WELFARE REFORM, RETURNS TO EXPERIENCE, AND WAGES: USING RESERVATION WAGES TO ACCOUNT FOR SAMPLE SELECTION BIAS

Jeffrey Grogger*

Abstract—One rationale for work-focused welfare reform was human capital theory: work today should raise experience tomorrow, which should raise future wage offers and reduce welfare dependency. Yet few studies have estimated the effect of welfare reform on wages. I approach the problem using a novel sample selection estimator based on reservation wage data. Reservation wages solve the selection problem using bivariate censored regression methods without the need for exclusion restrictions. Whereas OLS and conventional sample selection estimates suggest that reform had little effect on wages, the reservation-wage-adjusted estimates suggest that Florida's welfare reform experiment raised wages by about 4%.

I. Introduction

PROMOTING work was one of the primary rationales for welfare reform. One of the key arguments for work came from human capital theory. The notion was that work today would increase experience in the future, that increased experience would increase future wage offers, and that higher wages would reduce future welfare dependency. Welfare agencies adopted the slogan "a job, a better job, a career" to convey this notion to their clients.

Despite the policy interest in wage growth, little research has focused on the link between welfare reform and wages. Whereas over two dozen studies have estimated the effect of reform on work (Grogger & Karoly, 2005), only a handful have estimated the effect of reform on wages. Most of these studies analyze accepted wages without adjusting for the possibility that workers might represent a self-selected sample from the welfare population (Bloom et al., 2002; Card, Michalopoulos, & Robins, 2001). Such analyses may not identify the effect of reform on the offered wages of interest to policymakers.

The reason is that welfare reform generates experience gains in the future by reducing the wage at which recipients are willing to work in the present. Reforms such as time limits and financial incentives reduce recipients' reservation wages. All else equal, this increases employment and reduces the mean accepted wage among workers. Over time, however, additional employment results in greater experience, which should increase future wage offers. The combination of higher wage offers and lower reservation wages could either raise or lower mean accepted wages, but by themselves, accepted wages do not identify the effects of reform on offered wages.

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*Harris School, University of Chicago.

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Furthermore, there is debate over whether the wages of low-skill workers rise much with experience. Gladden and Taber (1999) and Loeb and Corcoran (2001) show similar returns to experience among low- and high-skill workers, but other studies suggest that low-skill workers enjoy little of the wage growth experienced by their higher-skill counterparts (Burtless, 1995; Edin & Lein, 1997; Moffitt & Rangarajan, 1989; Pavetti & Acs, 1997; Card & Hyslop, 2004; Dustmann & Meghir, 2005). Yet whether wages grow with experience is a critical determinant of whether welfare reform will increase offered wages.

My first objective in this paper is to estimate the effects of Florida's Family Transition Program (FTP) on wages roughly four years after the program began. FTP was a random-assignment welfare reform program with a time limit and a fairly generous financial work incentive. The potential for sample selection bias in this setting is particularly great, since even four years after the program was implemented, only 60% of program participants were employed. My second objective is to estimate the return to experience among welfare recipients.

To account for the sample selection problem, I propose a novel approach based on reservation wage data. In a simple model of labor force participation, the consumer will work if her offered wage exceeds her reservation wage, that is, her shadow price of leisure (Gronau, 1974; Heckman, 1974). This means that with data on reservation wages, the analyst can solve the selection problem and recover the parameters of the wage offer distribution by means of a censored bivariate regression model, where the reservation wages provide censoring thresholds for consumers who do not work. One advantage of this approach is that it does not require the potentially controversial exclusion restrictions often employed to identify more familiar sample selection estimators, such as Heckman's two-step estimator (Heckman, 1979).

The reservation wage data are available from the FTP evaluation. However, because they were collected in an effort to value employer-provided health care, the questions used to obtain them are overly complex. Perhaps due to this complexity, the reservation wage data appear to involve a substantial amount of measurement error. Extending the econometric model to account for measurement error shows that the resulting estimator still takes the form of a censored bivariate regression, but the measurement error affects which observations are treated as limit observations and which are treated as nonlimit observations.

Accounting for selection bias affects the results. Simple linear regressions suggest that FTP had little effect on wages four years after random assignment. This is not surprising

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given that selection and experience have countervailing effects on accepted wages. Using reservation wages to correct for selection bias, however, suggests that the program may have increased offered wages. Accounting for self-selection also raises the estimated return to experience.

In the next section, I discuss the data, after which I discuss estimation in section III. I present results in section IV. In the conclusion, I discuss the estimation method as well as the results. Although the estimator I employ was developed to solve the selection problem in a specific context, the approach could be used more generally if reservation wage data were collected more widely. I discuss how the quality of such data might be improved.

II. The Family Transition Program and the Data

A. FTP

FTP was a pilot welfare reform program carried out in Escambia County (Pensacola), Florida, which involved a random-assignment evaluation. Between May 1994 and February 1995, ongoing welfare recipients were randomly assigned to treatment and control groups at their biannual recertification interviews. Applicants were randomly assigned at the time of application. Bloom et al. (2000) provides details about the program's evaluation, as well as its effects on employment, earnings, and income.

FTP's treatment group was subject to time limits and a financial incentive. Most recipients could receive aid for only 24 months in any 60-month period, although more disadvantaged recipients could receive aid for 36 out of 72 months. Control group members were not subject to a time limit. Working treatment group members could keep the first $200 they earned each month, as well as 50% of the amount over $200. Working control group members faced the tax schedule from the Aid to Families with Dependent Children program. After the first four months of work, their marginal tax rate on earnings was 100% if they earned over $90 per month.

At the time of random assignment, both the time limit and the financial incentive should have increased employment. FTP's time limit should have reduced reservation wages among the treatment group (Grogger & Michalopoulos, 1999, 2003). The financial incentive also should have reduced the pretax wage at which the recipient was willing to work. The hope among policymakers was that the resulting increase in experience would eventually lead to higher wage offers and a path off welfare.[1]

B. Data Sources and Samples

Data on wages and reservation wages were collected four years after random assignment. Of the 2,815 recipients in the "report sample" that Bloom et al. (2000) analyzed, 2,160 were targeted for the four-year survey. Questionnaires were completed by 1,729 recipients, yielding a completion rate of 80%. The four-year survey collected information about employment, earnings, and hours at the time of the survey. I used these data to compute hourly wages. The survey also collected information on reservation wages, which I discuss in detail in the next section.

