

Do Treatment Services for Drug Users in Outpatient Drug-Free Treatment Programs Affect Employment and Crime?

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Abstract:

Using the 1992–1995 National Treatment Improvement Evaluation Study data, we employ principal components analysis to create treatment service factors based on both patient self-reports and treatment record extracts. We included these factors in multivariate models for 1,136 outpatient drug-free treatment patients to estimate the relationship between services and post-treatment employment and crime. Although our models indicated some significant predictors of employment and crime, the over- all effects of services were generally insignificant. We conclude that either services are unrelated to outcomes or if they are related then we are not measuring the key aspects of service provision that may be important. The study’s limitations are noted.

Keywords: crime | employment | substance user treatment | treatment services

Article:

Introduction

Numerous studies have found that drug user patients with longer lengths of stay in treatment fare better in posttreatment outcomes than those who leave treatment earlier (Condelli and Hubbard, 1994; De Leon, 1985; Etheridge, Craddock, Hubbard, and Rounds-Bryant, 1999; Hubbard, Craddock, Flynn, Anderson, and Etheridge, 1997; Hubbard et al., 1989; Simpson, Joe, Fletcher, Hubbard, and Anglin, 1999; Simpson and Sells, 1982). The relationship between time in treatment and treatment effectiveness is complex, but factors that influence an individual’s willingness to remain in treatment are also factors that contribute to successful treatment outcomes. These factors include interactions among individual needs, motivation factors, social pressures, and aspects of the treatment program itself such as policy and practices, counselor assignment, accessibility, and level of services offered (Simpson, 2004). Therefore, it is not

surprising that length of stay is consistently found to be a strong predictor of successful treatment outcomes. However, despite these findings, a large gap in our understanding of the drug abuse^{*} treatment,[†] process still exists, which hinders our ability to identify those treatment factors that are most effective.

In the past few years, in an attempt to better understand the treatment process and explain its effectiveness, researchers have focused attention on specific treatment services (Magura, Staines, Blankertz, and Madison, 2004; Etheridge et al., 1999; Fiorentine, 2001; Fiorentine and Anglin, 1996; Friedman, Lemon, and Stein, 2001; Joe et al., 1983; Kraft, Rothbard, Hadley, McLellan, and Asch, 1997; McLellan et al., 1994; McLellan, Arndt, Metzger, Woody, and O'Brien, 1993; Messina, Nemes, Wish, and Wraight, 2001). Despite the intuitive appeal that treatment effectiveness is directly linked to the receipt of treatment services, the empirical evidence to support this view has been inconsistent.

For example, Fiorentine and Anglin (1996) found that, after controlling for length of stay and treatment completion/retention, more frequent participation in group and individual counseling was associated with less relapse, but more frequent participation in family counseling and 12-step programs during treatment did not significantly affect relapse. In contrast, Fiorentine (2001) found that more frequent participation in individual counseling sessions did not decrease the likelihood of relapse, but patients who attended weekly 12-step meetings, attended five group counseling sessions per week, and completed treatment were less likely to relapse.

McLellan and colleagues (1993) found that methadone treatment patients who received more counseling and medical services had better outcomes (e.g., decreases in medical needs, welfare dependency, days of illegal activity, illegal income, psychological problems, and drug use and increases in employment) 24 weeks after treatment entry than patients who received less frequent counseling. However, in a follow-up study, Kraft and colleagues (1997) found that 6 months after the supplemental services were stopped these benefits were not sustained. The only statistically significant difference found was that the group that received enhanced services 6 months prior had higher abstinence rates for heroin than the other two groups. One possible conclusion from this finding is that short-term enhanced interventions are not sufficient to achieve desired long-term benefits. As noted by Kraft and colleagues (1997), it may be possible that the trends observed initially at 24 weeks would have been maintained if the intervention had continued through the 12-month assessment period. Another possibility is that external influences may have affected the outcomes. For example, although the three study groups were similar at the study's beginning, it is possible that after the intervention ended that the groups differentially received standard services or received services outside the program. As noted by the authors, treatment interventions from outside the program were not included in the follow-up period. However, an analysis of external treatment services received during the intervention found no differences in receipt among the three groups.

* The journal's style utilizes the category substance abuse as a diagnostic category. Substances are used or misused; living organisms are and can be abused. Editor's note.

† Treatment can be briefly and usefully defined as a planned, goal-directed change process, which is bounded (culture, place, time, etc.) and can be categorized into professional-based, tradition-based, mutual help-based (AA, NA, etc.) and self-help ("natural recovery") models. There are no unique models or techniques used with substance users—of whatever types—that aren't also used with non-substance users. Editor's note.

In a review and critique of the literature evaluating special vocational interventions for substance users in treatment, Magura et al. (2004) concluded that substance-user treatment modalities (e.g., residential, outpatient nonmethadone, methadone) have not been shown to be effective in increasing patients' employment after treatment (e.g., Hubbard et al., 1997; Schildhaus, Gerstein, Brittingham, Cerbone, and Dugoni, 2000). However, Magura et al. (2004) note that a few studies have found some effectiveness. For example, using data from the Alcohol and Drug Services Study (ADSS), Reif, Horgan, Ritter, and Tompkins (2004) found that unemployed outpatient nonmethadone patients who received employment counseling were more likely to work following treatment than those patients who did not receive employment counseling.

Nemes, Wish, and Messina (1999) studied services received by patients in therapeutic communities and found no relationship between services received and employment outcomes; however, they did find a relationship between services and crime outcomes. Specifically, they found that receiving group counseling sessions was associated with reduced likelihood of post-discharge arrest. Similarly, they also found that the number of total services received was negatively associated with post-discharge crime. In a related study, Messina and colleagues (2001) also found that the total number of services received was negatively associated with post-discharge arrest. However, specific services received (e.g., vocational, self-help, HIV education, and medical referrals) were not significantly associated with post-discharge arrest.

Findings such as these suggest that the typical drug user's treatment experience may better target factors related to criminal behavior rather than employment. For example, a reduction in drug use may lead to a reduction in criminal behavior, especially if that criminal behavior is drug-related. However, reducing drug use does not guarantee posttreatment employment. Drug users may lack the necessary job skills needed to obtain employment, and treatment programs may need to offer services that specifically target improving employability in addition to the usual counseling and educational services for drug user behavior. Furthermore, employment opportunity is also dependent on factors that are external to the treatment program such as the area's economic environment (e.g., demand for workers, rates of unemployment) as well as the job market for a range of skill sets.

Although the evidence that services may improve treatment effectiveness is accumulating, it is not conclusive. In some studies, it appears that certain services do affect treatment outcomes, but in other studies these effects are not present or they are in the opposite direction. One possible explanation for these inconsistent findings lies in the difficulty in reliably measuring the specific services received by patients. Retrospective self-reports of services received are subject to memory limitations, definitional problems, self-serving bias, and a broad list of other barriers. Data collected by abstracting treatment records address many of these limitations, but treatment records are often incomplete and their contents may vary across providers because they represent clinical summaries of what that specific provider judged to be important and do not include all services provided to patients. We hypothesize that a potential difficulty in establishing the link between services and outcomes comes from measurement error. If the number of services received is measured with error, then the estimated relationship between services and treatment outcomes may be biased toward zero.

