

NBER WORKING PAPER SERIES

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AND WAGES: USING RESERVATION WAGES
TO ACCOUNT FOR SAMPLE SELECTION BIAS

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Working Paper 11621
<http://www.nber.org/papers/w11621>

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
September 2005

I thank Kerwin Charles, Costas Meghir, Bruce Meyer, Derek Neal, Jeff Zabel and seminar participants at Brigham Young, Chicago, McMaster, the National Poverty Center, the Institute for Research on Poverty, the National Bureau of Economic Research, and the Econometric Society World Congress for helpful comments. Any errors are my own. The views expressed herein are those of the author(s) and do not necessarily reflect the views of the National Bureau of Economic Research.

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Welfare Reforms, Returns to Experience, and Wages: Using Reservation Wages to Account for Sample Selection Bias

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NBER Working Paper No. 11621

September 2005

JEL No. I3, J3, C3

ABSTRACT

Work was one of the central motivations for welfare reform during the 1990s. One important rationale for work was based on human capital theory: work today should raise experience tomorrow, which in turn should raise future wage offers and reduce dependency on aid. Despite the importance of this notion, few studies have estimated the effect of welfare reform on wages. Furthermore, several recent analyses suggest that low-skill workers, such as welfare recipients, enjoy only meager returns to experience, undermining the link between welfare reform and wages.

An important analytical obstacle is the sample selection problem. Since non-employment levels are high and workers are unlikely to represent a random sample from the population of former recipients, estimates that fail to account for sample selection could be seriously biased.

In this paper, I propose a method to solve the selection problem based on the use of reservation wage data. Reservation wage data allow one to solve the problem using censored regression methods. Furthermore, the use of reservation wage data obviates the need for the controversial exclusion restrictions sometimes used to identify familiar two-step sample selection estimators.

Correcting for sample selection bias matters a great deal empirically. Estimates from models that lack such corrections suggest that welfare recipients gain little from work experience. Estimates based on the reservation wage approach suggest that they enjoy returns similar to those estimated from other samples of workers. They also suggest that the particular reform program that I analyze may have raised wages modestly.

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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. Many welfare agencies adopted the slogan “a job, a better job, a career” to convey this notion to their clients.

Despite the apparent 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, with all but a few showing that reform increased employment (Grogger and Karoly 2005), only a handful have estimated the effect of reform on wages. Most of these studies analyze accepted wage distributions among workers in welfare reform experiments (Bloom et al., 2002; Card, Michalopoulos, and Robins, 2001). Since accepted wages are drawn from self-selected samples of workers, however, these analyses may not identify the effect of reform on the offered wages.

Complicating matters further, welfare reform has theoretically ambiguous effects on accepted wages. Most reform policies involve some combination of work requirements, time limits, and lower tax rates on recipients’ earnings. All of these policies could lead recipients to accept lower wages than they would have otherwise. Without adequate controls for such self-selection, reform could appear to reduce wages, at least in the short term. Whether reform increases wages in the longer term depends on the extent to which recipients’ wages rise with experience.

Although wages rise with experience in the standard human capital model, there is debate over whether the standard human capital model applies to low-skill workers such as welfare recipients. Although some recent studies suggest that wages rise with experience similarly among low- and high-skill workers (Gladden and Taber 1999; Loeb and Corcoran 2001), other studies suggest that low-skill workers enjoy little of the wage growth experienced by their higher-skill counterparts (Burtless 1995, Edin and Lein 1997, Moffitt and Rangarajan 1989, Pavetti and Acs 1997; Card and Hyslop 2004; Dustmann and Meghir 2005). Yet the extent to which wages grow with experience is a critical determinant of whether welfare reform will increase offered wages.

My objectives in this paper are to estimate the effects of a welfare reform program on wages roughly four years after the program began and to estimate the return to experience among welfare recipients. As suggested already, a major obstacle in this analysis is sample selection bias. As in many other contexts, a simple model of labor force participation indicates that the unobservable characteristics of consumers that influence wages also influence labor force participation (Heckman 1974). In the case of welfare recipients, the potential for bias would seem particularly great, since even after welfare reform, only about two-thirds of former recipients are likely to be working at any point in time (Isaacs and Lyon 2000). This means that wages are unobserved for one-third of the sample, so if there is positive selection into employment, a simple linear regression of wages on experience could result in biased estimates of the return to experience.

To solve 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. This means that with data on reservation wages, the analyst can solve the selection problem 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 the more familiar two-equation sample-selection estimators (Heckman 1979).

