences might be traced primarily to three differences in methodology that are easily tested. First, previous authors have not sought to examine whether the impact of children grew as time since the birth increased. While one might think that this omission would simply give an average impact of childbearing, a serious truncation bias can arise in most existing longitudinal data series. If one were using the NLSY through say, 1995, as earlier studies have done, the final ages of sampled women would be 30-37. Since the median age of first birth among women scoring in the top third is 27, many of the mothers would be captured only in the first year or two after birth, so the –average” impact found in earlier models would be only the initial impact for higher skill women. If the impact of childbearing on pay grows as time since the birth increases, then the eventual impact of childbearing will be considerably greater than average estimated for this late childbearing group.[12] This issue would be far less serious in the low scoring women where median age of first birth is closer to 20.

Second, we noted that including women who never give birth appears to bias the results in some cases, since our figures show that such women often are on lower wage trajectories even in the period before comparably scoring women become mothers. Again in Figures 2 to 4, this seems to be a serious problem, especially for high-scoring women.

Finally, we chose to limit the sample to women who have their first child after age 21 so that issues of educational endogeneity can be reduced, and to ensure that we can usually get some wage observations in the period prior to childbearing. As noted previously, for low-scoring women this method could introduce bias since over half have their first child prior to age 21.

Tables 5 and 6 illustrate the impact of these modeling features on the results for high- and low-skill women. In these tables the first column is the traditional estimate using the methodology common in the literature, and the last column is our preferred form. For low-skill women, the impact of the various modeling differences is quite small. Allowing for changing impacts as the child ages and limiting the sample to women who eventually become mothers does little to change the estimates. Limiting the sample to women who have children after age 21 does push up the estimates slightly, but the differences are not significantly different.

But for high-skill women, allowing effects to grow as the child ages pushes the apparent 5% effect found in previous literature up to nearly 12% by the 10th year. Limiting the sample to women who eventually have children pushes the impact up to 18%. Again, it is evident that women who never give birth have lower trajectories than women who eventually do. Finally, using only the sample of women who have children after age 21 increases the effect to 24%. Our conclusion is that modeling limitations in previous work probably hid the most of the sizable impacts of childbearing for high-skill women, while having little impact for lower skill women.

The Influence of Timing on the Career Costs of Childbearing

The timing of childbearing might influence the career consequences of children. The earnings of women seem to plateau in the period after they have children. This would seem particularly costly early in a career when age-earnings profiles are steep. Later, as profiles

[12] Note this issue remains in our use of a simple measure of number of additional children. Since many of these children are born later in a mother's life, our estimates of the impact of additional children probably suffers from

flatten out as a matter of course, a flatter profile might be less costly. And since the profiles of high-scoring women are steeper on average, it would seem plausible that the gains to waiting would be greater for this group.

Table 7 explores the issue of timing. The model includes an interaction term from which one can infer the impact of each additional year of waiting beyond age 21.[13] Once again, we find sizable differences by skill level. The apparent impact of waiting is mildly positive for women in the bottom third of AFQT scores, but the effect is insignificant. For higher skill women, the apparent benefits of delay are quite large and highly significant. The estimates imply that waiting 10 years reduces the cost of childbearing by more than one third. Taken at face value, Table 7 implies that high-skill women face much higher costs of childbearing (as a fraction of pay) and reap much greater financial benefits from postponing childbearing.

But the data on the timing issue are less clear cut than they appear in Table 7. In Table 8 we examine the impact of delay for high-skill women using two slightly different specifications. In the first column, we impose a common experience profile regardless of the timing of first birth and allow the effects of childbearing to differ depending on whether the child was born before or after the woman turned 28 (the median age of first childbirth for this group is 27). The first column again shows a smaller cost of childbearing for women who bear them later. In the next two columns, we estimate the model separately for women who had children prior to age 28 and those who had children at age 28 or later. When we split the sample in this way, there is no clear evidence that women who have children later face lower costs, except in the first 4 years since birth and, in fact, the results instead suggest that they may face higher costs. The reason the results differ is that the estimated wage profile for this late childbearing group is estimated to rise more slowly with experience initially, but to flatten out less quickly (the squared term is lower) than does the profile of the earlier childbearers.

In selecting the specification to pick, it should be remembered that the post-age-28 childbearing equation suffers from a relatively short post-childbirth period because the sample's age ranges from 41 to 49 in 2006, the last year we have data. In trying to estimate the impact on pay as time since the birth passes, the sample gets smaller and smaller and implicitly one is selecting people who had births closer and closer to age 28, so the longitudinal character of the

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some sample truncation bias.

data in not being used very effectively in this later model. In addition, we are asking the model to distinguish experience curvature from flattening due to childbearing in a relatively small group of women who are having children at almost precisely the time when the profile tends to flatten and when the data are reaching their end point. Given the difficulty of trying to tease so much out of the wage profiles of these women, we prefer the estimate that imposes a common functional form on the base profile for early and late childbearers, but we cannot fully reject the hypothesis that the costs of childbearing within skill groups do not differ depending on the timing.

What Explains the Fall in Earnings Among High-Scoring Mothers?

Several hypotheses have been suggested to explain the impact of childbearing on earnings. First, immediate pay and long-term wage growth may diminish in response to a move to part-time work by new mothers. Second, new mothers may withdraw from the labor force for several years or longer, and this interruption may knock them onto a slower career track. One might anticipate that patterns of withdrawal or moves to part-time status would be more common among low-skill than high-skill women, and indeed low-skill mothers are more likely to be outside the labor force than are higher skill mothers. But in fact the *changes* in work behavior after the birth of child are actually somewhat greater for high-skill women, especially with respect to part-time work. Higher scoring women work full-time all year much more than lower skill women do prior to their first birth, 70-75% versus 55-60%. But after birth roughly 35% of each group is working fully in the labor market in any of the 5 years after birth. Where the groups differ is on part-time work, with high-skill women being far more likely to be working part-time and low-skill women more likely to withdraw from the labor force altogether.

