

“But Does It Work?”

**2006 Evaluation of the 61st District Court
Drug Court**

Grand Rapids, Michigan

**Josef R. Soper, Ph.D.
Court Administrator**

**Opinions or points of view expressed are those of the author
and do not necessarily reflect the official position or policies of
the 61st District Court or the U.S. Department of Justice.**

Introduction

Even if drug courts are monumental failures – *which they are not* – the demand for non-incarceration alternatives will not soon abate. We cannot build our way out of the problem by simply incarcerating more people or legalizing drugs. Despite what has been written about the spectacular decline in crime in the United States (more about this below), the number of inmates in local jails rose 76.2% between 1990 and 2004. In sheer numbers that means an increase from 405,320 in 1990 to 713,990 in 2004. Drug enforcement at the federal, state, and local levels has everything to do with this increase. This went unnoticed at first because we were all focused on a steadily declining crime rate and jail construction kept pace with increasing demand. The total inmates in 1990 represented 104% of national capacity, while the 2004 total represented only 94% of our total capacity. What about the offender supply-side of this equation?

According to FBI arrest statistics, there is never likely to be a shortage of drug offenders in the United States. In 2002 there were 13,741,438 total arrests (not offenses known to the police, but actual arrests) and 11% (1,538,813) were for “Drug Abuse Violations” (excluding alcohol-related offenses). These numbers do not include non-drug offenses for which defendants with drug abuse problems were charged. Consider, for example, data provided by the Bureau of Justice Statistics: Over two-thirds (68.7%) of jail inmates nationwide used drugs at least once a week for at least a month; over half (52.6%) in the month before the arrest that put them in jail most recently; and, over a quarter (28%) reported doing drugs at the time of the offense. If crime has declined significantly in the United States, why is additional capacity needed for drug-related offenders?

By the end of 1995, 9,078,273 individuals had been charged with offenses ranging from curfew violations to first degree murder (more of the former than the latter, fortunately) and by 2004 the total dropped to 8,607,067 or a 5.2% decline. Not the spectacular decline we read about. Unfortunately, the media focuses only Part I offenses (Murder, Forcible Rape, Robbery, Aggravated Assault, Burglary, Larceny, Motor Vehicle Theft, and Arson) because they generate the most fear. During the same time period, these offenses declined by 16.7% for violent crimes and 22.8% for property crimes. This is a reduction worthy of headlines. The Part II offense category, which includes all of the drug-related crimes, must have behaved differently in order to offset the large decreases in Part I offenses. During the

same time period, drug abuse violations *increased by 25.8%* for individuals over 18 years of age; only vagrancy increased by a higher rate (37.5%). It is equally important to note that in 1995 drug offenses comprised 9.7% of total offenses charged; however, by 2004 the proportion had risen to 12.5%. Is this trend a reflection of how we feel about drug-related crimes?

Regardless of the vision promoted by Dr. Timothy Leary in the 1960s (“Turn on, Tune In, Drop Out”), as a nation we are not eager to embrace the widespread use of illegal drugs nor do a majority favor making recreational use of illegal substances legal. Marijuana has always been the most likely drug (currently illegal in most places) to be legalized. According to a Gallup poll, 84% were opposed to legalization in 1969 and by 2003 that opposition had dropped to 64% (only half the sample was available to be asked in 2003). These findings are consistent with a similar survey conducted by the U.S. Department of Health that also found some very real differences based on frequency of use and type of drug. For example, even marijuana is considered harmful by 52% of the sample if smoked once or twice a week. The percentages climb very steeply when the drug is cocaine or heroin (89% and 94%, respectively). The bottom line is that we still agree, more or less, that the use of traditionally illegal drugs is a bad idea. What about justice system professionals?

In a recent national survey of police chiefs, sponsored by the Police Foundation, drugs were still considered the single most important enforcement problem: 63% rated drug abuse as extremely or quite serious in their communities. At the same time, 67% believe that law enforcement has been unsuccessful in reducing the problem. On a positive note, 67% had very little confidence in mandatory minimum sentences as an answer. There are data to suggest that crime has become a less serious problem over time and that the justice system may be doing something right. For example, a Gallup poll revealed that when asked whether there was more or less crime than the year before, 89% of those who responded in 1989 said there was more (84%) or the same (5%). This comparison reached its nadir in 2001 (41% and 10%, respectively) and in 2002 began to climb until, in 2005, the percentages are 67% and 9%, respectively. What about the rest of the system?

The Bureau of Justice Statistics analyzed conviction data for 2002 and determined that at conviction 39% of felony drug offenders are sentenced to prison, 27% to local jails, and 34% to probation. Not all offenses were

included in the analysis, but rather the very serious violent and property offenses, drug possession and trafficking, weapons offenses, and, oddly enough, “Other” which included non-violent offenses such as vandalism. Vandalism convictions were much more likely to go to jail (35%) than felony drug offenders and only slight less likely to go to prison (35%) or get probation (30%). Historically, drug offenders have been a small percentage of inmate statistics. In 1983, for example, drug-related offenses were the most serious offense for only 9.3% of all jail inmates. By 2002 this had climbed to 30.2% - a three-fold increase in less than twenty years. During the same period virtually all other reported offenses had declined with the exception of assault which increased slightly from 8.6% to 12.7%, due possibly to a public demand for an official response to domestic assault. Re-arrest of drug offenders released in 1983 and 1994 for the same offense was 50.4% and 66.7% respectively. The reconviction rate for the same offense was 35.3% and 47.0%, respectively. Drug offenders had the lowest offense discrepancy between re-arrest and reconviction of the other offense groups. This suggests that if they are re-arrested for the same offense they are more likely to be re-convicted of the same offense. Drug offenders apparently specialize in one group of offenses compared to other offenders. If the problem is still serious and if locking offenders up (repeatedly) is not the answer, what should we be doing?

In 1972, 61% of respondents to a Gallup Poll indicated that they believed little progress had been made in coping with the problem of illegal drugs. Thirty years later, 60% said the same thing. In between there were times that the percentage shifted up and down but never below 52%. A Pew Research Center poll, focusing on attitudes toward the justice system, discovered that in 2002, 69% of the respondents agreed that the justice system should try to rehabilitate criminals and, in 2003, this percentage increased to 72%. We are left to assume that the balance of the respondents favored incarceration.

That brings us to the evaluation of a relatively small drug court in Grand Rapids, Michigan. All of the forgoing material serves to underscore the increased demand for effective and less expensive alternatives. If the past is prologue, the supply of drug offenders will not soon decline nor will the official response to drug addiction and all the damage that follows in its wake.

On September 1, 1989, the 11th Judicial Circuit Court, serving Miami, Florida, launched what would quickly become a national phenomenon: Drug Court. Since then an estimated 2,000 such specialized courts have sprung up around the country. Claims of success range from spectacular to truly extraordinary and some liken the effect to the discovery of penicillin. Hyperbole is not in short supply. The intent here is modest: “Does this drug court, in Grand Rapids, Michigan, work?”

Program evaluation has many purposes beginning with pragmatic: “Is the program being properly run?” to complex and, perhaps, unanswerable: “Has society benefited from an individual’s participation?” If the program is operated according to the original design, based on theory or empirical observation, the results must still be statistically significant before we can claim success and urge others to follow our lead. Since we are not always certain why (or how) an intervention actually works, care must be taken to avoid mistakes made by other well-intentioned interventions such as “Scared Straight” (Petrosino, et al, 2000).

In the case of drug courts the goal is to improve the lives of specially selected participants. While no one disagrees with this overall goal, it is a bit complicated to measure precisely. The first immediate success is to have a defendant complete the program according to the established performance rules. The underlying assumption is that when the course of treatment is faithfully followed, the defendant will not return to drug use. After this the results become a bit more difficult to identify and explain. For example, does the defendant then automatically become a more responsible member of society? Perhaps we should be satisfied if the defendant does not return to the justice system. The theory is that if people are able to break the drug/crime use cycle, they will have an opportunity to improve their lives. The data presented here cannot delve deeply into post program quality of life analysis that might answer that question.

The analysis presented here may be a bit more exhaustive than might normally be the case because the existing drug court literature can benefit from studies of actual drug court participants. Of additional interest beyond the program’s effectiveness is whether, compared to incarceration, the program is worth the cost. Because this is such a tremendously complicated issue, a separate study will attempt to sort out the facts from the hyperbole.

Literature Review

Despite the fact that drug courts have been part of the *therapeutic justice* approach since 1989, the published research was initially a mixture of quasi-experimental and anecdotal studies. More methodologically sophisticated studies are being published more regularly, some in refereed journals. There are, however, still no drug court programs that randomly select and assign program participants. Even if there were, the original selection point (detection and arrest by law enforcement) is not random. So we begin the analysis of drug courts with the understanding that randomness, the traditional foundation of scientific method, is absent.

Except where needed to assist the discussion there will be only limited recitation of current drug court literature. If the reader is interested, a good place to start is the Drug Court Publications Resource Guide, 4th Edition, National Drug Court Institute, 2002. One of the repeated references is a recently published nationwide recidivism study of drug court graduates (Roman, Townsend, and Bhati, 2003). Briefly, the authors undertook to evaluate recidivism using a very large sample of operational drug courts. Over 2,000 graduates from ninety-five separate programs were included in the analysis. Selected results will be compared to similar data analyzed for this study.

Every drug court is unique. Even courts in neighboring communities will be different enough to require different definitions of something as simple as “success”. When created, each drug court reflects the unique population it serves and the beliefs of justice system professionals responsible for making decisions. For this reason alone, comparing results between courts may be a fool’s errand. Even if a statewide data collection process were created, each court will enter the same data elements but attach different meanings to the data. If the program works for the community and the local justice system, it should perhaps be left alone.

One additional issue that must be considered by all drug courts, especially those that are maturing, is whether the inevitable changes in personnel alter the program philosophy by changing subtly its daily operation. Judges and prosecutors retire or lose re-election bids; court employees retire, are promoted or leave for other reasons; and the population under consideration changes over time. Fortunately for this evaluation,

program continuity is maintained because all of the original personnel are still with the program.

The Current Program

The 61st District Drug Court Program opened for business in late 1999. Eligibility criteria and areas of program emphasis were designed utilizing a treatment-oriented approach that includes community substance abuse treatment agencies in coordination with probation case managers, surveillance officers and one drug court judge. In order to be considered for admission to Drug Court, defendants must meet the following criteria:

1. Must be a resident of Kent County;
2. Must be charged with a non-violent offense;
3. Have an identified substance abuse problem;
4. Have no more than two prior felony convictions;
5. Have no history of violent offenses, weapons offenses, or arrests related to weapons or violent offenses;
6. Agree to participate in the program;
7. Must admit responsibility for substance abuse problems; and
8. Must be at least seventeen years of age.

While this protocol appears fairly specific, it should be noted that there is always the interaction of factors such as law enforcement and prosecutorial discretion; defense attorney recommendations to his/her client; assessment by gatekeepers, defendant motivation; and everything that happens to the defendant in her/his world away from Drug Court. It is impossible to control intervening influences in settings as complex as the justice system.

Drug Court emphasizes using urine-based drug testing to accurately assess the level of illegal substance use. Graduated sanctions, adjudication, and post-judgment hearings are held immediately after violations come to the attention of case managers. The goal is to deliver sanctions as quickly as possible. Additional program emphasis is placed on obtaining and maintaining employment as well as acquiring skill training. Program participants are intensively supervised by case managers whose focus is only Drug Court. One District Court judge supervises the program in order to provide continuity to the process. This study did not collect data about the delivery of specific program services.

