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Suffolk County Drug Court Evaluation

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Advisory Board
Suffolk County Drug Court
Programs

Delivered to:
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1.0 Introduction and Summary

This report documents an evaluation of four adult treatment drug court programs located in Suffolk County, Massachusetts. The evaluation incorporates both process and impact analyses conducted by Abt Associates Inc. The process evaluation examines program caseflow and participant characteristics to describe program intake, retention, and graduation. Using a comparison group of matched probationers who did not enter drug court, the impact analysis studies the receipt of alcohol and other drug treatment services following entry into drug court; and it studies probation revocations, criminal recidivism (new arrests and new convictions), and incarceration (Houses of Correction or jails) during a follow-up period beginning one year after the probationer entered drug court.

1.1 Process Analysis: A Summary

By policy, the four drug courts admit offenders with serious and chronic addiction histories and substantial criminal records. (See section 3.0.) To demonstrate the application of that policy, we assembled and integrated data from diverse sources, and we used those data to compare the prior histories of drug court participants and other drug-involved probationers. (See section 2.0.) By integrating drug court records with probation assessment records for drug-involved probationers, we established that drug court participants tend to have worse probation risk scores than do other drug-involved probationers (consistent with the eligibility policy of the four drug courts) based on:

- Prior criminal records and correctional histories
- Unstable residences and employment records

By integrating drug court records with state criminal history records, we established that drug court participants tend to have more extensive criminal histories based on:

- Previous arraignments
- Previous convictions

After integrating drug court records with House of Corrections Records, we found that drug court participants were more frequently incarcerated and spent more time incarcerated prior to their participation in drug court than did other drug-involved probationers. By integrating drug court records with substance abuse treatment records, we found that drug court participants had more frequently been in treatment, presumably reflecting more entrenched substance abuse problems, than was true of other drug-involved probationers.

We examined drug court program records for 833 probationers. Records showed the program status for 794 participants. Of the 794, about 13 percent were active in drug court at the time the records were assembled. Of the other 690 participants, about 42 percent were graduates, and the rest had unfavorable outcomes: 11 percent had outstanding warrants and the court committed or otherwise ended the participation of the other 47 percent.

1.2 Impact Analysis: A Summary

Program evaluators often use random assignment to assure that individuals who receive a treatment (here, participation in drug court) are no different from individuals who do not receive the treatment (here, non-drug court probationers) except that the former are “treated” while the latter are not. By establishing

pretreatment equivalence between the treated and untreated groups, random assignment assures that any observed post-treatment difference in outcomes can be attributed to the treatment itself. However, random assignment was impossible for this evaluation, which relied on archival data.

It would have been misleading to compare outcomes for drug court participants and other drug-involved probationers because the process analysis showed that program eligibility requirements caused drug court participants to differ from other drug-involved probationers in ways that are likely to affect outcomes. When faced with this type of problem, evaluators often use statistical procedures to “control” for pretreatment differences. We used a procedure called “matching based on propensity scores.” For every drug court participant who met criteria described later (about 300 drug court participants), we identified up to three probationers who were *observational equivalent*¹ to the drug court participant on characteristics such as criminal record and other factors. We then computed the differences between the outcomes for each of 300 drug court participants and their matched probation counterparts, and we averaged over those 300 differences to estimate an overall average treatment effect. Thus, comparisons made for purposes of the process analysis and for the impact analysis used different data sets, commensurate with the different purposes of the process and impact analyses.

For the impact analysis, the principal comparison is between probationers who **entered** drug court and their matched probation counterparts. Specifically, it is not a comparison of drug court completers and drug court dropouts; nor is it a comparison of drug court completers and matched probation counterparts. Comparing the outcomes for those who entered treatment with the outcomes for those who did not enter treatment is the standard way that program evaluators test for treatment effects (Bloom, 2005) and it is the principal way that other evaluators have studied drug court programs (GAO, 2005). Nevertheless, the Advisory Board requested that we also compare the outcomes for 158 drug court **graduates** with their matched probation counterparts, and this report also summarizes results from that comparison.

Using standard statistical analysis to examine outcomes, we found:

- Compared with non-drug court matched probationers, drug court **participants** (graduates and non-graduates):
 - Are 13 percent less likely to be arrested (0.46 arrest probability for drug court participants and 0.52 arrest probability for matched probationers);
 - Have 34 percent fewer convictions on average (1.45 convictions for drug court participants and 2.20 convictions for matched probationers);
 - Remain arrest-free for 15 percent longer on average (410 days for drug court participants and 356 days for matched probationers);
 - Are 24 percent less likely to be incarcerated (the probability of incarceration is 0.16 for drug court participants and 0.21 for matched probationers);
 - Have 35 percent fewer incidents of incarceration (an average of 0.20 events per drug court participant and 0.31 events for matched probationers); and
 - Have 36 percent fewer suspensions and revocations (an average of 0.25 for drug court participants and 0.39 for matched probationers).

¹ The meaning of observationally equivalent is explained later in this report. A non-drug court probationer is observationally equivalent to a drug court participant if he or she is like the drug court participant with respect to all observed variables that would affect outcomes: criminal record, history of substance abuse, and so on.

- Compared with non-drug court matched probationers, drug court **graduates**:
 - Are 33 percent less likely to be arrested (0.34 arrest probability for graduates and 0.51 arrest probability for matched probationers);
 - Have 47 percent fewer convictions (1.10 convictions for graduates and 2.09 convictions for matched probationers);
 - Remain arrest-free for 25 percent longer on average (480 days for graduates and 383 days for matched probationers);
 - Are 70 percent less likely to be incarcerated (the probability of incarceration is 0.06 for graduates and 0.20 for matched probationers);
 - Have 66 percent fewer incidents of incarceration (an average of 0.10 events per graduate and 0.29 events for matched probationers); and
 - Have 54 percent fewer suspensions and revocations (an average of 0.17 for graduates and 0.37 for matched probationers).
- Compared with their non-drug court matched probationers, drug court **participants** had a 35 percent higher probability of receiving substance abuse treatment (0.65 for drug court participants and 0.48 for matched probationers). Nevertheless, non-drug court probationers frequently received substance abuse treatment, so if substance abuse treatment were the principal ingredient of drug court success, we would only expect a modest incremental improvement from drug court participation. Of course, the provision of treatment is not the only or necessarily the most important element of a drug court program. Compared with non-drug court matched probationers, drug court **graduates** are neither more nor less likely to enter treatment. The apparent explanation is that drug court is successful at reducing relapse to substance abuse, and hence reduces the need for future treatment (beyond the treatment that was delivered as part of the drug court program).

Drug court programs are selective. In the absence of random assignment, therefore, an evaluation based on propensity score case matching (or any other quasi-experimental procedure) may not control for all elements of selection bias. Nevertheless, the best evidence at our disposal indicates that the four drug courts in Suffolk County have increased the receipt of substance abuse treatment and reduced criminal recidivism for a population of otherwise recalcitrant, drug-involved offenders. This evidence is consistent with reports from the Federal Government Accounting Office, which assessed the effectiveness of drug courts over 27 programs for which researchers had reported rigorous evaluations.

2.0 Data Assembly

The study was entirely retrospective. Following extensive review of human subjects and privacy concerns by Institutional Review Boards, Health Insurance Portability and Accountability Act (HIPAA) review boards and other agency authorities, we negotiated access to archival data from several cooperating agencies. Unless stated otherwise, the data include information recorded through Summer 2005. Figure 1 illustrates how the data were compiled.

Massachusetts Municipal/District Courts. With assistance from court and other staff, we created master lists of participants who entered the four drug courts between program startup (1999 or 2000) and Spring 2005. Data include a minimum of name, PCF# (The Board of Probation Central File Number) and program entry date, and some records also include a program exit date and status plus other personal identifiers (Social Security Number or SSN).

Massachusetts Department of Public Health (MDPH). The Bureau of Substance Abuse Services has maintained the Substance Abuse Management Information System (SAMIS) data on AOD treatment services delivered by State-licensed providers since 1991. Variables include treatment program admission and completion dates, treatment type, treatment modality, and referral source for each client episode.

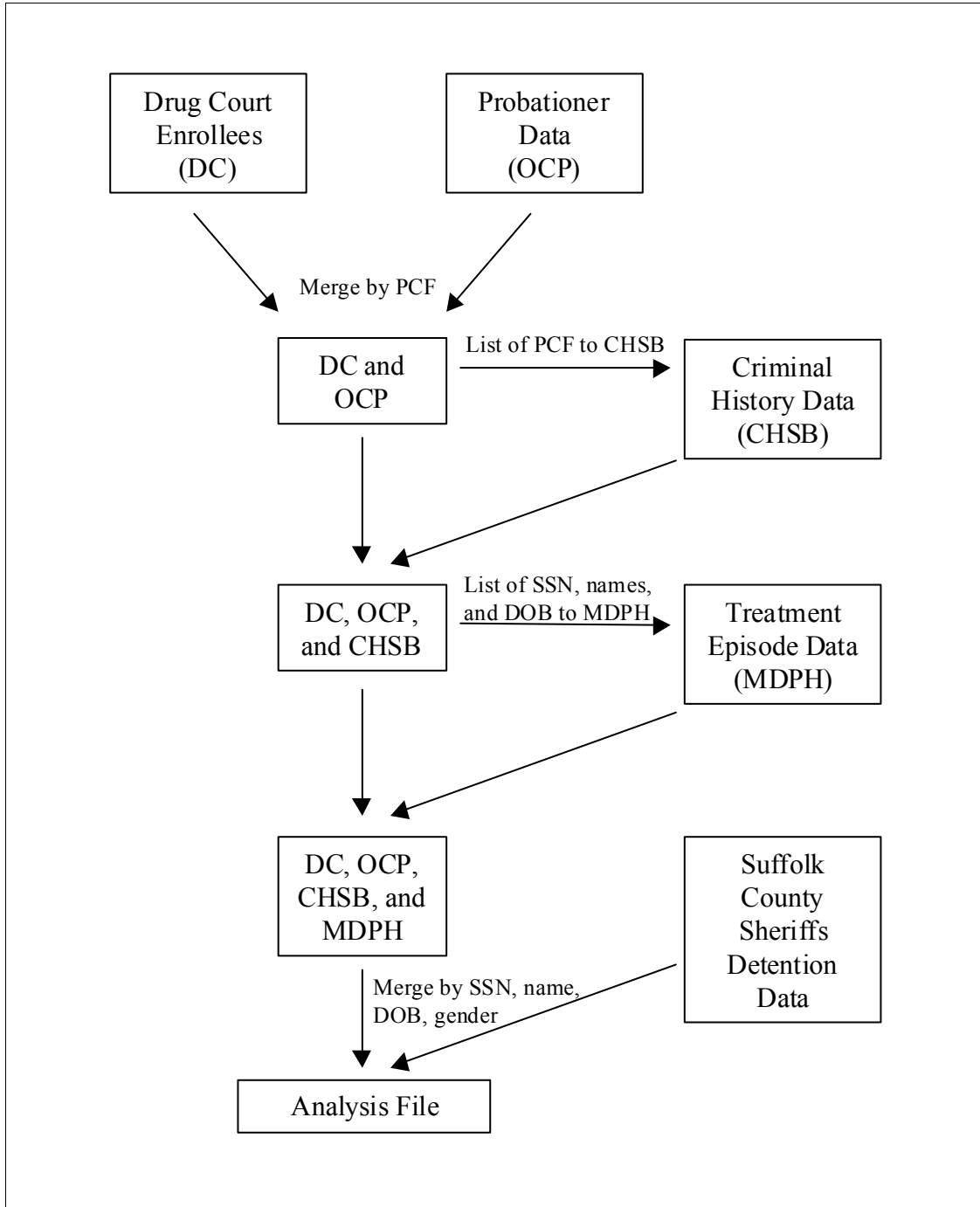
Office of Commissioner of Probation. We accessed electronic adult risk/need offender assessment data based on interviews conducted by probation officers at supervision intake and four-month follow-up (as available through 2003). Data include risk indicators such as criminal history, and service needs such as employment assistance (see Appendix C).² Indicators of problems related to substance abuse, and services needs specific to alcohol versus drug issues, were key to identification of non-drug court probationers for comparison. The probation records were missing for an estimated 30 percent of probationers. The Commissioner's Office told us that data entry was never done for these cases because of limited resources. Drug court status neither increased nor decreased the probability that the probation risk assessment would be missing.

Massachusetts Criminal History Systems Board. We used Criminal Offender Record Information (CORI) data, which record court arraignments and case dispositions, to identify criminal cases, convictions, and probation revocations Statewide since 1998. Data also contain personal identifiers necessary to submit data requests to other agencies (e.g., SSN and birthdates for MDPH data request), as well as demographics (e.g., race/ethnicity) unavailable elsewhere.