In addition to the survey data, administrative sources provide monthly data on welfare receipt and quarterly data on earnings covered by the unemployment insurance (UI) system. These data are available for a six-year observation window that begins two years prior to random assignment and extends through the time of the four-year survey. The UI system covers roughly 90 percent of all jobs in the United States, although it excludes self-employment, some government jobs, and independent contractors (U.S. Bureau of Labor Statistics, 1989). It misses casual employment paid in cash, which may be an important source of income for welfare recipients (Edin & Lein, 1997). To measure labor force experience, I sum the number of quarters with UI-covered earnings during the six-year observation window. Using an actual experience measure to estimate the return to experience raises an endogeneity issue, since actual experience is a function of past employment decisions (Gladden & Taber, 2000). I discuss my approach to this problem in section III.

The first two columns of table 1 compare summary statistics from the report sample and the survey sample. Both samples exhibit characteristics familiar from other studies of welfare populations. They are relatively young, poorly educated, and disproportionately nonwhite. Fewer than 15% of women in either sample received welfare in the 48th month after entering the program.

Average experience during the six-year observation window was 9.8 quarters in the report sample and 10.52 quarters in the survey sample. Although there are no data on experience prior to the observation window, it is useful to roughly estimate prior experience in order to compare my results below to previous estimates from the literature. Bloom et al. (2000) report that average age in the sample was 29.1 years at the time of random assignment, or 27.1 years at the beginning of the observation window. Average years of education were 11.1.[2] Assuming that one completes grade 11 at age 17 implies that sample members had been out of school for 10.1 years on average at the beginning of the observation window.

The average employment rate in the two years prior to random assignment was 0.26. Assuming that employment rate applies to the preobservation period, that is, the period prior to the beginning of the six-year observation window, implies that preobservation experience averaged about 2.6 years. One might be concerned that this employment rate

[1] In addition, the treatment group was subject to different asset limits and parental responsibility requirements than the control group was. The link between these differences and employment is less clear than that between time limits, financial incentives, and employment.

[2] Neither exact age nor exact years of education are available in the public use data that I use in this analysis.

TABLE 1.—SUMMARY STATISTICS FOR VARIOUS SAMPLES

Variable	Report Sample	Four-Year Survey Sample	Reservation Wage Sample	Employment Sample
Age < 20	0.071	0.073	0.073	0.060
Age 20–24	0.252	0.251	0.260	0.256
Age 25–34	0.456	0.447	0.449	0.455
Age 35–44	0.199	0.199	0.196	0.201
Age 45 or over	0.033	0.029	0.021	0.020
No diploma or GED	0.382	0.392	0.388	0.327
Diploma or GED	0.527	0.530	0.537	0.591
After high school	0.062	0.055	0.053	0.062
Education missing	0.029	0.023	0.023	0.019
White	0.438	0.423	0.422	0.433
Nonwhite	0.562	0.577	0.578	0.567
Number of children		2.12	2.16	2.04
		(1.32)	(1.32)	(1.28)
Received welfare, month 48	0.117	0.135	0.127	0.055
Experience (quarters)	9.80	10.52	10.91	13.09
	(7.22)	(7.18)	(7.11)	(6.89)
Employed, quarter 16	0.487	0.536	0.567	0.716
Employed, survey		0.592	0.620	1.000
Reservation wage			6.45	6.73
			(2.15)	(2.51)
Wage				7.15
				(3.16)
Sample size	2,815	1,729	1,548	959

Note: Figures in parentheses are standard deviations.

understates earlier experience, since many of the women in the sample, particularly the ongoing recipients, were on aid during the two years prior to random assignment. Applicants to the program, who spent less time on aid before random assignment than ongoing recipients did, had an average employment rate of 0.29 during the two years before they applied for aid.[3] Using this higher employment rate implies that preobservation experience averaged about 2.9 years.[4] Adding this to mean experience during the observation window suggests that average lifetime experience at the time of the four-year survey was roughly 5 to 6 years.

The next row of table 1 shows that roughly half the sample had UI-covered earnings 16 quarters after random assignment; the survey sample is somewhat more likely than the report sample to have positive UI earnings in the sixteenth quarter following random assignment. Within the survey sample, the difference between UI-covered employment and self-reported employment is fairly small as compared to other samples of former welfare recipients, where casual employment often results in differences of 10 to 20 percentage points (Isaacs & Lyon, 2000).

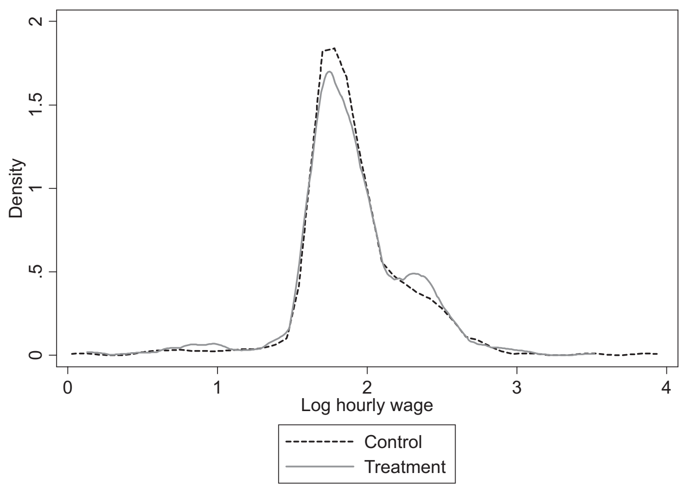
The third column of table 1 presents data on survey sample members for whom reservation wage data are available. Because the reservation wage data were used to value employer-provided health coverage, the questions were posed to all members of the survey sample rather than just to nonworkers. Of the 1,729 members of the sample survey, 1,548, or 89.5%, provided responses to the reservation wage question. The third column shows that this reservation wage sample is generally similar to the survey sample as a whole, with the exception that its employment rate and labor market experience are somewhat higher. This is the sample that will be used in estimating the censored regression models discussed in the next section.

Of the 1,548 members of the reservation wage sample, 959 worked, for an employment rate of 62%. This employment sample had greater levels of observable skill than those who were not working, as seen in the fourth column of table 1. Whereas nearly 39% of the reservation wage sample lacked both a diploma and a GED, only 33% of the employment sample had no high school credential. The employment sample also had considerably more work experience, having accumulated 13 quarters as compared to 10.9 in the reservation wage sample.