By agreement with the Kent County prosecutor, potential participants must plead guilty as charged before entering Drug Court. If they graduate, the original charges are completely dismissed by the prosecutor. At graduation the successful defendant is presented with a framed copy of the order dismissing the charges. Thereafter the record is expunged from local police records, state police computerized criminal histories, and 61st District Court records. The “carrot” in this program is having the original drug charge permanently erased. Failure results in the prosecutor acting on the original guilty plea.

The 61st District Drug Court officially graduated its first participant June 20, 2000. Since then 123 have graduated and 33 are currently actively enrolled. For general comparison purposes, the drug courts studied by Roman, et al (2003) were all operational before the 61st Drug Court; almost 40% were operational before 1995. The oldest is the first program in Miami, Florida founded September 1, 1989 and graduating over 6,000 defendants as of July 2001. Based on the volume of graduates, the 61st District Drug Court, with 125 graduates to date, would fit into the lower third of the courts studied by Roman (2003).

Data Collection

Four separate groups were identified for this study:

1. **Graduates:** All defendants who successfully graduated from Drug Court as of April 2005 (N = 123).

2. **Terminated:** All defendants admitted to Drug Court but for various reasons failed to graduate (N = 224).

3. **Offered:** All defendants who initially met entry criteria, were offered the opportunity, but decline to participate (N = 139). This cohort provides a basis for analyzing the future of defendants who were eligible but took another path. In a very loose sense they qualify, along with the next group, as controls.

4. **Bound Over:** This is a randomly selected group of defendants who were charged with the same drug-related offenses as those in the other three groups, however, they did not meet the threshold criteria for entry into the program and were instead bound over to the general trial court for

disposition (N = 280). In order to find this group, analysis was conducted to identify the most frequent offense charged (possession of controlled substance < 25 grams). During the study period there were 1,184 convictions for this specific offense. This group was selected based on having no contact with Drug Court and being bound over to the general trial court solely for the original drug offense.

Each of the four groups was further subdivided to create subgroups based on defendant case years. The inclusive case years for the three drug court-related groups were 1999, 2000, 2001, 2002, 2003, and 2004. Due to a small number of cases in 2004, it was combined with 2003. The only other data issue was that there were no identifiable 1999 Offered cases. In the first three groups virtually all cases were kept in the study. The fourth group was selected by random selection from a list of all cases arranged in case number order.

Offense data were collected for three distinct time periods: *prior*, *during*, and *post*. The *prior* time period began with the first conviction of anyone in the study, regardless of group and ended in late 1999 when Drug Court began accepting participants. The second time phase was limited to those who participated in Drug Court and covered only convictions recorded while they were in the program. The *post* time period began when a person either left the program (Graduated or Terminated) or with the next conviction following the one that resulted in their being included in the Bound Over group. The Offered group *post* phase began when they refused to participate in Drug Court.

The time periods create three small bias issues. First, older subjects criminal careers may be overrepresented in the *prior* conviction data depending on their age at career onset. That bias may affect some subjects because while Drug Court makes no distinction based on age, the longer a person engages in criminal behavior the more likely they are to be convicted for more crimes and eventually get charged with a felony. This record may in turn cause them to be rejected for consideration as Drug Court participants and may make the prosecutor pass them over for someone whose rap sheet is shorter. It may also cause them to reject the idea of changing their behavior now. Second, only two of the cohort groups have a defined *during* phase and that extends for the duration of their participation, regardless of the outcome. The other two cohort groups have no interim period. The Offered cohort *post* convictions begin to accrue immediately

after the offense for which they declined to participate. Third, the post period is, by definition, much shorter than the *prior* time period. For some subjects, the *prior* time period might extend fifteen twenty years into the past. The *post* evaluation time period is limited to six years.

Data collection for all subjects was conducted by District Court employees who are trained and certified to conduct automated criminal history inquiries via the Michigan State Police Law Enforcement Information Network (LEIN) and accurately interpret the resulting information. Positive identification was based on the defendant's SID (State Identification Number) and FBI identification number. For those cases where there was some confusion or uncertainty about whether the correct record had been found (most of them in the Bound Over group), the case was dropped and replaced with the next name on the list. This helped maintain the integrity of the random selection process.

Unlike the national study, all convictions from traffic to violent felonies were included in the offense data. Because all offenders had been processed through the 61st District Court during the Drug Court time frame, it was possible to include all local offenses. This creates a bias because minor offenses such as traffic tickets and minor misdemeanors are not recorded by the Michigan State Police or the FBI, which means that if those offenses are committed in another jurisdiction, they could not be collected by 61st District Court employees. It is, however, reasonable to assume that such a bias was operating before, during, and after program participation. It is likely that some minor offenses were committed, outside the jurisdiction, during program participation that never came to the attention of Drug Court personnel. Whether the evaluation attempts to account for all possible offenses that might be committed by participants makes a large difference in the outcome analysis (read: Recidivism).

Participant privacy was paramount because for the Graduated subjects the record of the offense that put them into drug court no longer exists after graduation. Nowhere on the coded data sheets do names appear, only the District Court case number. The CCH documents will be shredded and destroyed consistent with Michigan State Police LEIN policies once the raw data are no longer necessary. Until then, raw data and data entry sheets, with identifying case numbers are locked securely in an office. The remaining data, in SPSS format, will be retained for future analysis. All data in this

article are presented in aggregate form thus making the identification on an individual participant impossible.

Results

The primary working hypothesis is that Drug Court graduates will be convicted of significantly fewer crimes, especially drug-related offenses, following graduation. A second hypothesis is generated for the Terminated group that their criminal behavior will not be significantly reduced during the same *post* program time period. In other words, Graduates would show a statistically significant decline in post-program criminal convictions relative to Terminated participants. As interventions go, these are the most logical and desired results. It is further hypothesized that subjects in the remaining two cohort groups will show no significant change in their criminal behavior in the *post* time period phase.

Table 1 shows the basic demographic characteristics of the subjects in each of the four groups. Average ages of the groups are fairly close together.

TABLE 1
Demographic Characteristics for Each Cohort

	Graduated	Terminated	Offered	Bound Over
Subjects	123	224	139	280
AGE				
Mean	36.8	34.1	29.0	32.4
Range	18 - 63	18 - 58	17 - 65	17 - 58
RACE				
Black	65	142	82	195
White	58	82	51	71
Hispanic	0	0	6	14
GENDER				
Female	43	61	57	41
Male	80	163	82	239

One unsubstantiated inference that might be drawn is that graduates, having the highest average age at 36.8 years, have simply “aged out” or matured thus predisposing them to succeed. The youngest group (29.0 years), those offered the opportunity to participate but declined, may not have seen the value of Drug Court. Their *post* intervention conviction record will be critical for answering this question.

The national study provides some comparison data that helps put the Grand Rapids program in a national perspective. There is, however, one area where comparison will not be possible. There were only two Hispanic defendants in the Drug Court program and they were excluded from analysis because the very small number would skew all statistical analyses. There may also be fewer because the Grand Rapids District Court employs bilingual probation officers and currently operates a separate sobriety court for non-English speaking Hispanic defendants. In the national study there were several states with large Hispanic populations (e.g., Florida, Arizona, and California). Assuming a consistent distribution of drug crime, there are likely to be more Hispanic participants in those states than one would expect to find in any part of Michigan. Although none were included in the Drug Court data, a few are found in the other two subgroups. If Hispanic defendants are less likely to be referred to drug court or more likely to decline, then it is easy to understand why our overall percentage does not match the national sample. Between the national data and Grand Rapids, the distinction between Black and White defendants is minimal: 55.2% nationally and 59.7% in Grand Rapids were Black, respectively. Gender is closer still with 31.4% female and 68.6% male in the national study and in Grand Rapids the difference is 30% and 70%, respectively.

Consistent with national arrest and corrections data generally, women are outnumbered by men in this sample. For example, only 15% of the entire Bound Over group is female. Interestingly enough 35% of the Drug Court graduates are female, compared to 27% of those Terminated, suggesting that women may be either more motivated or have less serious records that increased their chances of being offered the opportunity; this position is supported by noting that 41% of those offered an opportunity were female. Some additional research into gender differences in these programs might be fruitful. Twice as many men as women graduated, but three times as many men were terminated. This suggests that once admitted to the program, women had a better chance of succeeding than men. Why this might be the

case is not addressed in this study. Men are also overwhelmingly overrepresented in the Bound-Over group by 6:1. In order to draw any generalizations from these trends, system wide data are needed to put the gender issue in the same perspective as race.

Some additional questions arise as to whether there might be some differences based on the interaction of Age, Race, and Gender. In the Grand Rapids data there are few significant interactions. Chi-Square analysis of Race and Gender produced no significant results between groups (chi-square = .527, ns). Whether there is a difference when Drug Court performance is added to the equation will be addressed later.

When Age is analyzed by Race and Gender something interesting happens with the Gender results. Age as a continuous variable revealed no statistically significant differences when analyzed with Gender. In other words, Age was independent of Gender. However, when Age was categorized identical to the national study, a significant difference emerged (chi-square = 11.632, $p > .02$). This strongly suggests that the categorization of ages in the national study (e.g., > 24 , 24 – 30, 30 – 36, 36 – 42, and > 42 Note the overlapping ranges.) may have introduced bias into the analysis. Race and Age, regardless of how Age is categorized, revealed no significant results (chi-square = 8.271, ns).

Stepwise multiple regression analysis was applied using Race and Gender as dependent variables on both Drug Court cohorts. Two models were set-up, one using *prior* convictions, the other *post* convictions. The question is whether either of these demographic variables has any predictive power in with Drug Court participants. When the model was applied to *prior* convictions and Race, ANOVA returned a significant result ($F = 3.815$, $p > .001$), however, the two offenses with significant negative coefficients were two of the rarest convictions in the entire data base: Felony OUIL and Misdemeanor OUIL. Why these are so rare (See Table 3, *infra*) in a group of substance abuse offenders is a mystery at this point. This suggests that the extreme scores might have been responsible for the significant result.

When Race and *post* intervention convictions were analyzed, the result was also statistically significant ($F = 3.280$, $p > .001$), however, this time it made some sense. Review of the coefficient table revealed Traffic convictions ($t = 2.388$, $p > .018$) and felony property convictions ($t = -3.466$, $p > .001$) were the significant predictors. Race was recoded into binary (0 =

White, 1 = Black), thus the positive coefficient suggests Black defendants will be convicted at a higher rate than White defendants for Traffic offenses and less likely to be convicted for Felony Property offenses. A simple tabulation of the data confirms that White participants committed Felony Property offenses at a rate twice that of Black defendants in the *post* program period. Moreover, Black defendants committed Traffic offenses at a rate five times that of White defendants in the *post* program period. Drug offenses did not achieve statistical significance.

In terms of who was in prison at the time of the *post* evaluation, chi-square analysis revealed significant differences but not unexpectedly. Only one woman (out of forty) was in prison at this time. Race revealed no significant difference with respect to who was in prison: Thirty-five (18%) Black; six (8%) White; and, four (29%) Hispanic. The percentage is a bit inflated for the last group because only fourteen (5%) defendants are Hispanic in the Bound Over group. Sliced differently, of those in prison, 78% are Black, 13% White, and 9% Hispanic.

When Gender models were analyzed using “prior” data, the results were also significant ($F = 2.228, p > .016$) and the only independent variable that surfaced was Traffic offenses ($t = 2.714, p > .007$). This suggests that women bring more traffic convictions to the program. The same offenses rise to the top when the “during” time period is analyzed ($t = 3.502, p > .001$). In the “post” time period, however, there are no significant predictor variables ($F = .917, ns$).

Offense Data by Cohort

The data in Table 2 clearly show some important distinctions between the major groups. Subjects who successfully graduated revealed the lowest number of convictions *prior* to program participation. This certainly suggests that conviction history up to the point of entering drug court may be important in terms of gaining entry into the program and may be predictive of behavior after program completion.