Suffolk County Sheriff's Department. Inmate movement data are available for the House of Corrections (HOC) since 1998, and for the jail since 2000. The jail houses pretrial detainees, whereas the HOC houses inmates who were convicted of crimes whose penalty requires serving relatively short periods of confinement. Offenders sentenced to longer periods of confinement, and offenders convicted of certain serious offenses, are housed in state prisons. We lacked data from state prison confinement, but drug court participants are not ordinarily involved in the type of serious, violent crime that requires a state prison sentence.

² Interviews should be conducted within 30 days of case disposition (sentencing) if the probationer is available, or within 30 days of community release if the probationer is in residential treatment, secure housing, or otherwise not immediately available. These data are missing for many probationers, but the consensus of probation experts is that these data are randomly missing, not systematically missing for drug court versus non-drug court probationers.

Figure 1 – A Schematic of Data Assembly



3.0 Process Evaluation

3.1 Suffolk County Drug Court Programs

The four drug court programs lie within inner city urban sections of the Greater Boston area. In 2003, 30.1% of male arrestees in Boston reported drinking heavily³ in the 30 prior days, and 80.3% tested positive for drugs; 51.3% tested positive for marijuana, 31.8% tested positive for cocaine, and 17.3% tested positive for heroin (Zhang, 2004)⁴. Statewide data show increased heroin use among AOD treatment patients (about 15% in FY1992, to nearly 40% in FY2002), and concomitant increases in intravenous drug use, acute care hospitalizations, and opioid-related fatal overdoses (Bureau of Substance Abuse Services, 2002).

The first drug court program in Massachusetts started in 1995 and drew probationers from jurisdictions throughout the Greater Boston area. As local programs opened in multiple jurisdictions, the cross-jurisdiction program was replaced with local programs. The four sites selected for study represent relatively long-standing and stable programs: Roxbury started in 3/1999, South Boston started in 9/1999, Chelsea started in 9/2000, and Brighton started in 11/2000. Together the four programs applied for and received in 2002 a Justice Department enhancement grant, which allowed the four courts to make various improvements to their drug court operations and to contract for this evaluation. The grant required formation of an Advisory Board comprising representatives of all four courts as well as representatives of the District Attorney, the defense bar, the Department of Public Health, and the Suffolk County Sheriff. (The Commissioner of Probation and the Office of Community Corrections declined invitations to participate on the Board.) The Board meets regularly to review policies and procedures, which are recorded in a manual. Representatives from other drug courts and service agencies are invited to observe and share information pertaining to treatment, monitoring, and other resources.

The program mission is to address criminal behavior resulting from addiction to drugs and alcohol in the target population of adult probationers (aged 17 or older) with serious criminal records (likely facing incarceration). Integrating assessment, appropriate treatment referral, judicial review, supervision, drug-testing, and application of graduated rewards and sanctions, the program is designed to restore probationers to a life of sobriety that is free of crime. The programs are postadjudication; that is, drug court participation is a condition of probation that may be ordered at sentencing or subsequently—such as at probation violation hearings or following time served on a split sentence.⁵ Referrals may originate at any time during the criminal justice process, whether pre-sentencing or pending release from incarceration.

Prospective program participants appear in the drug court session, and pending approval of the drug court team (see below), are admitted upon agreement to the rules. These may vary slightly by program, but basically include:

³ Heavy drinking is defined as consuming five or more drinks on at least one occasion in a month.

⁴ None tested positive for methamphetamine, or reported use in the prior 30 days.

⁵ In a special study cohort of 76 participants who entered one of eight drug courts between April and June of 2000, 26.3% began the program under the probation term of a House of Correction/split sentence (Massachusetts Sentencing Commission, 2002).

- remain drug and alcohol free,⁶
- enter treatment and comply with AOD treatment program policies and procedures;
- attend NA/AA meetings and submit verification at drug court sessions (generally 3 times weekly throughout drug court);
- appear for drug court sessions (weekly in Phase 1 and progressively less often in other phases);
- submit to random AOD testing;
- attend relapse prevention programming conducted by Probation; and,
- maintain 3 months sobriety before program graduation.

Termination from the program is decided on a case-by-case basis, but must be after a finding of probation violation, such as new offenses, chronic relapses, or consistent noncompliance. In some circumstances, terminated participants may be readmitted at a later date.

The programs operate on drug court dockets in four municipal/district courts, and are staffed by separate teams led by three judges (i.e., one judge served two of the programs during the majority of the period studied). Although several others parties—including treatment service liaisons, district attorneys, defense attorneys, and a court clerk—contribute to drug court sessions and the status meetings held in chambers beforehand, cases are managed by probation officers who are responsible for status reports.

Each of the four programs is moderate in capacity. Active rosters average 40 drug court participants who are in various phases of program completion, as well as a few who are on warrant status. Participants are expected to transition through three or four phases over a minimum of 12 months. As is true of most drug courts, however, time to completion is extended by behavior resulting in delays in phase progression. Graduated rewards and sanctions are not formalized, and may range from symbolic star stickers or additional urinalysis testing, to phase promotion or short-term jail stays.

Unlike some drug courts in other counties and states, these programs do not contract with one service provider. Participation in AOD treatment is mandatory, and program participants are responsible for obtaining services that are funded by public or private health care benefits. Nonetheless, probation officers provide referrals to various programs according to need, and as part of the federal grant a treatment specialist was added in 2002 to assist the four programs in referrals. Services delivered vary by participant background (e.g., language, ethnic/cultural and gender differences), treatment mode (e.g., detox, residential, intensive outpatient, or maintenance outpatient), and other unique needs (e.g., removal from home neighborhood) that may change over time. Service providers must report to the programs on participant attendance, performance, and urinalysis testing. Urinalysis testing is also conducted by probation officers for monitoring purposes, and is often administered prior to drug court sessions.

⁶ The general program policy is total abstinence, so participants are expected to detox from methadone and addictive drugs; however, rare exceptions may be made (e.g., medications including buprenorphine). Comorbid probationers are eligible provided they are able to comply with program rules.

3.2 Drug Court Program Participants

Table 1, which was taken from probation risk/needs assessment records, describes drug court participants and non-participating probationers who also abused substances.⁷ The purpose of this comparison is to distinguish drug court participants from other drug-involved probationers who did not participate in drug court.

To be included in the tabulations reported in Table 1, a probationer had to meet both of two conditions. First, he or she must have had a risk/needs assessment between 1998 and 2003. We did not collect probation records later than 2003 to allow for a period of recidivism, and a few drug court participants actually had assessments before 1998. Second, a probationer's risk needs assessment record must have been entered into the adult risk/need offender assessment database. As explained in appendix A, probation did not enter a significant number of assessment records. For these two reasons, the 519 drug court enrollees identified in Table 1 are fewer than the 833 drug court participants cited earlier in this report. More than 519 drug court enrollees appear in tables 2-4, because matches with other data sources were higher than they were with probation records.⁸

Appendix B describes analysis that used the data described in Table 1 to estimate the probability that a drug-involved probationer entered drug court conditional on that probationer's criminal history and other factors. Throughout the rest of this report, that estimated probability is called the *propensity score*, a technical term employed in the evaluation literature. We will draw on that analysis as we discuss Table 1.

Table 1 identifies a variable/characteristic of interest in the first column. It then reports the number of drug-involved probationers with that variable/characteristic (column two) and their proportion in the population (column 3). The population of drug-involved probationers is then separated into those who participated in drug court (columns four and five) and those who did not participate in drug court (columns six and seven), thereby allowing a comparison between drug court participants and other drug-involved probationers.

The table requires little interpretation. In some regards drug court participants are not unlike other drug-involved probationers. For example, drug court participants appear to be about the same age and to have similar unmet needs when compared with non-participants.

With respect to factors that are commonly seen as predictors of future criminal recidivism, drug court participants are on average worse risks than other drug-involved probationers. Drug court participants have less stable residential ties and less stable employment records. They have somewhat higher

⁷ Abused substances comprise alcohol and illegal drugs. As explained later in this report, we used logistic regression to estimate the probability that a probationer would participate in drug court. The predicted probability is the propensity score, and the propensity score figures importantly in our outcome analysis. To be included in the process analysis, a probationer had to (1) abuse alcohol or illegal drugs and (2) have a propensity score in excess of 0.04.

⁸ For Tables 2 through 4, we matched all assessment records for drug-involved probationers who were not drug court participants with the other data source (such as House of Correction data), and that match is reported as "other drug-involved probationers." We matched all drug court records with the other data source, and that match is reported as "drug court enrollees." The number of cases for "drug-involved probationers" is the sum of the previous two categories, and the mean for "drug involved probationers" is the weighted averaged across the previous two categories. Thus the number of drug-involved probationers varies across these tables.

probation risk scores (a low value for the risk score indicates a higher risk), which is partly a reflection of their prior criminal records and prior supervision histories.⁹ They appear to have been involved in crime at an earlier age. However, women – who tend to be less recidivistic than men – are more likely to be drug court participants even after controlling for other factors.

The analysis used to estimate propensity scores confirms this simple summary of Table 1. The probation risk assessment score is a twenty-eight-point scale, although most of the probationers represented by Table 1 have scores between 0 and 12 (maximum). Holding other variables constant, a one point decrease on the risk scale increases the probability of drug court participation by about 0.01. Holding other variables (including the risk score) constant, the propensity to participate in drug court increases with the seriousness of the probationer’s criminal record (especially with a history of convictions and incarcerations), and it increases with the number of times that the probationer had entered AOD treatment in the past. Holding all the above constant, those probationers who entered drug court were most likely to have had a probation officer diagnosis of an alcohol-relation need, a drug use-related need, or both. Based on the propensity score analysis, we conclude that those drug-involved probationers who are selected for drug court pose a higher risk of substance abuse relapse and criminal recidivism than those not selected. This finding is consistent with drug court policy.

Table 2, which was taken from criminal history records (CORI), provides a similar comparison based on criminal histories. (Convictions were tabulated on a per-count basis, so that when a person was convicted of three counts, this is recorded as three convictions.) Again, little interpretation is required, because the pattern is apparent. Drug court participants have criminal records that are on average much more serious than the records of drug-involved probationers who did not participate in drug court.

A third table is based on a match with the House of Corrections data. The story is not changed. At the time of their probation risk assessments, drug court participants had more frequent incarcerations, and they had served more time in custody, than had other drug-involved probationers.

⁹ The probation risk score is formula driven based on risk factors observed during the probationer’s initial assessment. See Appendix C.

Table 1

Comparing Drug Court Participants with Other Drug-Involved Probationers: Probation Records

Variable	Drug-Involved Probationers		Drug Court Enrollees		Other Drug-Involved Probationers	
	Cases	Prop.	Cases	Prop.	Cases	Prop.
Court						
Brighton	578	8.6	84	16.2	494	7.9
Chelsea	1304	19.3	66	12.7	1238	19.9
Roxbury	3515	52.1	254	48.9	3261	52.3
South Boston	1352	20.0	115	22.2	1237	19.9
Probation Intake Year						
1998	933	13.8	40	7.7	893	14.3
1999	1028	15.2	87	16.8	941	15.0
2000	1049	15.5	80	15.4	969	15.5
2001	1107	16.3	109	21.0	998	15.9
2002	1294	19.1	100	19.3	1194	19.1
2003	1371	20.2	103	19.9	1268	20.2
Age at Probation Intake						
17-18	418	6.2	20	3.9	398	6.4
19-21	800	11.8	45	8.7	755	12.1
22-25	868	12.8	58	11.2	810	12.9
26-30	964	14.2	88	17.0	876	14.0
31-35	1054	15.5	90	17.3	964	15.4
36-50	2334	34.4	192	37.0	2142	34.2
51 or older	344	5.1	26	5.0	318	5.1
Mean Age at Probation Intake	6782	33.2	519	34.0	6263	33.1
Gender						
Female	1040	15.3	129	24.9	911	14.5
Male	5742	84.7	390	75.1	5352	85.5
Initial Supervision Level						
Minimum	70	1.0	0	0.0	70	1.1
Moderate	980	14.5	31	6.0	949	15.2
Maximum	5732	84.5	488	94.0	5244	83.7
Mean Initial Total Risk Score	6782	8.8	519	6.0	6263	9.1
Offender Initial Risk Need Categories						
Prior record during the past five years						
Three or more	3520	51.9	376	72.5	3144	50.2
Two	1006	14.8	68	13.1	938	15.0
One	1020	15.0	43	8.3	977	15.6
None	1236	18.2	32	6.2	1204	19.2
Prior probation terms in the past five years						
Two or more	2385	35.2	301	58.0	2084	33.3
One	1957	28.9	136	26.2	1821	29.1
None	2440	36.0	82	15.8	2358	37.6

Table 1 (continued)**Comparing Drug Court Participants with Other Drug-Involved Probationers: Probation Records**