The reservation wage data are fortuitously available from the evaluation of a Florida welfare reform experiment. However, because they were collected in an effort to value employer-provided health care, the questions used to obtain them involve complexities that do not necessarily contribute to the elicitation of the textbook notion of a reservation wage. Perhaps due to this complexity, the reservation wage data appear to involve a substantial amount of measurement error.

To deal with this problem I extend the econometric model to account for measurement error. The resulting estimator still takes the form of a censored bivariate regression. However, the measurement error affects which observations are treated as limit observations and which are treated as non-limit observations.

Accounting for selection bias has important effects on the results. Simple linear regressions yield very small returns to experience. Standard two-step sample-selection estimators differ little from OLS because the instrument used to identify the wage equation is fairly weak. Using reservation wages to correct for selection bias, however, yields returns that are comparable to those observed in other samples of young workers.

The estimated effect of the reform program on wages is imprecise, but it suggests that the program may have increased wages.

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, particularly if recent developments in survey techniques were employed to collect it.

II. Data

My data come from the evaluation of Florida's Family Transition Program (FTP), which was a pilot welfare reform program carried out in Escambia County (Pensacola). FTP 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 percent 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 percent if they earned over \$90 per month. Both the time limit and the financial incentive provided treatment group members with an incentive to work.

In addition, the treatment group was subject to different asset limits and parental responsibility requirements than the control group. Furthermore, both groups were subject to work requirements that required recipients either to work or to participate in a welfare-to-work program. The welfare-to-work programs for both groups followed a work-first model which focused on job search rather than skills-building, but the programs were administered somewhat differently. The link between these differences and employment is less clear than that between time limits, financial incentives, and employment.

Survey data collected four years after random assignment provide information on wages and reservation wages. Of the 2,815 recipients in the “report sample” analyzed by Bloom, et al. (2000), 2,160 were targeted for the four-year survey. Questionnaires were completed by 1,729 recipients, yielding a completion rate of 80 percent. 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 below.

In addition to the survey data, I use data from administrative sources. These sources provide monthly data on welfare receipt and quarterly data on earnings covered by the Unemployment Insurance (UI) system during 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 U.S., although it

excludes self employment, some government jobs, and independent contractors (Bureau of Labor Statistics 1989). It misses casual employment paid in cash, which may be an important source of income for welfare recipients (Edin and Lein 1997). To measure labor force experience, I sum the number of quarters with UI-covered earnings during the six-year observation window. Using such 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 and 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 non-white. Fewer than 15 percent of women in both samples 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 years.¹ Assuming that one completes 11th grade at the age of 17, I infer that sample members had been out of school for 10.1 years on average at the beginning of the observation window.

¹ Neither exact age nor exact years of education are available in the public-use data that I use in this analysis.

The average employment rate in the two years prior to random assignment was 0.26. Assuming that employment rate applies to the pre-observation period, that is, the period prior to the beginning of the six-year observation window, implies that pre-observation experience averaged about 2.6 years. One might be concerned that this employment rate 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, had an average employment rate of 0.29 during the two years before they applied for aid.² Using this higher employment rate implies that pre-observation experience averaged about 2.9 years.³ 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 the Table shows that roughly half the sample was working four years after random assignment; the survey sample is somewhat more likely than the report sample to have positive UI earnings in the 16th 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 and Lyon, 2000).

² 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

³ 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 pre-observation experience. However, no such dip occurred; sample employment rates were generally rising during the two years prior to random assignment.

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 non-workers. Of the 1,729 members of the sample survey, 1,548, or 89.5 percent, provided responses to the reservation wage question. The third column of Table 1 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 below.

Of the 1,548 members of the reservation wage sample, 959 worked, for an employment rate of 62 percent. This employment sample had greater levels of observable skill than those who were not working, as seen in column (4). Whereas nearly 39 percent of the reservation wage sample lacked both a diploma and a GED, only 33 percent 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.

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 to \$6.50 an hour (corresponding to log wages of 1.79 to 1.87) and more in the range of \$10 an hour (corresponding to a log wage of 2.3). The figure suggests that FTP helped some workers escape the “\$6 ghetto” for somewhat better paying work.

Of course, Figure 1 compares wages among workers. If workers differ from non-workers 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 that were 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?

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

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 \$2000 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. Although nearly two-thirds of workers reported a wage that at least weakly exceeded their reservation wage, a sizeable minority reported the contrary.

These discrepancies could result from misreporting of either wages or reservation wages. Roughly 25 percent 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 survey respondents' reports of earnings expectations are generally optimistic when compared to future realizations.