A third possibility is that wages decline in response to mothers leaving their previous employer when they give birth (either by choice or because they cannot get back their previous job).[14] Women who make such a change give up any benefits they were gaining from firm-specific human capital and presumably lose their returns to tenure. Finally, it is possible that mothers are perceived (rationally or irrationally) as less willing or able to spend the ―extra hour"

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[13] The variable is equal to zero in the years before the woman has a child and equal to age of first birth-21 for the periods after a woman has given birth. It thus represents a scalar adjustment to the apparent effect of childbearing.
[14] The Family and Medical Leave Act of 1993 now allows new mothers to take up to 12 weeks unpaid leave and still be rehired into comparable jobs if the woman worked for an employer with 50 or more employees and had worked 1,250 hours in the 12 months prior who the leave.

that superiors may use as a signal of commitment to the enterprise, and are thus less likely to gain promotions.

Table 9 examines these hypotheses for higher scoring women. The first column gives the results previously reported. Column (2) shows the effect of controlling for part-time status and tenure on the job. Interestingly, although tenure with employer is significant, it does not explain a great deal of the reduced pay associated with children. Part time work status negatively affects hourly wages. In our own examination of this issue, we found that women with longer tenures at their employer when they gave birth were less likely to move to part-time status or change employers. Thus, the group for whom the loss of tenure might make the greatest difference often do not change employers.

Column (3) also controls for whether or not the person stayed with the same employer two years after birth and whether they did not work at all in the second full year after the birth of the child. This latter variable proves to be the most powerful by far. It appears that extended work interruptions may be the most costly response to childbearing. Recall that this is net of any direct cost of lost experience. Women who leave the workforce for significant periods may indeed fall out of the ―fast lane.” Column (4) adds in two-digit occupation dummies to test whether women may be moving to lower paying occupations after childbearing, perhaps to find greater flexibility or reduced stress. Adding the occupation effects has no real impact.

Yet, perhaps surprisingly, even after controlling for all these variables, over half of the impact remains. Column (5) focuses on a select sub-group: women who work full-time all year in the second full year after they give birth for the same employer as prior to giving birth. One would certainly expect this group to be among the least affected by childbearing. Though the smaller sample sizes push some of the coefficients to insignificance, the point estimates are close to those in column (4). In other words even if women work full-time at their same employer, on average their wage growth slows and over time their pay appears to be 14% lower. The data do not allow any judgment as to whether this pay penalty reflects the conflict of commitment reported by some women, or direct or subtle discrimination against mothers reported by others.

Finally, we explored two additional potential influences on childbearing pay penalties. One might suppose that the negative effects of childbearing on pay would differ depending on the marital status of the mother. Married mothers might be able to share the burden with a partner, or conversely they may feel less pressure to continue competing at work. We found very little evidence that marital status influenced the results. In part, this reflects differences across skill groups. Low-scoring women are the group vastly more likely to be unmarried at the time of birth, but they are also the group where childbearing seems to impose far lower career costs. High-skill women, on the other hand, are virtually always married at the time of their first birth, and unmarried high-skill mothers of young children are a rather select group[15].

We also examined whether the results differed for black women. Once again, we found little evidence that black women fare differently. We first allowed for an interaction between the cost of childbearing and being black. This term was completely insignificant in all AFQT categories. In addition, we estimated separate effects for blacks in the bottom third of AFQT (the only group where we have a sizable sample of blacks) and the results were very similar to those reported earlier.

Impacts for Men

Figure 5 shows the age earnings graphs for men in the top third of AFQT distribution broken out by the age that the men report they first became fathers.[16] This graph looks rather different from Figure 4, the one for high-skill women. While men who have children later end up with higher pay than those who have them earlier, one sees far less evidence that trajectories shift with the arrival of parenthood. Rather, men who have children later seem to start and end with higher pay. Most notable here is the position of childless men. Men who never have children, far from being the best performers in the labor market, appear to be among the worst. This may either be a case of poor labor market performance making such men unappealing partners, or some unmeasured characteristics hurting men in both settings. In either case, it is obvious that any estimates that show that men gain from childbearing by comparing those who have children and those who do not will suffer from severe selection bias.

Table 10 gives the estimated impact of childbearing for men using the same specifications as we used for women. Unlike previous literature we do see some evidence of negative consequences of parenthood for men. However, the 10 year estimate for high skill men is roughly one tenth that for women, and in nearly all cases the effects are not statistically

[15] In an analysis which complements our paper, Buckles (2008) finds that including indicators for race, education, marital status, number of children, experience and occupation in the wage regression reduces the gain to delaying childbirth by one year for high skill women (Table 7). She interprets these results as consistent with a human-capital story; if discrimination against mothers were driving the penalty for high skill women, then including these observable characteristics should not reduced the gains to delay.

significant. Only the 10 year impact for low-skill men reaches conventional levels of significance. And unlike the estimates for women, these are very sensitive to functional form and fail our specification tests. For example if we include a variable capturing the 3 years prior to birth, the results seem to evaporate. Thus, we find a hint that high-skill men may be now feeling some negative consequences, but overall we find little evidence of strong effects of parenthood on male pay. Moreover, we see no evidence that men gain as a result of childbearing.

The Perceived Costs of Childbearing

It appears that the earnings costs of childbearing for women differ dramatically by skill level. All women's earnings are hurt by the time they take off from the labor market, but high-skill women also face a very high penalty in the form of lower pay above and beyond the effects of lost experience. Moreover, early childbearers face additional penalties, and these are far larger for high-skill women. Table 11 illustrates the impact of childbearing and timing for women of different skill levels using the models from Table 7, showing a sharp differential timing effect by skill level. While our analysis left some uncertainty about the magnitude (if any) of the timing effect, it seems likely that women considering when to have children would notice that late childbearers appear to do better in their careers without fully adjusting for the differing potential curvature of women bearing children early and late. They, like we, have trouble determining the true counterfactual. Thus, we would argue that the perceived and perhaps the real cost of childbearing is shown on Table 11.

In generating Table 11 we made several assumptions. Our goal is not to determine an accurate cost of childbearing, but rather to give a sense of the different magnitudes involved.[17] Within each skill group, all women are assumed to have the same pre-child experience profile and returns to education regardless of the timing of their first birth or if they decide not to have a child at all. Since we are trying to compare the impact of a choice to have a child and the timing of that childbirth, we should assume the wage profile is not different for women who do not have

[16] Unmarried fathers report fewer children than do unmarried mothers. But one would expect that any children with whom the father is deeply involved and who thus might influence earnings would be reported.

