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Selection Bias in the Evaluation of Prison-Based Drug Treatment Programs

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1.0 Introduction

The randomized experimental design is the gold standard for evaluation research. The simplest version of this design requires that members of an eligible population be randomly assigned to either a treatment group or to a control group. Provided that other factors do not contaminate the experiment, comparing the outcomes for the treated group and the untreated group provides an unbiased measure of the average treatment effect.

Despite its appeal, a randomized design may be impractical in some settings, such as justice populations, where due process restricts randomization of otherwise equivalent populations to treated and untreated conditions. Even when implemented, randomized experiments often collapse as agencies thwart researchers' evaluation plans or subjects refuse to cooperate. Much of what researchers know (or think they know) about treatment programs comes from evaluations based on quasi-experimental designs. A quasi-experiment typically uses statistical controls in place of random assignment to establish an assumed equivalency between a treated group and a (generally) nonequivalent comparison group. The statistical control is sometimes compelling, but rarely convincing, because it does not transform *association* (treated subjects tend to have better outcomes) into *causation* (treatment causes better outcomes), as randomization does. Quasi-experimental designs invariably end with the caveat: "These findings might represent a treatment effect, but we cannot be sure because …" Still, not all quasi-experiments are created equal. Some have a long list of caveats, while for other quasi-experiments, the qualifications might be relatively innocuous. Indeed, a well-designed quasi-experiment can provide strong evidence for rejecting a null hypothesis that a program has no appreciable treatment effect.

The purpose of this study is to evaluate the effectiveness of a within-prison substance abuse treatment program at improving specified post-release behaviors of those inmates who received treatment. This evaluation was a quasi-experiment because the Federal Bureau of Prisons could not randomly assign inmates who abused substances to treated and untreated conditions. As is true of most substance abuse treatment outcome evaluations, the principal analytic problem was to deal with potential selection bias.

Economists and others have used selection bias adjustments for a long time, but there has been a recent flurry of research applying this approach to quasi-experiments. In the late 1970s, Heckman (1979) developed an influential approach for dealing with selection bias that some researchers took to be a solution, at least within the context where it could be applied.[1] Here, we refer to that approach as "Heckman-type" adjustments. Unfortunately, subsequent research has shown that Heckman's solution rests on strong distributional assumptions, and results are sensitive to getting those assumptions right (for example, LaLonde, 1986). This would be no problem if the assumptions were testable, but in many cases they are not or else the test lacks power. In his influential paper, LaLonda (1986) demonstrated that any quasi-experiment using Heckman's approach to control for selection bias could yield estimates of the treatment effect that suffered from large biases. Some methodologists may even have regarded LaLonda's demonstration as the end of quasi-experimental

[1] Heckman suggested a two-equation estimator. The first equation described the selection process, and the second described the post-treatment outcome. Parameter estimation required identification conditions, typically, that some of the variables that entered the first equation did not enter the second equation. This condition is difficult to satisfy in many practical settings.

design as a method for evaluating treatment programs (see Burtless, 1995). Such an assessment would be premature, because methods for dealing with selection bias continue to evolve (Manski and Nagin, 1998).

Heckman and his colleagues (for example, Heckman and Smith, 1995) have argued that LaLonda overstated the case against dealing with selection bias encountered by quasi-experimental design. Whatever the merit of their case, LaLonda's paper galvanized the development of alternative ways of dealing with selection bias. Recent theoretical expositions include Smith (1997), Heckman, Ichimura, Smith and Todd (1998), and Dehejia and Wahba (1999).[2] Those recent papers have stimulated our own approach to dealing with selection bias in quasi-experimental design.

2.0 Problem Statement

The Federal Bureau of Prisons (BOP) experimented with using in-prison therapeutic community treatment programs to improve the post-release behavior of drug-involved offenders following release from the Bureau=s custody. Using institutional records to establish baseline conditions, and other public records to monitor post-release behaviors, the Bureau sought to learn whether or not treatment:

- reduced revocations for offenders housed in halfway house confinement following release from prison;
- increased the percentage of time that offenders were employed following release from prison and halfway house confinement;
- decreased the rate of relapse to drug use (based on urine testing) following release from prison and halfway house confinement; and
- decreased the rate of criminal recidivism, defined alternately as:
    - being arrested following release from prison and halfway house confinement, and
    - being arrested or otherwise having supervision revoked during the period following release from prison and halfway house confinement.

The Bureau was unable to assign subjects randomly to treatment and to no treatment conditions, so it devised a quasi-experimental design to test for treatment effectiveness. Some Federal prisons had therapeutic community treatment programs (hereafter DAP facilities) and others did not (hereafter nonDAP facilities). Prisoners in DAP facilities did not differ materially from prisoners in nonDAP facilities, so the two populations were comparable for evaluation purposes. Within the DAP facilities, some offenders were offered and accepted treatment (hereafter the **DAP *treatment group***) while others either were not offered treatment or declined treatment that was offered (hereafter the **DAP *comparison group***). Of course, those offenders who were housed in nonDAP facilities did not receive treatment (hereafter the **nonDAP *control group***).

[2] At least one approach, the use of instrumental variables, predates La Londa. See the discussion in Maddala, 1983, chapter 9. Nevertheless, our impression is that attention to this approach has accelerated since LaLonda's paper.

The Bureau wanted to learn whether or not treatment improved the post-release performance for those who received treatment. However, the Bureau was concerned that a simple comparison of the outcomes for offenders who were treated (the DAP treatment group) with the outcomes for offenders who were not treated (the nonDAP control group and the DAP comparison group) could be misleading because of selection bias. In this case, the concern was that some unmeasured factors (such as motivation to change) that affect the decision to enter treatment might also affect post-release performance, so the relationship between treatment and post-release performance could be partly or wholly spurious. In addition to including control variables in a regression model, the Bureau adopted two analytic methods for dealing with selection bias: a *standard instrumental variables approach* and a *Heckman selection bias approach*.

The instrumental variable approach is the most straightforward. Because a prisoner=s assignment to a specific prison had nothing to do with whether or not he needed substance abuse treatment, selection bias does not affect a comparison between the outcomes for the nonDAP control group and the *combined* outcomes for the DAP treatment and comparison groups. To illustrate this approach, suppose that every prison holds identical populations comprising: those who would enter treatment if offered to them and those who would not enter treatment if offered to them. When treatment is offered, these populations can be identified, and when treatment is not offered, they cannot be identified. Let:

Paccept	The percentage of a prison population that would accept treatment if given the opportunity. Call this group A.
$1\text{-}P_{accept}$	The percentage of a prison population who would decline treatment if given the opportunity. Call this group B.
F_{accept}	The fraction of group A who would recidivate if treatment were not provided.
$F_{decline}$	The fraction of group B who would recidivate.

Then if treatment were provided to no one, the rate of recidivism among group A and group B combined can be written:

$$F_{untreated\ population} = P_{accept}\ F_{accept} + (1\text{-}P_{accept})F_{decline}.$$

This is the expected value of the observed proportion of failures in the nonDAP control group.

Suppose that, on average, treatment reduced the proportion of inmate who recidivate by an amount D. If treatment were provided to everyone who would accept it:

$$F_{treated\ population} = P_{accept}\ (F_{accept} - D) + (1\text{-}P_{accept})F_{decline}$$

Here D is the treatment effect. $F_{\text{treated population}}$ is the expected value of the observed proportion of failures in the combined DAP groups. A test of treatment effectiveness can be based on the differences between two observables: $F_{\text{treated population}}$ and $F_{\text{untreated population}}$. Some algebra shows that the expected value of the effect from treatment is:

$$D = (F_{\text{untreated population}} - F_{\text{treated population}})/P_{\text{accept}}.$$

This is one illustration of an instrumental variable approach to quasi-experimental design. It affords an estimate of the average treatment effect D and a measure of its statistical significance despite the fact that the treated and untreated groups may have failure rates that differ from each other for reasons that have nothing to do with the receipt of treatment.

The instrumental variable approach to evaluating treatment effectiveness is not much complicated by introducing control variables and using regression models. The introduction of control variables has three benefits: By reducing unexplained variance, the regression can reduce the standard error of estimate for the treatment effect. Second, the control variables can help adjust for any population difference between DAP and nonDAP facilities. And, third, the parameters associated with control variables have policy relevance for the Bureau.

The key is to develop a suitable instrument (Davidson and MacKinnon, 1993, for example). Suppose an analyst were to combine data from all three sources (nonDAP controls, DAP comparisons, and DAP treatment), assign a dummy variable coded one to those who received treatment and coded zero for those who did not, and then regress the outcome variable on this dummy variable and any control variables that seem appropriate. The problem with this approach is well known. The estimated regression parameter associated with the dummy variable will be biased and inconsistent if the dummy variable and the error term are not independent. Independence seems unlikely if any unmeasured factor (such as motivation) affects both the receipt of treatment and the outcome variable.

A solution is to identify an instrumental variable that is highly correlated with the dummy variable but that is distributed as independent of the error term. One suitable instrument is the estimated probability of entering and completing treatment, where this instrument might be estimated from a probit model. The dependent variable in the probit model is a dummy variable indicating whether or not the offender entered treatment. This probit model is estimated using just those data from the DAP subjects, since the nonDAP subjects have a zero probability by definition, so the instrument is set to zero for them. By substituting the instrument (the estimated probability of being treated) for the dummy variable, and estimating the regression, the parameter estimate associated with the instrument provides an estimate of the average treatment effect that is free of selection bias.

A second approach, called herein the *Heckman selection bias* approach (Heckman, 1979; Maddala, 1983) is somewhat more difficult to apply than is the instrumental variable approach. It requires the analyst to *jointly* model the selection into the sample and the post-release outcome. Here, note that the selection bias approach has much in common with the standard instrumental variable approach, and if the analyst is willing to limit his analysis to a linear-additive regression model, there is little to recommend the selection bias approach over the instrumental variable approach. However, as explained by Maddala (1983, p.261), the Heckman selection bias model can be used to study more complicated models where treatment interacts with other variables.

Appendix B provides a technical exegesis of the Heckman-type statistical models used in this study. There are four models:

- a lognormal survival model
- an exponential survival model
- a probit model
- a two-limit tobit model

Each of these four models has an adjustment for selection bias.

3.0 Findings

The Federal Bureau of Prisons sought to learn whether or not substance abuse treatment could improve post-prison release outcomes on several dimensions of behavior. This section first describes the outcome measures and the generic statistical models applied to evaluate the effectiveness of treatment for those outcome measures. Second, it discusses diagnostic tests that influenced the selection of specific parametric forms for each generic survival model. Finally, it presents findings in an abbreviated form; appendix B provides detailed findings.

Findings are reported for estimates of the treatment effect for each of three models: the traditional dummy variable model, the instrumental variable model, and the Heckman-type model. This joint presentation allows us to investigate how results differ when selection bias is taken into account. It also affords a comparison of results derived from the two selection bias models.

3.1 Generic Models

The Bureau was understandably interested to learn whether or not substance abuse treatment could reduce criminal recidivism. One way to define criminal recidivism is "being arrested for a new crime during a follow-up period." This criterion can be applied to all study subjects except for a few cases with missing data. For obvious reasons, a survival model is a useful way to study criminal recidivism, and we adopted a survival model here. The specific parametric assumptions will be discussed in section 3.2.

Applying the above criterion variable to all study subjects is potentially problematic. Federal probation officers supervised most but not all study subjects. (In the federal system, probation officers supervise both probationers and offenders released from prison on parole or supervised release. However, they only supervise offenders who have either been sentenced to supervised released or who have been released from prison before completing their entire prison term. Therefore, a few offenders complete their entire terms and are not supervised.) The supervision process itself may either affect behavior or affect what is observed about behavior, so we also applied a survival model to just those offenders who were supervised.

Still, treating an arrest as the criterion variable is problematic even when the statistical analysis is limited to people under supervision. The problem is that people under community supervision can be returned to prison for technical violations that have nothing to do with an arrest. Thus, a revocation for a technical violation is a *competing event*. Unless the competing event is independent of an arrest event, the parameters associated with the survival analysis will be biased and inconsistent. Because

similar underlying processing (such as a return to drug use) can trigger an arrest and a technical violation, assuming independence may be unwarranted. One way to deal with this problem is to treat the criterion variable as either an arrest or a revocation, and that is what we have done in a third approach to evaluating treatment outcomes.

Relapse to drug use is an entirely different outcome variable. We know about the relapse to drug use from a urine test that is positive for an illicit substance. (When a person refused a urine test, the assumption is that he or she would have failed it.) A survival model again seems like a reasonable approach, but it is only applicable to people who were subjected to urine testing, so we limited the analysis to those who were (1) under supervision and (2) subject to urine testing. Note that the intensity of urine testing decreases over time for those who successfully avoid testing positive. Of course, this means that the probability of being detected decreases over time, and consequently the estimates of the survival function for relapse to drug use conflates behavior by people under supervision (drug use) with behavior by probation officers (monitoring for drug use). This is not a problem for our analysis provided we interpret the findings appropriately. That is, judgement of "success" following release from prison comes from a combination of an objective urine test and a subjective expert judgement by a probation officer.

The Bureau was also wanted to test whether or not substance abuse treatment had a salutary effect on employment. The percentage of time employed during the follow-up period was deemed a suitable outcome measure. The analysis was limited to offenders who were under supervision because probation officers provided the employment data. We used a two-limit tobit model (0 percent employment and 100 percent employment limits) to test the treatment effect. A second criterion was the level of employment during the follow-up period. Because the level of employment was coded on an ordinal scale, we used an ordered probit model. This second model was applied only to people who were members of the workforce.

Finally, the Bureau sought to learn whether or not treatment improved performance in halfway house placements. Many federal offenders go through a transition period between prison and community supervision, during which they are placed in a halfway house. While in the halfway house, they are at liberty to work or go to school during the day, but they must return to the house when not occupied with approved activities. Not all federal offenders serve time in a halfway house, so the analysis was restricted to those who did serve time. The planned duration of time in a halfway house varies across offenders, so we adopted a survival model to study the time until failure (revocation from a halfway house placement) where successful completion was the censoring event.

3.2 Diagnostics

We performed several diagnostics for testing model specification. We used the instrumental variable approach in the first two diagnostic tests of the survival models. The first test was to fit three alternative versions of parametric survival models based on the lognormal, exponential and Weibull distributions. We selected the distribution with the highest likelihood as the "best model" because it provided the best fit to the data.

Of course, the best model is not necessarily a good model. The second test was to plot the integrated hazard for the selected model on the horizontal axis against minus the logarithm of the integrated hazard on the vertical axis. We used an approached recommended by Lancaster (1990, page 312) to

develop those graphs. If the model is a good one then the plot should fall on a 45 degree line. We judged whether or not the model was acceptable by inspection.

A third test was to compare the parameter estimates for the treatment effect provided by two models: the instrumental variable model and the Heckman-type adjustment model. Both should yield similar but not necessarily identical estimates. If they do not, they we would be suspicious of the distribution assumptions made about the mixture distribution adopted in the Heckman-type adjustment model.

3.3 Results

This section discusses the results of the diagnostic tests, and steps taken in response to those tests. Also, it presents findings from testing whether or not substance abuse treatment had a positive effect on post-release outcomes. Because the focus is on treatment outcomes, we only provide parameter estimates for the treatment variable in this section. Complete regression results appear in appendix B. When presenting the results, we differentiate men from women, because the Bureau wanted evaluations distinguished by gender. By gender, we report parameter estimates (slope coefficients) and associated t-scores from each of three methods used to generating estimates – the traditional approach of using a dummy variable to represent the receipt of treatment, the instrumental variable approach, and the Heckman-type adjustment approach.

The first test of treatment effectiveness used an arrest as the criterion variable and included 2099 men and 547 women in the analysis. Using the instrumental variable approach, we estimated survival models based on the lognormal, the exponential, and the Weibull distributions. Table 1 reports the values for minus the log-likelihood. The value closest to zero denotes the best model. On the basis of that test, we selected the exponential as the best survival distribution for men and the log-normal as the best survival distribution for women.[3]

Table 1

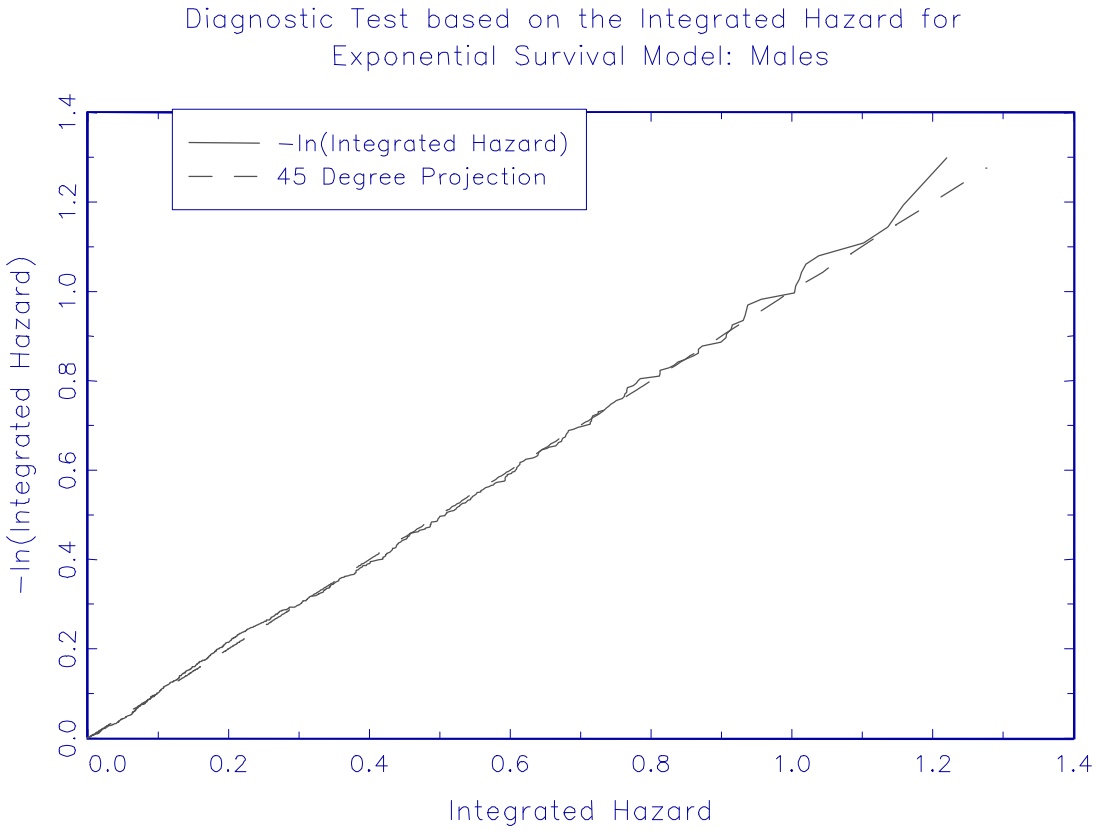
Diagnostic Tests of the Three Survival Models

		Minus Log-likelihood			
Outcome	Gender	Lognormal	Exponential	Weibull	Sample Size
Arrest, all offenders	Male	-2111	-2101	DNC	2099
Arrest, all offenders	Female	-360	-362	-361	547
Arrest, those supervised	Male	-1745	-1735	DNC	1842
Arrest, those supervised	Female	-257	-264	-258	473
Arrest or Revocation	Male	-2329	-2301	-2300	1842
Arrest or Revocation	Female	-425	429	-427	473
Relapse to drug use	Male	-2444	-2480	-2447	1692
Relapse to drug use	Female	-522	-531	-521	430
Halfway house failure	Male	-1010	-1012	-1002	1476
Halfway house failure	Female	-216	-213	-211	409

[3] The model based on the Weibull will always have a larger likelihood than the model based on the exponential, which is a special case of the Weibull. Unless the Weibull was significantly better than the exponential, we adopted the exponential.

The second test plots the integrated hazard against minus the logarithm of the integrated hazard. Plots, based on the best model as determined by the likelihood comparison, appear in figures 1and 2.

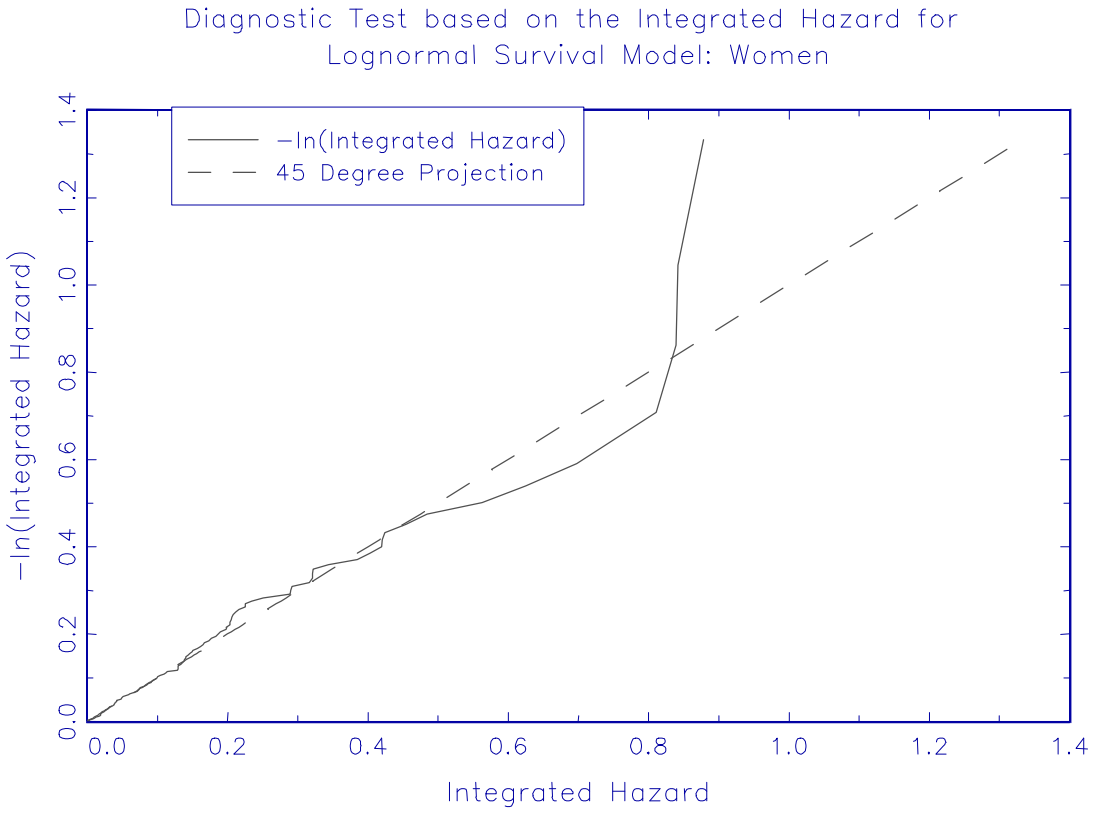
Figure 1



For men, the diagnostic test leads to the conclusion that the exponential is a suitable distribution for modeling the time until an arrest. For women, however, the test calls the distributional assumption into question. The model based on the log-normal performs well for the early part of the integrated hazard, but it does not perform well for the latter part. This observation will have consequences for the analysis.

About one of three men were arrested during the follow-up period. Other descriptive statistics regarding those men, and complete regression results, appear in appendix B. Table 2 reports the parameter estimates and t-scores for just the treatment effect. A t-score of –1.65 would be statistically significant at p=0.05 using a one-tailed test of significance, which seems justified given that we do not expect treatment to do any harm. For readers who prefer a two-tailed test, a t-score of –1.95 would be significant at p=0.05. Readers with other preferences regarding standards for statistical significance can set their own thresholds for determining when a result is statistically significant and when it is not.

Figure 2



Depending on the criterion for rejecting the null hypothesis, all three approaches agree that for men the treatment effect is statistically significant. (A negative parameter denotes a favorable treatment effect in the exponential model.) Two other comparisons are important, however. The first is that the instrumental variable approach and the Heckman-type adjustment approach produce parameter estimates that are larger than the estimate for the dummy variable model. The second is that the two methods used to adjust for selection bias yield estimates that are roughly consistent with each other, although they are not identical. Clearly an analyst should not be indifferent toward controlling for selection bias in this context.

For women, none of the three approaches suggest that treatment was effective at reducing criminal recidivism. The parameter estimates have the expect signs (positive denotes a favorable treatment effect in the log-normal model), but none approach statistical significance. Perhaps treatment did not work for women, but we have to be suspicious of the fact that, while the log-normal is the best of the three distributional assumptions maintained in this study, figure 2 showed that the log-normal is not especially descriptive of recidivism.

Table 2

Parameter Estimates and T-Score for Estimated Treatment Effect

		No Adjustment		Instrumental Variable		Heckman-Type	
		parameter	t-score	parameter	t-score	parameter	t-score
Arrest, all offenders	Males	-0.178	-1.851	-0.285	-1.989	-0.425	-2.315
Arrest, all offenders	Females	0.116	0.416	-0.005	-0.011	0.091	0.193
Arrest, those supervised	Males	-0.150	-1.529	-0.203	-1.335	-0.297	-1.511
Arrest, those supervised	Females	0.201	0.727	0.151	0.320	0.304	0.725
Arrest or Revocation	Males	-0.161	-2.165	-0.252	-2.193	-0.397	-2.797
Arrest or Revocation	Females	0.242	1.258	0.226	0.719	0.152	0.507
Relapse to drug use	Males	0.344	2.868	0.462	2.453	0.784	3.256
Relapse to drug use	Females	0.382	1.367	0.436	0.942	0.328	0.751
Employment	Males	0.033	1.188	0.023	0.532	0.066	1.188
Employment	Females	0.103	2.176	0.131	1.692	0.133	1.678
Employment Level	Males	0.026	0.450	0.036	0.270		
Employment Level	Females	0.185	1.420	0.508	2.070		
Halfway house failure	Males	0.525	4.505	0.194	0.764		
Halfway house failure	Females	0.389	1.470	0.470	0.927		

We tried two approaches to deal with the problem that the log-normal did not seem adequate to model the survival times. First, we censored the follow-up period at 12 months and at 18 months to see if any of the three maintained distributions worked better over a shorter span. They did not; the same diagnostic problems persisted. Second, we combined the instrumental variable approach with a Cox proportional hazard model, which does not impose any distributional assumptions. (It does, of course, impose restrictions on the hazards.) The resulting t-score was only –.06. Consequently, we conclude that treatment effectiveness has not been demonstrated for women, at least when using arrests for the entire population as the criterion.

As mentioned earlier, using an arrest as a criterion variable is problematic when the analysis is based on all offenders, because some were not under supervision when released from prison. An alternative approach is to limit the analysis to those who were under supervision. A total of 1842 men and 473 women were supervised and enter the following analysis.

Table 1 shows the results from the first diagnostic test. Those tests caused us to again select the exponential as the best way to represent the failure time for men and the log-normal as the best way to represent the failure time for women. The second diagnostic, the plots based on the integrated hazard, was similar to the plots for men shown above, so we do not show new plots here. For women, the plot of the integrated hazard (figure 3) is much improved, suggesting that the log normal is an acceptable failure time distribution.

Figure 3

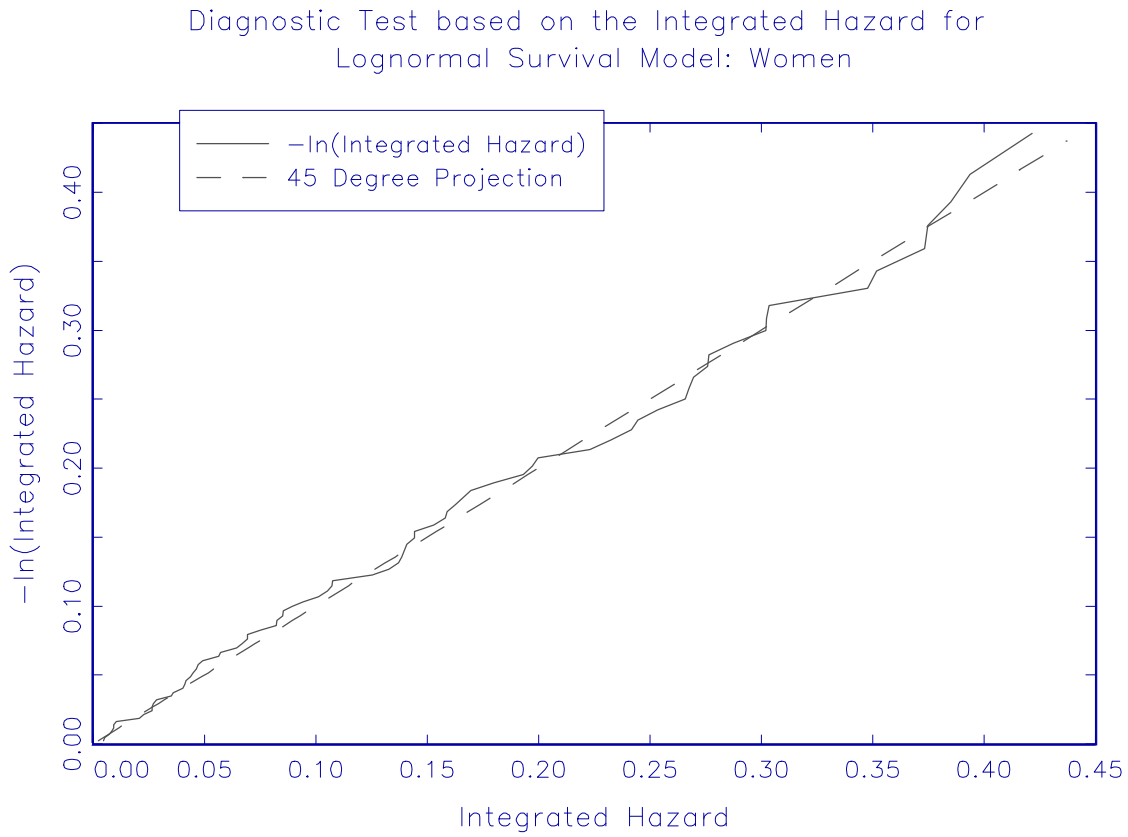


Table 2 summarizes results. About 33 percent of the men and 17 percent of the women were arrested. For men, the treatment effect would be judged as statistically significant only at p=0.10 in a one-tailed test. The models that account for selection bias increase the size of the treatment effect parameter but that finding would seem to be inconsequential given the small values for the t-scores. For women, the models agree that there is no significant treatment effect.

Also as mentioned earlier, using an arrest as a criterion of failure is problematic because revocation for a supervision violation is a competing event. That would not change the way we look at the problem if an arrest and revocation could be treated as stochastically independent, but an assumption of independence seems unjustified. A new model treats the outcome as either an arrest or a revocation.

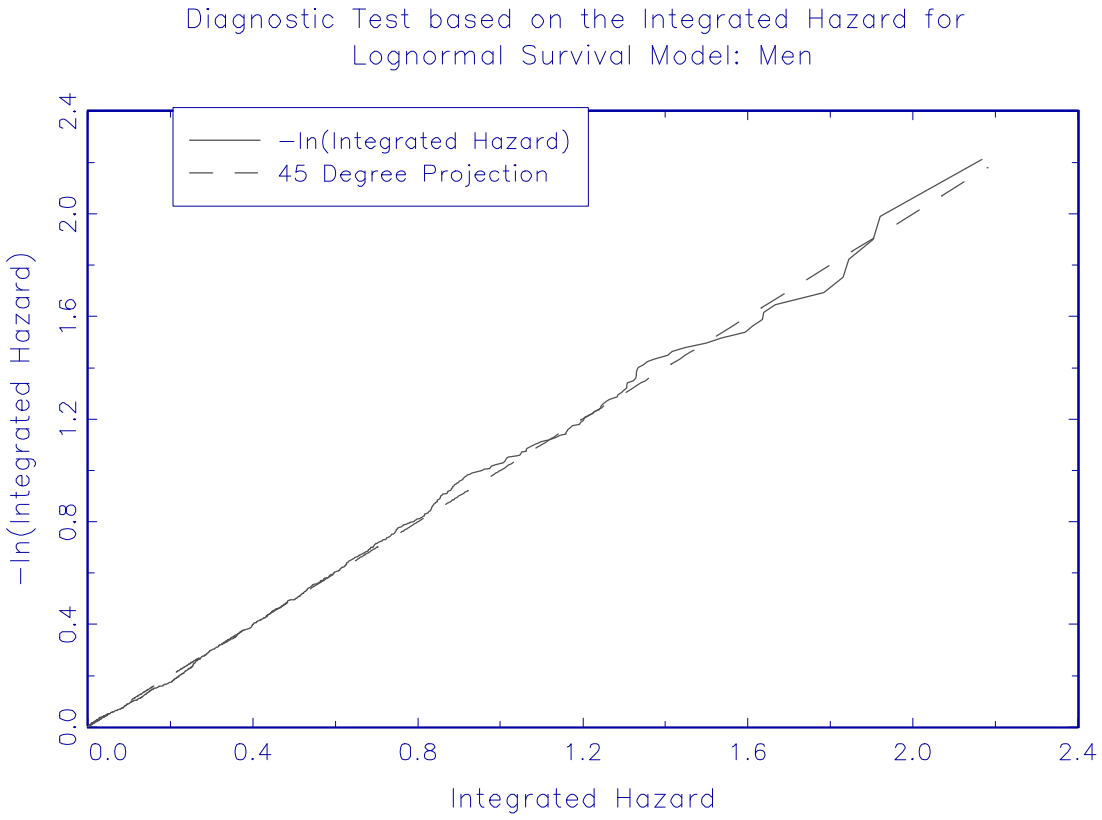
Diagnostic tests for this new model again lead us to adopt a survival model based on the exponential distribution for men and a model based on the log-normal distribution for women. See table 1. The plots of the integrated hazard were similar to the previous plots, so we do not show them.

All three methods of estimating the treatment parameter agree that the treatment effect is statistically significant for men. The two methods used to adjust for selection bias yield roughly similar parameters, which are larger than the treatment effect estimated in the dummy variable model. In fact, once we have controlled for selection bias, the treatment effect is nearly double or triple what we would otherwise estimate.

For women, the three approaches agree that substance abuse treatment does not seem to improve the post-release outcomes for women, at least when those outcomes are judged by an arrest or revocation. When the follow-up period is censored at 18 months, the parameter estimate was not statistically significant (t=0.33). A Cox proportional hazard model lead to the same findings. This leads us to infer that the treatment effect for women is not large and that model misspecification is probably not the explanation.

Next we analyzed the time until relapse to drug use. We could only do this for study subjects who were supervised and had their urine tested as a condition of supervision. There were 1692 males and 430 females. Diagnostic tests (see table 1) suggested that the lognormal model was better than the exponential model for both men and women. For women, the Weibull model was slightly better than the lognormal. The difference was slight, however, and given that we had not developed a Heckman-type adjustment correction for the Weibull, we adopted the lognormal.

Figure 4



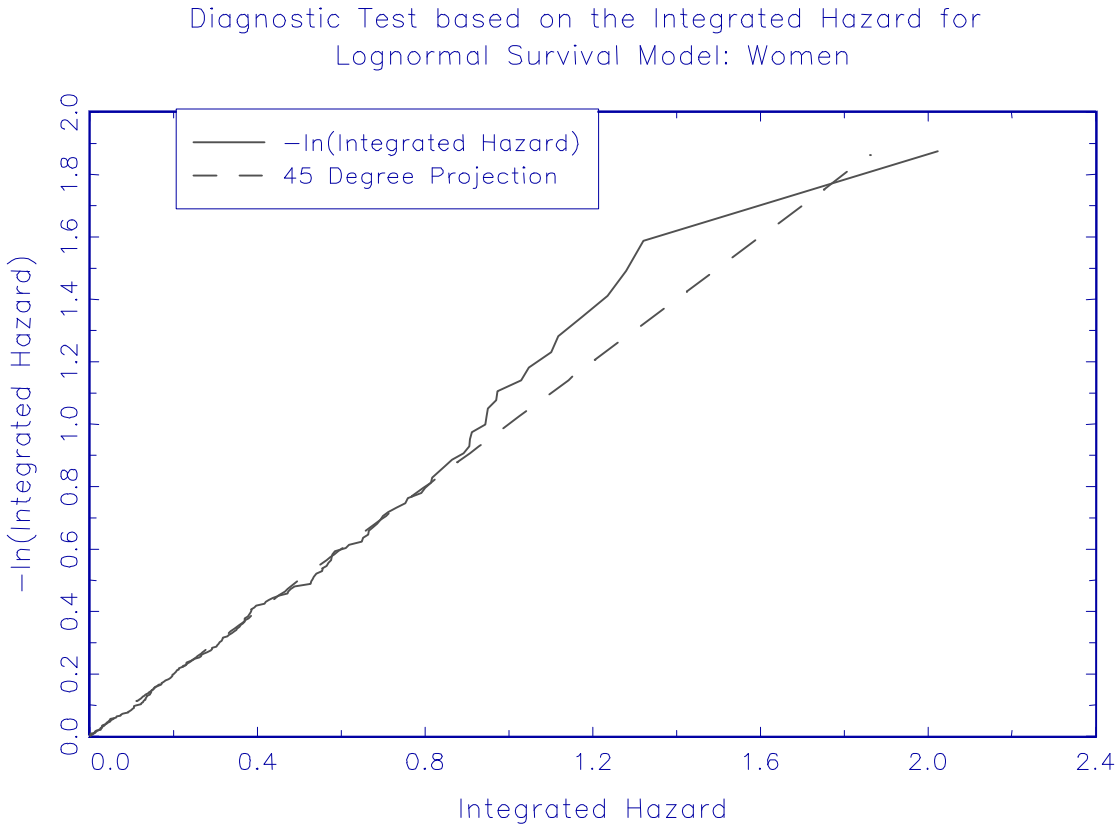
The second diagnostic, based on the integrated hazard plots, seemed to show that the assumption of the log-normal distribution was acceptable for men. See figure 4. However, we encountered the familiar problem for women. The model "worked" in the range of lower integrated hazard scores but not when the scores got larger. See figure 5.

Table 2 summarizes the regression results. For men, all three models agree that substance abuse treatment is effective at reducing subsequent relapse to drug use. The parameter estimates are highly

significant. Moreover, the parameter estimates for the two methods that adjust for selection bias are in substantive agreement, and both offer parameter estimates that are almost three times larger than what is derived from the dummy variable approach that does not adjust for selection bias.

We would judge that, for men, substance abuse treatment has a demonstrably favorable effect on reducing relapse to drug use. This does not appear to be the case for women. The test of the treatment effectiveness does not approach statistical significance in any of the three models. Restricting the follow-up period to 12 or 18 months did not improve the integrated hazard plot, nor did the findings change substantively. The treatment effect was not statistically significant in a Cox proportional hazard model

Figure 5



Findings reported to this point tell a coherent story. Substance abuse treatment reduced relapse to drug use for male offenders. A lower rate of relapse to substance abuse apparently leads to a lower level of criminal offending. We have not explicitly tested this assertion in a causal setting, but it seem like a reasonable (although tentative) inference to draw from these findings. Furthermore, treatment did not seem to reduce relapse rates for women, and if drug use "explains" criminal recidivism, then we would not expect treatment to affect criminal recidivism. Evidence is consistent with that tentative conclusion.

Next, we examined employment during the supervision period following release. The outcome measure was the percentage of time spend working. It was censored at 0 percent and 100 percent, so we used a two-limit tobit model to test whether or not substance abuse treatment increased employment rates. Men were employed 68 percent of the time on average during the follow-up period. Women were employed 59 percent of the time. Table 2 summarizes findings.

None of the three approaches suggests that treatment has improved employment rates for men. Curiously, all three approaches suggest that being treated improved employment rates for women. This is curious because there is no evidence that treated reduced the relapse to drug use for women, so it is difficult to tell a story how substance abuse treatment leads to improved employment outcomes. We should note, however, that the effect was not highly significant.

Another way to look at employment as a criterion value is to characterize work as (1) full time during all of the follow-up period, (2) full time during part of the follow-up period, (3) part-time, and (4) no job. We analyzed this outcome using the dummy variable approach as well as the instrumental variable approach. Results are summarized in table 2. Parameter estimates from the dummy variable model were virtually identical to estimates from the instrumental variable model. There is no that substance abuse treatment affected employment levels. However, the instrumental variable model suggests that the employment level improved for women. Given the previous finding – that the rate of employment improved for women following treatment – we might conclude that treatment improved post-release employment for females.

The final outcome considered in this study is failure during a halfway house placement. A total of 1476 men and 409 women entered halfway houses, and those men and women were the subject of this analysis. The outcome variable was failure for any reason during that placement. Note that the time spent in a halfway house predates time spent under probation supervision, as that term was defined earlier.

Twenty-three percent of the men and seventeen percent of the women failed at a halfway house placement. Diagnostics indicated that neither the log-normal nor the exponential were suitable candidate distributions for the survival analysis, because each was clearly dominated by the Weibull distribution. See table 1. The dominance of the Weibull distribution caused some problems, because we had not written a version of the Heckman-type adjustments based on the Weibull distribution. Instead, we used the routine programming in LIMDEP, but we only estimated the dummy variable model and the instrumental variable model. The results are summarized in table 2.

The dummy variable model would cause us to conclude that substance abuse treatment had a strong and beneficial effect on halfway house placement outcomes for men. The t-score is of unquestionable significance at 4.51. After adjusting for selection bias, however, the story is much different. We have no reason to believe that substance abuse treatment improves the halfway house experience. If only the dummy variable model were considered, an analyst might mistakenly conclude that substance abuse treatment helped offenders succeed in this transitional period between prison and community supervision.

For women, there is no disagreement between the two models. Substance abuse treatment seems to have no statistically significant effect on the halfway house experience.

Before leaving this section, it is worthwhile to return to table 2 and note how the parameter estimates associated with the treatment effect vary by estimation method. The three approaches yielded different estimates for men, and the treatment effect was stronger when estimated by methods that controlled for selection bias. The Heckman-type adjustment model helps explain the reasons for these differences.

The Heckman-type adjustment model provides an estimate of the correlation of the error terms that affect the selection into treatment and the failure rates, respectively. Table 3 summarizes estimates of those correlations and reports t-scores for those estimates. The likelihood functions used a transformation, so the correlation is represented as:

$$\rho = 2 - \frac{1}{1+e^{n}}$$

where p is the correlation. Allowing n to vary freely, ρ is constrained to fall between 0 and 1. The appendix reports n and its standard error. Table 3 reports ρ.

Our experience with selection bias models suggests that estimates of ρ typically have high standard errors, so one should probably not take a lack of statistical significance to mean there was no selection bias.[4] Nevertheless, for men the estimate of ρ is significant in two of the five regressions, and it seems to be sizable. The direction of the correlation suggests that the worst risks – those most likely to be rearrested and those most likely to relapse to drug use, holding observable covariates constant – are most likely to enter treatment. Therefore, the models that adjust for selection bias tend to estimate a stronger treatment effect than do models that do not adjust for selection bias.

For women, on the other hand, the estimated correlations are never large and they never approach significance. This explains why for women the models that adjust for selection bias give parameter estimates that are very similar to models that do not adjust for selection bias.

Table 3

Estimated Covariance Terms

	males		females	
	covariance	t-score	covariance	t-score
Arrest, all offenders	0.530	0.965	0.011	0.066
Arrest, those supervised	0.650	0.612	-0.085	-0.323
Arrest or revocation	0.822	2.483	0.050	0.399
Relapse to drug use	0.161	2.074	-0.161	0.162
Employment	0.048	-0.681	0.056	-0.474

[4] One might treat the t-score associated with the parameter ρ as a test of the null hypothesis of no selection bias. The problem with this approach is that it can lead implicitly to acceptance of the null hypothesis, which has no justification under statistical theory.

3.4 The Size of the Treatment Effect

Table 2 reported estimates and tests for significance of the parameters associated with the treatment effect. Because the statistical models are nonlinear, the parameters are difficult to interpret. Here we translate those estimates into metrics that are easier to understand.

We estimated the mean value for each variable that entered every regression. Of course, these means varied from regression to regression, because each regression used a somewhat different variable set and data. Those means are reported in a technical appendix. Using those means, together with the parameter estimates reported in a technical appendix, we computed the probability of failure (arrest, technical violation, and relapse to drug use) for the "average" offender within two years of release from prison. All calculations were based on the instrumental variable model. Those probabilities are reported in the third column of table 4.

We repeated this calculation after substituting a zero in place of the mean treatment effect. This provided an estimate of the probability of failure for someone who was not treated. Then we replicated the calculation after substituting a one in place of the mean treatment effect. This provided an estimate of the probability of failure for someone who was treated. The untreated estimate appears in column four; the treated estimate appears in column five.

Table 4

Estimated Failure Rates for Treated and Untreated Offenders

Failure Rates within Two Years

			Failure Rates	
Outcome	Gender	Overall	Without Treatment	With Treatment
Arrests, all offenders	Male	0.347	0.376	0.306
Arrests, all offenders	Females	0.161	0.160	0.161
Arrests, those supervised	Male	0.332	0.353	0.303
Arrests, those supervised	Females	0.167	0.175	0.153
Arrest or revocation	Males	0.490	0.525	0.443
Arrest or revocation	Females	0.278	0.297	0.245
Relapse to drug use	Males	0.550	0.585	0.499
Relapse to drug use	Females	0.398	0.426	0.350

As already mentioned, treated males tend to recidivate at lower rates than untreated males. The estimates vary with model specification. For an arrest, regardless of parole supervision status, an estimated 38 percent of the untreated group would recidivate compared with 31 percent of the treated group. For an arrest limited to males who were supervised post release, an estimated 35 percent of the untreated group would recidivate compared with 30 percent of the treated group. For an arrest or revocation, given post release supervision, an estimated 53 percent of the untreated group would recidivate compared with 44 percent of the treated group. Of course, judgement is subjective, but these appear to be sizable treatment effects.

For women, we found no statistically significant treatment effect. Consistent with that, treated and untreated females are arrested at about the same rates. We found no significant effect for treatment when an arrest or revocation was used as the criterion variable either. The point estimate, based on the "average" offender, implied that 30 percent of the untreated women would recidivate compared with 25 percent of the treated women. Perhaps a larger sample would have shown a statistically significant treatment effect for women, but we cannot know for sure.

Considering relapse to drug use, an estimated 59 percent of untreated men would relapse compared with 50 percent of treated men. Relapse rates are high whether people are treated or not, but those who are treated do much better than those who are not treated. An estimated 43 percent of untreated women relapsed compared with only 35 percent of those women who were treated. Again, the differences are large, but not statistically significant. Perhaps the sample is too small to provide reliable estimates for women.

Because the treatment effect was not generally significant when we considered employment and relapse to drug use, there seems to be no reason to translate the treatment effect into a new metric. The possible exception is that treatment may have improved the employment of women during the follow-up period. According to the results from the tobit model, treatment increased the proportion of time employed by about 13 days per 100 days released conditional on being employed sometime during the follow-up period. Once we remove the conditioning, the estimate is somewhat less than 13 days, but nevertheless, this provides a measure of scale.

4.0 Conclusions

Evaluations based on quasi-experimental designs are seldom definitive, but evidence from some quasi-experiments is more compelling that evidence from other quasi-experiments. In the substance abuse treatment field, evaluations have seldom risen to the level of "compelling" because they have failed to deal with a crucial issue: selection bias. Thus, although treatment outcome evaluations exist, evidence of treatment's effectiveness is lacking.

The Federal Bureau of Prisons could not implement a randomized field experiment to evaluated substance abuse treatment. Nevertheless, it found a creative way to conduct a quasi-experiment. It identified a group of offenders who were not eligible for treatment because they had not been housed in those facilities that offered treatment programs (nonDAP facilities). Because of the way that inmates are placed in BOP facilities, those inmates who were housed in nonDAP facilities were representative of those inmates who were housed in DAP facilities that offered substance abuse treatment. Comparing the outcomes for offenders housed in nonDAP facilities (none of whom were treated) with the outcomes for offenders who were housed in DAP facilities (some of whom were treated) provides a contrast that is valid regardless of selection into treatment. The quasi-experiment exploits this contrast.

Following this same logic, each of the DAP facilities introduced treatment at different times. Depending on where an inmate was housed, then, that inmate would have a greater or lesser chance of entering treatment. Our approach to dealing with selection bias also exploits this variation in the probability that an offender would enter and complete treatment. We judged the effectiveness of

treatment by looking for a correlation between the estimated probability of being treated and the post-release outcomes of interest to us. Selection bias does not affect that correlation.

This evaluation design could be implemented in other settings, including other prison settings where the comparability of treated subjects (none of whom received treatment) and treated subjects (some of whom received treatment) can be entertained. It may be applicable in still other settings, such as when people from a pre-program implementation period (none of whom were treated) can be treated as comparable to people from a program period (some of whom were treated). Still other settings would seem suitable – such as courts where some judges never assign defendants to treatment and other judges do so differentially.

By dealing explicitly with selection bias, we have attempted to provide an evaluation of substance abuse treatment that is compelling if not convincing. Indeed, the standard of "convincing" may be unobtainable by any evaluation. Possible unidentified contaminants may still affect these results. Perhaps the prisoners in DAP and nonDAP facilities were not as alike as seems to be the case. Perhaps treated offenders faced different post-release environments than untreated offenders for reasons that had nothing to do with being treated. An evaluator cannot eliminate all possible contaminants. But dealing with selection bias certainly increases the believability that substance abuse treatment can improve the post-release behaviors of substance-involved offenders who receive that treatment in a prison setting.

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Appendix A Models for Dealing with Selection Bias

This appendix develops two mathematical models of recidivism. One is based on a lognormal survival model and the other is based on an exponential survival model. We discuss both models in section A.1, because they raise similar analytic problems, which have similar solutions. In section A.2, we introduce a form of selection bias into both models and develop an estimation procedure (maximum likelihood) that yields consistent parameter estimates of the treatment effect provided the model is true. Deriving those estimates requires a model of the process by which subjects get into treatment, which we develop in section A.3. A probit model is developed in A.4; a two-limit tobit model appears in A.5.

A.1 The Basic Recidivism Model

Upon release from prison (including confinement in a half-way house), every offender has a *propensity* to recidivate. Recidivism means either that the offender was rearrested or that he tested positive for an illegal drug. These two events are analyzed separately. The propensity to recidivate can be expressed as a non-negative, increasing function of an underlying latent propensity score, Z. This score is in turn assumed to be a linear function of a dummy variable (coded 1 when the offender was treated and coded zero otherwise) and a vector of control variables. Thus, the propensity score is written:

$$Z_i = a_0 + a_1 TR_i + a_2 X_i + s\, e_{1i} \qquad \textbf{(1)}$$

where:

- Zi — a latent variable, measured on a continuous scale, so that within a specified time the probability of recidivism for the ith individual decreases as Zi increases.
- TR_I — a dummy variable coded 1 when the i^{th} offender was treated and coded 0 otherwise.
- X_i — a column vector of control variables such as age, gender, and race.
- α_0 — a scalar parameter **C** the constant term.
- α_1 — a scalar parameter **C** the treatment effect.
- α_2 — a row vector of parameters associated with the control variables.
- ε_{1i} — a random error term, identically and independently distributed as standard normal across the sample of offenders. We use ε as an error term in other equations, so the superscript A1@ is introduced to distinguish error terms across equations.
- σ — A scalar parameter. Alternatively, we might drop σ from (1) and assume that ε is distributed as normal with a mean of zero and variance of σ^2, but the derivations are simplified by using this first specification.

We eventually adopt two different assumption about how the latent variable Z affects the distribution of time until recidivism, but it is useful to first define the density and distribution functions for time until recidivism generically, and then substitute parametric distribution functions to get the lognormal and exponential models. Let:

t_i represent time until recidivism;

$\phi(t_i)$ represent the density function for time until recidivism; and

$\Phi(t_i)$ represent the cumulative distribution function for time until recidivism.

The follow-up period lasts M months. If recidivism occurs within M months, then we observe the time when it occurred. Otherwise we observe that recidivism did not occur within those M months. The generic likelihood function for recidivism during the first M months is written:

$$L_1 = \prod_i f(T_i)^{R_i}(1 - \Phi(M))^{1-R_i} \qquad \textbf{(2)}$$

where:

L_1 is the generic likelihood function for a survival model with censoring at M months;

T_i is the time (in months) until recidivism for the i^{th} subject when recidivism is observed;

R_i is coded 1 when recidivism happens within the six-month follow-up period and is coded 0 otherwise.

This generic likelihood function is standard for survival models (Kalbfeisch and Prentice, 1980; Lancaster, 1990). It is readily changed into the likelihood for the lognormal survival model by substituting the lognormal density and distribution functions into the generic form, and likewise, it is transformed into a variation of the exponential survival model by substituting density and distribution functions based on a modification of the exponential distribution. We take those steps below.

Following diagnostic tests, it might be reasonable to assume that the time until an arrest follows a lognormal distribution. In this case, $\ln(t_i) = Z_i$, and the density function for time until an arrest is written:

$$f_A(t_{A_i}) = \frac{e^{-0.5\frac{(\ln(t_{A_i}) - a_0 - a_1 TR_i - a_2 X_i)^2}{s^2}}}{t_{A_i}\sqrt{2p\ s^2}} \qquad \textbf{(3)}$$

where:

$\phi_A(t_{Ai})$ represents the lognormal density function for the distribution of time until arrest;

t_{Ai} time of arrest.

Substituting the lognormal density (3) and its distribution function into the generic likelihood function (2) yields the likelihood function for the lognormal survival model.

Also using diagnostic tests, time until a positive urine screen might follow an exponential distribution. The propensity to recidivate (1) is now written in the form:

Unlike the usual exponential model, this specification has an error term ε_1 that must be taken into

$$l_i = e^{Z_i} \qquad \textbf{(4)}$$

account in the analysis (see Heckman and Singer, 1985). This introduction of an error term into is a convenient and realistic[5] way to introduce selection bias into the model, although it does complicate the mathematics behind the development of the survival model. Thus, the density function for the time until recidivism is now written as the integral of a mixture distribution:

$$f_U(t_{U_i}) = \int_{e=-infinity}^{e=+infinity} l_i e^{-l_i t_{Ui}} h(e_1) d e_1 \qquad \textbf{(5)}$$

where:

$\phi_U(t_{Ui})$ represents the density function for the distribution of time until a positive urine test;

t_{Ui} time until a positive urine test;

$\eta(\varepsilon_1)$ the standard normal density function.

[5] The models developed here are sometimes called mixture models (Lancaster, 1990), and the $\eta(\varepsilon)$ is sometimes called the mixture distribution. Estimates of the parameters in the distribution of greatest interest to us (eg. the exponential) are sensitive to the assumptions made about the mixture distribution (Yamaguchi, 1986). A literature on criminal careers (Spelman, 1994) reports that offense rates have a skewed distribution across offenders, and this finding might be extended to assume that time until recidivism will be similarly skewed, so that the error distributions chosen for this analysis have some justification. Others (Schmidt and Witte, 1988; Rhodes, 1989) have found the lognormal to be a useful distribution for explaining recidivism. Nevertheless, future analyses will test the sensitivity of results to alternative assumptions made about the mixture distribution. For example, by using a power transformations (such as the Box-Cox power transformation), the distribution $\eta(\varepsilon)$ can be extremely flexible. Such tests are planned for the future.

The integration removes the unobserved ε_1 from the distribution. However, the presence of ε_1 will not be innocuous in discussions to follow. Equation (5) has no closed-form equivalent expression and requires numerical integration. Of course, this is also true of its cumulative distribution function, which requires a second integration over t_{Ui} from 0 to T_i.

A.2 Introducing Selection Bias

A problem occurs when subjects who receive treatment are selected on a non-random basis. This may happen because subjects self-select for treatment or because treatment personnel are selective, or both. To build selection bias into the lognormal and exponential models, we introduce a second latent variable, the propensity to enter treatment:

$$Y_i = b_0 + b_1 X_i + e_{2i} \quad \textbf{(6)}$$

Here:

Y_i a latent variable. The higher the value of Y, the more likely a person will enter treatment;

X_i a column vector of control variables, the same as defined earlier;

β_0 a scalar parameter;

β_1 a row vector of parameters conformable with X;

ε_{2i} a random error term that is distributed as standard normal;

and

when $Y \geq 0$, then treatment occurs (TR=1), and

when $Y < 0$, then treatment does not occur (TR=0).

Unless ε_1 and ε_2 are statistically independent, the variable representing treatment (TR) will not be independent of ε_1. It seems unlikely that the two will be independent, because they both are affected by excluded variables, such as motivation to change behavior. This correlation will cause the parameter estimate of the treatment effect (α_1) to be biased and inconsistent unless it is taken into account in the analysis.

One approach to overcoming this problem is to assume a parametric form for the joint distribution between ε_1 and ε_2, and to take that joint distribution into account in the likelihood functions (equation 2). Assuming that the two are distributed as bivariate normal, two cases are pertinent, the first for time until an arrest and the second for time until a positive urine test. Considering the first case (the lognormal distribution), the density function expressed previously as equation (3) is correct only for those cases that come from the nonDAP facility. For people who receive treatment, we use the conditional density function as represented by equation (7) in place of (3).

$$f_A(t_{A_i} \mid TR_i = 1) = \frac{e^{-0.5\frac{(\ln(t_{A_i})-a_0-a_1TR_i-a_2X_i)^2}{s_i^2}}}{t_{A_i}\sqrt{2p\,s_i^2}} \frac{H\left(\frac{b_0+b_1X_i+r\frac{\ln(t_{A_i})-a_0-a_1TR_i-a_2X_i}{s_i}}{\sqrt{1-r^2}}\right)}{H(b_0+b_1X_i)} \quad \textbf{(7)}$$

and for people who do not enter treatment and were members of the DAP comparison group, we use the conditional density function represented by (8) in place of (3).

$$f_A(t_{A_i} \mid TR_i = 0) = \frac{e^{-0.5\frac{(\ln(t_{A_i})-a_0-a_2X_i)^2}{s_i^2}}}{t_{A_i}\sqrt{2p\,s_i^2}} \frac{H_c\left(\frac{b_0+b_1X_i+r\frac{\ln(t_{A_i})-a_0-a_2X_i}{s_i}}{\sqrt{1-r^2}}\right)}{H_c(b_0+b_1X_i)} \quad \textbf{(8)}$$

where:

H the standard normal cumulative distribution function;

H_c the complement of the standard normal cumulative distribution function;

ρ the correlation between ε_1 and ε_2.

The conditional density functions (7) and (8) have cumulative distribution counterparts, which must also be substituted into (2). We do not show those distribution functions because they are just the appropriate specification of the bivariate normal cdf divided by the probability that the subject was treated (7) or was not treated (8).

The general approach to deriving this likelihood is explained in Maddala (1983, p. 266). Briefly, we start with the bivariate normal density involving ε_1 and ε_2. This can be written as $\eta(\varepsilon_1)\eta(\varepsilon_2|\varepsilon_1)$. We integrate this over the appropriate range for ε_2 to get the joint probability of t_A and entering treatment (equation (7)) or not entering treatment (equation (8)). We divide the results by the unconditional probability of entering treatment (equation (7)) or not entering treatment (equation (8)).

In essence, then, the likelihood function is different depending on whether the subject came from a nonDAP facility, came from a DAP facility but did not enter treatment, or came from a DAP facility and entered treatment. Nevertheless, the generic likelihood (2) holds; we just substitute the correct

density and distribution function depending on whether the subject is a member of the nonDAP control group, the DAP comparison group, or the DAP treatment group.

The generic likelihood function also has to be modified when the exponential model is used. When a subject comes from a nonDAP facility, equation (5) represents the density function. When the subject comes from a DAP facility and receives treatment, we use (9) in place of (5)

$$f_U(t_{U_i} | TR_i = 1) = \int_{e=-infinity}^{e=+infinity} l_i e^{-l_i t_{U_i}} h(e_1 | TR_i = 1) d e_1 \quad \textbf{(9)}$$

and when the subject comes from a DAP facility but does not receive treatment then we use (10) in place of (5)

$$f_U(t_{U_i} | TR_i = 0) = \int_{e=-infinity}^{e=+infinity} l_i e^{-l_i t_{U_i}} h(e_1 | TR_i = 0) d e_1 \quad \textbf{(10)}$$

where:

$\eta(\varepsilon_1|TR_i=1)$ is the normal density function conditional on $TR_i = 1$, and

$\eta(\varepsilon_1|TR_i=0)$ is the normal density function conditional on $TR_i = 0$.

and numerical integration was used to get these conditional distributions, because there is no closed-form expression. The density function for the error terms in (9) and (10) conditional on TR can be written:

$$h(e_1 | TR_i = 1) = \frac{\int_{e_2=-b_0-b_1X_1}^{infinity} h_b(e_1, e_2, r) de_2}{\int_{e_2=-b_0-b_1X_i}^{infinity} h(e_2) de_2} \quad \textbf{(11)}$$

$$= \frac{H\left(\frac{b_0 + b_1 X_1 + r e_1}{\sqrt{(1 - r^2)}}\right)}{H(b_0 + b_1 X_i)} h(e_1)$$

where:

η_b represents the density function for the bivariate normal (standard normal in this case), and

ρ represents the correlation between ε_1 and ε_2;

and a similar expression exists for $\eta(\varepsilon_1|TR_i=0)$. As before, the density functions have cumulative distribution (over t_U) function counterparts. These must be numerically computed with a double integral and substituted, as appropriate, into (2).

The likelihood function is different depending on whether the subject came from the nonDAP control group, the DAP comparison group, or the DAP treatment group. The generic likelihood (2) holds; we substitute the correct density and distribution function depending on whether the subject is a member of the nonDAP control group, the DAP comparison group, or the DAP treatment group.

A.3 Estimating the Probability of Selection into Treatment

Applying the adjustment described above for selection bias requires an estimate of β. Although the α and β parameters could be estimated jointly, it is easier (although less efficient) to estimate the β parameters from the probit model (equation (6)) and then maximize the likelihood expression (equation 2, after the appropriate substitutions) conditional on those estimates of β. Estimation of the probit model was not straightforward. Because the Bureau sampled the DAP comparison cases, we had to take that sampling into account by including the probability of being sampled as part of the likelihood function for the probit model. Thus, the probit model needs to be based on the joint probability of two events: entering treatment or not entering treatment, and being selected into the study sample. DAP treatment cases were selected with certainty, so they have a conditional selection probability equal to one, and nonDAP cases do not enter into this estimation, because those cases have a zero probability of entering treatment.[6] The likelihood for this model is written:

$$L_2 = \prod_i \frac{\mathrm{H}(b_0 + b_1 X_i)^{TR_i} \left(PS_i \left(1 - \mathrm{H}(b_0 + b_1 X_i)\right)\right)^{1-TR_i}}{\mathrm{H}(b_0 + b_1 X_i) + (PS_i (1 - \mathrm{H}(b_0 + b_1 X_i))} \quad \textbf{(13)}$$

where:

PS_I is the probability of selection into the study sample for the i^{th} case. When the subject received treatment, the probability is 1, because all treated subjects were included in the sample.

The logic of this approach is that the probit model represents the probability of occurrence of two events. In the first event, a subject either is selected for treatment or he is not selected for treatment. The second event **C** being included in the sample **C** is then conditional on the outcome of the first event. If the subject entered treatment, then he was included in the sample, but if he did not enter

[6] Actually, the DAP cases that received treatment were sampled with less than certainty. Assuming a sampling probability of one is convenient however, provided PS is adjusted accordingly.

treatment, he was included in the sample with a probability of PS_i. The likelihood function reflect the joint probability of those two events.

A.4 A Probit Model of Halfway House Failures

The Bureau chose to analyze failures in halfway house assignments as a dichotomous dependent variable **C** failure (coded 1) and success (coded 0). This decision suggested that a probit model would be an appropriate way to analyze outcomes. As before, we assume that every individual who is placed in a halfway house has a propensity to fail, expressed as a latent variable:
We have reused notation from above because there seems to be little risk of confusion. The Z again represents the latent variable, but now it applies to the propensity to fail in a halfway house confinement.

$$Z_i = \alpha_0 + \alpha_1 TR_i + \alpha_2 X_i + e_{3i}$$

An inmate fails when:

$Z_i \geq 0$

and he succeeds when:

$Z_i < 0$

Assuming that ε_3 and ε_2 are distributed as bivariate normal, the likelihood function for estimating the α can be written as

$$L_{41} = \prod_{i \in nonDAP\ CONTROL} \mathrm{H}(\alpha_0 + \alpha_2 X_i)^{h_i} [1 - \mathrm{H}(\alpha_0 + \alpha_2 X_i)]^{1-h_i}$$

for the nonDAP control group; as

$$L_{42} = \prod_{i \in DAP\ COMPARISON} \mathrm{N}(\alpha_0 + \alpha_2 X_i \mid TR_i = 0)^{h_i} [1 - \mathrm{N}(\alpha_0 + \alpha_2 X_i \mid TR_i = 0)]^{1-h_i}$$

for the DAP comparison group; and as

$$L_{43} = \prod_{i \in DAP\ TREATED} \mathrm{N}(\alpha_0 + \alpha_1 TR_i + \alpha_2 X_i \mid TR_i = 1)^{h_i} [1 - \mathrm{N}(\alpha_0 + \alpha_1 TR_i + \alpha_2 X_i \mid TR_i = 1)]^{1-h_i}$$

for the DAP treatment group, where:

h_i equals 1 when the subject failed and equals zero otherwise.

$N(\alpha_0+\alpha_2 X_i|TR_i=0)$ represents the distribution function conditional on the i^{th} subject=s not being treated and $N(\alpha_0+\alpha_1 TR_i+\alpha_2 X_i|TR_i=1)$ represents the distribution conditional on the i^{th} subject=s being treated:

$$N(a_0 + a_1 TR_i + a_2 X_i \,|\, TR_i = 1) = \frac{H_b(a_0 + a_1 TR_i + a_2 X_i, b_0 + b_1 X_i, r)}{H(b_0 + b_1 X_i)}$$

where:

H_b is the bivariate normal distribution function (standard normal in this case);

and a similar expression exists for $N(\alpha_0+\alpha_2 X_i|TR_i=0)$.

The likelihood function is then written:

$$L_3 = L_{31} L_{32} L_{33}$$

A.5 A Two-Limit Tobit Model of Employment

The Bureau chose to measure post-release employment as percentage of time employed during the six-month follow-up period. This could range from 0 for those who were never employed to 100 percent for those who were always employed. Both extremes were observed in the data.

Although an ordinary least squares regression might be used to analyze this outcome, OLS regression suffers from three problems when applied in this context. The first problem is that parameter estimates will be biased and inconsistent, because the outcomes have upper and lower limits, which are not taken into account by the estimation procedure. The second problem is that the standard errors will be inconsistent, because the error terms will necessarily be heteroscedastic. The third problem is that selection bias still needs to be taken into account. Although feasible generalized least squares can be used to deal with all these problems, an alternative approach is to use a two-limit tobit model (Maddala, p. 160).

As used here, this model assumes that the every offender has a propensity to be employed. Reusing the earlier notation, we write this propensity as:

$$Z_i = a_0 + a_1 TR_i + a_2 X_i + e_{4i}$$

The subject is unemployed at all times when

$Z_i < 0$,

and he is employed full time when

$Z_i > 100$,

and otherwise, time employed (TE_i) equals the latent variable, so:

$TE_i = Z_i$ when $Z_i \geq 0$ and $Z_i \leq 100$

The unknown parameters can be estimated by maximum likelihood. As before, we have to account for three conditions. When the study subject comes from the nonDAP control group, the likelihood is:

$$L_{41} = \prod_{i \in nonDAP\ CONTROL} \mathrm{H}\left(\frac{-\alpha_0 - \alpha_2 X_i}{\sigma}\right)^{E_{1i}} \left[\frac{h\left(\frac{TE_i - \alpha_2 X_i}{\sigma}\right)}{\sigma}\right]^{E_{2i}}$$

$$\left[1 - \mathrm{H}\left(\frac{100 - \alpha_0 - \alpha_1 TR_i - \alpha_2 X_i}{\sigma}\right)\right]^{1-E_{1i}-E_{2i}}$$

When the subject come from the DAP treatment group, the likelihood is:

$$L_{42} = \prod_{i \in DAP\ TREATED} \mathrm{N}\left(\frac{-\alpha_0 - \alpha_1 TR_i - \alpha_2 X_i}{\sigma} \mid TR_i = 1\right)^{E_{1i}} \left[\frac{n\left(\frac{TE_i - \alpha_0 - \alpha_1 TR_i - \alpha_2 X_i}{\sigma} \mid TR_i = 1\right)}{\sigma}\right]^{E_{2i}}$$

$$\left[1 - \mathrm{N}\left(\frac{100 - \alpha_0 - \alpha_1 TR_i - \alpha_2 X_i}{\sigma} \mid TR_i = 1\right)\right]^{1-E_{1i}-E_{2i}}$$

and when the subject comes from the DAP comparison group, the likelihood is:

$$L_{43} = \prod_{i \in DAP\ COMPARISON} \mathrm{N}\left(\frac{-\alpha_0 - \alpha_2 X_i}{\sigma}\right) TR_i = 0\ )^{E_{1i}} \left[\frac{n\left(\frac{TE_i - \alpha_0 - \alpha_2 X_i}{\sigma} \mid TR_i = 0\right)}{\sigma}\right]^{E_{2i}}$$

where:

E_{1i} equals 1 when the subject was unemployed for the entire follow-up period;

E_{2i} equals 1 when the subject was employed part (but not all) the follow-up period.

$$\left[1 - \mathrm{N}\left(\frac{100 - a_0 - a_1 TR_i - a_2 X_i}{s} \mid TR_i = 0\right)\right]^{1-E_{1i}-E_{2i}}$$

As before, N and ν represent the conditional distribution and density function, respectively. The conditional distribution function has already been presented as part of the probit model, and the density is similar to that for the uncensored part of the lognormal model, except that the dependent variable is in natural rather than logarithmic units.

Thus:

The likelihood function for the two-limit tobit model is written:

$$L_4 = L_{41} L_{42} L_{43}$$

Appendix B Descriptive Statistics and Regression Results

Data and statistical procedures have been described in the body of this report. This statistical appendix reports details about the regression results. It is organized by outcome criteria. For each criterion, it reports descriptive statistics for men and women, regression results for men, and regression results for women. Each table containing regression results reports the parameter estimates and t-scores for the dummy variable model, the instrumental variable model and the Heckman-type adjustment model. We were not always able to estimate the Heckman-type adjustment model parameters, however, so these do not always appear.

The Variables

Table B.1 reports variables that entered into the analysis.

Table B.1

Predictor Variables

	Variables
EBLAC	Race
ERACEOTH	Race
EHISP	Ethnicity
GRADEA	Educational level
AGERLSE	Age at release
EWORKJOB	Pre-incarceration (month before) employment status
ELEGITUN	Pre-incarceration (month before) employment status
EUNEMP	Pre-incarceration (month before) employment status
EPRIORCM	Criminal History: prior commitments
ALCONLY	Types of daily drug & alcohol use patterns before arrest
MJALC	Types of daily drug & alcohol use patterns before arrest
MJNOALC	Types of daily drug & alcohol use patterns before arrest
ONEALCY	Types of daily drug & alcohol use patterns before arrest
ONEALCN	Types of daily drug & alcohol use patterns before arrest
TWOALCY	Types of daily drug & alcohol use patterns before arrest
TWOALCN	Types of daily drug & alcohol use patterns before arrest
EIDAGASP	Mental Health Diagnoses – ASP and/or Depression
EDIAGDEP	Mental Health Diagnoses – ASP and/or Depression
EDUAGBTH	Mental Health Diagnoses – ASP and/or Depression
FLAGALL	Flag variable to select subjects
BADONE	Outcome variable
TIMETOAR	Time to arrest/censoring
AGEFIRCO	Age at first commitment
ESPOUDRG	Spouse had a drug problem
EPSTMHTX	History of mental health treatment
ERELIRY	100 or 200 level disciplinary infraction M months before release

Table B.1

Predictor Variables

	Variables
ENOCCC	CCC placement/no placement
ESUPRLNO	Released to supervision
FLAGARST	Flag variable to select subjects
ETXIND	Post-release services (initiated 1st month after release)
ETXGRP	Post-release services (initiated 1st month after release)
ETXBOTH	Post-release services (initiated 1st month after release)
EAAYES	Self-help group participation
UARATE	Frequency of au testing (avg. # of tests/mo.)
PC_RATE	Frequency of contacts w/probably officer (avg. #/mo.)
CC_RATE	Frequency of collateral contacts w/probably officer (avg. #/mo.)
ESPOUSE	Who living with upon release
ECOM_LAW	Who living with upon release
FLAGARRV	Flag variable to select subject
OVERALL	Outcome variable
FLAGDRUG	Flag variable to select subjects
DRUGGIE	Outcome variable
TIMETODG	Time to drug use/censoring (to 1st evidence of)
DAILYNO	Ever used drugs on a daily basis
EPSTDGTX	History of treatment: Drug treatment
EPSTETOH	History of treatment: Alcohol treatment
EDRUGIRY	Drug related disciplinary infraction M months before release

Criterion: Arrest for all Study Subjects

The first analysis used an arrest during the follow-up period as the criterion measure. All study subjects with valid data entered this analysis. Survival models were the estimation technique.

	Males		Females	
Variable	Mean	Std Dev	Mean	Std Dev
ARRESTED	0.3543	0.4785	0.1974	0.3984
COMPARISON	0.2549	0.4359	0.2048	0.4039
TIMETOAR	600.1729	398.6392	736.6965	381.2963
WASTREAT	0.3969	0.4894	0.3327	0.4716
AGEFIRCO	0.2633	0.0937	0.2963	0.0846
ESPOUDRG	-0.5312	0.8474	0.0750	0.9981
EPSTMHTX	-0.6179	0.7864	-0.1664	0.9870
ERELIRY	-0.7303	0.6832	-0.8099	0.5871
ENOCCC	-0.3930	0.9197	-0.4406	0.8985
ESUPRLNO	-0.7627	0.6469	-0.7404	0.6728
EBLACK	-0.2182	0.9621	-0.1097	0.9828
ERACEOTH	-0.5684	0.5475	-0.5192	0.5456
EHISP	-0.8371	0.5472	-0.8172	0.5769
GRADEA	0.6063	0.1039	0.5778	0.1094
AGERLSE	0.3717	0.0888	0.3502	0.0779
EWORKJOB	0.2468	0.8981	-0.0238	0.9025
ELEGITUN	-0.2639	0.5356	-0.3254	0.6378
EUNEMP	-0.2234	0.5891	-0.3254	0.6378
EPRIORCM	0.4226	0.9065	-0.1371	0.9915
ALCONLY	0.1925	0.3943	0.0951	0.2936
MJALC	0.0610	0.2394	0.0329	0.1786
MJNOALC	0.0929	0.2904	0.0841	0.2778
ONEALCY	0.0834	0.2765	0.1133	0.3173
ONEALCN	0.1067	0.3088	0.2157	0.4117
TWOALCY	0.0367	0.1880	0.0585	0.2349
TWOALCN	0.0515	0.2210	0.0841	0.2778
EDIAGASP	-0.2454	0.8838	-0.3528	0.7399
EDIAGDEP	-0.4645	0.6373	-0.3163	0.7801
EDIAGBTH	-0.4626	0.6402	-0.3784	0.7093
CASES	2099		547	

Rearrest: Males (Exponential Failure Time Model)

	Adjustment Method					
	Unadjusted		Instruments		Heckman-Type	
Parameters	Estimates	Est./s.e.	Estimates	Est./s.e.	Estimates	Est./s.
CONSTANT	-4.8135	-13.660	-4.7816	-13.527	-4.8396	-13.163
WASTREAT	-0.1775	-1.851	-0.2853	-1.989	-0.4254	-2.315
AGEFIRCO	-3.0864	-4.554	-3.1386	-4.632	-3.1679	-4.592
ESPOUDRG	0.0355	0.659	0.0339	0.630	0.0344	0.629
EPSTMHTX	0.0106	0.182	0.0109	0.187	0.0142	0.236
ERELIRY	0.2892	4.558	0.2907	4.589	0.2806	4.179
ENOCCC	0.0679	1.334	0.0762	1.518	0.0651	1.253
ESUPRLNO	-0.0245	-0.379	-0.0288	-0.443	-0.0236	-0.354
EBLACK	0.0473	0.470	0.0430	0.425	0.0228	0.217
ERACEOTH	0.1377	0.786	0.1428	0.814	0.1808	0.982
EHISP	-0.1513	-1.712	-0.1525	-1.734	-0.1516	-1.698
GRADEA	-0.0684	-0.143	-0.0795	-0.167	-0.0930	-0.193
AGERLSE	-3.5083	-5.491	-3.4285	-5.365	-3.4111	-5.226
EWORKJOB	-0.0756	-0.904	-0.0798	-0.956	-0.0710	-0.838
ELEGITUN	0.1257	0.736	0.1140	0.670	0.1080	0.633
EUNEMP	0.0288	0.225	0.0407	0.317	0.0364	0.280
EPRIORCM	0.4338	7.335	0.4279	7.234	0.4320	7.184
ALCONLY	0.3227	2.630	0.3410	2.772	0.3478	2.731
MJALC	-0.1135	-0.591	-0.1166	-0.612	-0.0968	-0.499
MJNOALC	0.0141	0.087	0.0322	0.198	0.0102	0.062
ONEALCY	-0.0114	-0.069	0.0106	0.063	0.0164	0.094
ONEALCN	0.0988	0.615	0.0997	0.622	0.0927	0.569
TWOALCY	0.2152	0.850	0.2296	0.906	0.2207	0.858
TWOALCN	-0.0281	-0.136	-0.0124	-0.060	-0.0112	-0.053
EDIAGASP	0.0084	0.098	0.0062	0.072	-0.0011	-0.012
EDIAGDEP	0.1248	0.948	0.1257	0.954	0.1230	0.913
EDIAGBTH	-0.1815	-1.378	-0.1791	-1.357	-0.1695	-1.227
Q	0.9837	5.957	0.9819	6.012	1.1053	3.054
SIGMA					-1.0898	-1.099
COVARIAN					1.1793	0.965
CASES	2099					

Rearrest: Females (Lognormal Failure Time Model)

	Adjustment Method					
	Unadjusted		Instruments		Heckman-Type	
Parameters	Estimates	Est./s.e.	Estimates	Est./s.e.	Estimates	Est./s.e.
CONSTANT	6.3336	2.323	6.2214	4.183	6.3008	3.805
WASTREAT	0.1159	0.411	-0.0052	-0.011	0.0905	0.193
AGEFIRCO	0.4064	0.172	0.3928	0.167	0.4061	0.173
ESPOUDRG	-0.1139	-0.598	-0.1134	-0.778	-0.1156	-0.776
EPSTMHTX	0.0579	0.409	0.0526	0.382	0.0573	0.416
ERELIRY	-0.5074	-2.608	-0.5080	-2.597	-0.5072	-2.604
ENOCCC	-0.2546	-1.743	-0.2659	-1.861	-0.2558	-1.789
ESUPRLNO	-0.2337	-1.374	-0.2402	-1.396	-0.2340	-1.379
EBLACK	-0.0626	-0.203	-0.0617	-0.210	-0.0619	-0.211
ERACEOTH	-0.3532	-0.698	-0.3652	-0.717	-0.3564	-0.701
EHISP	0.0271	0.079	0.0190	0.071	0.0241	0.088
GRADEA	-0.9026	-0.704	-0.9050	-0.761	-0.9065	-0.759
AGERLSE	4.0457	1.558	4.1679	1.647	4.0558	1.601
EWORKJOB	0.1619	0.641	0.1676	0.737	0.1630	0.709
ELEGITUN	-0.3321	-1.056	-0.3297	-1.044	-0.3322	-1.055
EUNEMP	0.2752	0.784	0.2629	0.765	0.2756	0.799
EPRIORCM	-0.6358	-3.513	-0.6434	-4.255	-0.6381	-4.154
ALCONLY	-0.0774	-0.167	-0.0725	-0.158	-0.0772	-0.169
MJALC	1.2612	1.212	1.2723	1.236	1.2634	1.221
MJNOALC	-0.2049	-0.360	-0.1955	-0.402	-0.2009	-0.404
ONEALCY	-0.1632	-0.348	-0.1423	-0.317	-0.1592	-0.352
ONEALCN	0.0893	0.259	0.0936	0.270	0.0911	0.264
TWOALCY	-0.4017	-0.607	-0.3940	-0.713	-0.3945	-0.701
TWOALCN	0.8330	1.350	0.8515	1.487	0.8362	1.452
EDIAGASP	-0.1206	-0.460	-0.1196	-0.468	-0.1208	-0.475
EDIAGDEP	-0.0240	-0.093	-0.0245	-0.095	-0.0245	-0.095
EDIAGBTH	0.0304	0.109	0.0309	0.113	0.0296	0.108
SIGMA	1.9016	2.227	1.8743	4.492	1.8921	3.994
Q	3.3272	0.094	2.5428	0.330	2.9872	0.220
COVARIAN					0.0214	0.066
CASES	547					

Rearrest: Females (Lognormal Failure Time Model)

	Adjustment Method					
	Unadjusted	Instruments		Heckman-Type		
Parameters	Estimates	Est./s.e.	Estimates	Est./s.e.	Estimates	Est./s.
CONSTANT	6.3336	2.323	6.2214	4.183	6.3008	3.805
WASTREAT	0.1159	0.411	-0.0052	-0.011	0.0905	0.193
AGEFIRCO	0.4064	0.172	0.3928	0.167	0.4061	0.173
ESPOUDRG	-0.1139	-0.598	-0.1134	-0.778	-0.1156	-0.776
EPSTMHTX	0.0579	0.409	0.0526	0.382	0.0573	0.416
ERELIRY	-0.5074	-2.608	-0.5080	-2.597	-0.5072	-2.604
ENOCCC	-0.2546	-1.743	-0.2659	-1.861	-0.2558	-1.789
ESUPRLNO	-0.2337	-1.374	-0.2402	-1.396	-0.2340	-1.379
EBLACK	-0.0626	-0.203	-0.0617	-0.210	-0.0619	-0.211
ERACEOTH	-0.3532	-0.698	-0.3652	-0.717	-0.3564	-0.701
EHISP	0.0271	0.079	0.0190	0.071	0.0241	0.088
GRADEA	-0.9026	-0.704	-0.9050	-0.761	-0.9065	-0.759
AGERLSE	4.0457	1.558	4.1679	1.647	4.0558	1.601
EWORKJOB	0.1619	0.641	0.1676	0.737	0.1630	0.709
ELEGITUN	-0.3321	-1.056	-0.3297	-1.044	-0.3322	-1.055
EUNEMP	0.2752	0.784	0.2629	0.765	0.2756	0.799
EPRIORCM	-0.6358	-3.513	-0.6434	-4.255	-0.6381	-4.154
ALCONLY	-0.0774	-0.167	-0.0725	-0.158	-0.0772	-0.169
MJALC	1.2612	1.212	1.2723	1.236	1.2634	1.221
MJNOALC	-0.2049	-0.360	-0.1955	-0.402	-0.2009	-0.404
ONEALCY	-0.1632	-0.348	-0.1423	-0.317	-0.1592	-0.352
ONEALCN	0.0893	0.259	0.0936	0.270	0.0911	0.264
TWOALCY	-0.4017	-0.607	-0.3940	-0.713	-0.3945	-0.701
TWOALCN	0.8330	1.350	0.8515	1.487	0.8362	1.452
EDIAGASP	-0.1206	-0.460	-0.1196	-0.468	-0.1208	-0.475
EDIAGDEP	-0.0240	-0.093	-0.0245	-0.095	-0.0245	-0.095
EDIAGBTH	0.0304	0.109	0.0309	0.113	0.0296	0.108
SIGMA	1.9016	2.227	1.8743	4.492	1.8921	3.994
Q	3.3272	0.094	2.5428	0.330	2.9872	0.220
COVARIAN					0.0214	0.066
CASES	547					

Criterion: Arrest for those Who Were Supervised

The specification for the second set of regressions was similar to that for the first. However, the data were restricted for offenders who were under active supervision. Also, covariates that are specific to people under supervision were added to the model.

	Males		Females	
Variable	Mean	Std Dev	Mean	Std Dev
ARRESTED	0.3344	0.4719	0.1667	0.3734
COMPARIS	0.2405	0.4275	0.1839	0.3878
TIMETOAR	588.1221	392.7973	722.5011	378.7840
WASTREAT	0.4142	0.4927	0.3615	0.4810
AGEFIRCO	0.2664	0.0958	0.2993	0.0847
ESPOUDRG	-0.5375	0.8435	0.0867	0.9973
EPSTMHTX	-0.6091	0.7933	-0.1966	0.9815
ERELIRY	-0.7427	0.6698	-0.8055	0.5932
ENOCCC	-0.4604	0.8880	-0.5391	0.8431
ETXIND	-0.3073	0.8455	-0.1691	0.8715
ETXGRP	-0.4729	0.6480	-0.4017	0.6272
ETXBTH	-0.4522	0.6780	-0.3404	0.7074
EAAYES	-0.6450	0.7644	-0.5391	0.8431
UA_RATE	2.4659	2.1926	2.4584	2.0207
PC_RATE	0.5321	0.6538	0.5436	0.4857
CC_RATE	0.6478	3.0766	0.5975	1.3477
ESPOUSE	-0.3952	0.8141	-0.6342	0.6976
ECOM_LAW	-0.4256	0.7794	-0.6490	0.6729
EBLACK	-0.2535	0.9532	-0.1142	0.9806
ERACEOTH	-0.5852	0.5461	-0.5159	0.5526
EHISP	-0.8350	0.5505	-0.8266	0.5633
GRADEA	0.6080	0.1032	0.5790	0.1093
AGERLSE	0.3736	0.0902	0.3513	0.0789
EWORKJOB	0.2258	0.9045	-0.0254	0.9086
ELEGITUN	-0.2758	0.5395	-0.3383	0.6309
EUNEMP	-0.2356	0.5932	-0.3362	0.6337
EPRIORCM	0.3746	0.9274	-0.2051	0.9798
ALCONLY	0.1911	0.3933	0.0930	0.2908
MJALC	0.0613	0.2400	0.0381	0.1915
MJNOALC	0.0955	0.2941	0.0867	0.2817
ONEALCY	0.0858	0.2801	0.1205	0.3259
ONEALCN	0.1086	0.3112	0.2135	0.4102
TWOALCY	0.0375	0.1899	0.0507	0.2197
TWOALCN	0.0489	0.2156	0.0846	0.2785
EDIAGASP	-0.2541	0.8792	-0.3679	0.7364
EDIAGDEP	-0.4642	0.6417	-0.3298	0.7792
EDIAGBTH	-0.4642	0.6417	-0.3911	0.7081
CASES	1842		473	

Rearrest: Males (Exponential Survival Model)

	Adjustment Method					
	Unadjusted		Instruments		Heckman-Type	
Parameters	Estimates	Est./s.e.	Estimates	Est./s.e.	Estimates	Est./s.e.
CONSTANT	-4.9447	-12.515	-4.9145	-12.390	-4.9522	-12.438
WASTREAT	-0.1502	-1.529	-0.2027	-1.335	-0.2967	-1.511
AGEFIRCO	-3.0678	-4.380	-3.0989	-4.418	-3.0848	-4.399
ESPOUDRG	0.0124	0.214	0.0128	0.222	0.0126	0.218
EPSTMHTX	0.0348	0.558	0.0337	0.539	0.0360	0.574
ERELIRY	0.2039	2.927	0.2067	2.971	0.2027	2.908
ENOCCC	0.0381	0.701	0.0459	0.852	0.0361	0.661
ETXIND	-0.0538	-0.597	-0.0497	-0.548	-0.0485	-0.536
ETXGRP	-0.1651	-1.274	-0.1718	-1.327	-0.1669	-1.287
ETXBTH	0.1717	1.452	0.1722	1.449	0.1687	1.423
EAAYES	-0.0379	-0.574	-0.0405	-0.611	-0.0389	-0.589
UA_RATE	-0.2506	-2.781	-0.2515	-2.788	-0.2492	-2.764
PC_RATE	0.3158	1.956	0.3004	1.847	0.3099	1.916
CC_RATE	0.5130	5.066	0.5134	5.064	0.5132	5.033
ESPOUSE	-0.2812	-3.366	-0.2856	-3.410	-0.2826	-3.373
ECOM_LAW	0.1607	1.971	0.1629	1.997	0.1610	1.971
EBLACK	0.0035	0.035	-0.0017	-0.016	-0.0077	-0.076
ERACEOTH	0.2053	1.215	0.2138	1.255	0.2241	1.308
EHISP	-0.1078	-1.182	-0.1090	-1.196	-0.1071	-1.172
GRADEA	-0.5232	-1.048	-0.5300	-1.062	-0.5411	-1.081
AGERLSE	-3.1335	-4.713	-3.0855	-4.627	-3.0593	-4.578
EWORKJOB	-0.0827	-0.960	-0.0896	-1.042	-0.0833	-0.966
ELEGITUN	0.1267	0.730	0.1183	0.681	0.1194	0.687
EUNEMP	0.0331	0.253	0.0461	0.351	0.0436	0.332
EPRIORCM	0.4254	6.964	0.4231	6.918	0.4232	6.915
ALCONLY	0.3534	2.680	0.3666	2.766	0.3615	2.733
MJALC	-0.0124	-0.060	-0.0185	-0.090	-0.0079	-0.038
MJNOALC	0.0106	0.065	0.0183	0.112	0.0132	0.081
ONEALCY	0.0552	0.321	0.0640	0.370	0.0680	0.392
ONEALCN	-0.0101	-0.059	-0.0119	-0.070	-0.0093	-0.055
TWOALCY	0.2246	0.906	0.2378	0.954	0.2395	0.961
TWOALCN	-0.0001	-0.000	0.0088	0.037	0.0065	0.028
EDIAGASP	0.0484	0.532	0.0486	0.533	0.0459	0.504
EDIAGDEP	0.0394	0.278	0.0340	0.241	0.0432	0.305
EDIAGBTH	-0.1572	-1.123	-0.1504	-1.070	-0.1594	-1.136
Q	1.3508	4.948	1.3360	4.971	1.4019	3.975
SIGMA					-1.8003	-1.325
COVARIAN					1.5519	0.924

Rearrest: Females (Lognormal Survival Model)

	Adjustment Method					
	Unadjusted		Instruments		Heckman-	
Type Parameters	Estimates	Est./s.e.	Estimates	Est./s.e.	Estimates	Est./s.
CONSTANT	4.6521	4.820	4.6718	4.812	4.6713	4.824
WASTREAT	0.2009	0.727	0.1509	0.320	0.3039	0.725
AGEFIRCO	1.2218	0.532	1.4382	0.625	1.2446	0.543
ESPOUDRG	-0.2996	-2.387	-0.2950	-2.341	-0.2976	-2.374
EPSTMHTX	0.0801	0.592	0.0607	0.456	0.0810	0.600
ERELIRY	-0.5391	-2.969	-0.5214	-2.886	-0.5395	-2.978
ENOCCC	-0.4482	-3.095	-0.4751	-3.380	-0.4440	-3.054
ETXIND	0.5737	2.390	0.5630	2.350	0.5772	2.394
ETXGRP	-0.3992	-0.916	-0.3721	-0.835	-0.4385	-0.961
ETXBTH	-0.2972	-1.030	-0.3079	-1.007	-0.2663	-0.876
EAAYES	0.0592	0.419	0.0693	0.490	0.0573	0.406
UA_RATE	0.1899	0.774	0.1834	0.741	0.1831	0.743
PC_RATE	0.0624	0.156	0.0710	0.176	0.0752	0.187
CC_RATE	-1.0293	-3.392	-1.0366	-3.382	-1.0414	-3.405
ESPOUSE	-0.0182	-0.075	-0.0156	-0.064	-0.0195	-0.080
ECOM_LAW	-0.0853	-0.289	-0.0732	-0.250	-0.0919	-0.309
EBLACK	-0.0490	-0.177	-0.0611	-0.221	-0.0545	-0.196
ERACEOTH	-0.5228	-1.118	-0.5234	-1.119	-0.5154	-1.097
EHISP	0.1692	0.704	0.1768	0.733	0.1654	0.689
GRADEA	-0.9823	-0.927	-0.9391	-0.874	-1.0098	-0.952
AGERLSE	3.8059	1.602	3.6829	1.507	3.7636	1.583
EWORKJOB	-0.2564	-1.064	-0.2517	-1.043	-0.2594	-1.074
ELEGITUN	0.1879	0.539	0.2198	0.634	0.1810	0.520
EUNEMP	0.3920	1.078	0.3479	0.960	0.4076	1.115
EPRIORCM	-0.6697	-4.988	-0.6651	-4.759	-0.6597	-4.795
ALCONLY	0.0519	0.114	0.0557	0.122	0.0570	0.125
MJALC	1.2434	1.578	1.2445	1.579	1.2309	1.563
MJNOALC	0.5490	1.425	0.5084	1.321	0.5379	1.392
ONEALCY	-0.5541	-1.341	-0.5787	-1.402	-0.5651	-1.367
ONEALCN	-0.1291	-0.337	-0.1521	-0.389	-0.1560	-0.396
TWOALCY	-0.1647	-0.315	-0.2332	-0.433	-0.2035	-0.378
TWOALCN	1.3846	2.482	1.3966	2.460	1.3558	2.402
EDIAGASP	-0.2094	-0.890	-0.2131	-0.904	-0.2148	-0.914
EDIAGDEP	0.0518	0.194	0.0573	0.213	0.0471	0.177
EDIAGBTH	0.4124	1.460	0.4327	1.502	0.4325	1.508
SIGMA	0.9337	8.589	0.9358	8.564	0.9318	8.579
Q	-0.5863	-2.761	-0.5865	-2.756	-0.5917	-2.772
COVARIANCE					-0.1714	-0.323

Criterion: Arrest or Revocation

The third set of regressions use an arrest or revocation as an outcome measure. Otherwise it is identical to the second set of regressions.

	Males		Females	
Variable	Mean	Std Dev	Mean	Std Dev
CENSOR	0.5483	0.4978	0.3425	0.4750
COMPARIS	0.2405	0.4275	0.1839	0.3878
TIMETOAR	588.1221	392.7973	722.5011	378.7840
WASTREAT	0.4142	0.4927	0.3615	0.4810
AGEFIRCO	0.2664	0.0958	0.2993	0.0847
ESPOUDRG	-0.5375	0.8435	0.0867	0.9973
EPSTMHTX	-0.6091	0.7933	-0.1966	0.9815
ERELIRY	-0.7427	0.6698	-0.8055	0.5932
ENOCCC	-0.4604	0.8880	-0.5391	0.8431
ETXIND	-0.3073	0.8455	-0.1691	0.8715
ETXGRP	-0.4729	0.6480	-0.4017	0.6272
ETXBTH	-0.4522	0.6780	-0.3404	0.7074
EAAYES	-0.6450	0.7644	-0.5391	0.8431
UA_RATE	2.4659	2.1926	2.4584	2.0207
PC_RATE	0.5321	0.6538	0.5436	0.4857
CC_RATE	0.6478	3.0766	0.5975	1.3477
ESPOUSE	-0.3952	0.8141	-0.6342	0.6976
ECOM_LAW	-0.4256	0.7794	-0.6490	0.6729
EBLACK	-0.2535	0.9532	-0.1142	0.9806
ERACEOTH	-0.5852	0.5461	-0.5159	0.5526
EHISP	-0.8350	0.5505	-0.8266	0.5633
GRADEA	0.6080	0.1032	0.5790	0.1093
AGERLSE	0.3736	0.0902	0.3513	0.0789
EWORKJOB	0.2258	0.9045	-0.0254	0.9086
ELEGITUN	-0.2758	0.5395	-0.3383	0.6309
EUNEMP	-0.2356	0.5932	-0.3362	0.6337
EPRIORCM	0.3746	0.9274	-0.2051	0.9798
ALCONLY	0.1911	0.3933	0.0930	0.2908
MJALC	0.0613	0.2400	0.0381	0.1915
MJNOALC	0.0955	0.2941	0.0867	0.2817
ONEALCY	0.0858	0.2801	0.1205	0.3259
ONEALCN	0.1086	0.3112	0.2135	0.4102
TWOALCY	0.0375	0.1899	0.0507	0.2197
TWOALCN	0.0489	0.2156	0.0846	0.2785
EDIAGASP	-0.2541	0.8792	-0.3679	0.7364
EDIAGDEP	-0.4642	0.6417	-0.3298	0.7792
EDIAGBTH	-0.4642	0.6417	-0.3911	0.7081
CASES	1842		473	

Rearrest or Revocation: Males (Exponential Survival Model)

	Adjustment Method					
	Unadjusted		Instruments		Heckman-Type	
Parameters	Estimates	Est./s.e.	Estimates	Est./s.e.	Estimates	Est./s
CONSTANT	-5.1599	-17.166	-5.1310	-17.071	-5.1609	-17.098
WASTREAT	-0.1608	-2.165	-0.2515	-2.193	-0.3968	-2.797
AGEFIRCO	-2.2678	-4.395	-2.3069	-4.466	-2.2857	-4.412
ESPOUDRG	-0.0398	-0.905	-0.0377	-0.859	-0.0399	-0.904
EPSTMHTX	0.0158	0.338	0.0155	0.331	0.0199	0.422
ERELIRY	0.3032	6.280	0.3062	6.360	0.3054	6.267
ENOCCC	0.0916	2.270	0.0988	2.485	0.0889	2.187
ETXIND	-0.0814	-1.236	-0.0728	-1.100	-0.0720	-1.084
ETXGRP	-0.0497	-0.551	-0.0528	-0.586	-0.0538	-0.592
ETXBTH	0.1583	1.917	0.1512	1.824	0.1542	1.846
EAAYES	-0.0004	-0.009	-0.0042	-0.088	-0.0021	-0.043
UA_RATE	-0.1265	-1.855	-0.1267	-1.861	-0.1232	-1.803
PC_RATE	0.1952	1.678	0.1878	1.611	0.1934	1.644
CC_RATE	0.5174	6.545	0.5183	6.558	0.5217	6.542
ESPOUSE	-0.3402	-5.301	-0.3441	-5.369	-0.3411	-5.280
ECOM_LAW	0.1844	3.009	0.1866	3.050	0.1846	2.991
EBLACK	0.1068	1.390	0.0982	1.270	0.0902	1.162
ERACEOTH	0.1382	1.048	0.1510	1.142	0.1669	1.254
EHISP	-0.0500	-0.738	-0.0505	-0.746	-0.0487	-0.713
GRADEA	-0.6066	-1.615	-0.6011	-1.602	-0.6181	-1.642
AGERLSE	-1.6387	-3.413	-1.5834	-3.298	-1.5536	-3.216
EWORKJOB	-0.1635	-2.577	-0.1700	-2.689	-0.1666	-2.615
ELEGITUN	0.1979	1.601	0.1841	1.495	0.1882	1.520
EUNEMP	0.0199	0.209	0.0411	0.432	0.0384	0.400
EPRIORCM	0.3812	8.215	0.3772	8.124	0.3786	8.117
ALCONLY	0.1543	1.502	0.1686	1.635	0.1682	1.623
MJALC	-0.0499	-0.303	-0.0469	-0.288	-0.0412	-0.254
MJNOALC	0.0475	0.375	0.0536	0.425	0.0466	0.366
ONEALCY	0.0994	0.777	0.1057	0.825	0.1181	0.910
ONEALCN	0.1274	1.048	0.1313	1.080	0.1325	1.081
TWOALCY	0.4060	2.349	0.4228	2.444	0.4366	2.492
TWOALCN	0.2385	1.461	0.2551	1.555	0.2484	1.517
EDIAGASP	0.0271	0.403	0.0285	0.424	0.0240	0.355
EDIAGDEP	-0.0171	-0.162	-0.0184	-0.175	-0.0083	-0.078
EDIAGBTH	0.0090	0.090	0.0075	0.075	0.0017	0.016
Q	2.4162	8.675	2.4213	8.747	2.5397	7.294
SIGMA					-1.5559	-2.974
COVARIANCE					2.3273	2.486

Rearrest or Revocation: Females (Lognormal Survival Model)

	Adjustment Method					
	Unadjusted		Instruments		Heckman-Type	
Parameters	Estimates	Est./s.e	Estimates	Est./s.e.	Estimates	Est./s.e
CONSTANT	6.0533	8.560	6.0129	8.496	6.0413	8.516
WASTREAT	0.2418	1.258	0.2264	0.719	0.1516	0.507
AGEFIRCO	2.1390	1.305	2.0540	1.251	2.1262	1.297
ESPOUDRG	0.0154	0.165	0.0213	0.230	0.0149	0.161
EPSTMHTX	0.1704	1.717	0.1654	1.667	0.1696	1.709
ERELIRY	-0.4919	-3.615	-0.4985	-3.643	-0.4920	-3.618
ENOCCC	-0.1465	-1.409	-0.1592	-1.532	-0.1508	-1.442
ETXIND	0.2102	1.329	0.2174	1.381	0.2104	1.331
ETXGRP	0.1406	0.561	0.1273	0.505	0.1505	0.597
ETXBTH	-0.5368	-2.892	-0.5303	-2.849	-0.5437	-2.919
EAAYES	0.1267	1.125	0.1245	1.104	0.1279	1.134
UA_RATE	0.1028	0.587	0.1050	0.598	0.1023	0.584
PC_RATE	0.0114	0.035	0.0184	0.057	0.0074	0.023
CC_RATE	-0.6965	-3.616	-0.6981	-3.609	-0.6980	-3.620
ESPOUSE	-0.0768	-0.390	-0.0643	-0.327	-0.0829	-0.420
ECOM_LAW	0.1597	0.783	0.1461	0.715	0.1680	0.819
EBLACK	-0.2057	-0.967	-0.2114	-0.988	-0.1971	-0.922
ERACEOTH	-0.0998	-0.266	-0.0941	-0.250	-0.1168	-0.309
EHISP	0.1479	0.848	0.1468	0.843	0.1435	0.821
GRADEA	-0.0722	-0.084	-0.0280	-0.033	-0.0603	-0.070
AGERLSE	0.8488	0.485	0.9650	0.549	0.8990	0.513
EWORKJOB	0.0991	0.615	0.1109	0.690	0.0967	0.599
ELEGITUN	-0.0060	-0.026	-0.0101	-0.043	-0.0030	-0.013
EUNEMP	0.0936	0.388	0.0763	0.316	0.0859	0.354
EPRIORCM	-0.4604	-4.653	-0.4614	-4.625	-0.4644	-4.663
ALCONLY	0.3188	0.901	0.3248	0.918	0.3166	0.893
MJALC	0.3865	0.719	0.3773	0.701	0.3982	0.739
MJNOALC	-0.0236	-0.070	-0.0422	-0.125	-0.0218	-0.064
ONEALCY	-0.4974	-1.705	-0.4772	-1.633	-0.4942	-1.692
ONEALCN	0.0346	0.137	0.0185	0.073	0.0456	0.179
TWOALCY	-0.3273	-0.776	-0.3356	-0.793	-0.3104	-0.732
TWOALCN	-0.0990	-0.283	-0.0880	-0.252	-0.0899	-0.256
EDIAGASP	-0.1743	-0.940	-0.1683	-0.902	-0.1700	-0.912
EDIAGDEP	0.1243	0.664	0.1181	0.629	0.1247	0.665
EDIAGBTH	-0.0513	-0.248	-0.0463	-0.220	-0.0635	-0.303
SIGMA	1.4029	9.443	1.4136	9.186	1.4088	9.314
Q	2.8021	1.163	2.9654	1.001	2.9044	1.072
COVARIANCE					0.0998	0.399
CASES	473					

Criterion: Relapse to Drug Use

Survival models were used to estimate the time until relapse to drug use. To enter this analysis, a study subject must have been supervised and been subjected to urine testing during the follow-up period.

	Males		Females	
Variable	Mean	Std Dev	Mean	Std Dev
RELAPSE	0.5378	0.4987	0.4209	0.4943
COMPARIS	0.2329	0.4228	0.1698	0.3759
TIMETODG	490.6596	421.2911	590.2698	431.2594
WASTREAT	0.4291	0.4951	0.3721	0.4839
WASTREAT	0.4291	0.4951	0.3721	0.4839
ESPOUDRG	-0.5201	0.8544	0.1302	0.9926
EPSTMHTX	-0.6111	0.7918	-0.2047	0.9800
DAILYNO	0.2506	0.4335	0.1698	0.3759
EPSTDGTX	-0.3085	0.9515	-0.1860	0.9837
EPSTETOH	-0.8735	0.4869	-0.9349	0.3554
EDRUGIRY	-0.7943	0.6077	-0.8605	0.5101
ENOCCC	-0.4941	0.8697	-0.5442	0.8399
ETXIND	-0.2524	0.8547	-0.0977	0.8743
ETXGRP	-0.4309	0.6556	-0.3558	0.6231
ETXBTH	-0.4090	0.6857	-0.2860	0.7093
EAAYES	-0.6336	0.7739	-0.5116	0.8602
UA_RATE	1.1401	0.5784	1.1603	0.5571
PC_RATE	0.3857	0.2919	0.4002	0.2694
CC_RATE	0.3395	0.4106	0.3420	0.4097
ESPOUSE	-0.4013	0.8104	-0.6442	0.6871
ECOM_LAW	-0.4267	0.7812	-0.6512	0.6754
EBLACK	-0.2459	0.9561	-0.1000	0.9832
ERACEOTH	-0.5839	0.5433	-0.5116	0.5493
EHISP	-0.8357	0.5494	-0.8279	0.5615
GRADEA	0.6067	0.1007	0.5743	0.1074
AGERLSE	0.3708	0.0883	0.3501	0.0747
EWORKJOB	0.2128	0.9058	-0.0419	0.9074
ELEGITUN	-0.2819	0.5397	-0.3488	0.6289
EUNEMP	-0.2358	0.6011	-0.3395	0.6413
EPRIORCM	0.3794	0.9255	-0.1953	0.9819
ALCONLY	0.1803	0.3845	0.0953	0.2940
MJALC	0.0650	0.2466	0.0372	0.1895
MJNOALC	0.0981	0.2975	0.0767	0.2665
ONEALCY	0.0904	0.2869	0.1186	0.3237
ONEALCN	0.1117	0.3151	0.2233	0.4169
TWOALCY	0.0390	0.1937	0.0558	0.2298
TWOALCN	0.0502	0.2185	0.0884	0.2842
EDIAGASP	-0.2482	0.8828	-0.3628	0.7376
EDIAGDEP	-0.4651	0.6383	-0.3279	0.7766
EDIAGBTH	-0.4639	0.6401	-0.3837	0.7123
CASES	1692		430	

Relapse to Drug Use: Males (Lognormal Survival Model)

	Adjustment Method					
	Unadjusted		Instruments		Heckman-Type	
Parameters	Estimates	Est./s.e.	Estimates	Est./s.e	Estimates	Est./s.e
CONSTANT	4.3625	8.389	4.3493	8.345	4.3642	8.406
WASTREAT	0.3435	2.868	0.4615	2.453	0.7843	3.256
ESPOUDRG	0.0422	0.602	0.0392	0.559	0.0400	0.572
EPSTMHTX	-0.0222	-0.285	-0.0218	-0.280	-0.0244	-0.315
DAILYNO	0.2096	1.263	0.2055	1.238	0.2169	1.309
EPSTDGTX	-0.2247	-3.499	-0.2224	-3.461	-0.2211	-3.452
EPSTETOH	-0.2000	-1.562	-0.2034	-1.587	-0.2107	-1.647
EDRUGIRY	-0.6996	-7.784	-0.7001	-7.781	-0.7018	-7.830
ENOCCC	-0.0974	-1.445	-0.1153	-1.727	-0.0935	-1.388
ETXIND	0.0079	0.076	0.0060	0.058	0.0004	0.004
ETXGRP	0.2275	1.533	0.2344	1.578	0.2265	1.528
ETXBTH	-0.5877	-4.385	-0.5867	-4.374	-0.5753	-4.297
EAAYES	0.0080	0.105	0.0131	0.171	0.0071	0.094
UA_RATE	-0.6843	-6.046	-0.6824	-6.030	-0.6871	-6.080
PC_RATE	-0.0828	-0.424	-0.0640	-0.327	-0.0640	-0.328
CC_RATE	-0.2202	-1.554	-0.2270	-1.600	-0.2227	-1.576
ESPOUSE	0.4045	3.950	0.4113	4.013	0.4072	3.981
ECOM_LAW	-0.1288	-1.258	-0.1384	-1.353	-0.1371	-1.340
EBLACK	-0.4800	-3.390	-0.4819	-3.394	-0.4561	-3.219
ERACEOTH	-0.0736	-0.297	-0.0780	-0.314	-0.1142	-0.461
EHISP	-0.3354	-3.105	-0.3375	-3.124	-0.3368	-3.123
GRADEA	1.0633	1.763	1.0520	1.744	1.0732	1.784
AGERLSE	2.2112	3.007	2.1053	2.856	2.0026	2.705
EWORKJOB	0.1874	1.763	0.1942	1.825	0.1926	1.816
ELEGITUN	-0.1883	-0.878	-0.1759	-0.819	-0.1721	-0.803
EUNEMP	-0.1376	-0.865	-0.1590	-0.996	-0.1679	-1.054
EPRIORCM	-0.4045	-5.991	-0.3995	-5.914	-0.3959	-5.866
ALCONLY	0.0978	0.562	0.0832	0.477	0.0740	0.425
MJALC	-0.4487	-1.767	-0.4453	-1.752	-0.4628	-1.826
MJNOALC	-0.4797	-2.197	-0.4981	-2.282	-0.4832	-2.217
ONEALCY	-0.0952	-0.419	-0.1050	-0.462	-0.1128	-0.498
ONEALCN	-0.6027	-2.889	-0.6095	-2.917	-0.6068	-2.916
TWOALCY	-0.7964	-2.601	-0.8335	-2.715	-0.8574	-2.792
TWOALCN	-0.5973	-2.182	-0.6387	-2.325	-0.6059	-2.218
EDIAGASP	-0.0599	-0.543	-0.0696	-0.631	-0.0679	-0.616
EDIAGDEP	-0.0816	-0.485	-0.0702	-0.417	-0.0912	-0.543
EDIAGBTH	0.0993	0.593	0.1023	0.610	0.1237	0.738
SIGMA	2.0137	25.992	2.0133	26.006	2.0291	25.921
Q	3.2306	3.335	3.2084	3.384	3.3762	3.055
COVARIAN					-0.3254	-2.074

Relapse to Drug Use: Females (Lognormal Survival Model)

	Adjustment Method					
	Unadjusted		Instruments		Heckman-Type	
Parameters	Estimates	Est./s.e	Estimates	Est./s.e	Estimates	Est./s.e.
CONSTANT	6.0648	4.907	5.9553	4.816	6.0615	4.904
WASTREAT	0.3822	1.367	0.4356	0.942	0.3279	0.751
ESPOUDRG	0.0342	0.248	0.0391	0.283	0.0338	0.245
EPSTMHTX	0.3449	2.305	0.3383	2.259	0.3452	2.307
DAILYNO	1.1195	2.417	1.1305	2.431	1.1179	2.412
EPSTDGTX	-0.2067	-1.411	-0.2119	-1.439	-0.2054	-1.401
EPSTETOH	0.0040	0.009	-0.0034	-0.008	0.0055	0.013
EDRUGIRY	-0.9896	-4.174	-0.9907	-4.168	-0.9898	-4.174
ENOCCC	0.0775	0.475	0.0620	0.381	0.0750	0.459
ETXIND	0.2300	1.036	0.2369	1.065	0.2305	1.039
ETXGRP	-0.1385	-0.377	-0.1646	-0.447	-0.1347	-0.367
ETXBTH	-0.8704	-3.220	-0.8568	-3.157	-0.8734	-3.226
EAAYES	0.4894	2.956	0.4757	2.875	0.4901	2.959
UA_RATE	-0.3551	-1.266	-0.3361	-1.190	-0.3588	-1.273
PC_RATE	-0.1266	-0.259	-0.1123	-0.228	-0.1295	-0.264
CC_RATE	-0.8217	-2.538	-0.8259	-2.544	-0.8226	-2.540
ESPOUSE	0.5315	1.713	0.5491	1.762	0.5279	1.697
ECOM_LAW	-0.2581	-0.853	-0.2750	-0.907	-0.2544	-0.839
EBLACK	-1.1114	-2.894	-1.1193	-2.912	-1.1075	-2.875
ERACEOTH	1.2357	1.744	1.2385	1.752	1.2292	1.730
EHISP	-0.1041	-0.396	-0.1116	-0.424	-0.1077	-0.409
GRADEA	-0.5320	-0.396	-0.5025	-0.373	-0.5299	-0.394
AGERLSE	4.3820	2.256	4.4226	2.260	4.4051	2.260
EWORKJOB	0.1250	0.528	0.1324	0.558	0.1243	0.525
ELEGITUN	0.0265	0.074	0.0151	0.042	0.0293	0.082
EUNEMP	-0.2544	-0.743	-0.2541	-0.739	-0.2595	-0.756
EPRIORCM	-0.3981	-2.861	-0.3969	-2.834	-0.4002	-2.867
ALCONLY	0.2044	0.377	0.2303	0.425	0.2034	0.375
MJALC	-0.6274	-0.866	-0.6446	-0.887	-0.6229	-0.860
MJNOALC	0.2422	0.427	0.2315	0.407	0.2460	0.433
ONEALCY	-0.1471	-0.291	-0.1123	-0.222	-0.1476	-0.292
ONEALCN	-0.2040	-0.496	-0.2249	-0.544	-0.1990	-0.483
TWOALCY	-0.9885	-1.630	-1.0107	-1.663	-0.9850	-1.624
TWOALCN	0.3366	0.596	0.3620	0.639	0.3380	0.598
EDIAGASP	0.2845	0.978	0.2779	0.948	0.2887	0.991
EDIAGDEP	0.1619	0.567	0.1654	0.576	0.1594	0.558
EDIAGBTH	-0.4256	-1.442	-0.4098	-1.380	-0.4314	-1.455
SIGMA	2.1963	17.279	2.2024	17.280	2.1964	17.279
Q	12.8513	0.063	12.5129	0.064	14.4636	0.032
COVARIANCE					0.0394	0.162

Criterion: Employment following Release

The criterion was the percentage of time that the study subject was employed during the follow-up period. We used a two-limit tobit model to analyze this outcome.

	Males		Females	
Variable	Mean	Std Dev	Mean	Std Dev
COMPARIS	0.2409	0.4277	0.1822	0.3864
EMPRATE	0.6809	0.3701	0.5865	0.3774
CONSTANT	1.0000	-----	1.0000	-----
WASTREAT	0.4140	0.4927	0.3623	0.4812
ESUPILL	-0.2201	0.9757	-0.2034	0.9801
NVERUNEM	-0.3829	0.8909	-0.5911	0.7544
NVRWORK	-0.6013	0.6001	-0.6716	0.6218
EVOCEDUC	-0.3435	0.9394	-0.4407	0.8986
PERCEN_U	0.1030	0.1736	0.0906	0.1725
AGEFIRCO	0.2665	0.0958	0.2994	0.0847
ENOCCC	-0.4615	0.8874	-0.5381	0.8438
ETXIND	-0.3064	0.8458	-0.1674	0.8716
ETXGRP	-0.4719	0.6488	-0.4004	0.6273
ETXBTH	-0.4522	0.6773	-0.3390	0.7075
EAAYES	-0.6450	0.7644	-0.5381	0.8438
UA_RATE	1.0504	0.6335	1.0585	0.6264
PC_RATE	0.3757	0.2926	0.3954	0.2694
CC_RATE	0.3367	0.4272	0.3386	0.4315
ESPOUSE	-0.3971	0.8133	-0.6335	0.6982
ECOM_LAW	-0.4265	0.7795	-0.6483	0.6735
WORKNA	0.0306	0.1722	0.0530	0.2242
EBLACK	-0.2534	0.9531	-0.1123	0.9808
ERACEOTH	-0.5849	0.5465	-0.5148	0.5527
EHISP	-0.8351	0.5503	-0.8263	0.5639
GRADEA	0.6085	0.1031	0.5793	0.1092
AGERLSE	0.3737	0.0902	0.3515	0.0789
EWORKJOB	0.2256	0.9041	-0.0233	0.9085
ELEGITUN	-0.2753	0.5398	-0.3369	0.6308
EUNEMP	-0.2348	0.5937	-0.3347	0.6336
EPRIORCM	0.3708	0.9290	-0.2076	0.9792
ALCONLY	0.1912	0.3933	0.0932	0.2910
MJALC	0.0612	0.2397	0.0381	0.1917
MJNOALC	0.0950	0.2933	0.0869	0.2819
ONEALCY	0.0863	0.2809	0.1208	0.3262
ONEALCN	0.1092	0.3120	0.2140	0.4106
TWOALCY	0.0371	0.1892	0.0508	0.2199
TWOALCN	0.0492	0.2162	0.0847	0.2788
EDIAGASP	-0.2556	0.8785	-0.3686	0.7370
EDIAGDEP	-0.4642	0.6425	-0.3326	0.7776
EDIAGBTH	-0.4648	0.6417	-0.3919	0.7086
FITTEDC	-0.6351	0.8208	-0.7853	1.2190
COMPLETE	0.4140	0.4927	0.3623	0.4812
CASES	1390			

Employment: Males (Two-Limit Tobit Model)

	Adjustment Method					
	Unadjusted		Instruments		Heckman-Type	
Parameters	Estimates	Est./s.e.	Estimates	Est./s.e.	Estimates	Est./s.e.
CONSTANT	0.3189	2.947	0.3225	2.965	0.3179	2.938
WASTREAT	0.0332	1.188	0.0233	0.532	0.0660	1.188
ESUPILL	-0.0123	-0.832	-0.0122	-0.824	-0.0121	-0.814
NVERUNEM	0.0806	2.724	0.0822	2.782	0.0805	2.720
NVRWORK	-0.0647	-1.578	-0.0669	-1.634	-0.0648	-1.581
EVOCEDUC	0.0098	0.683	0.0103	0.714	0.0090	0.627
PERCEN_U	0.2073	2.632	0.2051	2.598	0.2035	2.577
AGEFIRCO	0.6305	3.219	0.6314	3.219	0.6335	3.234
ENOCCC	-0.0932	-6.158	-0.0955	-6.365	-0.0930	-6.144
ETXIND	0.0149	0.584	0.0156	0.612	0.0142	0.558
ETXGRP	0.0574	1.563	0.0582	1.584	0.0576	1.570
ETXBTH	-0.1014	-3.045	-0.1023	-3.068	-0.1007	-3.021
EAAYES	0.0593	3.292	0.0597	3.312	0.0593	3.292
UA_RATE	0.0729	2.945	0.0740	2.987	0.0722	2.918
PC_RATE	0.0191	0.415	0.0198	0.430	0.0200	0.434
CC_RATE	-0.0547	-1.749	-0.0554	-1.771	-0.0549	-1.757
ESPOUSE	0.0864	3.716	0.0868	3.734	0.0864	3.720
ECOM_LAW	-0.0457	-1.941	-0.0462	-1.964	-0.0458	-1.947
WORKNA	-3.8637	-0.179	-3.8611	-0.179	-3.0405	-0.559
EBLACK	-0.0591	-1.962	-0.0604	-1.997	-0.0575	-1.900
ERACEOTH	-0.1118	-2.148	-0.1107	-2.121	-0.1146	-2.196
EHISP	-0.0814	-3.318	-0.0818	-3.332	-0.0814	-3.318
GRADEA	0.3736	2.681	0.3712	2.662	0.3761	2.698
AGERLSE	-0.6446	-3.324	-0.6464	-3.321	-0.6596	-3.381
EWORKJOB	0.1556	5.984	0.1561	6.002	0.1560	6.000
ELEGITUN	-0.1881	-3.624	-0.1881	-3.621	-0.1872	-3.607
EUNEMP	0.0223	0.592	0.0213	0.562	0.0203	0.537
EPRIORCM	-0.0294	-1.751	-0.0291	-1.736	-0.0289	-1.720
ALCONLY	-0.0194	-0.509	-0.0199	-0.520	-0.0211	-0.553
MJALC	0.0380	0.649	0.0385	0.657	0.0362	0.617
MJNOALC	0.0049	0.103	0.0028	0.059	0.0047	0.097
ONEALCY	-0.0147	-0.290	-0.0140	-0.275	-0.0166	-0.327
ONEALCN	-0.1051	-2.269	-0.1040	-2.241	-0.1055	-2.276
TWOALCY	-0.1460	-2.055	-0.1473	-2.069	-0.1500	-2.105
TWOALCN	-0.0064	-0.099	-0.0074	-0.114	-0.0069	-0.106
EDIAGASP	0.0315	1.238	0.0309	1.214	0.0317	1.245
EDIAGDEP	-0.0085	-0.223	-0.0066	-0.174	-0.0096	-0.253
EDIAGBTH	-0.0608	-1.622	-0.0625	-1.667	-0.0595	-1.587
Sigma	0.5104	40.117	0.5107	40.116	0.5106	40.076
COVARIANCE					-0.0953	-0.681
CASES		1390				

Employment: Females (Two Limit Tobit Model)

	Adjustment Method					
	Unadjusted		Instruments		Heckman-Type	
Parameters	Estimates	Est./s.e.	Estimates	Est./s.e.	Estimates	Est./s.e.
CONSTANT	0.2969	1.680	0.2876	1.618	0.2998	1.694
WASTREAT	0.1030	2.176	0.1306	1.692	0.1333	1.678
ESUPILL	-0.0009	-0.038	-0.0007	-0.028	-0.0013	-0.055
NVERUNEM	0.0951	1.964	0.0958	1.965	0.0972	1.998
NVRWORK	-0.1530	-2.495	-0.1574	-2.547	-0.156	-2.529
EVOCEDUC	0.0583	2.307	0.0624	2.476	0.0581	2.297
PERCEN_U	0.0965	0.754	0.0884	0.687	0.0938	0.732
AGEFIRCO	0.3421	0.778	0.3249	0.736	0.3399	0.773
ENOCCC	-0.0892	-3.317	-0.0935	-3.484	-0.0885	-3.283
ETXIND	-0.0271	-0.686	-0.0255	-0.645	-0.0274	-0.695
ETXGRP	0.0270	0.429	0.0175	0.277	0.0248	0.393
ETXBTH	-0.1132	-2.359	-0.1069	-2.217	-0.1119	-2.327
EAAYES	0.0646	2.385	0.0625	2.297	0.0642	2.37
UA_RATE	0.0347	0.829	0.0367	0.873	0.0357	0.851
PC_RATE	-0.0579	-0.688	-0.0531	-0.626	-0.0546	-0.647
CC_RATE	0.0031	0.061	0.0030	0.058	0.0046	0.09
ESPOUSE	-0.0512	-1.101	-0.0466	-0.997	-0.0496	-1.064
ECOM_LAW	0.0193	0.408	0.0126	0.265	0.0172	0.362
WORKNA	-3.2924	-0.122	-3.3159	-0.122	-3.0308	-0.055
EBLACK	-0.0948	-1.814	-0.1047	-1.976	-0.0986	-1.865
ERACEOTH	0.0822	0.889	0.0963	1.030	0.0884	0.946
EHISP	-0.0910	-2.162	-0.0893	-2.112	-0.0888	-2.097
GRADEA	0.0650	0.300	0.0587	0.270	0.0636	0.293
AGERLSE	-0.0087	-0.019	0.0092	0.020	-0.0186	-0.04
EWORKJOB	0.0601	1.508	0.0631	1.580	0.0605	1.517
ELEGITUN	-0.1131	-1.834	-0.1163	-1.879	-0.1147	-1.856
EUNEMP	0.0180	0.290	0.0179	0.287	0.0202	0.323
EPRIORCM	-0.0351	-1.377	-0.0339	-1.323	-0.0342	-1.339
ALCONLY	-0.0486	-0.604	-0.0434	-0.538	-0.0487	-0.605
MJALC	-0.0121	-0.099	-0.0200	-0.163	-0.0155	-0.126
MJNOALC	-0.0743	-0.890	-0.0791	-0.944	-0.0763	-0.913
ONEALCY	-0.1041	-1.342	-0.0950	-1.225	-0.105	-1.352
ONEALCN	-0.0293	-0.472	-0.0332	-0.531	-0.0328	-0.525
TWOALCY	0.0425	0.394	0.0344	0.318	0.0403	0.374
TWOALCN	0.0409	0.463	0.0459	0.518	0.0399	0.451
EDIAGASP	-0.0313	-0.649	-0.0331	-0.682	-0.0324	-0.671
EDIAGDEP	0.0848	1.837	0.0825	1.782	0.0851	1.84
EDIAGBTH	-0.0703	-1.367	-0.0649	-1.249	-0.0676	-1.305
Sigma	0.4272	23.398	0.4288	23.395	0.4276	23.347
COVARIANCE					-0.1112	-0.474
CASES	472					

Criterion: Work Level

We next analyzed the work level for those who were part of the labor force. Work level was coded as employed full time during the follow-up period, employed full-time during part of the follow-up period, employed part-time during the follow-up period, and unemployed during the follow-up period. An ordered probit model was the estimation technique.

	Males		Females	
Variable	Mean	Std Dev	Mean	Std Dev
WORKLEVL	2.1030	0.9053	1.9129	0.8429
WASTREAT	0.4149	0.4928	0.3638	0.4816
ESUPILL	-0.2195	0.9759	-0.2054	0.9798
NVERUNEM	-0.3779	0.8925	-0.5759	0.7677
NVRWORK	-0.5974	0.6023	-0.6674	0.6193
EVOCEDUC	-0.3404	0.9405	-0.4286	0.9045
PERCEN_U	0.1040	0.1739	0.0934	0.1741
AGEFIRCO	0.2655	0.0947	0.2982	0.0846
ENOCCC	-0.4658	0.8851	-0.5491	0.8367
ETXIND	-0.2990	0.8476	-0.1897	0.8604
ETXGRP	-0.4670	0.6493	-0.4018	0.6340
ETXBTH	-0.4468	0.6784	-0.3371	0.7175
EAAYES	-0.6405	0.7681	-0.5357	0.8453
UA_RATE	1.0578	0.6322	1.0593	0.6281
PC_RATE	0.3720	0.2922	0.3864	0.2609
CC_RATE	0.3397	0.4298	0.3372	0.4325
ESPOUSE	-0.3947	0.8133	-0.6317	0.6987
ECOM_LAW	-0.4227	0.7814	-0.6451	0.6765
EBLACK	-0.2458	0.9547	-0.1205	0.9802
ERACEOTH	-0.5801	0.5485	-0.5201	0.5512
EHISP	-0.8320	0.5549	-0.8304	0.5579
GRADEA	0.6086	0.1024	0.5813	0.1103
AGERLSE	0.3703	0.0878	0.3509	0.0782
EWORKJOB	0.2340	0.9063	-0.0112	0.9159
ELEGITUN	-0.2839	0.5266	-0.3438	0.6222
EUNEMP	-0.2335	0.5949	-0.3415	0.6252
EPRIORCM	0.3651	0.9312	-0.2054	0.9798
ALCONLY	0.1848	0.3882	0.0982	0.2979
MJALC	0.0627	0.2425	0.0357	0.1858
MJNOALC	0.0974	0.2966	0.0848	0.2789
ONEALCY	0.0851	0.2791	0.1138	0.3180
ONEALCN	0.1075	0.3098	0.2098	0.4076
TWOALCY	0.0381	0.1914	0.0513	0.2209
TWOALCN	0.0487	0.2153	0.0871	0.2822
EDIAGASP	-0.2548	0.8802	-0.3750	0.7343
EDIAGDEP	-0.4653	0.6423	-0.3393	0.7749
EDIAGBTH	-0.4681	0.6380	-0.3929	0.7124
CASES	1786			

Employment Level (Ordered Probit Model)

	Males				Females			
	No Adjustment Probit		Instrumental Variable Probit		No Adjustment Probit		Instrumental Probit	
Variable	Estimate	t-value	Estimate	t-value	Estimate	t-value	Estimate	t-value
WASTREAT	0.02597	0.45	0.03606	0.27	0.18534	1.42	0.50822	2.07
ESUPILL	-0.03236	-1.02	-0.03219	-1.01	0.01400	0.21	0.01462	0.22
NVERUNEM	0.17100	2.77	0.17211	2.79	0.20747	1.60	0.22997	1.74
NVRWORK	-0.15181	-1.71	-0.15332	-1.73	-0.30065	-2.01	-0.33460	-2.21
EVOCEDUC	0.03016	1.00	0.03055	1.01	0.10050	1.46	0.09979	1.45
PERCEN_U	0.47355	2.75	0.47063	2.74	0.75480	2.01	0.72516	1.89
AGEFIRCO	1.38322	3.47	1.38538	3.48	1.53628	1.42	1.51309	1.39
ENOCCC	-0.20155	-6.49	-0.20300	-6.65	-0.22995	-3.04	-0.22212	-2.94
ETXIND	0.02663	0.49	0.02703	0.50	-0.01448	-0.13	-0.01790	-0.16
ETXGRP	0.12230	1.71	0.12294	1.72	-0.03199	-0.19	-0.05838	-0.35
ETXBTH	-0.17736	-2.56	-0.17761	-2.55	-0.19438	-1.38	-0.17608	-1.25
EAAYES	0.09228	2.43	0.09233	2.43	0.21802	2.90	0.21024	2.81
UA_RATE	0.14870	3.00	0.14931	3.02	-0.05234	-0.46	-0.04431	-0.39
PC_RATE	0.08441	0.93	0.08461	0.93	-0.08256	-0.39	-0.05823	-0.28
CC_RATE	-0.08643	-1.48	-0.08681	-1.48	0.01850	0.16	0.03050	0.26
ESPOUSE	0.11846	2.39	0.11865	2.39	0.03648	0.28	0.05340	0.41
ECOM_LAW	-0.05722	-1.18	-0.05765	-1.19	-0.02548	-0.15	-0.04140	-0.25
EBLACK	-0.09593	-1.45	-0.09622	-1.45	-0.19298	-1.18	-0.23368	-1.42
ERACEOTH	-0.22021	-1.90	-0.22050	-1.90	0.15843	0.54	0.22070	0.75
EHISP	-0.12446	-2.39	-0.12469	-2.39	-0.14211	-1.39	-0.13189	-1.26
GRADEA	0.56576	1.96	0.56417	1.96	-0.12977	-0.24	-0.13453	-0.25
AGERLSE	-1.26443	-3.14	-1.27181	-3.13	-0.94267	-0.80	-1.04077	-0.88
EWORKJOB	0.28198	5.29	0.28248	5.29	0.09803	0.90	0.10188	0.94
ELEGITUN	-0.28627	-2.93	-0.28632	-2.94	-0.34720	-2.57	-0.35820	-2.67
EUNEMP	-0.01882	-0.23	-0.02003	-0.24	0.18215	1.04	0.19818	1.13
EPRIORCM	-0.04527	-1.28	-0.04485	-1.27	-0.06496	-0.94	-0.05566	-0.80
ALCONLY	-0.00785	-0.10	-0.00905	-0.12	-0.26930	-1.06	-0.26808	-1.06
MJALC	0.04787	0.39	0.04740	0.38	0.14608	0.49	0.12274	0.41
MJNOALC	-0.01015	-0.09	-0.01170	-0.11	-0.03871	-0.17	-0.05666	-0.25
ONEALCY	-0.05942	-0.53	-0.05990	-0.53	-0.32811	-1.58	-0.33829	-1.62
ONEALCN	-0.22569	-2.38	-0.22470	-2.37	-0.03998	-0.23	-0.07824	-0.45
TWOALCY	-0.27292	-1.82	-0.27581	-1.84	-0.09141	-0.32	-0.12273	-0.43
TWOALCN	0.06297	0.50	0.06160	0.49	-0.00513	-0.02	-0.01098	-0.05
EDIAGASP	0.07551	1.40	0.07509	1.39	-0.12524	-0.91	-0.13941	-1.00
EDIAGDEP	-0.03623	-0.44	-0.03543	-0.43	0.15092	1.17	0.15097	1.18
EDIAGBTH	-0.10535	-1.34	-0.10590	-1.35	-0.06027	-0.44	-0.02967	-0.21
Alpha_1	-0.53949	-2.34	-0.54469	-2.36	-1.20487	-2.62	-1.23474	-2.66
Alpha_2	-0.15219	-0.66	-0.15732	-0.69	-0.56802	-1.26	-0.59477	-1.31
Alpha_3	1.28771	5.58	1.28254	5.57	1.15777	2.60	1.13621	2.53

Criterion: Halfway House Outcomes

The final set of regressions uses the time until failure in a halfway house as the outcome variable. We used a survival model based on a Weibull distribution. Data were limited to those offenders who were assigned to halfway houses.

	Males		Females	
Variable	Mean	Std Dev	Mean	Std Dev
REVOCATION	0.2304	0.4212	0.1711	0.3771
COMPARIS	0.2425	0.4288	0.2249	0.4181
WASTREAT	0.4621	0.4987	0.3716	0.4838
ESUPILL	-0.2669	0.9640	-0.2274	0.9750
NVERUNEM	-0.3550	0.9050	-0.5917	0.7554
NVRWORK	-0.5942	0.5937	-0.6748	0.6179
EVOCEDUC	-0.3482	0.9377	-0.4768	0.8801
PERCEN_U	0.0947	0.1635	0.0824	0.1637
AGEFIRCO	0.2700	0.0945	0.2988	0.0841
ECCCIRY	-0.6450	0.7645	-0.7359	0.6779
EYEARNO	0.3543	0.8452	0.2005	0.7821
EYEARYES	-0.0820	0.6298	0.1247	0.7486
EBLACK	-0.2547	0.9561	-0.0685	0.9828
ERACEOTH	-0.5949	0.5334	-0.4866	0.5605
EHISP	-0.8415	0.5405	-0.8729	0.4886
GRADEA	0.6084	0.1005	0.5847	0.1050
AGERLSE	0.3733	0.0874	0.3524	0.0785
EWORKJOB	0.2778	0.8937	-0.0024	0.9007
ELEGITUN	-0.2527	0.5303	-0.3130	0.6339
EUNEMP	-0.2202	0.5737	-0.3081	0.6401
EPRIORCM	0.3699	0.9294	-0.1687	0.9869
ALCONLY	0.1890	0.3917	0.1100	0.3133
MJALC	0.0583	0.2343	0.0342	0.1820
MJNOALC	0.0908	0.2874	0.0880	0.2837
ONEALCY	0.0806	0.2723	0.1149	0.3193
ONEALCN	0.1043	0.3058	0.2078	0.4062
TWOALCY	0.0373	0.1895	0.0538	0.2259
TWOALCN	0.0508	0.2197	0.0782	0.2689
EDIAGASP	-0.2751	0.8715	-0.3961	0.7206
EDIAGDEP	-0.4682	0.6502	-0.3496	0.7749
EDIAGBTH	-0.4763	0.6379	-0.3961	0.7206
FITTEDC	-0.5977	0.8113	-0.7025	1.2150
COMPLETE	0.4621	0.4987	0.3716	0.4838
CASES	1476		409	

Failure in a Community Corrections Setting (Weibull Survival Model)

	Males				Females			
	No Adjustment		Instrument Variables		No Adjustment		Instrument Variables	
	Parameter Est.	T-Score	Parameter Est.	T-Score	Parameter Est.	T-Score	Parameter Est.	T-Score
Constant	3.966	9.837	4.291	10.016	6.020	5.622	5.998	5.766
WASTREAT	0.525	4.505	0.194	0.764	0.389	1.470	0.470	0.927
ESUPILL	-0.005	-0.093	-0.036	-0.688	-0.054	-0.450	-0.050	-0.417
NVERUNEM	-0.007	-0.074	-0.007	-0.066	-0.376	-1.493	-0.388	-1.485
NVRWORK	0.000	-0.002	-0.018	-0.129	0.237	0.889	0.245	0.885
EVOCEDUC	0.020	0.378	0.038	0.701	0.385	2.474	0.402	2.551
PERCEN_U	0.591	1.897	0.468	1.466	1.968	2.268	1.998	2.285
AGEFIRCO	3.119	4.356	3.178	4.331	5.139	2.878	5.175	2.806
EYEARNO	0.028	0.363	0.140	1.434	-0.046	-0.261	-0.018	-0.092
EYEARYES	-0.079	-0.928	-0.070	-0.808	-0.127	-0.759	-0.186	-1.114
EBLACK	-0.027	-0.240	-0.056	-0.507	-0.144	-0.471	-0.167	-0.553
ERACEOTH	-0.394	-1.968	-0.342	-1.721	0.377	0.670	0.430	0.774
EHISP	-0.052	-0.541	-0.053	-0.535	0.388	1.493	0.361	1.356
GRADEA	0.702	1.252	0.540	0.953	-0.255	-0.283	-0.312	-0.345
AGERLSE	-0.003	-0.005	0.115	0.161	-2.727	-1.668	-2.581	-1.565
EWORKJOB	-0.294	-2.769	-0.276	-2.497	0.534	2.442	0.561	2.564
ELEGITUN	0.620	2.703	0.638	2.624	0.131	0.436	0.120	0.392
EUNEMP	-0.012	-0.079	-0.067	-0.405	-0.433	-1.630	-0.441	-1.637
EPRIORCM	-0.235	-3.590	-0.240	-3.587	-0.285	-2.279	-0.292	-2.362
ALCONLY	-0.274	-2.059	-0.236	-1.756	0.152	0.381	0.130	0.329
MJALC	-0.008	-0.039	0.041	0.193	0.041	0.065	0.087	0.140
MJNOALC	0.232	1.240	0.214	1.142	0.023	0.046	-0.014	-0.027
ONEALCY	-0.212	-1.146	-0.157	-0.845	0.214	0.520	0.194	0.461
ONEALCN	-0.266	-1.645	-0.270	-1.587	0.147	0.477	0.110	0.348
TWOALCY	-0.391	-1.596	-0.288	-1.210	0.098	0.214	0.077	0.166
TWOALCN	-0.198	-0.915	-0.255	-1.158	0.258	0.514	0.291	0.568
EDIAGASP	0.145	1.629	0.134	1.483	-0.279	-1.239	-0.309	-1.375
EDIAGDEP	0.100	0.737	0.080	0.541	0.023	0.100	0.038	0.167
EDIAGBTH	-0.330	-2.537	-0.274	-2.119	-0.119	-0.508	-0.109	-0.465
Q	-0.233	-1.571	-0.410	-1.697	-6.480	0.000	-6.507	0.000
SHAPE	0.698	17.263	0.741	16.045	0.771	6.991	0.776	7.202