Whatever the reason for the discrepancies, it is clear that 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 and Flinn 2005).

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. This is at odds with the textbook notion of a reservation wage, which is the wage at which the consumer would be willing to work one hour. Given the other complexities of the question, one might wonder how salient the full-time condition was to the survey respondents. Since we will never know, I present one set of estimates below based on the assumption that the condition was salient, that is, that respondents indeed reported the wage necessary to induce them to work full-time. However, for the analysis that follows in the next section,

I treat the responses to the question as if the full-time language was not salient, that is, as if they reflected respondents' shadow price of leisure.

III. Estimation

Before dealing with the problem of measurement error, I first briefly develop the singly-censored bivariate regression model in its absence. This 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 wage and reservation wage equations are given by

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

$$r_i^* = X_{2i}\beta_2 + \delta_2 Z_i + u_{2i} \quad (2)$$

where w_i^* denotes the logarithm of consumer i 's wage and r_i^* denotes the logarithm of the consumer's reservation wage, both measured without error. The vector X_{1i} includes a vector of characteristics known to influence wages, such as education, race, and the number of children. In some of the regressions below, Z_i represents the treatment-group dummy, equal to one for members of the treatment sample and equal to zero for members of the control sample. In these regressions, δ_1 gives the effect of FTP on wages at the time of the four-year survey. In other regressions, Z_i represents the labor market experience measure discussed above. In these regressions, δ_1 gives the returns to experience. In this case, the assumption that returns are linear in experience is justified by the concentrated age distribution of the sample and is supported empirically. 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 non-wage characteristics of potential jobs. I assume that X_{1i} , X_{2i} , and Z_i 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 assume that u_{1i} and u_{2i} follow a bivariate normal distribution.

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

$$w_i^* \geq r_i^* . \quad (3)$$

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

In deriving the likelihood, there are two groups to consider: workers, who contribute non-limit observations, and non-workers, who contribute limit observations.

The bivariate density term contributed by workers, for whom $w_i^* \geq r_i^*$, is given by

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

where $f(u_{1i}, u_{2i})$ is the bivariate normal pdf. The density for these non-limit 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 re-write the left-hand side. The second line follows from the first via standard results on the truncated bivariate normal density (Johnson and Kotz, not dated, 112). Because the conditional joint density takes the convenient form given in the second line, the likelihood for the i th non-limit observation takes the simple form given in the third line.

The contribution to the likelihood for non-workers, for whom $w_i^* < r_i^*$, is given by

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

Letting n_0 represent the number of non-limit observations and n_1 represent 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 for 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 explicit solution. In the empirical work below, 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. Following Newey (1987) (see also Blundell and Smith 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. This is analogous to Hausman's (1978) linear IV estimate, with the result that 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 observed (log) wages and reservation wages be given by

$$w_i = w_i^* + \varepsilon_i \quad (4)$$

and

$$r_i = r_i^* + v_i \quad (5)$$

where $\varepsilon_i \sim N(0, \sigma_\varepsilon^2)$ and $v_i \sim N(0, \sigma_v^2)$ 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:

$$w_i = X_{1i}\beta_1 + \delta_1 Z_i + \eta_{1i} \quad (6)$$

$$r_i = X_{2i}\beta_2 + \delta_2 Z_i + \eta_{2i} \quad (7)$$

where $\eta_{1i} = u_{1i} + \varepsilon_i$ and $\eta_{2i} = u_{2i} + v_i$. 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 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 involves 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 non-limit 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 non-limit observations. All other observations, that is, both non-workers 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

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

For all other observations, the contribution to the likelihood is

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

Let n_0 denote the number of non-limit observations and let n_1 denote the number of limit observations. The sample likelihood is

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

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

A natural question is why the full set of workers cannot be treated as non-limit observations in the presence of measurement error. The reason is that to do so would require one to account 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

$$P(w_i^* \geq r_i^*)f(\eta_{1i}, \eta_{2i} | w_i^* \geq r_i^*) = P(u_{1i} \geq r_i^* - X_{1i}\beta_1 - \delta_1 Z_i)f(\eta_{1i}, \eta_{2i} | u_{1i} \geq r_i^* - X_{1i}\beta_1 - \delta_1 Z_i) \\ \neq f(\eta_{1i}, \eta_{2i})$$

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 non-limit 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. The Effect of FTP on Wages

Estimates of the effect of the FTP program on wages are presented in Table 3. The first column reports results from an ordinary least squares 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 with the singly-censored bivariate regression model. They represent the estimates one would obtain if one were to ignore 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 non-white 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 procedure to adjust for selectivity bias. Column (2) presents estimates from a probit model estimated from the full reservation wage sample including both workers and non-workers. The dependent variable is an employment dummy equal to one if the consumer is employed at the four-year survey and equal to zero otherwise. Column (3) presents the estimated wage equation, which includes the inverse Mills' ratio from the employment probit to correct for sample selection bias. Since the same variables are included in both the wage and employment equations, identification is via function form alone. One would not expect such a model to perform very well, but absent a plausible exclusion restriction or data on reservation wages, this is the type of model to which one might resort in attempt to deal with selectivity bias.