[17] We assumed that each woman had only one child. We assumed that low (middle; high) scoring women worked 80% (90%, 100%) of full time in the years prior to having children, 50% (50%; 60%) in the first five years after birth, 70% (80%; 80%) in the next 5 years, and back to 80% (90%; 100%) in subsequent years. These roughly correspond to the actual patterns for mothers of different AFQT scores in our data. We assumed that once wages leveled off, they remained constant until retirement. Finally, women work until age 65 and then retire. Present values of earnings were determined at age 18 using a 3% real discount rate.

children in this hypothetical exercise. Thus, the predicted wages for a woman who never has a child are based on the equation estimated for women who did have them.[18]

Two features of Table 11 are notable. First, the impact of delay for low-scoring women is much smaller than for high-skill women. A low-scoring women stands to gain less than $20,000 or 5% of lifetime earnings by waiting. By contrast, women in the top group gain nearly $125,000 or more than 15% of lifetime earnings. Second, and even more starkly, the costs to having children are vastly higher for high-skill women regardless of timing. If a low-scoring woman chooses to have children, she will give up 10 to 14% of her potential lifetime earnings. But a high scoring woman will give up nearly 21 to 33% of earnings. Even if such women wait until age 30 to have a child, the cost is nearly $230,000.

These figures offer a powerful reason why the birthing patterns of women might differ so much by level of skill. High-skill women apparently face very high costs from early childbearing and give up a great deal by having children at all. Consistent with these costs, high-skill women typically have children late, and many avoid childbirth altogether. By contrast, low-skill women face much lower costs of childbearing, benefit less from delaying childbirth, and consistent with these results, they tend to have children earlier.

Discussion

We have not sought to develop a full model of birth timing decisions here or prove that career costs are the driving force behind differential fertility patterns. A fuller model would take account of male partners, potential effects of childbearing on marriage, and a series of other issues. But these data alone point to powerful economic effects of childbearing, which likely influence patterns of fertility in turn. We suspect that dramatic changes in the 1960s: the women's movement, the pill, the expansion in work opportunities for women, and altered attitudes about maternal work and premarital sexual activity, all gave women a new ability to control fertility and potential incentives to do so. In effect, these social and economic changes allowed economic forces to play a much stronger role in decisions about fertility. But as we see, even today, the economic reasons to postpone or avoid childbearing appear vastly stronger for high-skill women than for low-skilled ones. Thus, it is the behavior of high skill women that has changed radically.

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[18] We have already established that the wage profiles of women who actually remain childless are often lower than those who have children among the most skilled women. The right comparison is what the women who become

The implications of these increasingly large differences in fertility patterns may be profound. Children born to low-skill women come early, when the mother is often earning very little money. Few of these mothers are married at the time of birth. Children born to high-skill women almost always enter the home of a married couple in their peak earning years. The potential differences in child outcomes are great.

Our research suggests that policymakers may want to consider whether the most effective strategy for influencing the timing of childbearing, especially teen childbearing, is to increase the benefits of delay by altering economic opportunities. Meanwhile, society must confront a more profound question: how, if at all, should the nation respond to the reality that the costs of childbearing fall disproportionately on women, leading to significant consequences for the pattern of American fertility.

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mothers would have earned had they remained childless.

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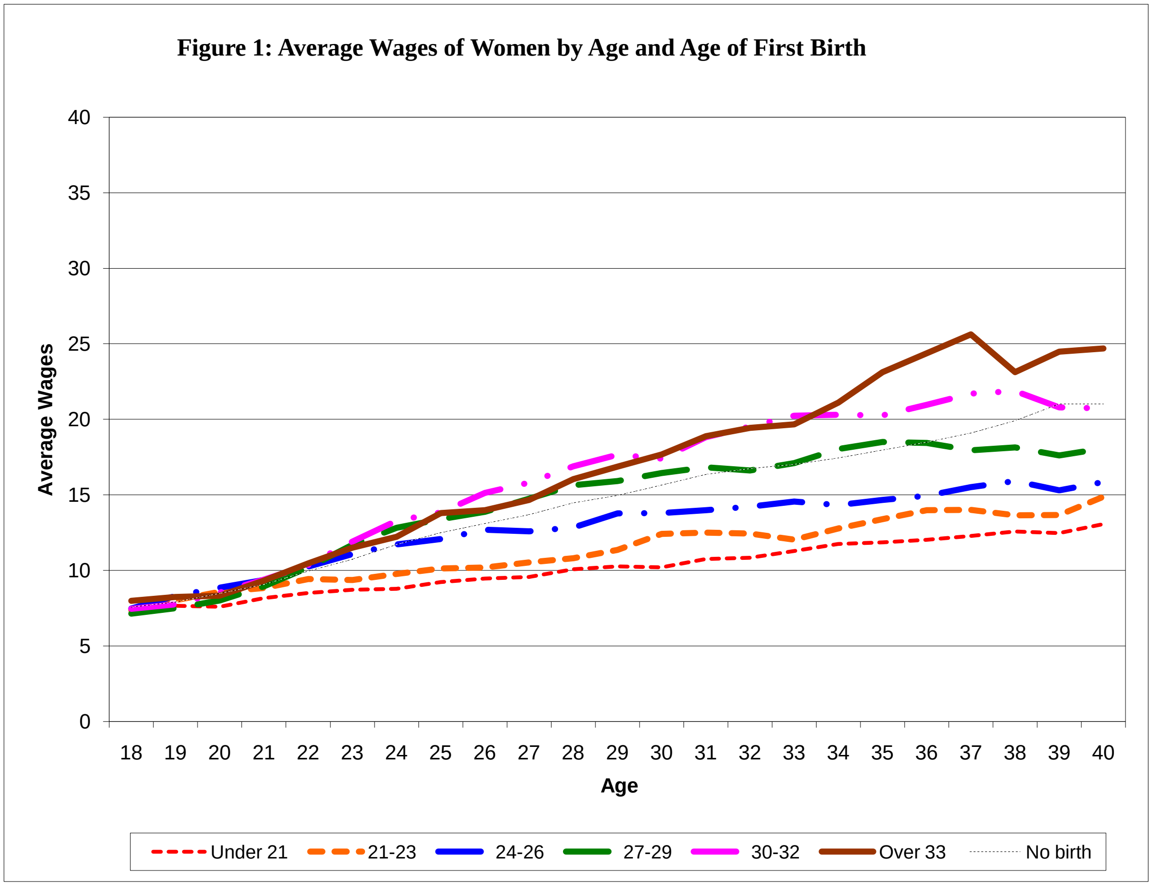

Figure 1 graphs the mean wage by age for women.Note that only 3% of births occur to women over the age of 35 and only 3% of births occur to women between the ages of 33 and 35.Wages are expressed in 2006 dollars.

