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Intensive judicial supervision and drug court outcomes: Interim findings from a randomised controlled trial

Craig Jones

***Aim**: To assess whether intensive judicial supervision (IJS) during the early stages of drug court reduces drug use and sanctioning rates.*

***Method**: The study employed a non-blinded randomised controlled trial to test the effect of IJS on early-phase substance use and sanctioning rates. All participants accepted onto the Parramatta Drug Court program between March 2010 and March 2011 were randomly allocated into either an IJS or supervision as usual (SAU) condition. The IJS group had phase 1 of their program extended from three to four months and appeared before the judge two times per week during phase 1. The SAU group appeared once per week for three months during phase 1.*

***Results**: Participants in the IJS group were significantly less likely to return positive urinalysis tests and had a significantly greater number of episodes of abstinence than participants in the SAU group. IJS participants were less likely to accrue sanctions than participants in the SAU group. There was no significant difference in the odds of having sanctions waived or having to serve sanctions in prison between the two groups.*

***Conclusion**: These interim findings provide strong evidence that intensively supervising drug court participants in the early phases reduces early-phase substance use and sanctioning rates.*

***Keywords**: Drug court, intensive judicial supervision, substance use, randomised controlled trial.*

INTRODUCTION

DRUG COURTS

There is a strong association between illicit drug use and crime. Between 70 and 80 per cent of imprisoned offenders report using illicit drugs in the months leading up to their incarceration (Kevin, 2010). By comparison, representative population-based surveys find past-year prevalence estimates of around 12 per cent for females and 17 per cent for males (Australian Institute of Health and Welfare, 2011). These are crude comparisons but they illustrate the disparity in substance use patterns between imprisoned and non-imprisoned people. These patterns do not show that drug use *causes* offending and there is considerable evidence that the onset of offending often precedes the onset of drug use (Wish & Johnson, 1986). However, it is clear that rates of offending increase as levels of illicit drug use increase (Makkai & Payne, 2003; Wish & Johnson, 1986).

In recent decades, criminal justice administrators across many different jurisdictions have invested in programs that aim to break this drug-crime link. Perhaps no other program has been implemented more widely for this purpose than drug courts. The first drug court was introduced in Miami, Florida, in 1989. In the two decades that followed, the Miami court model was adapted and developed in a number of other states across the United States. As at February 2011, there were 2,193 drug courts operating across the U.S. and a further 208 were being planned (U.S. Justice Programs Office, 2011). Drug courts have also been taken up in a number of other countries, including Australia, Bermuda, Brazil, Canada, the Cayman Islands, England, Ireland, Jamaica, Mauritius, New Zealand, Scotland and Wales (United Nations Office on Drugs and Crime, 2010). Drug courts now operate in all states of Australia. New South Wales opened its second drug court in 2011 and the newly elected Government has announced plans to open a third.

This bulletin has been independently peer reviewed.

Drug courts vary from one jurisdiction to the next but they all share some common features that differentiate them from traditional courts. Drug courts require participants to engage with treatment providers to treat the underlying causes of their offending. Abstinence is monitored by frequent supervised urine testing. There is also ongoing judicial interaction with each participant to monitor progress and to provide any supports they might need to maintain abstinence (U.S. Office of Justice Programs, 1997). Drug courts usually also operate under a system where sanctions (e.g. short custodial episodes) are imposed for non-compliance, and rewards (e.g. waiving of accrued sanctions, positive reinforcement for sustained periods of abstinence) are given for pro-social behaviours.

There is now relatively strong evidence from international and Australian research that drug courts reduce recidivism (Latimer, Morton-Bourgon, & Chretien, 2006; Lind et al., 2002; U.S. Government Accountability Office, 2005). A randomised trial of the Parramatta Drug Court in NSW found reductions in some stealing and drug offences (Lind et al., 2002). A subsequent re-evaluation of this Court found much larger effect sizes (Weatherburn, Jones, Snowball, & Hua, 2008), although the possibility of selection bias could not be entirely ruled out of that follow-up study.[1] Meta-analyses, which summarise the findings from multiple evaluations to determine an overall effect size, estimate that drug courts produce reductions in recidivism of between 8 and 26 per cent (Latimer et al., 2006; Shaffer, 2011; U.S. Government Accountability Office, 2005; Wilson, Mitchell, & MacKenzie, 2006).

While these evaluations provide reason to be optimistic about drug courts, governments must consider not only whether they are effective but also whether they are cost-effective. The initial evaluation of the Parramatta Drug Court found the Court was marginally more cost-effective than prison. The per diem cost for an individual on the Drug Court was estimated to be $144, compared with $152 for those sanctioned in the usual way. Significant reforms have since been made to the program that are likely to have improved its cost-effectiveness. Goodall, Norman and Haas (2008) estimate that the Drug Court now provides a net saving of $1.8m per year relative to conventional sanctions. However, one factor that still contributes to the high cost of this and other drug courts is the rate at which participants are returned to custody at the end of the program. The imposition of sanctions (short episodes in custody) for non-compliance also contributes to the cost of the program. Around half (52%) of the total costs of the Drug Court are associated with post-program incarceration. A further 12 per cent of the costs can be attributed to sanctioning.[2] These costs are not unique to the Parramatta Drug Court (Butzin, Saum, & Scarpitti, 2002; Taxman & Bouffard, 2005; Turner et al., 2002). However, any efforts that reduce these costs are likely to improve the overall cost-effectiveness of the program.

Findings such as these have led researchers to ask a new set of questions about drug courts. The issue of *whether* drug courts work having largely been settled, a second generation of research is beginning to ask *how* they work, *for whom* they work, and *what program conditions* can be altered to improve outcomes for those most at risk of program failure (Goldkamp, White, & Robinson, 2001a, 2001b).

INTENSIVE JUDICIAL SUPERVISION

There is evidence emerging from the United States that intensive judicial supervision (IJS) may be one means of improving program outcomes. Marlowe and colleagues from the University of Pennsylvania began a series of experiments in 1999 where they systematically varied the level of supervision for clients of the Wilmington, Delaware, Drug Court. Importantly, these researchers used the most rigorous study designs (i.e. experiments using randomisation to high and low supervision conditions) to test the effect of additional judicial supervision. This methodological rigour provides grounds for high levels of confidence in the findings they observed.

The Delaware Drug Court deals with offenders convicted for misdemeanour crimes such as low-level drug offences. The program is scheduled to be a minimum of 14 weeks duration, although participants usually take at least six months to complete the program. The usual level of supervision on that program is one judicial status hearing every four to six weeks. In their first studies, Marlowe and colleagues randomly allocated consenting participants into bi-weekly or as-needed supervision groups. Because the usual supervision requirements were every four to six weeks, those in the bi-weekly condition attended more frequently than normal and those in the as-needed group attended less frequently than usual.

Overall, the researchers found no effect of increased supervision on any outcomes (Festinger et al., 2002; Marlowe, Festinger, Dugosh, & Lee, 2005; Marlowe, Festinger, Lee, et al., 2003). However, in separate analyses, Festinger et al. (2002) found that high-risk participants performed much better under bi-weekly supervision than under supervision on an as-needed basis. They defined high-risk participants as those who had a diagnosis of antisocial personality disorder, a prior history of drug treatment, or both. These results were subsequently replicated in two other misdemeanour courts (Marlowe, Festinger, & Lee, 2003). In a later series of studies, the researchers prospectively matched levels of supervision according to levels of identified risk and again found favourable effects of intensive supervision for high-risk participants. In these studies, participants were randomly allocated into matched or unmatched groups. In the matched group, high-risk participants were required to attend bi-weekly judicial status hearings and low-risk participants attended on an as-needed basis. The as-needed basis was less frequent than supervision as usual. In the unmatched group, participants were supervised once every four to six weeks as usual. High-

risk participants in the matched group had better outcomes than high-risk participants in the unmatched group. The two groups of low-risk participants had similar outcomes, which suggests that levels of supervision are unrelated to outcomes for low-risk participants. (Marlowe, Festinger, Dugosh, Lee, & Benasutti, 2007; Marlowe, Festinger, Lee, Dugosh, & Benasutti, 2006).