In the employment equation, the treatment dummy has little 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 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 percent four years after the program began. The t-statistic is 1.81, which means that the estimate is significant at the 10 percent level but not at the five percent 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 by the time of the four-year follow-up. Although one might have expected the treatment and control groups to have the same shadow price of leisure on average at the beginning of the experiment, greater employment among the treatment group during the intervening four years may have led to greater accumulated earnings, raising 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.

Among the other coefficients in columns (4) and (5), age has no effect on either wages or reservation wages. Schooling has strong effects on wages. The effects of education on reservation wages, apparent in the no-diploma coefficient and the post-high school coefficient, suggest that schooling raises home productivity by less than it raises market productivity. The coefficients on the non-white variable indicate that non-whites have lower wages and reservation wages, all else equal, than their white counterparts. Children reduce wages and reservation wages, though the coefficient in the reservation wage equation is insignificant. The estimate of ρ shows substantial positive correlation between the unobservable determinants of wages and reservation wages.

B. The Return to Experience

Linear regression estimates of the return to experience are presented in Table 4. As above, these estimates are reported for purpose of comparison with the censored regression estimates to follow. The OLS estimate in column (1) is significant but small. Without controls for selection bias, one would conclude that former welfare recipients enjoyed little return to experience.

The next column reports a linear instrumental variables estimate. Using the treatment group dummy as an instrument for experience may abate the problem of endogenous experience. Since assignment to treatment was made at random, the treatment dummy should be uncorrelated with unobservable determinants of recipients' wages. However, even though assignment to treatment is random, linear instrumental

variables is unlikely to solve the selection problem, since the observability of the recipient's current wage depends on whether she is currently employed. The estimate is negative, with a standard error an order of magnitude greater than that of the OLS estimate.

Table 5 presents two sets of selectivity-corrected estimates of the return to experience. Columns (1) and (2) report the employment and wage equations, respectively, from Heckman's two-step estimator. The estimates of the employment equation in column (1) 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. As a practical matter, however, the treatment-group dummy has only a marginally significant effect on employment, limiting the extent to which it identifies the wage equation.

The estimated return to experience in column (2) is positive and significant but small. In fact, it is 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 one. 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 (3) and (4). Experience has positive effects on both wages and reservation wages. The small

positive effect of experience on reservation wages reported in column (4) may stem from the greater accumulation of earnings among the treatment group, as discussed above.

The estimated effect of experience on wages reported in column (3) is much 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. Thus experience decreases the relative selectivity of the sample, negatively biasing the OLS estimate.

The experience coefficient on wages in column (3) is significant and its magnitude suggests that welfare recipients enjoy a return of roughly 5.6 percent per year of experience. This is comparable to a number of other recent estimates in the literature that are based on samples with similar levels of experience. Gladden and Taber (1999) study respondents to the National Longitudinal Survey of Youth (NLSY) who received no education beyond high school. During their first 10 years out of school, white women in their sample accumulated five years of work experience and black women accumulated four years, on average. This is roughly comparable to the experience level of the FTP sample, which I estimated above at 5 to 6 years. Over the 10-year study period, the women in Gladden and Taber's sample enjoyed returns to experience of about 4 to 5 percent per year.

Loeb and Corcoran (2001) followed NLSY women from 1978, when they ranged in age from 14 to 21, until 1993, when they ranged between 27 and 34 years old. By age 27, women who had ever received welfare had accumulated an average of 3.9 years of experience. Their average return to experience was 6.8 percent. It is interesting to note that neither Gladden and Taber nor Loeb and Corcoran find the return to experience to

vary by other measures of skill. Gladden and Taber show that experience has similar effects on wages for both high school graduates and high school dropouts; Loeb and Corcoran report similar returns among women who had received welfare and women who had not received welfare.

