Persistence in Labor Supply Effects of Graduating in a Recession: The Case of High School Women*

Brad Hershbein - The University of Michigan

March 3, 2009

Preliminary Draft. Please do not distribute or cite without permission. Comments welcome.

Abstract

This paper explores the short- and long-term impacts of graduating high school during an economic recession on the labor supply of women. I develop a simple, dynamic choice model that allows an initial labor demand shock to have persistent effects on labor supply. I then test the implications of the model using the National Longitudinal Survey of Youth, 1979. With these data, I can track women who graduated from high school between 1975 and 1983, a period that spanned a severe recession, for the following 15 to 29 years. For identification, I exploit both temporal and spatial variation in initial labor market conditions by using the national, state, and in some specifications, metropolitan area unemployment rates. The results support the hypothesis that women who graduate during a period of high unemployment reduce their labor supply in the short run, with suggestive evidence that they instead substitute into earlier family formation. Impacts on various welfare measures, such as poverty status and receipt of government assistance, are also explored. In contrast with previous studies that have found persistent, negative wage effects to graduating in a recession among college-educated men, I find no significant impact on wages in my sample.[1]

JEL Classification: J12, J13, J22

Keywords: recession, female labor supply, high school graduates, family formation

*The author thanks Martha Bailey, Justin McCrary, Ryan Michaels, Melissa Kearney, and participants of the informal labor seminar at the University of Michigan and the lunch seminar at the Federal Reserve Bank of Boston for helpful comments. All remaining errors are his own.

[1] All unreported results that are mentioned in the text are available from the author upon request.

1 Introduction

A small but growing economics literature has explored the impact that labor market conditions at the time of labor market entry, often high school or college graduation, has on the long-term career profiles of workers. Specifically, much of this literature seeks to test empirically whether a negative labor market shock—a recession, for example—early in a worker's career has a persistent negative effect on the worker's labor market outcome relative to one who began working during a more upbeat economy. The subject has made for an interesting empirical question because different economic theories of labor supply offer different predictions. Common to nearly all the career effect studies is the predominant dependent variable of interest of wages. This focus is not surprising, given that wages are readily observed among those working in most datasets, and they are the most common indicator used to measure economic wellbeing.

Prominent examples in this literature include Beaudry and DiNardo (1991); Oreopolous, von Wachter, and Heisz (2005); Kahn (2006), and Oyer (2006, 2007). Beaudry and DiNardo (1991), for instance, in their landmark study of implicit wage contracting, examined how the unemployment rate at different points of an experienced prime-age male worker's career affected his wages. Oreopolous *et al.* (2005) use employer-employee matched data from Canada to study the career earnings and job characteristics of men who graduated college at different points in the business cycle. Kahn's (2006) study is similar, though her focus is on American, white male college graduates. And Oyer looks at how the placement and publishing records of economics PhD graduates (2006) or the placement and earnings of new MBAs (2007) vary with overall labor demand at the time they go on the job market. All of these studies find that early labor market conditions, generally proxied by the unemployment rate, have a negative, persistent impact on wages and/or job prestige.[2]

In addition to their emphasis on the outcomes of wages and job characteristics, this subset of the literature has another factor in common. The group being studied is always one considered to be highly attached to the labor force: men, the highly educated (college graduates or beyond), or highly educated men. It is implicitly presumed that the individuals in the sample *will* work; the

[2]Given their differing samples and methodologies, it is not surprising that the studies vary somewhat on the estimated duration of the negative effect, with some suggesting a transitory impact and others finding near-permanent effects. However, all of the papers identify a negative impact that persists for at least 8 to 10 years.

research question is a matter of how hard and for what rewards. This study takes a different tack: it attempts to determine whether initial negative labor market shocks affect a not-well-studied demographic group in this literature—high school graduate women—along other channels besides wages, such as the extent and timing of labor supply and home production. To my knowledge, no other paper has specifically investigated (1) whether macroeconomic conditions at the time of labor force entry produce enduring effects on the labor supply of women, (2) how these effects, if any, compare vis-à-vis men, and (3) how these effects interact with family formation, such as the decision to marry and whether and when to have children.

There are several reasons why high school graduate women are an important group to study. First, these women are likely to be less attached to the labor force than the men previously studied. For example, Blau and Kahn (2005) report that although women's responsiveness to working with respect to wages was declining in the 1980 to 2000 period, it remained significantly higher than that for men. Given this relatively higher elasticity, it is plausible that women on the cusp of first entering the labor force in a recessionary period of depressed real wages would be less likely to participate than men, other things equal. Second, most young women who are not in the labor force (and not in school) report in the Current Population Survey (CPS) that their major activity is taking care of home and family—home production.[3] If home production represents a viable alternative to market work (at least in the short run), do women at the margin select into it when the market wage falls, as it appears to in a recession? And if so, is home production observable in the form of earlier marriage or childbearing? Third, beginning in the early 1980s, women began to outnumber men in college-going. As Goldin (2006) has noted, the generation of women who graduated high school in the late 1970s and early 1980s had greater career ambitions than did their parents; rather than trying to find a job during a recession right after high school or starting a family, perhaps obtaining more education represented another appealing option. All of these hypotheses are empirically testable.

The goal of this study is to begin to look at how women's labor supply choices are affected when

[3]According to the March CPS, for the period 1975 through 1999, an average of 24 percent of women aged 18 to 40 reported not being in the labor force at any point during the year. Of these 24 percent, 70 percent reported their primary reason of not working as "taking care of home/family."

they graduate high school in an unfavorable labor market. The rest of the paper proceeds as follows. In the next section, I present a relatively simple discrete choice model of labor supply and derive an estimation equation. Section 3 discusses the dataset used to empirically test the predictions of the model, the National Longitudinal Survey of Youth 1979, and describes the estimation strategy. The fourth section presents the main results of the effects of an initial labor demand shock on the probability of labor force participation; it also covers several checks for robustness and explores some channels through which the effects might be operating, as well as looking at some welfare impacts. Section 5 offers a brief conclusion and thoughts for future research.

2 Theory and Model

In this section, I develop a relatively simple dynamic, structural framework that allows individuals to choose among working in the market, pursuing education, or engaging in home production in each period. Rather than attempting to solve a full-blown model of female labor supply[4], I derive from the model implications for comparative statics, particularly those of a labor demand shock at labor market entry on a woman's decision-making. The comparative static applications generate empirically testable predictions, and these tests are carried out in Sections 3 and 4.

2.1 Setup

The objective of the woman is to maximize her discounted lifetime utility over consumption:

$$Max \sum_{t=0}^{T} \beta^t U(C_t), \tag{1}$$

where β is the discount rate and C_t is period t consumption of a composite good. The time horizon is finite and is assumed to be known, with t = 0 corresponding to high school graduation.[5] Since

[4] Quintessential examples of this earlier literature include Heckman and Willis (1977) and Heckman and MaCurdy (1980); both focused on married women. Over the next two decades, increasingly more focus was placed on the evolution of female labor force participation over time or the welfare of women vis-à-vis men rather than the labor supply decision itself; see Goldin (1990). A recent exception is the structural lifecycle model of Ge (2006).

[5] High school graduation seems a plausible time for when women begin to make lifecycle choices: it roughly corresponds to the age of majority; schooling is no longer broadly compulsory, either legally or by family pressure; and family formation before this time is relatively rare. Still, because not all women graduate from high school

$U(\cdot)$ has only one argument and is presumed to be an increasing function, the problem above is identical to the maximization of the discounted sum of $\{C_t\}$, and it is with this identity that I proceed.

In each period, the woman makes a discrete, mutually exclusive choice whether to work in the market, engage in home production, or pursue more education. The output from market work and home production are presumed to be perfect substitutes, as in Gronau (1986), but here in a dynamic setting.[6] Further education, on the other hand, is costly, but it raises the potential market wage. In this context, each of the three actions is associated with a consumption (or, equivalently, income) flow that period. That is, if a woman works in the market, she earns w_t, her prevailing wage that period. If she engages in home production, she earns h_t, her productivity that period in the home sector. Last, if she chooses education, she *pays* in that period e_t, the cost of education.[7] This yields a period budget constraint:

$$C_t = P_t w_t + Q_t h_t + S_t e_t + A_t, \tag{2}$$

where P_t, Q_t, and S_t are indicator variables for whether the woman works in the market, works in home production, or chooses education, respectively, and A_t is an exogenous source of flow income in period t, perhaps from welfare or parental transfers.[8] For simplicity, there is no saving in this model.

If w_t, h_t, and e_t were all exogenous, the solution to the maximization problem would be quite simple: each period, the woman picks the option that grants her the highest consumption that period. Such a framework would be identical to a series of static optimization problems. However,

(approximately 25 percent of women in the data set I use did not graduate), the model should not be construed as representative of all young women of approximately age 18.

[6]Guryan, Hurst and Kearney (2008) have argued persuasively that child care, here assumed to be embedded within home production, is sufficiently different from other forms of home production in terms of income and educational elasticities that it should constitute a distinct alternative. While a formal model differentiating child care (CC) from other home production (OHP) is beyond the scope of this paper, it may be helpful to think of the payoff from what is called home production below to be the output of a CES production function. That is, $h = [\theta CC^{\gamma} + (1-\theta)OHP^{\gamma}]^{1/\gamma}$, where θ is the share parameter between the two factors, and $\varepsilon = \frac{1}{1-\gamma}$ is the constant elasticity of substitution between the two factors.

[7]Thus, there is no restriction that the consumption flow be non-negative each period; I abstract away from such subsistence models.

[8]Note that since A_t is exogenous, it does not affect the solution to the maximization problem.

the three parameters are not all exogenous but instead have their own equations of motion.

In particular, $w_t = w(y', y_t, s_t, \mu_t, \eta_t)$, where s_t is the cumulative level of education the woman has coming into period t, μ_t is a measure of cumulative market experience (the number of periods worked), and η_t is a normally-distributed white noise disturbance. y_t is a contemporaneous labor demand shock that is modeled as an AR(1) process; specifically, $y_t = \rho y_{t-1} + \epsilon_t$, with ϵ_t having a mean of 0 and drawn from an identical, independent distribution. y' is the labor demand shock in the period in which the woman decides whether to work in the market, *conditional* on not having worked in the market the previous period; it can be thought of as a parameter that captures wage characteristics of a potential new job.

Together, these parameters are characteristic of a labor supply model similar to the implicit contract of Beaudry and DiNardo (1991), augmented with the possibility for acquisition of human capital, both through formal education and learning-by-doing or on-the-job training. It is straightforward that w should be rising in s and μ, and many studies suggest that the marginal return of an additional year of s is greater than that of an additional year of μ; I return to this point in discussing the value function, below. Determining the roles of y_t and y' in the model is more involved. In a spot labor market, w_t should clearly move in the same direction as the current labor demand shock, y_t, and demand shocks from other time periods will have no effect. If there are labor market frictions, however, non-contemporaneous shocks may in fact influence w_t. In Beaudry and DiNardo's model, for instance, long-term wage contracting implies that the labor demand shock at the time a job begins (y') can have enduring effects on w, with a smaller role for y_t. Moreover, as Oreopoulos *et al.* (2005) and Kahn (2006) point out, many types of labor market frictions, not just implicit contracting, can lead to y' having persistent effects. For example, a slowdown in human capital accumulation through lower on-the-job experience could result due to early market non-employment or a bad job match in a tough labor market; either way, the initial shock may have effects on w for several periods. The model presented here remains flexible by allowing both the contemporaneous and initial labor demand shock to influence wages.

Moving to home production, the payoff (or productivity) is assumed to be independent of a woman's choices and instead a function of only her age, $h_t = a_1 age_t + a_2 age_t^2$. This simple quadratic fits the observed aggregate data remarkably well, as shown in Figure 1. Indeed, this shape is representative of typical preferences for family formation during the lifecycle, and may reflect the tradeoff between optimal timing of childbirth physiologically and the psychological and financial resources with which to raise a child, as in Miller (2005).[9]

The payoff to education in period t can be expressed as $e_t = -D - c(ability)$, where D is a constant and $c(\cdot)$ is a decreasing function in ability.[10] High-ability students may have lower psychic costs for additional education, or they may be more likely to earn scholarships that defray the cost of tuition. Either way, education is less costly to higher-ability students, although $c(\cdot)$ is normalized such that e_t is never positive.

There are several points worth mentioning when $t = 0$. First, by construction of the time horizon, $y' = y_0$; the initial labor market shock and the contemporaneous shock are the same. Second, by starting all women at high school graduation, s_0 is identical for all women (they are all exactly high school graduates) and, abstracting away from teenage work experience, $\mu_0 = 0$ for all women.[11] Thus the only systematic source of variation in w_0, the implicit wage offer a woman faces at $t = 0$, is y', the prevailing demand shock to the labor market that period.

2.2 The Value Function

Putting the components together allows for the construction of a discrete-choice value function:

$$V_t = max\{w_t + \beta E[V^w_{t+1}], h_t + \beta E[V^h_{t+1}], e_t + \beta E[V^e_{t+1}]\}. \tag{3}$$

With a few additional functional form assumptions, it is possible to decompose the different expectation functions. Specifically, let the return to a year of education be fixed at γ and the return to a year of experience be fixed at α. Importantly, $\gamma > \alpha$; not only does this assumption reflect empirical findings, but without it no agent would ever choose to pursue more education. Also, since

[9] While it is probable that there are heterogeneous effects in the real world, such that the coefficients should have i subscripts to reflect differences in preferences, for simplicity I stick to the homogeneous case.

[10] The measure of ability I use for the empirical analysis is the Armed Forces Qualifying Test (AFQT), described in the next section.

[11] Surprisingly, I have not been able to identify any recent economic studies on the effects of labor market experience at age 14–16 on labor market outcomes as an adult; perhaps the closest work is Michael and Tuma (1984), which finds suggestive evidence that working at age 14–15 is positively associated with working two years later.

in expectation, $y_{t+1} = \rho y_t$, let the contemporaneous labor demand shock affect the wage draw next period multiplicatively in the form $f(\rho y_t)$. It then follows that:

$$E[V_{t+1}^w] = max\{(1+\alpha)f(\rho y_t)w_t + \beta E[V_{t+2}^w], h_{t+1} + \beta E[V_{t+2}^h], e_t + \beta E[V_{t+2}^e]\}, \tag{4}$$

$$E[V_{t+1}^h] = max\{f(\rho y_t)w_t + \beta E[V_{t+2}^w], h_{t+1} + \beta E[V_{t+2}^h], e_t + \beta E[V_{t+2}^e]\}, \tag{5}$$

$$E[V_{t+1}^e] = max\{(1+\gamma)f(\rho y_t)w_t + \beta E[V_{t+2}^w], h_{t+1} + \beta E[V_{t+2}^h], e_t + \beta E[V_{t+2}^e]\}. \tag{6}$$

Solving this model fully is theoretically possible through dynamic programming techniques using backward induction. Instead, I adopt an alternative strategy that assumes (simplistically) that the time horizon is only two periods. Although in doing so I sacrifice some of the richness in the decision-making process over the lifecycle, I can still identify the impact of a labor demand shock at graduation on labor supply in a comparative static framework.