Figure 2: Average Wages of Women in the Bottom AFQT Third by Age and Age of First Birth

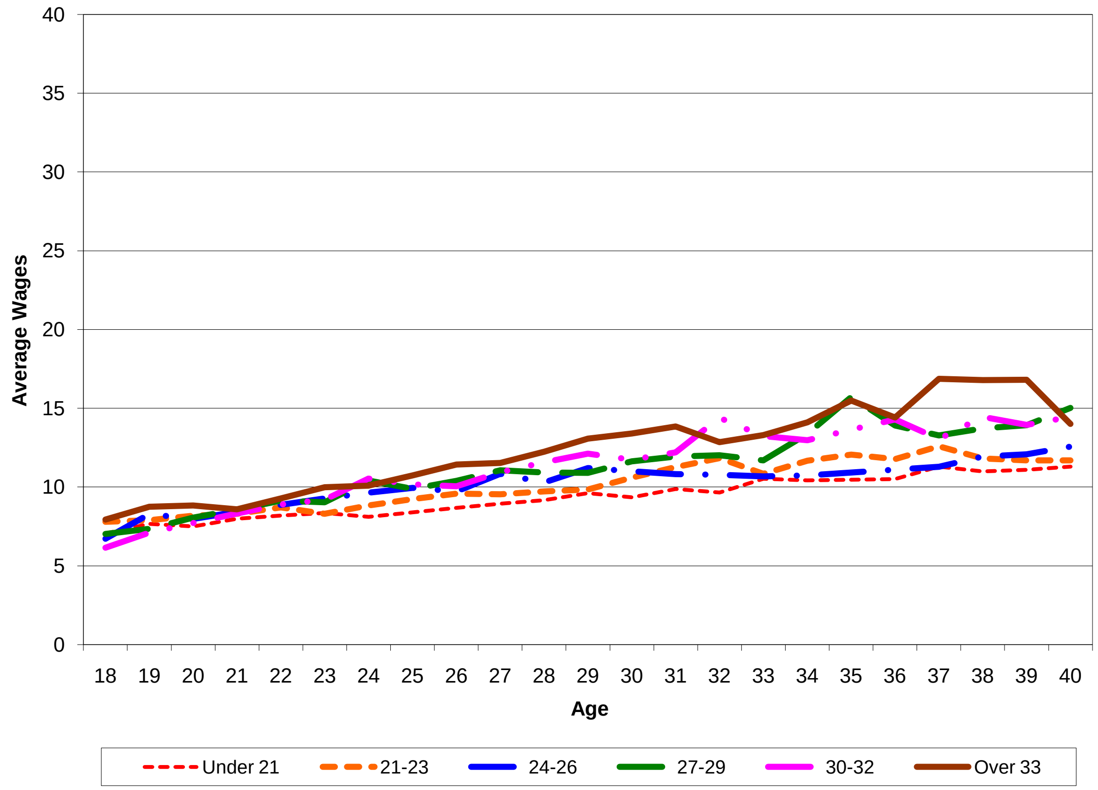

Figure 2 graphs the mean wage by age for women in the bottom AFQT third.Note that only 3% of births occur to women over the age of 35 and only 3% of births occur to women between the ages of 33 and 35. Wages are expressed in 2006 dollars.

Figure 3: Average Wages of Women in the Middle AFQT Third by Age and Age of First Birth

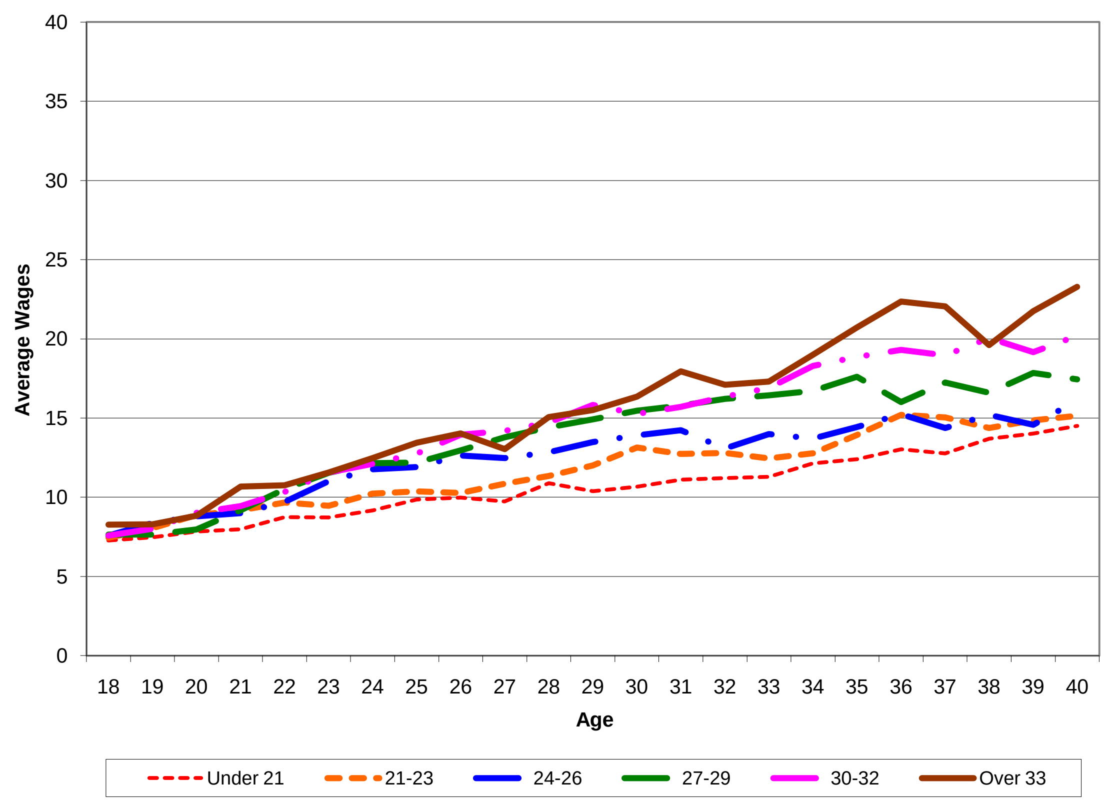

Figure 3 graphs the mean wage by age for women in the middle AFQT third.Note that only 3% of births occur to women over the age of 35 and only 3% of births occur to women between the ages of 33 and 35.Wages are expressed in 2006 dollars.