Ferber and Waldfogel (1998) follow NLSY women over the same period as Loeb and Corcoran and estimate the return to experience to be about 5 percent. Lynch (2001) also analyzes data from the NLSY. She reports that women earn an annualized return of about 11 percent per year of experience during the first three years after leaving school. Light and Ureta (1995) analyze a sample of women from the young women's cohort of the National Longitudinal Surveys (NLS), which preceded the NLSY. Their sample ranges in age between 16 and 39 with a mean of 25; average experience is 3 years. They estimate an average return to experience of 7 percent.

Card, Michalopoulos, and Robins (2001), Zabel, Schwartz, and Donald (2004), and Card and Hyslop (2005) analyze wage data from the Self-Sufficiency Program (SSP), a Canadian experiment that offered welfare recipients a substantial wage subsidy if they were willing to leave welfare and work full-time. When the experiment began, the SSP sample averaged 30 years of age and 7.4 years of lifetime work experience. Estimates of the return to experience differ across these studies. Zabel, Schwartz, and Donald report an estimate of 8.3 percent, whereas Card, Michalopoulos, and Robins report an estimate of 2 to 3 percent, and Card and Hyslop report essentially a zero return to experience. It is not clear why estimates from the same experiment differ so much.

Moving beyond the experience coefficient, one interesting pattern in the estimates warrants discussion. With the exception of an insignificant age coefficient, all of the

coefficients in the wage equation are larger in absolute value than their counterparts in the reservation 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 non-market activity, namely household production. If so, then the return to schooling (for example) in the market should 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 .⁴

As discussed above, experience may be endogenous in this model. Table 6 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), as discussed in Section III above. 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 non-workers. Results 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; non-whites worked more than whites, all else equal.

⁴ Implicitly I am assuming that the difference between σ_1 and σ_2 primarily reflects differences between $\text{var}(u_{1i})$ and $\text{var}(u_{2i})$, rather than differences between $\text{var}(\varepsilon_i)$ and $\text{var}(v_i)$.

To modify the Heckman two-step estimator, I replace actual experience in the wage equation with predicted values from the first-stage experience regression.⁵ There is no reason to expect this estimation scheme to perform well, particularly given the weak relationship between the treatment-group dummy and employment at the time of the four-year survey. I present these estimates for comparison purposes, since this is presumably the type of approach one might consider in order to deal with both selectivity and the potential endogeneity of experience in the absence of the reservation wage data.

The employment equation 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 equation 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 6 present the results. The estimated return to experience is marginally significant and larger than its counterpart in column (3) of Table 5. At first glance this may seem surprising. One of the reasons why 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

⁵ 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, ch. 10) and Wooldridge (2003, ch. 16).

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 percent, which is above the range of returns reported above. At the same time, the experience coefficient in column (4) of Table 6 is not significantly different from the experience coefficient in column (3) of Table 5, 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 there is little reason to favor the specification in columns (4) and (5) of Table 6 over that in columns (3) and (4) of Table 5. 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.

A reasonable question to ask is whether the estimated return to experience squares 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 (3) of Table 5, one would expect FTP to have increased wages by about 1.4 percent. This is lower than the 3.7 percent estimate of the effect of FTP reported in column (4) of Table 3, although it is within the confidence interval of that estimate.

C. Reservation Wages for Full-Time Work

One might object to the estimates above on the grounds that they treat the reported reservation wages as if they represented respondents' shadow price of leisure, even though the questionnaire language asked respondents for the lowest wage under which they would accept full-time work. If the full-time language were salient to respondents as they answered the question, the result could be a misspecified model, since the wage at which the respondent would accept full-time work should exceed the shadow price of leisure. Table 7 presents estimates from a model that accounts for this possibility.

To produce the estimates in Table 7 I have altered the censoring rule so that only full-time workers who report $w_i \geq r_i$ are treated as non-limit observations. This seems reasonable if we think of consumers as operating on an upward-sloping labor-supply curve, so that a high wage offer elicits full-time work, whereas a lower wage offer elicits at most part-time work. Treating consumers who work part-time (as well as non-workers) as limit observations amounts to treating them as if they received offers below the lowest wage for which they would accept full-time work, in accord with the language of the questionnaire item.

Panel A of Table 7 reports estimates of the effect of FTP on wages; panel B reports estimates of the return to experience. In both cases I report OLS (and in panel B, linear IV) estimates based on the sample of full-time workers with $w_i \geq r_i$ for comparison purposes (estimates from the Heckman two-step approach are omitted for brevity). The estimates are generally similar to their counterparts in Tables 3, 4 and 5. However, they are less precisely estimated. This is what one might expect if the language about full-time work was not particularly salient to consumers as they formulated their responses to the reservation wage question. In this case, one would prefer the more precise estimates in Tables 3, 4 and 5 to those in Table 7.