Maintaining the earlier assumptions, I make one more to define the model I implement empirically: in the second (and final) period, all women choose to work in the market. At first, this may seem like an overpoweringly strong assumption, but I argue it is not that radical. First, it should be clear that the choice of education in the final period is never viable: while it would generate costs, the agent would not be around to reap the benefits (Ben-Porath 1965). Second, by construction, the value of home production is eventually falling; in the long run, market work should have a higher payoff than home production.[12]

The value function can now be written as:

$$V_t = max\{w_t + \beta(1+\alpha)E[w_{t+1}], h_t + \beta(E[w_{t+1}]), e_t + \beta(1+\gamma)E[w_{t+1}]\}. \tag{7}$$

Here, $E[w_{t+1}]$ is the current period's expectation of the following period's base wage draw, unaugmented by additional schooling or work experience. Put differently, it is the expected wage draw next period for a woman who chose home production.[13] In this context, let $w_{t+1} = y_{t+1} + \eta_{t+1}$,

[12] Recall that there is no leisure in the model.

[13] It should be noted that it is also the expected wage draw for a member of the cohort who will make her first decision next period.

where η_{t+1} is a normally-distributed, i.i.d. random variable with mean 0. In the Appendix, I show how this value function leads to an estimation equation for the likelihood of working in period t:

$$P(\eta_t < \pi_0 + \pi_{1t}y' + \pi_2 y_t + \pi_3(a_1)age_t + \pi_4(a_2)age_t^2 + \pi_5 ability), \tag{8}$$

where y_t represents contemporaneous labor demand shocks that are exogenous from the standpoint of the individual, and the reduced form coefficients π_j implicitly include the effects of actual work experience (μ_t) and further schooling (s_t), which are, of course, endogenously chosen. From this equation, it should be evident that an adverse labor demand shock at the time of high school graduation reduces the probability that a woman will work the next period. The next two sections of the paper are concerned with how the initial labor demand shock affects the likelihood of working in the market in subsequent periods as well.

3 Data and Empirical Strategy

3.1 Discussion of Data

Capturing the presence of long-term effects, if any, resulting from labor market conditions at the time of a worker's expected entry is a task that calls for panel data, which explicitly tracks individuals over time. The principal reasons for the use of panel data are twofold. First, it is important to keep fixed the individuals under analysis in order to avoid possible composition bias. Second, the contemporary nature of most repeated cross-sections imply limited availability on any data from the past. The researcher cannot observe the actual timing and location of high school graduation, first labor market entry, real (not potential) work experience, and many other covariates that plausibly affect future labor supply. The likelihood of omitted variable bias would further restrict the chance of viable estimation. Again, the richness of a panel survey can provide these central background variables.

With these factors in mind, I employ the National Longitudinal Survey of Youth. The NLSY79 is an incredibly detailed panel data set that first interviewed 12,686 individuals aged 14 to 22 in 1979 and conducted follow-up interviews annually thereafter until 1994, when subsequent interviews became biennial. There have been a total of 21 rounds with data released; the most recent wave is 2004, when the respondents were between 40 and 47.[14] The NLSY has extensive information on attitudes and expectations, education, family formation, and work histories. I employ the Geocode version of the dataset, which contains state and MSA of residence for each survey year, as well as more detail on college attendance and degree completion. With the Geocode data, it is possible to identify the exact year, month, state, and MSA of high school graduation, and link this information to labor status variables.

Also of particular value is the presence of an ability measure, the Armed Forces Qualifying Test (AFQT), which was administered to nearly every respondent in the NLSY as a means of recalibrating the test for the military. Although no measure of ability is perfect, the AFQT is likely to be a good proxy for the ability variable in the model. Since respondents ranged in age from 15 to 23 at the time of administration, it is necessary to adjust the scores to make them comparable across cohorts. I follow the common method in the literature by age-adjusting and normalizing scores to have mean 0 and standard deviation 1.[15]

For the principal independent variable of interest, the labor market demand shock at the time of high school graduation (y'), I use the annual average unemployment rate as calculated by the Bureau of Labor Statistics (BLS).[16] More precisely, I match both the national *and* state level unemployment rates to each individual in the sample using the year and state of graduation data in the NLSY79. Two different levels of unemployment rates are used because it is not evident *a priori* which represents the more salient labor demand shock; moreover, while the national rate is measured fairly precisely relative to the state rates, the state rates provide potentially much more variation useful for identification. I address these issues more fully in the data appendix. Matching the national rate is possible for every sample individual, but since the state level unemployment rate series begins in 1976, I am currently unable to match a state-level unemployment rate to the

[14] For more information on the NLSY79, see the Bureau of Labor Statistics' web site: `http://www.bls.gov/nls/nlsy79.htm`.

[15] The exact procedure is to regress raw scores on year of birth dummies and divide the residuals from this regression by their sample cohort standard deviations.

[16] There is substantial agreement in the economics literature that during downturns the flow into employment is substantially reduced. As Hall (2005) puts it: "In the modern U.S. economy, recessions do not begin with a burst of layoffs. Unemployment rises because jobs are hard to find, not because an unusual number of people are thrown into unemployment." This statement accords with the unemployment rate serving as a decent proxy for labor demand.

high school class of 1975 (the oldest cohort in the NLSY79). Additionally, the NLSY79 itself has data on the MSA-level annual unemployment rates for its respondents, but these begin only in 1979, and thus provide useful data for only the youngest four cohorts in the sample. Consequently, I do not use the MSA-level rates in the main analysis, but I do employ them as a robustness check.[17]

3.2 Empirical Strategy

To test whether graduating during a recession has a long-lived effect on labor force participation (hysteresis), I organize the data into a panel format and run the following reduced-form equation that follows from the framework in the preceding section:

$$Y_{it} = \beta_0 + \beta_1 UR_{i0} + \beta_2 AFQT_i + \beta_3 exp_{it} + \beta_4 exp_{it}^2 + \gamma' year_t + \epsilon_{it}. \tag{9}$$

Here, $\mathbf{Y_{it}}$ is a measure of labor force attachment. $\mathbf{UR_{i0}}$ is the unemployment rate at high school graduation, $\mathbf{AFQT_i}$ is the age-standardized z-score from the AFQT variable, $\mathbf{exp_{it}}$ measures years elapsed since graduation (i.e., potential experience, a linear transformation of age), and $\mathbf{year_t}$ is a vector of calendar year-of-observation dummies (to control for the y_t in the model). The coefficient of interest, β_1, represents the average, long-term effect of the unemployment rate at time of graduation on the likelihood (or intensity) of labor force participation.

However, if the recession effect is temporary, the above specification will miss it. Thus, I also employ the following estimation equation:

$$Y_{it} = \beta_0 + \delta'[1(UR_{i0} * exp_{it})] + \beta_2 AFQT_i + \beta_3 exp_{it} + \beta_4 exp_{it}^2 + \gamma' year_t + u_{it}. \tag{10}$$

The additional term $[\mathbf{1}(\mathbf{UR_{i0}} * \mathbf{exp_{it}})]$ replaces $\mathbf{UR_{i0}}$ and consists of a vector of dummies of UR_{i0} interacted with each possible value of exp_{it}. The corresponding vector of coefficients, δ', gives the impact of the high school graduation unemployment rate on labor force participation *at each year after graduation.* The second equation can thus shed light on the time-specific magnitude of labor

[17] An effort to extend backwards the time series for state and MSA-level unemployment rates using published County and City Databooks and other sources is in progress, but is complicated by methodological differences over time. Additionally, I am also constructing alternative measures of labor demand shocks, such as payroll job growth and Bartik-style (industry-composition-adjusted) indicators.

supply effects, as well as their duration.

Both equations are estimated using the national-level unemployment rates and, separately, the state-level rates. As the labor demand shock in the latter group is economically motivated by both variation within a state over time and variation across states holding time fixed, the models with state-level unemployment rates also include state and year of graduation fixed effects. Table 1 presents summary statistics of the variables mentioned, both for the whole sample and broken down by unemployment rate quantile groups.[18]

3.3 Panel Aspects and Standard Errors

Ideally, the researcher would be able to exploit the panel nature of the NLSY79 by allowing for individual-level random or fixed effects that probably influence the decision to work. In the context of the model, for example, there likely is unobserved heterogeneity in say, motivation, that affects the wage draw. A fixed effects estimator could control for this unobserved heterogeneity. Unfortunately, since the measure of the initial labor demand shock (the unemployment rate) is time-invariant and individual specific, a fixed effects estimator would run into a multicollinearity problem.

Yet, under the assumption that the unobserved heterogeneity parameter distribution does not vary across cohorts, the consistency of the estimates should not be affected. This may not be a particularly strong assumption to make for the population, but it might be for the particular sample used in the analysis. Thus, as a rough check on the validity of this assumption, let us operate under the seemingly weaker assumption that ability, as measured by the AFQT, is highly positively correlated with the unobserved heterogeneity parameter that affects the likelihood of labor force participation. Appendix Table 4 presents sample means and standard errors of the normalized AFQT measure by high school graduation cohort. Two things bear mentioning. First, most of the scores are significantly positive, at around 0.2 standard deviations above 0. This is commensurate with the fact that the sample is conditioned on those who graduated from high

[18]For the national unemployment rate groups, *low* includes the graduating classes of 1978 and 1979, *medium* includes the classes of 1976, 1977, 1980, and 1981, and *high* includes the classes of 1975, 1982, and 1983. For the state unemployment rate groups, the state unemployment rate is adjusted for state fixed effects and then grouped into three approximately equal-sized categories.

school (full diploma) and on time (age 17, 18 or 19); this group is expected to be slightly above average. Second, most of the year means are not significantly different from one another; the two exceptions are 1976 and 1983. The difference between 1976 and most of the other years is small, about 0.1 standard deviations, and economically this minor difference should not be that important. The 1983 mean, on the other hand, is a clear outlier with a mean of 0.5 standard deviations below 0. There is an explanation for this low value, however. As the youngest cohort in the NLSY79 turned 15 during the calendar year 1979, the 1983 high school graduation cohort consists exclusively of students who turned 19 that year. This group may disproportionately comprise students who were held back a year, individuals who are presumably of lower than average ability. Because this finding calls into question the comparability of the 1983 cohort with the others, the subsequent regressions were also run omitting this cohort; results were not appreciably affected. On the whole, then, it appears that the cohorts are reasonably similar in ability, and by proxy, other unobserved heterogeneity that may affect the decision to participate in the labor force.

Still, because there may remain some unobserved heterogeneity, it is highly likely that the error terms for any given individual are correlated across time; further, this correlation may exist among individuals who graduated in the same year or same state and year. Following the suggestion of Bertrand, Duflo, and Mullainathan (2004) and Moulton (1986), the estimation procedure allows for arbitrary correlation of the errors within a high school graduation year cohort (for the national rate regressions) or within a year-state of graduation cohort (for the state-rate regressions).[19] Some additional identification issues are discussed in the data appendix.

[19] As the asymptotic properties of the resulting covariance matrix estimator rely on the number of clusters going to infinity, the reported standard errors for the national rate regressions, which are based on only nine clusters, may be problematic. Two potential robustness checks are (a) clustering at the individual level and (b) employing multi-level block bootstrap variance estimation. Using the first approach does not substantively alter the estimates. The second approach has been suggested by Cameron, Gelbach, and Miller (2008) and will be implemented shortly.

4 Results

4.1 Labor Supply

The results from probit estimation of equation (9) appear in Table 2 as mean marginal effects. That is, in this model, the coefficient on the high school graduation unemployment rate can be interpreted as the average effect on the woman's propensity to participate in the labor market, net of the other control variables, over the entire time horizon (15 to 23 years following graduation). Columns (1) and (2) use the national unemployment rate, while the state-level unemployment rate specifications are in columns (3) and (4). The odd columns correspond to the specification outlined in Section 3, while the even columns add family background covariates. The unemployment rate coefficient is negative for the national level specifications, while it is positive in the state level specifications. In none of the specifications, however, is it statistically significantly different from 0. If it were, the inference would be that initial labor market conditions had a *permanent* effect on the propensity to participate in the labor force, a severe form of hysteresis indeed. Thus, it is important to check whether there might be temporary, yet still somewhat persistent effects of the graduation unemployment rate on labor force participation that would not be detected in the above analysis. In order to explore this scenario, Table 3 presents the results from estimating equation (10), which allows the initial unemployment rate to have a different impact on labor force participation for each year subsequent to graduation.

Before moving on to these key results, however, it is worth noting the effects of the other explanatory variables in the context of the model. The coefficient on ability, as proxied by AFQT, is significantly positive: across the specifications, a one-standard-deviation increase in ability increases the probability of being in the labor force by 3 to 4 percentage points. This effect, while somewhat small in economic terms,[20] is consistent with the predications of the model: In the long run, the more able women obtain more education, have higher wage offers, and are more likely to work.[21]

[20]As a stylized example, holding other covariates fixed, a woman at the 98^{th} percentile of the ability distribution has an expected probability of being in the labor force only 6–8 percentage points higher than a woman at the median.

[21]In an unreported regression, I examine the effect of AFQT score on highest grade completed at 10 years after high school graduation (about the mean experience level over the sample period). A one-standard-deviation increase in AFQT is associated with 1.21 more grades completed, with a standard error of 0.03.

Similarly, the coefficient on potential experience is significant and positive, while its square is significant and of the opposite sign. While these signs are the opposite of what would be expected in the simple two-period model, they are logical in a multiple period framework. Specifically, in the long run, potential experience in the reduced-form equation also captures actual work experience, μ_t. As μ_t is endogenous in the model, it is not added as a covariate, but since nearly all women in the sample eventually work, it seems plausible that the potential experience coefficient is capturing the returns to work experience and is thus positive.

