ARTICLE: The Relationship Between Delay Discounting, Judicial Supervision, and Substance Use Among Adult Drug Court Clients

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BIO: We thank Don Weatherburn for supervising this research and Wai-Yin Wan for helpful guidance with statistical analyses. We also thank Roger Dive and all of the Drug Court team for supporting the research, and Corrective Services New South Wales for supporting access to LSI-R data. In particular, we thank Pat Mendham, Filiz Eminov, and Jaklin Naimi for assistance with data collection and all the participants who agreed to complete our questionnaire. Craig G. A. Jones, School of Psychology, University of New South Wales, Sydney, New South Wales, Australia, and New South Wales Bureau of Crime Statistics and Research, Sydney, New South Wales, Australia; Richard I. Kemp, School of Psychology, University of New South Wales, Sydney, New South Wales, Australia; Jennifer S. K. Chan, School of Mathematics and Statistics, University of Sydney, New South Wales, Australia.

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LEXISNEXIS SUMMARY:
... Jones (2013) conducted tests to identify whether there was any interaction between supervision and the variables available in that study (e.g., prior criminal record, age, sex) and found no evidence of any interaction with those variables. ... Domain scores and total LSI-R scores are calculated by summing across the identified risk factors within each risk domain. *Table 1 *Characteristics of Intensive Judicial Supervision (IJS) *and Supervision as Usual (SAU) Participants Who Completed the *Questionnaire (n = 93) and Who Had an Eligible Level of Service Inventory--Revised (LSI-R; n = 111) *Questionnaire *LSI-R IJS SAU IJS SAU Characteristic (n = 46) (n = 47) (n = 50) (n = 61) Male (%) 87.0 83.0 82.0 82.0 Age(M) 33.2 32.6 32.3 31.8 Aboriginal Australian (%) 8.7 8.5 10.0 11.5 In pharmacotherapy (%) 69.6 70.2 74.0 73.8 Drug of dependence (%) Alcohol 15.2 8.5 14.0 6.6 Amphetamine 34.8 27.7 38.0 26.2 Benzodiazepines 28.3 19.2 28.0 24.6 Cannabis 54.4 53.2 52.0 45.9 Cocaine 10.6 16.0 9.8 Heroin 67.4 76.6 74.0 83.6 Index offence (%) Break and enter 34.8 25.5 32.0 24.6 Driving 10.9 14.9 Theft 32.6 44.7 32.0 45.9 Other 21.7 14.9 26.0 16.4 Concurrent offences (M) 7.8 13.0 8.4 10.4 Sentence (M years) 1.2 1.0 1.2 1.0 Days to questionnaire (mean) 93.6 92.7 Discount rate (M k) 0.032 0.038 Treatment history (%) 87.2 80.4 -- -- Days to LSI-R (M) -- -- 201.1 150.4 On-program LSI-R (%) -- -- 62.0 54.1 LSI-R score (M) 29.6 30.8 Criminal history (10) -- -- 7.0 6.9 Education-employment (10) -- -- 6.5 7.1 Financial (2) -- -- 1.6 1.7 Family/marital (4) -- -- 1.4 1.9 Accommodation (3) -- -- 0.9 1.0 Leisure-recreation (2) -- -- 1.4 1.5 Companions (5) -- -- 2.3 2.2 Alcohol-drug (9) -- -- 5.5 5.8 Emotional-personal (5) -- -- 1.5 Attitudes-orientation (4) -- -- 1.3 1.4 Hearings/week (M) 1.8 1.0 Positive tests (IRR) 0.63 1.00 * 0.63 1.00 * n = 109. The data were also assumed to contain nonignorable dropout because participants had weeks in which their drug use was unobserved because of periods of incarceration (i.e., differential intermittent dropout) and because participants could be terminated from the program for noncompliance after varying lengths of time in treatment (i.e., differential final dropout). ... Although this study suggests that US is most beneficial for low-risk participants, the lower odds of substance use among participants who scored more highly on the attitudes--
orientation domain of the LSI-R is consistent with Festinger and Marlowe's earlier research (Festinger et al., 2002; Marlowe et al., 2003; Marlowe et al., 2006). ... Whether DD for hypothetical monetary rewards reflects trait-based discounting across these outcome domains is unclear, although such research as does exist suggests that discount rates for hypothetical and real outcomes are highly correlated (Yi, Mitchell, & Bickel, 2010).

HIGHLIGHT:

Intensive judicial supervision improves adult drug court outcomes, but past research has found it to be most effective for high-risk participants (Marlowe, Festinger, Lee, Dugosh, & Benasutti, 2006). The aim of this study was to assess whether there are limits to the effectiveness of intensive judicial supervision. On the basis of previous research, a set of possible risk factors was identified among high-risk participants who took part in a randomized controlled trial of intensive supervision on an Australian drug court. Random effects binomial regression models were fitted to measures of on-program substance use to estimate whether intensive supervision modified the effect of these risk factors on substance use. Intensive supervision was found to be effective for lower risk participants, particularly those with low delay discount rates (i.e., less impulsive decision makers) and less risky social environments. Intensive supervision was found to be more effective for participants presenting with poorer attitudes toward the criminal justice system. Intensive supervision, therefore, appears to offset the risk associated with procriminal attitudes. These results suggest that future drug court interventions may be most effective if they target impulsiveness and participants' social networks. Implementation and evaluation of psychosocial interventions such as contingency management and aftercare ought to be priority areas for investigation.

Keywords: drug court, judicial supervision, risk-needs-responsivity, delay discounting, impulsivity

TEXT:

[*454] There is a strong positive relationship between illicit drug use and crime. Although there is some debate about the causal direction of the relationship, it is clear that rates of offending often increase once offenders become dependent on illicit drugs (Makkai & Payne, 2003; Wish & Johnson, 1986). Substance users are also vastly overrepresented in criminal justice systems around the world (Bewley-Taylor, Hallam, & Allen, 2009).