In this article, we develop a model of the relationship between treatment services and outcomes that directly incorporates the potential for errors in the measurement of treatment services. We combine two measures of treatment services (self-reports and record extractions) to create treatment service constructs that have less noise than either source of treatment service provision measures alone. Assuming that the measurement errors for the two data sources are uncorrelated, service measures that include a combination of the two data sources may better capture the true treatment experience and increase our ability to account for the variance in employment and crime outcomes. We then evaluate the extent to which these refined treatment service measures are associated with posttreatment employment and crime behavior. We hypothesize that, controlling for length of stay and treatment completion, patients who receive more treatment services are more likely to be employed and less likely to commit a crime after treatment than patients who receive fewer treatment services.

Data

Our analysis is a secondary data analysis that uses data collected as part of the National Treatment Improvement Evaluation Study (NTIES), which was conducted by the National Opinion Research Center in collaboration with RTI. NTIES was an observational study funded by the Center for Substance Abuse Treatment (CSAT) to determine how CSAT demonstration grants initiated between 1989 and 1992 were being used and to estimate the improvement in treatment services generated by such funding. NTIES provides data on treatment services and outcomes on the largest drug user treatment follow-up sample in the substance-user treatment field (National Evaluation Data Services [NEDS], 1999a).

The NTIES sample was a purposive sample chosen to meet the specific needs of CSAT and is therefore not representative of patients or programs nationally. Rather, it is representative of the program areas in CSAT's demonstration grant program in 1990–1991. The NTIES sample consisted of patients in treatment programs that received direct funding from any of three demonstration grant areas: the Target Cities program aimed at improving treatment in large cities, the Critical Populations program aimed at improving access and treatment for disadvantaged populations, and the Criminal Justice program aimed at providing services to individuals within the criminal justice system (U.S. Department of Health and Human Services [USDHHS], 1997). These grants typically supported under-served populations, such as minorities, pregnant women, at-risk women, public housing residents, welfare recipients, those in the criminal justice system, and adolescents. Therefore, when compared with populations nationally, NTIES findings are most representative of low-income groups receiving treatment in public sector programs (NEDS, 1999b).

NTIES collected patient data between 1992 and 1995 on 6,593 patients enrolled for treatment at one of 71 participating treatment programs representing six treatment modalities: methadone maintenance, methadone detoxification, outpatient drug-free, short-term residential, long-term residential, and correctional (see the NTIES final report [USDHHS, 1997] for more details on the study design). Participating programs were located in 21 geographic areas located throughout the United States. Included areas consisted of cities (e.g., Philadelphia, Los Angeles, Seattle) and regions of States (e.g., southern Alabama, northern Nevada) located in each of the four U.S. Census regions.

Of the 6,593 participating patients, 4,526 patients (69%) completed all three data collection interviews: intake, discharge, and a 1-year follow-up. The intake questionnaire collected general demographic information and data on treatment history and behavior in the 12 months prior to the current treatment episode. The discharge questionnaire collected data on the patient's treatment experience, including information on specific services received, drug and alcohol use during treatment, and other behaviors and living conditions during treatment. The follow-up questionnaire collected similar information as the intake questionnaire but for the posttreatment period. Each of the questionnaires was developed by a team of experts with backgrounds in substance user treatment and mental health as well as individuals with expertise in the cognitive processes of interviewing. In addition, cognitive interviews, focus groups, and pretests were done to assess the respondent's burden and to ensure that questions were comprehensible to the targeted population (USDHHS, 1997). NTIES researchers also conducted record extractions from patients' clinical records to collect information on treatment-related items such as primary diagnoses, services received, reasons for discharge, and lengths of stay.

For this study, we included only patients from the largest treatment modality, outpatient drug-free, who completed all three interviews—intake, discharge, and follow-up (N = 1,333; approximately 60% of the original outpatient drug-free intake sample).¹ We excluded patients who were less than 18 years of age because adolescent substance users may have different treatment objectives, experiences, and outcomes than adult substance users (Mark et al., 2006). Such differences may include different lengths, intensity, and topographies of drug use (Mark et al., 2006; Stewart and Brown, 1995) as well as age-related differences that may lead to less time to encounter major problems related to alcohol and drug use among adolescents (Filstead et al., 1989), differences in treatment received (e.g., more family involvement with adolescent treatment), or treatment objectives (e.g., focus on completing education rather than obtaining employment). We excluded patients who left treatment within 1 week of admission to eliminate individuals with little or no treatment exposure. The NTIES data did not collect detailed information on patient engagement or rapport with counselors, and therefore we were not able to control for level of patient involvement in the treatment process. We also excluded patients who were in school or training at the time of follow-up, and we limited our sample to patients who were not in treatment at the time of the follow-up interview. We felt that both groups of individuals may be less likely to seek employment. After applying our exclusion criteria, our sample included 1,136 patients. Finally, 168 patients were excluded from the employment analysis and 191 patients were excluded from the crime analysis due to missing data on analysis variables. Therefore, our final samples include 960 outpatient drug-free patients in 26 programs for the employment analysis and 945 outpatient drug-free patients in 23 programs for the crime analysis.

Variables

Our outcome variables are post-treatment employment status and criminal activity. We examine employment because, as noted by Magura et al. (2004), treatment professionals often view employment as a facilitator to recovery, a deterrent against relapse, and as a good indicator of a patient's separation from his previous drug-using lifestyle. We examine crime because it has been shown to be associated with the largest social costs of drug use (Hubbard et al., 1989; Institute of Medicine [IOM], 1990). We first considered dichotomous measures of current

employment status and whether a crime was committed over the follow-up period. The employment status variable was equal to one if the patient was currently employed either part-time or full-time at the time of the follow-up interview (approximately 12 months after leaving treatment) and zero otherwise. Crimes included selling drugs, trading sex for money, robbery, shoplifting, burglary, and assaults. We excluded more violent crimes, such as rape and murder, because these crimes are very different from the other crimes and the incidence of these crimes is too low for separate analyses. The crime status variable was equal to one if the patient committed at least one of the included crimes in the follow-up period and zero otherwise. For those who worked or committed a crime, we analyzed continuous measures of hours worked per week at the time of the follow-up interview and the number of crimes committed over the follow-up period.

The main explanatory variables of interest were treatment services. Information on receipt of treatment services was reported in two ways: self-reports by the patients at the time of discharge and program reports through record extraction. Data on self-reported services received were collected from the patients during the discharge survey. Data from patients' clinical records were extracted by the NTIES research staff around the time of the patient's discharge. A list of services that appeared in the patient discharge survey and that were extracted from patient records appears in Appendix A.