Returning to the main coefficients of interest, Table 3 reports the marginal effects of the high school graduate unemployment rate on the probability of being in the labor force, broken down by year since graduation.[22] In the national rate specifications in columns (1) and (2), net effects are large in magnitude and significantly negative for the first four years after graduation. In the first year, the average woman is 2 percentage points less likely to be in the labor force per percentage point rise in the unemployment rate. Thus, a woman who graduated in a severe recession like that of 1982, in which the unemployment rate rose 3 percentage points above its long-term average, would be 6 percentage points (or about 8.5 percent) less likely to be a labor force participant one year after graduation. Following this sharp drop in the first year out, the net effect begins to diminish, but persists another three years. Full recovery is reached five years out, and there appears to be no significant effect, either negative or positive, after this point. Indeed, the results of a test that the interaction between the initial unemployment rate and time since graduation is linear, as might be the case if recovery is gradual, are reported at the bottom of Table 3. Under the null of linearity, the differences between adjacent year interaction terms should all be the same. With all p-values < 0.001, linearity is clearly rejected across specifications; after the initial shock, recovery is relatively quick. Figure 2 plots out the predicted marginal effects from column (1).

In contrast with the national rate specifications, the state-level specifications in columns (3) and (4) indicate no significant effect whatsoever of the unemployment rate on labor force participation, and show substantially smaller point estimates. Why the marked difference between the models? The answer is not clear, but there are a few possibilities.

[22] Although the underlying regression used all available experience years, Table 3 reports effects only up to 15 years after graduation; no effects beyond 15 years are significant.

With a full set of state of graduation and year of graduation dummies, the state-level specifications are especially demanding of the data. There are a total of 408 possible state-year-graduation cells (51 times 8) to which about 2,000 individuals can belong. Of course, the individuals are not distributed uniformly across states or even years, and the actual number of non-empty cells is 267. Furthermore, many of these non-empty cells have few individuals in them, as some states are sparsely represented in the sample. With such small cell sample sizes, achieving reliable identification of the state unemployment rate effect, separate from the state fixed effect, may be tenuous. If this identification problem is present, it is not clear how to resolve it. Two potential workarounds—omitting from the analysis states with few individual graduates or combining states into groups to increase cell sample size—both have shortcomings: the first gets rid of valuable data and the second is rather *ad hoc*, with no guidance as to how to combine states or re-weight variables.[23]

Another possible explanation is that the national unemployment rate is more salient to decision-making than is the state unemployment rate. This might be because the national rate is more widely known, a better bellwether of labor demand, or, perhaps most plausibly, simply better measured. To explore this possibility, I checked the coefficients on the year of graduation dummies in the state level regressions (these are not reported in Table 3). If it is really the state-level unemployment rate that matters, then these coefficients should not have a systematic and significant pattern. Yet, I find that these coefficients do have such a pattern: there are negative marginal effects in the years with higher national unemployment rates, and these effects are generally greater in magnitude the higher is the unemployment rate.[24]

The third and somewhat unpleasant possibility is that national rate coefficients are inconsistent or that the standard errors are incorrect or both.[25] While the national rate results are robust to different assumptions about the distribution of the error term (that is, a logit model and a linear

[23] I attempted the first approach, with no change in results. Kahn (2006) employs the second approach in her sample; while she identifies the state groups, she does not mention how the variables are re-weighted. Another alternative, which I also attempted, was to run an OLS regression using state-year-experience cells with the number of individuals in each cell serving as analytic weights. Results from this regression produced a negative, marginally significant coefficient one year after graduation, but no significant net effects for years two through four.

[24] As with all unreported statistics, these figures are available from the author upon request.

[25] A probit model implicitly assumes homoskedasticity in calculating coefficient estimates; not adjusting for the clustering can bias the estimate. Additionally, as discussed in Section 3, asymptotic inference with few clusters is strained.

probability model produce very similar results), it is worth checking the robustness of the estimates more broadly.

Therefore, Table 4 presents results using alternate measures of labor force participation. While the metric used in Tables 2 and 3 was a simple binary variable indicating participation at the time of the survey, the measures in Table 4 are continuous and better capture participation over a whole year, not just a point in time.[26] All the specifications in Table 4 include family background fixed effects. Beginning again with the national level specifications, the first column has as the dependent variable the number of weeks spent in the labor force (for the calendar year preceding that of the survey). The pattern of the coefficients is quite similar to that of Table 3: a strong negative coefficient at one year after graduation that fades away over the next three to four years. The same story is true in column (3), which looks at the effect on annual hours worked. Statistical significance, however, is now slightly weaker. The net effect at up to three years after graduation is significant at the 5 percent level for weeks in the labor force and annual hours worked, but a test of the impact at four years out fails to reject the null of zero for either measure. Yet, mildly weaker effects should be expected in this specification. Both weeks in the labor force and annual hours worked are likely measured with more error than is the binary participation decision. As a result, standard errors become larger and the power of the regression falls. The finding of a similar pattern in the coefficients and statistical significance is reassuring and consistent with the earlier results.

Turning to the state-level specifications, the pattern of an initial negative effect followed by a recovery lasting three to four years is now evident. Although effects are not statistically significant for weeks in the labor force and significant only one year out for annual hours worked, the evidence is still quite suggestive, given the likelihood of measurement error. Broadly speaking, the national-level results in Table 3 do not appear to be a statistical fluke. As a further robustness check, columns (5) through (7) in Table 4 report results for binary participation, weeks in the labor force, and annual hours worked using the *metropolitan area* unemployment rate at the time of high school graduation. This measure of labor demand provides the greatest variation but stresses the data the

[26]Since the dependent variable is (approximately) continuous, these models are estimated with OLS rather than probit.

hardest: the measure is available for only the 1979 through 1983 graduation year cohorts,[27] and the specifications are run with *state* (not MSA) fixed effects, although standard errors are clustered at the MSA-grad year level. The marginal effects are more variable across the dependent variable specifications than are the national coefficients, but they are statistically significant at 1 percent for the first four years in columns (6) and (7), and continue to evince the pattern shown in the other specifications.[28]

4.2 College Enrollment

Because the negative labor supply effects shown in Tables 3 and 4 appear to last around four years, the typical length of study for a baccalaureate degree, it might be that the initial labor demand shock is inducing women into college rather than into home production. Table 5 demonstrates that this is not the case. The specifications shown in this table are identical to those in Table 3 with the exception that the dependent variable is now a binary variable for whether the individual is currently enrolled in college. Examining the first four rows of coefficients, there is no positive net effect of the unemployment rate on enrollment. If anything, women are *less* likely, not more likely, to immediately enroll as a result of a high unemployment rate at the time of high school graduation.

Moreover, across all four specifications, there is a small but generally statistically significant net negative effect on enrollment rates five through seven years after graduation. While identifying the cause(s) of this phenomenon is beyond the scope of this paper, I will speculate on one possibility. Perhaps college-going for women in the sample is relatively inelastic to labor market conditions at high school graduation, but going to graduate school is not. This might be the case, for example, if credit constraints are more binding during times of weaker labor demand: an individual who has amassed debt to pay for college may be reluctant to accumulate more by immediately entering graduate school, and instead might prefer to work or at least put off further schooling. Some support for this hypothesis can be found by looking at the rows for the effects 12 or more years

[27] I am working on extending this series back using City and County Databooks.

[28] Curiously, columns (5) through (7) suggest that labor supply effects due to graduating in a recession may be quite persistent, unlike the national and state estimates. However, the magnitude of the effects in later years is considerably smaller than in the early years.

after graduation: net effects are now significant *and positive* in columns(1) and (2).[29] Although entirely suggestive, this evidence is consistent with women who graduated high school in a recession delaying graduate school (or additional college) until after they have worked, begun a family, or both. Such a scenario is worthy of future research, as it has interesting implications for the expected return to schooling, the discount rate, and related research on the timing of schooling.[30]

In any case, the main takeaway of Table 5 is that women who graduate high school in a recession are not differentially induced into college enrollment relative to women who graduate during more favorable labor demand conditions. As expected and predicted by the model, though, the probability of enrollment is sharply rising in AFQT score (ability), as can be seen near the bottom of the table. Finally, and also consistent with the model, the probability of enrollment declines with time elapsed since graduation, reflecting the diminished horizon over which to enjoy higher market earnings.

4.3 Home Production

Since the results of Tables 3 through 5 together suggest that a high unemployment rate at high school graduation induces at least some women into home production, it is worth exploring the channels through which this home production might operate. In addition to family formation, as described earlier, these channels include such activities as home maintenance, cooking, laundry, shopping, and bill paying, among others. However, as the NLSY79 is devoid of time-use and expenditure data, it is not possible to test empirically whether these latter specific activities are affected by the unemployment rate at high school graduation. Thus, Figures 3 and 4 and Table 6 instead present results relating the initial unemployment rate to the timing of first marriage and the timing of first child, both important indicators of family formation, and plausibly an important part of home production.[31]

[29] Although Table 5 truncates experience years beyond 15, these later interaction year effects are also positive.

[30] This scenario, for instance, would run counter to the findings of Bedard and Herman (2008), in which the enrollment of women in graduate school was not sensitive to the unemployment rate at the time of their college graduation.

[31] Dehejia and Lleras-Muney (2004), for example, show that unemployment rates and pregnancy rates of less educated women are negatively correlated—especially for white women—a finding which they attribute to less-skilled women substituting from market work into fertility when the wage offer is low.

Figures 3 and 4 are Kaplan-Meier graphs showing the fraction of respondents who have not yet been married or had a child, respectively, by time elapsed (in months) since high school graduation. Each figure has two graphs; and in turn, each graph has two time series. Within each graph, the two time series divide individuals into groups based on whether their high school graduation unemployment rate was above or below the median in the sample. The first graph is based on the national-level unemployment rate, while the second graph in each figure uses the state-level unemployment rate (adjusted for state-level fixed effects).

These figures thus present non-parametric estimates of the distribution of time to first marriage (first child) by unemployment rate groups. If women who graduate high school in a recession are more likely to engage in home production immediately thereafter, and this home production takes the form of family formation as specified, then we would expect these women to have distributions that on average lie below and to the left of the distributions of women who graduate during better economic conditions. Examining all four graphs, this generally seems to be the case, with the stronger evidence coming from the national rate graphs. Of course, these graphs are merely suggestive, and more analytical, testable estimates appear in Table 6.

Since the two dependent variables of interest here are months elapsed until first marriage and months elapsed until first child, each of the six specifications in Table 6 is based on one observation per individual. (The specifications in Tables 3 through 5 contained multiple observations per individual). For each dependent variable, three models were estimated: a hazard model with a Weibull distribution (accelerated failure time metric) using the national-level unemployment rate as the regressor of interest (columns 1 and 4); the same but using the state-level unemployment rate as the primary regressor (columns 2 and 5); and a simple OLS model using the national-level unemployment rate (columns 3 and 6). All models also include as a regressor the normalized AFQT score.[32] For the Weibull specifications, the dependent variable is implicitly in logs; thus the estimate coefficient can be interpreted as a percent change per unit change in the regressor. The OLS specifications represent unit-unit changes. All specifications include family background fixed

[32] Omitting the AFQT score from the estimation model does not appreciably affect the coefficient estimate on the unemployment rate; as Table 1 indicates, AFQT score and high school graduation unemployment rate are basically uncorrelated.

effects.

With the exception of column (2), the coefficient estimate on the high school graduation unemployment rate is negative, as expected, and statistically significant in columns (1) and (3). It should be noted, however, that these estimates are derived from relatively few observations and the power of these tests is not large. Although I cannot confirm statistically that the increase in home production takes the explicit form of earlier marriage and earlier childbirth, the evidence is certainly in the right direction.[33]

4.4 Wages

In order to check whether initial labor market conditions affect labor market outcomes principally through labor supply in my sample of high school graduate women, Table 7 presents results from regressions similar to those in Table 3, but with log hourly real wages rather than labor force participation as the dependent variable.[34] The purpose of this exercise is twofold. First, it is another test of the prediction of the model that when initial labor market conditions lower the wage offer, women at the margin (near their reservation market wage) will choose not to work in the market. Further, among the women who do continue to work, the model predicts that the unemployment rate should have no appreciable effect on wages. To see this, recall that $w_{it} = y_t + \eta_{it}$ in the initial time period, with η_{it} representing (normally distributed) white noise, and that all women have the same reservation wage in the initial time period conditional on ability.[35] When y_t is low, as in a recession, the women who continue to choose to work, holding ability constant, are necessarily those with high draws of η. Thus, as long as ability is orthogonal to the unemployment rate, the unemployment rate should not affect the wages of workers, *conditional on them choosing to work*; the lower y_t and higher η_{it} basically cancel each other out.

[33] As a further test, I also conducted an analysis employing U.S. Vital Statistics administrative data on birth records. Unfortunately, with state-year-level birth rates among high school graduate 18 to 20-year-old women as the dependent variable, a specification with state-specific linear time trends produced a point estimate on the unemployment rate (lagged one year) that was not significantly different from zero.

[34] Recall that the participation decision from the model in Section 2 was based on implicit wage offers; the empirical difficulty in using wages explicitly is that they are not observed for non-workers. In the analysis, I use log hourly real wages (CPI-U-RS-deflated, year 1977 dollars) only for person-years with a valid wage observation; no attempt was made to impute wages for non-workers or workers not reporting a wage.

[35] More realistically, some specifications include family background characteristics that may also influence the reservation wage, thus further "whitening" η_{it}.

The second reason for this test is to serve as a contrast with the results of studies mentioned in the introduction. Examining groups highly attached to the labor force (generally, college graduate men), they found persistent, negative wage effects but no labor supply effects of initial labor market conditions. Because the underlying labor force participation decisions among their samples are likely very different from those in mine, one would not expect their results necessarily to generalize to different groups.

Table 7 confirms the predictions of the model and shows wage results quite different than those found among other samples. Aside from two isolated blips, there are no statistically significant effects of the high school graduation unemployment rate on log wages. Although the point estimates for the first four years after graduation are negative across the four specifications, they are small in magnitude, between one-sixth and one-half of what Kahn (2006) finds in her sample of NLSY79 college graduate white men, for example.

4.5 Welfare

The negative effects on labor supply found above also suggest the possibility of an adverse welfare impact. While a more thorough investigation of the full welfare consequences of graduating in a recession would be a separate study in its own right, I examine a few poverty-related indicators in Table 8 as a cursory inspection. The same specifications as before are run with three different dependent variables: poverty status (whether the respondent is a family unit that falls below the poverty threshold), the logarithm of real family income, and a binary variable for receipt of government assistance (any of AFDC, food stamps, or SSI).