[3] The term *applicant* applies to anyone who applied for aid during the period of random assignment. Many had received aid during previous spells. Such cycling on and off the rolls is common among welfare recipients.

[4] One might be concerned that employment exhibits an Ashenfelter dip, that is, a sharp decrease just before random assignment. Such a dip could cause me to underestimate preobservation experience. However, no such dip occurred; sample employment rates were generally rising during the two years prior to random assignment.

FIGURE 1.—KERNEL DENSITY ESTIMATES OF WAGE DISTRIBUTION OF WORKERS, BY TREATMENT STATUS



The next row of the table shows that mean wages among workers are $7.15 per hour. Figure 1 presents further data on wages in the form of kernel density estimates of wage distributions estimated separately for the treatment and control groups. As compared to the control group density, the treatment group density has less mass in the range of about $6.00 to $6.50 an hour (corresponding to log wages of 1.79 to 1.87) and more in the range of $10.00 an hour (corresponding to a log wage of 2.3). Figure 1 suggests that FTP helped some workers escape the "$6 ghetto" for somewhat better-paying work.

C. *Reservation Wage Data*

Of course, figure 1 compares wages among workers. If workers differ from nonworkers along unobservable dimensions in the same way that they differ along observable dimensions, the result could be sample selection bias. I account for sample selection bias using reservation wage data elicited by the first of the following pair of questions:

1. Suppose that next month you were unemployed and had no medical benefits, and someone offered you a full-time job with *employer-paid full medical benefits.* What is the *lowest* wage per hour that the employer could offer and still get you to take the job?
2. Suppose that next month you were unemployed and had no medical benefits, and someone offered you a full-time job with *no* employer-paid health benefits. What is the *lowest* wage per hour that the employer could offer and still get you to take the job?

Question 1 is clearly quite challenging, requiring the respondent to evaluate a scenario that may be quite at odds with her current situation. Although the nonworkers could presumably evaluate the unemployment condition stipulated by this question, such an evaluation would presumably be more difficult for the 62% of recipients who were currently working. Furthermore, the condition regarding the lack of health benefits in question 2 involved another hypothetical scenario for the 60% of the sample members who currently had health coverage (and possibly for the 85% of the sample whose children had coverage). This additional level of complexity was likely to pose particular difficulties for the majority covered by Medicaid, which would have continued to provide coverage even in the event of a job loss. Since question 2 is substantially more complex than question 1, I restrict my attention to question 1 in my analysis.

Given the complexities of the questionnaire items, one might reasonably be concerned about the quality of the responses, or whether the questions seemed so hypothetical that respondents failed to take them seriously. There are a few ways to gauge the quality of these data. The first is to note that the value of health insurance implied by responses to the two questions averages $1.03 per hour, or about $2,000 per year at full-time work. This value accords at least roughly with the price of health insurance policies, which one would not have expected if respondents had treated the questions dismissively.

Second, since the question was posed to workers, one can compare workers' reservation wages to their reported wages. At the aggregate level, the reservation wages seem sensible, as shown in table 2. They are generally low, in line with the wages typically paid to low-skill workers. Except at the very bottom of the wage distribution, where many wage reports fall beneath the federal minimum wage, wages exceed reservation wages, at least weakly, as theory requires.

However, comparing individual reports reveals a number of discrepancies, that is, observations where workers report reservation wages in excess of their current wage. Roughly one-third of workers reported a reservation wage that exceeded their wage. These discrepancies could result from misreporting of either wages or reservation wages. Roughly 25% of the discrepant observations involved wage reports below the federal minimum wage. Other discrepancies involved reservation wages that exceeded the current wage by a small, even amount, such as 25 cents, or that appeared to represent rounding up to such an amount, for example, from $5.15 to 5.50. One possibility is that despite the prefatory language in the questionnaire, these respondents reported the wage at which they would be willing to leave their current job. Another is that respondents interpreted the question as asking about the wage they might expect under the circumstances given. Dominitz (1998) has shown that

TABLE 2.—DISTRIBUTION OF WAGES AND RESERVATION WAGES AMONG WORKERS

Percentile	Mean	5	10	25	50	75	90	95
Wage	7.15	4.38	5.15	5.50	6.27	7.90	10.70	12.50
Reservation wage	6.74	5.00	5.15	5.15	6.00	7.00	9.00	10.00

Note: Sample size is 959.

survey respondents' reports of earnings expectations are generally optimistic when compared to future realizations.

Whatever the reason for the discrepancies, they need to be accounted for in order to use the reservation wage data to deal with the sample selection problem. To do this, I assume that both the wage and the reservation wage are measured with error. I then derive the likelihood for the sample of error-laden data. This is akin to the approach taken in some structural search models, where measurement error is invoked to rationalize observations that run contrary to theory, such as job changes that involve wage reductions or accepted wages that fall below stated reservation wages (van den Berg, 1990; Flinn, 2002; Dey & Flinn, 2005).[5]

III. Estimation

Before dealing with the problem of measurement error, I first develop the singly censored bivariate regression model in its absence, which allows me to discuss most simply how using reservation wages as censoring thresholds can solve the sample selection problem. I also discuss the restrictive conditions under which solving the selection problem also solves the problem of endogenous labor market experience.

A. *No Measurement Error*

The offered wage and reservation wage equations are given by

$$w_i^* = X_{1i}\beta_1 + \delta_1 Z_i + u_{1i} \tag{1}$$

$$r_i^* = X_{2i}\beta_2 + \delta_2 Z_i + u_{2i}, \tag{2}$$

where w_i^* denotes the logarithm of consumer i's offered wage and r_i^* denotes the logarithm of the consumer's reservation wage, both measured without error. In one set of regressions below, Z_i represents the treatment group dummy, equal to 1 for members of the treatment sample and 0 for members of the control sample. In these regressions, δ_1 gives the effect of FTP on offered wages at the time of the four-year survey. Random assignment to treatment plus homogeneity of the treatment effect implies that δ_1 represents the average treatment effect of the program on the population of welfare recipients. Since FTP was an experimental implementation of a program that was being considered for statewide implementation, the average treatment effect is a parameter of considerable policy interest.

In other regressions, Z_i represents the labor market experience measure already discussed. In these regressions, δ_1 gives the return to experience on offered wages. Preliminary analyses showed that returns are roughly linear in experience, as might be expected in light of participants' limited age range.