While this program of research suggests that IJS might be an effective means of improving both the effectiveness and cost-effectiveness of drug courts, this cannot merely be assumed to be true in all settings. Program requirements vary from one jurisdiction to the next, as do the characteristics of those taking part in drug court programs. Importantly, most of the work undertaken by Marlowe et al. has been undertaken in misdemeanour drug courts. In Australia, drug courts tend to be used as alternatives to prison. While Marlowe and colleagues provided a partial replication of their findings in two felony drug courts, the sample size was too small to draw firm conclusions about the effectiveness of IJS among felony offenders (Marlowe, Festinger, & Lee, 2004). There is no guarantee therefore that the same pattern of results could be expected in Australian drug courts.

Beginning in March 2010, the Parramatta Drug Court implemented a trial of IJS to determine whether it has the same effect in NSW as it appears to have had in the Delaware Drug Court. For a period of one year, all new Parramatta Drug Court participants were randomly allocated into either IJS or supervision as usual (SAU) conditions. Both groups received identical programs with the exception of their respective judicial supervision requirements and the length of time they were scheduled to remain in phase 1 of the program. Participants in the IJS group had phase 1 extended from three to four months and were required to report back to the judge two times per week during this phase. Participants in the SAU group were required to report back once per week during phase 1, which was scheduled for a usual minimum of three months.

THE CURRENT STUDY

The NSW Bureau of Crime Statistics and Research independently evaluated the trial and this report describes the interim results of that evaluation. The two research questions investigated in this interim report are:

1. Is there any evidence of a reduction in early-phase substance use among those allocated to the IJS condition relative to the SAU group?
2. Is there any evidence of a reduction in early-phase sanctioning rates among those allocated to the IJS condition relative to the SAU group?

The report is structured as follows. The next section briefly describes the operation of the Parramatta Drug Court. The subsequent section describes the methods employed to address the two research questions. The interim findings from the trial are then described. The final section discusses the implications of the findings and proposes some possible areas for future research.

THE PARRAMATTA DRUG COURT

A great deal has already been written about the Parramatta Drug Court and only the directly relevant features of the program are described here. Interested readers are referred to previous evaluation reports for further information about the operation of the court (Taplin, 2002; Weatherburn et al., 2008).

The program is an alternative to prison. Participants who are deemed to be eligible and suitable to take part in the Parramatta Drug Court program are required to pass through three phases of treatment (initiation/stabilization, consolidation, reintegration) before they are eligible to graduate. Participants are required to submit frequent supervised urine tests and report back to the Court regularly during each phase. Supervised urine testing is scheduled three times per week during phase 1, and twice weekly during phases 2 and 3. Participants are ordinarily required to report back to the Drug Court once each week during phase 1, once every two weeks during phase 2 and monthly during phase 3. The minimum time participants must spend on the program is 12 months, although program graduates are usually on the program for longer than this.

In the initial years of the Drug Court program, participants were given immediate short custodial sanctions if they tested positive to any substance. However, this system was difficult to administer in practice, mainly due to the processes involved in suspending participants' welfare payments for the short periods of time spent in custody. It was also very expensive. The Drug Court has since adopted a flexible system of sanctioning whereby participants can accumulate up to 14 sanction days before having to serve them. In accordance with the Court's philosophy of encouraging honesty and accountability, non-disclosed instances of drug use incur more severe sanctions than admitted episodes of use. More serious indiscretions, such as failing to attend counselling sessions or failing to attend court, can result in the imposition of immediate sanctions. Participants can also be rewarded with waived sanctions for sustained periods of abstinence.

The Drug Court observes three levels of graduation. Those who pass through each stage of treatment and remain abstinent for a specified period of time on phase 3 are deemed to be tier 1 (or 'gold medal') graduates. Those who substantially comply with the requirements of the program but who might still display occasional minor non-compliance with program requirements (e.g. one or two episodes of cannabis use) are deemed to be tier 2 (or 'silver medal') graduates. Those who are terminated from the program but who are deemed to have made sufficient progress to warrant non-custodial orders are deemed to be tier 3 (or 'bronze medal') graduates.

METHOD

DESIGN

The design of the study was an open, or non-blinded, randomised controlled trial. The trial is referred to as 'open' because neither the judicial officer nor the participants were blinded to the condition to which they had been allocated. Random allocation into intensive supervision and supervision as usual conditions was considered to be appropriate here because the court does not have the capacity to supervise every participant twice weekly during phase 1 of the program. In the absence of any other means of deciding who should be more intensively supervised, random allocation was deemed to be the fairest method of assignment. It has the additional benefit of counter-balancing all participant characteristics that might impact on program outcomes across the two groups. Any difference in outcomes between the two groups can quite confidently be ascribed to the level of judicial supervision they received.

PARTICIPANTS

Participants were the first 160 Parramatta Drug Court clients who had their matters stood over for initial sentence on or after 1 March 2010. The last participant was recruited into the study on 23 March 2011. While 160 participants were initially randomised into the study, 24 were subsequently withdrawn (n=14 IJS and n=10 SAU participants). Three of these 24 participants (n=2 IJS; n=1 SAU) were found to be ineligible after they had been stood over for sentence and were excluded from participation in the program. No outcome data were available for these three participants and they were excluded from all analyses. The remaining 21 participants (n=12 IJS; n=9 SAU) were treated in residential rehabilitation facilities and did not have any level of face-to-face judicial supervision. These 21 participants were excluded from the primary analyses in this report although, as described in the 'analyses' section below, intention-to-treat analyses were conducted to ensure that their exclusion did not bias the results. It should be noted, however, that eligibility screening and allocation into residential rehabilitation beds is completely unrelated to the supervision condition to which participants were assigned. Eligibility is strictly actuarial. Allocation to residential rehabilitation is decided according to the participants' level of need and the availability of beds in residential treatment facilities.

The final sample therefore consisted of 136 participants (n=66 IJS; n=70 SAU). The characteristics of the 136 participants are shown in Table 1 by the group to which they were assigned. The mean age of participants at program commencement was 32.2 years in the IJS group and 32.5 years in the SAU group. Most participants in both groups were male (84.9% male in the IJS group cf. 82.9% in the SAU group), non-Indigenous (86.4% IJS; 92.9% SAU) and treated in the community (84.9% IJS; 80.0% SAU). Approximately two-thirds of both groups were treated via pharmacotherapy for heroin dependence (69.7% IJS; 67.1% SAU). Most participants were convicted for break, enter and steal, or another property offence as their most serious offence (72.7% IJS; 64.3% SAU). Participants in the SAU group had slightly more concurrent offences dealt with at their index appearance than participants in the IJS group (median = 6.0 for the SAU group cf. 5.0 for the IJS group). The IJS group were initially sentenced to a suspended prison sentence of 14.4 months, on average, while participants in

Table 1. Characteristics of participants on IJS trial (n=136)