As part of the original NTIES study, assessments were done to determine the agreement between self-reported service receipt and service receipt recorded in clinical records. The agreement across services between these two data collection methods varied. Agreement between the self-reports and record extracts was greatest for group and individual counseling with over 90% of those patients self-reporting receipt also having records that indicated receipt of these services (USDHHS, 1997). Agreement was much lower for other services such as job training, academic training, and legal assistance. Only 20% of the patients whose records indicated receipt of job training had self-reported receiving job training. Similarly only 6% of the patients whose records indicated receipt of legal assistance self-reported receiving legal services (USDHHS, 1997). Given that services outside of the core individual and group counseling are provided less frequently and to fewer individuals, it may not be surprising that such discordance exists between the record extracts and self-reports. Survey research conducted by Higgins, McLean, and Conrath (1985) suggests that events that are less frequent may be less likely to be recalled by the patient or recorded by a staff person in a clinical record. Similarly, events that are external to the program (e.g., services provided through referrals to other facilities) may also be less likely to be patient-reported or staff-recorded than services provided directly at the clinic. Discordance between self-reports and record extracts supports our hypothesis that measures of service receipt may suffer from measurement error.

For each self-reported service, patients were asked if the service was received at the program, through a referral by the program, or elsewhere. Because patients were not asked to report the quantity of each service received, we created a dichotomous variable for each service equal to one if that service was received by the patient at the program or through a program referral and zero if the service was not received or was received elsewhere. We did not include information on the quality of services received because this information was not collected from patients or provided in the record extracts.

For each record-extracted service, research staff recorded whether the patient had received the service 1 time, 2 to 3 times, 4 to 10 times, 11 or more times, or an unknown number of times. For all services except individual and group counseling, we created a dichotomous measure equal to one if the patient received the service at least one time. This was done so that we could match record-extracted services with self-reported services.

In addition to these discrete service variables, we also created separate measures for individual and group counseling that were derived from the record extraction data. Data on individual and group counseling were not collected in the patient self-reports. We used four dichotomous variables for each of the individual and group counseling variables that measured the amount of counseling received: received 1 to 10 sessions; received 11 or more sessions; received an unknown number of sessions; and received no sessions, which was the reference category. The categories used in our analysis matched those used in the data collection surveys. We created a measure for unknown sessions because some patients' records indicated they had received counseling but did not indicate the amount received. Thirteen percent of our sample fell into this category for individual counseling and 12% for group counseling, and these patients were spread across all of our programs. No significant differences were found in the dependent and independent variables between those who received known and unknown amounts of counseling.

Because we wanted to separate the effect of treatment services from other treatment process variables, we included measures for length of stay and treatment completion (see Zarkin, Dunlap, Bray, and Wechsberg, 2002, for analyses with these two variables using a similar data set). Length of stay was defined as weeks in treatment and was determined from information provided on administrative discharge forms from the program and verified by the patient in the treatment discharge and follow-up interviews. We defined treatment completion as a dichotomous variable equal to one if the patient completed treatment as recognized by the program at the time of the discharge interview and zero otherwise. Completion status was identified from information collected on patients' record extraction forms. If a program reported that the reason for a patient's discharge was "completed planned treatment," then the patient was classified as a treatment completer. If a program reported that the patient was discharged for any other reason or that the patient had not completed treatment by the time the record extraction form was completed, then the patient was classified as a noncompleter. Reasons for leaving treatment other than completion included referral to another program; discharge prior to completion by program administration; and discharge prior to completion by patient choice, death, or incarceration.

To control for patients' drug-use severity at intake, we used self-reported pretreatment drug use variables. These variables included days of alcohol use in the 30 days prior to treatment for alcohol; days of heavy alcohol use (defined as number of days having gotten drunk in past 30 days); and days of cocaine, heroin, marijuana, and crack use.

To control for patients' prior treatment histories, we included a dichotomous measure of whether patients received mental health inpatient services in the 12 months prior to treatment, a dichotomous measure of whether patients received mental health outpatient services in the 12 months prior to treatment, and a measure of the number of past drug- user treatment episodes (one prior episode, two or more prior episodes, and number of episodes unknown with no prior treatment episodes as the reference category). In addition, we included a variable to indicate if

the patient was required or strongly encouraged to attend treatment by someone in the criminal justice system. Finally, we included several demographic variables in our models—age, gender, race/ethnicity, marital status, number of dependents, homelessness, and years of schooling—to control for differences in patient characteristics that may affect treatment outcomes.

The NTIES design was such that patients had treatment departures at different calendar times and had varying lengths of follow-up periods. Because of business cycle conditions, the date at which a patient leaves a program may affect crime and employment opportunities. Therefore, we created a group of dichotomous variables to control for differences in patients' dates of treatment departure. We created three variables indicating whether the patient received the follow-up questionnaire in the first half of 1994, the second half of 1994, or the first half of 1995. In addition, because patients with longer follow-up periods may have more time in which to be employed or commit a crime, we created a continuous variable for the number of days from administration of the treatment discharge questionnaire to administration of the follow-up questionnaire.

Methods

Data on services received were captured using two methods: patients' self-reports and record extractions. Each of these methods did not necessarily capture data on the same services, and for those services for which they did we found discrepancies between the two measures. One possible reason for these discrepancies may be reporting errors. Patients are subject to memory limitations, definitional problems, and other barriers that may limit their ability to accurately report receipt of services. On the other hand, program personnel may not record all services received in patient records.

The most straightforward method of studying the relationship between services and outcomes is to regress outcomes on the services variables, controlling for severity and the other covariates discussed above. We attempted this approach and the self-reported and record-extracted services were insignificant. One possible cause of this result is multicollinearity created by including many similar services in the equation. Another possible cause is measurement error, which tends to bias the estimated coefficients toward zero. The solution to both of these problems is to reduce the dimensionality of the services variables by combining them.

To combine the dichotomous treatment service indicators, we conducted a principal components analysis of the services correlation matrix whose columns are the 24 self-reported services and the 22 record-abstracted services. One advantage of this approach is that it places no structure on the services data a priori. By examining the eigenvalues in the matrix, we determined that five factors best explained the variance reflected in the services correlation matrix. Factor selection and identification was based on the empirical results of the factor loadings. For each individual i , we derived a factor-based service score for factor K , N_{ik} , such that

$$N_{ik} = \sum_j w_{jk} S_{ij} \quad K = 1, \dots, 5 \quad (1)$$

where w_{jk} is the value of the factor-loading for a specific service j in factor K , and S_{ij} is a dichotomous service measure equal to one if individual i received service j . This

factor-based method is a common procedure to reduce the dimensionality of the services matrix while still capturing the mutual correlation of the service variables (Kim and Mueller, 1978). Recognizing that our factor solutions may be subject to sampling errors, in deriving our factor-based scores we focused on variables with substantial loadings and ignored the remaining variables with minor loadings. Therefore, for each factor K , we only included those services in Eq. (1) for which the absolute value of the factor loading was greater than or equal to 0.40 (Kim and Mueller, 1978).²

Multivariate Models

The objective of our analysis was to determine the effect of treatment services, as measured by the factor-based scores, on posttreatment employment and criminal behavior, holding constant length of stay, treatment completion status, pretreatment employment and criminal behavior, and other pretreatment patient variables. We included pretreatment measures of outcomes because behavior tends to be correlated over time, and they serve as additional measures for patients' severity upon treatment entry.