It seems fairly clear that subjects terminated from the program had a more extensive conviction history. Such results support the notion that defendants with less extensive criminal conviction histories may be better risks. This will be explored in greater detail later by analyzing the types of

offense convictions such as *prior* drug offenses versus prior traffic offenses. It may be that convictions for a specific group of offenses (e.g., substance

TABLE 2
Rate of Convictions for Each Time Period and Cohort

	Graduated	Terminated	Offered	Bound Over
Subjects	123	224	139	280
PRIOR				
Total	424	980	503	1815
Mean	3.40	4.40	3.60	6.40
DURING				
Total	30	151	NA	NA
Mean	0.20	0.70	NA	NA
POST				
Total	97	475	413	885
Mean	0.80	2.10	3.00	3.20
TOTAL	551	1606	916	2700

abuse-related offenses), *prior* to program entry, is more predictive of program failure. By itself this is a significant finding suggesting that the selection process could be tightened up a bit. At the same time it would be rash to conclude that *prior* offense history is the Holy Grail for ensuring program success. Admission to alternative programs, especially those that promise less vigorous prosecution, will always be subject to undocumented discretionary decision-making.

When considering offenses committed *during* Drug Court participation, something interesting happens. Table 2 reveals the first visible effect apparently attributable to Drug Court participation: There is a dramatic decline from 3.4 convictions per defendant *prior* to participation to 0.2 *during* participation for Graduates and down from 4.4 to 0.70 for Terminated subjects. This effect may be similar to intensive probation studies. In other words, closer surveillance theoretically results in better

performance. Results from studies during the 1970s (Oakland, California) revealed that defendants under intensive supervision tended to fail probation significantly more often than those unsupervised because probation officers violated probationers on technical matters that were often ignored under normal supervision (e.g., curfew, possession of alcohol, etc.). Since Drug Court is based on a form of intensive supervision, a higher failure rate might be expected. The data here do reveal that 65% of Drug Court participants were terminated. According to program staff, however, they were not necessarily terminated for one or two minor technical violations, but rather for a combination of problems, including commission of new crimes. This discussion requires additional investigation in order ensure that the same problem that plagued intensive supervision three decades ago is not reasserting itself anew.

The data in Table 2 also highlights a secondary effect Drug Court creates as an effective intervention. As can be clearly seen, Graduates were markedly less likely to be convicted for any type of offense following program participation. This differs somewhat from the phase *during* program participation because intensive supervision may have had an effect. The secondary effect is seen clearly in the Terminated subjects. The *post* decline is also significant despite the fact that they failed to complete the program. While this does not necessarily mean that simple contact with drug court will vastly improve peoples' lives - the Terminated group was responsible for almost five times as many *post* program convictions as the Graduates - it does suggest that the program has a substantial residual effect for those who participate but do not graduate.

The other two groups, Offered and Bound Over, had no distinct *during* phase. Their *prior* phase ran up to the point of their conviction for the offense that put them in one of these groups. Their *post* phase began immediately thereafter. The only condition that would approximate the *during* phase of Drug Court participants would be imprisonment during the six year *post* program time period. Imprisonment is hardly the methodological equivalent of being at liberty while participating in drug court.

The subjects in the Offered group showed virtually no difference at all between *prior* and *post* periods. The Bound Over group showed a significant decline during the *post* period. Why might this be the case? Part of the effect is certainly attributable to the fact that forty-five (16%) of the Bound Over

subjects were in prison during most of the *during* and *post* study time period. Many will continue to show “improvement” because their out dates are in the future. Despite this reduction in the ranks, those outside prison walls managed to achieve the highest average conviction rate of the four cohort groups. Perhaps we expect too much lasting deterrent effect from imprisonment.

Convictions by Cohorts

Tables 3, 4, and 5 provide additional detail on what types of offenses members of each group were convicted for in the *prior*, *during* and *post* time periods. Keep in mind that the total number of offenses exceeds the number of subjects because many of the participants were convicted for more than one offense or the same offense multiple times. The widely held position that a small number of defendants are convicted of a disproportionate percentage of offenses (the so-called 30/70 rule) will be addressed later in this paper.

Table 3 shows some interesting detailed results for *prior* criminal histories. For example, after miscellaneous and traffic convictions, Graduates had the highest percentage (31%) of drug convictions, *within the group*, going into the program. This is consistent with Drug Court’s intent to accept hard cases and not engage in what is known as “cherry picking”. This term refers to programs that carefully select participants who have a better chance of success because of a less extensive prior criminal history. Such programs shows very low failure rates.

The negative side to this approach is that the people who really need the program get passed over as high risk. The primary justification for cherry picking is program survival. Politicians are not likely to continue funding programs that have less that spectacular success rates.

An interesting anomaly is the very low incidence of drunk driving convictions in any of the groups. This is unexpected because substance abuse usually includes alcohol abuse and that is frequently expressed in drunk driving convictions. Assault was identified as an offense that barred participation in the program. Despite this prohibition, there were 113 such convictions in the three program groups. More importantly, fifteen (12%) Drug Court participants who successfully graduated had *prior* convictions for assault when they entered the program. It is instructive to note that forty-

four (20%) with prior assault convictions were terminated. Whether the mere existence of an assault conviction should be a bar to program participation should be examined in more detail.

TABLE 3
Breakdown of *PRIOR* Convictions by Offense
Category and Cohort Group

	Graduated	Terminated	Offered	Bound Over
	N = 123	N = 224	N = 139	N = 280
Felony Drug	90	150	92	317
Misd. Drug	<u>40</u>	<u>89</u>	<u>37</u>	<u>111</u>
<i>Total</i>	<i>130</i>	<i>239</i>	<i>129</i>	<i>428</i>
Felony OUIL	4	5	0	2
Misd. OUIL	<u>15</u>	<u>31</u>	<u>13</u>	<u>51</u>
<i>Total</i>	<i>19</i>	<i>36</i>	<i>13</i>	<i>53</i>
Felony Assault	7	21	8	70
Misd. Assault	<u>13</u>	<u>39</u>	<u>25</u>	<u>87</u>
<i>Total</i>	<i>20</i>	<i>60</i>	<i>33</i>	<i>157</i>
Felony Property	46	124	81	331
Misd. Property	<u>40</u>	<u>64</u>	<u>43</u>	<u>176</u>
<i>Total</i>	<i>86</i>	<i>188</i>	<i>124</i>	<i>507</i>
Traffic	93	302	100	310
Miscellaneous	76	155	104	360
<i>Grand Total</i>	<i>424</i>	<i>980</i>	<i>503</i>	<i>1815</i>

One of the more important questions is whether there is a significant effect created by *prior* criminal conviction histories for Graduated and Terminated participants. According to chi-square analysis there is no effect. To simplify the comparison of prior conviction data and to eliminate empty cells in chi-square tables, conviction data were recoded in two ways. First,

categories were collapsed across felony and misdemeanor categories thus creating, for example, “Prior Property Convictions” from “Prior Felony Property” and “Prior Misdemeanor Property”. Traffic and Miscellaneous were not originally divided into felony and misdemeanor because most of the offenses are misdemeanors or civil infractions. The second recoding was to convert continuous data (e.g., 0, 1, 2, 3, 4, etc.) into binary (0 = No and 1 = yes). Subjects were distinguished as either having no *prior* convictions or at least one prior conviction. Unlike Table 2, this analysis speaks only to “if” there was at least one conviction, not how many convictions per participant. The resulting chi-square analysis revealed no statistically significant differences between Graduated and Terminated in any of the conviction categories. This is good news because it suggests that virtually all of the individuals selected to participate started the program with similar conviction histories. This also suggests that conviction history may not be as important to program success as originally hypothesized.

The next truly important question is whether the entire panoply of *prior* convictions, for all subjects, can predict *post* conviction in the limited area of drug and OUIL offenses. That, after all, is what Drug Court is attempting to prevent. All of the *prior* convictions were analyzed in four separate stepwise regression models in an attempt to predict the four critical dependent variables: Felony drug, Misdemeanor drug, Felony OUIL, and Misdemeanor OUIL convictions during the *post* intervention time period. Only the *post* misdemeanor drug convictions achieved a significant outcome ($F = 2.306, p > .011$). Drug Court case managers probably already know that the best predictor of future misdemeanor drug convictions is past convictions for the same offense. This may also suggest that drug offenders tend to operate within a narrow range of offenses – primarily drug-related – while other offenders are involved in more of a criminal life style that sometimes includes drug-related offenses. These may appear to be similar participants; however, the former may be motivated to get off drugs while the latter is simply using the program as a way to avoid jail.

In Tables 4 and 5 the data for cohorts Offered and Bound Over are identical because the *during* and *post* time periods do not change. Their *post* period begins with the first conviction following the drug conviction that triggered their inclusion in this study. This makes it difficult to compare these two groups to the Graduated and Terminated subjects on many of the measures. They do, however, serve as excellent comparison groups for other issues.

Table 4 provides some insight as to what happened during program participation. Using the same recoding procedure as seen above in Table 3,

TABLE 4
Breakdown of *DURING* Convictions by Offense
Category and Cohort

	Graduated	Terminated	Offered	Bound Over
	N = 123	N = 224	N = 139	N = 280
Felony Drug	2	23	55	103
Misd. Drug	<u>2</u>	<u>6</u>	<u>22</u>	<u>37</u>
<i>Total</i>	4	29	77	140
Felony OUIL	1	0	1	3
Misd. OUIL	<u>2</u>	<u>6</u>	<u>11</u>	<u>16</u>
<i>Total</i>	3	6	12	19
Felony Assault	0	1	12	20
Misd. Assault	<u>0</u>	<u>3</u>	<u>13</u>	<u>38</u>
<i>Total</i>	0	4	25	58
Felony Property	0	5	22	56
Misd. Property	<u>0</u>	<u>4</u>	<u>32</u>	<u>100</u>
<i>Total</i>	0	9	54	156
Traffic	19	64	124	279
Miscellaneous	4	39	121	233
<i>Grand Total</i>	30	151	413	885

Graduated and Terminated subjects were compared on offense data *during* program participation. If those who were terminated were asked to leave the program because of new convictions during their time in the program – which seems most likely – then chi-square analysis should reveal statistically significant differences. And it does. Graduated participants were

significantly less likely to be convicted for Traffic offenses (chi-square = 4.367, $p > .037$); Miscellaneous offenses (chi-square = 9.814, $p > .002$); Property offenses (chi-square = 4.497, $p > .034$); and Substance Abuse offenses (chi-square = 6.364, $p > .012$). In the cases of assault and property offenses, Graduates totaled zero offenses. This may have impacted chi-square analysis. That problem notwithstanding, it is clear that Terminated subjects ended their participation in Drug Court by committing new offenses, not simply by collecting technical violations. Technical violation data, such as missed appointments, dirty urine drops, attitude and so forth were not collected in this study, but might prove an interesting area of future inquiry.

The rest of the results are fairly consistent with expectations. Bound over subjects, who had the worst records in the *prior* time period, continued to over-achieve. Those in the Offered group appear to have performed more like the Bound Over group approaching almost half (47%) of their total convictions *during* the same time period. The Terminated group did not do quite as well but did achieve 37% as many convictions as the Offered group. This suggests that there may be something fundamentally different about the subjects in the Offered group despite the fact that they were initially judged appropriate for Drug Court. Further research will be needed to sort that out. Moreover, fifteen (12%) Drug Court participants who successfully graduated had prior convictions for assault when they entered the program.