Figure 4: Average Wages of Women in the Top AFQT Third by Age and Age of First Birth

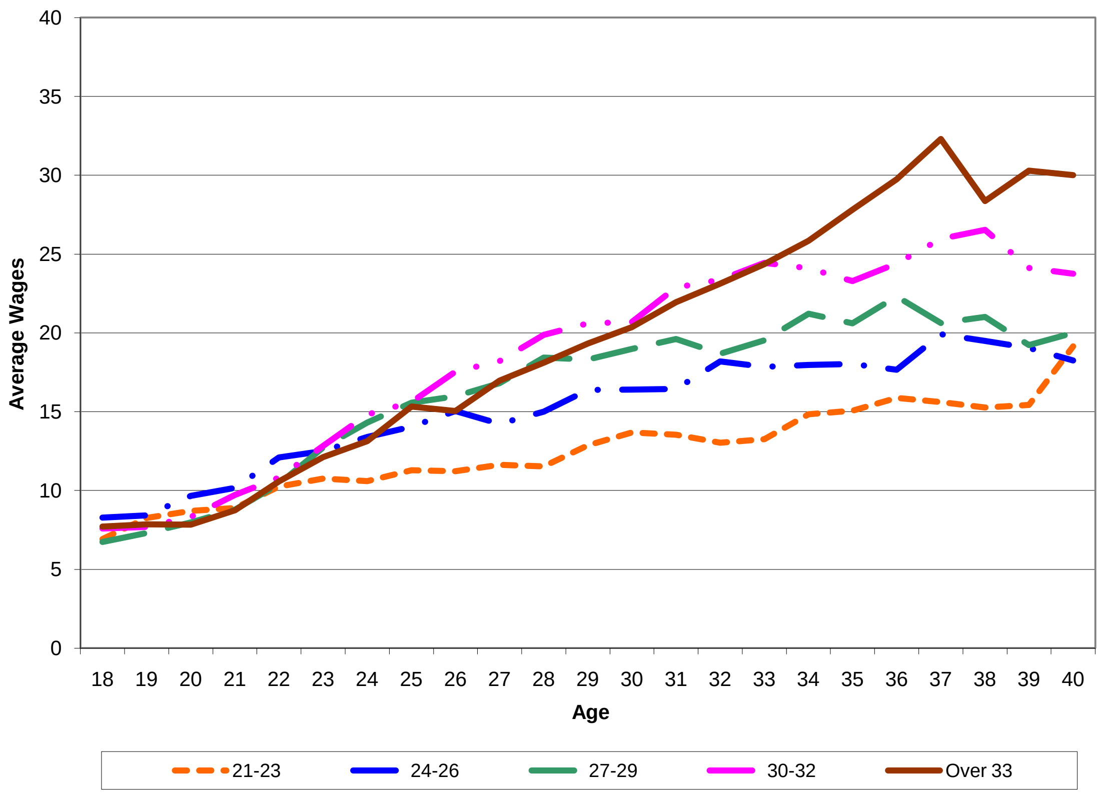

Figure 4 graphs the mean wage by age for women in the top AFQT third.Note that only 6% of births occur to women over the age of 33.Wages are expressed in 2006 dollars.