Perhaps surprisingly, graduating high school during a recession seems to have no short-term impact on the likelihood of being in poverty for the average American female. In both the national and state-level regressions (columns (1) and (2)), the coefficients are relatively small and statistically insignificant for at least the first eight years after graduation. However, after this period, when women are in their late 20s to early 30s, the picture changes: the national-level regressions indicate a statistically significant and economically meaningful increase in the poverty rate. A dozen years after graduation, a woman who graduated when the national unemployment rate was 3 percentage points higher has a 2.1 percentage point, or over 20 percent, greater chance of being in poverty. Although the coefficients from the state-level regression do not reach statistical significance, their magnitudes, too, are larger in this time period than soon after graduation.[36]

It is not clear why there appears to be a delayed inducement into poverty. One possibility is that recent high school graduates are still receiving transfers (either cash or in-kind) from their parents or other relatives that can cushion against the negative shock. If these transfers diminish as the graduate enters her late 20s, and welfare effects to graduating in a recession are persistent, then the pattern shown in columns (1) and (2) of Table 8 are rationalizable. Some support for this conjecture can be found by restricting the estimation to the NLSY79 low-income and minority sample, as found in Appendix Table 6. Family resources among graduates in this group are likely to be substantially more limited than in the nationally representative sample, making additional family transfers to graduates unlikely. Indeed, the results show strong poverty inducement effects for the first three years after graduation that then gradually fade away, consistent with reduced transfers.[37]

To further investigate this issue, columns (5) and (6) look at an inclusive measure of government assistance: receipt of AFDC, food stamps, *or* SSI. For the first four years after graduation, there are no positive, significant effects of the high school graduation unemployment rate on government assistance and, in fact, the point estimates for the national-level regressions are *negative.* The state-level coefficients show weakly statistically significant positive effects from five to eight years out and, interestingly, fade just as the poverty coefficients become positive and statistically significant. Although hardly conclusive, this is suggestive of graduates first receiving parental or family transfers, then government transfers when family transfers run out or become insufficient, and then exhibiting higher poverty rates when government transfers run out or become insufficient. Indeed, contrasting the results in Table 8 with those in Appendix Table 6 shows that the latter estimates imply a much quicker uptake in government assistance, as would be the case if family transfers were not available.

[36] Although not shown in Table 8, effects farther out after graduation are *not* statistically significant; while standard errors rise, the point estimates drop substantially in magnitude.

[37] These results also create another puzzle as to why the effects appear persistent for the nationally representative sample but not the low-income and minority sample. I leave this question to future research.

Poverty status and government assistance, however, are relatively narrow measures of welfare. Columns (3) and (4) of Table 8 present marginal effects on the broader measure of the logarithm of real family income. The point estimates in both the national and state-level regressions are negative and large from two to seven years after graduation. The national estimates imply that even five years after graduation in a bad recession, a woman's family income is up to 10 percent lower than that of her more fortunate counterpart.[38] The effects, however, do seem to fade after eight years' time. (The pattern for the low-income sample is similar.)

As an additional check, the specifications in Table 8 were also all run at the MSA level, with the resulting coefficients showing a large and statistically significant positive impact of the high school graduation unemployment rate on government assistance for several years after graduation. The results of poverty and log real family income regressions resembled the national pattern and, if anything, were stronger.

5 Discussion and Conclusion

This paper set out to test for the existence and extent of negative labor supply effects on women graduating high school during a recession. In contrast with previous related literature that found negative wage effects but minimal labor supply effects among samples of highly educated men, the model and empirical evidence presented here support the converse. There is suggestive, and in some cases, statistically significant evidence that women who graduate high school during a recession are less likely to work for up to four years afterward. Further, the women affected at the margin are *not* substituting into education and seem instead to be more likely to engage in home production. While only suggestive (the signs are right), the shift into home production seems to be associated with earlier family formation, as measured by time to first marriage and first child. It would appear that early labor demand shocks for this sample of high school graduate women, historically not considered to be a group with the strongest attachment to the labor force, act through participation and not wages, as they do in other studies of primarily highly educated men.

[38] Not directly explored is the family structure behind family income, but recall the results on age of first marriage and first childbirth from Table 6.

It should be emphasized, however, that these conclusions are preliminary, and further work remains to be done. For example, are the above results robust to using alternative measures of labor demand shocks, such as payroll job growth and Bartik-style labor demand indicators? There is no perfect measure for labor demand, and each of the above metrics as well as the unemployment rate is partially commingled with labor supply in general equilibrium. By using a variety of indicators, however, it may be possible to partially disentangle these push and pull factors and obtain a more thorough test of the model's predictions.

Moreover, would things change if one studied college-educated women? If the greater level of education translated into higher potential lifetime earnings, would their behavior more closely resemble that of men from the other studies? Theoretically, the answer is ambiguous. It is quite possible that the simple model presented here does not capture the effects of education on the value (productivity) of home production. Indeed, Ge (2006), also using the NLSY79, finds that a significant determinant of college-going is the expected return to marriage, beyond labor market returns. Figuring out the relative importance of the labor market returns vis-à-vis the home production returns is an empirical question worthy of future research.

Future work should also explore whether these findings vary across generations. In a few years, it will be possible to use the NLSY97, a similar panel study for cohorts born between 1980 and 1984, to conduct an analysis analogous to that done here. With these additional data, it will be possible to gauge how responsiveness to initial labor market conditions for women has evolved recently, and in so doing obtain a clearer picture of at least one facet of female labor supply.

Finally, and perhaps most important, is the future work needed to place the results found here into a broader context. While there do not appear to be permanent effects to labor supply, or even strong effects on family formation, this does not imply that there are no long-term effects on affected women's welfare. Indeed, the rather rudimentary analysis on poverty status, family income, and government assistance receipt suggests that there may be some persistent, negative welfare effects at the lower end of the socioeconomic distribution. Although it is beyond the scope of the current study, I expect subsequent research to explore how the temporary reductions in the likelihood of working found here translate more thoroughly into lifetime income,[39] health, the selection into and duration of marriage, the quantity and quality investments in children,[40] and many other dimensions of social interest.

[39]See especially Jacobsen and Levin (1995).
[40]See especially Hotz and Miller (1988).

Appendix

Derivation of the estimation equation from the simple value function

In Section 2, I described a relatively simple value function:

$$V_t = max\{w_t + \beta(1+\alpha)E[w_{t+1}], h_t + \beta E[w_{t+1}], e_t + \beta(1+\gamma)E[w_{t+1}]\}. \quad (11)$$

Applying some algebra, and adjusting time subscripts to begin at $t = 0$:

$$V_0 = max\{w_0 + \alpha E[w_1], h_0, e_0 + \gamma E[w_1]\}. \quad (12)$$

The woman will choose to work in the first period if:

$$w_0 + \alpha E[w_1] > h_0 and w_0 + \alpha[w_1] > e_0 + \gamma E[w_1]. \quad (13)$$

Since $w_t = y_t + \eta_t$ and $y_t = \rho y_{t-1} + \epsilon_t$, it follows that $w_0 = y' + \eta_0$ and $E[w_1] = E[y_1 + \eta_1] = E[\rho y' + \epsilon_0 + \eta_1] = \rho y\prime$. Substituting in:

$$y' + \eta_0 + \alpha\rho y' > h_0 and y' + \eta_0 + \alpha\rho y' > e_0 + \gamma\rho y'. \quad (14)$$

Rearranging and summing the inequalities:

$$2y' + 2\alpha\rho y' - \gamma\rho y' - h_0 - e_0 > -2\eta_0. \quad (15)$$

Dividing by two and rearranging:

$$\eta_0 > -(1 + \alpha\rho - \frac{1}{2}\gamma\rho)y' + \frac{1}{2}h_0 + \frac{1}{2}e_0. \quad (16)$$

Thus, the probability that the woman works in the first period is:

$$P(\eta_0 > -(1 + \alpha\rho - \frac{1}{2}\gamma\rho)y' + \frac{1}{2}h_0 + \frac{1}{2}e_0). \quad (17)$$

Exploiting the normality of η:

$$P(\eta_0 > -(1 + \alpha\rho - \frac{1}{2}\gamma\rho)y' + \frac{1}{2}h_0 + \frac{1}{2}e_0) = P(\eta_0 < (1 + \alpha\rho - \frac{1}{2}\gamma\rho)y' - \frac{1}{2}h_0 - \frac{1}{2}e_0). \tag{18}$$

Denote $\pi \equiv 1 + \alpha\rho - \frac{1}{2}\gamma\rho$. Substituting in for the definitions of h and e imply:

$$P(\eta_0 < \pi y' - \frac{1}{2}(a_1 age_0 + a_2 age_0^2) - \frac{1}{2}(-D - c(ability)). \tag{19}$$

Finally, let $c(ability) = K - ability$, for some positive constant K. Then:

$$P(\eta_0 < \pi y' - \frac{1}{2}a_1 age_0 - \frac{1}{2}a_2 age_0^2 - \frac{1}{2}ability + \frac{1}{2}(D + K)). \tag{20}$$

This, of course, is a conventional probit model. The actual empirical estimation is based from this model, with some modifications discussed in Section III. In particular, the above two-period framework is generalized to hold for two *blocks* of time. While everyone works in the second block, as in the two-period case, the first block consists of several periods in which the agent can choose whether to work. Making this step leads to the reduced-form equation:

$$P(\eta_t < \pi_0 + \pi_1 y' + \pi_2 y_t + \pi_3(a_1)age_t + \pi_4(a_2)age_t^2 + \pi_5(ability)), \tag{21}$$

where y_t represent contemporaneous labor demand shocks that are exogenous from the standpoint of the individual, and the reduced form coefficients π_j implicitly include the effects of actual work experience (μ_t) and further schooling (s_t), which are, of course, endogenously chosen.

Data Appendix

Although the supplemented NLSY79 is rich in the variables needed for empirically testing the implications of the model in Section 2, it is not without a few shortcomings. One of these, common to almost every panel dataset, is the relatively small sample size. A total of 12,686 individuals were surveyed, but because the focus of the analysis is on the labor supply choices of high school graduate women, the actual empirical sample is considerably smaller. Specifically, I restrict the sample to women who graduated high school on time (at age 17, 18, or 19), had neither been married nor had a child at the time of graduation, and have valid AFQT scores. Also, a recurring problem with panel data is respondent attrition. As both small sample size and attrition may affect the internal validity of the estimates, I detail below my strategies to deal with these issues. Appendix Tables 1 and 2 provide details about how conditioning the sample affects the sample size, both overall and by graduation year cohort.

On the topic of external validity, it should be emphasized that the NLSY79 is cohort-specific to the latter half of the Baby Boomer generation. This presents two concerns. First, one must be careful in extrapolating any inference made using these data to other cohorts. As Goldin (2006) has emphasized, the expectations, educational attainment, and career profiles of women born during the 20^{th} century changed quite rapidly from generation to generation. Thus, for example, it is quite possible that the generation of women who faced labor market entry during the early 1990s or early 2000s recessions had (have) different responses than those women who faced the early 1980s recessions.[41]

Second, the younger Baby Boomers are an unusually large cohort. After peaking at around 120 births per 1000 women aged 15 to 44 in 1957, the fertility rate began a steady decline that lasted until the mid 1970s, reaching a low of around 65. Nonetheless, the Baby Boom is generally dated as lasting through 1964 because the birth rate, though falling, remained historically high (around 105 in 1964, approximately the level in 1949, three years after the start of the Baby Boom).[42] The sheer size of the cohort may have important implications on schooling and labor market decisions.

[41] I hope to examine whether this is in fact the case in future work, drawing upon the recent NLSY97 of cohorts born between 1980 and 1984, and whose (post-college) labor market entry bracketed the recession in the early 2000s.

[42] These figures are from U.S. Vital Statistics: `http://www.cdc.gov/nchs/products/pubs/pubd/vsus/vsus.htm`

Falaris and Peters (1992) show that the size of both past and future cohorts (that is, the timing of birth relative to whether the birth rate is rising or falling) affects both the amount of education an individual receives and the age at which one completes formal schooling. Specifically, cohorts born during the upswing of the cycle tend both to get more education and take longer per additional year to get it than do cohorts born during the cycle downswing; cohorts born at peaks or troughs fall in between. Thus, in the NLSY79, we might expect to see slightly less education and earlier labor market entry for the younger cohorts. However, Falaris and Peters find that the cyclical effects for women, while statistically significant, are quite small relative to those for men. The authors hypothesize that the gender difference may be due to women's smaller total labor supply and thus weaker incentives to obtain more school in order to mitigate the negative wage effects of excess supply. This explanation can nest with business cycle effects on women's labor supply, but it suggests caution in disentangling the demographic cycle from the business cycle. As shown below, controlling for cohort effects is possible in the state-level rate analysis but not the national rate analysis; if Falaris and Peters are right, omitted variable bias from missing cohort effects in the national rate analysis should be no more than a trivial concern. Nonetheless, this issue is also discussed below.

Cohort Size Effects

A potential shortcoming of the national rate regressions is the inability to control for cohort size. By restricting the sample to women who graduated high school at more or less the same age, any indicator for cohort size would be almost perfectly collinear with the national unemployment rate in the equation. However, since cohort size is falling in time in the NLSY79 sample, the results of Welch (1979) suggest that the younger cohorts should be faced with higher wage offers (and, hence, incentives to participate in the labor market), *ceteris paribus*, than the older cohorts. But it is the younger cohorts in the sample who experienced the highest unemployment rates upon graduation: 9.7 for the 1982 grads and 9.6 for the 1983 grads. Thus, to the extent that cohort effects are present, we would expect the bias to go *against* finding negative unemployment rate effects. While this line of reasoning suggests the national rate regressions should not be biased, especially given the argument of Falaris and Peters (1992) discussed above, the state-level unemployment rate regressions have the advantage of being able to control explicitly for cohort size effects by including a vector of graduation year dummies.

Measurement Error

Of perennial concern is the possibility of measurement error in the data.[43] Specifically, if any of the above regressors are noisily measured, then consistency in the estimates of β is threatened. Fortunately, there is little reason to believe this is the case in the current application. The measurement of ability, the AFQT score, comes directly from administrative records and is merged into the main data file. Also of particular importance are the precise measurements of the variables related to high school graduation: the respondent's age at graduation, the year (and month) of graduation, and the state of graduation. Although these data are reported by the respondent, the relevant questions are asked in multiple survey years. Respondent-reported inconsistencies were subsequently cleaned (using other relevant collected data) by CHRR, the agency charged with maintaining the NLSY79. Additionally, several consistency checks were performed by the current author, and remaining discrepancies were found to be few.

Endogeneity

Central to consistent estimation of β_1 and δ' is the assumption that the high school graduation unemployment rate is exogenous to other unobserved factors that influence the labor force participation decision. In their analysis of college graduates, both Kahn (2006) and Oreopoulos *et al.* (2005) are careful to account for possible endogeneity in the timing and location of college graduation with prevailing economic conditions. This might be the case if a college student postpones graduation, or transfers to another school in response to labor market conditions. Consequently, both papers include specifications that instrument for the timing and location of graduation.[44] Looking at high school graduates, rather than college graduates, however, likely avoids this poten-

[43] For an excellent review of how measurement error can plague the econometrician, see Bound, Brown, and Mathiowetz (2001).