Table 5 allows us to see what each Drug Court cohort did following departure from the program, as well how the two non-participant groups performed during the same time period. First the good news: As a percentage of individual group convictions, Graduates had only twenty drug-related convictions (20.4%). Graduates were responsible for only 5.2% of the total convictions following graduation. Most of this group's subsequent convictions were Traffic and Miscellaneous (56.7%). As might be expected from the data already presented in Table 2, Graduated and Terminated subjects performed differently once leaving Drug Court. In all five categories, Terminated subjects out-performed Graduates in *post* program convictions: Traffic (chi-square = 9.779, $p > .002$); Miscellaneous (chi-square = 11.181, $p > .001$); Property (chi-square = 14.654, $p > .0001$); Assault (chi-square = 13.132, $p > .001$); and Substance Abuse (chi-square = 7.515, $p > .006$). Please note that the offense categories have been collapsed

TABLE 5
Breakdown of *POST* Convictions by Offense
Category and Cohort

	Graduated	Terminated	Offered	Bound Over
	N = 123	N = 224	N = 139	N = 280
Felony Drug	14	50	55	103
Misd. Drug	<u>6</u>	<u>28</u>	<u>22</u>	<u>37</u>
<i>Total</i>	20	78	77	140
Felony OUIL	0	1	1	3
Misd. OUIL	<u>4</u>	<u>9</u>	<u>11</u>	<u>16</u>
<i>Total</i>	4	10	12	19
Felony Assault	3	17	12	20
Misd. Assault	<u>2</u>	<u>24</u>	<u>13</u>	<u>38</u>
<i>Total</i>	5	41	25	58
Felony Property	6	46	22	56
Misd. Property	<u>7</u>	<u>31</u>	<u>32</u>	<u>100</u>
<i>Total</i>	13	77	54	156
Traffic	41	171	124	279
Miscellaneous	14	98	121	233
<i>Grand Total</i>	97	475	413	885

to reduce empty cells in the analysis. Again, the difference between Table 5 and the chi-square analysis is that Table 5, like Tables 3 and 4, displays total convictions independent of how many defendants were responsible for the convictions. Tables 11 and 12 will explore the issue of how small numbers of defendants can be responsible for large numbers of convictions and possibly skew results.

The Bound Over group performed worst with 47.3% of total *post* program convictions: 44% of drug convictions, 42% of OUIL convictions,

52% of property convictions, and 47% of traffic/miscellaneous convictions. They managed to do this despite the fact that forty-five of them spent much of the *post* program time in prison. As a group used for comparison to the Drug Court participants, those who were prosecuted on the original felony drug charges have some differences that should be briefly explained. First, on a strictly percentage basis, there are far fewer women in the group: 15% compared to the other three groups (41% Offered, 27% Terminated, and 35% Graduated). Racially there is a similar difference: 75% are minority (Black and Hispanic) compared to the other groups (65% for both Offered and Terminated, and 53% Graduated).

Race by Gender is statistically significant perhaps partly due to the fact that there are no female Hispanics in the sample and empty cells skews chi-square analysis. Interestingly enough, women in the Bound Over group are almost evenly divided between Black (n = 21) and White (n = 20). The only meaningful observation from this might be that women continue to be treated differently in the system. Returning to Table 1, remember that 35% of Drug Court graduates were women and even 27% of the Terminated participants were women. Forty-one percent of those to whom the opportunity was offered were women. Compare that to 15% in the Bound Over group and it would appear that women are less often prison-bound. This is not an uncommon observation and may simply be an artifact of social bias that favor women.

Prior – Post Correlation Analysis

One of the most important questions is whether prior convictions correlated with post convictions beyond chance. There is some research to suggest that defendants imprisoned for some crimes are very likely to be rearrested for the same offense after release (see, BJS Special Report, Recidivism of Prisoners Released in 1994, NCJ 193427, June 2002). Drug offenders are rearrested 41.2% of the time for the same offense for which they were originally incarcerated; compared, for example to assault at 22.0%. So the question for the next five tables is whether similar patterns emerge for this group. Table 6 presents aggregate level analysis for all subjects without regard to cohort membership. As the data were reviewed in the previous tables, it became obvious that there were some differences between groups that should have been more homogenous. For example, Drug Court Graduates, Terminations, and Offered, were all initially selected

according to the same criteria. That commonality suggests that perhaps they might behave similarly after program participation.

TABLE 6
Prior and Post Correlations for
All Subjects

Prior Felony Drug	r = .103, p > .01	Post Miscellaneous
	r = .076, p > .05	Post Misd. Property
	r = .095, p > .01	Post Felony Drug
Prior Misd. Drug	r = .128, p > .01	Post Misd. Drug
Prior Felony OUIL	ns	
Prior Misd. OUIL	r = - .073, p > .05	Post Misd. Drug
	r = .087, p > .05	Post Felony OUIL
Prior Felony Assault	r = .098, p > .01	Post Miscellaneous
Prior Misd. Assault	r = .092, p > .05	Post Miscellaneous
Prior Felony Property	r = .119, p > .01	Post Felony Property
	r = .178, p > .01	Post Misd. Property
Prior Misd. Property	r = .216, p > .01	Post Misd. Property
Prior Traffic	r = .121, p > .01	Post Traffic
Prior Miscellaneous	r = .310, p > .01	Post Miscellaneous
	r = .084, p > .05	Post Misd. Property

Note in Table 6 that the only prior offenses category with no *post* intervention correlation is felony OUIL. Why drunk driving offenses are so rare in this entire group (see Table 3, *op. cit.*) is still a mystery. The offense is not on the prohibited list and it seems logical that abuse of alcohol would be part of the offense constellation for all four cohort groups. Many of the correlations match perfectly the logic that “Past is Prologue”. For example,

prior felony drug offenses are significantly related to *post* felony drug offenses and property offenses. This perhaps points to the fact that drug users also become involved in theft to support their increasingly expensive habit and felony convictions will invariably follow a string of misdemeanor convictions. The puzzle alluded to earlier is the collection of miscellaneous offenses (e.g., soliciting for prostitution, creating a disturbance, indecent exposure, false information to an officer, etc.). This group, a constellation of its own, is significantly related not only to itself but three other major offense categories (felony drug, felony and misdemeanor property). Property offenses are very homogeneous and are most strongly related to each other from *prior* to *post*. OUIL offenders, few though they may be, appear to logically graduate from misdemeanors to felonies but reveal a negative correlation with *post* drug convictions. While they may graduate to felony OUIL, they avoid drug offenses. Misdemeanor drug offenders do not appear to graduate to felony drug offenses.

The next four tables, 7 through 10, seek to shed some light on whether each of the four groups differed in their relationship between *prior*, *during*, and *post* intervention convictions. Since we are most interested in how subjects fared following program participation, the correlations are presented from the *post* intervention perspective. It is important to keep in mind that not only should correlations be statistically significant, but the variables should have some logical relationship to each other that is meaningful in this context.

Table 7 explores the *post* program conviction relationships of the most important group, Graduates. After all, if we can identify some logical offense relationships, it might help Drug Court managers focus on keeping people in the program by paying closer attention to defendants with certain offense backgrounds. That is a wonderful notion but the most striking thing in Table 7 is that there are no significant relationships between *post* intervention drug and alcohol convictions and any *prior* offenses. *Prior* drug or alcohol convictions are not a bar to program participation; however, it would appear that those who graduate do not have a significantly extensive history of such offenses. This may suggest that the program's intention to accept defendants with a longer history of drug convictions is being tempered by gatekeepers who have different objectives. For example, prosecutors may not make the initial referral if they are at all nervous about defendants who have a significant drug offense history in the local community. Referring hard cases might send the wrong message to the

streets that officials have become soft on crime. Law enforcement, not part of the formal referral process may also quietly lobby to make sure chronic offenders are removed from the streets.

Table 7
Post, Prior, During Correlations for
Drug Court Graduates Only

Post Felony Drug	ns	
Post Misd. Drug	r = .311, p > .01	<i>During Traffic</i>
	r = .315, p > .01	<i>During Miscellaneous</i>
Post Felony OUIL	ns	
Post Misd. OUIL	r = .365, p > .01	<i>During Traffic</i>
	r = .268, p > .01	<i>During Miscellaneous</i>
Post Felony Assault	ns	
Post Misd. Assault	ns	
Post Felony Property	ns	
Post Misd. Property	r = .222, p > .01	Prior Miscellaneous
	r = .285, p > .01	Post Felony Assault
Post Traffic	r = .228, p > .01	Prior Miscellaneous
	r = .268, p > .01	Post Felony Assault
Post Miscellaneous	r = .245, p > .01	Prior Miscellaneous
	r = .362, p > .01	Post Felony Assault

The only significant correlations with *post* drug and alcohol convictions involves *during* program participation and then only for traffic and miscellaneous offenses. On the face of it this seems counterintuitive. It also suggests that both offense groups were (correctly) considered unimportant to the gatekeepers because everyone in this group graduated.

Both of these categories provide wide latitude for seriousness. Traffic offenses can range from speeding to an accident involving injury or death. Miscellaneous is a catch-all category created for this study that includes an even wider range of unrelated offenses. Further analysis might be appropriate to determine whether there are any significant relationships lurking about in the data.

TABLE 8
Post, Prior, During Correlations for
Terminated Drug Court Participants Only

Post Felony Drug	r = .261, p > .01	<i>During Felony Property</i>
	r = -.136, p > .05	Prior Misd. Assault
Post Misd. Drug	r = .206, p > .01	Prior Misd. Drug
Post Felony OUIL	r = .289, p > .01	Prior Misd. OUIL
Post Misd. OUIL	r = .152, p > .05	<i>During Miscellaneous</i>
	r = .147, p > .05	Prior Felony Assault
Post Felony Assault	r = .434, p > .01	<i>During Felony Assault</i>
	r = -.150, p > .05	Prior Traffic
Post Misd. Assault	r = .229, p > .01	<i>During Misd. Property</i>
Post Felony Property	ns	
Post Misd. Property	ns	
Post Traffic	r = .264, p > .01	<i>During Traffic</i>
	r = .136, p > .05	<i>During Misd. Property</i>
	r = .267, p > .01	Prior Felony Assault
Post Miscellaneous	r = .314, p > .01	<i>During Miscellaneous</i>
	r = .548, p > .01	Prior Miscellaneous

Table 8, conversely, tells us just about everything we want to know about why this cohort was terminated. First, there are statistically significant

correlations between *post* program drug/alcohol convictions and *prior* drug/alcohol convictions. Second, this cohort came to the program with assault records that were significantly related to *post* assault convictions and assault convictions *during* the program. The statistically significant offense constellation present in this cohort is greater than that of the Bound Over group (Table 10, *infra*). Based on this information it is not surprising defendants in this group failed. Does this mean there is no hope for someone presenting such a record? Not necessarily. Correlations identify the strength and direction of the relationship but do not guarantee causality. The relationship does not suggest that a history of assault, whether related to drug use or not, bodes ill for this type of therapeutic intervention.

TABLE 9
Post, Prior, During Correlations for
Offered Drug Court Participants Only

Post Felony Drug	r = -.169, p > .05	Prior Misd. Property
Post Misd. Drug	r = .241, p > .01	Prior Misd. Drug
Post Felony OUIL	Ns	
Post Misd. OUIL	Ns	
Post Felony Assault	Ns	
Post Misd. Assault	ns	
Post Felony Property	ns	
Post Misd. Property	r = .299, p > .01	Prior Misd. Property
	r = .194, p > .05	Prior Miscellaneous
Post Traffic	r = .257, p > .01	Prior Traffic
Post Miscellaneous	r = .193, p > .05	Prior Misd. Property

It is possible that if defendants in the Offered cohort had participated, they might have done better than the Terminated cohort. Table 9 reveals a

correlation between *prior/post* drug offenses that suggests the defendants in this cohort might have benefited from the program if the intervention is effective. This group also reveals the fewest *prior/post* correlations and that suggests they might have done well if they had chosen to participate. Why they declined requires further examination.