Drug courts have emerged as one of the primary policy responses to address this overrepresentation. The first drug court was established in Miami, Florida in 1989. There are now more than 2,300 courts operating across the United States (U.S. Justice Programs Office, 2013). Drug courts have also been established in Australia, Belgium, Bermuda, Brazil, Canada, the Cayman Islands, Chile, England, Ireland, Jamaica, Mexico, New Zealand, Norway, Scotland, Suriname, and Wales (International Association of Drug Treatment Courts, 2011)

Drug courts aim to reduce the prison population by treating the problem underlying the offending person, rather than dealing with the offending person through strictly punitive means. Although the operational characteristics of drug courts vary from one jurisdiction to the next, most adult drug courts tend to share the following features: They integrate drug treatment into the criminal justice process; they operate as alternatives to custody; they require regular monitoring of drug use through supervised drug testing; and participants are usually required to report back frequently to the judicial officer. Prosocial behaviors are rewarded, and antisocial behaviors are admonished.

Although early studies were hampered by methodological problems, there is accumulating evidence to suggest that adult drug courts reduce recidivism. There have now been eight metaanalyses estimating their effect on recidivism (Aos, Miller, & Drake, 2006; Downey & Roman, 2010; Latimer, Morton-Bourgon, & Chretien, 2006; Lowenkamp, Holsinger, & Latessa, 2005; [455] MacKenzie, 2006; Mitchell, Wilson, Eggers, & MacKenzie, 2012; Shaffer, 2011; Wilson, Mitchell, & MacKenzie, 2006), the most recent of which identified 92 evaluations meeting eligibility criteria for inclusion (Mitchell et al., 2012). All eight meta-analyses have revealed effects in favor of drug courts over matched comparison groups, and the average estimated effect size is in the order of 10 to 12 percentage points.

Despite evidence for their effectiveness, drug courts find themselves at somewhat of a crossroads. Their effect on recidivism, while statistically significant, is not large. More methodologically rigorous studies tend to find more modest effect sizes (e.g., Shaffer, 2011), which suggests that the true effect size may be somewhere lower than 10%.
Most studies only follow participants for a relatively short time after participating in the program, therefore the long-term benefits of drug courts remain largely unknown. Drug courts are also limited in their ability to meaningfully reduce the prison population because of issues of scale. Recent research suggests that for every one drug court participant in the United States there are 27 offenders who are potentially eligible but not placed on a program (Bhati, Roman, & Chaflin, 2008). Two of the most significant barriers to placement on drug court are restrictive eligibility criteria and program capacity (Sevigny, Pollack, & Reuter, 2013). Imposing overly restrictive criteria can have the effect of systematically excluding those most in need of drug treatment.

One element of drug courts that is assumed to be critical to drug court effectiveness but which also adds to capacity constraints is the requirement that participants regularly report back to the judge (National Association of Drug Court Professionals, 1997). If judicial supervision is not critical to the success of drug court, it would be much easier to bring them to scale. For example, there is some evidence that the Hawaii HOPE program is effective in reducing recidivism (Hawken & Kleiman, 2009). Like drug courts, the HOPE model has a strong focus on drug treatment, regular supervised urine testing, and swift and certain sanctioning for noncompliance. Unlike drug courts, the HOPE program does not require judicial supervision, which means that the program has much greater capacity than drug court.

Research suggests that the judge does have an impact on participant outcomes. Most notably, researchers at the Treatment Research Institute at Pennsylvania University conducted a series of experimental studies where the level of judicial supervision was varied according to a randomized schedule for participants in several misdemeanor drug courts. The theory behind these experiments was that if judicial oversight has any benefit, increasing exposure to the judge should result in better outcomes for participants. In their initial studies, the researchers found no overall effect of increased judicial supervision (Festinger et al., 2002). However, in separate planned comparisons, high-risk participants were found to remain abstinent for significantly longer under biweekly supervision than under supervision on an as-needed basis. High-risk participants were those who had a diagnosis of antisocial personality disorder (ASPD), a prior history of drug treatment, or both. Low-risk participants tended to do just as well regardless of their level of judicial supervision. These results were subsequently replicated in two other misdemeanor courts (Marlowe, Festinger, & Lee, 2003) and extended by prospectively matching high-risk participants with higher levels of supervision at baseline (Marlowe et al., 2006).

This intensive judicial supervision (IJS) effect was replicated and extended in a recent study carried out in an Australian drug court. Jones (2013) reported on a randomized controlled trial where all participants entering an Australian Drug Court over the course of one year were randomly allocated to receive either IJS or supervision as usual (SAU). Those participants in the IJS condition reported to the judge twice per week, whereas those in the SAU condition reported back the usual one time per week. Participants were not prospectively matched on underlying risk in the Jones study because most had long histories of antisocial behavior and prior experience in drug treatment (Jones, 2013). Because most participants would meet eligibility criteria for ASPD and treatment history, there was no basis on which to prospectively match participants on those risk factors. Jones (2013) found that the risk of substance use and sanctioning decreased by more than 40% among the IJS group as a whole relative to those under SAU conditions.

The available evidence therefore suggests that high-risk participants respond well to US (Jones, 2013; Marlowe et al., 2006), but low-risk participants do equally well on drug court irrespective of the level of judicial supervision (Marlowe et al., 2006). Marlowe et al. (2006) explained this interaction effect in terms of the risk--needs--responsivity framework, or the notion that those most at risk of failure require the greatest intensity of treatment (Andrews, Bonta, & Hoge, 1990). A number of low-risk participants also had very low levels of drug dependency; indeed, some may not have been drug dependent at all (DeMatteo, Marlowe, Festinger, & Arabia, 2009). It is reasonable to expect that these participants might succeed on drug court with or without judicial oversight.

One question that remains unanswered is whether participants who present with certain risk factors are more or less likely to respond to intensive judicial supervision. Festinger et al. (2002) did not break their high-risk participants into further risk subgroups. Jones (2013) conducted tests to identify whether there was any interaction between supervision and the variables available in that study (e.g., prior criminal record, age, sex) and found no evidence of any interaction with those variables. However, the risk measures used in that study were limited to static variables available from administrative datasets, which do little to explain why US might work to reduce future substance use.
If IJS works at least in part by bringing forward otherwise delayed rewards and sanctions (i.e., by increasing the celerity of rewards and punishments), there are reasons for assuming that there would be limits to the effectiveness of judicial supervision. Economic theories of deterrence propose that offenders constantly engage in a subconscious "weighing up" of the relative benefits (utility) and costs (disutility) of engaging in acts of deviance. Whenever the utility of deviance outweighs its disutility, the actor will engage in the behavior (e.g., Ehrlich, 1973). However, these rational calculations have been shown to be constrained by individual heterogeneity in the rate at which people discount future outcomes. For example, Nagin and Pogarsky (2001) conducted an experimental study where college students were presented with a hypothetical opportunity to drive under the influence of alcohol. More present-oriented participants, who showed a stronger preference to delay punishment, were more likely to drive on this task. In a later study, Nagin and Pogarsky (2003) found greater willingness to cheat on a test among these deep delay discounters.