[5] One further cause for concern is that the reservation wage question asks for the wage at which the respondent would be willing to work full-time. In Grogger (2005), I explicitly model this possibility. The resulting estimates are similar to those reported below, though less precise.

The vector X_{1i} includes a vector of characteristics known to influence wages, such as education, race, and number of children. The vector X_{2i} includes characteristics thought to influence the consumer's reservation wages. The vectors X_{1i} and X_{2i} may differ, although they need not, and in this application, they include the same variables. Employing the reservation wage data in the manner described below eliminates the need for the often-controversial exclusion restrictions typically used to identify self-selection models. The terms u_{1i} and u_{2i} represent unobservable factors that influence wages and reservation wages, respectively. The term u_{1i} captures unobservable labor market productivity. The term u_{2i} reflects unobservables that affect the shadow price of time. It may also reflect respondents' beliefs about the nonwage characteristics of potential jobs. I assume that X_{1i}, X_{2i}, and Z_i and are uncorrelated with u_{1i} and u_{2i}. This assumption is justified in the case where Z_i represents the treatment group dummy. Below I discuss how I deal with the potential endogeneity of past experience. I allow u_{1i} and u_{2i} to be correlated and assume that they follow a bivariate normal distribution.

A simple model of labor force participation says that the consumer works if her offered wage (weakly) exceeds her reservation wage, that is, if

$$w_i^* \geq r_i^*. \tag{3}$$

This model yields what I refer to as a singly censored bivariate regression estimator, where the reservation wages serve as censoring thresholds for nonworkers.

To see this, consider two groups: workers, who contribute nonlimit observations, and nonworkers, who contribute limit observations. Workers, for whom $w_i^* \geq r_i^*$, contribute a bivariate density term to the likelihood that is given by

$$\begin{aligned}
P(w_i^* \geq r_i^*) f(u_{1i}, u_{2i} | w_i^* \geq r_i^*) \\
&= P(u_{1i} \geq r_i^* - X_{1i}\beta_1 - \delta_1 Z_i) \\
&\quad \times f(u_{1i}, u_{2i} | u_{1i} \geq r_i^* - X_{1i}\beta_1 - \delta_1 Z_i) \\
&= P(u_{1i} \geq r_i^* - X_{1i}\beta_1 - \delta_1 Z_i) \\
&\quad \times \frac{f(u_{1i}, u_{2i})}{P(u_{1i} \geq r_i^* - X_{1i}\beta_1 - \delta_1 Z_i)} \\
&= f(u_{1i}, u_{2i}),
\end{aligned}$$

where $f(u_{1i}, u_{2i})$ is the bivariate normal pdf. The density for these nonlimit observations is given by the product of two terms: the probability that the wage (weakly) exceeds the reservation wage and the conditional joint density of the disturbance terms, given that the wage exceeds the reservation wage. The right-hand side of the first line above simply uses equation (1) to rewrite the left-hand side. The second line follows from the first via standard results on the truncated bivariate normal density (Johnson & Kotz, n.d.). Because the conditional joint density takes the convenient form given in the second line, the likelihood for the ith nonlimit observation takes the simple form given in the third line.

For nonworkers, we do not observe w_i^*, but we do observe r_i^*. Moreover, theory tells us that $w_i^* < r_i^*$ or, equivalently, that $u_{1i} < r_i^* - X_{1i}\beta_1 - \delta_1 Z_i$. When the joint density of the disturbances is integrated with respect to u_{1i}, a nonworker's contribution to the likelihood is

$$\int_{-\infty}^{r_i^*-X_{1i}\beta_1-\delta_1 Z_i} f(u_{1i}, u_{2i})du_{1i}.$$

When n_0 represents the number of nonlimit observations and n_1 represents the number of limit observations, the sample likelihood is given by

$$\ln L = \sum_{n_0} \ln f(u_{1i}, u_{2i}) + \sum_{n_1} \ln \int_{-\infty}^{r_i^*-X_{1i}\beta_1-\delta_1 Z_i} f(u_{1i}, u_{2i})du_{1i}.$$

Under certain conditions, this model solves not only the sample selection problem, but also the endogeneity problem that arises because experience represents the summation of past employment decisions. As I show in the appendix, these conditions are restrictive: they require reservation wages, and all determinants of the wage except experience, to be time invariant. In this case, the consumer will either work in all periods of her life or in none. Her entire career trajectory depends on whether she works in the first period of her working life. Conditional on that first decision, employment is deterministic, so experience is conditionally exogenous. Solving the selection problem for the first period implicitly solves the endogeneity problem, but since the consumer's employment decision is the same in every period, solving the selection problem in any period (including the period four years after random assignment) is equivalent to solving it in the first period. Although these conditions are too restrictive to be realistic, they suggest that if the variables that determine employment (other than experience) are dominated by time-invariant components, then solving the selection problem may help mitigate the endogeneity problem that arises from including actual experience as a regressor, even though it does not completely solve it.

Under more realistic conditions, the endogeneity problem may require an instrumental variable, for which I use the treatment group dummy as an instrument for experience. The treatment dummy should provide a valid instrument, because FTP provided an incentive to work for the treatment group and assignment to treatment was made at random. Intuitively, the instrument accounts for the potential endogeneity of past experience, whereas the reservation wage accounts for contemporaneous selection into employment. Following Newey (1987; see also Smith & Blundell, 1986), I first regress experience on the treatment group dummy and the other exogenous variables in the model, then include the residuals from this first-stage regression in the singly censored bivariate regression model. The coefficient on the experience residual should provide a test of the null hypothesis of no misspecification against the alternative of endogenous experience, accounting for self-selection.

B. *Measurement Error*

To account for measurement error, let (log) offered wages and reservation wages be given by

$$w_i = w_i^* + \varepsilon_i \tag{4}$$

and

$$r_i = r_i^* + \nu_i, \tag{5}$$

where $\varepsilon_i \sim N(0, \sigma_\varepsilon^2)$ and $\nu_i \sim N(0, \sigma_\nu^2)$ may be correlated with each other but are assumed to be independent of X_{1i}, X_{2i}, Z_i, u_{1i}, and u_{2i}. The observable wage and reservation wage equations are:

$$w_i = X_{1i}\beta_1 + \delta_1 Z_i + \eta_{1i} \tag{6}$$

$$r_i = X_{2i}\beta_2 + \delta_2 Z_i + \eta_{2i}, \tag{7}$$

where $\eta_{1i} = u_{1i} + \varepsilon_i$ and $\eta_{2i} = u_{2i} + \nu_i$. I assume that η_{1i} and η_{2i} follow a bivariate normal distribution with zero means, variances σ_1^2 and σ_2^2, respectively, and correlation coefficient ρ.