In Table 10 we encounter the group from which we should expect worse - and a quick review of Table 2 reminds us that they were responsible for 2700 convictions (with the Terminated group coming in right behind them) – but instead find a smaller offense constellation than found in the Terminated group.

TABLE 10
Post, Prior, During Correlations for
Bound Over Subjects Only

Post Felony Drug	ns	
Post Misd. Drug	ns	
Post Felony OUIL	ns	
Post Misd. OUIL	r = .152, p > .05	Prior Misd. OUIL
Post Felony Assault	ns	
Post Misd. Assault	r = .138, p > .05	Prior Misd. Assault
	r = .185, p > .01	Prior Misd OUIL
Post Felony Property	r = .223, p > .01	Prior Felony Property
Post Misd. Property	r = .224, p > .01	Prior Felony Property
	r = .234, p > .01	Prior Misd. Property
Post Traffic	ns	
Post Miscellaneous	r = .280, p > .01	Prior Miscellaneous
	r = .118, p > .05	Prior Felony Drug

The correlations are much more logical here, however. Consider that all of the *post* convictions offenses have a significant identical prior offense conviction (highlighted). It is possible that the drug- and alcohol-related offenses they committed that allowed them to be included here are not part of their usual routine. Moreover, this group would almost certainly fail if they were put in Drug Court but *not* because substance abuse offenses are such a small part of their offense constellation, but because of the sheer volume of offenses they commit. Prison may have reduced to opportunity for some but did not diminish the productivity of those at liberty. All of the highlighted correlations are consistent with a so-called criminal career. The pattern from *prior* to *post* is offense specific. Whatever they did *prior*, they continued to do in the *post* program phase. Interestingly enough, drug/alcohol offenses are not well represented in their offense constellation and that may be due to being charged with more serious offenses than minor drug offenses. Again, the gatekeepers may be making decisions that are consistent with their obligations to control crime.

Multiple Convictions

Tables 11 and 12 provide yet another perspective for evaluating the effectiveness of Drug Court and the inherent differences between the cohorts in this study. Table 11 may look a bit busy at first, but the purpose is easily divined. There exists a body of literature that suggests the presence of a relatively small group of offenders who are responsible for a disproportionate share of criminal offenses. The immediate question, of course, is whether such a cadre of high volume offenders is responsible for skewing the statistics in this study. This phenomenon is variously known as the 20/80 or 30/70 rule. Roughly translated this means that 20% or 30% of offenders commit 80% or 70% of the crime. Table 11 allows visual evaluation of whether this theory impacts the baseline data (*prior* convictions) against which the effectiveness of Drug Court is evaluated. This theory, if valid, would cause problems when all cohorts were collapsed into one large group. Hidden over-achievers might tend to skew the results. This also demonstrates that program participants are not new to the justice system thus establishing the program's claim to focus on repeat offenders.

The conviction data have been divided at the 1-4, 5-9, and 10+ breaks. This may appear somewhat arbitrary (because it is) but after reviewing several different studies, it became obvious that there are no standardized breakpoints. This one simply felt good. The first two lines in Table 11 (and

12) identify subjects in each cohort with no prior convictions. Either data were missed or this relatively small group was completely new to the justice system. For each section the lines are read as follows:

1 – 4 Convictions refers to number immediately to the right (e.g., 213) and that represents the total number of convictions for participants with between 1 and 4 convictions each. 5 – 9 and 10+ are read the same way.

% Cohort refers to the percentage of subjects contributing the number of convictions just discussed: 82 subjects represent 66.7% of all Graduated subjects.

% Convictions represents the percentage of total cohort convictions for which this group is responsible. In this case 213 convictions represent 51.7% of the total Drug Court Graduates convictions.

% Total Convictions is mathematically identical to the above formula with a new denominator thus showing what percentage of the grand total of all convictions (N=3,708) this subset of Graduated cohorts (N=82) was responsible for (N=213).

Each cohort brings a slightly different composite picture to the study. For example, Graduate prior history behaves exactly as might be logically expected if we theorize that individuals with increasingly larger numbers of convictions decrease proportionately in the population. Defendants at the extreme end of the scale (10+ convictions) are responsible for relatively little effect overall: 3.3% of the cohort delivered 9.7% of the total cohort prior convictions and just barely 1% (1.1%) of the total convictions for all cohort groups. When compared to the Terminated group, this may help explain why they were terminated or why the Graduates succeeded. Note that 8.0% of the Terminated group was responsible for 22.2% of cohort convictions and 5.8% of the total convictions. Although this cohort was selected according to the same criteria as the Graduates, there appears to be at least this much of an *a priori* difference.

The two remaining cohorts showed other differences. Almost half (48.1%) of the convictions in the Offered cohort were provided by 28.2% of the subjects. The Bound Over cohort achieves roughly the same ratio: 21.8% of the subjects were responsible for 43% of the cohort convictions but, and

TABLE 11
Comparison of PRIOR Multiple Convictions by Cohort

	Graduated	Terminated	Offered	Bound Over
	N = 123	N = 224	N = 139	N = 280
Prior Only				
None	10	21	13	15
% Cohort	8.10%	9.40%	9.40%	5.50%
1 - 4 Convictions	213	275	197	214
Subjects	82	111	82	86
% Cohort	66.70%	50.00%	59.00%	30.70%
% Convictions	51.70%	68.60%	39.20%	11.70%
% Total Conv.	5.70%	7.40%	5.30%	5.80%
5 - 9 Convictions	159	478	242	827
Subjects	26	75	39	118
% Cohort	21.10%	33.50%	28.10%	42.10%
% Convictions	38.60%	49.40%	48.10%	45.30%
% Total Conv.	4.30%	12.90%	6.50%	22.30%
10 + Convictions	40	215	64	784
Subjects	4	18	5	61
% Cohort	3.30%	8.00%	3.60%	21.80%
% Convictions	9.70%	22.20%	12.70%	43.00%
% Total Conv.	1.10%	5.80%	1.70%	21.10%
Total Prior	412	968	503	1825

this is important: That same sub-group was responsible for 21.1% of total convictions. Expanded to the entire pool, the 61 Bound Over subjects represent 8% of the total subjects (N=766) and they, in turn, were responsible for 21.1% of the total convictions. Not quite as lopsided as theorized. After all, the Bound Over cohort was responsible for just short of half (49%) of all convictions and represented 37% of the total subjects.

Obviously if overachievement can impact the baseline going into the intervention, it can also impact results at the other end. Either way, the potential impact has to be identified and then, if necessary, a decision must be made about how to treat extreme scores. Sometimes the decision is to leave them in the mix; after all, they were originally included without knowing what they brought to the analysis. To exclude them now because they threaten an otherwise significant result is inappropriate and just a bit unscientific.

Table 12 is the logical counterpart to Table 11 for evaluating the other part of this question. True to form the Graduates performed exactly as expected: Only one subject registered more than ten convictions and that person graduated. Moreover, this cohort showed the single greatest decline in overall convictions from its own baseline by 79.4%. Mathematically, however, it does look a bit lopsided: < 1% of the subjects was responsible for over 15% of the convictions. This is one of those instances when reporting summary statistics can be misleading. Terminated subjects performed much like they did for the baseline: Rounding up, 12% were responsible for over a third on the convictions. Despite the fact that they were terminated from Drug Court, their overall post intervention conviction reduction hit 53.3%.

Subjects in the Offered cohort turned things a bit upside down. Whereas they resembled the Graduates in the baseline data, they look here more like Bound Over subjects. First, in the 10+ convictions category, 5.8% of the cohort was responsible for 33.1% of the convictions. That averages out to 17 convictions per participant. Even assuming that the one subject with 36 post intervention convictions was a mistake (in more ways than one!), omitting him or her lowers the rate 14.4 convictions. High numbers are not unusual: Taken together, there are 11 (excluding the one with 36) subjects with more than 15 post intervention convictions each, totaling 195 convictions by themselves (in the baseline data the numbers were 15 and 258, respectively). They cannot simply be omitted because their inclusion makes one group or another look bad. The other contribution made by this cohort is a reduction from baseline of only 17.7%. Even the Bound Over cohort did much better than that at 51.5% from baseline. The Bound Over subjects performed much as they did in baseline: 6.4% garnered 30.1% of the convictions.

TABLE 12
Comparison of POST Multiple Convictions by Cohort

	Graduated	Terminated	Offered	Bound Over
	N = 123	N = 224	N = 139	N = 280
Post Only				
None	87	77	42	65
% Cohort	70.70%	34.40%	30.20%	23.20%
1 - 4 Convictions	49	227	133	339
Subjects	30	117	67	153
% Cohort	24.40%	52.20%	48.20%	54.70%
% Convictions	57.60%	50.20%	32.10%	38.30%
% Total Conv.	2.70%	12.40%	7.20%	18.50%
5 - 9 Convictions	23	162	144	280
Subjects	4	26	22	44
% Cohort	3.30%	11.60%	15.80%	15.70%
% Convictions	27.10%	35.80%	34.80%	31.60%
% Total Conv.	1.30%	8.80%	7.80%	15.30%
10 + Convictions	13	63	137	266
Subjects	1	5	8	18
% Cohort	< 1.0%	2.20%	5.80%	6.40%
% Convictions	15.30%	13.90%	33.10%	30.10%
% Total Conv.	< 1.0%	3.40%	7.50%	14.50%
Total Prior	412	968	503	1825
Total Post	85	452	414	885
% Change	-79.40%	-53.30%	-17.70%	-51.50%

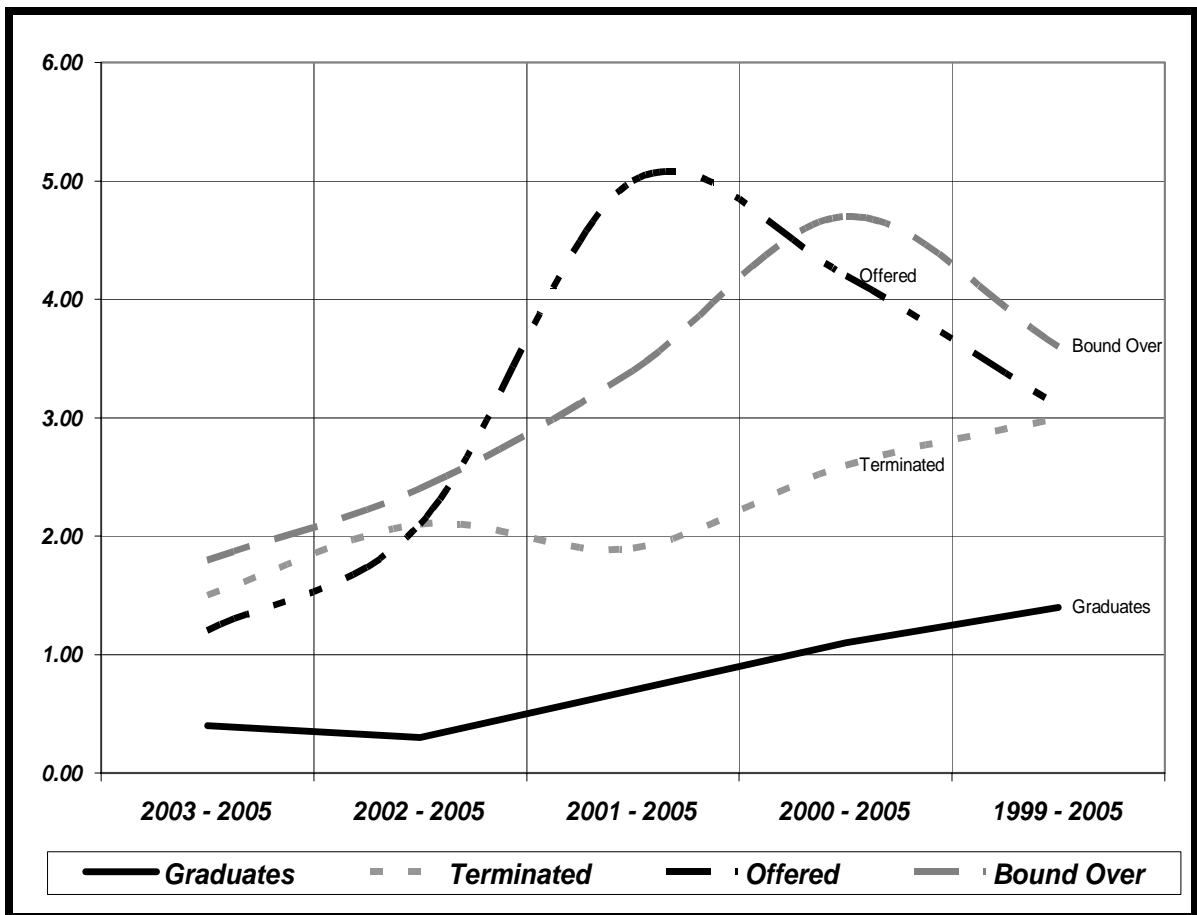
It would appear that the selection process favors defendants with less extensive records, however, consistent with the program's philosophy, admission is not restricted to those with very limited *prior* criminal records. To their credit the gatekeepers outside the Court's jurisdiction offer the opportunity to defendants who might not otherwise get such a chance. Because the results generally suggest that success may be independent of