[44] For instruments, Kahn uses year of birth and state of residence, and Oreopolous *et al.* use date of college entry.

tial endogeneity problem. It seems quite implausible that the timing of high school graduation is affected by economic conditions, especially since more education (college) is readily available as an alternative. Therefore, the subsequent analysis treats the unemployment rates as exogenous and an exclusionary instrument is not used.

However, it is still possible that the unemployment rate could be correlated with other *observable* factors that influence labor force participation, and thus bias the unemployment rate coefficients. In particular, the possibility of parental transfers in reaction to the unemployment rate is worrisome. To guard against this possibility, some specifications also include a set of pre-graduation family background measures.[45]

Sample Attrition

As with any survey-based panel data set, respondent attrition is present in the NLSY79, and Appendix Table 1 shows that nearly one-third of the viable sample has attrited by the last survey year of 2004. Not shown in the table, however, is that most of the attrition occurs relatively late in the sample (mid 1990s and afterward). Thus, if much of the effect of the initial labor demand shock is concentrated relatively soon after graduation, as the model suggests, estimation should not be significantly plagued by sample attrition. Nevertheless, to address the potential problem, each estimation equation was run on two samples, one using all available person-years and the other using only individuals who were interviewed every survey year. The resulting sets of estimates were not appreciably different; sample attrition does not seem to be a major problem.

[45]This vector includes mother's and father's education, indicators for race, a dummy for whether the mother was working when the respondent was age 14, a dummy for whether the respondent was born in the South, and a dummy for whether the respondent grew up speaking a foreign language at home.

Works Cited

Beaudry, Paul, and John DiNardo. 1991. "The Effect of Implicit Contracts on the Movement of Wages Over the Business Cycle: Evidence from Microdata." *Journal of Political Economy* 99(4) August: 665-688.

Becker, Gary S. 1991. *A Treatise on the Family.* Cambridge: Harvard University Press.

Bedard, Kelly, and Douglas A. Herman. 2008. "Who Goes to Graduate/Professional School? The Importance of Economic Fluctuations, Undergraduate Field, and Ability." *Economics of Education Review.* 27(2): 197-210.

Ben-Porath, Yoram. 1967. "The Production of Human capital and the Life Cycle of Earnings." *Journal of Political Economy* 75(4): 352-365.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How Much Should We Trust Differences-in-Differences Estimates?" *Quarterly Journal of Economics* 119(1) February: 249-275.

Blau, Francine D., and Lawrence M. Kahn. 2005. "Changes in the Labor Supply Behavior of Married Women: 1980-2000." NBER Working Paper 11230.

Blundell, Richard, and Thomas MaCurdy. 1999. "Labor Supply: A Review of Alternative Approaches." *Handbook of Labor Economics*, Volume 3, Part 1. O. Ashenfelter and D. Card, eds. Amsterdam: Elsevier Science Publishing. 1559-1695.

Bound, John, Charles Brown, and Nancy Methiowetz. 2001. "Measurement Error in Survey Data." *Handbook of Econometrics*, Volume 5, Part 13. J. Heckman and E. Learner, eds. Amsterdam: Elsevier Science Publishing. 3705-3843.

Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller. 2008. "Bootstrap-Based Improvements for Inference with Clustered Errors." *Review of Economics and Statistics* 90(3): 414-427.

Dehejia, Rajeev, and Adriana Lleras-Muney. 2004. "Booms, Busts, and Babies' Health." *Quarterly Journal of Economics* 119(3): 1091-1130.

Falaris, Evangelos M., and H. Elizabeth Peters. 1992. "Schooling Choices and Demographic Cycles." *Journal of Human Resources* 27(4) Autumn: 551-574.

Ge, Suqin. 2006. "Women's College Choice: How Much Does Marriage Matter?" Virginia Tech mimeo.

Goldin, Claudia. 2006. "The Quiet Revolution that Transformed Women's Employment, Education, and Family." Richard T. Ely Lecture. *American Economic Review* 96(2) May (Papers and Proceedings): 1-21.

Gronau, Reuben. 1986. "Home Production-A Survey." *Handbook of Labor Economics*, Volume 1. O. Ashenfelter and R. Layard, eds. Amsterdam: Elsevier Science Publishers. 273-304.

Guryan, Jonathan, Erik Hurst, and Melissa Kearney. 2008. "Parental Education and Parental Time with Children." *Journal of Economic Perspectives* 22(3) Summer: 23-46.

Hall, Robert E. 2005. "Employment Efficiency and Sticky Wages: Evidence from Flows in the Labor Market." *Review of Economics and Statistics.* August 87(3): 397-407.

Heckman, James J., and Thomas E. MaCurdy. 1980. "A Life Cycle Model of Female Labour Supply." *Review of Economic Studies* 47(1) January: 47-74.

Heckman, James J., and Robert J. Willis. 1977. "A Beta-logistic Model for the Analysis of Sequential Labor Force Participation by Married Women." *Journal of Political Economy* 85(1) February: 27-58.

Hotz, V. Joseph, and Robert A. Miller. 1988. "An Empirical Analysis of Life Cycle Fertility and Female Labor Supply." *Econometrica* 561(1) January: 91-118.

Jacobsen, Joyce P., and Lawrence M. Levin. 1995. "Effects of Intermittent Labor Force Attachment on Women's Earnings." *Monthly Labor Review* 118(9) September: 14-19.

Kahn, Lisa B. 2006. "The Long-Term Labor Market Consequences of Graduating from College in a Bad Economy." Harvard University mimeo.

Michael, Robert T., and Nancy Brandon Tuma. 1984. "Youth Employment: Does Life Begin at 16?" *Journal of Labor Economics* 2(4): 464-476.

Miller, Amalia R. 2005. "The Effects of Motherhood Timing on Career Path." University of Virginia mimeo.

Moulton, Brent R. 1986. "Random Group Effects and the Precision of Regression Estimates." *Journal of Econometrics* 32: 385-397.

National Vital Statistics Reports, Centers for Disease Control. 2006. “Births: Final Data for 2004.” 55(1): September 29.

Oreopoulos, Phil, Till von Wachter, and Andrew Heisz. 2005. “The Short- and Long-Term Career Effects of Graduating in a Recession: Hysteresis and Hetereogeneity in the Market for College Graduates.” Columbia University mimeo.

Oyer, Paul. 2006. “Initial Labor Market Conditions and Long-Term Outcomes for Economists.” *Journal of Economic Perspectives* 20(3) Summer: 143-160.

Oyer, Paul. 2007. “The Making of an Investment Banker: Stock Market Shocks, Career Choice, and Lifetime Income.” Stanford GSB mimeo.

Welch, Finis. 1979. “The Effect of Cohort Size on Earnings: The Baby Boom Babies’ Financial Bust.” *Journal of Political Economy* 87(5) Part 2: Education and Income Distribution: S65-S97.

Table 1: Sample Means of Selected Variables by Unemployment Rate Group
(standard errors in parentheses)

			National			*State*	
UR Group	All	Low	Medium	High	Low	Medium	High
Age-standardized AFQT	0.200	0.195	0.205	0.196	0.215	0.191	0.169
	(0.017)	(0.033)	(0.023)	(0.037)	(0.030)	(0.030)	(0.033)
Years since HS graduation	13.356	13.248	13.394	13.406	13.349	13.280	12.572
	(0.0288)	(0.010)**	(0.035)	(0.102)	(0.023)	(0.042)**	(0.054)**
National unemployment rate	7.406	5.946	7.361	9.235			
	(0.026)	(0.006)**	(0.008)**	(0.026)**	–	–	–
State unemployment rate	7.515	5.972	7.599	10.178	6.067	6.827	9.705
	(0.049)	(0.045)**	(0.052)**	(0.143)**	(0.041)**	(0.048)**	(0.082)**
Currently in labor force	0.775	0.798	0.776	0.744	0.786	0.795	0.739
	(0.010)	(0.002)+	(0.012)	(0.022)*	(0.008)	(0.007)**	(0.010)**
Annual weeks in labor force	39.789	40.484	39.568	39.417	39.929	40.671	38.548
	(0.422)	(0.184)	(0.705)	(0.781)	(0.410)	(0.392)**	(0.509)*
Annual hours worked	1389.8	1425.7	1373.2	1382.2	1399.0	1424.6	1324.7
	(25.74)	(1.640)	(44.62)	(41.14)	(22.85)	(21.18)**	(27.09)*
Currently enrolled	0.130	0.142	0.130	0.119	0.133	0.135	0.130
	(0.005)	(0.001)	(0.007)	(0.013)	(0.007)	(0.007)	(0.007)
Log wages	1.404	1.420	1.398	1.398	1.405	1.406	1.377
	(0.017)	(0.005)	(0.027)	(0.045)	(0.014)	(0.016)	(0.022)
Months from graduation to 1st marriage	62.892	63.323	63.499	60.949	63.907	63.107	60.576
	(1.159)	(1.195)	(1.697)	(2.954)	(2.133)	(2.109)	(2.335)
Months from graduation to 1st child	83.544	84.746	84.070	80.933	85.359	83.381	79.174
	(2.335)	(0.936)	(3.559)	(6.463)	(2.601)	(2.979)	(2.578)+
Observations	59,940	16,627	30,024	13,689	18,387	17,982	18,144

+ significant at 10%, * significant at 5%, ** significant at 1%
(1) Adjusted for state fixed effects
(2) (exp) n = 55,398 due to younger cohorts
(3) (lfp) n = 37,820 (not asked after 1998)
(4) (wkslf) n = 40,893
(5) (hrswk) n = 37,797
(6) (enr) n = 43,313
(7) (logw) n = 31,069
(8) (1st marr) n = 1,717
(9) (1st chid) n = 1,460

Significance: Low column shows significance relative to medium, medium column shows significance relative to high, high column shows significance relative to low.

Standard errors for exogenous variables (AFQT, years since graduation, both unemployment rates) are clustered at the individual level. Standard errors for endogenous variables (the remainder) are clustered at high school graduation year (national) or graduation year–state (state).

Table 2: Probit Regressions on Average Labor Force Participation
(standard errors in parentheses)

	(1)	(2)	(3)	(4)
Geographic UR level	*National*	*National*	*State*	*State*
Mean dep variable	0.794	0.798	0.795	0.798
HS Grad Unemployment Rate	-0.004	-0.002	0.001	0.001
	(0.004)	(0.003)	(0.004)	(0.005)
Normalized AFQT	0.030	0.044	0.029	0.044
	(0.007)**	(0.007)**	(0.007)**	(0.008)**
Potential Experience	0.012	0.012	0.017	0.016
	(0.002)**	(0.003)**	(0.004)**	(0.004)**
Potential Experience squared	-0.001	-0.001	-0.001	-0.001
	(0.0001)**	(0.0001)**	(0.0002)**	(0.0002)**
Family background FE	No	Yes	No	Yes
McFadden's R-squared	0.007	0.011	0.019	0.023
Observations	34,784	30,001	31,407	27,129

+ significant at 10%, * significant at 5%, ** significant at 1%

(1) Reported are mean marginal effects
(2) All regressions also include year of observation dummies, and state level regressions include state and graduation year fixed effects
(3) Standard errors are clustered at high school graduation year (national) or year-state (state)
(4) Restricting the sample to non-attriters does not appreciably affect the results

Table 3: Probit Regressions on Labor Force Participation by Experience Year
(standard errors in parentheses)

	(1)	(2)	(3)	(4)
Geographic UR level	*National*	*National*	*State*	*State*
Mean dep variable	0.794	0.798	0.795	0.798
HS Grad UR, 1 year after	-0.019	-0.021	-0.003	-0.007
	(0.003)**	(0.003)**	(0.005)	(0.006)
2 years after	-0.013	-0.014	0.000	-0.003
	(0.002)**	(0.002)**	(0.005)	(0.005)
3 years after	-0.011	-0.010	0.001	0.000
	(0.003)**	(0.002)**	(0.004)	(0.005)
4 years after	-0.009	-0.008	0.000	-0.002
	(0.004)*	(0.003)**	(0.004)	(0.005)
5 years after	0.000	0.002	0.007	0.006
	(0.004)	(0.003)	(0.004)	(0.005)
6 years after	0.000	0.003	0.006	0.006
	(0.005)	(0.003)	(0.004)	(0.005)
7 years after	0.002	0.003	0.007	0.006
	(0.005)	(0.004)	(0.004)	(0.005)
8 years after	0.001	0.003	0.005	0.005
	(0.006)	(0.005)	(0.004)	(0.005)
9 years after	-0.002	0.000	0.002	0.002
	(0.007)	(0.005)	(0.005)	(0.005)
10 years after	-0.002	0.001	0.002	0.002
	(0.007)	(0.005)	(0.005)	(0.006)
11 years after	-0.003	-0.002	0.001	0.000
	(0.006)	(0.005)	(0.005)	(0.006)
12 years after	-0.004	-0.002	0.000	-0.001
	(0.007)	(0.005)	(0.005)	(0.006)
13 years after	-0.005	-0.001	0.000	0.000
	(0.006)	(0.005)	(0.005)	(0.006)
14 years after	-0.005	-0.002	-0.001	-0.001
	(0.005)	(0.004)	(0.005)	(0.006)
15 years after	-0.006	-0.002	-0.001	-0.001
	(0.005)	(0.004)	(0.005)	(0.006)
Normalized AFQT	0.030	0.044	0.029	0.044
	(0.007)**	(0.007)**	(0.007)**	(0.008)**
Potential Experience	-0.016	-0.020	0.008	0.003
	(0.014)	(0.011)+	(0.011)	(0.012)
Potential Experience squared	0.001	0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Family background FE	No	Yes	No	Yes
F-test of linear interaction (p-value	0.000	0.000	0.000	0.000
McFadden's R-squared	0.010	0.014	0.021	0.026
Observations	34,784	30,001	31,407	27,129

+ significant at 10%, * significant at 5%, ** significant at 1%

(1) Reported are mean marginal effects
(2) All regressions include year of observation dummies, and state level regressions include state and graduation year fixed effects
(3) Standard errors are clustered at high school graduation year (national) or year-state (state)
(4) The F-test is for the null that the time interactions effects follow a linear trend (constant difference between adjacent years)