[*456] In our study, we test whether the behavioral adjustments observed in the interim findings reported by Jones (2013) were less pronounced among those who discount delayed outcomes more steeply (i.e., among those who are more impulsive decision makers). To assess this hypothesis, participants were approached and asked to complete a nine-item delay discounting (DD) task. Random effects binomial regression models were then fitted to the data to test whether DD modified the effect of judicial supervision on substance use frequency. Although this study was primarily concerned with DD and judicial supervision, we also sought to explore whether US has limited impact for participants presenting with other well-established static and dynamic risk factors. To test these relationships, program outcome data from the same US trial were linked to Level of Service Inventory--Revised (LSI--R) records collected by the state correctional system. The LSI--R was developed by Canadian researchers (Andrews & Bonta, 1995) as a risk-needs assessment instrument for criminal populations, and it is widely used by correctional institutions worldwide. It consists of 54 items measuring 10 empirically validated static and dynamic recidivism risk domains: criminal history, education--employment, financial, family--marital, accommodation, leisure--recreation, companions, alcohol--drug problems, emotional--personal, and attitudes--orientation. If there are risk limits beyond which US ceases to be effective, participants presenting with higher levels of risk on these measures would be expected to be less responsive under US conditions.

Method

Setting and Design of Larger RCT

The setting and characteristics of the US trial are described in detail by Jones (2013). The relevant features are as follows. The court is a single-judge court, and the current judicial officer has presided since 2004. Participants pass through three phases of treatment, submitting supervised urine tests three times weekly during Phase 1 (3 months duration) and twice weekly during Phases 2 (3 months) and 3 (6 months). Participants ordinarily report to the judge once weekly during Phase 1, once every 2 weeks during Phase 2, and monthly during Phase 3. These supervision levels are somewhat higher than those observed in many courts in the United States, which reflects the importance ascribed to judicial supervision in the Australian context. Substance use incurs sanctions (i.e., days in prison), which can accumulate to 14 days before participants serve them in custody. Participants can also be rewarded with waived sanctions for abstinence and other positive behaviors.

Participants who were accepted onto the program during the study period were allocated into the US or SAU group according to a block-randomized schedule. As reported by Jones (2013), the groups were well balanced on all measured covariates, which suggests that the randomization was effective. Those who were allocated to the US group appeared before the judge twice weekly for 4 months during Phase 1. The SAU group received the standard level of supervision, which was once per week. The U.S. studies on which this research was based included a supervision-as-needed group, which received supervision only once or twice during the course of the program. This previous research essentially tested the absolute effect of the judge by comparing very frequent supervision (twice weekly) with very little supervision (less than once every 6 weeks). These low supervision levels were not considered to be appropriate for the higher risk participants on the program under observation in this study. As such, the decision was taken to compare high supervision to supervision as usual. The key advantage of this approach is that it provides an assessment of the impact of additional judicial hearings in a setting where hearings are already very frequent.
Figure 1 shows the points of attrition from the larger RCT to the samples that responded to the questionnaire and had a valid LSI-R record available for this study. Of the 160 participants randomized into the larger RCT under intention-to-treat conditions, 80 were allocated to each of the IJS and SAU groups. Three (1.9%) participants were found to be ineligible post-randomization, and 17 (10.6%) were being treated in residential rehabilitation facilities for the duration of the data collection period for this study. Because these participants were not required to attend the court registry or to appear before the judge they were not eligible for this part of the study. LSI-R records were available for 111 (79.3%) participants. Of the 140 participants who were eligible to take part, 112 (80.0%) were approached and asked to complete the questionnaire. The remaining 28 participants (20%) had been terminated from the program before they could be approached for this interview addendum to the larger RCT. The attrition rate was similar across both supervision groups, and the resulting groups were well matched on measured covariates (see Table 1). Of the 112 participants approached to take part in the questionnaire, 93 consented. The nominal response rate to the survey (acceptances divided by acceptances plus nonacceptances) was therefore 83.0%.

Procedure

The nurse who supervised urinalysis testing recruited participants into the questionnaire study. Recruitment occurred immediately after participants had provided a urine sample. The nurse explained the nature of the study in broad terms and clearly communicated to participants that the research was independent of their program and the results of the research would be kept confidential. Participation was voluntary, and participants were not compensated financially for their involvement. The questionnaire was self-completed. Participants placed completed questionnaires in a sealed envelope addressed to the primary author, and the court staff arranged postage. The mean number of days between program commencement and completion of the questionnaire was 93 days (range: 40-301 days). One-way analysis of variance (ANOVA) revealed that there was no difference between IJS and SAU participants in the time taken to administer the questionnaire: US = 93.6 days (range: 42-215 days), SAU = 92.7 days (range: 40-301 days); F(1, 91) = 0.01, p = .934.

LSI–R records were obtained from administrative records held by the New South Wales (NSW) Bureau of Crime Statistics and Research. Records were linked to Drug Court and other criminal justice variables on the basis of the participants’ names, dates of birth, and where available their unique police or corrective services identification numbers using a standardized probabilistic matching methodology (Hua & Fitzgerald, 2006). Participants can have multiple LSI-R records because the instrument is administered each time a community or custodial correctional order is made. The most recent LSI-R record was selected for the current study, even if the questionnaire was administered while participants were on the program.

Measures

Dependent variable. A “positive” urinalysis test was defined as any test where the participant tested positive to any drug, admitted any drug use to the court, failed to attend a scheduled drug test, failed to provide a urine sample at that test episode, or both (0 = no, 1 = yes). Positive tests were summed within weeks to generate a weekly count of positive drug tests (range: 0-4). The number of drug tests submitted per week was included as an offset variable in the analyses (range: 1-5). At the time the data were extracted for this study (January 31, 2013), the 93 participants who responded to the questionnaire had contributed 13,468 individual drug tests over 5,789 person weeks. The 111 participants who had an LSI-R had contributed 14,174 individual drug tests over 6,120 person weeks.