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 wages. Data from a Florida welfare reform evaluation suggest 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 percent. Since FTP increased experience by about 3 months, this implies that FTP should have raised wages by 1.4 percent, on average. Direct estimates indicate that FTP may have increased wages by 3.7 percent, although that estimate is imprecise enough to include 1.4 percent in its confidence interval.

To estimate the effects of reform and experience on 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 wage exceeds her reservation wage,

reservation wages provide censoring thresholds for non-workers 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. This stands in contrast to much recent econometric work, which develops non-parametric estimators that presuppose the existence of a valid instrument (see, e.g., Pagan and Ullah 2005). An important direction for future work on the reservation wage approach is to determine the extent to which potentially restrictive distributional assumptions can be relaxed.

At a more basic level, extending the applicability of the approach would require new data collection, since none of the ongoing surveys commonly used in labor market research currently collect data on reservation wages. This seems to be more of an opportunity than a limitation. One of the lessons of the analysis above is that, unless one can collect wage and reservation wage data without error, one would have to collect reservation wage data not just from non-workers, but from everyone in the sample. The analysis above shows that collecting reservation wage data from non-workers alone, though intuitive, would not allow one to estimate wage equations consistently.

Furthermore, it seems likely that the extent of the measurement error could be reduced. In the FTP survey, roughly one-third of the workers reported reservation wages in excess of their wages. This is a substantial amount of error, but then, the reservation

wage data were not collected for the purpose of solving the sample selection problem. Put differently, even when faced with complex questions involving hypothetical situations aimed at valuing health insurance, two-thirds of the workers provided reservation wage data that were consistent with economic theory. Questions designed to elicit the textbook notion of a reservation wage presumably could do better.

Two recent advances in survey methodology seem particularly promising. 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 and 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.

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 III.A. 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 date t should be thought of as the date of the four-year survey. The modified wage equation is given by

$$w_{it}^* = X_{1it}\beta_1 + \delta_1 Z_{it} + u_{1it} \quad (\text{A1})$$

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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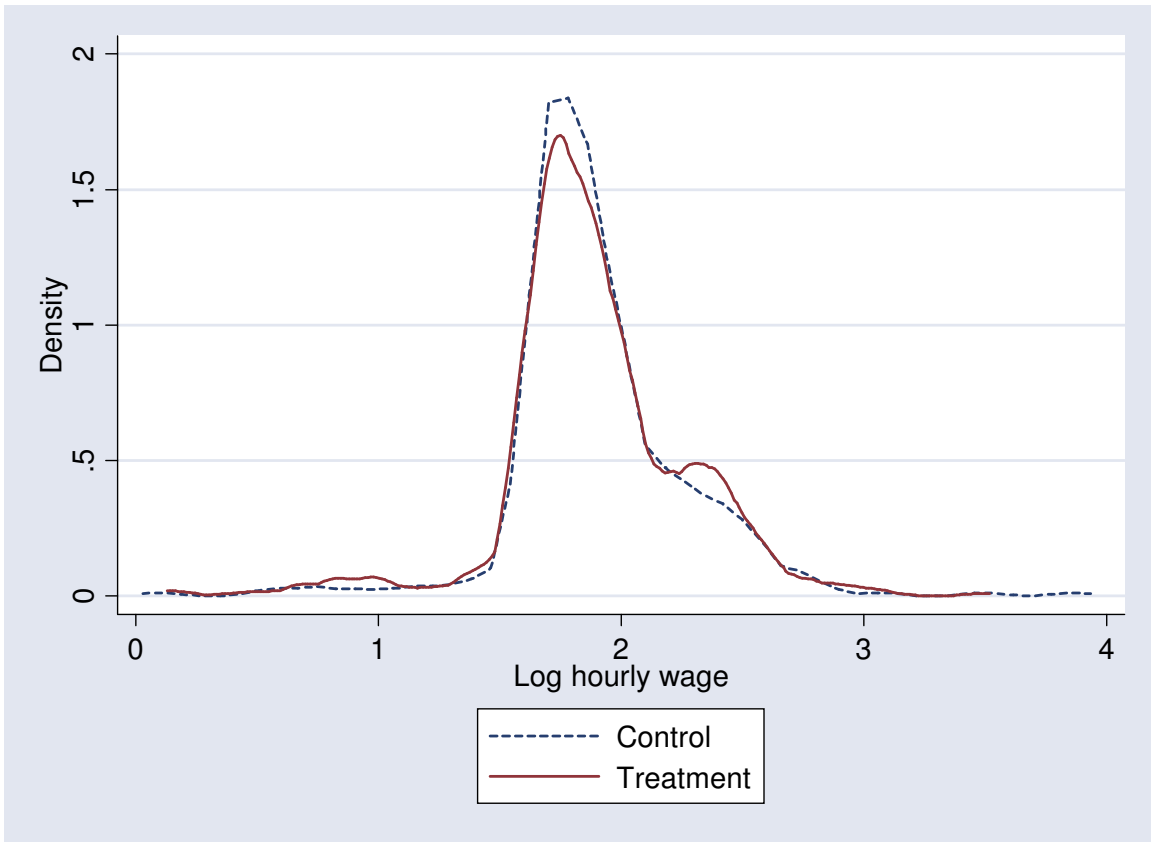