Table 4: Regressions for Alternative LFP Measures by Experience Year
(standard errors in parentheses)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable	*Weeks in Labor Force*	*Weeks in Labor Force*	*Ann. Hours Worked*	*Ann. Hours Worked*	*LFP*	*Weeks in Labor Force*	*Ann. Hours Worked*
Geographic UR level	*National*	*State*	*National*	*State*	*Metro*	*Metro*	*Metro*
Mean dep variable	40.8	40.9	1,465	1,467	0.800	40.8	1,476
HS Grad UR, 1 year after	-0.83	-0.41	-48.9	-28.6	-0.007	-0.77	-44.5
	(0.129)**	(0.275)	(10.4)**	(13.5)*	(0.004)+	(0.173)**	(9.6)**
2 years after	-0.54	-0.19	-35.0	-18.1	-0.004	-0.51	-36.1
	(0.156)**	(0.257)	(10.7)*	(12.9)	(0.004)	(0.161)**	(8.7)**
3 years after	-0.45	-0.21	-26.6	-13.9	-0.004	-0.56	-35.0
	(0.194)*	(0.260)	(10.9)*	(12.9)	(0.003)	(0.151)**	(7.9)**
4 years after	-0.23	-0.06	-14.9	-4.9	-0.008	-0.49	-27.3
	(0.199)	(0.249)	(11.9)	(12.6)	(0.004)*	(0.161)**	(7.7)**
5 years after	-0.02	0.12	3.8	12.0	-0.003	-0.39	-14.3
	(0.208)	(0.252)	(12.5)	(12.7)	(0.004)	(0.181)*	(8.4)+
6 years after	0.07	0.16	12.0	16.2	-0.005	-0.35	-10.3
	(0.216)	(0.250)	(11.8)	(12.5)	(0.004)	(0.181)+	(8.2)
7 years after	0.03	0.10	6.5	9.3	-0.007	-0.40	-16.1
	(0.247)	(0.255)	(12.8)	(12.7)	(0.004)	(0.181)*	(8.3)+
8 years after	0.02	0.04	4.1	6.1	-0.006	-0.33	-12.3
	(0.293)	(0.260)	(14.7)	(12.9)	(0.004)	(0.187)+	(8.6)
9 years after	-0.09	-0.06	0.4	3.1	-0.009	-0.42	-11.8
	(0.310)	(0.278)	(16.3)	(13.5)	(0.005)+	(0.211)*	(9.2)
10 years after	-0.10	-0.07	-2.3	0.6	-0.009	-0.44	-11.9
	(0.323)	(0.285)	(16.5)	(13.9)	(0.005)+	(0.213)*	(9.3)
11 years after	-0.14	-0.12	-5.0	-3.0	-0.010	-0.44	-11.9
	(0.327)	(0.292)	(17.5)	(14.0)	(0.005)*	(0.217)*	(9.4)
12 years after	-0.21	-0.18	-10.6	-8.1	-0.008	-0.49	-14.6
	(0.315)	(0.289)	(16.3)	(14.5)	(0.005)+	(0.217)*	(9.9)
13 years after	-0.14	-0.14	-11.3	-11.4	-0.007	-0.34	-15.6
	(0.327)	(0.288)	(16.9)	(14.4)	(0.004)	(0.218)	(9.4)+
14 years after	-0.17	-0.16	-14.1	-13.0	-0.008	-0.41	-15.5
	(0.323)	(0.291)	(16.4)	(14.6)	(0.004)+	(0.225)+	(10.2)
15 years after	-0.16	-0.14	-14.5	13.7	-0.004	-0.29	-16.5
	(0.326)	(0.283)	(15.6)	(14.2)	(0.004)	(0.230)	(9.1)+
R-squared	0.020	0.037	0.046	0.066	0.033	0.044	0.081
Family background FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	32,721	29,534	33,073	29,850	14,925	16,170	16,306

+ significant at 10%, * significant at 5%, ** significant at 1%

(1) All columns represent OLS regressions, except for column (5), which is a probit (mean marginal effects reported)

(2) Regressions include all RHS variables as specified in the notes to Table 3

(3) Standard errors are clustered at high school graduation year (national), or year-state (state), or year-MSA (metro)

(4) Regressions are based on all available observations

Table 5: Probit Regressions on College Enrollment by Experience Year
(standard errors in parentheses)

	(1)	(2)	(3)	(4)
Geographic UR level	*National*	*National*	*State*	*State*
Mean dep variable	0.140	0.142	0.143	0.146
HS Grad UR, 1 year after	-0.003	-0.005	0.000	0.002
	(0.004)	(0.004)	(0.004)	(0.004)
2 years after	-0.001	-0.004	0.001	0.003
	(0.002)	(0.003)	(0.004)	(0.004)
3 years after	0.000	-0.004	0.001	0.002
	(0.002)	(0.002)	(0.003)	(0.003)
4 years after	0.001	-0.002	0.002	0.004
	(0.002)	(0.002)	(0.003)	(0.003)
5 years after	-0.006	-0.010	-0.006	-0.005
	(0.001)**	(0.002)**	(0.003)+	(0.003)
6 years after	-0.007	-0.011	-0.008	-0.007
	(0.002)**	(0.002)**	(0.003)*	(0.003)*
7 years after	-0.005	-0.009	-0.006	-0.005
	(0.002)*	(0.002)**	(0.003)+	(0.003)
8 years after	-0.002	-0.007	-0.005	-0.004
	(0.002)	(0.002)**	(0.003)	(0.003)
9 years after	0.003	-0.002	0.000	0.000
	(0.002)	(0.002)	(0.003)	(0.003)
10 years after	0.002	-0.002	-0.002	-0.001
	(0.003)	(0.002)	(0.003)	(0.003)
11 years after	0.005	0.000	0.000	0.000
	(0.003)	(0.003)	(0.003)	(0.003)
12 years after	0.007	0.003	0.001	0.001
	(0.004)*	(0.003)	(0.003)	(0.003)
13 years after	0.010	0.006	0.002	0.002
	(0.003)**	(0.003)+	(0.003)	(0.003)
14 years after	0.012	0.008	0.002	0.002
	(0.003)**	(0.003)*	(0.004)	(0.004)
15 years after	0.015	0.011	0.004	0.004
	(0.003)**	(0.003)**	(0.004)	(0.004)
Normalized AFQT	0.078	0.077	0.080	0.078
	(0.004)**	(0.004)**	(0.005)**	(0.005)**
Potential Experience	-0.043	-0.039	-0.038	-0.034
	(0.010)**	(0.009)**	(0.007)**	(0.007)**
Potential Experience squared	0.001	0.001	0.001	0.001
	(0.0004)**	(0.0003)*	(0.0003)**	(0.0003)**
Family background FE	No	Yes	No	Yes
McFadden's R-squared	0.171	0.195	0.191	0.213
Observations	40,277	34,756	36,405	31,489

+ significant at 10%, * significant at 5%, ** significant at 1%

(1) Reported are mean marginal effects
(2) All regressions include year of observation dummies, and state level regressions include state and graduation year fixed effects
(3) Standard errors are clustered at high school graduation year (national) or year-state (state)

Table 6: Regressions for Months Elapsed until First Marriage and First Child
(standard errors in parentheses)

	(1)	(2)	(3)	(4)	(5)	(6)
Dep Variable	*Marriage*	*Marriage*	*Marriage*	*Child*	*Child*	*Child*
Model	*Weibull AFT*	*Weibull AFT*	*OLS*	*Weibull AFT*	*Weibull AFT*	*OLS*
Geographic UR level	*National*	*State*	*National*	*National*	*State*	*National*
Mean dep variable	64.7	64.4	64.2	87.4	86.7	89.0
HS Grad Unemployment Rate	-0.025	0.002	-1.049	-0.013	-0.002	-1.849
	(0.011)*	(0.017)	(0.471)*	(0.017)	(0.012)	(0.880)+
Normalized AFQT	0.093	0.089	6.469	0.117	0.125	11.491
	(0.026)**	(0.033)**	(1.642)**	(0.015)**	(0.025)**	(2.274)**
Constant	3.852	3.518	21.942	4.188	3.799	43.071
	(0.184)**	(0.326)**	(10.616)+	(0.184)**	(0.192)**	(15.557)*
Family background FE	Yes	Yes	Yes	Yes	Yes	Yes
Log Pseudo-Likelihood	-1796.2	-1589.5	–	-1233.0	-1062.7	–
R-squared	–	–	0.09	–	–	0.14
Observations	1443	1307	1033	1223	1099	892

+ significant at 10%, * significant at 5%, ** significant at 1%

(1) State level regressions also include state and year fixed effects
(2) Standard errors are clustered at high school graduation year (national) or year-state (state)
(3) OLS regressions are on a sample of never attriters.

Table 7: Regressions on Log Hourly (Real) Wages by Experience Year
(standard errors in parentheses)

	(1)	(2)	(3)	(4)
Geographic UR level	*National*	*National*	*State*	*State*
Mean dep variable	1.425	1.429	1.419	1.425
HS Grad UR, 1 year after	-0.012	-0.008	-0.008	-0.004
	(0.009)	(0.010)	(0.006)	(0.007)
2 years after	-0.014	-0.012	-0.010	-0.007
	(0.007)	(0.008)	(0.006)+	(0.006)
3 years after	-0.012	-0.010	-0.009	-0.005
	(0.006)+	(0.007)	(0.006)	(0.006)
4 years after	-0.010	-0.010	-0.007	-0.003
	(0.006)	(0.006)	(0.006)	(0.006)
5 years after	-0.005	-0.005	-0.001	0.002
	(0.006)	(0.006)	(0.006)	(0.006)
6 years after	-0.001	-0.002	0.003	0.007
	(0.005)	(0.006)	(0.006)	(0.006)
7 years after	-0.001	-0.002	0.003	0.006
	(0.006)	(0.006)	(0.006)	(0.006)
8 years after	-0.002	-0.003	0.004	0.008
	(0.005)	(0.006)	(0.006)	(0.007)
9 years after	-0.004	-0.006	0.003	0.007
	(0.005)	(0.006)	(0.006)	(0.007)
10 years after	-0.003	-0.005	0.004	0.008
	(0.006)	(0.007)	(0.007)	(0.007)
11 years after	-0.005	-0.007	0.003	0.007
	(0.006)	(0.007)	(0.007)	(0.007)
12 years after	-0.004	-0.006	0.003	0.007
	(0.006)	(0.007)	(0.007)	(0.007)
13 years after	-0.006	-0.009	0.003	0.008
	(0.007)	(0.008)	(0.007)	(0.008)
14 years after	-0.005	-0.009	0.003	0.007
	(0.007)	(0.008)	(0.007)	(0.008)
15 years after	-0.009	-0.012	0.002	0.006
	(0.008)	(0.009)	(0.008)	(0.008)
Normalized AFQT	0.197	0.193	0.194	0.191
	(0.010)**	(0.015)**	(0.009)**	(0.012)**
Potential Experience	0.061	0.066	0.067	0.068
	(0.017)**	(0.019)**	(0.012)**	(0.013)**
Potential Experience squared	-0.002	-0.002	-0.002	-0.002
	(0.0005)*	(0.001)*	(0.000)**	(0.000)**
Family background FE	No	Yes	No	Yes
R-squared	0.255	0.270	0.293	0.304
Observations	29,970	26,001	27,096	23,560

+ significant at 10%, * significant at 5%, ** significant at 1%

(1) All columns represent OLS regressions, with same RHS variables as specified in the notes to Table 3
(2) Regressions are based on observations that report a valid wage; no attempt to correct for selection was made
(3) Standard errors are clustered at high school graduation year (national), or year-state (state)

Table 8: Regressions on Welfare Measures by Experience Year
(standard errors in parentheses)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	*Poverty Status*	*Poverty Status*	*Log Real Family Income*	*Log Real Family Income*	*Govt assistance*	*Govt assistance*
Geographic UR level	*National*	*State*	*National*	*State*	*National*	*State*
Mean dep variable	0.095	0.097	9.648	9.644	0.067	0.067
HS Grad UR, 1 year after	-0.002	-0.003	-0.016	0.007	-0.005	0.002
	(0.004)	(0.003)	(0.017)	(0.013)	(0.005)	(0.003)
2 years after	0.002	0.000	-0.030	-0.011	-0.003	0.004
	(0.004)	(0.003)	(0.016)+	(0.012)	(0.005)	(0.003)
3 years after	0.003	0.001	-0.037	-0.018	-0.003	0.004
	(0.004)	(0.003)	(0.014)*	(0.011)	(0.005)	(0.003)
4 years after	0.005	0.002	-0.037	-0.020	-0.003	0.004
	(0.004)	(0.003)	(0.013)*	(0.010)+	(0.005)	(0.003)
5 years after	0.005	0.001	-0.031	-0.016	-0.001	0.004
	(0.003)	(0.003)	(0.012)*	(0.011)	(0.005)	(0.003)+
6 years after	0.003	-0.001	-0.022	-0.011	-0.001	0.005
	(0.003)	(0.003)	(0.010)+	(0.011)	(0.004)	(0.003)+
7 years after	0.004	0.001	-0.022	-0.014	-0.001	0.005
	(0.003)	(0.003)	(0.010)+	(0.010)	(0.004)	(0.003)+
8 years after	0.004	0.001	-0.013	-0.007	0.000	0.005
	(0.003)	(0.003)	(0.010)	(0.010)	(0.004)	(0.003)+
9 years after	0.007	0.003	-0.010	-0.005	0.000	0.004
	(0.003)*	(0.003)	(0.011)	(0.010)	(0.004)	(0.003)
10 years after	0.005	0.001	-0.007	-0.005	0.000	0.003
	(0.003)	(0.003)	(0.011)	(0.010)	(0.004)	(0.003)
11 years after	0.008	0.004	-0.005	-0.006	-0.001	0.002
	(0.003)**	(0.003)	(0.009)	(0.011)	(0.004)	(0.003)
12 years after	0.007	0.003	-0.007	-0.007	0.000	0.002
	(0.003)*	(0.003)	(0.010)	(0.011)	(0.004)	(0.003)
13 years after	0.007	0.005	-0.003	-0.008		
	(0.004)+	(0.003)	(0.012)	(0.011)		
14 years after	0.007	0.005	-0.008	-0.008		
	(0.003)*	(0.003)	(0.011)	(0.012)		
15 years after	0.006	0.003	0.000	-0.003		
	(0.003)+	(0.003)	(0.011)	(0.011)		
(McFadden's) R-squared	0.089	0.108	0.133	0.156	0.143	0.187
Family background FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	29,923	26,856	28,462	25,628	28,805	25,597