Characteristic	Group IJS (n=66)	Group SAU (n=70)	*p*-value
Age (mean years)	32.2	32.5	.861
Sex (%)			.753
Male	84.9	82.9	
Female	15.1	17.1	
Indigenous (%)			.213
Yes	13.6	7.1	
No	86.4	92.9	
Treatment type (%)			.459
Community-based	84.9	80.0	
Supported	15.1	20.0	
Treatment modality (%)			.749
Abstinence	30.3	32.9	
Pharmacotherapy	69.7	67.1	
Most serious index offence (%)			.393
Break, enter and steal	39.4	25.7	
Theft/fraud	33.3	38.6	
Driving	12.1	15.7	
Other	15.2	20.0	
Concurrent offences (median)	5.0	6.0	.089
Initial sentence (mean months)	14.4	15.0	.721
Courts appearances in 5 years prior to index (mean) [a]	5.1	4.7	.356
Number of court appearances resulting in prison in 5 years prior to index (%) [a]			.292
0	29.7	44.3	
1	31.3	27.1	
2	17.2	15.7	
3+	21.9	12.9	

[a] Prior criminal record was missing for two IJS participants

the SAU group were sentenced to 15 months. The number of prior finalised court appearances in the five years leading up to program commencement was similar across the two groups (mean = 5.1 for the IJS group cf. 4.7 for the SAU group), as was the number of court appearances resulting in a prison sentence (70.3% of the IJS group and 55.7% of the SAU group had received one or more prison sentences in the five years prior to commencement). Critically, there were no statistically significant differences between IJS and SAU participants on any of these characteristics, which indicates that adherence to the randomisation schedule was effective.

The comparability of the two groups on these measured covariates provides confidence that excluding the 21 participants treated in residential facilities did not create any imbalances in observed participant characteristics. While the possibility that there were imbalances in unobserved characteristics cannot be ruled out, similarity between the two groups on these measured covariates suggests that the risk is very low.

PROCEDURE

Participants are generally considered to have started the program at the point where their matters are stood over for initial sentence. This involves remanding participants in custody for a two-week period of detoxification and setting a court date at which to adjudicate the initial (suspended) prison sentence. The names of all participants who had their matters stood over for sentence were forwarded to the NSW Bureau of Crime Statistics and Research (BOCSAR). The researcher allocated each participant into the IJS or SAU group according to a randomised schedule. Participants were randomised within blocks of eight to ensure a balanced accrual into the respective conditions over time.

Once a participant had been assigned to the IJS or SAU conditions, the researcher advised all practitioners involved in the trial by return email of the group to which the participant had been assigned. Colour-coded stickers were attached to the participants' case files to identify those who had been assigned to the IJS group. Participants were advised at the outset if they were assigned to the IJS group. The nature of the trial was explained to participants in general terms and they were advised that they had been selected to be part of the trial. It was explained to them that the Judge would have liked to see everyone twice each week but that he only had time to see about half of all participants this frequently. It was explained that the Judge had to select half of the participants to report back more frequently and that the fairest way to do this was to select people at random. The participant was then advised of the requirement to see the Judge two times per week. The intensive supervision was generally viewed as a positive or supportive initiative and was well-received by participants.

Two substantive changes were made to the program conditions for participants who were allocated to the IJS group. The first was that phase 1 of their program was extended from the standard minimum period of three months to a minimum of four months. The second change was that they were required to see the judge two times per week (usually on a Monday and a Wednesday) instead of the usual one time during phase 1. The additional report-back to the Judge was arranged to occur on a day that the participants had to attend the registry to provide a supervised urine test, so as not to impose unreasonable travel demands on the IJS participants.

ADHERENCE TO GROUP ASSIGNMENT

Table 2 shows time spent on phase 1 of the program, the mean number of scheduled judicial report-backs per free week during phase 1 of the program, the mean number of judicial report-backs attended per free week during phase 1, and the rate at which participants failed to attend scheduled report-back per free week on phase 1. Participants in both groups spent approximately four months on phase 1 of the program (p=.918). While the IJS group were expected to spend more time on phase 1 than SAU participants, the non-significant difference in time spent on phase 1 reflects the fluid nature of transitions between phases on the program. It also means that any effect of IJS can be attributed to the level of judicial supervision and not to the fact that IJS participants had greater exposure to other phase 1 conditions (the greater number of supervised urinalysis tests, in particular).

On average, participants in the IJS condition were scheduled to attend 1.83 judicial report-backs per free week on phase 1. By comparison, those in the SAU group were scheduled to attend 1.07 judicial report-backs per free week. A one-way analysis of variance confirmed that this difference was statistically significant (p<.001). Participants in the IJS group also attended a significantly greater number of judicial report-backs than those in the SAU group (1.65 per free week cf. 0.79 per free week; p<.001).[3] There was a trend toward higher 'fail to attend' rates among SAU participants but this was not statistically significant (0.25 failures to attend per free week for the SAU group cf. 0.17 per free week for the IJS group; p=.092). This latter finding is particularly important because one of the risks of IJS is an increase in non-compliance due to an increase in the demands placed on participants. There was no evidence to suggest that this occurred.

As a final check on the fidelity of group assignment, Table 2 also shows the number and proportion of participants within each group who received the allocated level of supervision. The number of scheduled report-backs was rounded to the nearest whole integer for this comparison. It can be seen that most IJS participants (88.9%) were scheduled to report back twice per free week on phase 1 as intended. Similarly, most of those assigned to the SAU condition (91.2%) were scheduled to report

Table 2. Adherence to group assignment fidelity

	Group [a]		
	IJS (n=63) Mean (SD)	SAU (n=68) Mean (SD)	*p*-value
Number of days on phase 1 [b]	129.71 (55.26)	130.87 (74.42)	.918
Scheduled report-backs	1.83 (0.33)	1.07 (0.27)	<.001
Attended report-backs	1.65 (0.53)	0.79 (0.35)	<.001
Failed to attend report-backs	0.17 (0.25)	0.25 (0.28)	.092
Scheduled report-backs	N (% within group)	N (% within group)	<.001
0	0 (0.0%)	2 (2.9%)	
1	6 (9.5%)	62 (91.2%)	
2	56 (88.9%)	4 (5.9%)	
3	1 (1.6%)	0 (0.0%)	

[a] Five participants spent less than three weeks on the program and a reliable estimate of their attendances per free week on the program could not be calculated.

[b] All 136 participants were included in this comparison. Time on phase 1 was counted as the time between program commencement and either termination or progression to phase 2.

back once per free week on average. A manual check of the records for those who appeared to have been assigned to the incorrect level of sanctioning indicated that all but one participant had indeed been correctly assigned to their respective groups. The apparent incorrect assignment arose partly because some participants spent very little time on the program and partly through mismeasurement of the exposure (free-time) variable. This latter problem arose because time spent at-large or in custody beyond the length of formal sanctions could not be readily identified from the drug court database. The one participant who was deliberately and incorrectly assigned to the wrong condition was assigned to the SAU condition but was deemed to be too high-risk to supervise only once per week. This participant was therefore scheduled to appear before the Judge twice per week during phase 1.

Collectively, these findings suggest that the Drug Court adhered very closely to the group assignment and that participants in the IJS condition received a greater 'judicial dose' than those in the SAU group. IJS participants were scheduled to report-back to the court 71 per cent more often than the SAU group during phase 1. IJS participants attended report-backs about twice as often as SAU participants.

DATA SOURCE

All data reported in this bulletin were sourced from the NSW Drug Court database. This database is administered by the NSW Judicial Commission and the Bureau receives regular downloads of data for its criminal courts publication. Among other things, the database records court appearance details, supervised urine test outcomes and sanctions accrued while on the program. The data employed for this study were current to 11 July 2011.

VARIABLES

Dependent variables

The two groups of dependent variables in this study were the outcomes of each supervised urinalysis episode and whether sanctions were issued at a given court appearance.