Figure 1
Kernel Density Estimates of Wage Distribution of Workers, by Treatment Status

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
Post 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
Non-white	0.562	0.577	0.578	0.567
Number of kids		2.12 (1.32)	2.16 (1.32)	2.04 (1.28)
Received welfare, month 48	0.117	0.135	0.127	0.055
Experience (quarters)	9.80 (7.22)	10.52 (7.18)	10.91 (7.11)	13.09 (6.89)
Employed, qtr. 16	0.487	0.536	0.567	0.716
Employed, survey		0.592	0.620	1.000
Reservation wage			6.45 (2.15)	6.73 (2.51)
Wage				7.15 (3.16)
Sample size	2,815	1,729	1,548	959

Note: Figures in parentheses are standard deviations.

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

Sample size is 959.

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:	Log wage	Employment	Log wage	Log wage	Log reservation wage
Variable	(1)	(2)	(3)	(4)	(5)
Treatment dummy	-0.013 (0.025)	0.111 (0.066)	0.016 (0.115)	0.037 (0.020)	0.016 (0.012)
Age < 20	0.050 (0.056)	-0.179 (0.137)	0.001 (0.203)	-0.037 (0.047)	-0.011 (0.024)
Age 25-34	0.014 (0.031)	0.031 (0.081)	0.022 (0.048)	0.010 (0.025)	0.022 (0.014)
Age 35-44	-0.039 (0.037)	0.024 (0.102)	-0.033 (0.051)	-0.024 (0.031)	-0.010 (0.018)
Age 45 and over	0.080 (0.092)	-0.208 (0.234)	0.026 (0.241)	0.015 (0.072)	0.012 (0.042)
No diploma, GED	-0.163 (0.027)	-0.372 (0.070)	-0.262 (0.390)	-0.191 (0.022)	-0.082 (0.012)
Post high school	0.177 (0.052)	0.117 (0.157)	0.204 (0.122)	0.208 (0.045)	0.188 (0.026)
Non-white	-0.053 (0.019)	-0.017 (0.068)	-0.057 (0.035)	-0.098 (0.021)	-0.049 (0.012)
Number of kids	-0.018 (0.008)	-0.083 (0.027)	-0.040 (0.088)	-0.019 (0.008)	-0.005 (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-square/ln L	0.071			-527.8	
Sample size	959	1548	959	1,548	

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

Table 4
Linear Regression Estimates of the Return to Experience

Estimator:	OLS	IV
Sample: Variable	Employment sample (1)	Employment sample (3)
Experience	0.0035 (0.0018)	-0.0143 (0.0292)
Age < 20	0.054 (0.056)	0.028 (0.072)
Age 25-34	0.015 (0.031)	0.014 (0.032)
Age 35-44	-0.040 (0.037)	-0.033 (0.042)
Age 45 and over	0.093 (0.092)	0.021 (0.153)
No diploma, GED	-0.156 (0.027)	-0.192 (0.066)
Post high school	0.179 (0.052)	0.173 (0.055)
Non-white	-0.058 (0.026)	-0.030 (0.054)
Number of kids	-0.018 (0.010)	-0.020 (0.011)
R-square	0.074	
Sample size	959	959

Notes: Dependent variable is log wage. Standard errors in parentheses. In addition to variables shown, all models include a missing-education dummy.