DD. DD was measured using the nine large magnitude reward items on the DD task described by Kirby et al. (1999). In this task, participants make a series of choices between two hypothetical monetary rewards. Each choice is between an amount of money available immediately and a larger amount of money available after some delay. Preference for the smaller sooner reward is deemed to reflect impulsive decision making. The rate at which people discount the value of delayed outcomes has been found (e.g., Mazur, 1987) to best fit a hyperbolic function that is well described by the following formula:

\[ V = \frac{A}{1 + kD} \]
where $V$ is the present value of the delayed outcome, $A$ is the actual value the outcome, $A$: is a free parameter that describes the rate at which people discount delayed outcomes and $D$ represents the delay. The choices participants made on the nine questionnaire items were used to estimate each participant's discount rate parameter ($\hat{k}$) as per the method proposed by Kirby et al. (1999).

**LSI-R risk domains.** The nine LSI-R risk domains were analyzed separately. A full description of each LSI-R domain and the items used to score those domains can be found in the LSI-R manual (Andrews & Bonta, 1995). Each of the LSI-R items is scored by a combination of face-to-face questionnaire administration and corroboration with administrative records. Individual items are scored as either yes/no answers or on 4-point scales ranging from 0 to 3. All items are subsequently dichotomized through a standardized scoring sheet to indicate whether the risk factor is present (0 = no, 1 = yes). Domain scores and total LSI-R scores are calculated by summing across the identified risk factors within each risk domain.

Table 1
Characteristics of Intensive Judicial Supervision (IJS) and Supervision as Usual (SAU) Participants Who Completed the Questionnaire (n = 93) and Who Had an Eligible Level of Service Inventory--Revised (LSI-R; n = 111)

<table>
<thead>
<tr>
<th>Characteristic</th>
<th>Questionnaire</th>
<th></th>
<th>LSI-R</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>IJS</td>
<td>SAU</td>
<td>IJS</td>
<td>SAU</td>
</tr>
<tr>
<td></td>
<td>(n = 46)</td>
<td>(n = 47)</td>
<td>(n = 50)</td>
<td>(n = 61)</td>
</tr>
<tr>
<td>Male (%)</td>
<td>87.0</td>
<td>83.0</td>
<td>82.0</td>
<td>82.0</td>
</tr>
<tr>
<td>Age(M)</td>
<td>33.2</td>
<td>32.6</td>
<td>32.3</td>
<td>31.8</td>
</tr>
<tr>
<td>Aboriginal Australian (%)</td>
<td>8.7</td>
<td>8.5</td>
<td>10.0</td>
<td>11.5</td>
</tr>
<tr>
<td>In pharmacotherapy (%)</td>
<td>69.6</td>
<td>70.2</td>
<td>74.0</td>
<td>73.8</td>
</tr>
<tr>
<td>Drug of dependence (%)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Alcohol</td>
<td>15.2</td>
<td>8.5</td>
<td>14.0</td>
<td>6.6</td>
</tr>
<tr>
<td>Amphetamine</td>
<td>34.8</td>
<td>27.7</td>
<td>38.0</td>
<td>26.2</td>
</tr>
<tr>
<td>Benzodiazepines</td>
<td>28.3</td>
<td>19.2</td>
<td>28.0</td>
<td>24.6</td>
</tr>
<tr>
<td>Cannabis</td>
<td>54.4</td>
<td>53.2</td>
<td>52.0</td>
<td>45.9</td>
</tr>
<tr>
<td>Cocaine</td>
<td>17.4</td>
<td>10.6</td>
<td>16.0</td>
<td>9.8</td>
</tr>
<tr>
<td>Heroin</td>
<td>67.4</td>
<td>76.6</td>
<td>74.0</td>
<td>83.6</td>
</tr>
<tr>
<td>Index offence (%)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Break and enter</td>
<td>34.8</td>
<td>25.5</td>
<td>32.0</td>
<td>24.6</td>
</tr>
<tr>
<td>Driving</td>
<td>10.9</td>
<td>14.9</td>
<td>10.0</td>
<td>13.1</td>
</tr>
<tr>
<td>Theft</td>
<td>32.6</td>
<td>44.7</td>
<td>32.0</td>
<td>45.9</td>
</tr>
<tr>
<td>Other</td>
<td>21.7</td>
<td>14.9</td>
<td>26.0</td>
<td>16.4</td>
</tr>
<tr>
<td>Concurrent offences (M)</td>
<td>7.8</td>
<td>10.3</td>
<td>8.4</td>
<td>10.4</td>
</tr>
<tr>
<td>Sentence (M years)</td>
<td>1.2</td>
<td>1.2</td>
<td>10.3</td>
<td>9.3</td>
</tr>
<tr>
<td>Prior prison(a) (M)</td>
<td>1.8</td>
<td>1.1 *</td>
<td>2.0</td>
<td>1.4</td>
</tr>
<tr>
<td>Days to questionnaire (mean)</td>
<td>93.6</td>
<td>92.7</td>
<td>--</td>
<td>--</td>
</tr>
<tr>
<td>Discount rate (M k)</td>
<td>0.032</td>
<td>0.038</td>
<td>--</td>
<td>--</td>
</tr>
<tr>
<td>Treatment history (%)</td>
<td>87.2</td>
<td>80.4</td>
<td>--</td>
<td>--</td>
</tr>
<tr>
<td>Days to LSI-R (M)</td>
<td>--</td>
<td>--</td>
<td>201.1</td>
<td>150.4</td>
</tr>
<tr>
<td>On-program LSI-R (%)</td>
<td>--</td>
<td>--</td>
<td>62.0</td>
<td>54.1</td>
</tr>
<tr>
<td>LSI-R score (M)</td>
<td>--</td>
<td>--</td>
<td>29.6</td>
<td>30.8</td>
</tr>
<tr>
<td>Criminal history (10)</td>
<td>--</td>
<td>--</td>
<td>7.0</td>
<td>6.9</td>
</tr>
<tr>
<td>Education-employment (10)</td>
<td>--</td>
<td>--</td>
<td>6.5</td>
<td>7.1</td>
</tr>
<tr>
<td>Financial (2)</td>
<td>--</td>
<td>--</td>
<td>1.6</td>
<td>1.7</td>
</tr>
<tr>
<td>Family/marital (4)</td>
<td>--</td>
<td>--</td>
<td>1.4</td>
<td>1.9 *</td>
</tr>
<tr>
<td>Accommodation (3)</td>
<td>--</td>
<td>--</td>
<td>0.9</td>
<td>1.0</td>
</tr>
<tr>
<td>Leisure-recreation (2)</td>
<td>--</td>
<td>--</td>
<td>1.4</td>
<td>1.5</td>
</tr>
<tr>
<td>Companions (5)</td>
<td>--</td>
<td>--</td>
<td>2.3</td>
<td>2.2</td>
</tr>
<tr>
<td>Alcohol-drug (9)</td>
<td>--</td>
<td>--</td>
<td>5.5</td>
<td>5.8</td>
</tr>
<tr>
<td>Emotional-personal (5)</td>
<td>--</td>
<td>--</td>
<td>1.5</td>
<td>1.4</td>
</tr>
<tr>
<td>Attitudes-orientation (4)</td>
<td>--</td>
<td>--</td>
<td>1.3</td>
<td>1.4</td>
</tr>
</tbody>
</table>
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</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>IJS (n = 46)</td>
<td>SAU (n = 47)</td>
<td>IJS (n = 50)</td>
</tr>
<tr>
<td>Hearings/week (M)  (^b)</td>
<td>1.8</td>
<td>1.0 **</td>
<td>1.8</td>
</tr>
<tr>
<td>Positive tests (IRR)  (^c)</td>
<td>0.63</td>
<td>1.00 *</td>
<td>0.63</td>
</tr>
</tbody>
</table>