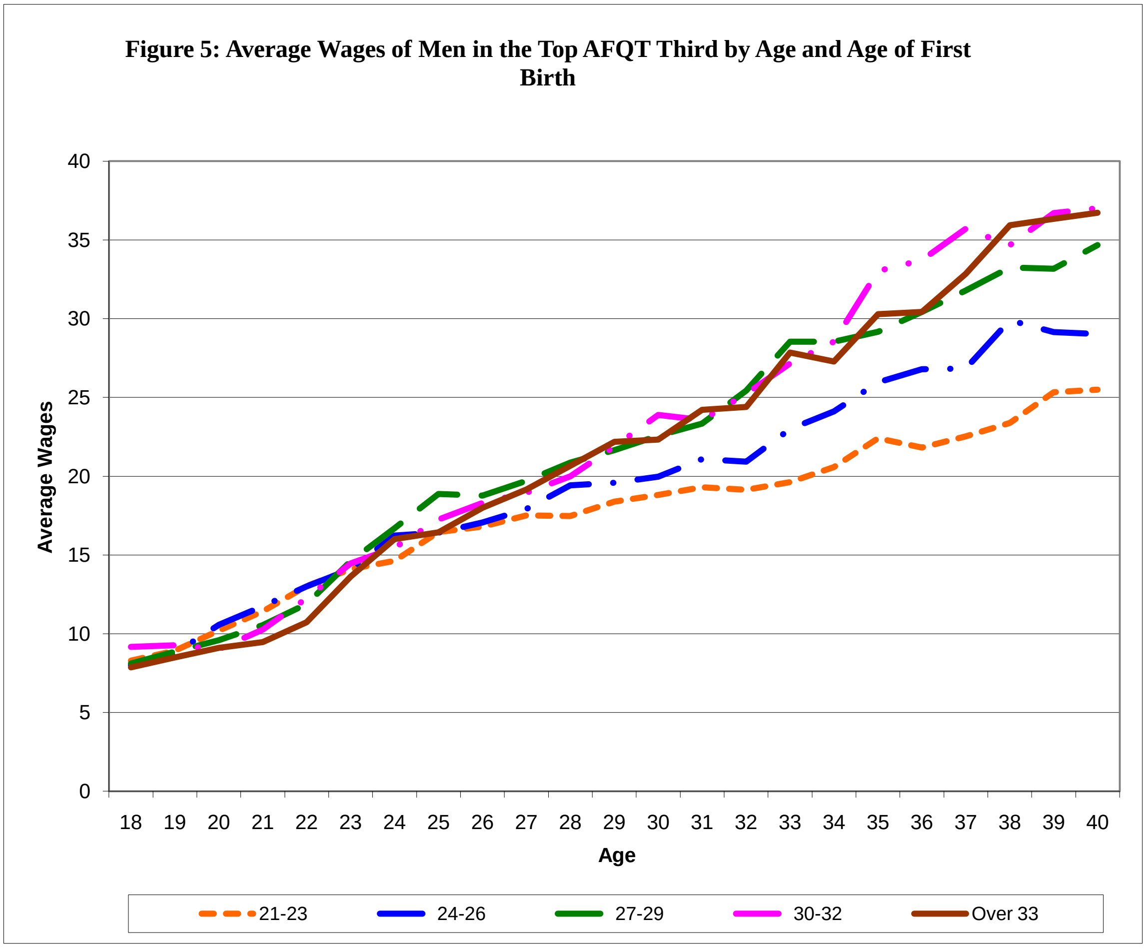

Figure 5 graphs the mean wage by age for men. Wages are expressed in 2006 dollars.

Table 1: Birth Patterns of Women by Level of Education, Women born in 1960-1964

Level of Education	Percent with First Birth by Age 25	Percent with First Birth by Age 30	Percent with First Birth by Age 40	Average Number of Children Born by Age 40
Dropouts	78	83	86	2.5
HS Grads	64	79	83	1.9
Some College	49	70	81	1.8
College Graduate	20	50	74	1.6

Source: Columns 1 and 2 are from Ellwood and Jencks (2004) based on June CPS data. Columns 3 and 4 are calculated averaging the population of women who were 40 in June 2000, 40 or 41 in June 2002, and 40 or 41 in June 2004.

Table 2: Fixed Effects Log Wage Regression for Women using Potential Experience Women Aged 20 and Over Who Had a Child After Age 21

	All Women	Low AFQT Women	Mid AFQT Women	High AFQT Women
Education	0.1376	0.0985	0.1243	0.1441
	(0.0076)	(0.0209)	(0.0138)	(0.0107)
Potential Experience (age-schooling-5)	0.0628	0.0411	0.0552	0.0767
	(0.0035)	(0.0067)	(0.0058)	(0.0058)
Potential Experience Squared	-0.0010	-0.0005	-0.0008	-0.0013
	(0.0001)	(0.0002)	(0.0002)	(0.0002)
First 4 years After First Birth	-0.0950	-0.0742	-0.0735	-0.1058
	(0.0129)	(0.0279)	(0.0207)	(0.0210)
Years 5-9 After First Birth	-0.1770	-0.0822	-0.1353	-0.2379
	(0.0200)	(0.0380)	(0.0321)	(0.0346)
Years 10 and higher After First Birth	-0.2607	-0.1199	-0.2153	-0.3508
	(0.0277)	(0.0502)	(0.0418)	(0.0524)
Number of Additional Children	-0.0533	-0.0363	-0.0432	-0.0702
	(0.0097)	(0.0161)	(0.0158)	(0.0175)
Fixed Effects?	YES	YES	YES	YES
Observations (Unique clusters)	2434	764	809	720
R-squared	0.592	0.504	0.566	0.589

Clustered standard errors in parentheses.

Table 3: Fixed Effects Log Wage Regression for Women using Actual Experience Women Aged 20 and Over Who Had a Child After Age 21

	All Women	Low AFQT Women	Mid AFQT Women	High AFQT Women
Education	0.0987	0.070	0.0831	0.0981
	(0.0082)	(0.023)	(0.0135)	(0.0119)
Actual Experience in Years	0.0652	0.044	0.0642	0.0776
	(0.0033)	(0.007)	(0.0047)	(0.0051)
Actual Experience Squared	-0.0011	-0.0007	-0.0011	-0.0014
	(0.0001)	(0.0002)	(0.0002)	(0.0002)
First 4 years After First Birth	-0.0681	-0.06	-0.0636	-0.0808
	(0.0123)	(0.03)	(0.0195)	(0.0207)
Years 5-9 After First Birth	-0.1208	-0.04	-0.1069	-0.1752
	(0.0186)	(0.04)	(0.0312)	(0.0312)
Years 10 and higher After First Birth	-0.1760	-0.06	-0.1662	-0.2355
	(0.0244)	(0.04)	(0.0397)	(0.0442)
Number of Additional Children	-0.0252	-0.03	-0.0205	-0.0274
	(0.0104)	(0.02)	(0.0156)	(0.0192)
Fixed Effects?	YES	YES	YES	YES
Observations (Unique clusters)	2330	739	784	706
R-squared	0.614	0.521	0.588	0.611

Clustered standard errors in parentheses. Experience in years calculated as hours of work experience divided by 2000 hours per year.

Table 4: Fixed Effects Log Wage Regressions for Women using Actual Experience Including Variable for 3 Years Prior to Birth
Women Aged 20 and Over Who Had a Child After Age 21

	All Women	Low AFQT Women	Mid AFQT Women	High AFQT Women
Education	0.0983	0.0694	0.0838	0.0973
	(0.0081)	(0.0227)	(0.0134)	(0.0119)
Actual Experience in Years	0.0643	0.0426	0.0655	0.0753
	(0.0037)	(0.0082)	(0.0052)	(0.0057)
Actual Experience Squared	-0.0011	-0.0007	-0.0012	-0.0013
	(0.0001)	(0.0002)	(0.0002)	(0.0002)
3 Years Prior to First Birth	0.0112	0.0150	-0.0160	0.0258
	(0.0130)	(0.0271)	(0.0210)	(0.0214)
First 4 years After First Birth	-0.0596	-0.0454	-0.0757	-0.0608
	(0.0181)	(0.0390)	(0.0279)	(0.0304)
Years 5-9 After First Birth	-0.1106	-0.0263	-0.1216	-0.1507
	(0.0242)	(0.0527)	(0.0378)	(0.0407)
Years 10 and higher After First Birth	-0.1633	-0.0409	-0.1844	-0.2059
	(0.0302)	(0.0614)	(0.0469)	(0.0536)
Number of Additional Children	-0.0250	-0.0285	-0.0207	-0.0269
	(0.0104)	(0.0166)	(0.0156)	(0.0192)
Fixed Effects?	YES	YES	YES	YES
Observations (Unique clusters)	2330	739	784	706
R-squared	0.614	0.521	0.588	0.611

Clustered standard errors in parentheses. Experience in years calculated as hours of work experience divided by 2000 hours per year.