Variable	Drug-Involved Probationers		Drug Court Enrollees		Other Drug-Involved Probationers	
	Cases	Prop.	Cases	Prop.	Cases	Prop.
Age of first offense						
16 or younger	2020	29.8	194	37.4	1826	29.2
17 to 19	2055	30.3	173	33.3	1882	30.0
20 to 23	1058	15.6	74	14.3	984	15.7
24 or older	1649	24.3	78	15.0	1571	25.1
Residence changes during the past 12 months						
Two or more	2200	32.4	254	48.9	1946	31.1
One	1918	28.3	130	25.1	1788	28.5
None	2664	39.3	135	26.0	2529	40.4
Employment during the past 12 months						
3 months or less	3934	58.0	385	74.2	3549	56.7
4 to 8 months	1068	15.8	80	15.4	988	15.8
9 months or more	1780	26.3	54	10.4	1726	27.6
Family structure						
Few or negative ties	4167	61.4	315	60.7	3852	61.5
Strong positive ties	2615	38.6	204	39.3	2411	38.5
Substance abuse						
Abuse	4813	71.0	334	64.4	4479	71.5
In treatment/prior problem	1800	26.5	180	34.7	1620	25.9
No problem	169	2.5	5	1.0	164	2.6
Attitude toward supervision plan						
Unresponsive/uncommitted	6508	96.0	499	96.2	6009	95.9
Responsive/committed to plan	274	4.0	20	3.9	254	4.1
Offender Initial Need Categories						
Percent with Alcohol-Related Need	4250	62.7	330	63.6	3920	62.6
Percent with Drug Use-Related Need	5673	83.7	494	95.2	5179	82.7
Percent with an Education Need	2699	39.8	219	42.2	2480	39.6
Percent with an Employment Need	3939	58.1	368	70.9	3571	57.0
Percent with a Family Need	3851	56.8	297	57.2	3554	56.7
Percent with a Social Need	5079	74.9	416	80.2	4663	74.5
Percent with a Counseling Need	5741	84.7	475	91.5	5266	84.1
Percent with Financial Management Needs	4537	66.9	398	76.7	4139	66.1

Table 2**Comparing Drug Court Participants and Other Drug-Involved Probationers: Criminal History**

Variable	Drug-Involved Probationers		Drug Court Enrollees		Other Drug-Involved Probationers	
	Cases	Mean	Cases	Mean	Cases	Mean
Number of arraignments before probation intake date	6795	11.8	552	18.4	6243	11.2
Number of convictions before probation intake date	6795	17.9	552	34.8	6243	16.4
Number of violent convictions before probation intake date	6795	2.8	552	3.6	6243	2.7
Number of sex convictions before probation intake date	6795	0.1	552	0.0	6243	0.1
Number of property convictions before probation intake date	6795	5.6	552	13.8	6243	4.9
Number of other convictions before probation intake date	6795	5.8	552	9.4	6243	5.5
Number of drug convictions before probation intake date	6795	3.4	552	7.5	6243	3.0
Number of revoked suspended sentences before probation intake date	6795	3.6	552	8.4	6243	3.2

Table 3**Comparing Drug Court Participants with Other Drug-Involved Probationers: House of Corrections**

Variable	Drug-Involved Probationers		Drug Court Enrollees		Other Drug-Involved Probationers	
	Cases	Mean	Cases	Mean	Cases	Mean
Percent with any HOC incarcerations before probation intake date	7096	16.5	833	24.7	6263	15.4
Total number of days in HOC before probation intake date	7096	33.5	833	58.9	6263	30.1

Table 4 is based on a match between probation records and records from the Massachusetts Department of Public Health. Although the statistics appearing in this table are straightforward, showing that drug court participants have more frequently been in substance abuse treatment, some interpretation appears essential. One interpretation is that drug court participants have been more aggressive at seeking substance abuse treatment, but while this interpretation is possible, it seems unlikely given that drug court participants lead lives that are in general more disorganized than the lives of other drug-involved probationers. A more likely explanation is that entering substance abuse treatment is often a consequence of legal entanglement, and in that regard, drug court participants have more frequently been involved with the criminal justice system. An entirely different interpretation is that drug court participants have substance abuse problems that (on average) are more entrenched than those of other drug-involved probationers. The consequence is that they have more frequently entered substance abuse treatment. We suspect that both explanations—compulsion from the CJS and more entrenched problems—account for the observation that drug court participants have been in treatment more frequently than have other drug-involved probationers.¹⁰

¹⁰ There is a problem with this table. We ended counting prior substance abuse episodes on the date of the probation assessment, but that may have occurred after entry into drug court for probationers on split-sentences. For example, an examination of Table 1 shows that 35 percent of drug court participants were in treatment when they entered probation, compared with 26 percent of other drug-involved probationers. Prior entry into treatment would be overstated (for drug court participants) as a consequence. The bias cannot offset the pattern shown in Table 4, however. We tabulated the number of treatment admissions for the one-year period immediately prior to entering probation. Drug court participants averaged 1.9 treatment admissions and 0.7 treatment completions; other drug-involved probationers averaged 0.8 treatment admissions and 0.3 treatment completions. Subtracting these figures from the statistics reported in Table 4 would not reverse the pattern that drug court participants had more prior treatment experiences than did other drug-involved probationers.

Table 4**Comparing Drug Court Participants with Drug-Involved Probationers: Substance Abuse Treatment**

Variable	Drug-Involved Probationers		Drug Court Enrollees		Other Drug-Involved Probationers	
	Cases	Mean	Cases	Mean	Cases	Mean
Percent with any AOD treatment episodes before probation intake	6782	42.8	519	75.1	6263	40.1
Number of AOD treatment episodes before probation intake	6782	3.8	519	9.2	6263	3.3
Number of AOD treatment episode completions before probation intake	6782	1.7	519	3.8	6263	1.5
Percent with any inpatient treatment episodes before probation intake	6782	35.8	519	69.0	6263	33.1
Percent with any outpatient treatment episodes before probation intake	6782	43.1	519	72.4	6263	40.7

3.3 Drug Court Program Admission and Status

Table 5 provides basic statistics regarding the flow of cases through the four drug courts. Two observations may be useful. The first is that the drug court records were collected for longer periods in some courts compared with others, so the “year of program entry” should be interpreted cautiously for 2005. Second, it appears that the use of the code “committed” in Roxbury and South Boston is largely equivalent to the use of the code “terminated” in Brighton and Chelsea. While we can observe most outcomes, we cannot be sure of the eventual outcomes for drug court participants classified as “active” and “absconded/warrant”. Nevertheless, it appears that more than half of all drug court participants will leave drug court prior to graduation. The graduation rate is comparable to that experienced by other drug court programs, which ranged between 27 percent and 66 percent in 27 programs reviewed by the Government Accounting Office (GAO, p. 6).

Table 5**Drug Court Program Entry and Exit**

Variable	Overall		Brighton		Chelsea		Roxbury		South Boston	
	Cases	Mean	Cases	Mean	Cases	Mean	Cases	Mean	Cases	Mean
Number of cases	833		129		154		381		169	
Year of program entry										
Missing ¹¹	68	8.2	0	0.0	0	0.0	6	1.6	62	36.7
1998	20	2.4	0	0.0	1	0.6	18	4.7	1	0.6
1999	124	14.9	0	0.0	0	0.0	122	32.0	2	1.2
2000	109	13.1	29	22.5	25	16.2	48	12.6	7	4.1
2001	131	15.7	17	13.2	42	27.3	67	17.6	5	3.0
2002	105	12.6	26	20.2	18	11.7	54	14.2	7	4.1
2003	120	14.4	22	17.1	29	18.8	47	12.3	22	13.0
2004	116	13.9	30	23.3	30	19.5	19	5.0	37	21.9
2005	40	4.8	5	3.9	9	5.8	0	0.0	26	15.4
Year of program exit										
Missing	708	85.0	129	100.0	29	18.8	381	100.0	169	100.0
2001	16	1.9	0	0.0	16	10.4	0	0.0	0	0.0
2002	41	4.9	0	0.0	41	26.6	0	0.0	0	0.0
2003	20	2.4	0	0.0	20	13.0	0	0.0	0	0.0
2004	24	2.9	0	0.0	24	15.6	0	0.0	0	0.0
2005	24	2.9	0	0.0	24	15.6	0	0.0	0	0.0
Final program status										
Active	104	12.5	30	23.3	20	13.0	27	7.1	27	16.0
Graduated	290	34.8	35	27.1	30	19.5	159	41.7	66	39.1
Terminated	127	15.2	47	36.4	71	46.1	5	1.3	4	2.4
Absconded/ Warrant	74	8.9	14	10.9	21	13.6	23	6.0	16	9.5
Committed	199	23.9	0	0.0	0	0.0	143	37.5	56	33.1
Unknown	39	4.7	3	2.3	12	7.8	24	6.3	0	0.0

4.0 Impact Evaluation

4.1 Research Questions

The basic research question asked whether or not probationers who participated in drug court had different outcomes than did similarly situated probationers who did not participate in drug court. There were several outcomes, which fell into three generic categories:

- Criminal recidivism and incarceration
- Performance on probation
- Receipt of substance abuse treatment

¹¹ Fifty of the 62 participants in South Boston with missing dates had the year of the drug court entry but not the specific date.

We expect that drug court participants will more frequently participate in substance abuse treatment. We expect that they will have less criminal recidivism. And we expect that they will have different probation experiences. Of course, the null hypothesis is that drug court participants perform neither better nor worse than other similarly situated probationers.

4.2 Approach

The key step when performing evaluation research is to identify a *counterfactual*: evidence of how members of a treated group would have performed had they not been treated. In the present case, the *treatment* is participation in drug court. The counterfactual is provided by the performance of similar probationers who did not participate in drug court. The evaluation rests on a comparison of the outcomes for drug court participants and other probationers.

For the counterfactual to be useful, and thus for the comparison to be useful, an evaluator has to compare drug court participants with other probationers who are identical in every way except for participation in drug court. When that is true, any difference between the average outcomes for drug court participants and other probationers must be attributable to participation in drug court.

Of course, no two individual are ever identical, and what researchers really do is select treated and comparison subjects so that they are *observationally* the same—that is, they are the same on variables such as age, gender, race, and criminal history. In the absence of a random experiment, wherein treated and controlled subjects would be randomly assigned either to drug court or the control condition, researchers use statistical models to establish this observational equivalence. Our choice was to use matching based on propensity scores.

The technique is detailed in Appendix B. Here we note that for every drug court participant, we identified up to three non-drug court probationers who were observationally equivalent. Under restrictive conditions, statistical theory allows us to assert that the observed difference between the outcome for the drug court participant and the average outcome for the three matched probationers is an unbiased estimate of the effectiveness of drug court for that specific drug court participant. The overall average effectiveness of drug courts is then the average of these individual estimates of treatment effectiveness. We report that overall average in the following tables and discuss the estimates in the following text.

Before examining those average treatment effects, however, four comments may be useful. The **first** comment is that study subjects were at risk for varying lengths of time. Because of the way that we assembled the data, the follow-up period was the same for every probationer in a matched set,¹² so variation across follow-up periods does not affect the within-set comparisons. The length of the follow-up period does vary across the matched sets, and the average treatment effect is the average over those sets. This has no important implications for our statistical tests, nor for interpreting the findings, provided we understand the construction of the estimator.¹³

¹² The observable follow-up period actually differed across the four members of the matched set. We identified the shortest follow-up period within the matched set, and then we truncated the follow-up period for every other member of the matched set to conform.

¹³ Possibly the treatment effect varies systematically with the length of the follow-up period. We eventually test for this possibility by regressing the estimated treatment effect for every set of probationers on the length of the follow-up period.

Our analysis used two definitions of the follow-up period. For some purposes, we defined the follow-up period to begin when the probationer entered drug court. This definition seemed most appropriate when answering questions about activities that were a direct and fairly immediate consequence of drug court participation, specifically, the receipt of substance abuse treatment. For other purposes, we defined the follow-up period to begin one year after the probationer entered drug court.¹⁴ This definition seemed most appropriate when answering questions about outcomes that required a drug court participant to receive a “dose” of drug court. Criminal recidivism falls into this category. Furthermore, we required the follow-up period to be at least six months to allow time for differences between drug court and non-drug court probationers to manifest.

The **second** comment is that we cannot be sure that the propensity score method accounts for all the differences across probationers within the matched set. We are especially concerned that judicial observations of a probationer’s behavior can trigger an offer to participate in drug court. Because that behavior is not captured by our data, and because that behavior is likely to be a harbinger of future behavioral outcomes, the process of selection into drug court raises the serious possibility of statistical selection bias.¹⁵ Drug court judges tell us that they often recruit participants who are having trouble on regular probation. Consequently, it seems likely that the estimated treatment effect would be biased so as to understate the effectiveness of drug court, and the analysis will be prejudicial against finding a favorable treatment effect for drug courts.