In our sample, we had a large number of zeros for the outcome variables, with 51% of patients not employed at follow-up and 82% of patients not committing any crimes in the posttreatment period. To model this pattern of zeros, we used a two-part model (Jones, 2000) that divided the analysis into two steps: whether employed (commit a crime) and then, conditional on being employed (committing a crime), how many hours of work (how many crimes to commit).

First, using the full sample, we examined separately how treatment services affected the propensity to be employed at the time of the follow-up survey or to commit a crime during the follow-up period. Because our dependent variables for these models are dichotomous, standard ordinary least squares regression was inappropriate. Thus, we used logistic regression of the following form:

$$\Pr(Y_{\text{post}} = 1) = f(\beta_0 + \beta_1 * Y_{\text{pre}} + \beta_2 * \text{SERVICES} + \beta_3 * \text{LOS} + \beta_4 * \text{TXCOMP} + \beta_5 * X) \quad (2)$$

where Y is the discrete employment or crime outcome at the individual patient level, equal to one if the individual is employed or committed a crime as appropriate; Y_{pre} is a discrete pretreatment measure of our outcome variable; SERVICES is a vector containing the five factor-based scores for treatment services, the four dichotomous measures of the amount of individual counseling received, and the four dichotomous measures of the amount of group counseling received; LOS (length of stay) is a continuous variable equal to the number of weeks in treatment; TXCOMP (treatment completion) is a dichotomous variable equal to one if the individual completed treatment; and X is a vector that represents various pretreatment individual characteristics, the patient's treatment departure date, and length of the follow-up period.

Equation (2) allows us to examine the effect of treatment services in explaining post-period employment or criminal activity, controlling for both length of stay and treatment completion. It is an extension of the model presented in Zarkin et al. (2002) that focused on treatment completion while controlling for length of stay. Zarkin and colleagues found that both length of stay and treatment completion were significantly related to posttreatment employment, but only

length of stay was significantly related to posttreatment crime. In this study, we hypothesize that, controlling for length of stay and treatment completion, patients who receive more treatment services (SERVICES) are more likely to be employed and less likely to commit a crime after treatment than patients who do not.

In the second part of our analysis, we examined the effect of treatment services on hours worked per week (given that the individual worked) and the number of crimes committed during the follow-up period (given that the individual committed a crime). Our empirical specification is identical to the logistic models except that the dependent variables are now continuous, allowing us to use ordinary least squares regression. The regression equation is of the following form:

$$Y_{\text{post}} = \beta_0 + \beta_1 * Y_{\text{pre}} + \beta_2 * \text{SERVICES} + \beta_3 * \text{LOS} + \beta_4 * \text{TXCOMP} + \beta_5 * X \quad (3)$$

where Y is hours worked per week or the number of crimes committed.³ The explanatory variables are the same as those defined for Eq. (2). Equation (3) is estimated only for those individuals who were employed or committed a crime during the posttreatment period.

A potential problem for Eqs. (2) and (3) is selection bias. The potential for selection bias arises because individuals were not randomly assigned to varying treatment services, to varying lengths of stay, or to treatment completion/noncompletion. The observed treatment variables may be based in part on program-level variables unobserved by the researcher that may also be correlated with employment or criminal behavior. We are unable to control specifically for these unobservables so they are included in the error term. Thus, the error term may be correlated with treatment service variables, length of stay, and treatment completion, resulting in biased coefficients.

To address this selection bias, we used regression models (Heckman and Hotz, 1989; Heckman and Robb, 1985) in which we included program-level indicator variables to control for unobserved differences across programs that are fixed within treatment programs and do not vary over time. These differences may include differences in programmatic focus, program intensity, treatment philosophy, demographic composition of the patients and staff, and staff training. Almost certainly, these differences exist across programs. By including program-level dichotomous indicators, the estimation methodology uses variation in the treatment variables across patients within each treatment program to identify the effects of treatment services, length of stay, and treatment completion. By using within-program variation in these variables, we control for potential bias caused by differences in unobservables across programs. For the continuous outcomes in Eq. (2), we used an ordinary least squares model with the program-level indicators included. Because the logit is a nonlinear model (Eq. (1)), we used a conditional logit model (StataCorp, 1999).

Results

Table 1 presents the factors and factor loadings for the ancillary treatment services. Our principal components analysis of the service data produced five service variable factors. We interpreted the meaning of each factor by examining the loadings of each observed service variable. The largest loadings on Factor 1 are daycare (1.00) and assistance getting benefits (0.98), followed

closely by housing assistance (0.87), transportation services (0.83), legal services (0.72), and pregnancy-related services (0.72). We interpreted this factor as being logistical services.

The largest loadings on Factor 2 are related to medical services. Being tested for TB (0.76), being tested for AIDS (0.73), and receiving any type of medical service (0.72) are medical services, as are receiving counseling about AIDS (0.61) and receiving an intake exam from a nurse or physician (0.48).

The largest loadings on Factor 3 are parenting training and education-oriented parenting counseling activities (referred to as parenting training). The receipt of classes on planning or preventing pregnancy has the largest loading (0.68) followed closely by receiving classes on being a better parent (0.66) and receiving pregnancy services (0.65). Most of the remaining loadings are either directly or indirectly related to parenting services.

The largest loadings on Factor 4 are skills training activities. The largest loading is for classes in how to do things for oneself such as buying groceries and cleaning (0.85), followed by receiving counseling about getting along with others (0.79) and learning English (0.76). The remaining items are also life skills training activities and are not directly related to drug-user treatment itself. Unlike the first three factors in which both self-reported and recorded-extracted variables are represented, all of the variables in Factor 4 are self-reported.

Factor 5 is defined by the same types of skills training variables as Factor 4, but these measures are record-extracted variables. The largest loadings are for receiving interpersonal skills training (0.98) and practical skills (0.97).

Table 2 shows the mean values of our analysis variables for the $N = 960$ sample. Approximately 32% of the sample was employed prior to treatment compared with 49% employed at the time of the follow-up interview. Those employed at follow-up ($N = 468$) worked an average of 19 hours per week prior to treatment compared with 41 hours at the time of the follow-up interview. Approximately 89% of the sample reported committing at least one crime in the 12 months prior to treatment compared with only 18% who reported committing a crime during the follow-up period. Of those committing a crime in the follow-up period ($N = 149$), the average frequency of crimes in the 12 months prior to treatment was about 192 occurrences compared with 35 occurrences during the follow-up period.