At the time the data were extracted, the 136 participants in the IJS trial had undergone a total of 9,534 supervised urinalysis episodes. Five separate measures of urinalysis outcomes were constructed for each of these test episodes:

1. Episodes of use (*use*): defined as '1' if the participant tested positive[4] to any drug, admitted any drug use to the court, failed to attend a scheduled drug test and/or failed to provide a urine sample at that test episode, and '0' otherwise;
2. Positive to any drug (*p_any*): defined as '1' if the participant tested positive[5] to any drug at that episode, and '0' otherwise;
3. Positive to any hard drug (*p_hard*): defined as '1' if the participant tested positive to 6-acetylmorphine, amphetamine, cocaine, monoacetylmorphine, or an opiate other than methadone at that episode, and '0' otherwise;
4. Admitted any drug (*a_any*): defined as '1' if the participant admitted use of amphetamine, benzodiazepines, cocaine, heroin/opiates, (illicit) methadone, cannabis or another drug at that episode, and '0' otherwise; and
5. Admitted hard drug (*a_hard*): defined as '1' if the participant admitted using amphetamine, cocaine, heroin or another opiate at that episode, and '0' otherwise.

The imposition or removal of sanctions was observed per free week on the program. At the time the data were extracted,

the 136 participants had contributed 3,558 free person-weeks on the program. Three separate sanctioning measures were constructed:

1. Sanctions accrued per week (*accrued*): was a binary outcome indicating whether sanctions were issued to the participant during that week. This variable took the value '0' if no sanctions were accrued during the week and '1' if one or more sanction days were accrued;
2. Sanctions issued to serve per week (*served*): was a binary outcome indicating whether sanctions were issued to serve during that week (i.e. a participant was ordered to spend a short period of time in custody). This variable took the value '0' if no sanctions were issued to serve during the week and '1' if sanction days were issued to serve; and
3. Sanctions waived per week (*waived*): was a binary outcome indicating whether any sanctions were waived during that week. This variable took the value '0' if no sanctions were waived during the week and '1' if one or more sanction days were waived.

Independent variables

The primary independent variable employed in the current study was the group to which participants had been assigned (IJS or SAU). It was not necessary to adjust for most of the other participant characteristics in the main analyses because the randomisation effectively balanced the participants on all characteristics that might impact on their drug use and sanctioning outcomes. The one exception was concurrent offences. Table 1 shows that participants in the IJS group had slightly fewer concurrent offences than those in the SAU group. While this difference was not statistically significant in the as-treated analysis (p=.089, see Table 1), it was statistically significant when the groups were compared on an intention-to-treat basis (p=.035, see Appendix Table A1). A five-category (1-2, 3-4, 5-6, 7-12, 13+) concurrent offence variable was therefore created and adjusted for as described in the 'sensitivity analysis' section below.

ANALYSES

It was hypothesised that participants in the IJS group would have more favourable patterns of early-phase substance use and sanctioning rates than participants in the SAU group. The following specific hypotheses were tested in the current study:

1. Participants allocated to the IJS group will return a smaller proportion of 'positive' drug tests overall than participants in the SAU group;
2. Participants allocated to the IJS group will have a lower odds of returning a 'positive' drug test at each occasion of testing than participants in the SAU group;
3. Participants allocated to the IJS group will have significantly longer periods of abstinence than participants in the SAU group;
4. Participants allocated to the IJS group will have lower odds of accruing sanctions for drug use and other disallowed behaviours than participants in the SAU group;
5. Participants allocated to the IJS group will have lower odds of serving periods of time in custody as a result of drug use and other disallowed behaviours than participants in the SAU group; and
6. Participants allocated to the IJS group will have greater odds of having previously accrued sanctions waived as a result of pro-social behaviours than participants in the SAU group.

To test the first hypothesis, chi-square tests were conducted to determine whether there was any difference between groups in the proportion of urinalysis tests showing evidence of recent drug use (by each measure of drug use listed in the 'dependent variables' section). This analysis was conducted in SAS v9.2.

To test the second hypothesis, a random effects panel model was utilised to account for both the within-participant clustering of drug test outcomes and the between-participant variation in drug test outcomes. In this second analysis, each participant was treated as a panel and each of their urinalysis outcomes was treated as the dependent variable (0=no evidence of recent use, 1=evidence of recent use). The panel model estimates the odds of returning a positive test at each urinalysis episode for the IJS group relative to the SAU group. This second analysis was conducted in Stata v11 using the xtlogit procedure.

To test the third hypothesis, the maximum number of episodes of abstinence observed by each participant in the trial was calculated. The mean of these abstinence episodes was then calculated within each group and a one-way analysis of variance was used to test whether there was any difference between the groups in the maximum period of abstinence. This analysis was conducted using SAS v9.2.

To test the fourth hypothesis, the proportion of weeks on which participants accrued sanctions was compared across the two groups. A random effects panel model was then utilised to estimate whether there was any difference in the odds of accruing sanctions during each free week on the program. Each participant was again treated as a panel and the variable *accrued* was the dependent variable. The xtlogit procedure in Stata v11 was again used to estimate sanctioning rates for IJS participants relative to the SAU group. Weeks during which participants were in custody or otherwise absent from the program were treated as missing and were dropped from this analysis.

The methods used to test hypotheses five and six were the same as that employed to test hypothesis four, except the variables *served* and *waived* were treated as the dependent variables.

SENSITIVITY ANALYSES

Sensitivity analyses were conducted in order to test whether removal of the 21 people who were treated in residential rehabilitation introduced any bias into the sample. Each of the analyses above were repeated with these 21 participants included in the sample. The results of this pure intention-to-treat (ITT) analysis were very similar to the results with these 21 removed. All ITT analyses are reported in the Appendix.

Sensitivity analyses were also conducted to assess whether the small differences in concurrent offence counts between the two groups had any impact on the analyses. The random effects panel models used to assess hypotheses 2, 4, 5 and 6 were repeated adjusting for the participants' concurrent offence profile. The resulting adjusted odds ratio estimates were very similar after adjusting for concurrent offences. These odds ratios are reported in Tables A4 and A5 of the Appendix.

RESULTS

SUBSTANCE USE

Figure 1 shows the proportion of tests within each group where there was evidence of recent substance use, presented by each measure of substance use and by group. By each measure, the IJS group returned significantly fewer tests showing evidence of substance use. For example, the measure '*use*' indicates that SAU participants tested positive to one or more drugs, admitted use, failed to attend or failed to provide a urine sample in 25.2 per cent of all tests provided by members of that group. By comparison, 17.4 per cent of tests provided by participants in the IJS group were positive according to this definition. The trend was the same across each of the other four measures of substance use. These differences were highly statistically significant (p<.001 on each measure).

Table 3 shows the estimated odds of returning a 'positive' urinalysis test at any urinalysis test, by the group to which participants had been assigned. Odds ratios less than 1 in these analyses indicate that the IJS group had lower odds of testing 'positive' to illicit drug use on each occasion of urinalysis testing. Odds ratios greater than 1 would indicate that the IJS group had increased odds of testing positive relative to SAU participants. Odds ratios close to or equal to 1 would indicate no significant difference between groups in their odds of testing positive. The effects shown in Table 3 were all large, all in the same direction and all statistically significant across the five measures of drug use. In each case, the odds of an IJS participant returning a test showing evidence of recent drug use were close to half that of the SAU participants. The effects were similar when estimated on a purely ITT basis (see Table A2 in the Appendix) and after adjusting for concurrent offences (see Table A3 in the Appendix).