The full-information likelihood for this model consists of three equations: equations (6) and (7) and an employment equation derived by substituting (1) into (3) and solving. The apparent advantage of the full-information likelihood is that it uses all the data on observed wages and reservation wages, plus the information on employment status, which, according to (3), is a function of the true offered wage and reservation wage rather than their observed counterparts. The problem with it is that it is not identified. The three-by-three covariance matrix has six parameters, whereas there are only four moments available to identify them. Fortunately, one can write down a limited-information likelihood function where all the regression parameters in equations (1) and (2) are identified, as is the two-by-two covariance matrix of the η_{ji} terms.

In deriving the limited-information likelihood, there are two groups to consider. As in the simple case without measurement error, the groups correspond to the limit and nonlimit observations. However, in the presence of measurement error, the key is to note that only workers who report $w_i \geq r_i$ can be treated as nonlimit observations. All other observations, that is, both nonworkers and workers with discrepant wage reports, must be treated as limit observations. Wage reports from the discrepant observations are not utilized in this approach, which is why I refer to the result as a limited-information likelihood.

For workers who report $w_i \geq r_i$, the contribution to the likelihood is given by

$$
\begin{aligned}
&P(w_i \geq r_i) f(\eta_{1i}, \eta_{2i} | w_i \geq r_i) \\
&\quad = P(\eta_{1i} \geq r_i - X_{1i}\beta_1 - \delta_1 Z_i) \\
&\qquad \times f(\eta_{1i}, \eta_{2i} | \eta_{1i} \geq r_i - X_{1i}\beta_1 - \delta_1 Z_i) \\
&\quad = P(\eta_{1i} \geq r_i - X_{1i}\beta_1 - \delta_1 Z_i) \\
&\qquad \times \frac{f(\eta_{1i}, \eta_{2i})}{P(\eta_{1i} \geq r_i - X_{1i}\beta_1 - \delta_1 Z_i)} \\
&\quad = f(\eta_{1i}, \eta_{2i}).
\end{aligned}
$$

For all other observations, the contribution to the likelihood is

$$
\int_{-\infty}^{r_i - X_{1i}\beta_1 - \delta_1 Z_i} f(\eta_{1i}, \eta_{2i}) d\eta_{1i}. \tag{8}
$$

Let n_0' denote the number of nonlimit observations, and let n_1' denote the number of limit observations. The sample likelihood is

$$
\begin{aligned}
\ln L &= \sum_{n_0'} \ln f(\eta_{1i}, \eta_{2i}) \\
&\quad + \sum_{n_1'} \ln \int_{-\infty}^{r_i - X_{1i}\beta_1 - \delta_1 Z_i} f(\eta_{1i}, \eta_{2i}) d\eta_{1i}.
\end{aligned}
$$

This is again a singly censored bivariate regression model, albeit with a different definition of the limit and nonlimit observations.

A natural question is why the full set of workers cannot be treated as nonlimit observations in the presence of measurement error. The reason is that to do so would require accounting for the fact that employment decisions are based on equation (3), whereas the observed model is given by equations (6) and (7). Thus, the density associated with workers in the presence of measurement error is given by

$$
\begin{aligned}
&P(w_i^* \geq r_i^*) f(\eta_{1i}, \eta_{2i} | w_i^* \geq r_i^*) \\
&\quad = P(u_{1i} \geq r_i^* - X_{1i}\beta_1 - \delta_1 Z_i) \\
&\qquad \times f(\eta_{1i}, \eta_{2i} | u_{1i} \geq r_i^* - X_{1i}\beta_1 - \delta_1 Z_i) \\
&\quad \neq f(\eta_{1i}, \eta_{2i}).
\end{aligned}
$$

The problem is that employment, the conditioning event, is a function of the true wage and reservation wage, whereas the data consist of the observable, error-laden wage and reservation wage. As a result, the conditional joint density on the right-hand side of the first line above cannot be rewritten in the same convenient manner as could the conditional joint density in the model without measurement error. The second line above shows that simply treating all the workers as nonlimit observations is likely to yield inconsistent estimates, since the contribution to the likelihood that one would attribute to such observations would be incorrect.

IV. Results

A. *Effect of FTP on Wages*

Estimates of the effect of FTP on wages are presented in table 3. The first column reports results from an OLS regression of log wages on the FTP treatment dummy, age dummies, education dummies, a race dummy, and the number of children. Although there is no reason to expect these estimates to have desirable properties, I present them for purposes of comparison. They represent the estimates one would obtain by ignoring the sample selection problem altogether.

The coefficient on the treatment dummy is negative and insignificant. By itself, this estimate gives little reason to think that FTP had much effect on wages at the four-year mark. As for the other estimates in the model, most are consistent with expectations. Although the age dummies are insignificant, the education variables have strong and significant effects, and the nonwhite dummy is negative and significant. The coefficient on the number of children is negative and significant, but small.

The next two columns present estimates from Heckman's (1979) two-step sample selection estimator. Given the assumed normality of the error terms, this is the natural estimator to which to compare the singly censored bivariate regression estimator. One might not expect the Heckman two-step estimator to perform very well, since the lack of exclusion restrictions means that identification is based on functional form alone. However, absent a plausible exclusion restriction or data on reservation wages, one might resort to this type of model in order to deal with selectivity bias. Column 2 presents estimates from the employment probit estimated from the full reservation wage sample, where the dependent variable is an employment dummy equal to 1 if the consumer is employed at the four-year survey and equal to 0 otherwise. Column 3 presents the estimated wage equation, which includes the inverse Mills ratio from the employment probit to correct for sample selection bias.[6]

In the employment equation, the coefficient on the treatment dummy shows that FTP had a marginally significant effect on employment status at the four-year mark, even though the treatment group worked significantly more than the control group during the first three years of the experiment (Bloom et al., 2000). Otherwise the estimates largely

[6] Given normality, this can be thought of as a control function estimator, where the inverse Mills ratio represents a generalized residual from the employment probit. See Heckman and Robb (1985).