The **third** comment is that the sample size entering the outcome analysis (typically about 300 cases) is considerably smaller than the sample size used to construct Table 5 (833 cases). Appendix A provides a detailed explanation of why the sample fell from 833 to 398 cases, reporting that there are two principal reasons:

- First, because of budget constraints, the state went through periods when it did not enter probation assessments into its electronic database. If we could not match a drug court record with a probation assessment, then we could not match the drug court case with a drug-involved probationer, so the drug court case had to be excluded from the outcome analysis. The State has assured us that probation assessment records are missing for institutional reasons that have nothing to do with the probability that a probationer will be selected for drug court,¹⁶ so while the

¹⁴ Of course, matched probationers did not have a drug court date, so we had to identify an equivalent date. We matched drug court participants and other probationers based on their propensity scores, otherwise restricting the matches to be with probationers from the same court. Once we had identified the matched set, we determined the number of months between when the drug court participant entered probation and when he or she entered drug court. Call the time between entering probation and entering drug court X. Then to determine the start date for the matched probationers, we added X to the beginning of their probation dates. This calculation provided the beginning of the follow-up period for the first definition of the follow-up period; we added 365 days to X to determine the beginning of the follow-up period for the second definition.

¹⁵ Evaluators use the term “selection bias” when referring to the possibility that the treated group (drug court participants) and the comparison group (matched non-participating probationers) differ in ways that are not recorded in the data and that those differences, not drug court participation per se, may account for part of the estimated treatment effect. “Selection bias” is a technical term and holds no implications that the courts are somehow biased.

¹⁶ One might be concerned that the drug courts place a special burden on probation officers, so that drug court cases are more prone to have missing probation records. There is no reason to believe that this is true. The assessments are the same for drug court cases and for other probation cases, so there is no special burden.

reduction in case size will affect the precision of our estimates, it should not introduce any bias into our inferences.

- The second principal reason why the sample fell from 833 to 398 cases is that some drug court participants had probation assessments after 2003, while our probation records included assessment performed through 2003. There is little practical loss from this latter attrition because these unmatched drug court cases would typically have had follow-up periods that were too short to be included in the analysis.

There are two reasons why the sample then fell from 392 drug court cases to about 300 cases (actually, between 248 and 344 drug court cases depending on the follow-up period and the outcome measure).

- First, for technical reasons explained in Appendix B, we could not always find adequate matches based on the propensity score for drug court participants. This resulted in a loss of 53 cases.
- Second, some drug court cases are too recent to afford a minimum six-month follow-up period especially when the follow-up period began one year after the probationer entered drug court. We also note that the number of cases varies depending on the definition of the follow-up period and depending on the outcome measure. The reason for the first differences is that we required the follow-up period to be at least six months, so there were more cases when the risk period started earlier (the first definition) than when it started later (the second definition). The reason for the second difference is that the outcome measures came from different data sources, and each source used a different cut-off date for providing data.

The **fourth** comment is that we are using an *intent-to-treat* model, which researchers consider being the most rigorous (and least biased) way to estimate treatment effectiveness (Bloom, 2005) and has been the standard for rigorous drug court evaluation research (GAO, 2005, p. 10). Once accepted into drug court, a probationer is considered to be a drug court participant even if he or she ultimately refuses any degree of participation in drug court. The estimated treatment effect will obviously understate the effect of drug court on those who complete drug court.

Researchers sometimes estimate another form of the treatment effect called the *effect of treatment on the treated*, which in this case would be applicable to individuals who participated materially in drug court, whether or not they graduated from drug court. Suppose for example that 25 percent of drug court participants dropped-out of drug court after a few days, so we could reasonably conclude that drug court had neither helped them nor harmed them. Then the remaining treatment effect could be attributed exclusively to the other 75 percent of drug court participants, some of whom ultimately graduated from drug court and others of whom ultimately failed to graduate. We could make this attribution if we had an estimate of the percentage of drug court participants who failed to participate materially in drug court, but such estimates were only available for Chelsea, where 3 percent of participants spent fewer than 30 days and 8 percent spent fewer than 50 days in the drug court program. If these percentages are representative of the other drug courts, we could not expect the effect of treatment on the treated to be much larger than the average treatment effect from the intent-to-treat model perspective.

Representatives of the Suffolk County Drug Court Program Advisory Board felt that it would be useful to compare the outcomes for program graduates with their matched probationers. While this comparison cannot provide an unbiased estimate of a treatment effect, it can answer the straightforward question:

How do the outcomes for program graduates compare with the outcomes for their matched counterparts who did not participate in drug court?

Interpretation requires caution. Graduates and non-graduates are both “treated” in the sense that they both place demands on drug court program resources. The graduates are successful participants; the non-graduates are failures. The estimated treatment effect based on the intent-to-treat framework takes both successes and failures into account, but a comparison of graduates and their matched counterparts only takes successes into account.¹⁷ Furthermore, a comparison of graduates and their matched counterparts is susceptible to selection bias (GAO, 2005, p.19) if unmeasured factors – such as motivation to change behavior – have an important role in explaining success, because graduates are likely to be more motivated to alter their behaviors than are their matched counterparts.¹⁸

4.3 Findings

Table 6 summarizes results based on the outcomes for drug court participants who were matched with up to three drug-involved probationers. The first column identifies the outcome measures. The mean (and standard deviation) for drug court participants appears in columns four and five, respectively. The mean (and standard deviation) for matched comparison probationers appears in columns six and seven, respectively. Our interest is focused on the difference in the mean outcome for drug court participants and the matched comparison probationers (labeled the estimated treatment effect in the table). Light shading indicates that the differences is statistically significant at $p < 0.05$. Heavy shading indicates that the difference is statistically significant at $p < 0.10$. We used one-tailed tests of significance because while we were uncertain that drug court would do any good, we were confident that it should do no harm.

¹⁷ An analogy may be useful. Suppose that 100 patients were treated for cancer, and the treatment regimen lasts one year. Between months six and twelve, 60 patients abandon the treatment regimen because it appears to be ineffective. The other 40 patients complete the treatment regimen. Success is defined as living for two years. Of those 60 patients who abandoned the treatment, 10 live for two years; of those 40 patients who completed the treatment, 30 live for two years. Suppose that 100 matched patients received no treatment, and 20 of them were still alive after two years.

From the intent to treat framework, 40 patient who entered treatment were successful, and 20 comparison patients were successful, so the treatment effect is $0.3 = 40/100 - 10/100$. Basing the treatment effect on those who actually completed treatment, we might conclude that the treatment effect is $0.65 = 30/40 - 10/100$. The second estimate of treatment effectiveness might be justifiable if physicians could triage patients so that only those patients who have a high probability of benefiting from treatment and of completing the treatment regimen would receive treatment.

The analogy might be extended to drug court. Judges select offenders for drug court. Once that selection is performed, all drug court participants receive a dose of treatment in the form of supervision, support and substance abuse treatment. Some fail (the analogue to leaving cancer treatment) and some graduate (the analogue to completing the cancer treatment regimen), but all enter the base used to estimate treatment effectiveness based on the intent-to-treat perspective.

¹⁸ The problem here is that the matching was done without taking motivation to change into account. Essentially, the graduate demonstrates that motivation by completing the drug court program. If motivation to change is randomly distributed within the matched set, then graduates are more likely to be motivated than are their matched counterparts, and the resulting comparison is biased.

Drug court participants (including graduates and non-graduates) and the matched probationers (none of whom participated in drug court) were frequently arrested during the follow-up period.¹⁹ However, we find that drug court participants are less likely to be arrested ($p < 0.05$); they have fewer future convictions on average ($p < 0.05$); and they remain arrest-free for longer periods on average ($p < 0.05$).²⁰ The differences in arrest rates are not large, but nevertheless, participation in drug courts appeared to lower the arrest rate by about 13 percent (from 52% to 46%).²¹ Participation in drug court reduced the number of convictions during the follow-up period by about 34 percent (from 2.20 to 1.45).²² Participation in drug court increased the time until criminal recidivism by about 15 percent (from 356 days to 410 days).

Drug court participants have a lower probability of being incarcerated following their drug court experience than do the matched probationers ($p < 0.05$). They also have fewer future incarcerations ($p < 0.05$). And they have fewer episodes of suspensions and revocation ($P < 0.10$). During the follow-up period, about 16 percent of the drug court participants were incarcerated at least once, compared with 21 percent of the matched probationers, a reduction of 24 percent. Also during the follow-up period, drug court participants experienced an average of 0.20 episodes of incarceration, while the matched probationers experienced an average of 0.31 episodes of incarceration, a reduction of 35 percent. Over the same period, drug court participants averaged 0.25 suspensions/revocations while the matched probationers experienced an average of 0.39 suspensions/revocations, a 36 percent differences.

Curiously, when compared with their matched counterparts, drug court participants spent more time in custody during the follow-up period ($P < 0.05$), an observation that requires some investigation given that drug court participants are less likely to be incarcerated. In 33 percent of the comparisons, drug court participants actually spent less time in custody than did the matched probationers; in 23 percent of the comparisons, drug court participants spent more time in custody; and in 44 percent of the comparisons they spent the same amount of time (typically, zero). Thus, the explanation for why drug court participants spent more time in custody on average is that a few of the drug court participants served lengthy terms. This explanation is reinforced by the observation that the standard deviation for time in custody for drug court participants is 110.9 days, while the standard deviation for their matched comparisons is just 47.1 days. Additionally, these statistics may be misleading by the fact that we could

¹⁹ The Massachusetts Sentencing Commission (2002) used criminal activity records information (CARI) to compile aggregate statistics on new arrests among criminal offenders released into the community. Half of the 76 participants, who entered one of the study's eight drug courts between April and June of 2000, recidivated within one year. Our findings are consistent with those reported by the Sentencing Commission.

²⁰ The variable "Days until First Arrest" requires some explanation. Data censoring occurs when an arrest did not occur between the beginning and end of the follow-up period. When data were censored, we substituted the length of the follow-up period for the time until recidivism.

²¹ To compute these percentages, let A be the outcome for the matched probationers and let B be the outcome for the drug court participants. Then the percentage change is $(A-B)/A$. GAO (2005, p. 45) reports that drug courts have reduced the probability of a new arrest by 10 to 30 percent, but we must be cautious when comparing the GAO report with current estimates. First, GAO reported estimates only for drug court programs that showed a statistically significant effect, so that programs with smaller effects are likely excluded from the estimate. Second, the percentage reduction pertains to the year after entering drug court, while our estimates pertain to the period that begins one year after entering drug court. Third, our estimates pertain to a variable-length follow-up period. Nevertheless, GAO's report of a 10 to 30 point difference between the arrest rate for drug court participants and non-drug court participants seems comparable to the estimates reported in this present study.

²² GAO (2005, p.48) reported that drug court participants had convictions rates that are roughly about 10 percentage points lower than non-drug court participants.

not capture prison terms in our data, and the higher incarceration rate for matched probationers may mean that they more frequently spent time in prison.

We expect that drug court participants would have greater rates of participation in substance abuse treatment than did their matched probationers. This expectation is decisively confirmed by the four measures of treatment receipt, each of which was statistically significant at $P < 0.05$. The statistics probably understate the relative receipt of treatment by drug court participants, because some drug court participants entered drug court several months before their probation assessment date, and that early entry would not be reflected in the statistical testing.²³

Although drug court did increase the receipt of treatment by 35 percent (from 48% to 65%), substance abuse treatment is not rare for non-drug court probationers. If receipt of substance abuse treatment were the principal ingredient of drug court effectiveness, then there is probably little basis for expecting drug court participants to have radically different outcomes from those of their matched comparisons.

Knowing that a probationer received substance abuse treatment required us to match probation records with substance abuse treatment records, and it seems possible that the matching was imperfect. If true, we would understate the receipt of treatment, but there is no reason to expect that the understatement is larger for drug court participants than for other probationers.

²³ A drug court participant must necessarily be on probation, but the probation assessment occurs sometime after an offender enters probation. The delay may be substantial when the offender receives a split sentence so that the early stage of his or her probation is spent in custody. We found that the assessment is done within 15 days for about 36 percent of drug court participants and within 45 days for 72 percent of drug court participants. The elapsed time between entering probation and receiving an assessment is more than 60 days for 13 percent of drug court participants.

As noted earlier, we can compare the outcomes for graduates with the outcomes for their matched counterparts, and we can compare the outcomes for non-graduates with their matched counterparts. These two sets of treatment effects are reported in Table 7. The table reports the number of cases that enter into these calculations and the standard error for that estimate. For convenience, the table repeats the estimates for participants in general, which was already reported in Table 6.