Patients with higher factor scores received more types of services. Most patients received very few types of these services as indicated by the low mean values for each of these factor-based scores. In addition to ancillary services, patients received individual and/or group counseling. Most patients received either 1 to 10 individual counseling sessions (46%) or 11 or more individual counseling sessions (35%). Six percent of the sample did not receive any individual counseling, and 13% received an unknown number of individual counseling sessions. Most patients were also likely to receive some group counseling, with 31% of patients receiving 1 to 10 sessions, 33% receiving 11 or more sessions, and 12% receiving an unknown number of sessions. Twenty-four percent of patients did not receive group counseling sessions.

Table 1
Treatment service factor analysis ($N = 960$)

Services	Factor loadings (absolute values)
Factor 1: Logistical services	
Daycare for children	1.00
Assistance getting benefits	0.98
Housing assistance	0.87
Transportation services	0.83
Legal services	0.72
Pregnancy-related services	0.72
Job training	0.63
Room and board	0.61
Help with transportation (self-reported)	0.53
Parenting skills training	0.47
Employment counseling	0.46
Other physician services	0.43
Medical services for pregnancy (self-reported)	-0.43
Factor 2: Medical services	
Tested for TB (self-reported)	0.76
Tested for AIDS (self-reported)	0.73
Any kind of medical services excluding hospital stays (self-reported)	0.72
Counseling or classes to learn about AIDS (self-reported)	0.61
Room and board	0.57
Attend self-help groups (self-reported)	0.56
Help with getting money or payments from government (self-reported)	0.49
Intake exam by physician or nurse practitioner	0.48
Factor 3: Parenting training and education-oriented parenting counseling	
Classes, counseling, or medical care about planning/preventing pregnancy (self-reported)	0.68
Child care or money to get child care or help getting child care (self-reported)	0.66
Counseling or classes for problems raising children or to become better parent (self-reported)	0.66
Medical services for pregnancy (self-reported)	0.65
Parenting skills training	0.55
AIDS education	-0.44
Factor 4: Life skills training (self-reported)	
Classes on how to do things for self such as buying groceries, cooking, cleaning house, etc. (self-reported)	0.85
Counseling or classes to help get along better with others (self-reported)	0.79
Services to learn English (self-reported)	0.76
Spanish or other non-English services	0.67

(Continued on next page)

Table 1
Treatment service factor analysis ($N = 960$) (*Continued*)

Services	Factor loadings (absolute values)
Any individual, group, or family counseling for family problems (self-reported)	0.67
Counseling or classes for people who have been abused (self-reported)	0.59
Classes to help get a job, hold a job, or improve job skills (self-reported)	0.56
Counseling or classes to help plan future (self-reported)	0.50
Counseling or treatment for problems with emotions, nerves, or mental health (self-reported)	0.46
Factor 5: Life skills training (record-extracted)	
Interpersonal skills training	0.98
Practical skills training	0.97
Nurse/nurse practitioner services	0.66
Self-help groups	0.48
Parenting skills training	0.40

Only 21% of the sample completed their planned treatment protocol as reported by the program. The average length of stay for patients in the sample was approximately 18 weeks. Approximately 40% of the sample was required or encouraged to attend treatment by the criminal justice system. This percentage is comparable to both the Treatment Outcomes Prospective Study (TOPS) and the Drug Abuse Treatment Outcome Study (DATOS) that found that 31 and 42% of their outpatient drug-free patients had received a criminal justice referral, respectively (Craddock, Rounds-Bryant, Flynn, and Hubbard, 1997; Hubbard et al., 1989).

The average number of days of alcohol use in the past 30 days prior to treatment was about 5 days. The average number of days of use for each of the other drugs was less than 2 days in the past 30 days prior to treatment. Thus, the primary drug of use immediately prior to outpatient treatment was alcohol.‡

Only 7% of the sample received inpatient mental health services during the 12 months prior to treatment, and 12% received outpatient mental health services. These two categories were not mutually exclusive, with 4% of the sample receiving both inpatient and outpatient mental health services in the 12 months prior to treatment. The average age of patients in the sample was approximately 34 years. Seventy-seven percent of the sample was non-White and 72% was male.

Multivariate Results

The first column of Table 3 presents the parameter estimates for the analysis of posttreatment employment probability. In general, we did not find a strong association between treatment services (the five treatment service factors, individual counseling, or group counseling) and

‡ The reader is reminded that programs treating the broad range of heterogeneous substance users consistently do not assess the patient's use of tobacco products, their patterns of eating (for eating disorders), and rarely if ever, include these behaviors in treatment planning, implementation, and assessment (process and outcomes). Editor's note.

Table 2
Variable means

Variables (<i>N</i> = 960)	Means (standard deviations)
Employment status	
Employed in pretreatment period	0.32 (0.47)
Employed in posttreatment period ^a	0.49 (0.50)
Hours worked per week in pretreatment period (conditional on being employed in the follow-up period)	19.21 (22.24)
Hours worked per week in posttreatment period (conditional on being employed in the follow-up period)	41.03 (13.06)
Crime	
Committed a crime in pretreatment period	0.89 (0.32)
Committed a crime in posttreatment period	0.18 (0.39)
Number of crimes committed in pretreatment period (conditional on committing a money crime in the follow-up period)	191.98 (176.80)
Number of money crimes committed in posttreatment period (conditional on committing a money crime in the follow-up period)	34.89 (46.83)
Treatment variables	
Logistical services (factor-based score)	0.73 (1.04)
Medical services (factor-based score)	1.20 (1.26)
Parenting training and education-oriented parenting counseling services ("parenting training" factor-based score)	0.03 (0.50)
Life skills training (self-reported) (factor-based score)	0.78 (1.00)
Life skills training (record-abstracted) (factor-based score)	0.26 (0.55)
Received no individual counseling sessions	0.06 (0.24)
Received 1 to 10 individual counseling sessions	0.46 (0.50)
Received 11 or more individual counseling sessions	0.35 (0.48)
Received unknown number of individual counseling sessions	0.13 (0.33)
Received no group counseling sessions	0.24 (0.43)
Received 1 to 10 group counseling sessions	0.31 (0.46)
Received 11 or more group counseling sessions	0.33 (0.47)
Received unknown number of group counseling sessions	0.12 (0.33)
Completed treatment	0.21 (0.41)
Length of stay (weeks)	18.29 (14.65)
Time out of treatment (weeks) at time of follow-up interview	41.96 (12.61)
Treatment was required by criminal justice system	0.41 (0.49)
No prior drug-user treatment episodes	0.38 (0.49)
One prior drug-user treatment episode	0.24 (0.43)
Two or more prior drug-user treatment episodes	0.34 (0.47)
Number of prior drug-user treatment episodes unknown	0.04 (0.20)