Table 5: Specification and Sample Effects in Fixed Effects Log Wage Regressions Low AFQT Women

	All Low AFQT Women	All Low AFQT Women	Low AFQT Women who Have a Child	Low AFQT Women over Age 20 who have a Child After Age 21
Education	0.0585	0.0588	0.0537	0.0698
	(0.0082)	(0.0082)	(0.0097)	(0.0228)
Actual Experience in Years	0.0473	0.0466	0.0475	0.0437
	(0.0032)	(0.0035)	(0.0040)	(0.0073)
Actual Experience Squared	-0.0009	-0.0009	-0.0009	-0.0007
	(0.0001)	(0.0001)	(0.0001)	(0.0002)
Has First Child	-0.0262	-	-	-
	(0.0174)	-	-	-
First 4 years After First Birth	-	-0.0313	-0.0309	-0.0563
	-	(0.0170)	(0.0174)	(0.0252)
Years 5-9 After First Birth	-	-0.0159	-0.0166	-0.0392
	-	(0.0223)	(0.0236)	(0.0371)
Years 10 and higher After First Birth	-	-0.0151	-0.0178	-0.0569
	-	(0.0265)	(0.0286)	(0.0446)
Number of Additional Children	-0.0166	-0.0194	-0.0192	-0.0291
	(0.0079)	(0.0083)	(0.0084)	(0.0165)
Fixed Effects?	YES	YES	YES	YES
Observations (Unique clusters)	2413	2413	2057	739
R-Squared	0.487	0.487	0.468	0.521

Clustered standard errors in parentheses. Experience in years calculated as hours of work experience divided by 2000 hours per year.

Table 6: Specification and Sample Effects in Fixed Effects Log Wage Regressions High AFQT Women

	All High AFQT Women	All High AFQT Women	High AFQT Women who Have a Child	High AFQT Women over Age 20 who have a Child After Age 21
Education	0.1013	0.0993	0.1013	0.0981
	(0.0052)	(0.0052)	(0.0060)	(0.0119)
Actual Experience in Years	0.0641	0.0663	0.0722	0.0776
	(0.0036)	(0.0036)	(0.0044)	(0.0051)
Actual Experience Squared	-0.0012	-0.0012	-0.0013	-0.0014
	(0.0001)	(0.0001)	(0.0001)	(0.0002)
Has First Child	-0.0432	-	-	-
	(0.0196)	-	-	-
First 4 years After First Birth	-	-0.0311	-0.0615	-0.0808
	-	(0.0191)	(0.0200)	(0.0207)
Years 5-9 After First Birth	-	-0.0901	-0.1342	-0.1752
	-	(0.0273)	(0.0301)	(0.0312)
Years 10 and higher After First Birth	-	-0.1176	-0.1823	-0.2355
	-	(0.0350)	(0.0410)	(0.0442)
Number of Additional Children	-0.0454	-0.0247	-0.0245	-0.0274
	(0.0141)	(0.0153)	(0.0154)	(0.0192)
Fixed Effects?	YES	YES	YES	YES
Observations (Unique clusters)	1251	1251	897	706
R-Squared	0.627	0.627	0.625	0.611

Clustered standard errors in parentheses. Experience in years calculated as hours of work experience divided by 2000 hours per year.

Table 7: Fixed Effects Log Wage Regressions for Women using Actual Experience. Differential Impact by Age of First Birth Women Aged 20 and Over Who Had a Child After Age 21

	All Women	Low AFQT Women	Mid AFQT Women	High AFQT Women
Education	0.0982	0.0698	0.0826	0.0984
	(0.0082)	(0.0228)	(0.0136)	(0.0119)
Actual Experience in Years	0.0655	0.0436	0.0644	0.0783
	(0.0033)	(0.0073)	(0.0048)	(0.0051)
Actual Experience Squared	-0.0012	-0.0007	-0.0012	-0.0015
	(0.0001)	(0.0002)	(0.0002)	(0.0002)
First 4 years After First Birth	-0.1175	-0.0615	-0.0919	-0.1691
	(0.0214)	(0.0359)	(0.0332)	(0.0399)
Years 5-9 After First Birth	-0.1662	-0.0440	-0.1331	-0.2551
	(0.0250)	(0.0420)	(0.0401)	(0.0453)
Years 10 and higher After First Birth	-0.2129	-0.0609	-0.1875	-0.3008
	(0.0281)	(0.0468)	(0.0437)	(0.0524)
Number of Additional Children	-0.0234	-0.0287	-0.0195	-0.0248
	(0.0104)	(0.0164)	(0.0155)	(0.0192)
Impact of Each Additional Year Past 21 for Age of First Birth	0.0068	0.0008	0.0039	0.0112
	(0.0026)	(0.0047)	(0.0043)	(0.0043)
Fixed Effects?	YES	YES	YES	YES
Observations (Unique clusters)	2330	739	784	706
R-Squared	0.614	0.521	0.588	0.611

Clustered standard errors in parentheses. Experience in years calculated as hours of work experience divided by 2000 hours per year.