Interest is focused on the estimated effect for graduates, specifically, a comparison of the outcomes for graduates and their matched probationers. With respect to the reduction in arrest rates, the estimate based on graduates alone is 2.6 times larger than it was for participants (-0.171 for graduates and -0.067 for participants in general). With respect to the reduction in convictions, the estimate based on graduates alone is 1.3 times larger (-0.753 for graduates and -0.991 for participants). With regard to future incarceration, the estimate for graduates alone is 3.0 times larger (-0.14 for graduates and -0.047 for participants). Similar favorable comparisons arise when using other measures of recidivism and criminality for comparison.

Compared with their non-drug court matched probationers, drug court graduates are less likely to be arrested ($P < 0.05$), to remain arrest free for a longer period ($P < 0.05$), to have fewer convictions ($P < 0.05$), and are more likely to avoid incarceration ($P < 0.05$) and to spend less time incarcerated ($P < 0.05$).

Compared with their non-drug court matched probationers, drug court graduates:

- Are 33 percent less likely to be arrested (0.34 arrest probability for graduates and 0.51 arrest probability for their matched probationers);
- Have 47 percent fewer convictions (1.10 convictions for graduates and 2.09 convictions for matched probationers);
- Remain arrest-free for 25 percent longer on average (480 days for graduates and 383 days for matched probationers);
- Are 70 percent less likely to be incarcerated (the probability of incarceration is 0.06 for graduates and 0.20 for matched probationers);
- Have 66 percent fewer incidents of incarceration (an average of 0.10 events per graduate and 0.29 events for matched probationers); and
- Have 54 percent fewer suspensions and revocations (an average of 0.7 for graduates and 0.37 for matched probationers).

Our findings are consistent with those reported by GAO (2005, p.7):

Positive results concerning recidivism are closely associated with program completion. Specifically, while drug court participation is generally associated with lower recidivism, the recidivism of program completers is lower than for participants in comparison or control groups.

The GAO concludes that: “Thus, strong practices that encourage program completion may enhance the success of drug court programs in relation to recidivism.” (GAO, 2005, p.7.) While we disagree with the GAO that such an inference *necessarily* follows from the data, it seems reasonable to suppose that increased retention in drug court would have a salutary effect on program outcomes. Unfortunately, GAO was unable to identify how courts could increase retention.

Table 6

Comparative Outcomes for Drug Court Participants and Matched Probationers

Overall Difference Descriptive Statistics	Estimated Treatment Effect		Outcome for Drug Court		Outcome for Matched Probationers	
	Mean	Std Error	Mean	Std Error	Mean	Std Error
CRIMINAL RECIDIVISM						
Arrest (303)	-0.067	0.031	0.455	0.499	0.523	0.317
Number of Convictions (303)	-0.753	0.218	1.449	3.045	2.201	3.006
Days until First Arrest (303)	54.34	15.121	410.4	288.2	356.0	191.1
Incarceration (303)	-0.047	0.024	0.162	0.369	0.209	0.247
Number of Total Incarcerations (303)	-0.108	0.038	0.201	0.511	0.309	0.457
Number of Jail Admissions (303)	-0.136	0.088	0.525	1.332	0.661	0.894
Number of Days in Custody in Suffolk County (248)	19.085	7.548	41.0	110.9	21.9	47.1
Number of Days in Suffolk County HOC (248)	17.682	7.046	35.5	104.2	17.8	40.1
Number of Days in Nashua Street Jail (248)	1.175	1.546	5.492	20.814	4.317	12.601
RECEIPT OF SUBSTANCE ABUSE TREATMENT						
Any Treatment after Drug Court Date (341)	0.164	0.029	0.645	0.479	0.481	0.309
Number of Treatment Episodes after Drug Court (341)	0.620	0.239	2.704	3.937	2.084	2.470
Any Inpatient Treatment after Drug Court Date (341)	0.165	0.029	0.563	0.497	0.398	0.293
Any Outpatient Treatment after Drug Court Date (341)	0.230	0.030	0.628	0.484	0.398	0.292
PERFORMANCE ON PROBATION						
Any Suspensions/Revocations (303)	-0.006	0.019	0.092	0.290	0.098	0.183
Number of Suspensions/Revocations (303)	-0.138	0.089	0.254	0.989	0.392	1.163

Notes: The estimated treatment effect is based on the matched pairs and does not necessarily equal the difference between the outcome for drug court participants and the mean outcome for matched probationers because we could not always match three probationers with every drug court participant, a necessary condition for the estimated treatment effect to equal the difference of means. On average, a matched set had 1 drug court participant and 2.93 non-drug court probationers.

Further analysis (reported in Appendix B) affords some additional conclusions. We used regression analysis to determine how the drug court treatment effect (for participants, not graduates) varied with three factors. The first factor was the length of the follow-up period. If treatment effectiveness increased over time, then the regression would show that treatment effectiveness had a positive correlation with the length of the follow-up period. If the opposite were true, the correlation would be negative. The second factor was the propensity score, that is, the estimated probability that a probationer would enter drug court. If treatment effectiveness increased with the propensity score, then we would conclude that drug court does the most good for the most serious offenders, and we would conclude the opposite if treatment effectiveness decreased with the propensity score. Finally, we investigated whether or not the treatment effect varied across the courts.

With respect to the length of the follow-up period, we found that a statistically significant diminution in the treatment effect with respect to:

- Receipt of treatment during the follow-up period
- Receipt of outpatient treatment during the follow-up period

Although the size of the treatment effect decreased over time, it did not disappear, at least not for several years. Perhaps the size of the treatment effect decreased over time because substance abuse is a relapsing condition. Relapse leads to new entanglements with the law, perhaps to be followed by new admissions to drug court.²⁴

We find some evidence that the treatment effect diminishes as the propensity score gets larger, but only for the incarceration outcomes: probability of incarceration, number of jail admissions, and number of days in custody in Suffolk County. Given that the treatment effect with respect to recidivism is not affected by the size of the propensity score, we are not inclined to make much of this effect.

The treatment effect appeared to vary across the drug courts, but only with respect to the number of convictions and jail admissions. The finding was statistically significant at $P < 0.10$ for both. Again, because we only find that this difference holds for two outcomes, we are inclined to dismiss the importance of the findings. Of course, failing to find a difference across the courts could be a consequence of fairly small court-specific samples, but the best evidence available to us argues that drug court is equally effective in each of the four settings.

²⁴ We also find that the treatment effect increases for the dependent variable “days until first arrest.” This could happen if the treatment effect causes a constant proportionate increase in time until criminal recidivism, so that an increased length of the follow-up period would necessarily increase the average difference between drug court participants and matched probationers. Otherwise, it is difficult to understand why the probability of criminal recidivism is invariant with respect to the length of the follow-up period while the time until criminal recidivism increases with respect to the length of the follow-up period.

Table 7**Comparative Outcomes for Graduates/New Graduates and Matched Probationers**

	All Participants			Graduates			Non-Graduates		
		Estimate	Std. Err.		Estimate	Std. Err.		Estimate	Std. Err.
Arrest	303	-0.067	0.031	148	-0.171	0.042	155	0.032	0.043
Number of Convictions after Probation Date	303	-0.753	0.218	148	-0.991	0.324	155	-0.525	0.290
Days Until First Arrest	303	54.344	15.121	148	97.054	20.903	155	13.563	20.920
Incarceration	303	-0.047	0.024	148	-0.140	0.028	155	0.041	0.035
Number of Total Incarcerations	303	-0.108	0.038	148	-0.191	0.053	155	-0.028	0.052
Number of Jail Admissions	303	-0.136	0.088	148	-0.417	0.112	155	0.131	0.129
Number of Days in Custody in Suffolk County	248	19.085	7.627	126	-7.802	5.512	122	46.853	13.914
Number of Days in Suffolk County HOC	248	17.682	7.046	126	-5.958	4.820	122	42.096	12.970
Number of Days in Nashua Street Jail	248	1.175	1.546	126	-1.844	1.235	122	4.292	2.818
Any Treatment after Drug Court Date	341	0.164	0.029	158	0.040	0.043	183	0.271	0.037
Number of Treatment Episodes after Drug Court Date	341	0.620	0.239	158	0.080	0.330	183	1.086	0.334
Any Inpatient Treatment after Drug Court Date	341	0.165	0.029	158	0.057	0.042	183	0.259	0.038
Any Outpatient Treatment after Drug Court Date	341	0.230	0.030	158	0.234	0.044	183	0.226	0.040
Any Suspensions/Revocations after Probation Date	303	-0.006	0.019	148	-0.041	0.020	155	0.028	0.030
Number of Suspensions/Revocations after Probation Date	303	-0.138	0.089	148	-0.203	0.108	155	-0.075	0.133

Notes: See notes to Table 6. Graduates have successfully completed the Drug Court Program. Non-Graduates comprise everyone else, including offenders who are still active in the program.

5.0 Conclusions

Drug court programs are selective. In the absence of random assignment, therefore, an evaluation based on propensity score case matching (or any other quasi-experimental procedure) may not control for all elements of selection bias. Nevertheless, the best evidence at our disposal indicates that the four drug court in Suffolk County have increased the receipt of substance abuse treatment and reduced criminal recidivism for a population of otherwise recalcitrant, drug-involved offenders. This evidence is consistent with an extant literature that drug courts can be effective at reducing criminal recidivism across diverse settings (GAO, 2005; Belenko, 2001).

Appendix A

Data Assembly

This appendix discusses the assembly of data for the drug court evaluation. Although we started with 833 drug court participants, some of the analysis files retained fewer than 300 drug court participants. Case matching across files explains some of this attrition, reducing the sample from 833 to 398. Table 8, and the text in this memo, explains the reasons for attrition from 833 to 398. (We discuss the attrition from 398 to 300 later.) We tabulated program status for most of these 833, and that tabulation appears as Table 8.

Table 8 – Number of Drug Court Participant/Entry Date Observations, by Site and Inclusion to the Analysis File

	Brighton	Chelsea	Roxbury	South Boston	Total
Total	129	154	381	169	833
Observations that Match to Probation Data	84	66	254	115	519
OCP entry date after DC entry date	24	14	79	41	158
DC entry date after final OCP exit date	12	11	23	36	82
DC entry date between OCP entry and exit dates	48	41	152	38	279
Participant Records with OCP and CORI data*	81	65	252	114	512
Participants with OCP and CORI data, but DC entry date after last probation exit date	70	55	229	78	432
Participants with OCP and CORI data, DC entry before probation exit, and a non-missing DC entry date	70	55	229	44	398

*This row may also be considered a the match including the MDPH and SCSD data, as non-matches from those dataset are assumed to be zero

The study was entirely retrospective. Following extensive review of human subjects and privacy concerns by Institutional Review Boards, Health Insurance Portability and Accountability Act (HIPAA) review boards and other agency authorities, we negotiated access to archival data from several cooperating agencies. Unless stated otherwise, the data include information recorded through Summer 2005. Figure 1 (see main text) illustrates how the data were compiled.

Massachusetts Municipal/District Court

With assistance from court and other staff, we created master lists of participants who entered the four drug courts between program startup (1998 to 2000) and Spring 2005. Data include a minimum of name, PCF# (The Board of Probation Central File Number) and program entry date. Records from Brighton included some additional personal identifiers such as date of birth, gender, and Social Security Number. Chelsea included the drug court program exit date.

Prior to processing the rest of the data there were some records that required some scrutiny. First, there were a handful of duplicate PCF numbers from each drug court. In most cases, these duplicates had very different entry dates (e.g. one entry in 1999 and another in 2003). However, for one instance in Roxbury

and one in South Boston a duplicate PCF number existed for a common drug court entry date. We removed those duplicates from our analysis file. In addition, there were two participants in Roxbury that also appeared in Chelsea and one participant in South Boston that also appeared in Brighton, all with similar entry dates. We removed the Roxbury and South Boston records from our analysis file. We also removed one record from Roxbury whose status was indicated as “Out.” The unduplicated totals appear in the first row on Table 7: **833** drug court participants.

Office of Commissioner of Probation (OCP)

We accessed electronic adult risk/need offender assessment data based on interviews conducted by probation officers at supervision intake and four-month follow-up (as available). These data covered probation entry from 1998 through 2003. Data include risk indicators such as criminal history, and service needs such as employment. Indicators of problems related to substance abuse, and services needs specific to alcohol versus drug issues, were key to identification of non-drug court probationers for comparison.

We matched the OCP data to the drug court participants using the PCF number. An unconditional match of PCF number/drug court entry date combinations yielded 536 matches of our 833 unduplicated records. Since a participant could have more than one spell on probation, and since we were matching data from different time periods, we took the best match between an OCP record and drug court participant. If a drug court entry occurred within a probation period, we ignored all other potential matches. If not, we then took the match that the probation entry date was closest to the drug court entry date. If there were two drug court entries but only one probation record, we matched the probation record to the closest drug court entry. These rules trimmed the unconditional match of 536 to **519** drug court participants.