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Table 2
Variable means (*Continued*)

Variables (<i>N</i> = 960)	Means (standard deviations)
Drug use prior to treatment	
Days of alcohol use in past 30 days prior to treatment ^b	5.13 (8.53)
Days of heavy alcohol use in past 30 days prior to treatment	1.86 (4.83)
Days of cocaine use in past 30 days prior to treatment	0.55 (2.96)
Days of heroin use in past 30 days prior to treatment ^b	0.44 (2.92)
Days of marijuana use in past 30 days prior to treatment	1.19 (4.48)
Days of crack use in past 30 days prior to treatment	1.52 (4.70)
Mental health treatment history prior to treatment	
Received inpatient mental health treatment in past 12 months	0.07 (0.26)
Received outpatient mental health treatment in past 12 months	0.12 (0.32)
Demographics	
Age	33.82 (8.71)
Male	0.72 (0.45)
Non-White	0.77 (0.42)
Married	0.23 (0.42)
Number of dependents	1.11 (1.62)
Homeless in past 30 days prior to treatment	0.07 (0.26)
Years of schooling	11.47 (2.04)

employment. We did find a negative relationship between the parenting training factor and posttreatment employment that was statistically significant ($p < 0.01$). Surprisingly, the odds of posttreatment employment among individuals who received parenting training services was almost half (odds ratio = 0.56; $e^{-0.584}$) that of individuals who did not receive these services. The strongest predictor of posttreatment employment was pretreatment employment. The odds ratio of 3.58 ($e^{1.275}$) suggests that the odds of posttreatment employment among individuals employed prior to treatment entry is 3.58 times that of individuals not employed prior to treatment entry.

Although the treatment service factors individually did not have a strong association with post-treatment employment, results from an F-test of joint significance found that the five treatment service factors were jointly significant ($p < 0.05$). In contrast, the individual and group counseling variables were not jointly significant. Furthermore, unlike previous studies (e.g., Zarkin et al., 2002), we did not find significant effects for length of stay or treatment completion. This suggests that when treatment services are not included in the model, length of stay and treatment completion may be capturing the effects of these treatment services. By including services in the model, the separate effects of length of stay and treatment completion are diminished.⁴

Column 2 of Table 3 shows the results of regressing hours worked per week on the treatment service variables, length of stay, and treatment completion. This regression is limited to individuals who reported being employed at the follow-up interview. We did not find a significant relationship between any treatment services and hours worked per week

Table 3
Multivariate analyses of employment and crime in follow-up period

Variables	Probability of employment at follow-up		Linear models for hours worked per week		Probability of crime during follow-up period		Linear models for number of crimes during follow-up period	
	Conditional logit (standard error)	960	Site-level fixed effects (standard error)	468 ^a	Conditional logit (standard error)	945	Site-level fixed effects (standard error)	149 ^b
N		960		468 ^a		945		149 ^b
Logistical services (factor-based score)		0.173* (0.105)		-0.697 (0.782)		-0.145 (0.128)		-9.748 (6.035)
Medical services (factor-based score)		-0.177* (0.097)		0.272 (0.832)		0.273** (0.122)		9.688 (5.879)
Parenting training (factor-based score)		-0.584*** (0.208)		2.878 (1.811)		0.407* (0.246)		3.520 (11.222)
Life skills training (self-reported, factor-based score)		-0.084 (0.099)		0.526 (0.823)		0.064 (0.121)		4.671 (5.054)
Life skills training (records-extracted, factor-based score)		0.022 (0.188)		2.204 (1.583)		0.213 (0.242)		-10.330 (10.091)
Received 1 to 10 individual counseling sessions		0.163 (0.344)		-3.896 (2.646)		-0.468 (0.393)		-21.933 (14.775)
Received 11 or more individual counseling sessions		0.348 (0.403)		-3.043 (2.970)		-0.523 (0.463)		-41.441** (18.581)
Received unknown number of individual counseling sessions		1.489** (0.716)		-8.546 (6.029)		-0.724 (0.781)		-10.599 (32.931)
Received 1 to 10 group counseling sessions		0.170 (0.234)		-1.216 (1.964)		0.453 (0.289)		7.258 (12.342)
Received 11 or more group counseling sessions		0.181 (0.281)		-0.939 (2.230)		0.116 (0.375)		24.405 (17.233)
Received unknown number of group counseling sessions		-0.616 (0.521)		0.424 (4.918)		-0.056 (0.575)		2.916 (19.657)
Length of stay (weeks)		0.015* (0.008)		0.084 (0.065)		-0.014 (0.011)		-0.465 (0.452)
Completed treatment		0.429* (0.218)		0.608 (1.640)		-0.351 (0.284)		-10.674 (12.133)
Employed at treatment entry		1.275*** (0.190)		-		-0.675*** (0.247)		-2.249 (10.578)
Hours worked per week in past 12 months prior to treatment		-		0.056* (0.033)		-		-
Committed a money crime in past 12 months prior to treatment		-		-		1.367*** (0.458)		-
Number of crimes committed in 12 months prior to treatment		-		-		-		0.026 (0.024)
Days of alcohol use in past 30 days prior to treatment		-0.017 (0.012)		-0.016 (0.107)		0.013 (0.014)		0.025 (0.517)
Days of "heavy alcohol use" in past 30 days prior to treatment		0.004 (0.021)		-0.420** (0.174)		-0.025 (0.025)		-1.370 (1.003)
Days of cocaine use in past 30 days prior to treatment		-0.010 (0.036)		0.244 (0.394)		0.007 (0.031)		-1.589 (1.099)
Days of heroin use in past 30 days prior to treatment		-0.070* (0.042)		-0.313 (0.554)		0.003 (0.029)		5.910*** (1.521)

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Table 3
Multivariate analyses of employment and crime in follow-up period (*Continued*)