Table 3. Odds of returning a urinalysis test showing evidence of recent drug use, by group (n=9,534)

Measure	Group	Odds ratio (95% CI)	*p*-value
Use	IJS	0.56 (0.35,0.89)	.014
	SAU	-	
P_any	IJS	0.58 (0.34,1.00)	.05
	SAU	-	
P_hard	IJS	0.45 (0.23,0.88)	.019
	SAU	-	
A_any	IJS	0.58 (0.36,0.94)	.025
	SAU	-	
A_hard	IJS	0.55 (0.32,0.93)	.025
	SAU	-	

Note. CI = confidence interval

Figure 2 shows the maximum number of episodes of abstinence averaged across members within each of the two supervision groups. The average number of episodes of abstinence was higher for the IJS group relative to the SAU group on each of the five measures of substance use. For example, on the measure '*use*', participants in the IJS group had an average maximum of 29.8 urinalysis test episodes without returning a positive value on this measure during their time on the program. The average maximum number of episodes of abstinence was 18.6 for participants in the SAU group. These differences were statistically significant on three of the five measures (*use, p_hard, a_hard*) and close to the conventional level of statistical significance on the other two measures (p=.053 for *p_any*; p=.056 for *a_any*).

SANCTIONS

At the time the data were extracted for this report, participants in the trial had contributed 3,558 person-weeks at which they appeared in court on one or more occasions. Sanction days were accrued by one or more participants on 1,295 (36.4%) of these weeks. The number of weeks on which sanctions were issued to serve (n=189, 5.3%) and the number of weeks on which sanctions were waived (n=484, 13.6%) were both much lower. Figure 3 shows the proportion of weeks on which sanctions were accrued, served and waived by the group to which participants had been assigned. Participants in the SAU group accrued sanctions on a significantly greater proportion of weeks than participants in the IJS group (sanctions were accrued on 41.6% of person-weeks for the SAU group cf. 31.4% for the IJS group; p<.001). SAU participants also had sanctions issued to serve on a greater proportion of weeks, although this difference was not statistically significant (5.9% for SAU cf. 4.7% of weeks for the IJS group; p=.103). IJS participants were slightly more likely to have sanctions waived for positive behaviours than

Figure 1. Percentage of tests showing evidence of recent drug use, by group

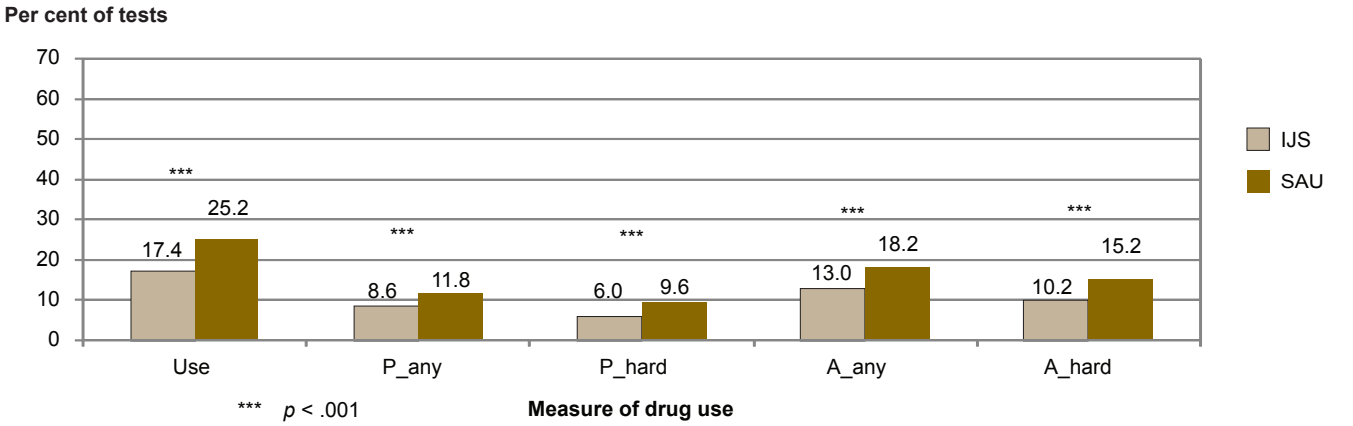


Figure 2. Average maximum episodes of abstinence, by group

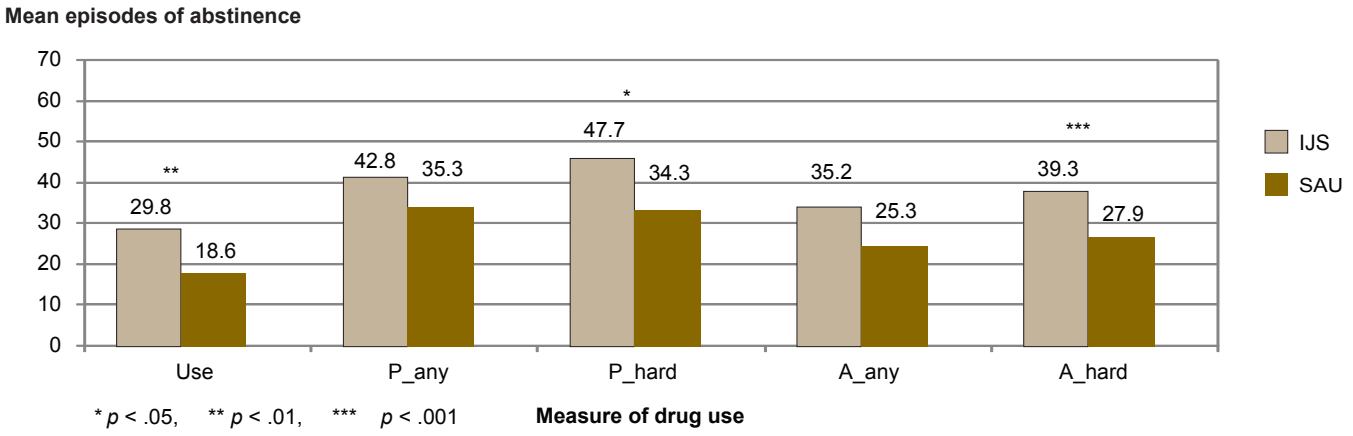
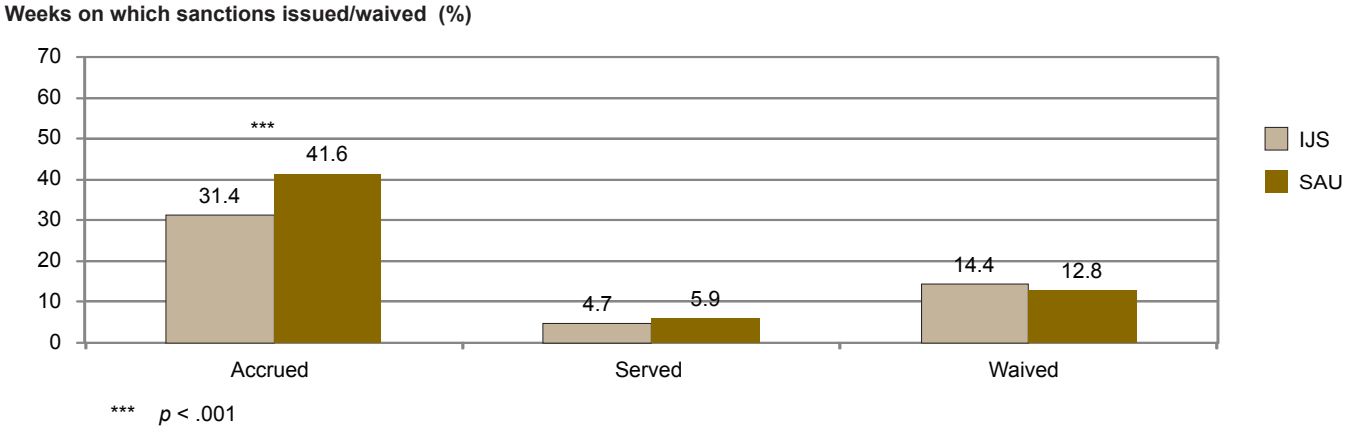


Figure 3. Proportion of weeks on which sanctions were accrued, served or waived, by group



participants in the SAU group, although this difference was also not statistically significant (14.4% of weeks for the IJS group cf. 12.8% of weeks for the SAU group; *p*=.175).