Table 8: Fixed Effects Log Wage Regressions Seperated by Age of First Birth High AFQT Women Aged 20 and Over Who Had a Child After Age 21

	All High AFQT Women	High AFQT Women: Birth Before Age 28	High AFQT Women: Birth Age 28 or Older
Education	0.0991	0.0922	0.1047
	(0.0120)	(0.0197)	(0.0147)
Actual Experience in Years	0.0759	0.0741	0.0716
	(0.0051)	(0.0108)	(0.0056)
Actual Experience Squared	-0.0013	-0.0016	-0.0009
	(0.0002)	(0.0003)	(0.0002)
Birth Before Age 28: First 4 Years After First Birth	-0.1192	-0.1032	-
	(0.0271)	(0.0350)	-
Birth Before Age 28: Years 5-9 After First Birth	-0.1893	-0.1577	-
	(0.0368)	(0.0521)	-
Birth Before Age 28: Years 10 and Higher After First Birth	-0.2373	-0.1494	-
	(0.0476)	(0.0698)	-
Birth Age 28 or Older: First 4 years After First Birth	-0.0464	-	-0.0706
	(0.0256)	-	(0.0272)
Birth Age 28 or Older: Years 5-9 After First Birth	-0.1535	-	-0.1989
	(0.0389)	-	(0.0426)
Birth Age 28 or Older: Years 10 and Higher After First Birth	-0.2354	-	-0.3210
	(0.0525)	-	(0.0609)
Number of Additional Children	-0.0280	-0.0156	-0.0457
	(0.0189)	(0.0256)	(0.0238)
Fixed Effects?	YES	YES	YES
Observations (Unique clusters)	706	337	369
R-Squared	0.611	0.561	0.630

Clustered standard errors in parentheses. Experience in years calculated as hours of work experience divided by 2000 hours per year.

Table 9: Fixed Effects Log Wage Regressions Seperated by Age of First Birth High AFQT Women Aged 20 and Over Who Had a Child After Age 21

	High AFQT	High AFQT	High AFQT	High AFQT	High AFQT stayers/full-time*
Education	0.0981	0.1038	0.1054	0.0972	0.0838
	(0.0119)	(0.0124)	(0.0123)	(0.0117)	(0.0219)
Experience in Years	0.0776	0.0656	0.0648	0.0612	0.0809
	(0.0051)	(0.0056)	(0.0056)	(0.0053)	(0.0120)
Experience in Years Squared	-0.0014	-0.0012	-0.0013	-0.0011	-0.0017
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0003)
First 4 years After First Birth	-0.0808	-0.0732	-0.0407	-0.0331	-0.0795
	(0.0207)	(0.0211)	(0.0233)	(0.0229)	(0.0419)
Years 5-9 After Birth	-0.1752	-0.1571	-0.1173	-0.0924	-0.1216
	(0.0312)	(0.0347)	(0.0368)	(0.0351)	(0.0691)
Years 10 and higher After First Birth	-0.2355	-0.2102	-0.1591	-0.1438	-0.1357
	(0.0442)	(0.0499)	(0.0539)	(0.0510)	(0.1100)
Number of Additional Children	-0.0274	-0.0380	-0.0382	-0.0243	-0.0813
	(0.0192)	(0.0227)	(0.0226)	(0.0198)	(0.0366)
Tenure with Employer	-	0.0184	0.0176	0.0167	0.0135
	-	(0.0027)	(0.0028)	(0.0027)	(0.0046)
Part Time of Part Year	-	-0.0391	-0.0408	-0.0313	-0.0809
	-	(0.0173)	(0.0173)	(0.0168)	(0.0385)
New Employer in 2nd Full Year	-	-	-0.0376	-0.0313	-
	-	-	(0.0419)	(0.0389)	-
Didn Not Work in 2nd Full Year	-	-	-0.1731	-0.1523	-
	-	-	(0.0657)	(0.0633)	-
2 Digit Occupation Dummies	No	No	No	Yes	No
Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations (Unique clusters)	706	706	706	706	133
R-Squared	0.611	0.597	0.598	0.623	0.623

* Worked Full-year, Full-time at Pre Birth Employer in 2nd Full Year After Birth. Clustered standard errors in parentheses. Experience in years calculated as hours of work experience divided by 2000 hours per year.

Table 10: Fixed Effects Log Wage Regressions for Men using Actual Experience Men Aged 20 and Over Who Had a Child After Age 21

	All Men	Low AFQT Men	Mid AFQT Men	High AFQT Men
Education	0.1237	0.0238	0.1124	0.1140
	(0.0076)	(0.0227)	(0.0165)	(0.0091)
Actual Experience in Years	0.0530	0.0386	0.0462	0.0608
	(0.0023)	(0.0049)	(0.0041)	(0.0041)
Actual Experience Squared	-0.0007	-0.0006	-0.0006	-0.0009
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
First 4 years After First Birth	0.0045	-0.0045	0.0398	0.0511
	(0.0116)	(0.0283)	(0.0324)	(0.0275)
Years 5-9 After First Birth	-0.0468	-0.0379	0.0006	0.0329
	(0.0183)	(0.0390)	(0.0450)	(0.0372)
Years 10 and higher After First Birth	-0.1356	-0.1132	-0.0410	-0.0253
	(0.0264)	(0.0519)	(0.0575)	(0.0502)
Number of Additional Children	-0.0036	-0.0197	-0.0160	0.0118
	(0.0077)	(0.0141)	(0.0147)	(0.0115)
Fixed Effects?	YES	YES	YES	YES
Observations (Unique clusters)	2754	1004	788	801
R-Squared	0.643	0.559	0.596	0.663

Clustered standard errors in parentheses. Experience in years calculated as hours of work experience divided by 2000 hours per year.

Table 11: Simulated Present Value of Lifetime Earnings Women with One or No Children by AFQT Third and Timing of First Birth Discounted to Age 18

	Age of First Birth				
	20	22	25	30	Never Gave Birth
Low AFQT	403,290	406,631	411,967	420,423	469,712
Middle AFQT	523,951	533,465	550,741	578,097	692,056
High AFQT	736,482	744,551	790,962	865,447	1,095,416

Based on models in Table 7. Assumes 3% real discount rate, and that wages level off (rather than fall) when at the negative effect of the experience squared term dominates the positive effect of experience. Assumes only one child. “Never Gave Birth” is an out-of-sample prediction from that model (which is based only on people who give birth) to project what their pay would have been had they postponed childbearing forever. Low AFQT women are assumed to get 12 years of schooling, and to work 80% of full time (80% of 2000 hours) in the years prior to having children, 50% in the first five years after birth, 70% the next 5 years, and back to 80% in subsequent years. Middle AFQT mothers are assumed to get 13 years of schooling and work 90%, 50%, 80%, 90% respectively; for high AFQT, schooling is assumed to be 16 years, and their work patterns would be 100%, 60%, 80%, 100%. Women retire at age 65 and have no further earnings.