Variables	Probability of	Linear models	Probability of	Linear models for
	employment at follow-up Conditional logit (standard error)	worked per week for hours Site-level fixed effects (standard error)	crime during follow-up period Conditional logit (standard error)	number of crimes during follow-up period Site-level fixed effects (standard error)
Days of marijuana use in past 30 days prior to treatment	-0.015 (0.018)	0.010 (0.154)	0.034* (0.019)	0.426 (0.870)
Days of crack use in past 30 days prior to treatment	-0.032 (0.020)	0.075 (0.204)	0.052** (0.020)	1.872** (0.891)
Received inpatient treatment for mental illness in past 12 months	-0.260 (0.312)	0.486 (2.563)	0.455 (0.344)	12.775 (14.217)
prior to treatment				
Received outpatient treatment for mental illness in past 12 months	-0.082 (0.260)	-4.677** (2.023)	0.288 (0.309)	-16.667 (14.094)
prior to treatment				
Male	0.442** (0.207)	7.553*** (1.794)	0.191 (0.264)	-1.840 (11.603)
Age	0.133** (0.063)	0.554 (0.594)	-0.112 (0.073)	2.341 (3.978)
Age squared	-0.002** (0.001)	-0.007 (0.008)	0.001 (0.001)	-0.027 (0.056)
Married	0.108 (0.192)	-0.704 (1.558)	0.406* (0.239)	-14.366 (11.183)
Number of dependents	-0.039 (0.052)	-0.534 (0.438)	-0.040 (0.063)	-2.517 (2.638)
Nonwhite	-0.472** (0.218)	-0.617 (1.637)	0.248 (0.280)	-0.198 (12.090)
Highest grade completed	0.117*** (0.041)	0.086 (0.325)	-0.064 (0.053)	3.458 (2.378)
Treatment was required/encouraged by criminal justice system	0.125 (0.191)	-0.953 (1.528)	0.442* (0.230)	-6.580 (10.819)
Homeless during past 30 days prior to treatment	0.262 (0.299)	7.840*** (2.559)	-0.004 (0.362)	-5.624 (14.152)
Unknown number of prior treatment episodes	-1.034** (0.452)	-0.210 (4.108)	-0.514 (0.696)	22.529 (36.701)
One prior treatment episode	0.138 (0.201)	-3.673** (1.618)	0.156 (0.258)	-2.769 (11.694)
Two or more prior treatment episodes	0.018 (0.187)	-4.047** (1.554)	0.502** (0.236)	-0.450 (10.030)
Administered NPAQ 2nd half 1994	-0.382 (0.291)	2.540 (2.514)	0.332 (0.371)	-8.528 (16.980)
Administered NPAQ 1st half 1995	-0.145 (0.184)	-1.014 (1.451)	0.212 (0.230)	1.891 (10.102)
Time out of treatment at follow-up	0.019** (0.008)	0.142** (0.065)	0.011 (0.010)	0.010 (0.505)
Constant	— (—)	22.956* (11.948)	— (—)	-28.787 (80.674)

^a Analysis included only those individuals who were employed in the follow-up period.

^b Analysis included only those individuals who committed at least one crime in the follow-up period.

*Statistically significant at $p < 0.10$.

**Statistically significant at $p < 0.05$.

***Statistically significant at $p < 0.01$.

at follow-up. We also did not find a significant effect of length of stay and treatment completion. Having one or more prior treatment episodes decreased the hours worked per week at follow-up compared to individuals with no prior treatment. Surprisingly, being homeless in the 30 days prior to entering treatment was associated with substantially larger posttreatment hours of work ($p < 0.01$).

Column 3 of Table 3 presents results from the analysis of treatment services and the probability of committing a crime. For most of the treatment service factors, we did not find any significant effect on posttreatment crime. An exception was the medical services factor in which we found that the odds of committing a crime among individuals receiving medical services was about 1.3 times ($e^{0.273}$) that of individuals not receiving medical services ($p < 0.05$). Although the treatment service factors individually did not have a strong association with posttreatment employment, results from an F-test of joint significance found that the five treatment service factors were marginally significant ($p < 0.10$). In contrast, the individual and group counseling variables were not jointly significant. Length of stay and treatment completion did not have a significant effect on the odds of committing a crime during the follow-up period.

The two strongest predictors of post-treatment crime were whether the individual had committed a crime in the past 12 months prior to treatment and whether the individual was employed at treatment entry. The odds of committing a posttreatment crime among individuals committing a crime prior to treatment were almost four times ($e^{1.367}$) that of individuals who did not commit a crime prior to treatment. The odds of a posttreatment crime among individuals employed at treatment entry was half (odds ratio = 0.51; $e^{-0.675}$) that of individuals not employed at treatment entry.

The last column of Table 3 presents the regression results for the number of crimes committed by individuals who reported committing a crime in the follow-up period. We found that individual counseling had a significant effect on the number of crimes committed, with individuals receiving 11 or more counseling sessions committing about 40 fewer crimes in the post-treatment period than individuals receiving no individual counseling sessions ($p < 0.05$). The other treatment service variables were not significant. Although both length of stay and treatment completion had a negative effect on the number of crimes, their effects were not statistically significant.

Discussion

Establishing the value of drug-user treatment depends in part on being able to show that treatment leads to improved outcomes. Although no true consensus exists on what constitutes successful drug-user treatment, it is usually accepted that decreased drug use, decreased criminal involvement, and improvement in employment are positive outcomes. Although length of stay in treatment has often been found to be a significant predictor of positive treatment outcomes (Condelli and Hubbard, 1994; De Leon, 1985; Etheridge et al., 1999; Hubbard et al., 1997; Hubbard et al., 1989; Simpson et al., 1999; Simpson and Sells, 1982), little is known about the specifics of the treatment process and why those patients who remain in treatment longer fare better. To understand this process, researchers have examined the effect of specific treatment services on treatment outcomes. Surprisingly, the findings from these studies have not shown an

unambiguous benefit of treatment services.

We hypothesized that a potential difficulty in making the link between services and outcomes arose from measurement error. We reasoned that retrospective self-reports were likely affected by a variety of cognitive-based errors that may have attenuated the relationship between services and outcomes. Retrospective record extractions were also subject to some fallibility, but these errors were not likely to be the same as those in self-reported data. Assuming that the measurement errors for the two data sources were uncorrelated, service measures that include a combination of the two data sources might best capture the treatment experience and increase our ability to account for the variance in employment and crime outcomes.

This article directly incorporates the potential for errors in the measurement of treatment services. We combined two sources of treatment service measures—self-reports and record extractions—and created treatment service factors that have less noise than either source of treatment service measures alone. Although models that included the treatment service factors indicated some significant predictors of crime and employment, the overall effects were generally insignificant. A couple of the significant results were in the unexpected direction. We did find some joint significance among the five service factors for both employment and crime outcomes. An interesting finding was that, unlike previous studies, we did not find significant effects for length of stay or treatment completion. This suggests that when treatment services are not included in the model, length of stay and treatment completion may be capturing the effects of these treatment services.

It is possible that the *receipt* of treatment services is not as important as the *quality* of treatment services or the rapport that is built between the counselor and patient (e.g., Joe, Simpson, Dansereau, and Rowan-Szal, 2001; Magura, 2000). Simply identifying whether a patient received a given service may be a limited indicator of effective treatment because it may ignore other more salient features, such as patient/treatment staff rapport, whether treatment services were appropriately matched to patients' needs, and the quality of treatment services. Furthermore, there may be unobserved characteristics for which we cannot control in the model that may be moderators for the treatment service effect. For example, it may be possible that patients' motivation or level of engagement with the treatment process may affect the association between treatment services and outcomes, with more motivated patients having more significant association between services and outcomes.

Finally, although we developed a method that seeks to minimize measurement error in treatment service variables, the treatment services data we used may be too error-ridden to enable the finding of a significant treatment services effect. Possibly, the method used to collect treatment services data in NTIES (and other similar large studies) needs to be refined to collect treatment services more accurately. Based on our results, we encourage researchers to review their treatment services instrumentation to ensure that the questions on treatment services received capture the desired information.