Table 4 shows the results of three random effects panel models estimating the probability of accruing sanctions, the probability of having sanctions issued to serve and the probability of having previously-accrued sanctions waived per free week on the program. As with the results shown in Figure 3, there was a statistically significant difference between groups in the likelihood of accruing sanctions (OR=0.58, *p*=.004) but no significant difference between groups in the odds of having sanctions issued to serve (OR=0.75, *p*=.167) or having sanctions waived (OR=1.21, *p*=.329). The effects were similar when estimated on a purely ITT basis (see Table A4 in the Appendix) and after adjusting for concurrent offences (see Table A5 in the Appendix).

Table 4. Odds of having sanctions accrued, served and waived per free week on the program, by group (n=3,558)

Measure	Group	Odds ratio (95% CI)	*p*-value
Accrued	IJS	0.58 (0.40,0.84)	.004
	SAU	-	
Served	IJS	0.75 (0.50,1.13)	.167
	SAU	-	
Waived	IJS	1.21 (0.82,1.79)	.329
	SAU	-	

Note. CI = confidence interval

DISCUSSION

The early signs from this trial are very encouraging. Strong support was found for hypotheses 1 to 3, whereby those assigned to the IJS condition were much less likely to use illicit drugs by any measure than those under supervision as usual. SAU participants returned between 37 and 60 per cent more 'positive' urinalysis tests than participants in the IJS group, depending on which measure of illicit drug use was observed. The odds of an IJS participant returning a urine test result that indicated use of any drug were approximately half that of a participant in the SAU group. The mean number of occasions of abstinence were between 21 and 60 per cent higher for IJS participants relative to SAU participants, again depending on which measure of illicit drug use is observed. Hypothesis 4 was also supported, whereby the proportion of weeks on which IJS participants accrued sanctions was 25 per cent lower than the proportion of weeks on which SAU participants accrued sanctions. While the direction of the relationship between group membership and having sanctions issued to serve (hypothesis 5) and having sanctions waived (hypothesis 6) appeared favourable for IJS participants, these differences were not statistically significant.

These results are in contrast to those found by Marlowe et al. in the early part of their program of research. Their initial studies, which compared IJS and supervision on an as-needed basis found no overall benefit from intensive supervision (Festinger et al., 2002; Marlowe et al., 2005; Marlowe, Festinger, Lee, et al., 2003). The reasons for this discrepancy are not clear but it is likely that differences in the characteristics of participants on the respective programs at least played a part. The Delaware Drug Court, which was the setting for most of Marlowe's studies, deals with misdemeanour drug offences such as use or possession of drugs, or possession of drug paraphernalia (Festinger et al., 2002). Participants in the Parramatta Drug Court are convicted of more serious offences such as burglary, fraud and other theft offences. The Marlowe et al. program of research has repeatedly demonstrated that IJS is more effective for high-risk participants, who they define as having a diagnosis of anti-social personality disorder and/or a history of treatment. It is possible that there is a much greater representation of participants in the Parramatta Drug Court who meet these criteria. In other words, it is possible that most of the Parramatta Drug Court participants are high-risk, which explains why there was an overall net benefit of IJS for participants in this trial.

These results provide enough evidence to suggest that the Drug Court should increase levels of supervision during the early stages of the program. The Parramatta Drug Court has, in fact, already implemented this change in policy. All new entrants to the Court are now supervised twice per week for the first six weeks of the program and levels of supervision are decreased thereafter only if participants are showing sustained periods of program compliance. While this is certainly a recommended approach based on the evidence presented in this report, these supervision requirements are quite onerous on Court time. An issue for future research, therefore, is whether there are any participant characteristics that might predict who is more likely to respond to this intensive supervision. On the basis of Marlowe et al.'s findings (Festinger et al., 2002; Marlowe et al., 2007; Marlowe, Festinger, & Lee, 2003; Marlowe et al., 2004; Marlowe et al., 2006), markers of participant risk would be a useful starting point. Precisely which markers of risk are the most important is still very much an open question. It is not clear, for example, why antisocial personality disorder and having a longer treatment history might make participants more amenable to higher levels of judicial supervision. These questions should be unpacked in future research.

Also an issue for future research is to assess whether IJS has any long-term impact on the rate at which sanctions are issued to serve and waived. Sanctions were only issued to serve on around one in 20 person-weeks, and waived on around one in seven weeks. The relative rarity of these outcomes provides little

statistical power to pick up small differences in these outcomes across the two groups. It will also be important for future research to assess whether there is any difference between the two groups in retention in treatment, likelihood of progressing to later program stages, graduation rates and re-offending. It is too early to estimate the effect of IJS on these outcomes because a significant proportion of participants are still on the program. The sample size may ultimately prove to be too small to estimate some of these effects, unless the effect sizes turn out to be very large. This was not an issue of poor study design but rather a consequence of having access to reliable repeated measurements of drug use and sanctioning data. There is a strong relationship between frequency of early-phase substance use, and program and re-offending outcomes (Jones & Kemp, 2011). There is good reason to believe, therefore, that participants in the IJS trial will have better outcomes than those in the SAU condition, even if these outcomes cannot be directly measured. It would have been unethical to continue the trial in light of these positive findings just to attain a large enough sample to estimate its impact on treatment retention and re-offending. Future research will nevertheless explore whether there is any relationship between IJS and these outcomes.

While this study provides very robust evidence that IJS improves outcomes for drug court participants, it is not without limitations. One potential limitation is that there were other key differences between the IJS and SAU groups that could potentially have impacted on outcomes. While not so much a weakness of the study, it is important to be clear that the term 'intensive judicial supervision' is really a shorthand description of the intervention. Other team members, such as the defence and prosecution solicitors and the clinical nurse consultant, often play an active role in the report-back as well. The IJS effect may therefore be as much due to the interaction with the judicial officer as it is to the interaction with other team members. However, the interaction with the judicial officer is considered to be a particularly important feature of drug courts. The supervision relationship is hypothesised to increase retention in treatment at least in part by "communicat[ing] to participants—often for the first time—that someone in authority cares about them and is closely watching what they do" (U.S. Office of Justice Programs, 1997, p.15).

Another (notional) difference between IJS and SAU groups is that participants in the IJS group also had phase 1 of their program extended from three to four months while participants in the SAU condition were scheduled to a minimum of three months on phase 1. Had this extension played out, it would have introduced the possibility that other differences between phase 1 conditions could have played a role in bringing about the observed outcomes. The most obvious factor is the frequency of supervised urine testing, which occurs three times per week during phase 1 and bi-weekly in phase 2. As it happened, there was no difference in time spent on phase 1 between the IJS and SAU participants (see Table 2). We can be very confident, therefore, that the observed effects are due to the frequency of supervision and not frequency of urinalysis testing.