criminal history, further research into the gatekeepers in these programs would be useful.

Recidivism by Offense and Cohort

Figure 1 looks backward in time to answer one of the major questions in such studies and that is how long the treatment does the effect last. There are usually two central questions: How soon do graduates re-offend? And, do they eventually return to their pre-program offense levels?

FIGURE 1
Post Intervention Conviction Rates for Each Cohort



Both of these questions can be addressed in the present study for each of the cohort groups. What is most interesting is whether subjects in each group, alone and relative to one another, begin to re-offend and at what rate. The first group of subjects at the extreme right end of the graph had cases

that commenced in late-1999 (graduating in 2000). Each of the data points (e.g., 1999 – 2005, 2000 – 2005, etc.) represents the cumulative time for each group's post program criminal history. Using the 1999 – 2005 data point, we can see that Graduates identified in the 1999 cohort have returned to a 1.4 conviction rate; Terminated to a 3.0 rate; Offered to a 3.1 rate; and Bound Over to a 3.6 conviction rate.

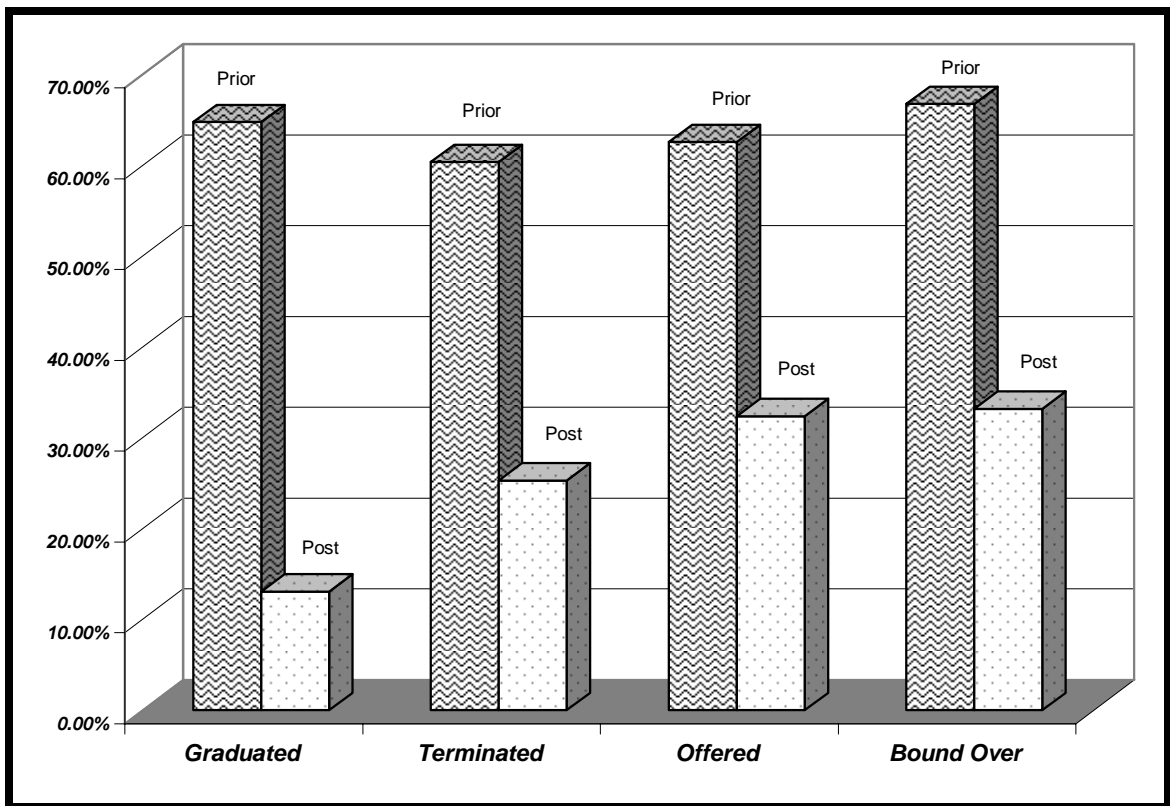
One obvious finding is that as each group moves farther away from the Drug Court influence, the number of convictions begins to rise. The 2004 – 2005 Graduate cohort is confusing because there were only 23 cases to track and for only a very short time. Additionally, there were only two graduates when the data were being collected and two post program convictions. The Bound Over group from 2000 did much worse than any of the other Bound Over cohorts averaging over five convictions per defendant. Those individuals offered an opportunity to participate in 2001 did a little better than the group that followed in 2002, but the trend line shows a fairly steady increase in convictions based on the age of the cohort group. Overall the visual presented here is consistent with the intended result of therapeutic justice: Successful participants do better for longer periods than non-participants.

Peering into the future, all of the groups appear to start out at about the same place as 2004 – 2005 cohorts. By 2010, if each continues to behave in approximately the same way, the distribution may indeed appear as it does now for the 1999 – 2005 groups. Based on the analysis in Figure 1, there is no doubt that the program Graduates are reconvicted at a significantly lower rate than subjects in the other three groups. It might also be concluded, although only tentatively, that prison is less effective at reducing recidivism than alternative interventions such as Drug Court. It is also clear that many were screened out for good reason.

Figures 2 through 8 provide the best visual representation of how each cohort performed relative to each other, by offense type, following the intervention period. Please recall those who were Offered a chance and those Bound Over to Circuit Court had no interim period when they were actively engaged in a program of any kind. Therefore their *post* time period began as soon as they were arrested for the drug-related offense that identified them as a potential participant in this study.

Figure 2 provides the most graphic comparison of the series because it shows vividly the effectiveness of Drug Court intervention. Each cohort group brought an almost identical drug offense conviction history to the study. Drug-related convictions range very tightly from 60.4% to 66.8% - not statistically significant. The post intervention results are visibly undeniable. Graduates made the most significant reduction from 64.8% drug-related convictions to only 13.1% being convicted again for similar crimes.

Figure 2
Drug Offense Recidivism by Cohort



It should be noted again that some bias may be present because the prior time period is much longer than the post intervention period. The average age for the Graduate group, for example, is 36.8 years (see Table 1, *infra*). For those who may have started in their early twenties, that makes the time period to amass convictions at least ten years. Conversely, the post intervention time period runs from early 2000 to mid-2005, not quite six years. If one group, for reasons unknown, amassed their criminal history in a

much shorter time period than the other groups, the comparison would be less accurate. Whether this is the case is unknown. Regardless, if the bars showing *prior* conviction rates are removed, the effect is still strong for the intervention.

When compared to the national study (Roman, 2003) the graduates here performed quite well. The national un-weighted one year recidivism rate was 14.4% and the two year rate was 25.5% compared to the 13.1% rate covering over five years for this study. Furthermore, the national study used only convictions verified through the FBI computerized criminal history records. This study included any convictions that occurred locally but because they were misdemeanors were not reported to the Michigan State Police. This means that the local subjects had a greater chance to accumulate more convictions than the national group. Even defendants who had been terminated from drug court did as well (25.3%) as the national group (25.5%).

Making a distinction between convictions reportable to the FBI and those not reported is artificial at best, potentially biased and misleading at worst. It is also possible that crime data, aggregated from the individual department level all the way to the FBI, loses some of its original accuracy. This also raises another issue with some studies. The definition of recidivism can create success where none really exists. In other words, if we restrict failure (reconviction) to only offenses that pass federal muster (reportable to the FBI), a great many so-called lesser offenses will be missed. Every effort should be made to identify new convictions regardless of how minor they appear.

Variations on the definition of recidivism are legion in the field of criminology. Which one an evaluator chooses will frequently dictate the results of the evaluation and the likelihood of continued support and funding. All are defensible but should be thoroughly defined somewhere in the study using them. For example, if the only post program conviction to be counted is one that matches the original program-entry offense, then the success rate will be truly spectacular. On the other hand, if any subsequent arrest constitutes program failure, then the program is doomed.