Study's Limitations

Our study has some limitations that should be noted. First, the results presented here reflect the

experience of CSAT-supported outpatient drug-free treatment programs, so they are not generalizable to the universe of all outpatient drug-free treatment programs. NTIES results are most reflective of public sector programs that serve lower income individuals. Another limitation is that NTIES used a purposive sampling strategy and is not a true random sample of drug user treatment programs. However, it is important to note that most large-scale drug-user treatment surveys (e.g., DATOS, TOPS) use purposive samples and are not nationally representative of all treatment programs. Furthermore, we do not include variables on the programs' policies and staff characteristics that may affect treatment outcomes. Our use of a program-level fixed effects model helps to control for program differences that may affect outcomes, but it does not allow us to examine specific policies or staff traits within our models.

Another limitation is the censored nature of our service variables. Ideally, we would like to include quantity of services received in our models and be able to address the selection criteria used by programs in determining which patients received services as well as the content of services received; however, the nature of the data collection used in NTIES and our use of both self-reports and record extractions made it impossible to quantify the number of ancillary services received. Instead, we relied on dichotomous measures of ancillary services received, which were then transformed into our factor-based scores. Therefore, for ancillary services, we were only able to examine whether receipt of particular services mattered but not whether patients receiving more of a given service had better outcomes. For individual and group counseling sessions, we were able to include quantity received, but these variables are somewhat limited because they were collected categorically and censored at 11 or more sessions. Our results showed that individual and group counseling were not significant in any of our regressions. Furthermore, NTIES did not collect data on treatment service matching or content of services provided at programs.

Finally, we do not have data on specific external factors that may affect patient outcomes such as the economic environment that the individual faces once they leave treatment and the community support systems available to them. Although we attempted to control for variations in economic environment that patients may face due to different times of discharge by including in our model variables representing time of year of treatment release and time out of treatment, we were not able to examine specific external factors. However, our inability to include such variables is only an issue if these factors are correlated with the treatment service variables. We do not have any reason to expect such correlation to exist.

Conclusions

Despite these limitations, our results are noteworthy for several reasons. First, we focus on outpatient drug-free treatment, which is the largest drug-user treatment modality but is relatively understudied. Our results thus provide insights into an important treatment modality. Second, our analysis is performed on 960 adults in 28 treatment programs across the United States. Most previous studies of treatment services have only examined a handful of drug-user treatment programs with a small number of patients in a limited geographic region. Finally, we take advantage of the information provided in both self-reports and record extractions by combining these data in an attempt to develop more refined measures of services received that may help predict posttreatment employment and crime behavior. However, even with the use of our more

refined treatment service measures, we generally found no significant effect of treatment services on posttreatment employment and crime.

Perhaps the most useful finding from this study is that it highlights a common trade-off between data breadth and data depth. Researchers and policy-makers often want data elements across a wide range of domains so that they can tackle multiple research questions within a given study. But collecting such data may come at the cost of more in-depth information for a smaller number of domains. The results of this study suggest that researchers and policy makers may want to reconsider this trade-off between data breadth and depth. The data on treatment services that is most often collected in drug-user treatment outcome studies, especially large-scale studies, may not be adequate to examine the effect of treatment services on outcomes. Rather than simple service utilization counts, future studies should consider collecting more in-depth data on patients' treatment services experiences and the program policies and processes that may influence these experiences. These data may include variables that capture the level of treatment engagement, rapport between patient and counselor, content of treatment services, treatment satisfaction, and whether services were appropriately matched to patients. Collecting such data is a necessary step to help researchers and policy makers continue to disentangle treatment's black box.

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Notes

1. Statistical tests of mean differences in baseline variables between those outpatient drug-free patients who completed all three interviews and those who did not complete all interviews revealed no statistical differences on most variables. Those patients who did not complete all interviews were slightly more likely to be male (74% versus 70%) and slightly less likely to have had the intake interview in a prison setting (7% versus 5%) compared to those patients who completed all three interviews.
2. Although we rotated to the simplest structure, we still had multiple loading for several variables. We decided to allow the variables to define more than one factor and accept the naturally occurring intercorrelation among the factors.
3. Due to the skewness of the crime and hours data, we estimated two specifications of each model—with and without logged crime (and hours) variables as dependent variables. The results differed little between the two specifications, and we present the unlogged results for ease of interpretation. Logged results are available from the corresponding author upon request.
4. We also ran an interactive model in which the indicator variable for treatment completion was interacted with the treatment service variables, and these models did not produce any statistically significant findings. This suggests that the effect of services on posttreatment crime and employment are not different between these 2 groups. The output for these analyses is available from the corresponding author upon request.

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Appendix A

Self-Reported Services

1. Any self-help groups (e.g., Cocaine Anonymous, Narcotics Anonymous, Alcoholics Anonymous)
2. Counseling or classes specifically about problems with drugs or alcohol
3. Services to help with criminal charges such as seeing a lawyer
4. Child care or given money for child care
5. Counseling or classes for problems in raising children or help to become better parent
6. Counseling or classes, especially for people who have been abused
7. Individual, group, or family counseling for family problems
8. Counseling or classes to help with getting along better with others, such as teaching ways to help speak up for self or control temper
9. Classes or lessons on how to do things for self like buying groceries, cooking, cleaning house, or handling money
10. School or any classes or lessons to get a GED or a high school, technical school, or college diploma
11. Services such as classes or lessons to help learn English
12. Classes or lessons to help get job, help hold job, or improve job skills
13. Help with a housing problem, such as finding a place to live, getting a better place, or helping hold on to house or apartment
14. Help getting money or payments from government
15. Counseling or treatment for problems with emotions, nerves, or mental health
16. Days received any kind of medical services (excluding hospital stays) such as medical tests, physical exams, getting cast put on, getting an injection
17. Test for TB
18. Test for AIDS
19. Counseling or classes to learn about ways not to get or spread AIDS
20. Classes, counseling, or medical care about preventing or planning pregnancy
21. Classes, counseling, or medical care about planning pregnancy while pregnant
22. Counseling or classes to help plan future including how to handle problems after leaving treatment

Record-Extracted Services

1. Intake exam by physician or nurse practitioner
2. Other physician services
3. Psychiatrist or psychologist visits
4. Nurse/nurse practitioner services
5. Spanish or other non-English services
6. Individual counseling
7. Group counseling
8. Family counseling
9. Employment counseling
10. Job training
11. Academic training
12. Self-help groups (including AA and NA)
13. Practical skills training
14. Interpersonal skills training
15. Parenting skills training
16. AIDS education
17. Assistance getting benefits
18. Legal services
19. Transportation services
20. Pregnancy-related services
21. Day care for children
22. Housing assistance
23. Room and board
24. Post-discharge planning