\(^a\) n = 109. \(^b\) Four participants who completed the questionnaire and 5 participants with an LSI-R were on the program for less than 3 weeks and they were not included in this calculation. \(^c\) IRR = Incident rate ratio of having one or more positive drug tests per week.

* p < .05. ** p < .01.

Note. Chi-square tests were used to test for between-groups differences on categorical outcomes, and Wilcoxon tests were used to test for differences in continuous or ordinal outcomes.

Control variables. Participants who responded to the questionnaire were asked how many times they had entered treatment prior to entering the drug court, and this was subsequently coded as a binary variable (0 = never, 1 = one or more times). A number of variables were also extracted from administrative datasets to control for any potential differences between US and SAU participants. The controls were sex; age at program commencement; whether participants identified as Aboriginal or Torres Strait Islander Australians; whether participants were being treated by way of pharmacotherapy for heroin dependence; drugs of dependence; primary offense for which they were referred to the drug court; number of concurrent offenses dealt with by the drug court; initial sentence length; number of prison sentences participants had received in the 5 years prior to drug court participation; time between commencement on the program and completion of the questionnaire--date of administration of the LSI-R.

Analyses

The dependent variable for each analysis was a count of positive drug tests for Person \(i\) in each Week \(t\) with an offset for the number \([\text{#459}]\) of tests contributed by Person \(i\) in Week \(t\). The outcome was modeled using random effects binomial regression with conditional first-order autoregressive (AR1) terms and adjustment for nonignorable treatment dropout. Individual random effects were estimated to account for intraindividual clustering and interindividual heterogeneity in substance use. The AR1 terms, which were represented as the proportion of positive tests in Week \(t-1\), were included to account for the fact that frequency of drug use for Person \(i\) in Week \(t\) is highly correlated with drug use frequency for Person \(i\) in Week \(t-1\). The natural log of the total number of weeks participants spent on the program was also included as a control given that participants dropped out of treatment after varying intervals.

The DD and LSI-R variables resulted in poorly fitting models when entered as ordinal scale variables. All variables were subsequently dichotomized, and a series of supervision by risk dummy variables were entered into the models. Each risk factor was entered one at a time, resulting in 11 separate models (i.e., one model for each of the 10 LSI-R domain scores plus one model for DD). Each of the potential control variables that were significantly different across supervision group membership was also included in the models. As can be seen in Table 1, the only significant difference between the two groups was in the number of prior prison episodes in the 5 years prior to their current drug court program. This was not significant in any of the multivariate models and it was dropped from the final models in the interests of parsimony. The outcome model in each analysis was therefore given by

\[
\text{Logit} \left[ \frac{\Pr(y_{iit} = 1 | n_{iit})}{n_{iit}} \right] = m [i] + b [0] + b [1-n]X
+ b [t] \ln t + b [ly_{i,t-1} / n_{i,t-1}] (2)
\]
where \( y_{ij} \) is the drug test result (1 for positive and 0 for negative) in the \( j \) test; \( j = 1 ... n_{it} \) for Person \( i \) in Week \( t \); \( y_{it} \) is the number of positive drug tests contributed by Person \( i \) in Week \( t \); \( n_{it} \) is the number of tests contributed by Person \( i \) in Week \( t \); \( m_{i} \) are the individual-specific random intercepts; \( b[0] \) is the overall intercept; \( X \) is a vector of supervision, risk, and control variables; \( \ln(n_{it}) \) is the natural log of the number of weeks on the program at Time \( t \), and \( y_{i(t-1)} / n_{i(t-1)} \) is the AR1 term representing the proportion of positive tests for Person \( i \) at Time \( t - 1 \).

The data were also assumed to contain nonignorable dropout because participants had weeks in which their drug use was unobserved because of periods of incarceration (i.e., differential intermittent dropout) and because participants could be terminated from the program for noncompliance after varying lengths of time in treatment (i.e., differential final dropout). Of the 5,789 person weeks contributed by participants who completed the questionnaire, no drug tests were conducted on 426 person weeks (7.4%). Of the 6,120 person weeks contributed by participants who had an LSI-R, no drug tests were conducted on 448 person weeks (7.3%). Dropout was treated as nonignorable in this study in that it was assumed to depend on missing observations at or after the time of dropout (Chan, Leung, Choy, & Wan, 2009). This dropout mechanism was explicitly modeled using a separate logit model, and a link function was used to link the linear function of covariates to the mean of the dropout model. The dropout model in each analysis was given by

\[
\text{Logit} \left[ \Pr(\ell_{it} = 1) \right] = a[0] + a[\ell] \ln t + a[p] y_{it} \ln n_{it},
\]

where \( \ell_{it} \) is the indicator of a missing urinalysis test for Person \( i \) in Week \( t \); \( a[0] \) is the intercept; \( \ln(n_{it}) \) is the natural log of time on the program; and \( y_{it} / n_{it} \) is the term for the informative dropout model, which is the present proportion of positive tests. At the time of dropout (both intermittent and final), \( y_{it} \) is unobserved and is estimated using the binomial model with parameters \( n_{it} \) and \( \beta[P_{it}] = \Pr(y_{it} = 1 / n_{it}) \), as given by Equation 2. It should be noted that the dropout model (see Equation 3) could contain any linear combination of covariates, but time on the program was the only significant predictor of treatment dropout among the measured covariates.