TABLE 3.—ESTIMATES OF THE EFFECT OF FTP ON WAGES FOUR YEARS AFTER RANDOM ASSIGNMENT

Estimator:	OLS	Heckman Two-Step		Singly Censored Bivariate Regression	
Sample:	Employment Sample	Reservation Wage Sample	Employment Sample	Reservation Wage Sample	
Dependent Variable: Variable	Log Wage (1)	Employment (2)	Log Wage (3)	Log Wage (4)	Log Reservation Wage (5)
Treatment dummy	−0.013	0.111	0.016	0.037	0.016
	(0.025)	(0.066)	(0.115)	(0.020)	(0.012)
Age < 20	0.050	−0.179	0.001	−0.037	−0.011
	(0.056)	(0.137)	(0.203)	(0.047)	(0.024)
Age 25–34	0.014	0.031	0.022	0.010	0.022
	(0.031)	(0.081)	(0.048)	(0.025)	(0.014)
Age 35–44	−0.039	0.024	−0.033	−0.024	−0.010
	(0.037)	(0.102)	(0.051)	(0.031)	(0.018)
Age 45 and over	0.080	−0.208	0.026	0.015	0.012
	(0.092)	(0.234)	(0.241)	(0.072)	(0.042)
No diploma, GED	−0.163	−0.372	−0.262	−0.191	−0.082
	(0.027)	(0.070)	(0.390)	(0.022)	(0.012)
After high school	0.177	0.117	0.204	0.208	0.188
	(0.052)	(0.157)	(0.122)	(0.045)	(0.026)
Nonwhite	−0.053	−0.017	−0.057	−0.098	−0.049
	(0.019)	(0.068)	(0.035)	(0.021)	(0.012)
Number of children	−0.018	−0.083	−0.040	−0.019	−0.005
	(0.008)	(0.027)	(0.088)	(0.008)	(0.005)
Inverse Mills ratio			0.467		
			(1.825)		
σ_1				0.356	
				(0.009)	
σ_2					0.226
					(0.003)
ρ				0.553	
				(0.028)	
R^2/ln L	0.071			−527.8	
Sample size	959	1,548	959	1,548	

Notes: Standard errors in parentheses. In addition to variables shown, all models include a dummy for missing education.

accord with expectations. In the wage equation, the coefficient on the treatment dummy is positive, but it is small and dwarfed by its standard error. Indeed, the standard errors on all the coefficients are quite high. This is likely due to collinearity with the inverse Mills ratio, since the model is identified solely on the basis of functional form.

Columns 4 and 5 report results from the singly censored bivariate regression model described above. The coefficients in the wage equation are generally estimated more precisely than their counterparts from the Heckman two-step estimator. The coefficient on the treatment group dummy in the wage equation suggests that FTP raised wages by 3.7% four years after the program began. The t-statistic is 1.81, which means that the estimate is significant at the 10% level but not at the 5% level.

The coefficient on the treatment group dummy in the reservation wage equation is positive, suggesting that FTP slightly but insignificantly raised recipients' reservation wages at the time of the four-year follow-up. This runs counter to the expectation that FTP's time limit should have reduced reservation wages among the treatment group, even four years after random assignment. However, beyond FTP's contemporaneous effect on reservation wages, its cumulative effect on employment during the four-year follow-up period may have led to greater accumulated earnings, which would raise the treatment group's reservation wages. Indeed the positive but insignificant effect of FTP on reservation wages is consistent with the positive but insignificant effect of the program on savings at the time of the four-year survey (Bloom et al., 2002, appendix C).

Furthermore, the small reservation wage effect revealed in column 5 helps explain why the OLS estimate of the effect of FTP in column 1 is biased downward. Although FTP raised reservation wages, it raised wages by a greater amount. As a result, workers in the treatment group constitute a relatively less selective sample than workers in the control group, imparting a negative selection bias on the treatment dummy coefficient.

Indeed, with the exception of an insignificant age coefficient, all of the coefficients in the reservation wage equation are smaller in absolute value than their counterparts in the wage equation. This is what one might expect. Presumably the wage represents the maximum value of the consumer's time across different types of market activity, that is, across different types of jobs. In contrast, the reservation wage represents the value of the consumer's time in a single type of nonmarket activity: household production. If so, then the return to schooling (for example) in the market should

TABLE 4.—ESTIMATES OF THE RETURN TO EXPERIENCE

Estimator	OLS	Heckman Two-Step		Singly Censored Bivariate Regression	
Sample:	Employment Sample	Reservation Wage Sample	Employment Sample	Reservation Wage Sample	
Dependent Variable: Variable	Log Wage (1)	Employment (2)	Log Wage (3)	Log Wage (4)	Log Reservation Wage (5)
Experience	0.0035		0.0036	0.0139	0.0023
	(0.0018)		(0.0018)	(0.0015)	(0.0008)
Treatment dummy	0.054	0.111			
	(0.056)	(0.066)			
Age < 20	0.015	−0.179	0.026	−0.016	−0.007
	(0.031)	(0.137)	(0.073)	(0.043)	(0.024)
Age 25–34	−0.040	0.031	0.019	0.004	0.022
	(0.037)	(0.081)	(0.033)	(0.024)	(0.014)
Age 35–44	0.093	0.024	−0.037	−0.022	−0.009
	(0.092)	(0.102)	(0.040)	(0.030)	(0.018)
Age 45 and over	−0.156	−0.208	0.064	0.078	0.023
	(0.027)	(0.234)	(0.109)	(0.071)	(0.042)
No diploma, GED	0.179	−0.372	−0.215	−0.151	−0.076
	(0.052)	(0.070)	(0.095)	(0.022)	(0.013)
After high school	−0.058	0.117	0.194	0.205	0.186
	(0.026)	(0.157)	(0.061)	(0.044)	(0.026)
Nonwhite	−0.018	−0.017	−0.060	−0.117	−0.032
	(0.010)	(0.068)	(0.028)	(0.021)	(0.012)
Number of children	0.074	−0.083	−0.031	−0.005	−0.005
		(0.027)	(0.023)	(0.005)	(0.005)
Inverse Mills ratio			0.276		
			(0.424)		
σ_1				0.346	
				(0.009)	
σ_2					0.226
					(0.004)
ρ				0.555	
				(0.022)	
ln L				−480.6	
Sample size	959	1,548	959	1,548	

Notes: Standard errors in parentheses. In addition to variables shown, all models include a dummy for missing education.

exceed the return to schooling in the home. Similarly, the residual variation in market wages should exceed the residual variation in reservation wages, which is precisely what we see in the estimates of σ_1 and σ_2.[7]

B. *The Return to Experience*

In table 4, I present estimates of the return to experience where experience is taken to be exogenous. I relax this assumption below. Linear regression estimates appear in the first column. The OLS estimate is significant but small. Without controls for selection bias, one would conclude that former welfare recipients enjoyed little return to experience.