There are two additional reasons for case attrition at this stage of data assembly. One reason is that the risk assessments were only available for probation cases that began between the beginning of 1998 and the end of 2003. About 156 drug court cases began in 2004 and 2005, so risk assessment were unavailable for many of them. This is not an important loss for the evaluation, because recent drug court cases could not enter the analysis due to insufficient follow-up time. We discussed the low match rate with our contact at OCP, and he informed us that at times OCP would not have the staff available to enter the probation assessments into their electronic database. He indicated there was not a systematic exclusion of entering the records. In other words, we may think of the OCP assessment database as an unbiased sample of all OCP assessments.

We cannot precisely partition the sample attrition between these two sources. However, we note that the match rate for probations who entered drug court between 1999 and 2003 averaged about 70 percent. For those who entered drug court during 2004, the match rate was 46 percent, and for those who entered in 2005, it was 38 percent. It was 52 percent for those who entered drug court during 1998, presumably because several of them (only 21 people) entered probation during 1997 or earlier. Prorating the 70 percent match between 1998 and 2003 over the entire period, we estimate that about 83 percent of the case attrition is attributable to missing risk assessment records, and the other 17 percent is attributable to drug court cases that began after 2003.

Massachusetts Criminal History Systems Board (CHSB)

Criminal Offender Record Information (CORI) data that record court arraignments and case dispositions were used to identify criminal cases, convictions, and probation revocations statewide from 1998 to September 2005. These data also yield personal identifiers necessary to submit data requests to other agencies (e.g., SSN and dates of birth for MDPH data request), as well as demographics (e.g., race/ethnicity) unavailable elsewhere.

After matching the OCP records with the drug court participants, we requested CORI data from the CHSB. We sent CHSB the PCF numbers of drug court participants and non-participants to be included in the analysis. For the drug court participants, we requested data for only those who matched to an OCP record.²⁵ For the other probationers, we requested data for those who would have had some probability of entering drug court. These probationers were mainly those for whom risk assessment identified some alcohol or drug treatment need.

From the CORI data, for drug court participants we computed the number of arraignment dates, person, sex, property, drug and other offenses, total incarcerations, total convictions, and a count of arrests, counts of person, sex, property, drug and other convictions prior and subsequent from the drug court entry date. We also calculated the date of first arrest, arraignment, and arraignment leading to incarceration after the drug court entry date. For non-participants we computed these measures using the probation entry date as their reference date.

Nearly all of the PCF numbers we requested from CHSB were found. We excluded any records from our analysis file that did not match to any CORI data. This reduced our analysis file from 519 to **512** drug court participants.

Massachusetts Department of Public Health (MDPH)

The Bureau of Substance Abuse Services maintains the Substance Abuse Management Information System (SAMIS) data on AOD treatment services delivered by State-licensed providers since 1991. Variables include treatment program admission and completion dates, treatment type, treatment modality, and referral source for each client episode.

Since MDPH does not maintain PCF number in their databases, we needed the CORI data to request treatment data from MDPH. We provided them with a number of identifiers found only on the CORI data – Social Security Number, name, gender, and date of birth. We also included a sequence number so we could easily match their data to our analysis file. MDPH performed the match between their database and the identifiers, and returned every treatment episode from 1990 to the fall of 2005.

From the MDPH data we computed the number of inpatient, outpatient, and total alcohol and drug treatment episodes prior to and subsequent from the drug court entry date for participants and a comparable date for matched probationers. For those records in our analysis file that did not match to an MDPH data we set the number of treatment episodes to zero.

²⁵ We sent a subsequent request to CHSB for the remaining drug court participants for Bristol that did not match to an OCP record. One hundred twenty-four of the 128 possible participant-entry combinations matched to CORI data.

Suffolk County Sheriff's Department (SCSD)

Inmate movement data are available for the House of Corrections (HOC) since 1998, and for the Nashua Street Jail since 2000. The jail houses pretrial detainees, whereas the HOC houses inmates who were convicted of crimes whose penalty requires serving relatively short periods of confinement. Offenders sentenced to longer periods of confinement, and offenders convicted of certain serious offenses, are housed in state prisons.

PCF numbers were not available on the SCSD data, so we matched our analysis file to the SCSD using a combination of Social Security Number, name, date of birth, and gender. We computed the number of admissions and amount of time served in both the HOC and jail prior to and one-year subsequent to the drug court entry date for participants and a comparable date for matched probationers. Similarly to the MDPH data, in those instances where we could not match our analysis file to the SCSD data, we assumed the number of admissions and time served were zero.

Final Analysis File

The final analysis file makes two other reductions for the 512 drug court participants with OCP and CORI data. First, we removed anyone whose drug court entry date was after their last probation exit date, or when only the drug court entry was available, when the drug court entry year was after their last probation exit year.²⁶ This reduced our 512 records to **432** drug court participants, because we needed the drug court entry date for the analysis. We then removed all of the drug court participants whose exact drug court entry date was missing. These were mainly participants from South Boston between 1998-2000. This further reduced the analysis sample to **398** drug court participants.

Table 5 (see main text) summarizes program status for 833 drug court participants by court. The program exit date is frequently missing, sometimes because the drug court participant is still in the program, and sometimes because the court records do not report the exit date. The table reports program status as of the date that these data were assembled.

We had to reduce the file from 398 to between 248 and 344 drug court participants to support the impact analysis. The reasons for this additional data attrition are discussed in the next appendix, but we can summarize here. About 53 of the 398 were lost for technical reasons specific to the use of the propensity score estimator. Most of the rest were lost because there was insufficient time during the follow-up period to assure at least six-months at risk of recidivism. Thus, the loss was largest when we defined the follow-up period as starting one year after the probationer entered drug court, and it was least when we defined the follow-up period to begin when the probationer entered drug court. The loss was largest when the impact measure came from the Suffolk County Sheriff's Department, which had the earliest cut-off date for data assembly, and it was least when the data came from Criminal History Systems Board.

²⁶ An offender must be on probation to enter drug court. Some offenders served probation sentences during period T1 and then a second probation sentence during period T2, during which they also entered drug court. The condition described in the text happened when we had the probation assessment form for the first probation sentence, but we lacked the probation assessment form for the second sentence, the one associated with drug court.

Appendix B

Analysis

The key step when performing evaluation research is to identify a *counterfactual*: evidence of how a treated group would have performed had its members not been treated. In the present case, the *treatment* is participation in drug court. The counterfactual is provided by the performance of similar probationers who did not participate in drug court. The evaluation rests on a comparison of the outcomes for drug court participants and other probationers.

For the counterfactual to be useful, and thus for the comparison to be useful, an evaluator has to compare drug court participants with other probationers who are identical in every way except for participation in drug court. When that is true, any difference between the average outcomes for drug court participants and other probationers must be attributable to participation in drug court.

Of course, no two individuals are every identically the same, and what researchers really do is select treated and comparison subjects so that they are observationally the same—that is, they are the same on variables such as age, gender, race, and criminal history. In the absence of a random experiment, wherein treated and controlled subjects would be randomly assigned to drug court or the control condition, researchers use statistical models to establish this observational equivalence. Our choice was to use matching based on propensity scores (Rosenbaum and Rubin, 1984; Smith, 1997).

To explain this approach, we began with electronic records for all probationers who entered probation between 1998 and 2003. Those records provided a probation officer's assessment of the probationer's risk to fail probation, as well as observations about specific risk factors, such as drug use. We matched those probation records with information about who participated in drug court. We then used all the data from the probation record to estimate the probability that a probationer would participate in drug court conditional on his or her risk factors and other variables reported by the probation officers. The resulting prediction is the preliminary *propensity score*. The higher the propensity score, the higher the probability that a probationer will participate in drug court; the lower the propensity, the lower that probability.

The propensity score is preliminary because we took one more step to get a final propensity score. Specifically, we excluded all observations for probationers who had propensity scores of 0.04 or lower, leaving us with probationers who have a material probability of participating in drug court. We then estimated the propensity score again, and that second estimate became the final propensity score, the one that we used in the subsequent analysis.

The reason for this second step is that data with very low propensity scores are essentially uninformative, so there is little cost in excluding it. On the other hand, we feel more confident in our regression specification when it is applied to a more restrictive set of data. Thus, the final propensity score should be a more accurate estimate of the probability of entering drug court over the range of data that are the most important for our analysis.

The cost of being more confident of avoid specification error in the propensity score is that we reduce the sample size. We subjected findings to sensitivity analysis by use less restrictive criteria for including a

drug court participant in the file and decided that concern with specification was valid. Less restrictive selection criteria provided a larger sample, but also, different results.

Once we had estimated the final propensity scores, we matched every drug court participant with up to three non-drug court probationers who had propensity scores nearest to the drug court participant.²⁷ We placed two side-constraints on those matches: First, a match was only allowed for probationers under the supervision of the same court. Second, a match was only allowed when the probationer had a propensity score within 0.05 of the propensity score for the drug court participant. Thus, the match sometimes involves fewer than three non-drug court probationers, and some drug court participants were excluded because they had no matches.

According to statistical theory, these matched sets of four probationers per set are observationally equivalent provided that all variables that affect the outcomes enter into the estimation of the propensity scores. We can estimate the treatment effect for any drug court participant as the difference between the outcome for that drug court participant and the average outcomes for the three comparison probationers. Then the average of this difference over all drug court participants is an unbiased estimate of the treatment effect. We based our evaluation findings on the estimate of that treatment effect.

Having lost cases because of concern about the specification of the propensity score and because of concerns about the validity of case matching for some drug court participants, we also lost cases because of the need for a follow-up period that lasted at least eighteen months after initiation of drug court.

Specifically, to assemble the analysis file, we took the following steps:

- We matched drug court records with probation records and used logistic regression to estimate the probability that a probationer would enter drug court.
- Using the above regression results, we eliminated all probationers who had a propensity score less than 0.04. We then used logistic regression to estimate the propensity score for the remaining probationers. This second propensity score was used in the rest of the analysis.
- Using the estimated propensity score, we matched every drug court participant with three probationers whose propensity scores were closest to that of the drug court participants. We disallowed any match when the matched propensity scores would have differed by 0.05 or more, so not all drug court participants had three matches. Matching led to matched sets of one drug court participants and typically three probationers.
- We examined the total available follow-up period for every member of the matched set. After identifying the set member whose follow-up period was the shortest, we truncated the follow-up periods for the other members of the set so that every member of the set had the same length of follow-up.
- We then identified two follow-up periods. One pertained to the period after the drug court participant entered drug court. Specifically, we computed T, the days from when the probationer

²⁷ Matching can be done in alternative ways (Becker and Ichino, 2002). We used “nearest neighbor” matching, which has the advantage of being straightforward to apply and explain. Researchers sometimes use “kernel matching.” This will provide a more efficient estimate (i.e., a more precise estimate) of treatment effectiveness, at the cost of considerable additional computing and complicated explanations. Note that we matched every drug court participant to three probationers. The additional probationers increase the efficiency of our estimates, but there is not much gain after a three-to-one match. Additional, more distant matches have propensity scores that increasingly differ from the propensity score for the drug court participant, so there is some question about introducing bias by expanding the number of comparison subjects.

entered probation and when that same probationer entered drug court. When T was negative, we set it equal to zero. For every member of the matched set, the follow-up period began T days after that member entered probation. The second follow-up period was constructed in the same way, except that we added 365 days to T, so the second follow-up period began one year after the drug court participant entered drug court.

- We used the first follow-up period when studying the receipt of treatment. We used the second follow-up period when studying recidivism and contacts with the justice system.

An advantage of the propensity score approach is that the analysis is simple compared with other approaches such as covariance models, instrumental variables and regression discontinuity designs. Let:

D_i This is the outcome variable for the i th person who entered drug court.

D_{ij} This is the outcome variable for the j th ($j=1, 2$ or 3) probationer who is matched with the i th drug court participant.

Then an unbiased estimate of the treatment effect for the i th drug court participant is:

$$[1] \quad \hat{\Delta}_i = D_i - \frac{1}{3} \sum_{j=1}^3 D_{ij}$$

This is just the difference between the observed outcome for the i th drug court participant and the average outcome for the three probationers who are matched with that drug court participant. When there were fewer than three matches, of course, the summation is over a number smaller than 3. An estimate for the average treatment effect is:

$$[2] \quad \hat{\Delta} = \frac{1}{N} \sum_{i=1}^N \hat{\Delta}_i$$

Here N is the total number of matched sets ($i=1 \dots N$). The standard error for this estimator is complicated because the estimated treatment effects do not come from the same distribution. In addition, we sought to test some additional hypotheses that could not be tested with [2] alone. Thus, we estimated a regression model:

$$[3] \quad \hat{\Delta}_i = \beta_0 + \beta_1 RISK_i + \beta_2 PROP_i + \beta_3 COURT_i + e_i$$

RISK Time at risk for the i^{th} matched set, based on time at risk for the drug court participant.