FIGURE 3
OUIL Offense Recidivism by Cohort

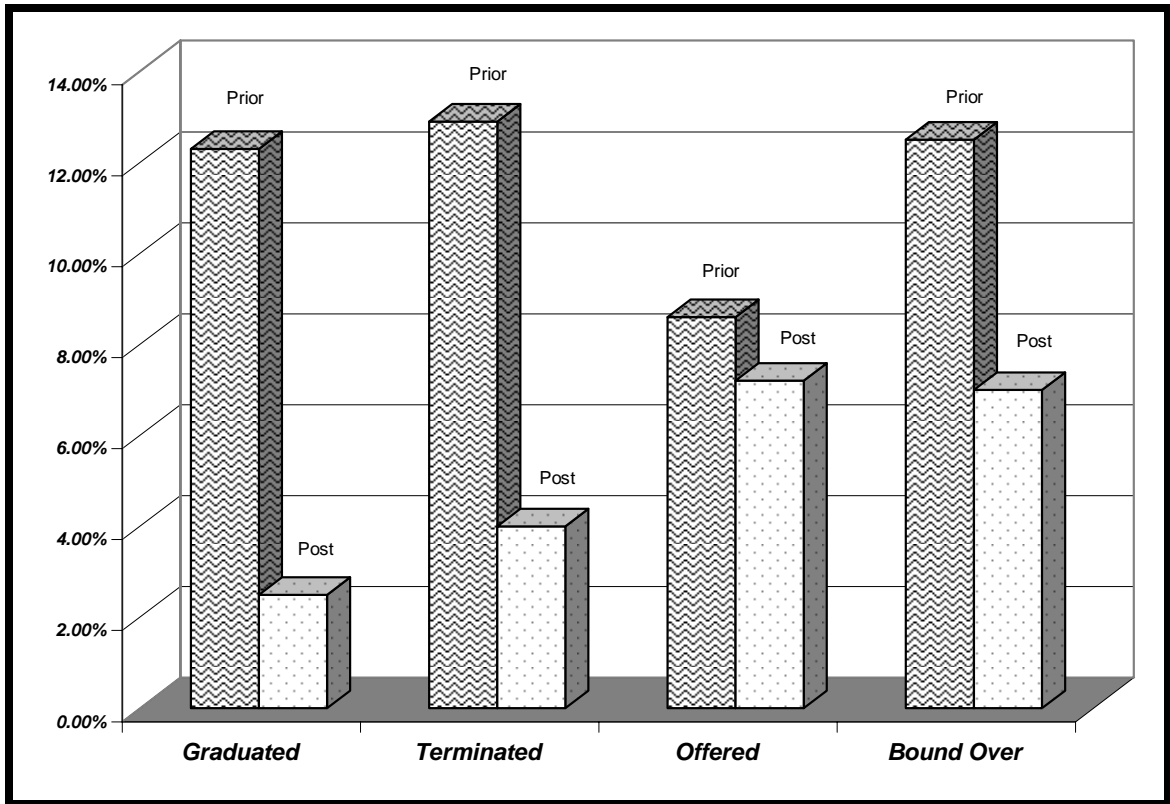


Figure 3 is comparable to Figure 2 with the exception of defendants who were offered a chance but refused. What is unusual in Figure 3 is the comparatively small number of OUIL convictions (N=121) for all defendants prior to their entry or selection: None of the groups reached even 13%. Note, however, that the Y-axis tops out at only 14%. Put these percentages on a scale similar to Figure 2 and OUIL convictions would simply disappear. Their post program OUIL conviction rates appear to drop even lower. Additional research will be necessary to explore this interesting issue. The anomaly is that substance abuse treatment professionals generally agree that drug users are usually poly-drug users and alcohol is certainly one of the most abused drugs available. There may well be other factors interacting here but sufficient data were not collected at this time to explore the issue. Future research might look deeper into the poly-drug issues facing drug/sobriety court participants.

FIGURE 4
Assault Offense Recidivism by Cohort

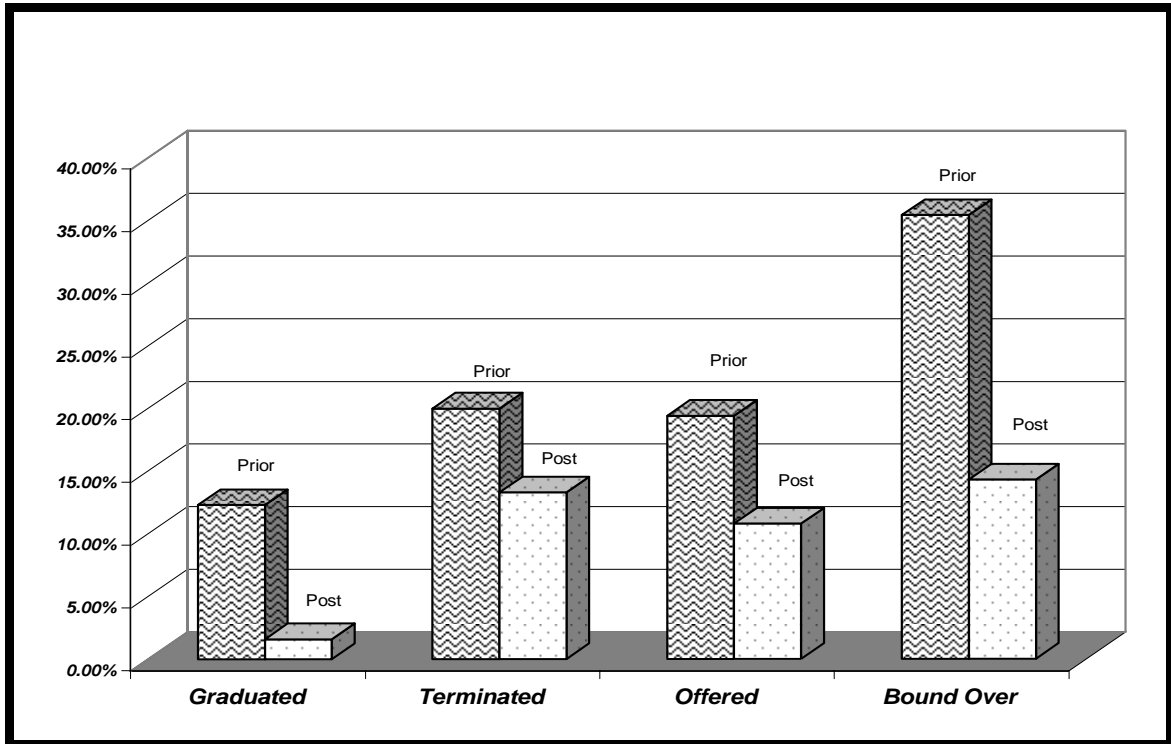


Figure 4 shows us that assault is fairly rare (less than 20% with convictions) in the three program-related groups because that particular offense is a red flag during the referral process. Consistent with that prohibition, the Bound Over group shows the highest percentage of assault convictions *prior* to program participation, but even then less than 35% have such convictions. In other words if defendants were arrested for qualifying drug offenses but had one or more convictions for assault, they would tend to concentrate in the Bound Over group. Interestingly enough the Bound Over *post* program assault convictions drop substantially. Some Graduates were admitted (12.3%) despite the prohibition on assault and the cohort group *post* intervention conviction rate dropped to almost zero (1.6%). As a possible future research topic, it might be useful to determine exactly which specific offenses, and their frequency, are most predictive of failure. Furthermore, some of the prohibitions may be a function of gatekeepers such as the prosecutor.

FIGURE 5
Property Offense Recidivism by Cohort

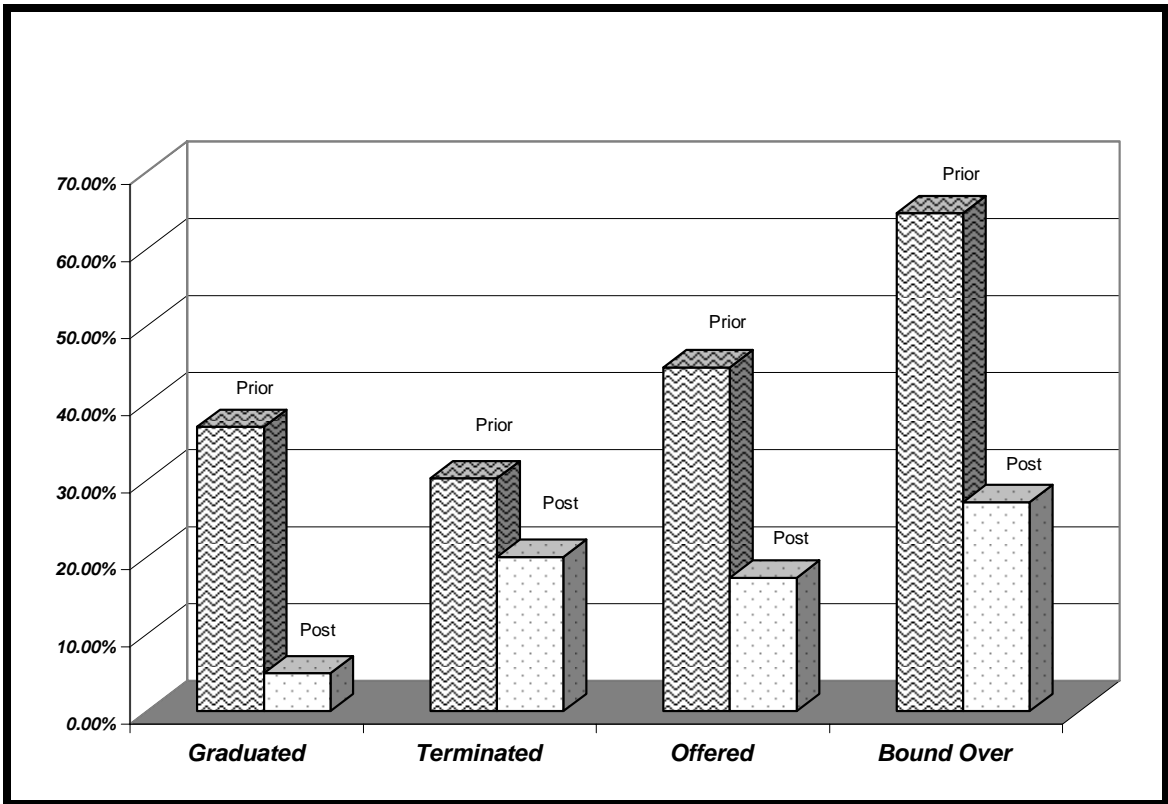
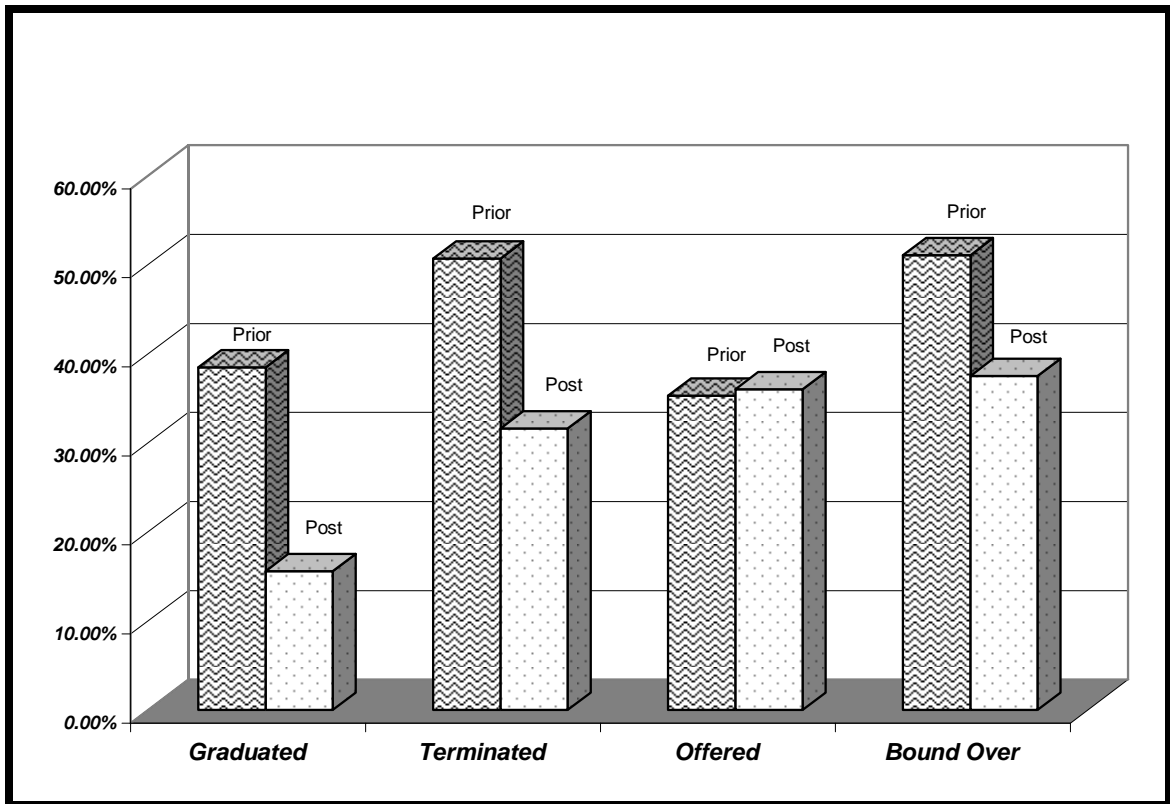