+ significant at 10%, * significant at 5%, ** significant at 1%

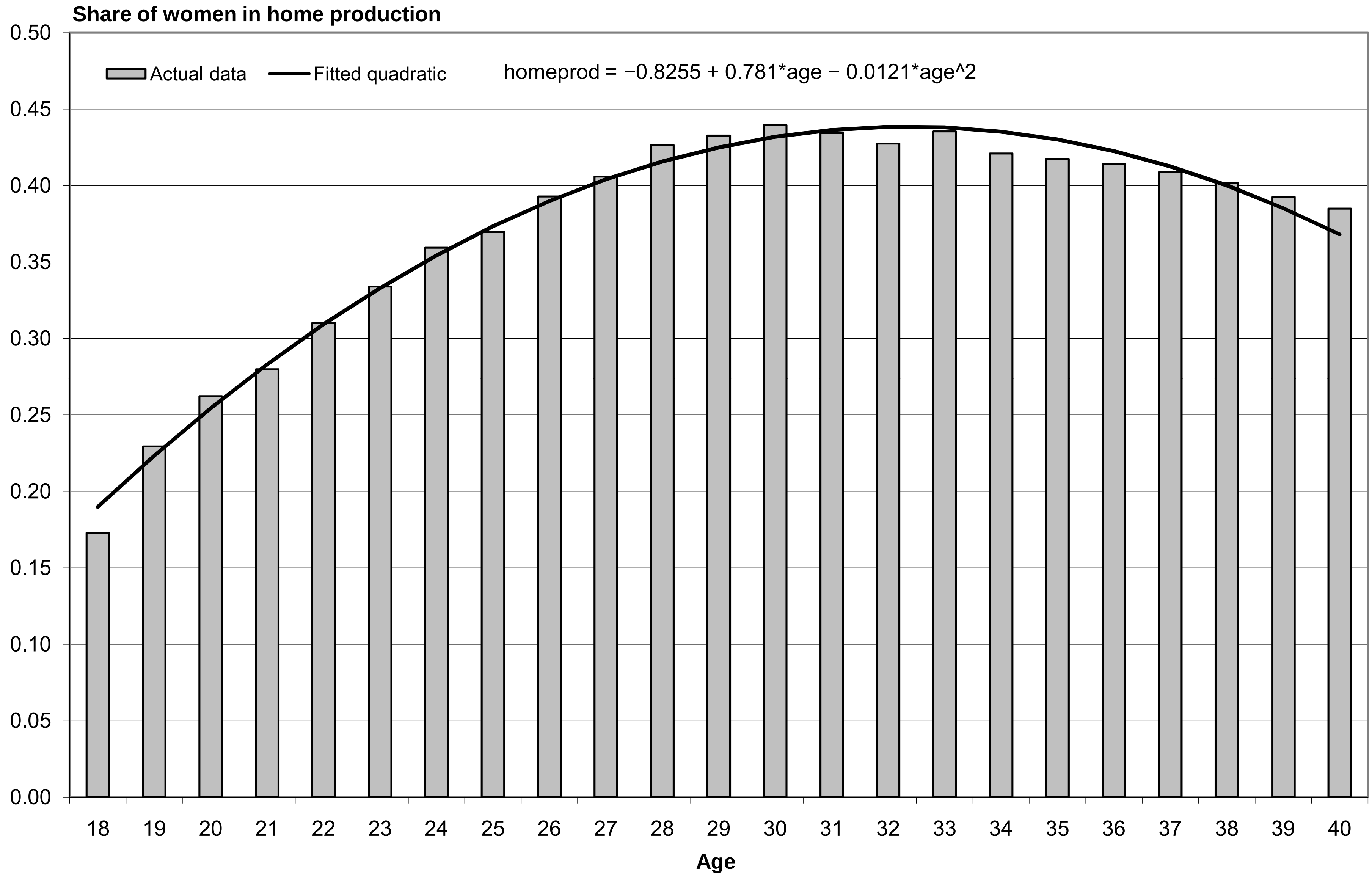
(1) Reported are mean marginal effects

(2) Regressions include all RHS variables as specified in the notes to Table 3

(3) Standard errors are clustered at high school graduation year (national), or year-state (state)

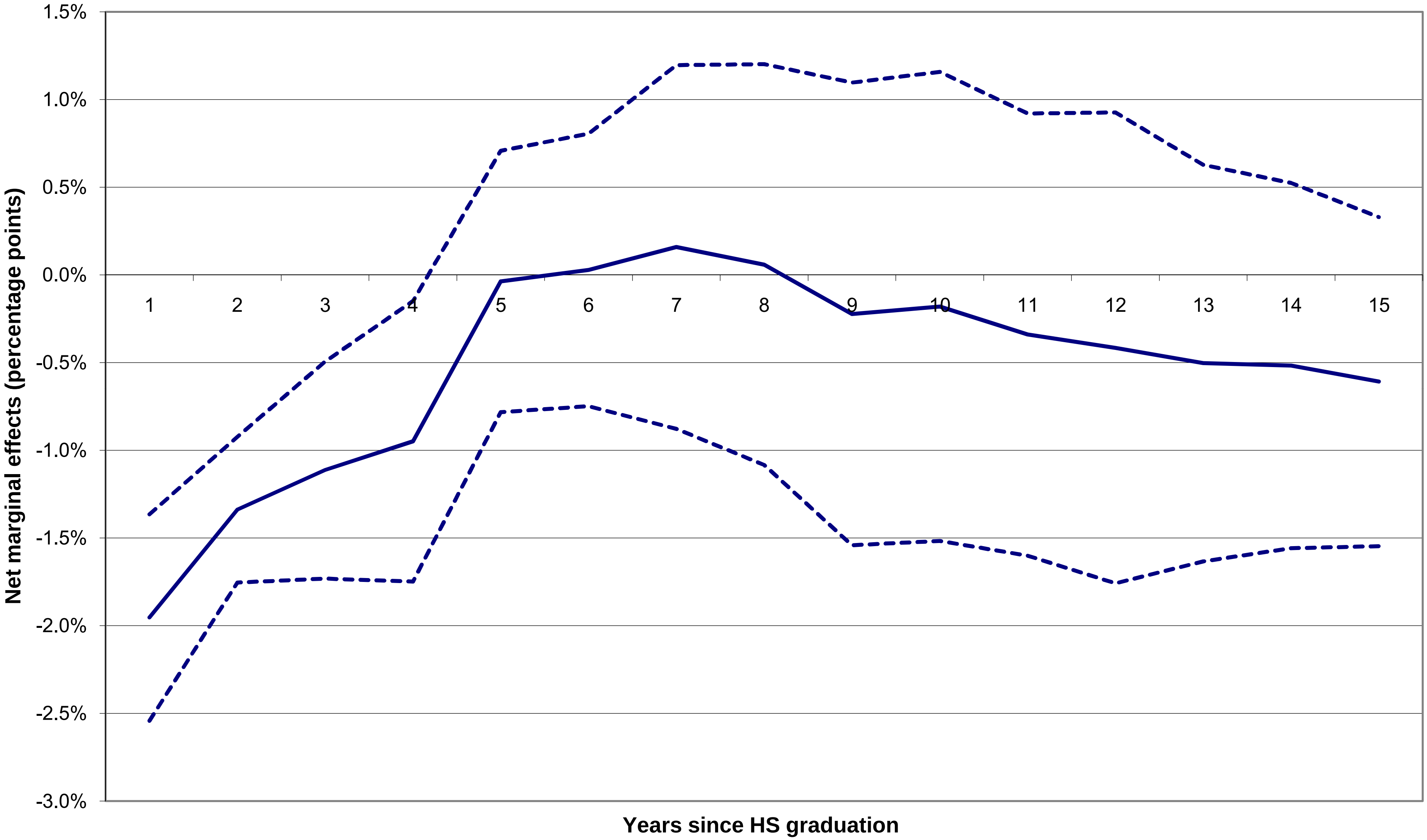
(4) Government assistance includes any of (a) AFDC, (b) food stamps, or (c) SSI, and is measured only through 1995 to avoid conflating effects of welfare reform

Figure 1
Fraction of women engaging in home production by age, March CPS, 1970 through 1990



Source: Author's calculations using IPUMS March CPS data, years 1970 through 1990
Note: A respondent is coded as engaging in home production if she is not in the labor force, not retired or diabled, and not enrolled in school.

Figure 2: Net marginal effects of HS graduation unemployment rate on the probability of being in the labor force, by years since graduation



Notes: Data are taken from column 1 of Table 2 and represent the net marginal effect (in percentage points), evaluated at covariate means, on the likelihood of being a labor force participant *per percentage point increase in the national unemployment rate at the time of high school graduation.* Dashed lines represent the 95 percent confidence interval.

Figure 3: Time to First Marriage Kaplan-Meier Graphs

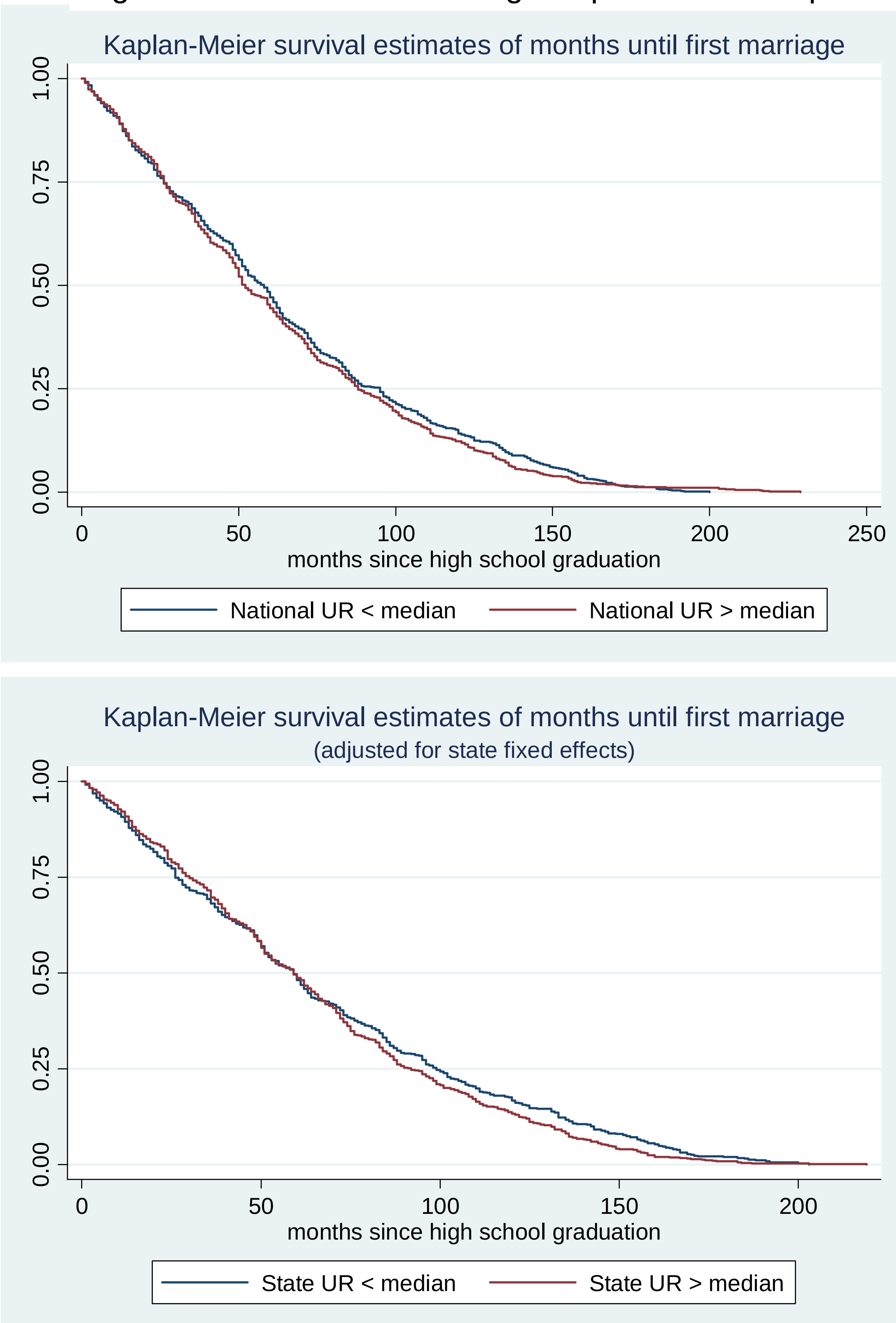
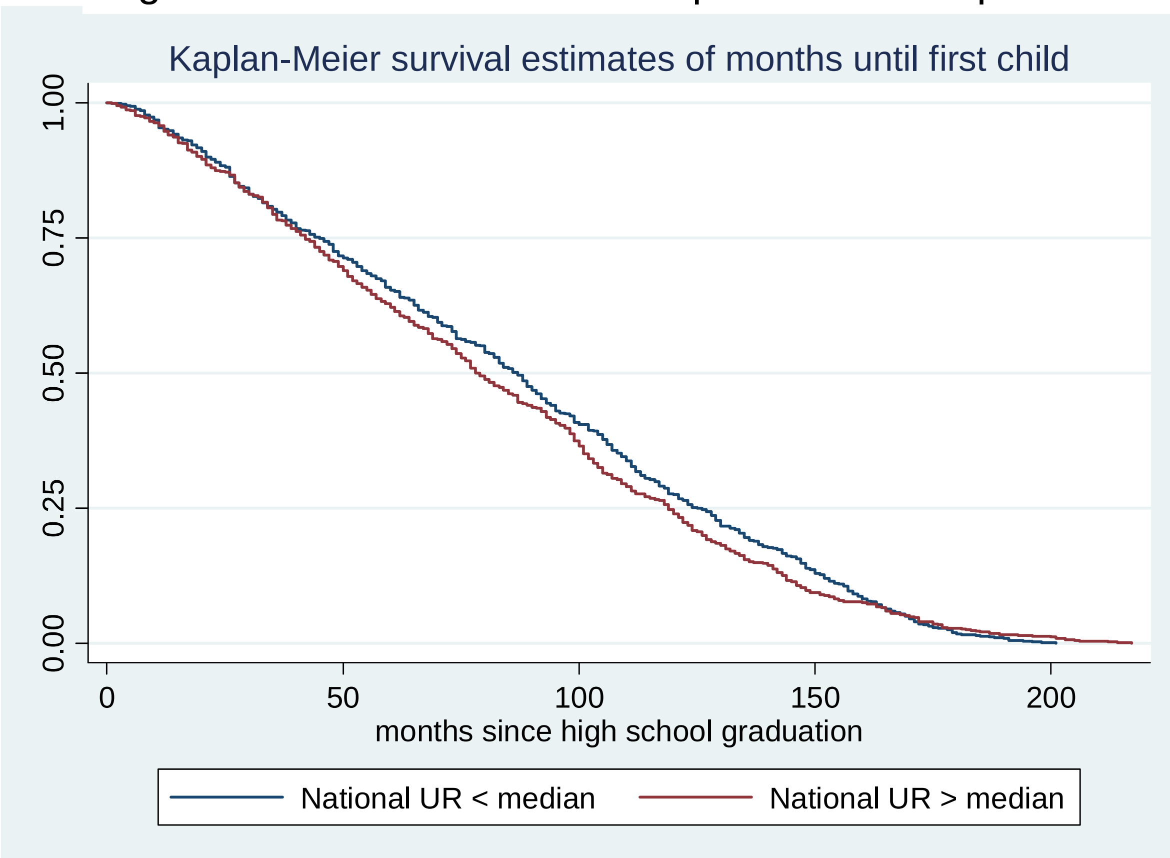
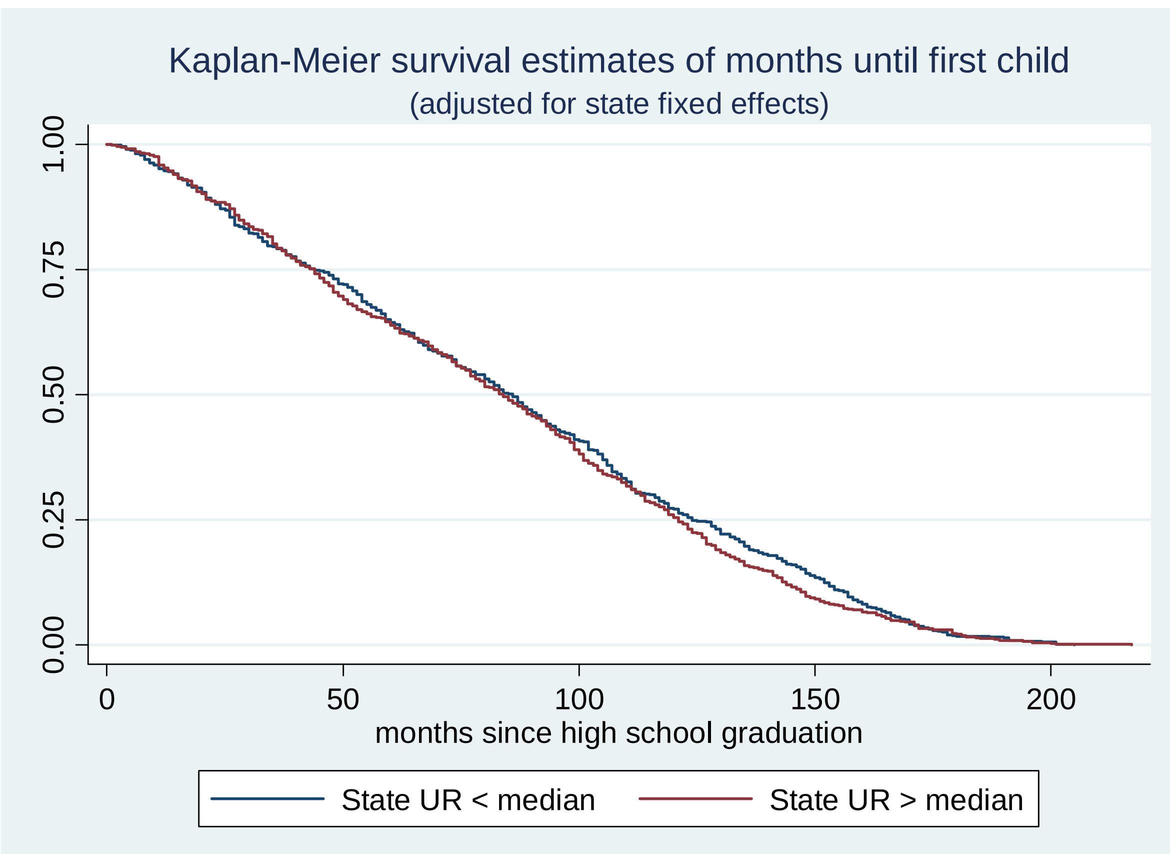