There is one more substantial cautionary note about the results of this trial and that relates to the fact that it was a non-blinded randomised trial. In medicine, where researchers might be comparing the effect of a new drug relative to a placebo on some outcome, the optimal means of measuring the effect of the drug is to blind both the participant and the physician to the treatment to which they have been assigned. This reduces the possibility of bias due to differential interactions between physician and patient according to the condition to which they have been assigned. It was clearly not possible to blind participants and the team to the condition to which participants had been assigned in this study. In fact, differential interactions between IJS participants and the drug court team was precisely the effect we were trying to estimate.

The non-blinded nature of this trial does, nevertheless, introduce two possible sources of bias. First, it is possible that the Judge might have been (subconsciously) more or less inclined to impose sanctions on participants in the IJS group than those in the SAU group. While possible, this is unlikely given that the Drug Court has a published policy on sanctioning and this policy is well known to both the team members and the participants. In fact, the Drug Court team tends not to discuss sanctions at regular team meetings as they all know exactly what sanctions will flow from any breaches of program conditions. Any failure to impose the expected sanctions would be a clear departure from what everyone would expect to happen and it is very unlikely that this would have occurred without any team member or participant noticing. Furthermore, even if this bias did exist, the difference in urinalysis outcomes provides strong independent evidence that IJS improves program compliance.

The second source of bias that cannot be ruled out is what is known as the "Hawthorne effect". This refers to the possibility that IJS participants might have responded so positively simply because it was clear to them that they were being studied. If this were the case, it would diminish the effectiveness of IJS when incorporated into regular drug court procedures. While this possibility cannot be discounted, anecdotal evidence suggests that the risk is likely to be minimal in this study. Participants were informed at the outset that they had been selected to take part in the trial but they had no interaction with the researchers at all during their time on the program and the fact that they were even part of the IJS trial was not something that was particularly salient during their time on the program. After an initial realisation that they were part of a trial, reports suggest that the level of judicial supervision was very much like supervision as usual for both groups.

In summary, the initial results of this trial suggest that intensive judicial supervision is a very effective means of encouraging compliance with the Parramatta Drug Court program. The fact that this evidence arises from a randomised trial provides a great deal of confidence that the effects are caused by the IJS intervention and not by some unmeasured factor or set of factors. It will be important for future research to uncover the mechanisms that give rise to this IJS effect. For example, it is not clear at present whether the IJS effect is due to greater exposure to the judge and the other team members, or whether it might be explained by changes in the perceived risk or celerity of apprehension for non-compliant behaviour. Likewise, it is not clear from this study whether IJS is more effective for people who present with particular characteristics. For example, given that IJS brings forward the rewards and sanctions that are so critical to drug courts (U.S. Office of Justice Programs, 1997), IJS may be more effective for participants who present with greater levels of impulsivity, for whom a week is a very long time. Understanding these sorts of interactions will be critical to the development of the future strategies to improve drug court policies and procedures. It will also be important for future research to identify the long-term impact of IJS on program outcomes and re-offending.

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NOTES

1 By selection bias, we mean that there may have been systematic differences between the Drug Court and comparison groups in that study, which may have led to misleading estimates of the impact of the program on rates of re-offending.

2 Partly in an attempt to ameliorate the latter of these costs, the Parramatta Drug Court introduced a system of graduated sanctioning. Under this scheme, participants can accrue 14 sanction days before having to spend that time in custody. They also have the opportunity to earn back sanctions already accrued for sustained periods of abstinence and other pro-social behaviours. While the issue of cost-effectiveness has not been conclusively re-examined subsequent to these changes, graduated sanctions have significantly reduced Drug Court costs (Goodall et al., 2008) and are likely to have improved the cost-effectiveness of the court.

3 The denominator for these estimates was time between the participants' first scheduled judicial report-back and either the end of phase 1 or program termination. Any sanctions that had been issued to serve were then subtracted from this time to calculate the number of free weeks on phase 1. This calculation underestimates the true rate at which report-backs were scheduled and attended because it takes no account of time during which participants may have absconded from the program. This could not be subtracted from the measure of exposure time with any degree of accuracy.

4 A positive result was deemed to be one where a participant returned a positive test for 6-acetylmorphine, alprazolam, amphetamine, cannabis, clonazepam, cocaine, diazepam, flornitrazepam, monoacetylmorphine, 3,4-methylenedioxymethamphetamine, methylamphetamine, nordiazepam, an opiate other than methadone, oxazepam, temazepam, or another proscribed drug. A positive test for cannabis was only recorded where the THC level exceeded the level on the preceding urinalysis test to avoid false-positives due to residual traces of THC from previous tests.

5 A positive result was defined as above.

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APPENDIX

The following Appendix Tables give the results of sensitivity analyses addressing sources of bias in the analyses reported in main body of the report. Table A1 shows the participant characteristics when the 21 participants who were treated in residential rehabilitation facilities were included in the sample. Table A2 shows the estimated odds of returning a positive drug test on an ITT basis. Table A3 shows the estimated odds of returning a positive drug test after adjusting for concurrent offences. Table A4 shows the estimated odds of having sanctions accrued, served and waived on an ITT basis, while Table A5 shows these odds after adjusting for concurrent offences. Figures A1 to A3 show, respectively, the proportion of positive tests on an ITT basis, the average maximum days of abstinence on an ITT basis, and the proportion of weeks on which sanctions were accrued, served and waived on an ITT basis. In each of these sensitivity analyses, the results are similar to those shown in the main body of the report.

Table A1. Characteristics of participants on IJS trial on purely intention-to-treat basis (n=157)

	Group		
Characteristic	**IJS (n=78)**	**SAU (n=79)**	***p*-value**
Age (mean years)	31.6	32.6	.432
Sex (%)			.969
Male	80.8	81.0	
Female	19.2	19.0	
Indigenous (%)			.190
Yes	14.1	7.6	
No	85.9	92.4	
Treatment type (%)			.580
Residential rehabilitation	15.4	11.4	
Community-based	71.8	70.9	
Supported	12.8	17.7	
Treatment modality (%)			.953
Abstinence	35.9	35.4	
Pharmacotherapy	64.1	64.6	
Most serious index offence (%)			.189
Break, enter and steal	41.0	25.3	
Theft/fraud	33.3	40.5	
Driving	12.8	13.9	
Other	12.8	20.3	
Concurrent offences (median)	5.0	6.0	.035
Initial sentence (mean months)	14.0	14.3	.804
Courts appearances in 5 years prior to index (mean) [a]	5.1	4.7	.356
Number of court appearances resulting in prison in 5 years prior to index (%) [a]			.585
0	33.8	43.0	
1	29.7	29.1	
2	17.6	15.2	
3+	18.9	12.7	

[a] Prior criminal record was missing for four IJS participants

Table A2. Odds of returning a urinalysis test showing evidence of recent drug use on a purely intention-to-treat basis, by group (n=10,570)

Measure	Group	Odds ratio (95% CI)	*p*-value
Use	IJS	0.59 (0.38,0.93)	.023
	SAU	-	
P_any	IJS	0.64 (0.38,1.08)	.095
	SAU	-	
P_hard	IJS	0.45 (0.24,0.88)	.019
	SAU	-	
A_any	IJS	0.60 (0.37,0.96)	.034
	SAU	-	
A_hard	IJS	0.55 (0.33,0.93)	.026
	SAU	-	

Note. CI = confidence interval

Table A3. Odds of returning a urinalysis test showing evidence of recent drug use after adjusting for concurrent index offence count, by group (n=9,534)

Measure	Group	Adjusted odds ratio (95% CI)	*p*-value
Use	IJS	0.54 (0.34,0.88)	.013
	SAU	-	
P_any	IJS	0.57 (0.33,0.99)	.046
	SAU	-	
P_hard	IJS	0.48 (0.24,0.96)	.038
	SAU	-	
A_any	IJS	0.60 (0.37,0.97)	.039
	SAU	-	
A_hard	IJS	0.59 (0.34,1.02)	.057
	SAU	-	