The next two columns present the employment and wage equations, respectively, from Heckman's two-step estimator. The estimates of the employment equation in column 2 are identical to those reported in column 2 of table 3; they are repeated here for clarity. In this model, the treatment group dummy appears only in the employment equation, so in principle, it contributes to the identification of the wage equation. Its practical contribution is limited, however, since the treatment group dummy has only a marginally significant effect on employment.

The estimated return to experience in column 3 is positive and significant but almost identical to the OLS estimate in table 4. The reason is that the inverse Mills ratio is completely insignificant, with a *t*-statistic less than 1. If the model were convincingly identified, one might infer from the insignificant Mills ratio that self-selection bias was essentially absent from these data. However, since the treatment dummy is only marginally significant in the employment equation, an alternative interpretation is that identification is weak, and as a result, the inverse Mills ratio provides a poor control for sample selection bias.

Estimates from the censored bivariate regression model appear in columns 4 and 5. Experience has positive effects on both wages and reservation wages. The small positive effect of experience on reservation wages reported in column 5 may stem from the greater accumulation of savings among the treatment group, as discussed above. The estimated effect of experience on wages reported in column 4 is larger than the OLS estimate reported in table 3. This is the direction of bias one would expect given the small effect of experience on reservation wages. Experience increases reservation wages, but it increases wages by a greater amount.

[7] Implicitly I am assuming that the difference between σ_1 and σ_2 primarily reflects differences between var(u_{1i}) and var(u_{2i}), rather than differences between var(ε_i) and var(ν_i).

TABLE 5.—SELECTIVITY-CORRECTED IV ESTIMATES OF THE RETURN TO EXPERIENCE

Estimator:	OLS (First Stage)	Heckman Two-Step with IV		Singly Censored Bivariate Regression with IV	
Sample:	Reservation Wage Sample	Reservation Wage Sample	Employment Sample	Reservation Wage Sample	
Dependent Variable: Variable	Experience (1)	Employment (2)	Log Wage (3)	Log Wage (4)	Log Reservation Wage (5)
Experience			0.0148	0.0332	0.0154
			(0.1079)	(0.0185)	(0.0107)
Treatment dummy	1.069	0.111			
	(0.350)	(0.066)			
Experience residual				−0.019	−0.013
				(0.019)	(0.011)
Age < 20	−1.339	−0.179	0.020	−0.008	0.009
	(0.739)	(0.137)	(0.091)	(0.045)	(0.028)
Age 25–34	−0.255	0.031	0.026	0.009	0.026
	(0.431)	(0.081)	(0.069)	(0.025)	(0.014)
Age 35–44	−0.419	0.024	−0.027	−0.014	−0.003
	(0.539)	(0.102)	(0.081)	(0.031)	(0.018)
Age 45 and over	−4.435	−0.208	0.091	0.161	0.080
	(1.262)	(0.234)	(0.296)	(0.107)	(0.062)
No diploma, GED	−2.976	−0.372	−0.219	−0.094	−0.037
	(0.375)	(0.070)	(0.118)	(0.059)	(0.034)
Post high school	0.456	0.117	0.197	0.197	0.181
	(0.609)	(0.157)	(0.081)	(0.044)	(0.027)
Nonwhite	1.412	−0.017	−0.078	−0.144	−0.071
	(0.362)	(0.068)	(0.257)	(0.033)	(0.019)
Number of children	−0.381	−0.083	−0.035	−0.007	0.000
	(0.142)	(0.027)	(0.050)	(0.011)	(0.006)
Inverse Mills ratio			0.467		
			(1.825)		
σ_1				0.345	
				(0.009)	
σ_2					0.226
					(0.004)
ρ				0.555	
				(0.022)	
R^2	0.067				
Sample size	1,548	1,548	959	1,548	

Note: Standard errors in parentheses. In addition to variables shown, all models include a dummy for missing education.

Thus, experience decreases the relative selectivity of the sample, negatively biasing the OLS estimate.

The experience coefficient on wages in column 4 is significant, and its magnitude suggests that welfare recipients enjoy a return of roughly 5.6% per year of experience. This is comparable to a number of other recent estimates in the literature that are based on samples of workers with similar levels of experience. Studies by Gladden and Taber (1999), Loeb and Corcoran (2001), Ferber and Waldfogel (1998), Lynch (2001), Light and Ureta (1995), Card et al. (2001), and Zabel, Schwartz, and Donald (2004) all report returns to experience for young workers that range between 3% and 8%, with most of the estimates clustered around 5%.[8]

However, experience may be endogenous. Table 5 reports two sets of estimates intended to deal with both selectivity bias and potentially endogenous experience. The first extends the Heckman two-step approach to deal with an endogenous regressor. The second adapts the singly censored bivariate probit model along the lines of Newey (1987) and Smith and Blundell (1986), as discussed in section III. Both estimators make use of a first-stage regression of experience on the treatment dummy and the other exogenous regressors in the model. This regression is based on the full reservation wage sample, including workers and nonworkers. Results for the first-stage regression are shown in column 1. They show that FTP raised experience by about one-quarter over the four-year follow-up period. Education raised experience, whereas children reduced it; nonwhites worked more than whites, all else equal.

To modify the Heckman two-step estimator, I replace actual experience in the wage equation with predicted values from the first-stage experience regression.[9] I present these estimates for comparison purposes, since this is presumably the type of approach one might consider in order to

[8] Card and Hyslop (2004) are an exception. They report that returns to experience among Canadian welfare recipients are about zero. It is not clear why their estimates differ from those of Zabel et al., who analyze data from the same welfare experiment.

[9] This is similar to the estimator proposed by Heckman (1976), except that the endogenous regressor is observed in the full sample in my case, whereas it was observed only in the self-selected sample in his. See also Amemiya (1985) and Wooldridge (2001).

deal with both selectivity and the potential endogeneity of experience in the absence of the reservation wage data. The employment probit reported in column 2 is exactly the same as that presented in column 2 of table 3. As above, I report it again here for clarity. In the wage equation, reported in column 3, the effect of experience is positive, although the coefficient is only a fraction of its standard error. Most of the other estimates are similarly imprecise. This is the result of effectively using the treatment dummy twice—once in the employment probit to handle self-selection and again as an instrument for experience.

To modify the singly censored bivariate regression model, I add the residuals from the first-stage experience regression to both the wage and reservation wage equations. Columns 4 and 5 of table 5 present the results. The estimated return to experience is marginally significant and larger than its counterpart in column 4 of table 4. At first glance this may seem surprising. One of the reasons that experience may be endogenous in a wage regression is that past employment is positively correlated with past wages. Persistent unobservables that cause higher wages should cause higher employment, in which case estimates that fail to account for such observables should yield upward-biased estimates of the return to experience. However, past employment is negatively correlated with past reservation wages, so if the unobservables that influence past wages are sufficiently correlated with past reservation wages, it is conceivable that estimates that fail to account for such correlation could be negatively biased. Put differently, negative bias may arise if the current wage disturbance is more highly correlated with past reservation wage disturbances than with past wage disturbances, once current-period self-selection is accounted for.

The estimate corresponds to an annualized return to experience of roughly 13%, well above the range of returns reported above. At the same time, the experience coefficient in column 4 of table 5 is not significantly different from the experience coefficient in column 4 of table 4, where experience is treated as exogenous given the control for sample selection bias. Moreover, the Hausman test computed from the coefficients on the first-stage residuals shows little reason to favor the specification in columns 4 and 5 of table 5 over that in columns 4 and 5 of table 4. The coefficients on the first-stage residuals are about the same magnitude as their standard errors. The F-statistic for the joint significance of both coefficients is 1.76 ($p = 0.41$), failing to reject the null of no misspecification. As suggested above, this may indicate that the unobservable characteristics that influence employment are dominated by time-invariant components, in which case the bivariate censoring model would largely account for the endogeneity of experience at the same time that it accounts for self-selection into the labor force.

How does the estimated return to experience square with the estimated effect of FTP? Since FTP increased experience by one-quarter over the four-year follow-up period, this calculation is easy to make. Based on the estimated return to experience in column 4 of table 4, one would expect FTP to have increased wages by about 1.4%. This is lower than the 3.7% estimate of the effect of FTP reported in column 4 of table 3, although it is within the confidence interval of that estimate.

V. Conclusions

The human capital benefits of work provided an important rationale for welfare reform. Yet little research has focused on the question of whether welfare reform has increased offered wages. My analysis suggests that Florida's FTP program raised wages by about 3.7% some four years after the program began. It also suggests that former welfare recipients enjoy returns to experience that are similar to those enjoyed by more general samples of young workers. My best estimate is that each year of work increases future wages by about 5.6%. Since FTP increased experience by about three months, this implies that FTP should have raised wages by 1.4%, on average. Direct estimates indicate that FTP may have increased wages by 3.7%, although that estimate is imprecise enough to include 1.4% in its confidence interval.

To estimate the effects of reform and experience on offered wages, I have employed a novel approach based on reservation wage data to deal with the sample selection problem. The approach exploits a simple model of labor supply. Since the model predicts that the consumer will work if her offered wage exceeds her reservation wage, reservation wages provide censoring thresholds for nonworkers, which can be used to solve the selection problem. Since the selection problem is so pervasive in labor market research, it is useful to discuss how the approach might be made more broadly applicable.

One useful step would be to relax the distributional assumptions that I have maintained. An advantage of the reservation wage approach is that it eliminates the need for the often controversial exclusion restrictions that are typically employed to identify self-selection models. This benefit comes with a cost, however, in that I have imposed normality to derive my estimator. It would be useful to determine the extent to which potentially restrictive distributional assumptions can be relaxed.

At a more basic level, extending the approach would require new data collection, since none of the ongoing surveys commonly used in labor market research collect data on reservation wages. One of the lessons of the analysis is that unless one can collect wage and reservation wage data without error, one would have to collect reservation wage data from everyone in the sample, not just nonworkers.

Two recent advances in survey methodology seem particularly promising for reducing measurement error in the reservation wage data. The first involves "unfolding brackets," where respondents are queried about a decreasing sequence of reservation prices until they indicate one to be unacceptable. This approach has been used to elicit information about future income expectations in the Health and Retirement Survey (Hurd, 1999). Anchoring the reservation wage brackets about the current wage may help to reduce the extent to which workers report reservation wages that exceed their wage. Another approach would be to pose probabilistic questions regarding the likelihood that the respondent would find a given wage (again, within a sequence) acceptable. Such probabilistic sequences have been used to elicit consumers' expectations about future earnings, among other things (Dominitz & Manski, 1991). An appealing feature of this approach is that the reported probabilities could be incorporated directly into the likelihood used in estimation. In sum, it seems it should be possible to obtain better data on reservation wages, which could provide a useful tool for labor market researchers who confront the sample selection problem.

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APPENDIX

Sample Selection and the Endogeneity of Experience

To provide conditions under which accounting for selection bias also accounts for the endogeneity of experience requires some additional notation. Specifically, I add time subscripts t to the model in section IIIA. This does not imply the availability of panel data; the time subscript is needed to make explicit the link between current experience and past employment. The data t should be thought of as the date of the four-year survey. The modified wage equation is given by

$$w^*_{it} = X_{1it}\beta_1 + \delta_1 Z_{it} + u_{1it}, \tag{A1}$$

where the variables in the model are the same as those discussed above.

To derive the needed conditions, write labor market experience Z_{it} explicitly as the sum of past employment, so $Z_{it} = \sum_{j=1}^{J} 1(w^*_{it-j} \geq r^*_{it-j})$, where $t - J$ represents the first period of the consumer's working life and $1(A)$ is the indicator function, so $1(A) = 1$ if A is true and $1(A) = 0$ otherwise.

Now let $X_{1it} = X_{1i}$, $u_{1it} = u_{1i}$, and $r^*_{it} = r^*_i$. At the beginning of the consumer's career, when $t - J = 1$, $Z_{i1} = 0$, we have

$$w^*_{i1} = X_{1i}\beta_1 + u_{1i},$$

and the consumer works if $w^*_{i1} \geq r^*_i$. Furthermore, if she works in period 1, then she works in all periods. Conversely, if she does not work in period 1, she never works. This means that experience is deterministic once the first-period employment decision is made, so solving the first-period selection problem also solves the endogeneity problem. But since experience is deterministic once first-period employment is known, solving the selection problem in any period is equivalent to solving it in the first period.

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