Figure 4: Time to First Child Kaplan-Meier Graphs





Appendix Table 1: Sample Sizes and Attrition in the NLSY79 Female Cross-Section Sample

	Number	Percent	Percent of HS on-time grads
1) Female respondents in cross-section sample	3108	100.0%	–
2) + also graduate high school	2409	77.5%	–
3) + also graduate at age 17-19 (on time)	2337	75.2%	100.0%
4) + also have valid AFQT score	2228	71.7%	95.3%
5) + also can identify state of graduation	2219	71.4%	95.0%
6) + also have valid state UR at year of graduation	2020	65.0%	86.4%
(state urnemployment rates not available until 1976)			
7) + also interviewed every year (1979 through 2004)	1389	44.7%	59.4%

Appendix Table 2: Empirical Sample Sizes By Cohort

	National			
Year graduated high school	Frequency (Total)	Frequency (Obs every year)	Unemployment Rate (1st col)	Unemployment Rate (2nd col)
1975	193	127	8.5	8.5
1976	249	168	7.7	7.7
1977	283	186	7.1	7.1
1978	292	192	6.1	6.1
1979	309	221	5.8	5.8
1980	299	210	7.1	7.1
1981	281	198	7.6	7.6
1982	273	190	9.7	9.7
1983	41	26	9.6	9.6
Total	**2220**	**1518**	**mean = 7.41**	**mean = 7.40**

	State			
Year graduated high school	Frequency (Total)	Frequency (Obs every year)	Unemployment Rate (1st col) (mean)	Unemployment Rate (2nd col) (mean)
1975	–	–	–	–
1976	249	168	8.00	7.93
1977	281	185	7.05	6.97
1978	290	190	5.98	5.96
1979	309	221	5.97	5.94
1980	298	210	7.50	7.53
1981	280	198	7.90	7.88
1982	271	190	10.17	10.19
1983	41	26	10.27	9.60
Total	**2019**	**1388**	**mean = 7.52**	**mean = 7.49**

Appendix Table 3: Observation Frequencies of Labor Force Status by Year of High School Graduation and Years Since Graduation

NLSY79 Female Cross-Section

National UR sample

Years since graduation	Total Obs	Obs 1975	Obs 1976	Obs 1977	Obs 1978	Obs 1979	Obs 1980	Obs 1981	Obs 1982	Obs 1983
1	1486	0	0	0	292	306	299	277	271	41
2	1764	0	0	283	288	308	298	278	268	41
3	2001	0	249	279	290	307	296	275	264	41
4	2178	193	247	279	290	307	288	274	259	41
5	2153	188	246	277	289	303	285	271	254	40
6	2136	190	243	279	283	302	282	264	254	39
7	2115	190	245	276	276	295	284	255	255	39
8	2101	191	243	266	274	291	279	264	254	39
9	2077	187	242	263	268	286	278	260	255	38
10	2078	187	240	262	269	291	274	261	253	41
11	2067	180	239	258	274	287	274	262	252	41
12	2023	179	232	262	275	291	277	259	248	0
13	1802	181	237	258	275	284	272	260	0	35
14	1742	180	232	262	269	284	269	0	246	0
15	1514	176	236	260	271	280	0	255	0	36
16	1449	178	235	260	266	0	266	0	244	0
17	1186	175	234	254	0	273	0	250	0	0
18	926	177	226	0	261	0	262	0	0	0
19	694	177	0	243	0	274	0	0	0	0
20	486	0	225	0	261	0	0	0	0	0
21	410	172	0	238	0	0	0	0	0	0
22	226	0	226	0	0	0	0	0	0	0
23	170	170	0	0	0	0	0	0	0	0
Total	34784	3271	4277	4759	4971	4969	4483	3965	3577	512

State UR sample

Years since graduation	Total Obs	Obs 1976	Obs 1977	Obs 1978	Obs 1979	Obs 1980	Obs 1981	Obs 1982	Obs 1983
1	1481	0	0	290	306	298	277	269	41
2	1757	0	281	286	308	297	277	267	41
3	1994	249	277	288	307	295	274	263	41
4	1978	247	277	288	307	287	273	258	41
5	1959	246	275	287	303	284	271	253	40
6	1938	243	277	281	302	281	263	252	39
7	1919	245	274	274	295	283	255	254	39
8	1904	243	264	272	291	278	264	253	39
9	1884	242	261	266	286	277	260	254	38
10	1885	240	260	267	291	273	261	252	41
11	1882	239	257	272	287	273	262	251	41
12	1838	232	260	273	291	276	259	247	0
13	1616	237	256	273	284	271	260	0	35
14	1556	232	260	267	284	268	0	245	0
15	1334	236	258	269	280	0	255	0	36
16	1265	235	258	264	0	265	0	243	0
17	1009	234	252	0	273	0	250	0	0
18	746	226	0	259	0	261	0	0	0
19	515	0	241	0	274	0	0	0	0
20	484	225	0	259	0	0	0	0	0
21	237	0	237	0	0	0	0	0	0
22	226	226	0	0	0	0	0	0	0

Appendix Table 4:
Sample Means of Standardized AFQT by High School Graduation Cohort

High School Graduation Year	*Mean*	*Std error*	*Significantly different from:*
1975	0.296	0.051	1976**, 1983***
1976	0.115	0.048	1977*, 1980**, 1982*, 1983***
1977	0.231	0.047	1983***
1978	0.208	0.048	1983***
1979	0.183	0.046	1983***
1980	0.278	0.043	1983***
1981	0.183	0.049	1983***
1982	0.236	0.052	1983***
1983	-0.543	0.129	–

Significance levels are from t-tests of differences in means for the specified years
* significant at 10%, ** significant at 5%, *** significant at 1%

Appendix Table 5: Probit Regressions on College Enrollment 1-4 Years After Graduation
(standard errors in parentheses)

	(1)	(2)	(3)	(4)
Geographic UR level	*National*	*National*	*State*	*State*
Family background FE	*No*	*Yes*	*No*	*Yes*
1 Year After				
mean	0.471	0.485	0.472	0.485
Coefficient on UR	0.003	-0.005	0.008	0.000
	(0.008)	(0.011)	(0.012)	(0.011)
Coefficient on AFQT	0.235	0.222	0.254	0.229
	(0.010)**	(0.011)**	(0.012)**	(0.018)**
Observations	1,485	1,271	1,476	1,264
2 Years After				
mean	0.413	0.423	0.414	0.423
Coefficient on UR	0.006	-0.002	0.010	0.006
	(0.007)	(0.006)	(0.010)	(0.010)
Coefficient on AFQT	0.248	0.230	0.260	0.233
	(0.011)**	(0.016)**	(0.012)**	(0.016)**
Observations	1,764	1,519	1,753	1,512
3 Years After				
mean	0.357	0.367	0.358	0.368
Coefficient on UR	-0.003	-0.009	-0.008	-0.002
	(0.004)	(0.005)+	(0.009)	(0.009)
Coefficient on AFQT	0.255	0.239	0.268	0.240
	(0.011)**	(0.017)**	(0.011)**	(0.014)**
Observations	2,001	1,722	1,988	1,714
4 Years After				
mean	0.315	0.326	0.324	0.337
Coefficient on UR	-0.010	-0.014	-0.005	-0.006
	(0.012)	(0.012)	(0.008)	(0.008)
Coefficient on AFQT	0.252	0.245	0.261	0.243
	(.008)**	(0.013)**	(0.011)**	(0.015)**
Observations	2,177	1,870	1,973	1,696

+ significant at 10%, * significant at 5%, ** significant at 1%

(1) Reported are mean marginal effects

(2) Standard errors are clustered at high school graduation year (national) or year-state (state)

Appendix Table 6: Regressions on Welfare Measures by Experience Year
(Low-income and Minority Sample)
(standard errors in parentheses)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	*Poverty Status*	*Poverty Status*	*Log Real Family Income*	*Log Real Family Income*	*Govt assistance*	*Govt assistance*
Geographic UR level	*National*	*State*	*National*	*State*	*National*	*State*
Mean dep variable	0.186	0.188	9.343	9.337	0.146	0.148
HS Grad UR, 1 year after	0.015	0.004	-0.040	0.001	0.012	0.008
	(0.006)**	(0.006)	(0.012)**	(0.019)	(0.006)+	(0.006)
2 years after	0.012	0.004	-0.043	-0.008	0.011	0.010
	(0.006)*	(0.006)	(0.010)**	(0.018)	(0.006)+	(0.006)+
3 years after	0.013	0.006	-0.054	-0.020	0.008	0.009
	(0.007)+	(0.006)	(0.009)**	(0.017)	(0.006)	(0.005)+
4 years after	0.011	0.007	-0.050	-0.018	0.007	0.011
	(0.007)	(0.005)	(0.008)**	(0.016)	(0.008)	(0.005)*
5 years after	0.008	0.005	-0.043	-0.014	0.004	0.010
	(0.007)	(0.005)	(0.008)**	(0.015)	(0.008)	(0.005)*
6 years after	0.005	0.004	-0.038	-0.014	0.002	0.009
	(0.007)	(0.005)	(0.008)**	(0.015)	(0.008)	(0.005)+
7 years after	0.004	0.004	-0.026	-0.004	0.001	0.010
	(0.007)	(0.005)	(0.009)*	(0.015)	(0.008)	(0.005)*
8 years after	0.003	0.005	-0.023	-0.001	-0.002	0.007
	(0.007)	(0.005)	(0.011)+	(0.014)	(0.009)	(0.005)
9 years after	0.002	0.004	-0.011	0.008	-0.001	0.007
	(0.007)	(0.006)	(0.011)	(0.015)	(0.009)	(0.005)
10 years after	0.001	0.004	-0.009	0.008	-0.001	0.008
	(0.008)	(0.006)	(0.009)	(0.015)	(0.010)	(0.005)
11 years after	0.002	0.006	-0.002	0.012	-0.002	0.005
	(0.007)	(0.006)	(0.010)	(0.015)	(0.010)	(0.005)
12 years after	0.000	0.003	-0.001	0.010	-0.001	0.003
	(0.007)	(0.006)	(0.009)	(0.016)	(0.011)	(0.005)
13 years after	-0.001	0.005	0.006	0.013		
	(0.007)	(0.006)	(0.009)	(0.016)		
14 years after	-0.002	0.004	0.013	0.017		
	(0.007)	(0.007)	(0.007)	(0.017)		
15 years after	-0.004	-0.001	0.014	0.022		
	(0.006)	(0.006)	(0.007)+	(0.017)		
(McFadden's) R-squared	0.101	0.117	0.139	0.156	0.104	0.134
Family background FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	21,143	18,815	19,803	17,647	21,536	19,126

+ significant at 10%, * significant at 5%, ** significant at 1%

(1) Reported are mean marginal effects
(2) Regressions include all RHS variables as specified in the notes to Table 3
(3) Standard errors are clustered at high school graduation year (national), or year-state (state)
(4) Government assistance includes any of (a) AFDC, (b) food stamps, or (c) SSI, and is measured only through 1995 to avoid conflating effects of welfare reform