Note. CI = confidence interval

Table A4. Odds of having sanctions accrued, served and waived per free week on the program on a purely intention-to-treat basis, by group (n=3,973)

Measure	Group	Odds ratio (95% CI)	*p*-value
Accrued	IJS	0.63 (0.43,0.91)	.013
	SAU	-	
Served	IJS	0.82 (0.57,1.17)	.270
	SAU	-	
Waived	IJS	1.15 (0.77,1.71)	.486
	SAU	-	

Note. CI = confidence interval

Table A5. Odds of having sanctions accrued, served and waived per free week on the program after adjusting for concurrent index offence count, by group (n=3,558)

Measure	Group	Adjusted odds ratio (95% CI)	*p*-value
Accrued	IJS	0.60 (0.41,0.89)	.010
	SAU	-	
Served	IJS	0.73 (0.48,1.12)	.150
	SAU	-	
Waived	IJS	1.30 (0.87,1.94)	.201
	SAU	-	

Note. CI = confidence interval

Figure A1. Percentage of tests showing evidence of recent drug use on purely intention-to-treat basis, by group

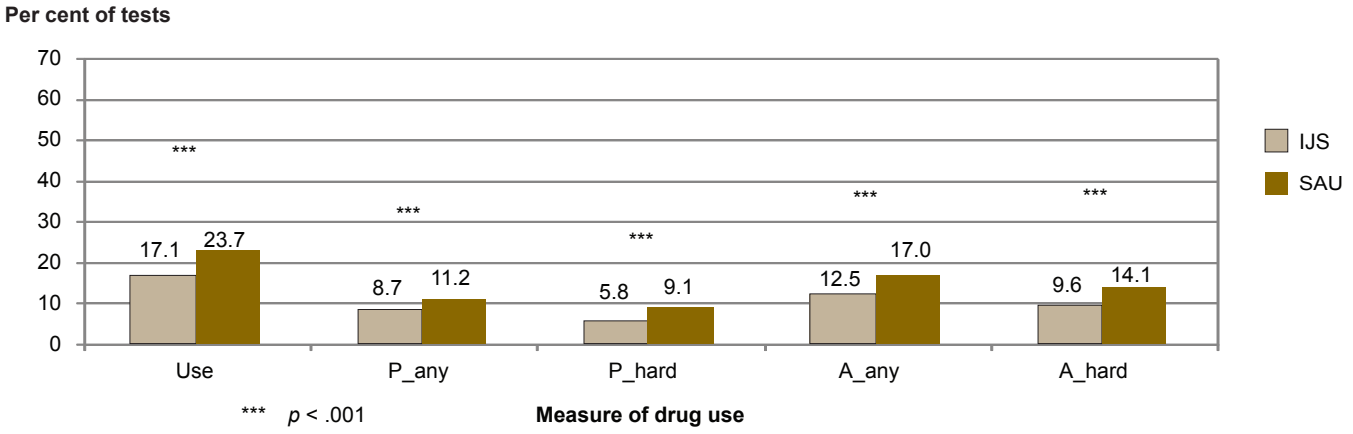


Figure A2. Average maximum episodes of abstinence on purely intention-to-treat basis, by group

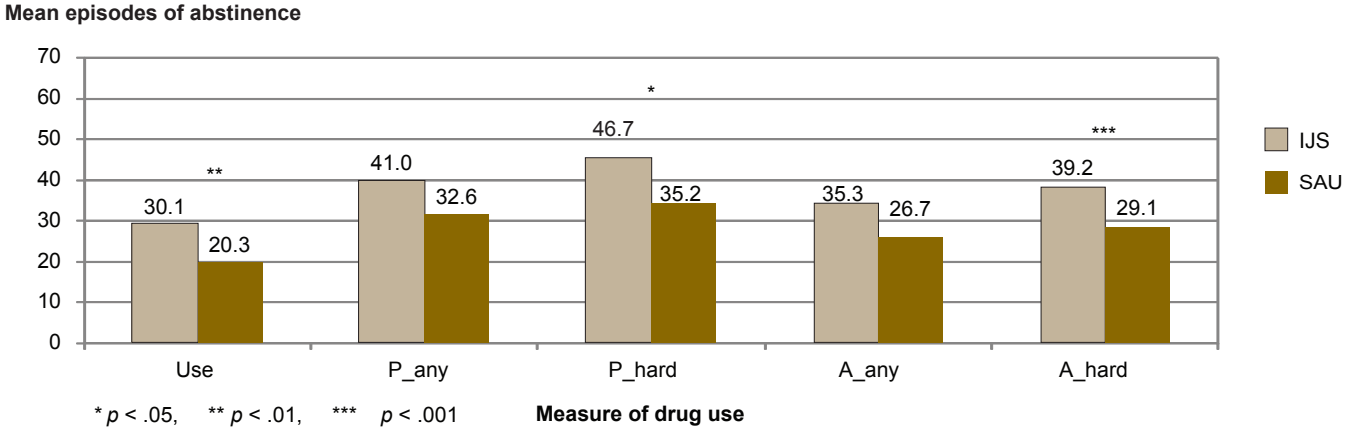
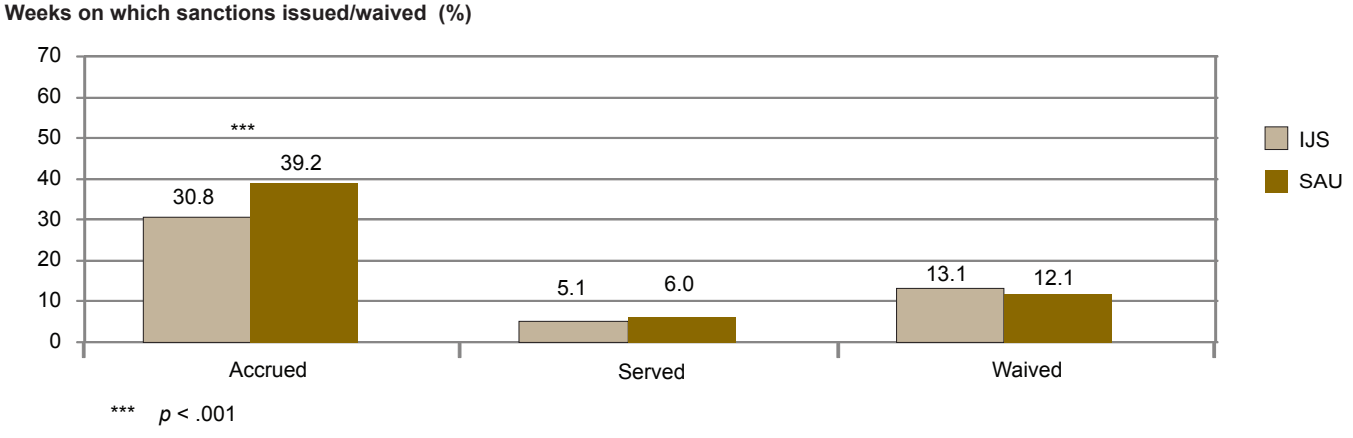


Figure A3. Proportion of weeks on which sanctions were accrued, served or waived on purely intention-to-treat basis, by group



NSW Bureau of Crime Statistics and Research - Level 8, St James Centre, 111 Elizabeth Street, Sydney 2000
bcsr@agd.nsw.gov.au • www.bocsar.nsw.gov.au • Ph: (02) 9231 9190 • Fax: (02) 9231 9187
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