The parameters that were included in the final outcome model were fitted to an ordinary least squares regression to derive variance inflation diagnostics to test for multicollinearity. All variance inflation factors were less than 2, which suggests that collinearity was not problematic. All parameters were estimated through the Bayesian Markov Chain Monte Carlo method implemented efficiently using the Bayesian software WinBUGS version 1.4.3. The WinBUGS code for Model 1 in Table 2 is given in the Appendix as an example.

**Results**

**Sample Representativeness**

Table 1 shows the distribution of characteristics for IJS and SAU participants separately for participants who completed the questionnaire and who had an LSI-R available. The only characteristic on which the two questionnaire subgroups differed was in the frequency of prior imprisonment (\( M = 1.8 \) episodes in the 5 years prior to program entry for the IJS group cf. 1.1 for the SAU group), \( F(1, 90) = 4.73, p = .038 \). The only statistically significant differences between US and SAU participants who had an LSI-R available was in the mean score on the family-marital LSI-R domain (Wilcoxon’s Z = -2.04, \( p = .041 \)). Participants in the SAU group had higher average scores (\( M = 1.9 \)) than US participants (\( M = 1.4 \)), which suggests that the SAU group had poorer marital or family relationships, were more likely to have criminal family members or spouses, or both.

**Risk, Supervision, and Substance Use**

Table 2 shows the results of the regression models estimating the relationship between supervision, risk, and substance use outcomes. Model 1 includes dummy variables representing the interaction between supervision level and \( k \) scores dichotomized at the mean value. Relative to SAU participants with low DD rates (i.e., less impulsive decision makers were the referent category), the odds of substance use were lower among US participants with low delay discount rates, odds ratio (OR) = 0.59, 95% confidence interval (CI) [0.36, 0.99]. IJS participants who had high delay discount rates had odds of substance use that were not significantly different from low discounting SAU participants, OR = 0.93, 95% CI [0.48, 1.78]. It is interesting to note from Table 2 that high-discounting SAU participants had odds of substance use that were not significantly different from low discounting SAU participants, OR = 1.17, 95% CI [0.61, 2.22].
We found no evidence of any interaction between level of supervision and scores on the criminal history, education--employment, financial, family--marital, accommodation, or emotional--personal risk domains. Table 2 also shows that the IJS effect was only significant for participants who scored low on the leisure--recreation, companion, and drug--alcohol risk domains (Models 2 through 4, respectively). The odds of substance use were lower among IJS participants who spent more leisure time in meaningful activity, OR = 0.46, 95% CI [0.26, 0.88], had fewer criminal acquaintances, OR = 0.55, 95% CI [0.35, 0.86], and had less problematic drug or alcohol use, OR = 0.45, 95% CI [0.25, 0.79]. On the other hand, Model 5 in Table 2 shows that US was effective for participants who had higher scores on the attitudes-orientation domain. Whereas SAU participants who scored higher on this domain had higher odds of substance use than SAU participants who had lower scores on this domain, OR = 2.03, 95% CI [1.17, 3.45], these higher odds of use were not apparent among IJS participants presenting with procriminal attitudes, OR = 1.10, 95% CI [0.62, 1.91].

Discussion

The current findings provide evidence that intensive judicial supervision is more effective for participants who present with low delay discount rates and lower scores on a number of well-established risk factors for poor criminal justice outcomes. Low discounters in the US group had significantly lower odds of substance use on the program than low-discounting SAU participants. High discounter participants in both supervision groups had odds of substance use that were not statistically different from low-discounting SAU participants. These findings are consistent with previous research showing that individual heterogeneity in the extent to which people discount the future is strongly related to the deterrent effectiveness of the law (e.g., Nagin & Pogarsky, 2003).

Moreover, IJS only appears to be effective for participants who spend more of their leisure time in meaningful activity, associate with fewer criminal peers, and have less severe substance abuse problems. The only evidence to suggest that higher risk participants responded better under US conditions was found on the attitudes-orientation domain of the LSI-R. Although SAU participants had higher odds of substance use if they had higher scores on the attitudes-orientation risk domain (OR = 2.03), high-risk IJS participants had odds of substance use similar to low-risk SAU participants (OR = 1.10). This finding suggests that US may offset the risk associated with the procriminal attitudes that many participants present with when they begin the drug court program.

These findings both support and are in contrast to those observed in the misdemeanor courts studied by Marlowe, Festinger, and others in the United States (Festinger et al., 2002; Marlowe et al., 2003; Marlowe et al., 2006). These researchers found IJS to be more effective for participants presenting with antisocial characteristics (ASPD or a history of drug treatment). The current findings are consistent with this earlier research insofar as IJS was more effective for people presenting with antisocial attitudes. However, our findings also stand in contrast to this earlier research insofar as US was not effective for participants presenting with a range of other risk factors, most notably impulsiveness and social risk factors. US does not appear to result in improved outcomes for people who steeply discount the value of outcomes over short periods of time. IJS conditions in this study effectively reduced the delay between judicial status hearings from once every 7 days to a maximum of 5 days between hearings. Even 5 days may be too long for the potential rewards and sanctions delivered during status hearings to greatly influence participants’ decision making. US also seems to be ineffective for people who have a number of social risk markers, such as a high number of criminal peers and poor use of leisure time.

Although this study suggests that US is most beneficial for low-risk participants, the lower odds of substance use among participants who scored more highly on the attitudes-orientation domain of the LSI-R is consistent with Festinger and Marlowe’s earlier research (Festinger et al., 2002; Marlowe et al., 2003; Marlowe et al., 2006). The four items that comprise the attitudes-orientation domain measure the extent to which participants have attitudes that are supportive of crime and are unfavorable toward convention, their sentence, and the penalties and supervision they receive. This study could not identify why greater exposure to the judge might be effective only for people presenting with more antisocial attitudes. However, it is possible that there is something about the relationship with the judicial officer that helps to reshape these attitudes, particularly in relation to criminal justice supervision.