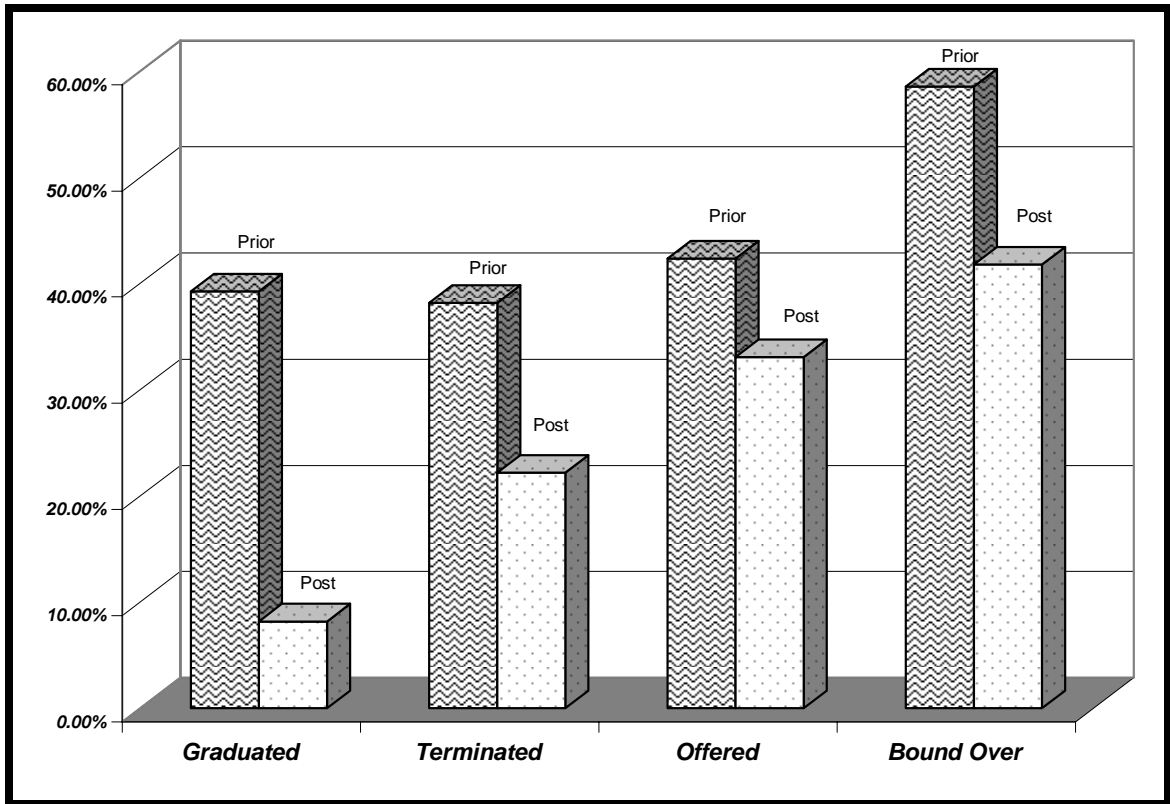
Figure 5 shows the distribution of property convictions for each of the cohort groups. This is obviously an offense committed predominantly in the *prior* time period by defendants eventually Bound Over to Circuit Court (over 60%), however, their *post* program behavior was more consistent with two of the other cohorts, excluding Graduates. While participants terminated from Drug Court did attempt to catch-up in the *post* program time period, the Bound Over group continued to out-pace them slightly. If property offenses are linked to drugs (theft to support a habit) then beating the drug habit should significantly reduce the need to steal property. While cause/effect may be a long way off, that appears to have happened here. We would need to know the nature of the *prior* property theft convictions before drawing conclusions about whether the reduction we see here is related to kicking the drug habit.

FIGURE 6
Traffic Offense Recidivism by Cohort



Although traffic convictions, shown in Figure 6, are admittedly not as serious as the forgoing offenses, it is important to note that driving convictions came back quite well in the *post* time period. For unknown reasons the Offered cohort had the lowest *prior* conviction rate, but then returned the only *post* program conviction rate that was higher than the *prior* time period. They must have worked hard at it because the *prior* time period is longer. A detailed analysis of specific offenses for each group might tell us whether the quality of the offenses differed between groups. Some individuals may be committing only minor traffic infractions (minor speeding, rolling through a stop sign, etc.) while others may have records involving offenses such as careless driving, excessive speed, running stop lights, and so on. The difference between them is that the latter group of offenses is potentially more harmful to the public and shows greater disregard for the law.

FIGURE 7
Miscellaneous Offense Recidivism by Cohort



Miscellaneous convictions for minor crimes such as soliciting for prostitution, creating a disturbance, indecent exposure, false info, etc. performed a little bit differently in Figure 7. Three of the cohort groups had roughly identical *prior* conviction rates, however, convictions in the *post* intervention period moved consistently upward like a set of evenly spaced stairs beginning with the group that had the most direct supervision (Graduates) to the group with perhaps much less supervision (Bound Over) once released. A notable percentage of the Bound Over cohort was also in prison during much of the *post* intervention time period but as a group still managed to get convicted more than the other groups. As stated earlier, a detailed treatment of this offense group is definitely needed to determine which of the many offenses are more closely related to drug offenses. It might help answer which offenses dropped out of the Graduates offense repertoire.

FIGURE 8
Drug Court Graduate Recidivism by Offense Category

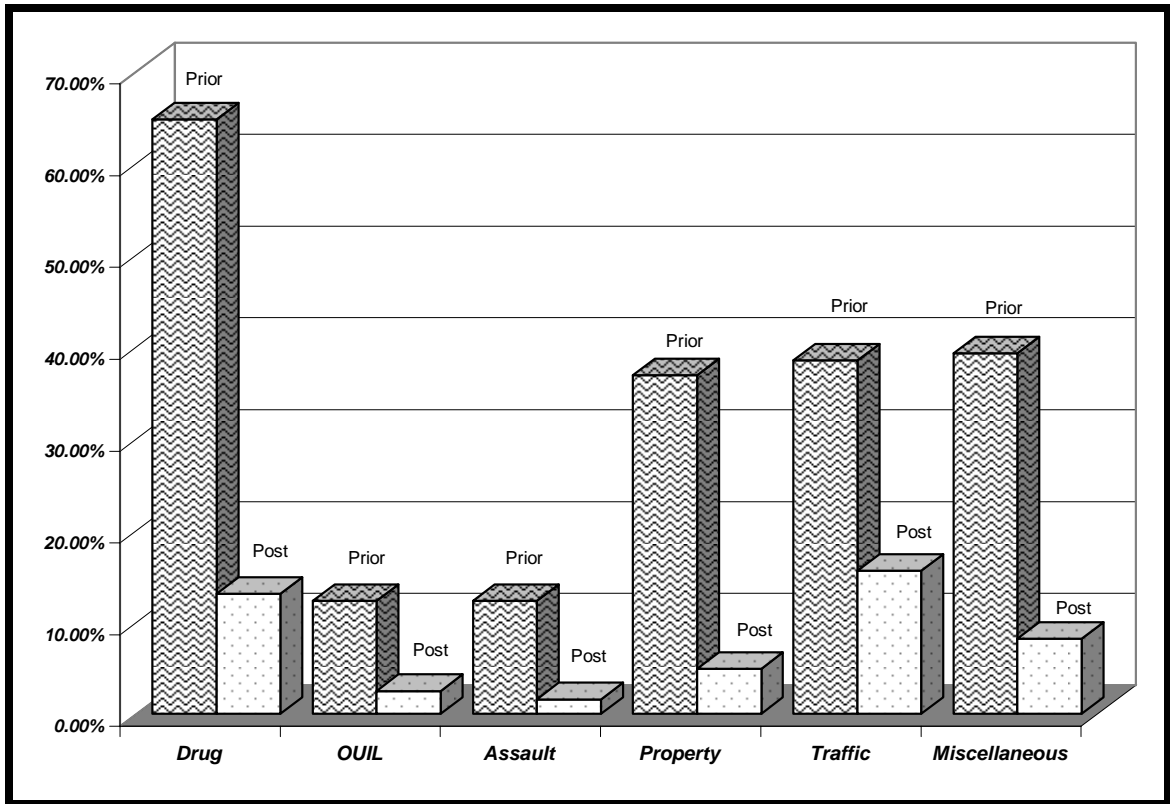


Figure 8 provides a summary of only the drug court graduates with respect to the data presented in the preceding series of figures. The goal is to show how well the graduates performed relative to the conviction baggage they brought with them into the program. There is no question that those admitted to the program were obviously involved in drug-related crime prior to enrolling in the program (the offense that got them into the program is not included in the prior count). Some programs identify defendants without significant offense histories based on the philosophy that getting offenders as early as possible is the only way to end the cycle. That may be true. The approach certainly leads to very high success rates. It is also of equal or greater importance to attempt to salvage those are caught in the cycle and want desperately to get out. Figure 8 pretty clearly demonstrates that this program works.

Conclusions

The most important question generally asked of any therapeutic justice interventions is, “Did it work?” Based on the forgoing analysis, the answer is “Yes.” More so than negative results, positive results require explanation. After all, the null hypothesis has been rejected!

One of the most frequent criticisms of drug court evaluation is that it is unscientific. By this, most critics mean that a true scientific approach or methodology was not used and, therefore, the results are suspect. They are, technically speaking, correct. And probably always will be. It is a criticism that can be fairly applied to virtually everything evaluated in the justice system. In order to apply truly scientific methods to interventions the serious student must consult the Holy Grail of research methodology, Experimental and Quasi-Experimental Designs for Research by Donald T. Campbell and Julian C. Stanley. My copy was in its fifteenth printing in 1963. Without recounting in excruciating detail what is required to achieve true experimental research, suffice it to say that there are very, very few opportunities to engage in true random selection and random assignment in the justice system. The only randomness in the justice system may be an arrest; however, those same arrests may not be representative of crime or criminals in general. Human discretion and the almost certain bias that it creates, is visible at all levels of decision making in the system. If such discretion were controllable, that would have happened long ago.

So let us dispense with any discussion of selection bias or any other kind of bias. The fact of the matter is that programs such as drug courts are created because people in a position to implement them, do. Whether borne of frustration at the few alternatives available or some misguided desire to help those in trouble, the simple fact is that someone has attempted to do something different and we all want to know whether it worked. True, we would also like to know “why” (for purposes of replication elsewhere), but given the incredible combination of unknowns, most of them involving human behavior, we simply must be content with agreeing the intervention worked in one place, at one time, for a specific purpose.

So what have we learned about the 61st District Court intervention?

1. One-third of Drug Court participants graduated.
2. Graduates, as a group, were older than other subjects.
3. More men than women participated in Drug Court.
4. Graduates entered the program with the lowest prior conviction rate.
5. Program participants, regardless of success, had the lowest conviction rate while in the program.
6. For all subjects their conviction rate increased in the post-program time period but never came close to their prior conviction rate.
7. Over time, graduates showed much less likelihood of being reconvicted for any offenses.

But what did we fail to learn?

1. Why do some defendants succeed? And others fail?
2. If randomly assigned to Drug Court, would some of the Bound Over subjects have succeeded?
3. Why did two-thirds of drug court participants fail?
4. Why did there appear to be a lasting effect for participants even after they failed?
5. What aspect of drug court has the greatest impact on defendants?

Some of these questions may be unanswerable, while others may require new types of methodologies to get at the information. Thus far the literature, and this study is no different, provides the same sort of limited answers that only tell us that the program worked. We still do not know why. And perhaps we never will. The bottom line, as corny as it sounds, is that this program, at this time and location, with dedicated staff, motivated clients, and enthusiastic service providers, is making a difference. While it is certainly a good idea to continue to evaluate intervention programs, it is a bad idea to make data collection and evaluation as important as delivering the services.