PROP The propensity score for the i th matched set, based on the propensity score for the drug court participant.

COURT Dummy variables representing the drug courts

e An error term, independent across matched sets, but possibly heteroscedastic.

Specification [3] allows us to test whether or not the size of the treatment effect varies with several factors. First, it seems possible that the treatment effect might vary with the time at risk. For example,

the effectiveness of drug courts might be small in the short run but larger in the long run. Or, possibly, the effectiveness of drug courts erodes as the period at risk increases. Parameter β_1 is an estimate how the treatment effect varies with the length of the follow-up period.

Second, it seems that drug court may be more effective for some types of offenders than for other types of offenders. It is complicated to test this hypothesis in full generality, but one test seems especially appropriate. The propensity score PROP is an estimate of the probability that a probationer will enter drug court. The parameter β_2 is an estimate of whether those offenders who are the most likely to be accepted into drug court perform better or worse than those offenders who are the least likely to be accepted into drug court.

Third, the introduction of dummy variables for jurisdictions affords a comparison of how the drug courts perform relative to each other. The small sample size precludes high power for this test.

We performed statistical testing of the error terms and discarded the null hypothesis of homoscedasticity. Consequently we used a robust (or sandwich) estimator for the covariance matrix. Call this covariance matrix V.

We estimated the treatment effect at the mean for all drug court participants. Then the average treatment effect is:

$$[4] \quad \hat{\Delta} = \begin{bmatrix} 1 & \bar{R} & \bar{P} & \bar{C}_1 & \bar{C}_2 & \bar{C}_3 \end{bmatrix} \begin{bmatrix} \hat{\beta}_0 \\ \hat{\beta}_1 \\ \hat{\beta}_2 \\ \hat{\beta}_3 \\ \hat{\beta}_4 \\ \beta_5 \end{bmatrix}$$

where \bar{R} through \bar{C}_3 represent the sample mean values for the variables that appear in [3]. The estimated sampling variance for [4] is:

$$[5] \quad \sigma_{\hat{\Delta}}^2 = \begin{bmatrix} 1 & \bar{R} & \bar{P} & \bar{C}_1 & \bar{C}_2 & \bar{C}_3 \end{bmatrix} \mathbf{V} \begin{bmatrix} 1 \\ \bar{R} \\ \bar{P} \\ \bar{C}_1 \\ \bar{C}_2 \\ \bar{C}_3 \end{bmatrix}$$

A suitable test statistic for the average treatment effect is:

$$[6] \quad \frac{\hat{\Delta}}{\sigma_{\hat{\Delta}}}$$

This has a standard normal asymptotic distribution. A conservative test is based on the t-distribution. Of course, tests of the null hypotheses that involve individual parameters (β_1 and β_2) are traditional tests that use the square-root of the diagonal of V as the standard error. We used an F-test to test the null hypothesis that the courts have equivalent outcomes.

We also report the summary statistic R^2 . The summary statistic is typically small. This is often the case in cross-section analysis, and furthermore, we warn the reader that this summary statistic requires careful interpretation. The dependent variable is the estimate treatment effect for the matched set, so the summary statistic simply estimates how much of the variance in that estimated treatment effect can be explained by time at risk, the probability of selection into the drug court program, and the jurisdiction. This has nothing to do with how much statistical modeling might explain differences in outcomes across probationers.

Findings

Before we could apply the estimator described by equations [3] and [4], we had to estimate the propensity score. Here we only discuss the second propensity score estimate, as the first estimate was simply used to prune the sample. We used a logistic regression where the outcome measure was a dummy variable that indicated that a probationer did or did not enter drug court.

Table 9 identifies variables that entered into the analysis. (See appendix C for a fuller description of these variables.) Table 10 identifies parameter estimates from the logistic regression. We focus attention on Table 10.

The first column of Table 9 identifies the independent variable that entered the logistic regression, and the second column provides a short description for each variable. The third column reports the parameter estimate, the fourth column reports the Chi-square statistic for the null hypothesis that the parameter is zero, and the fifth column reports the p-value for the Chi-square statistic.

It is difficult to provide a useful summation of how drug courts select participants by examining the parameter estimates and test statistics. We have added an additional column. The fifth column reports the marginal effect—or, roughly, how a unit change in a variable affects the probability of entering drug court. This is simply:

$$marginal = \bar{P}(1 - \bar{P})\alpha$$

where \bar{P} is the mean propensity to enter drug court (about 0.18 in the *matched* set) and α represents a generic parameter from column 3.

Table 9

Variables Entering the Propensity Score Estimation

Variable	Description	N	Mean	Std Dev	Minimum	Maximum
DC_FLAG	Any drug court participation	3689	0.097	0.296	0	1
Male	Male dummy variable	3689	0.807	0.395	0	1
age_assess	Age at assessment	3689	33.817	10.060	17.166	70.951
age1off_1719	Age at 1st offense was between 17 and 19 (16 or less is excluded)	3689	0.318	0.466	0	1
age1off_2023	Age at 1st offense was between 20 and 23 (16 or less is excluded)	3689	0.138	0.345	0	1
age1off_24plus	Age at 1st offense was 24 or more (16 or less is excluded)	3689	0.178	0.382	0	1
Brighton	Brighton court dummy variable (Roxbury is excluded)	3689	0.115	0.320	0	1
Chelsea	Chelsea court dummy variable (Roxbury is excluded)	3689	0.154	0.361	0	1
Sboston	South Boston court dummy variable (Roxbury is excluded)	3689	0.139	0.346	0	1
viol_	Count of person-related offenses before probation intake date	3689	7.679	11.464	0	63
sex_	Count of sex-related offenses before probation intake date	3689	0.156	0.670	0	5
prop_	Count of property-related offenses before probation intake date	3689	12.589	23.971	0	146
drug_	Count of drug-related offenses before probation intake date	3689	9.014	12.797	0	72
other_	Count of other types of offenses before probation intake date	3689	13.025	17.202	0	98
Totincar	Total number of incarcerations	3689	2.661	4.084	0	38
Totconv	Total number of convictions	3689	24.448	35.426	0	380
num_hoc_b	Number of Suffolk County HOC incarcerations before	3689	0.444	0.939	0	5
b_treat	Any AOD treatment episodes before probation intake	3689	0.662	0.473	0	1
b_inpat	Any inpatient treatment episodes before probation intake	3689	0.565	0.496	0	1
b_outpat	Any outpatient treatment episodes before probation intake	3689	0.659	0.474	0	1
y1998	Dummy variable for 1998	3689	0.063	0.243	0	1
y1999	Dummy variable for 1999	3689	0.135	0.341	0	1
y2000	Dummy variable for 2000	3689	0.175	0.380	0	1
y2001	Dummy variable for 2001	3689	0.203	0.403	0	1
y2002	Dummy variable for 2002	3689	0.180	0.384	0	1
P_SCORE	Total initial probation risk score	3689	6.601	4.461	0	24
P_ALCNEED	Initial probation risk assessment: Alcohol-related need	3689	0.620	0.485	0	1
P_DRUGNEED	Initial probation risk assessment: Drug use-related need	3689	0.958	0.201	0	1

Table 9

Variables Entering the Propensity Score Estimation

Variable	Description	N	Mean	Std Dev	Minimum	Maximum
P_EDUCNEED	Initial probation risk assessment: Education need	3689	0.428	0.495	0	1
P_EMPNEED	Initial probation risk assessment: Employment need	3689	0.689	0.463	0	1
P_FAMNEED	Initial probation risk assessment: Marital/family need	3689	0.571	0.495	0	1
P_SOCNEED	Initial probation risk assessment: Social need	3689	0.805	0.396	0	1
P_COUNNEED	Initial probation risk assessment: Counseling need	3689	0.908	0.289	0	1
P_FINNEED	Initial probation risk assessment: Financial management need	3689	0.754	0.431	0	1

Table 10**Propensity Score Analysis: Entering Drug Court**

Variable	Description	Estimate	Std Error	Chi-Square	p-value	Marginal Effect
Intercept	Intercept	-4.867	0.678	51.610	<.0001	
Male	Male dummy variable	-0.327	0.153	4.560	0.033	-0.048
age_assess	Age at assessment	0.010	0.008	1.440	0.230	0.001
age1off_1719	Age at 1st offense was between 17 and 19 (16 or less is excluded)	-0.180	0.153	1.370	0.241	-0.026
age1off_2023	Age at 1st offense was between 20 and 23 (16 or less is excluded)	-0.164	0.200	0.670	0.412	-0.024
age1off_24plus	Age at 1st offense was 24 or more (16 or less is excluded)	-0.327	0.223	2.140	0.143	-0.048
Brighton	Brighton court dummy variable (Roxbury is excluded)	1.483	0.204	52.870	<.0001	0.219
Chelsea	Chelsea court dummy variable (Roxbury is excluded)	0.019	0.183	0.010	0.917	0.003
Sboston	South Boston court dummy variable (Roxbury is excluded)	-0.965	0.207	21.690	<.0001	-0.142
viol_	Count of person-related offenses before probation intake date	-0.023	0.007	11.370	0.001	-0.003
sex_	Count of sex-related offenses before probation intake date	-0.127	0.095	1.780	0.182	-0.019
prop_	Count of property-related offenses before probation intake date	-0.002	0.005	0.120	0.734	0.000
drug_	Count of drug-related offenses before probation intake date	0.016	0.006	7.820	0.005	0.002
other_	Count of other types of offenses before probation intake date	-0.015	0.006	6.490	0.011	-0.002
Totincar	Total number of incarcerations	0.008	0.022	0.140	0.708	0.001
Totconv	Total number of convictions	0.011	0.005	4.360	0.037	0.002
num_hoc_b	Number of Suffolk County HOC incarcerations before	0.258	0.054	22.420	<.0001	0.038
b_treat	Any AOD treatment episodes before probation intake	0.386	0.282	1.880	0.171	0.057
b_inpat	Any inpatient treatment episodes before probation intake	0.543	0.219	6.160	0.013	0.080
b_outpat	Any outpatient treatment episodes before probation intake	0.444	0.196	5.130	0.024	0.066
y1998	Dummy variable for 1998	-1.133	0.341	11.060	0.001	-0.167
y1999	Dummy variable for 1999	-0.369	0.214	2.960	0.085	-0.054
y2000	Dummy variable for 2000	0.091	0.186	0.240	0.625	0.013
y2001	Dummy variable for 2001	0.321	0.171	3.530	0.060	0.047
y2002	Dummy variable for 2002	-0.297	0.185	2.570	0.109	-0.044
P_SCORE	Total initial probation risk score	-0.080	0.020	15.490	<.0001	-0.012
P_ALCNEED	Initial probation risk assessment: Alcohol-related need	0.277	0.130	4.510	0.034	0.041

Table 10**Propensity Score Analysis: Entering Drug Court**

Variable	Description	Estimate	Std Error	Chi-Square	p-value	Marginal Effect
P_DRUGNEED	Initial probation risk assessment: Drug use-related need	1.454	0.469	9.590	0.002	0.215
P_EDUCNEED	Initial probation risk assessment: Education need	0.065	0.128	0.260	0.613	0.010
P_EMPNEED	Initial probation risk assessment: Employment need	-0.060	0.158	0.140	0.705	-0.009
P_FAMNEED	Initial probation risk assessment: Marital/family need	-0.479	0.149	10.410	0.001	-0.071
P_SOCNEED	Initial probation risk assessment: Social need	0.136	0.173	0.620	0.432	0.020
P_COUNNEED	Initial probation risk assessment: Counseling need	0.612	0.242	6.400	0.011	0.090
P_FINNEED	Initial probation risk assessment: Financial management need	0.046	0.176	0.070	0.796	0.007

The main text identified several outcome measures. Formula [3] identified the specification for regressions that we applied to these outcome measures. Formula [4] explained how we used the results from the regression analysis to estimate the average treatment effect; formula [5] explained the formula for the standard error for this estimated treatment effect; and formula [6] presented the test statistics. Table 11 reports results.

The table identifies the outcome measure starting with Arrest and continuing through the number of Days until First Arrest. For each outcome measure, the tables report two regressions: The first excludes drug court identification and the second includes drug court identification. The two regressions provide the basis for an F-test of the null hypothesis that the treatment effect does not vary across the four drug courts. Results from the F-test are reported in the last two columns. The tables also report the summary statistic: adjusted-R².

As described in the main text, we also estimated the average treatment effect for drug court graduates. Results are reported in Table 12 and are summarized in the main text.

Table 11**Regression Analysis of Outcome Measures**

	Regressions without Courts		Regressions with Courts		F-Test With/without Courts	p-value of F-Test
	Estimate	z score	Estimate	z score		
Arrest						
Intercept	-0.182	-2.266	-0.191	-2.183		
Years at Risk	0.000	0.350	0.000	0.440		
Propensity Score	0.505	2.116	0.341	1.437		
Brighton			0.213	2.386		
Chelsea			-0.027	-0.300		
South Boston			0.006	0.050		
R-Square	0.018		0.038		2.080	0.103
Incarceration						
	Estimate	z score	Estimate	z score		
Intercept	-0.143	-2.422	-0.151	-2.415		
Years at Risk	0.000	-0.406	0.000	-0.220		
Propensity Score	0.626	3.041	0.532	2.501		
Brighton			0.131	1.785		
Chelsea			-0.082	-1.323		
South Boston			0.056	0.702		
R-Square	0.045		0.064		2.050	0.107

Table 11, continued

Regression Analysis of Outcome Measures

	Regressions without Courts		Regressions with Courts		F-Test With/without Courts	p-value of F-Test
Any Treatment after Probation Date						
	Estimate	z score	Estimate	z score		
Intercept	0.350	4.386	0.346	4.023		
Years at Risk	0.000	-2.326	0.000	-2.397		
Propensity Score	-0.136	-0.720	-0.144	-0.714		
Brighton			0.010	0.115		
Chelsea			0.117	1.260		
South Boston			-0.060	-0.600		
R-Square	0.016		0.023		0.789	0.501
Number of Treatment Episodes after Probation Date						
	Estimate	z score	Estimate	z score		
Intercept	0.358	0.547	0.378	0.559		
Years at Risk	0.000	0.216	0.000	0.140		
Propensity Score	0.754	0.441	0.491	0.253		
Brighton			0.237	0.333		
Chelsea			0.578	1.087		
South Boston			-0.501	-0.817		
R-Square	0.001		0.004		0.399	0.754

Table 11, continued

Regression Analysis of Outcome Measures

	Regressions without Courts		Regressions with Courts		F-Test With/without Courts	p-value of F-Test
Number of Days in Custody in Suffolk County						
	Estimate	z score	Estimate	z score		
Intercept	-14.385	-1.199	-22.777	-1.517		
Years at Risk	0.029	1.187	0.032	1.257		
Propensity Score	102.678	2.378	91.238	1.861		
Brighton			23.886	0.890		
Chelsea			40.195	1.677		
South Boston			-18.340	-0.629		
	0.019		0.036		1.490	0.218
R-Square						
Number of Days in Suffolk County HOC						
	Estimate	z score	Estimate	z score		
Intercept	Estimate	t-stat	Estimate	t-stat		
Years at Risk	-12.556	-1.197	-21.563	-1.620		
Propensity Score	0.029	1.322	0.032	1.403		
Brighton	83.772	2.129	77.832	1.713		
Chelsea			18.589	0.726		
South Boston			40.594	1.737		
R-Square			-11.747	-0.458		

Table 11, continued

Regression Analysis of Outcome Measures

	Regressions without Courts		Regressions with Courts		F-Test With/without Courts	p-value of F-Test
Number of Days in Nashua Street Jail						
	Estimate	z score	Estimate	z score		
Intercept	-1.903	-0.623	-1.427	-0.401		
Years at Risk	-0.001	-0.334	-0.002	-0.367		
Propensity Score	20.063	1.364	14.595	0.966		
Brighton			5.503	1.267		
Chelsea			-0.049	-0.019		
South Boston			-6.195	-1.091		
R-Square	0.015		0.024		0.811	0.489
Any Inpatient Treatment after Probation Date						
	Estimate	z score	Estimate	z score		
Intercept	0.262	3.311	0.269	3.143		
Years at Risk	0.000	-0.809	0.000	-0.894		
Propensity Score	-0.234	-1.155	-0.246	-1.158		
Brighton			0.002	0.025		
Chelsea			0.052	0.575		
South Boston			-0.056	-0.557		
R-Square	0.006		0.008		0.244	0.865

Table 11, continued

Regression Analysis of Outcome Measures

	Regressions without Courts		Regressions with Courts		F-Test With/without Courts	p-value of F-Test
Any Outpatient Treatment after Probation Date						
	Estimate	z score	Estimate	z score		
Intercept	0.328	3.979	0.322	3.551		
Years at Risk	0.000	-2.125	0.000	-2.157		
Propensity Score	0.283	1.424	0.290	1.363		
Brighton			-0.003	-0.040		
Chelsea			0.114	1.479		
South Boston			-0.047	-0.420		
R-Square	0.017		0.022		0.668	0.572
Any Suspensions/Revocations after Probation Date						
	Estimate	z score	Estimate	z score		
Intercept	-0.001	-0.028	-0.016	-0.303		
Years at Risk	0.000	0.196	0.000	0.242		
Propensity Score	-0.070	-0.641	-0.020	-0.179		
Brighton			-0.034	-0.801		
Chelsea			0.051	0.936		
South Boston			0.037	0.552		
R-Square	0.001		0.007		0.542	0.654

Table 11, continued

Regression Analysis of Outcome Measures

	Regressions without Courts		Regressions with Courts		F-Test With/without Courts	p-value of F-Test
Number of Suspensions/Revocations after Probation Date						
	Estimate	z score	Estimate	z score		
Intercept	-0.184	-0.687	-0.103	-0.274		
Years at Risk	0.000	-0.096	0.000	-0.214		
Propensity Score	0.420	0.822	0.284	0.558		
Brighton			0.000	0.001		
Chelsea			0.241	1.332		
South Boston			-0.494	-0.766		
R-Square	0.002		0.013		1.105	0.347
Number of Total Incarcerations after Probation Date						
	Estimate	z score	Estimate	z score		
Intercept	-0.130	-1.132	-0.150	-1.266		
Years at Risk	0.000	-1.130	0.000	-1.016		
Propensity Score	0.712	1.806	0.617	1.429		
Brighton			0.152	1.353		
Chelsea			-0.032	-0.392		
South Boston			0.072	0.762		
R-Square	0.030		0.038		0.791	0.499

Table 11, continued

Regression Analysis of Outcome Measures

	Regressions without Courts		Regressions with Courts		F-Test With/without Courts	p-value of F-Test
Number of Jail Admissions after Probation Date						
	Estimate	z score	Estimate	z score		
Intercept	-0.569	-2.391	-0.596	-2.348		
Years at Risk	0.000	0.312	0.000	0.441		
Propensity Score	2.043	2.441	1.575	1.829		
Brighton			0.619	2.410		
Chelsea			-0.171	-0.696		
South Boston			0.086	0.371		
R-Square	0.035		0.057		2.364	0.071
Number of Convictions after Probation Date						
	Estimate	z score	Estimate	z score		
Intercept	-0.644	-0.827	-0.612	-0.683		
Years at Risk	-0.001	-0.712	-0.001	-0.708		
Propensity Score	1.941	0.698	0.637	0.216		
Brighton			1.477	2.296		
Chelsea			0.343	0.631		
South Boston			-0.690	-0.755		
R-Square	0.009		0.031		2.272	0.080

Table 11, continued

Regression Analysis of Outcome Measures

	Regressions without Courts		Regressions with Courts		F-Test With/without Courts	p-value of F-Test
Days until First Arrest	Estimate	z score	Estimate	z score		
Intercept	-12.452	-0.200	-12.605	-0.189		
Years at Risk	0.205	2.138	0.201	2.073		
Propensity Score	-286.632	-1.884	-213.553	-1.469		
Brighton			-88.506	-1.933		
Chelsea			28.637	0.827		
South Boston			2.001	0.055		
R-Square	0.073		0.088		1.701	0.167

Table 12**Estimated Treatment Effects, Standard Errors and Tests of the Null Hypothesis****Estimated Treatment Effects**

Outcome Measure	n_obs	estimate	stderr	t	p_value
Follow-up from start of drug court					
Arrest	344	-0.078	0.030	-2.584	0.010
Incarceration	344	-0.037	0.025	-1.490	0.137
Any Treatment after Probation Date	341	0.164	0.029	5.701	0.000
Number of Treatment Episodes after Probation Date	341	0.620	0.239	2.589	0.010
Number of Days in Custody in Suffolk County	341	23.674	10.002	2.367	0.019
Number of Days in Suffolk County HOC	341	16.069	7.786	2.064	0.040
Number of Days in Nashua Street Jail	341	6.660	4.642	1.435	0.152
Any Inpatient Treatment after Probation Date	341	0.165	0.029	5.679	0.000
Any Outpatient Treatment after Probation Date	341	0.230	0.030	7.733	0.000
Any Suspensions/Revocations after Probation Date	344	0.017	0.020	0.857	0.392
Number of Suspensions/Revocations after Probation Date	344	-0.080	0.092	-0.877	0.381
Number of Total Incarcerations after Probation Date	344	-0.124	0.044	-2.814	0.005
Number of Jail Admissions after Probation Date	344	-0.130	0.090	-1.451	0.148
Number of Convictions after Probation Date	344	-0.765	0.265	-2.882	0.004
Days until First Arrest	344	70.881	21.115	3.357	0.001
Follow-up from start of drug court plus 1 year					
Arrest	303	-0.067	0.031	-2.168	0.031
Incarceration	303	-0.047	0.024	-1.985	0.048
Any Treatment after Probation Date	285	0.061	0.030	2.041	0.042
Number of Treatment Episodes after Probation Date	285	0.168	0.169	1.000	0.318
Number of Days in Custody in Suffolk County	248	19.085	7.627	2.502	0.013
Number of Days in Suffolk County HOC	248	17.682	7.046	2.510	0.013
Number of Days in Nashua Street Jail	248	1.175	1.546	0.760	0.448
Any Inpatient Treatment after Probation Date	285	0.015	0.029	0.525	0.600
Any Outpatient Treatment after Probation Date	285	0.144	0.030	4.799	0.000

Table 12**Estimated Treatment Effects, Standard Errors and Tests of the Null Hypothesis**

Estimated Treatment Effects

Outcome Measure	n_obs	estimate	stderr	t	p_value
Any Suspensions/Revocations after Probation Date	303	-0.006	0.019	-0.294	0.769
Number of Suspensions/Revocations after Probation Date	303	-0.138	0.089	-1.551	0.122
Number of Total Incarcerations after Probation Date	303	-0.108	0.038	-2.843	0.005
Number of Jail Admissions after Probation Date	303	-0.136	0.088	-1.549	0.123
Number of Convictions after Probation Date	303	-0.753	0.218	-3.451	0.001
Days until First Arrest	303	54.344	15.121	3.594	0.000

APPENDIX C: ADULT RISK/NEED OFFENDER ASSESSMENT

ADULT RISK/NEED OFFENDER ASSESSMENT

Name _____ PCF # _____
(First) (Last) (Middle)

D.O.B. ____/____/____ S.S.# ____/____/____ Sex _____ CT# _____

Probation From ____/____/____ to ____/____/____

Offense(s) #1 _____ #2 _____

Offender Ethnicity: White Black Hispanic Asian/Pacific Islander
 Cape Verdean American Indian

OFFENDER RISK CATEGORIES	INITIAL	FOUR MONTH	TWELVE MONTH	EIGHTEEN MONTH
1. PRIOR RECORD DURING THE PAST 5 YEARS 0 = three or more 1 = two 2 = one 4 = none				
2. PRIOR PERIODS OF PROBATION SUPERVISION DURING THE PAST 5 YEARS 0 = two or more 1 = one 4 = none				
3. AGE AT FIRST OFFENSE 0 = 16 or younger 1 = 17-19 2 = 20-23 3 = 24 or older				
4. RESIDENCE CHANGES DURING THE PAST 12 MONTHS 0 = two or more 2 = one 3 = none				
5. EMPLOYMENT DURING THE PAST 12 MONTHS 0 = three months or less 1 = four-eight months 2 = nine months or more				
6. FAMILY STRUCTURE 0 = offender has few or negative family ties 3 = offender maintains strong positive family ties				
7. SUBSTANCE ABUSE 0 = abuse leading to disruption of functioning 1 = presently in treatment or prior problem 3 = no apparent problem				
8. ATTITUDE 0 = unresponsive/uncommitted to suggested supervision plan 3 = responsive/committed to suggested supervision plan				
TOTAL RISK SCORE				

Initial Assessment			Four Month			Twelve Month			Eighteen Month		
Date:	By:		Date:	By:		Date:	By:		Date:	By:	
Max.	Mod.	Min.	Max.	Mod.	Min.	Max.	Mod.	Min.	Max.	Mod.	Min.

OFFENDER NEED CATEGORIES	INITIAL	FOUR MONTH	TWELVE MONTH	EIGHTEEN MONTH	SUPERVISION LEVEL
Educational					Maximum = 0 - 12 Moderate = 13 - 20 Minimum = 21 - 27
Employment					
Verbal/Family					
Social					OFFENDER NEED KEY
Alcohol					Problem = P
Drug Use					No Problem = NP
Counseling					
Financial Management					

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