Table 5
Selectivity-Corrected Estimates of the Return to Experience

Estimator:	Heckman two-step		Singly-censored bivariate regression	
Sample:	Reservation wage sample	Employment sample	Reservation wage sample	
Dependent variable:	Employment	Log wage	Log wage	Log reservation wage
Variable	(1)	(2)	(3)	(4)
Experience		0.0036 (0.0018)	0.0139 (0.0015)	0.0023 (0.0008)
Treatment dummy	0.111 (0.066)			
Age < 20	-0.179 (0.137)	0.026 (0.073)	-0.016 (0.043)	-0.007 (0.024)
Age 25-34	0.031 (0.081)	0.019 (0.033)	0.004 (0.024)	0.022 (0.014)
Age 35-44	0.024 (0.102)	-0.037 (0.040)	-0.022 (0.030)	-0.009 (0.018)
Age 45 and over	-0.208 (0.234)	0.064 (0.109)	0.078 (0.071)	0.023 (0.042)
No diploma, GED	-0.372 (0.070)	-0.215 (0.095)	-0.151 (0.022)	-0.076 (0.013)
Post high school	0.117 (0.157)	0.194 (0.061)	0.205 (0.044)	0.186 (0.026)
Non-white	-0.017 (0.068)	-0.060 (0.028)	-0.117 (0.021)	-0.032 (0.012)
Number of kids	-0.083 (0.027)	-0.031 (0.023)	-0.005 (0.005)	-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)
In L				-480.6
Sample size	1548	959	1,548	

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

Table 6
Selectivity-Corrected IV Estimates of the Return to Experience

Estimator:	OLS (1st 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:	Experience	Employment	Log wage	Log wage	Log reservation wage
Variable	(1)	(2)	(3)	(4)	(5)
Experience			0.0148 (0.1079)	0.0332 (0.0185)	0.0154 (0.0107)
Treatment dummy	1.069 (0.350)	0.111 (0.066)			
Experience residual				-0.019 (0.019)	-0.013 (0.011)
Age < 20	-1.339 (0.739)	-0.179 (0.137)	0.020 (0.091)	-0.008 (0.045)	0.009 (0.028)
Age 25-34	-0.255 (0.431)	0.031 (0.081)	0.026 (0.069)	0.009 (0.025)	0.026 (0.014)
Age 35-44	-0.419 (0.539)	0.024 (0.102)	-0.027 (0.081)	-0.014 (0.031)	-0.003 (0.018)
Age 45 and over	-4.435 (1.262)	-0.208 (0.234)	0.091 (0.296)	0.161 (0.107)	0.080 (0.062)
No diploma, GED	-2.976 (0.375)	-0.372 (0.070)	-0.219 (0.118)	-0.094 (0.059)	-0.037 (0.034)
Post high school	0.456 (0.609)	0.117 (0.157)	0.197 (0.081)	0.197 (0.044)	0.181 (0.027)
Non-white	1.412 (0.362)	-0.017 (0.068)	-0.078 (0.257)	-0.144 (0.033)	-0.071 (0.019)
Number of kids	-0.381 (0.142)	-0.083 (0.027)	-0.035 (0.050)	-0.007 (0.011)	0.000 (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-square	0.067				
Sample size	1548	1548	959	1548	

Note to Table 6: Standard errors in parentheses. In addition to variables shown, all models include a missing-education dummy

Table 7
Estimates of the Effect of FTP on Wages and of the Return to Experience, Treating
Reservation Wage Responses as Minimum Offers Needed to Induce Full-Time
Work

<i>A: Effect of FTP</i>				
Estimator:	OLS	Singly-censored bivariate regression		
Sample:	Non-limit and ≥ 35 hours/wk.	Reservation wage sample		
Dependent variable:	Wage	Wage		
Variable	(1)	(2)		
Treatment-group dummy	0.020 (0.028)	0.029 (0.023)		
R-square/ lnL	0.131			
Sample size	466	1548		
<i>B. Return to Experience</i>				
Estimator:	OLS	IV	Singly-censored bivariate regression	Singly-censored bivariate regression-IV
Sample:	Non-limit and ≥ 35 hours/wk.	Non-limit and ≥ 35 hours/wk.	Reservation wage sample	Reservation wage sample
Dependent variable:	Wage	Wage	Wage	Wage
Variable	(1)	(2)	(3)	(4)
Experience	0.0025 (0.0021)	0.0154 (0.0228)	0.0142 (0.0015)	0.0260 (0.0214)
R-square/ lnL	0.133		-465.2	
Sample size	466	466	1548	1548

Notes: Standard errors in parentheses. In addition to variables shown, all models include all variables shown in Table 3 plus a missing-education dummy. The model in column (4) of panel B also includes the residual from the first stage regression. Results for reservation wage equations in singly-censored bivariate regression models are not shown.