One of the things that separates drug court from standard court procedures is the care and compassion shown by the judge when interacting with participants. As Judge Herbert Klein, who is often credited with founding the drug court model, remarked, the way the judge communicates with participants ‘is a pronouncement from those in
authority to some of our least powerful and most ignored citizens that we care about you and want to reach out and help you..." (Klein, 1996). Recent research carried out by the current authors identified that participants under US conditions rated their personal relationship with the judge as being stronger than those under SAU conditions (Jones & Kemp, in press). This provides indicative evidence that the judicial effect may be due, at least in part, to the formation of a close personal relationship between the participant and the judge. If so, and if participants who score low on the attitudes-orientation item of the LSI-R already have reasonably favorable attitudes toward supervision, it stands to reason that US would be maximally effective among those who might previously have held quite hostile attitudes toward that supervision. However, at this stage these suggestions are speculative, and more evidence is needed to determine the basis of the improved performance of participants under the US conditions.

The current findings also provide several areas for practitioners to explore to improve outcomes for participants. It is clear from the current findings that participants were only responsive to US if they had low DD scores (i.e., were less impulsive decision makers) and low scores on the leisure--recreation, companion, and drug--alcohol domains of the LSI-R. An alternative approach may be required for high discounters and those with poorer outcomes on these other risk domains. One alternative for high discounters may be contingency management (CM). Contingency management involves the provision of regular rewards for abstinence, usually in form of cash payments. When viewed through the lens of DD, these incentives may serve to keep regular large rewarding outcomes salient in participants' minds whenever opportunities for substance use arise. Although the two randomized controlled trials of CM in drug courts to date have found no overall benefit (Marlowe, Festinger, Dugosh, Arabia, & Kirby, 2008; Prendergast, Hall, Roll, & Warda, 2008), both of these studies had acknowledged weaknesses. The rewards delivered in the study described by Marlowe et al. (2008) were relatively small and delivered infrequently. This may have limited their effectiveness as reinforcements for continued abstinence. Prendergast et al. (2008), on the other hand, gave participants the option of using accrued credits to pay court costs and many participants chose to use them in this way. Decision making is highly dependent on whether risks are framed in terms of gains or losses (Kahneman & Tversky, 1979). Avoiding losses may therefore have qualitatively different reinforcing properties than receiving rewards for continued abstinence.

The observation that US was not effective for participants with significant social risk factors is consistent with other research showing poorer treatment outcomes among patients presenting with low social supports (e.g., Dobkin, Civita, Parahekas, & Gill, 2002). Significant benefits for drug court participants could be achieved if policies and practices were to focus on strengthening attachments to prosocial networks and institutions outside of the drug court environment. There is a significant body of research showing higher graduation and lower recidivism rates among drug court participants who are employed compared with those who are unemployed (e.g., Brown, 2010; Brown, Allison, & Nieto, 2011; Butzin, Saum, & Scarpitti, 2002; Deschenes, 2009; Mateyoke-Scrivner, Webster, Staton, & Leukefeld, 2004; Roll, Prendergast, Richardson, Burdon, & Ramirez, 2005; Rossman, Roman, Zweig, Rempel, & Lindquist, 2011). Whether this reflects a causal association or whether people with better outcomes self-select into employment is not clear. However, strengthening ties to employment agencies and rigorously evaluating the outcome of encouraging participants into employment is an avenue worth exploring, particularly for those individuals who would otherwise spend little time in meaningful social activities.

These findings also identify avenues for new research on drug court effectiveness. It was surprising, for example, to observe that there was no significant difference in the odds of substance use among high- and low-discounting participants under usual supervision conditions. Contrary to prior studies (e.g., Passetti, Clark, Mehta, Joyce, & King, 2008), this suggests that discounting was not a strong predictor of treatment outcomes on this program. Previous studies have revealed modest correlations between DD using monetary choice tasks and conventional measures of impulsiveness, such as the Barratt Impulsiveness Scale (Kirby et al., 1999). Although this suggests that DD may be a marker for impulsiveness, the relationship between temporal discounting and other measures of impulsivity requires investigation. It would be particularly useful to investigate whether there is any relationship between questionnaire-based measures of self-control and DD given their strong association with offending (Gottfredson & Hirschi, 1990; Grasmick, Tittle, Bursik, & Arneklev, 1993).

Future research also ought to examine whether discount rates for gains and losses are equally related (or unrelated) to likelihood of substance use on the program. Different neural pathways are activated when people make risky decisions that may result in gains or losses (Heilbroner, Hayden, & Platt, 2010). If participants are motivated to take part in drug court programs to avoid losses (e.g., loss of liberty, loss of income, family--relationship breakdown), discounting for losses may be more influential than discounting for gains. This has practical implications for how contingencies on drug courts are administered. If discounting for gains were strongly related to
drug court outcomes, improvements in drug court performance could be obtained by increasing the magnitude of immediacy of rewards on the program. If, on the other hand, discounting for losses were strongly related to drug court outcomes, increasing the size or immediacy of sanctions may be one means of increasing program compliance. However, the first stage of this policy development process is to understand how participants discount gains and losses and whether these discount rates relate to program performance.

Finally, the current research, like that which preceded it (Jones, 2013), only examined a limited subset of the outcomes that drug courts aim to achieve. Other markers of program performance include time spent in custody while on the program and final completion rates. The ultimate goal of drug court practice is to reduce the likelihood that participants will return to the criminal justice system. Future research, therefore, also should explore whether intensive supervision increases drug court completion rates, reduces time spent in custody, and reduces long-term rates of reoffending.

Although our findings inform the question of for whom US is more or less effective, the study was not without limitations. First, although the subsamples under observation in this study were representative of participants in the larger RCT, the benefits of randomization are diminished once subgroup analyses are conducted. As a result, we cannot rule out the influence of other confounding factors when assessing the associations between DD, the LSI-R risk domains, and substance use. However, this is true of any observational study of this nature, and it is important to note that there were few significant differences between US and SAU participants on their measured characteristics.

