Alternative Solutions to the Problem of Selection Bias in an Analysis of Federal Residential Drug Treatment Programs

William Rhodes
Abt Associates

Bernadette Pelissier[1]
Gerald Gaes
William Saylor
Scott Camp
Susan Wallace
Federal Bureau of Prisons

Please address all correspondence to:
William Rhodes
Abt Associates
55 Wheeler Street
Cambridge, MA 02138-1168

Authors' Notes: The opinions expressed in this paper by Abt Associates and Bureau of Prisons staff are solely the opinions of the authors and are not intended to represent the policies or procedures of the Federal Bureau of Prisons or the Department of Justice.

This evaluation was funded in part by the National Institute on Drug Abuse through an interagency agreement with the Bureau of Prisons.

IN PRESS: Accepted by Evaluation Review in September 2000

[1] Bernadette Pelissier served as project director.

ALTERNATIVE SOLUTIONS TO THE PROBLEM OF SELECTION BIAS IN AN ANALYSIS OF FEDERAL RESIDENTIAL DRUG TREATMENT PROGRAMS

ABSTRACT

In an evaluation of prison-based residential drug treatment programs, we use three different approaches to modeling post-release outcomes. Two of the approaches, the instrumental variable and the Heckman approach, attempt to minimize selection bias as an explanation for treatment outcomes. The results from these analyses are compared to regression analysis in which treatment effects are measured by a dummy variable. We discuss the advantage of using more than one method to increase confidence in findings within the context of selection bias issues. The advantages of the Heckman approach, which models selection bias and controls for its consequences, are discussed. Three-year outcome data for 2,315 Federal inmates are used in the analyses where we separately examine the outcome measures of recidivism and drug use for men and women.

ALTERNATIVE SOLUTIONS TO THE PROBLEM OF SELECTION BIAS IN AN ANALYSIS OF FEDERAL RESIDENTIAL DRUG TREATMENT PROGRAMS

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 criminal 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.[2] 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, LaLonda, 1986). This would be no problem if the assumptions were testable, but in many cases they are not or else the test lacks

[2]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.

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 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).[3] Those recent papers have stimulated our own approach to dealing with selection bias in a quasi-experimental design.

PROBLEM STATEMENT

The Federal Bureau of Prisons (BOP) designed 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 and interviews with research subjects to establish baseline conditions, and interviews with Probation officers to monitor post-release

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

behaviors, the Bureau sought to learn whether or not treatment:

1) 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; and

1) decreased the rate of relapse to drug use (based on urine testing) 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[4]) and others did not (hereafter non-DAP facilities). Prisoners in DAP facilities did not differ materially from prisoners in non-DAP 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 non-DAP facilities did not receive treatment (hereafter the non-DAP *control group*).

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)

[4]DAP refers to Drug Abuse Program.

with the outcomes for offenders who were not treated (the non-DAP 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 non-DAP 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:

P_{accept} 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 that 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:

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

This is the expected value of the observed proportion of failures in the non-DAP 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:

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

Here D is the treatment effect. $F_{treated\ population}$ 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: $F_{treated\ population}$ and $F_{untreated\ population}$. Some algebra shows that the expected value of the effect from treatment is:

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

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 the estimate for the treatment effect. Second, the control variables can help adjust for any population difference between DAP and non-DAP 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 (non-DAP 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 instrumental variable is independent of the error term because one of the predictors is the probability-of-volunteering coefficient and the amount of time housed at a DAP site at a time when treatment was available. Inmate assignment to institutions during the study period was independent of drug use history. Furthermore, institutions started their drug treatment programs at different times and this affected

the rate of volunteering for treatment.[5] This probit model is estimated using just those data from the DAP subjects, since the non-DAP 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 may be 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.

RESEARCH DESIGN

RESEARCH SUBJECTS AND DATA

Our sample includes 2315 individuals – 1193 treatment subjects, plus 592 DAP comparison subjects and 530 non-DAP control subjects. Treatment subjects were sampled from

[5] Details about the instrumental variable and other details about the research methods used are found in an unpublished report about this evaluation effort (Pelissier et al., 1998).

Alternative Solutions to the Problem of Selection Bias

20 DAP sites with in-prison residential drug treatment programs which offered, at a minimum, 500 hours of treatment over a 9 month period. These 2315 subjects represent the 86 percent of subjects who were released to supervision.[6] A wide variety of background factors known to be related to recidivism and treatment outcomes, including a number of factors related to drug use that have seldom been examined in previous evaluation studies, were included as covariates.

We sought to learn whether or not in-prison substance abuse treatment could improve post-prison release outcomes on two dimensions of behavior. The Bureau was 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 later.

Applying the above criterion variable to all study subjects is potentially problematic. Federal Probation officers supervised most but not all study subjects.[7] 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.

---

[6] One of our models of recidivism, does however include both supervised and non-supervised subjects, and thus has a larger sample size.

[7] 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, some offenders complete their entire term and are not supervised.

Alternative Solutions to the Problem of Selection Bias

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.

MODELING STRATEGY

We modeled male and female outcomes separately because men and women were in separate treatment programs. In addition, previous literature suggests that the process of change from a drug using and criminal lifestyle to one without drug use and criminal activity may differ between men and women. A thorough representation of male and female differences would have required the inclusion of a large number of interaction terms in analyses of men and women combined.

Parametric survival models typically assume that all subjects must eventually recidivate (fail) if given enough time. An alternative assumption is that a proportion (PRO) will recidivate given a follow-up period of infinite length, but 1-PRO will never recidivate. The likelihood function is easily modified to accommodate this split-population assumption (see Greene,1998). We assumed that:

$$\text{PRO} = \frac{1}{1+e^{-Q}}$$

where Q is a parameter to be estimated.[8]

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.

[8]The parameter Q is reported in the Tables contained in Appendix A.

Alternative Solutions to the Problem of Selection Bias

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 are not similar, then we would be suspicious of the distribution assumptions made about the mixture distribution adopted in the Heckman-type adjustment model.

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

for men and women appear in Appendix A (Tables A1 through Tables A8). In addition, the codebook for the variables used in these regressions is contained in Appendix B. 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.

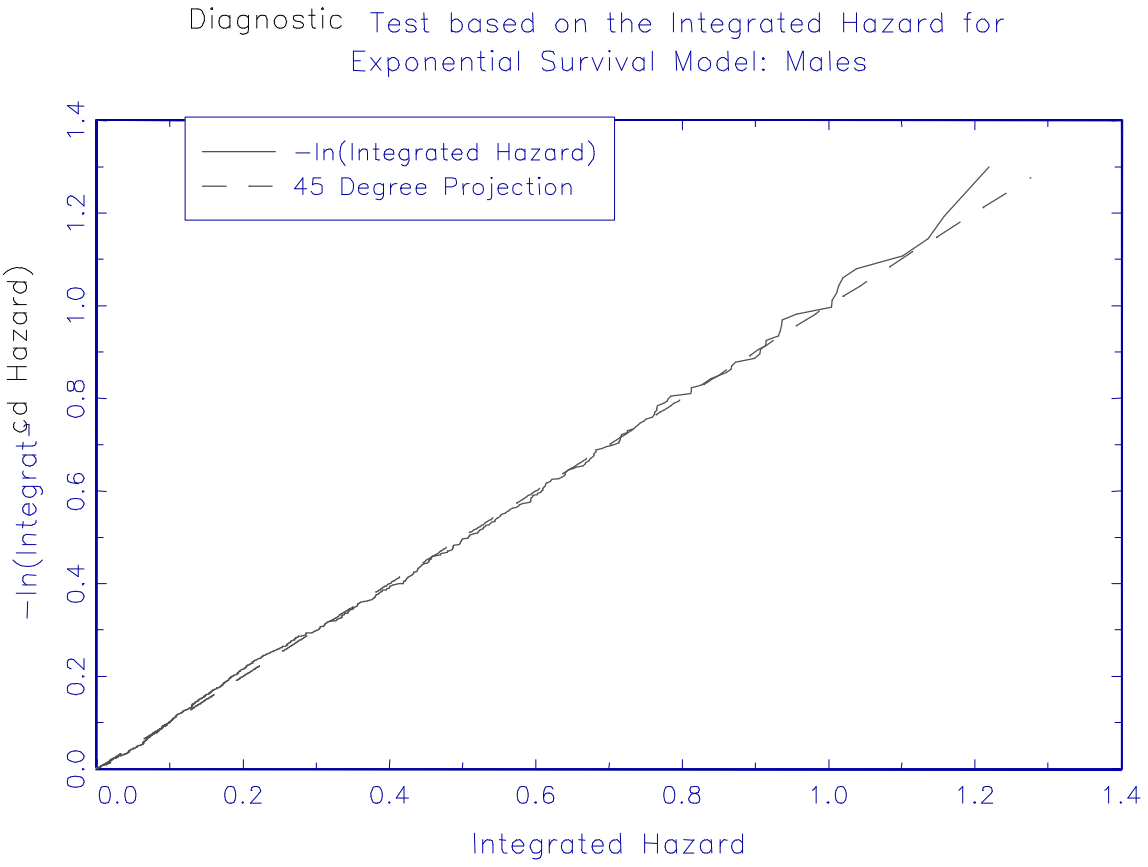
The first test of treatment effectiveness used an arrest as the criterion variable and included 2099 men and 547 women in the analysis. Using the instrumental variable approach, we estimated survival models based on the lognormal, the exponential, and the Weibull distributions. Table 1 reports the values 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.[9]

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 1 and 2.

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

[9] 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.

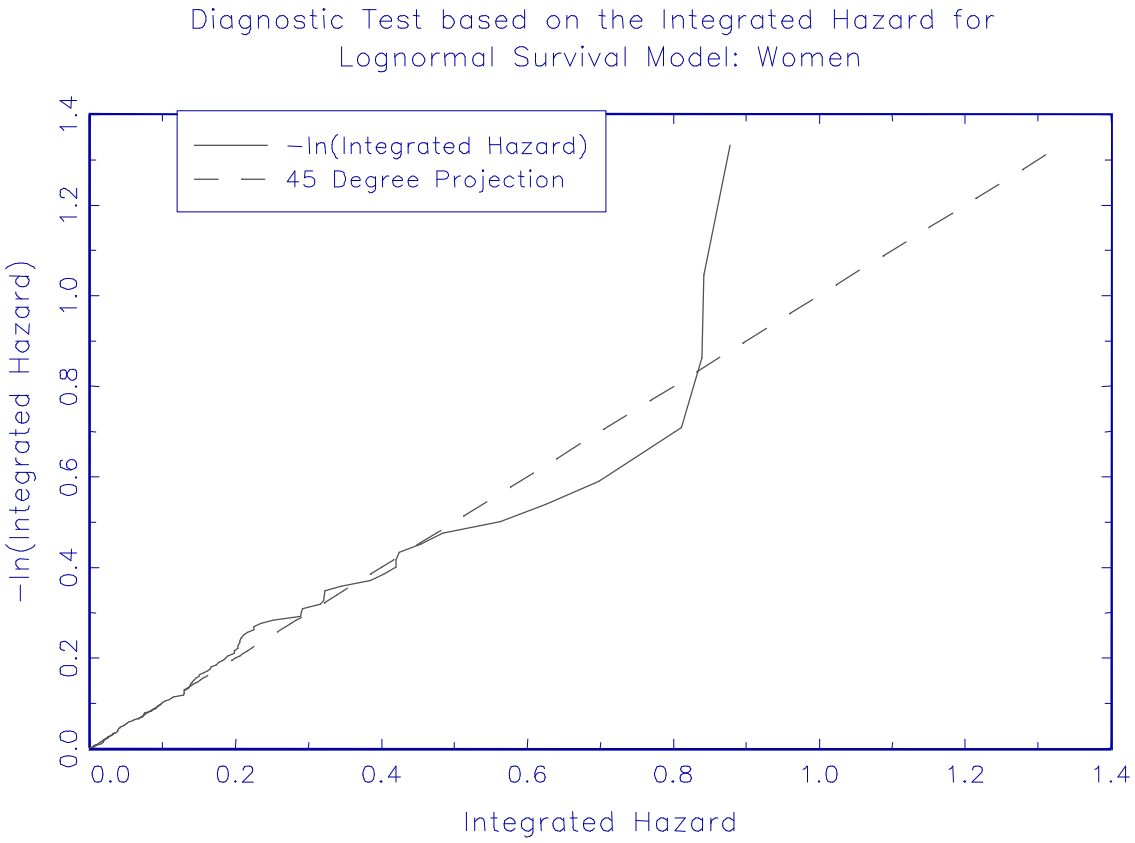
Figure 1



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. 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. Furthermore, unlike basic research where

Figure 2



it is more desirable to take a conservative approach to making conclusions about relationships between various phenomena, evaluation research may be better served by decreasing the probability that an effective treatment is falsely found to be ineffective (Type II error).

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.

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

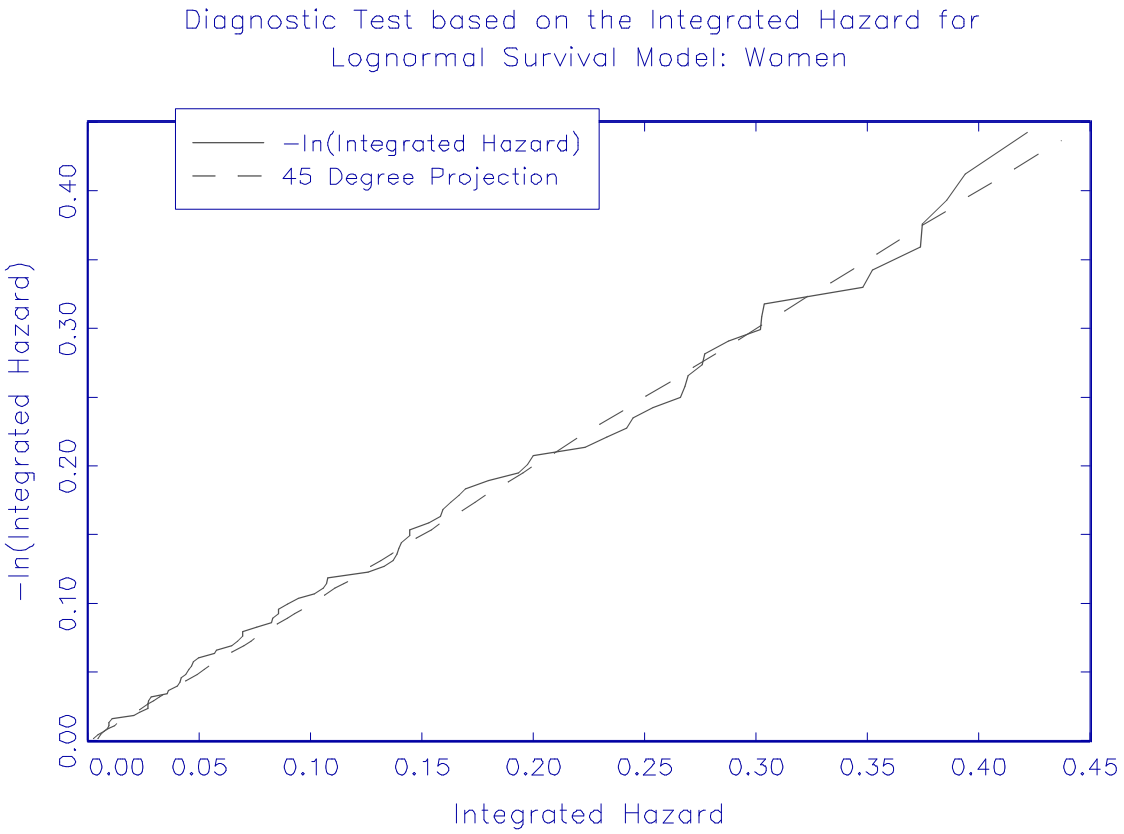

Diagnostic Test based on the Integrated Hazard for
Lognormal Survival Model: Women
−ln(Integrated Hazard)
45 Degree Projection
−ln(Integrated Hazard)
0.00
0.10
0.20
0.30
0.40
0.00
0.05
0.10
0.15
0.20
0.25
0.30
0.35
0.40
0.45
Integrated Hazard

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

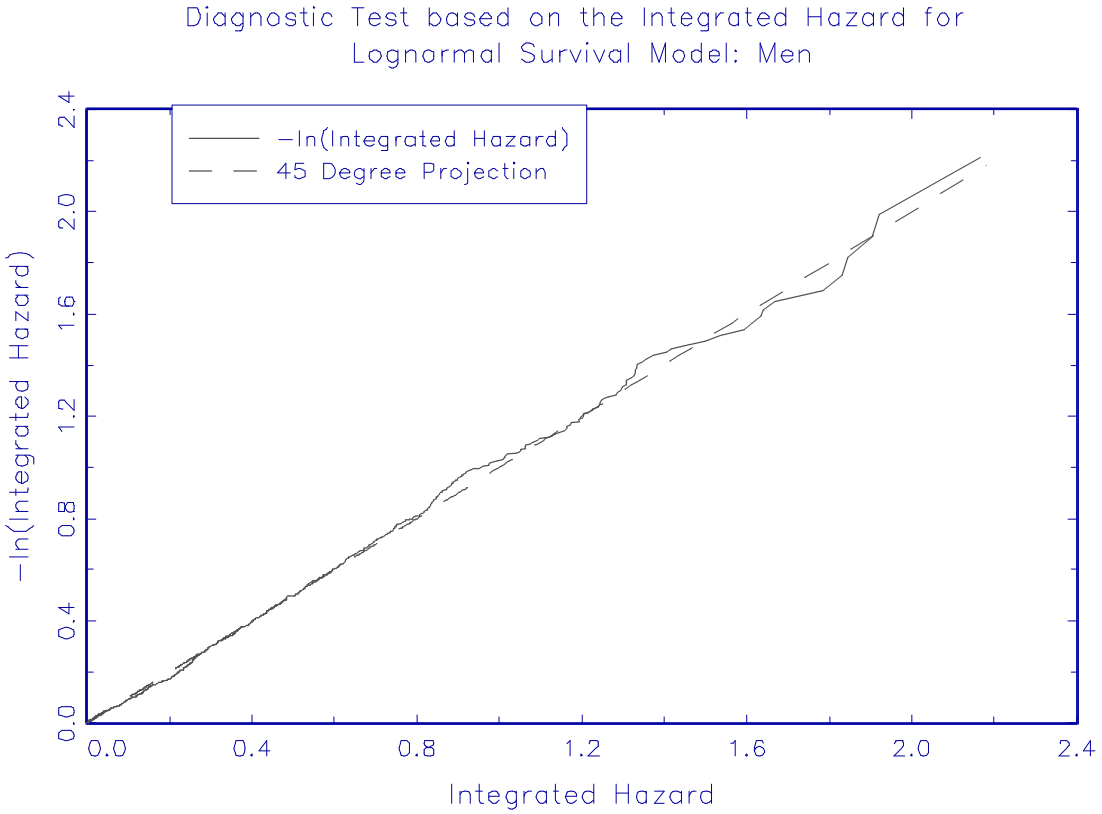
arrest or revocation. When the follow-up period is censored at 18 months, the parameter estimate was not statistically significant (t=0.33). A Cox proportional hazard model lead to the same findings. This leads us to infer that the treatment effect for women is not large and that model misspecification 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.

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.

Figure 4

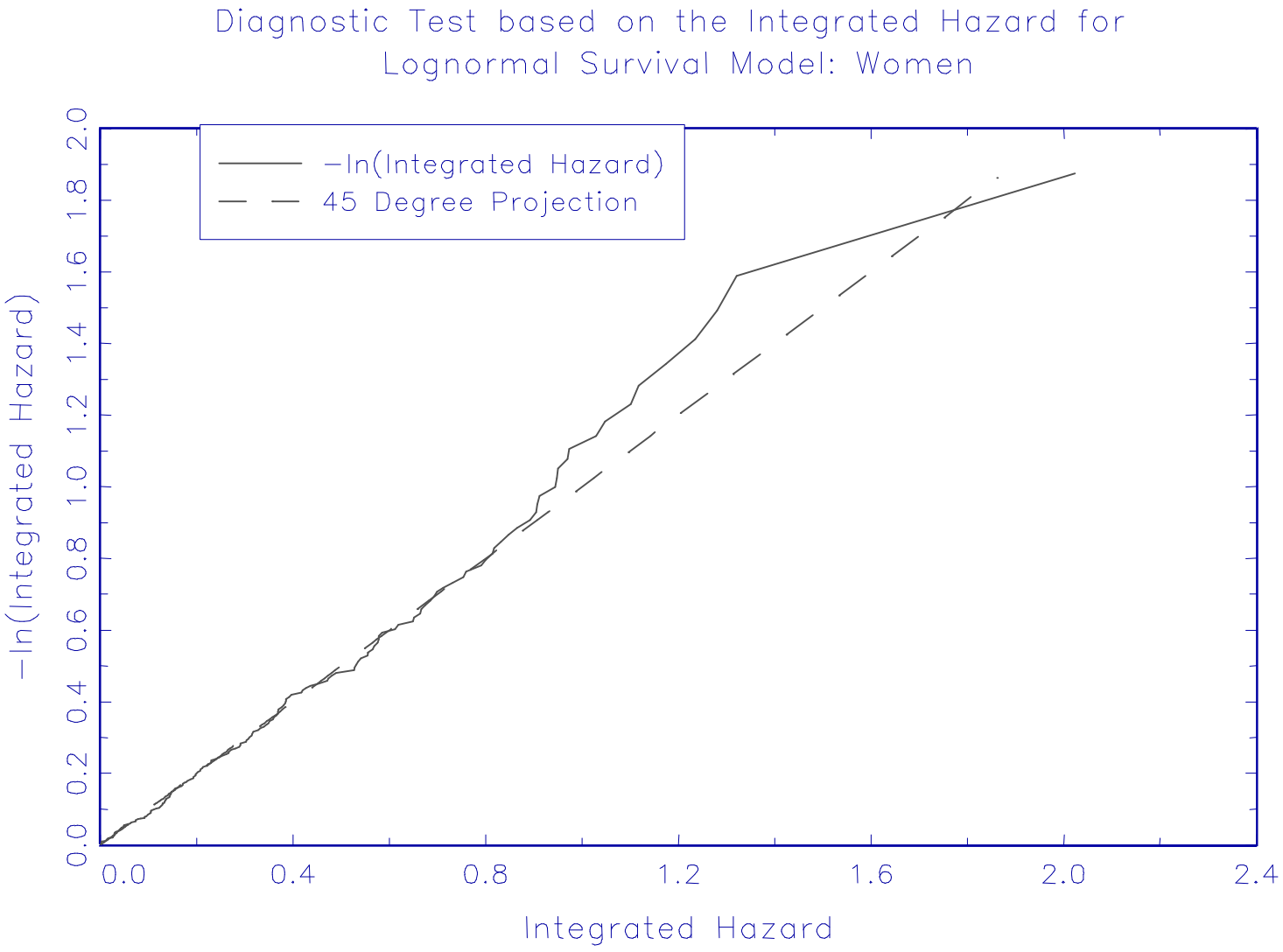

Diagnostic Test based on the Integrated Hazard for
Lognormal Survival Model: Men
−ln(Integrated Hazard)
45 Degree Projection
−ln(Integrated Hazard)
Integrated Hazard
0.0
0.4
0.8
1.2
1.6
2.0
2.4

4

Alternative Solutions to the Problem of 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



Alternative Solutions to the Problem of Selection Bias

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.

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. Parameter estimates from 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:

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

where ρ is the correlation. Allowing $<$ to vary freely, ρ is constrained to fall between 0 and 1. Table 2 reports $<$ and its standard error and Table 8 reports ρ .

Alternative Solutions to the Problem of Selection Bias

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.[10] 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 – were 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 bias.

CONCLUSIONS

Evaluations based on quasi-experimental designs are seldom definitive, but evidence from some quasi-experiments is more compelling than 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.

[10]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.

Alternative Solutions to the Problem of Selection Bias

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 (non-DAP facilities). Because of the way that inmates are placed in BOP facilities, those inmates who were housed in non-DAP facilities were representative of those inmates who were housed in DAP facilities that offered substance abuse treatment. Comparing the outcomes for offenders housed in non-DAP 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 non-DAP 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.

REFERENCES

Bloom, Howard S. (1984). "Accounting For No-Shows in Experimental Evaluation Designs," *Evaluation Review*, 8(2) April: 225-246.

Burtless, G. (1995). "The Case for Randomized Field Trials in Econometric and Policy Research," *The Journal of Economic Perspectives*, 9(2);63-84.

Davidson, Russell and MacKinnon, James G. (1993). *Estimation and Inference in Econometrics.* New York: Oxford University Press

Dehejia, R. and Wahba, S. (1999). "Causal Effects in Nonexperimental Studies: Reevaluating the Evaluation of Training Programs," *Journal of the American Statistical Association* 94(48):1053-1062.

Green, William (1998). *LIMDEP: Version 7 Users Manual*, Econometric Software, Inc.

Heckman, J. (1979). "Sample Selection Bias as a Specification Error," *Econometrica*, 12, 1-145.

Heckman, J. and Smith, J. (1995). "Assessing the Case for Social Experiments," *The Journal of Economic Perspectives*, 9(2): 85-110.

Heckman, J., Ichimura, H., Smith, J. and Todd, P. (1998). "Characterizing Selection Bias Using Experimental Data," *Econometrica* 66(5):1017-1098.

Kalbfleisch, John and Prentice, Ross (1980). *The Statistical Analysis of Failure Time Data*. New York: John Wiley and Sons, Inc.

LaLonda, R. (1986). "Evaluating the Econometric Evaluation of Training Programs with Experimental Data," *American Economic Review*, 76:604-20.

Lancaster, Tony (1990). *The Econometric Analysis of Transition Data*. New York: Cambridge University Press.

Maddala, G. (1983). *Limited Dependent and Qualitative Variables in Econometrics*. New York: Cambridge University Press.

Manski, C. and Nagin. D. (1998). "Bounding Disagreements about Treatment Effects: A Case Study of Sentencing and Recidivism." In *Sociological Methodology*, A. Raftery, ed. Boston, MA and Oxford, U.K.: Blackwell Publishers.

Rhodes, William (1989). "The Criminal Career: Estimates of the Duration and Frequency of Crime Commission," *Journal of Quantitative Criminology* 5(1): 3-32.

Rhodes, W. (1997). "Models for Dealing with Selection Bias in an Evaluation of a Prison-Based Drug Treatment Program." Paper prepared at the request of the Federal Bureau of Prisons and submitted to the National Institute on Drug Abuse by contract through Behavioral and Health Research Inc.

Rhodes, W., Hyatt, R. and Scheiman, P. (1996). "Predicting Misconduct with Drug Tests of Arrestees: Evidence from Eight Settings," *Journal of Quantitative Criminology*.

Smith, H. (1997). "Matching with Multiple Controls to Estimate Treatment Effects – Observational Studies." In *Sociological Methodology 1997,* A. Raftery, ed. Boston MA and Oxford U.K.: Blackwell Publishing Company, 325-354.

Spelman, William (1994). *Criminal Incapacitation.* New York: Plenum Press.

Truitt, L., Rhodes, W., Seeherman, A., Carrigan, K. and Finn, P. (1999). "Phase I: Process and Impact Evaluations of Escambia County, Florida and Jackson County, Missouri Drug Courts." Draft report submitted by Abt Associates Inc. to the National Institute of Justice, October 8, 1999.

Yamaguchi, K. (1986). "Alternative Approaches to Unobserved Heterogeneity: The Analysis of Repeatable Events.@ In N. Tuma, ed. *Sociological Methodology 1986.* San Francisco: Jossey-Bass.

Appendix A – Tables

Table 1
Diagnostic Tests of the Survival Models

Outcome	Gender	Minus Log-likelihood 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

Table 2

Parameter Estimates and T-Score for Estimated Treatment Effect

		Unadjusted		Instrumental Variable		Heckman-Type	
Outcome	Gender	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

Table 3
Estimated Covariance Terms

Outcome	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

Table A1

Rearrest for New Offense: Male Subjects, Supervised and Unsupervised

Exponential Failure Time Model

	Unadjusted		Instrumental Variable		Heckman-Type	
Parameters	Estimates	Est./s.e.	Estimates	Est./s.e.	Estimates	Est./s.e.
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	0.9840	5.957	0.9819	6.012	1.1053	3.054
SIGMA					-1.0898	-1.099

COVARIANCE					1.1793	0.965

Table A2

Rearrest: Female Subjects, Supervised and Unsupervised

Lognormal Failure Time Model

	Unadjusted		Instrumental Variable		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
Q	1.9016	2.227	1.8743	4.492	1.8921	3.994
SIGMA	3.3272	0.094	2.5428	0.330	2.9872	0.220

COVARIANCE	0.0214	0.066

Table A3

Rearrest for New Offense: Male Subjects, Supervised Only

Exponential Survival Model

	Unadjusted		Instrumental Variable		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
COVARIANCE					1.5519	0.924

Table A4

Rearrest for New Offense: Female Subjects, Supervised Only

Lognormal Survival Model

	Unadjusted		Instrumental Variable		Heckman-Type	
Parameters	Estimates	Est./s.e.	Estimates	Est./s.e.	Estimates	Est./s.e.
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

Table A5

Rearrest or Revocation: Male Subjects, Supervised Only

Exponential Survival Model

	Unadjusted		Instrumental Variable		Heckman-Type	
Parameters	Estimates	Est./s.e.	Estimates	Est./s.e.	Estimates	Est./s.e.
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

Table A6

Rearrest or Revocation: Female Subjects, Supervised Only

Lognormal Survival Model

	Unadjusted		Instrumental Variable		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

Table A7

Relapse to Drug Use: Male Subjects, Supervised Only

Lognormal Survival Model

	Unadjusted		Instrumental Variable		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
COVARIANCE					-0.3254	-2.074

Table A8

Relapse to Drug Use: Female Subjects, Supervised Only

Lognormal Survival Model

	Unadjusted		Instrumental Variable		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

APPENDIX B – CODEBOOK FOR VARIABLES USED IN ANALYSES

Unless otherwise indicated, all of the nominal variables are coded as effects vectors.

Age at Time of First Commitment
AGEFIRCO: A continuous variable.

Age at Time of Release from Current Incarceration
AGERLSE: A continuous variable.

Alcohol Treatment History
EPSTETOH: Coded 1 if there was no previous inpatient or outpatient alcohol treatment.
Excluded group had previous history of alcohol treatment.

Diagnoses of Depression and Antisocial Personality
EDIAGDEP: Coded 1 if diagnosis of depression only.
EDIAGASP: Coded 1 if diagnosis of antisocial personality only.
EDIAGBTH: Coded 1 if both diagnoses.
Excluded group has neither diagnosis.

Disciplinary Infraction Before Release
A) ERELIRY: Coded 1 if individual had one or more 100 or 200 level (lowest level is 400) disciplinary infractions within 6 months before release from BOP custody.
Excluded group had no 100 or 200 disciplinary infraction 6 months before release.

B) EDRUGIRY: Coded 1 if individual had one or more drug related disciplinary infractions within 6 months before release from BOP custody.

Excluded group had no drug related disciplinary infraction 6 months before release.

Drug Treatment History

EPSTDGTX: Coded 1 if no previous inpatient or outpatient drug treatment.

Excluded group had previous history of drug treatment.

Drug Use in the Year Before Arrest. (These are dummy-coded variables with no daily alcohol or illicit drug use during year before arrest serving as excluded group and coded as 0).

ALCONLY: Coded 1 if used alcohol only on a daily basis.

MJALC: Coded 1 if used alcohol and marijuana only on a daily basis.

MJNOALC: Coded 1 if used marijuana only on a daily basis.

ONEALCY: Coded 1 if used alcohol and only one illicit drug other than marijuana on a daily basis.

ONEALCN: Coded 1 if did not use alcohol but used only one illicit drug other than marijuana on a daily basis.

TWOALCY: Coded 1 if used alcohol and two or more illicit drugs other than marijuana on a daily basis.

TWOALCN: Coded 1 if did not use alcohol but used only two or more illicit drugs other than marijuana on a daily basis.

Drug Use - Lifetime (These is a dummy-coded variable with the excluded group comprised of those who never used an illicit drug on a daily basis and coded as 0).

DAILYNO: Coded 1 if ever used an illicit drug on a daily basis.

Education

GRADEA: Number of years of education: A continuous variable.

Employment Status in Month Before Incarceration

EWORKJOB: Coded 1 if working full- or part-time.

ELEGITUN: Coded 1 if unemployed because in school, a homemaker, retired, or disabled.

EUNEMP: Coded 1 if unemployed but looking for work.

Excluded group is composed of those unemployed because of involvement in illegal drug use or illegal activities, because the individual has never been employed, or due to other reasons.

Ethnicity

EHISP: Coded 1 for Hispanics.

Excluded group consists of non-Hispanics.

Involved in Post-Release Self-Help Group

EAAYES: Coded 1 if involved in self-help group within first month after release to supervision.

Excluded group did not participate in self-help after release from prison.

Level of selection bias
COVARIAN—Ordinal variable.

Living with Spouse
ESPOUSE: Coded 1 if living with spouse upon release to supervision.
ECOM_LAW: Coded 1 if living with common-law partner.
Excluded group not living with spouse or common-law partner.

Mental Health Treatment History
EPSTMHTX: Coded 1 if subject received no previous inpatient or outpatient mental health treatment.
Excluded group had previous history of mental health treatment

Monthly Rate of Urine Testing
UA_RATE: Average number of urinalysis tests per month during first 6 months of release.

Monthly Rate of Personal Contacts with Probation Officer
PC_RATE: Average number of personal contacts – face-to-face visits at probation office, home or work or telephone contacts – per month during first 6 months of release.

Monthly Rate of Collateral Contacts by Probation Officer
CC_RATE: Average number of collateral contacts – at home or office or by telephone – per month during first 6 months of release.

Prior Commitments
EPRIORCM: Coded 1 if had a major or minor prior commitment.
Excluded group had no prior commitment.

Race
EBLACK: Coded as 1 if black
ERACEOTH: Coded as 1 if of other race.
Excluded group are those of white race.

Received CCC Placement
ECCCNO: Coded 1 if did not receive CCC placement.
Excluded group did not receive a CCC placement.

Split Population

Q:

Spouse With Drug Problem

ESPOUDRG : Coded 1 if spouse ever had a drug problem (before incarceration of subject).

Excluded group did not have a spouse with a drug problem.

Supervised After Release

ESUPRLNO: Coded 1 if not released to supervision.

Excluded group are those who were released to supervision.

Type of Post-Release Treatment

ETXIND: Coded 1 if began receiving individual counseling services only during first month under supervision by a Probation officer.

ETXGRP: Coded 1 if began receiving group counseling services only during first month under supervision by a Probation officer.

ETXBTH: Coded 1 if began receiving both individual and group counseling services during first month under supervision by a Probation officer.

Excluded group had no post-release treatment upon release to supervision.

Type of Subject (used for Heckman model)

WASTREAT: Conditional probability of completing treatment if entered treatment.

Appendix C – 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:

$$Z_i = \alpha_0 + \alpha_1 TR_i + \alpha_2 X_i + \sigma\epsilon_{1i} \qquad \textbf{(2)}$$

w

here:

- 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 assumptions 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:

$$L_1 = \prod_i \phi(T_i)^{R_i}(1 - \Phi(M))^{1-R_i} \quad \textbf{(3)}$$

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 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:

$$\phi_A(t_{A_i}) = \frac{e^{-0.5\frac{(\ln(t_{A_i}) - \alpha_0 - \alpha_1 TR_i - \alpha_2 X_i)^2}{\sigma^2}}}{t_{A_i}\sqrt{2\pi\sigma^2}} \quad \textbf{(4)}$$

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:

$$\lambda_i = e^{Z_i} \quad \textbf{(5)}$$

Unlike the usual exponential model, this specification has an error term ε_1 that must be taken into account in the analysis (see Heckman and Singer, 1985). This introduction of an error term is a convenient and realistic[11] 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:

$$\phi_U(t_{U_i}) = \int_{\epsilon=-\infty}^{\epsilon=+\infty} \lambda_i e^{-\lambda_i t_{U_i}} \eta(\epsilon_1) d\epsilon_1 \qquad \textbf{(6)}$$

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.

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

[11]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 (e.g, 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 transformation (such as the Box-Cox power transformation), the distribution $\eta(\varepsilon)$ can be extremely flexible. Such tests are planned for the future.

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.

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:

$$Y_i = \beta_0 + \beta_1 X_i + \epsilon_{2i} \quad \textbf{(7)}$$

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 treatment=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 non-DAP facility. For people who receive treatment, we use the conditional density function as represented by equation 7 in place of equation 3.

$$\phi_A(t_{A_i}|TR_i = 1) = \frac{e^{-0.5\frac{(\ln(t_{A_i}) - \alpha_0 - \alpha_1 TR_i - \alpha_2 X_i)^2}{\sigma_i^2}}}{t_{A_i}\sqrt{2\pi\sigma_i^2}} \frac{H\left(\frac{\beta_0 + \beta_1 X_i + \rho\frac{\ln(t_{A_i}) - \alpha_0 - \alpha_1 TR_i - \alpha_2 X_i}{\sigma_i}}{\sqrt{1-\rho^2}}\right)}{H(\beta_0 + \beta_1 X_i)} \quad \textbf{(8)}$$

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

$$\phi_A(t_{A_i}|TR_i = 0) = \frac{e^{-0.5\frac{(\ln(t_{A_i}) - \alpha_0 - \alpha_2 X_i)^2}{\sigma_i^2}}}{t_{A_i}\sqrt{2\pi\sigma_i^2}} \frac{H_c\left(\frac{\beta_0 + \beta_1 X_i + \rho\frac{\ln(t_{A_i}) - \alpha_0 - \alpha_2 X_i}{\sigma_i}}{\sqrt{1-\rho^2}}\right)}{H_c(\beta_0 + \beta_1 X_i)} \quad \textbf{(9)}$$

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 in equations 7 and 8 have cumulative distribution counterparts, which must also be substituted into equation 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 (equation 7) or was not treated (equation 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 (equation7) or not entering treatment (equation 8).

In essence, then, the likelihood function is different depending on whether the subject came from a non-DAP facility, came from a DAP facility but did not enter treatment, or came

from a DAP facility and entered treatment. Nevertheless, the generic likelihood (equation 2) holds; we just substitute the correct density and distribution function depending on whether the subject is a member of the non-DAP 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 non-DAP facility, equation 5 represents the density function. When the subject comes from a DAP facility and receives treatment, we use equation 9 in place of equation 5.

$$\phi_U(t_{U_i}|TR_i = 1) = \int_{\epsilon=-\infty}^{\epsilon=+\infty} \lambda_i e^{-\lambda_i t_{U_i}} \eta(\epsilon_1|TR_i = 1) d\epsilon_1 \qquad \textbf{(10)}$$

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

$$\phi_U(t_{U_i}|TR_i = 0) = \int_{\epsilon=-\infty}^{\epsilon=+\infty} \lambda_i e^{-\lambda_i t_{U_i}} \eta(\epsilon_1|TR_i = 0) d\epsilon_1 \qquad \textbf{(11)}$$

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 equations 9 and 10 conditional on TR can be written:

$$\eta(\epsilon_1|TR_i=1) = \frac{\int_{\epsilon_2=-\beta_0-\beta_1 X_1}^{\infty} \eta_b(\epsilon_1,\epsilon_2,\rho)d\epsilon_2}{\int_{\epsilon_2=-\beta_0-\beta_1 X_i}^{\infty} \eta(\epsilon_2)d\epsilon_2} \quad \textbf{(12)}$$

$$\eta(\epsilon_1|TR_i=1) = \frac{\int_{\epsilon_2=-\beta_0-\beta_1 X_1}^{\infty} \eta_b(\epsilon_1,\epsilon_2,\rho)d\epsilon_2}{\int_{\epsilon_2=-\beta_0-\beta_1 X_i}^{\infty} \eta(\epsilon_2)d\epsilon_2} \quad \textbf{(13)}$$

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 equation 2.

The likelihood function is different depending on whether the subject came from the non-DAP control group, the DAP comparison group, or the DAP treatment group. The generic

likelihood (equation 2) holds; we substitute the correct density and distribution function depending on whether the subject is a member of the non-DAP control group, the DAP comparison group, or the DAP treatment group.

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 we 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 non-DAP cases do not enter into this estimation, because those cases have a zero probability of entering treatment.[12] The likelihood for this model is written:

$$L_2 = \prod_i \frac{H(\beta_0 + \beta_1 X_i)^{TR_i} (PS_i (1 - H(\beta_0 + \beta_1 X_i)))^{1 - TR_i}}{H(\beta_0 + \beta_1 X_i) + (PS_i (1 - H(\beta_0 + \beta_1 X_i))} \quad \textbf{(14)}$$

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

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