A second limitation relates to the risk measures used in this study. The use of discounting as a measure of impulsiveness is critically dependent on two assumptions. First, monetary choice tasks assume that discounting for hypothetical monetary rewards reflects discounting of nonmonetary outcomes. This assumption is critical because success on drug court is not contingent on monetary choices. There are many short-, medium- and long-term gains and losses that participants must weigh up on drug court, and temporal discounting loses its potency as a risk marker if there is a large disconnect between the way in which these outcomes are discounted and the way in which hypothetical monetary rewards are discounted. Whether DD for hypothetical monetary rewards reflects trait-based discounting across these outcome domains is unclear, although such research as does exist suggests that discount rates for hypothetical and real outcomes are highly correlated (Yi, Mitchell, & Bickel, 2010). The second assumption is that temporal discounting reflects a time-stable personality trait. This is also critical because if discount rates change over time, the order of causation would be difficult to untangle in this study. Kirby (2009) found 1-year stability in discount rates among undergraduate students that were in line with those typically found among personality traits. These findings therefore provide encouragement to continue to explore the relationship between temporal discounting and drug court outcomes.

The use of recorded LSI-R profiles for the drug court participants also needs to be considered with some caution. There was some variation in time between LSI-R administration and program start date across participants. This time variable was included as a sensitivity test in the analyses, but it did not meaningfully affect any of the parameter estimates and was consequently dropped from the models. Perhaps more important, LSI-R records were linked even if they were administered while participants were on the drug court program. In our study, we assumed that these risk factors would not be changed by virtue of being under intensive supervision. However, because some of the items on the LSI-R are dynamic (especially drug and alcohol use), it is possible that involvement in the US brought about changes in these risk factors and hence affected the LSI-R scores. There was no evidence from Table 1 that US and SAU differed on any of the risk domains other than the family--marital domain. This was not found to be a significant effect modifier in this study. Although we can be reasonably confident that including these LSI-R records was appropriate, we add this caveat as a cautionary note.

Finally, although a difference in risk profiles would seem to be the most likely explanation for the discrepancy between the current findings and those reported previously (Festinger et al., 2002; Marlowe et al., 2003; Marlowe et al., 2006), it is also possible that high-risk participants may have been more likely to be detected under high-supervision conditions. Research shows that intensively supervising offenders through the criminal justice system can lead to increases in detection rates for technical violations, particularly among those most at risk of rule violations (Petersilia & Turner, 1993). The actual frequency of supervised urine testing was not varied in this study. However, self-reported use was counted as a positive drug test in the dependent variable. Because US participants reported back to the court more frequently than SAU participants, they had a greater opportunity to self-report use to the judicial officer. This raises the possibility that any benefits that might have been afforded by higher supervision
might have been offset to some degree among high-risk participants if they were also more likely to self-report recent substance use. This seems unlikely given that positive drug tests more commonly arose from detected, rather than self-reported, use. Nevertheless, this possibility cannot be ruled out.

In summary, our study suggests that US is more beneficial than SAU for participants who present with low delay discount rates, less severe alcohol and drug problems, and who present without social risk factors such as a large number of antisocial peers and poor use of leisure time. In light of these findings, behavioral interventions such as CM should be explored to see whether they reduce substance abuse among high-DD participants. Research and evaluation should also explore means of reducing the risk associated with social risk factors, such as programs that bolster employment [^463] opportunities or provide other means of using leisure time appropriately. US does appear to be effective for participants who present with poor attitudes toward conventional criminal justice process, which is consistent with the research on which this study was based. This may be a direct effect of the nature of the interaction between the judge and the participant, although further research would be required to bear this issue out more fully.

References


Shaffer, D. K. (2011). Lookin...


Appendix

WinBUGS Code for Random Effects Models

WinBUGS code for conditional AR1 random effects binomial regression model with adjustment for nonignorable treatment dropout, estimating interaction between \( k \) and supervision level.

Model

\[
\text{for (i in 1:N)} \quad \# \text{Start Data Model} \\
\text{y[i] [\#x7E]} \quad \text{dbin(p[i],n[i])} \\
\text{logit(p[i]) <- beta0i[id[i]] + beta0 + betakg2*kgdum2[i] + betakg3*kgdum3[i] + betakg4*kgdum4[i] + betat*lnweek[i] + betal*lagy[i]} \\
\text{d[i] [\#x7E]} \quad \text{dbern(pd[i])} \\
\text{logit(pd[i]) <- alpha0 + alphat*lnweek[i] + alphal*yn[i]} \\
\text{lnweek[i] <- log(week[i])} \\
\text{[\*465] \ yn[i] <- y[i]/n[i]} \\
\] \quad \# \text{end Data Model}

\[
\text{for (i in 1:T)} \quad \# \text{Start Random Intercept Model} \\
\text{beta0i[i] [\#x7E]} \quad \text{dnorm(0.0, tau)} \\
\] \quad \# \text{end Random Intercept Model}

\text{beta0 [\#x7E]} \quad \text{dnorm(0, 0.000001)} \# \text{Start Priors} \\
\text{betakg2 [\#x7E]} \quad \text{dnorm(0, 0.000001)} \\
\text{betakg3 [\#x7E]} \quad \text{dnorm(0, 0.000001)} \\
\text{betakg4 [\#x7E]} \quad \text{dnorm(0, 0.000001)} \\
\text{betat [\#x7E]} \quad \text{dnorm(0, 0.000001)} \\
\text{betal [\#x7E]} \quad \text{dnorm(0, 0.000001)} \\
\text{tau [\#x7E]} \quad \text{dgamma(0.001, 0.001)} \\
\text{sig <- 1/tau} \\
\text{alpha0 - dnorm(0, 0.000001)} \# \text{Start Priors} \\
\text{alphat [\#x7E]} \quad \text{dnorm(0, 0.000001)} \\
\text{alphal [\#x7E]} \quad \text{dnorm(0, 0.000001)} \# \text{end Priors}
\]

\text{list(beta0 = l,betakg2 = l,betakg3 = l,betakg4 = l,betat = l,betal = l,tau = l,alpha0 = l, alphat = l,alphal = l)}

\text{list(N [\#x3D] 5789,T [\#x3D] 93)}

\text{id[i] y[i] n[i] week[i] kgdum2[i] kgdum3[i] kgdum4[i] laby[i] d[i]} \# \text{read in data}
Legal Topics: