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Effectiveness of Using Incentives to Improve Parolee Admission and Attendance in Community Addiction Treatment

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Abstract

This study is a randomized effectiveness trial of the use of incentives to improve treatment utilization among parolees in community treatment. In prison, Admission phase parolees were randomized to Admission Incentive (N=31) or Education (N=29). Attendance phase parolees entering community treatment were randomized to Attendance Incentive (N=104) or Education (N=98). There was no main effect for incentives in either study phase. Neither admission to community treatment (Incentive 60%, Education 64%; $p=.74$), nor intervention completion (Incentive 22%; Education 27%; $p=.46$) appeared to be impacted. Time-in-treatment was predicted by age, first arrest age, and type of parole status (Cox regression $p<.05$), but not by treatment group. Providing incentives did not increase the likelihood that parolees enrolled in or stayed in community treatment. In light of this finding, criminal justice practitioners who are considering incentives to increase admission or retention should be aware that they may not produce the desired outcomes.

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Introduction

Drug Use, Crime, and Treatment

Extensive research demonstrates the close relationship between drug use and crime (Anglin & Perrochet, 1998; Bennett, Holloway, & Farrington, 2008; MacCoun, Kilmer, & Reuter, 2003; Newcomb, Galaif, & Carmona, 2001; White & Gorman, 2000). Drug abuse intensifies rates of income-generating criminal activities, and reductions in drug use result in decreases in drug-related criminal activity (Anglin & Speckart, 1988; Deitch, Koutsenok, & Ruiz, 2000; Farabee, Joshi, & Anglin, 2001; Nurco, Hanlon, Kinlock & Duszynski, 1988). In addition, the prevalence of drug use is high among criminal offenders. Data from the Arrestee Drug Abuse Monitoring (ADAM II) program in 2012 indicated that anywhere from 62% to 86% of arrestees across sites tested positive for at least one illicit drug at the time of arrest (ONDCP, 2012). In 2004, 69% of state prisoners reported that they used drugs regularly (Mumola & Karberg, 2006). Studies of recidivism have found that 40% or more of offenders return to prison within a year of their release from prison, with the percentage higher among those with a history of heavy drug use (Anglin, Prendergast, Farabee & Cartier, 2002; Martin, Butzin, Saum & Inciardi, 1999; Pew Center on the States, 2011; Prendergast, Hall, & Wexler, 2003).

Community treatment following release from prison-based treatment can be key to long-term success; community treatment builds on the prison treatment experience and facilitates clients' reintegration into the community (Butzin, Martin, & Inciardi, 2005; Butzin, O'Connell, Martin & Inciardi, 2006; Knight, Simpson, & Hiller, 1999; Martin et al., 1999; Prendergast, Hall, Wexler, Menick & Cao, 2004; Prendergast, Wellisch, & Wong, 1996; Sacks, Chaple, Sacks, McKendrick & Cleland, 2012; Wexler, Melnick, Lowe, & Peters, 1999; Wexler et al., 1999). Length of stay in community treatment appears to be a significant predictor of positive post-treatment outcomes (Field, 1989; Hubbard, Collins, Rachal & Cavanaugh, 1988; Inciardi, Martin & Butzin, 2004; Knight et al., 1999; Simpson, 1979; Simpson, Joe, & Brown, 1997; Simpson & Sells, 1982). In a meta-analysis, McMurran and Theodosi (2007) found that failing to complete community treatment was associated with increased levels of reoffending.

Despite the effectiveness of community treatment following release from prison, a substantial portion of clients either do not show up to enroll in treatment or drop out before completion, even with corrections departments or other criminal justice agencies paying for all or most of the costs of treatment. An evaluation of treatment programs for prisoners and parolees funded by the California Department of Corrections and Rehabilitation reported that among prison treatment participants who had a referral to community treatment, typically less than 50% attended any treatment following release to parole (Anglin, Prendergast, & Farabee, 1999; Prendergast, Anglin, Burdon, & Messina, 2003). The benefits of treatment cannot be realized if offenders do not enroll in treatment or do not remain in treatment for a sufficient length of time; hence, the need for approaches that encourage enrollment and retention. One possible approach is to use incentives to reinforce the positive steps that offenders take to continue in treatment.

Contingency Management (CM)

The use of reinforcement to increase desired behaviors has a long tradition of application in the behavioral field and, more specifically, in affecting positive outcomes of alcohol and drug treatment (Higgins & Silverman, 1999; Higgins, Alessi, & Datona, 2002; Petry et al., 2005; Roll et al., 2006). Contingency Management (CM) interventions are grounded upon principles of operant conditioning and the assertion that behaviors exist as a product of their consequences – behaviors met with positive consequences will generally persist while those met with negative consequences will not. CM interventions typically utilize positive reinforcement to encourage desirable behavior related to treatment goals with delivery of a reinforcing “event” contingent upon the performance of a particular behavior, resulting in the increased frequency of that behavior. Researchers oftentimes employ vouchers exchangeable for cash or retail goods to promote a specific aspect of treatment adherence, such as program attendance or abstinence from certain behaviors such as drug use. Although most CM interventions involve positive reinforcement, researchers have also achieved success with a combination of more frequent positive reinforcement and less frequent negative punishment (i.e., reducing the voucher amount when an undesirable behavior occurs) (Higgins, 1999). Efficacy research has documented the ability of contingency management procedures to increase the frequency of negative urine samples for illicit drugs, including opiates, cocaine, and methamphetamine (for reviews, see Kidorf & Stitzer, 1999; Roll, 2007; Silverman, Preston, Stitzer, & Schuster, 1999; for meta-analyses, see Lussier, Heil, Mongeon, Badger & Higgins, 2006; Prendergast, Podus, Finney, Greenwell & Roll, 2006).

CM techniques are applicable to a wide range of behaviors associated with drug use or with drug treatment. Higgins and Petry (1999), in a review of the use of CM in substance abuse treatment, found that in addition to reducing substance use, CM may be useful in improving treatment attendance. More recently, CM research directed at substance abuse treatment retention or attendance has expanded into a variety of settings and formats, including inpatient, outpatient, methadone, telephone or in-person treatment and rehabilitative programs such as job skills training; and with a variety of populations (pregnant women, youth dependent on marijuana, veterans, methadone users, general populations in community outpatient treatment, and dually diagnosed) (Alessi, Hanson, Weiners & Petry, 2007; Bride & Humble, 2008; Brigham, Winhusen, Lewis, & Kropp, 2010; Businelle, Rash, Burke, & Parker, 2009; Carroll et al., 2006; Helmus, Saules, Schoener & Roll, 2003; Jones, Haug, Stitzer & Svikis, 2000; Jones, Haug, Silverman, Stitzer & Svikis, 2001; Ledgerwood, Alessi, Hanson, Godley & Petry, 2008; Rhodes et al., 2003; Sinha, Easton, Renee-Aubin & Carroll, 2003; Walker et al., 2010). In general, research on CM for treatment attendance has shown mixed effectiveness results--some studies show promising results, while some are hampered by design issues such as non-random assignment, attrition, and post-hoc analysis (Brigham et al., 2010; Businelle et al., 2009; Ledgerwood et al., 2008; Rhodes et al., 2003).

Three studies tested CM with community treatment populations (Alessi et al., 2007; Helmus et al., 2003; Walker et al., 2010) with mixed results with respect to attendance. Alessi et al. (2007) did not find a main effect for CM, while Helmus et al. (2003; using a dually

diagnosed population) and Walker et al. (2010; using an A–B design) found that CM did increase attendance.

Two studies focused on marijuana-dependent young adults (Sinha et al., 2003; Carroll et al., 2006), and although they differed in length of treatment (Sinha et al., 2003 involved a three-session intervention program, while Carroll et al., 2006 reinforced eight sessions), both found a significant main effect for CM on treatment retention.

Three studies focused on women – two focusing on pregnant women (Jones et al., 2000; Jones et al., 2001) and one on African American women on welfare (Bride & Humble, 2008). Again results were mixed. Bride and Humble (2008) found that the incentive group had more days in treatment, as did Jones et al. (2001) in their study of pregnant women in methadone maintenance. Bride and Humble noted that low-magnitude incentives used over the four-week study period did not present an extensive burden on the treatment program, and thus offered a low-cost, manageable method of improving treatment attendance. However, in an earlier study, Jones et al. (2000) found that \$5 per day did not significantly improve retention in abstinence-treated pregnant women.

Overall, there is some research to suggest CM's effectiveness in increasing and sustaining treatment retention in therapeutic programs, primarily in substance-using adults. However, many research programs on CM focus on controlled environments rather than real-world settings, where CM's application has yet not been heavily examined. In addition, CM's application to high-risk parolee populations has not been addressed.

CM research with adult criminal justice populations in community settings is limited. In most cases, CM research has been undertaken with juvenile offenders or adults in institutional settings (Remington & Remington, 1987; Rutherford, 2006; Viglione & Sloas, 2012). Polakow and Doctor (1974) reinforced attendance and constructive participation in probation programming and found that CM in the form of probation sentence reductions significantly improved outcomes in attendance, probation violations, arrests, and employment. Recently, Prendergast and colleagues (Prendergast, Hall, Roll & Warda, 2008; Hall, Prendergast, Roll & Warda, 2009) reported on a CM study with a community population of criminal justice clients enrolled in drug court. Low-cost incentives for drug abstinence and/or adherence to treatment plan activities failed to achieve statistically significant improvements in drug use or attendance, most likely due to the greater influence of the judge and the sanctions that he could impose. Likewise, Marlowe et al. (2008) found no main effects for CM in a drug court when reinforcing for attendance and drug court compliance. Nonetheless, Marlowe and Wong (2008) suggest that structured behavioral interventions such as CM are better suited for high-risk offenders. However, as mentioned above, Carroll et al. (2006) conducted a study with young adult marijuana users in the criminal justice system who presumably are lower-level offenders. They found that therapies with CM added to reinforce attendance and drug abstinence were more effective in increasing retention than the therapies alone (however, their study involved only eight sessions).

As noted by Bauer et al. (2001), there is often a gap between the performance of an intervention under highly controlled conditions and its performance in general clinical settings – the “efficacy-effectiveness gap” (IOM, 1985). In general, CM has been shown to be efficacious in tightly controlled studies with substance abusers. Recently, two studies showed effectiveness at a community level. Ledgerwood et al. (2008) found that participants who enrolled in community treatment programs during periods of CM implementation attended a greater number of therapy sessions than those who enrolled when CM was not in effect. Walker et al. (2010) showed successful implementation of CM in existing community treatment groups; implementation of CM resulted in a significant increase in outpatient attendance. However, these studies did not focus on CJ populations. Trotman and Taxman (2011) noted the considerable barriers encountered in a study of CM implementation by probation officers. For example, reinforcement was necessarily delayed up to six days because clients met with their probation officers only once a week.

Clearly, findings on the use of CM with criminal justice clients are mixed, but there are indications that it could be a useful tool in improving treatment retention for this difficult population. No recent study has assessed the effectiveness of CM with a high-risk population of adult parolees. It remains to be seen whether CM is generalizable to and effective in community treatment settings given the constraints and pressures of treating high-risk drug-abusing offenders.

Current Study

The purpose of this study was to move CM from the research setting to the community treatment setting to determine whether it is effective for promoting treatment admission and retention in a program for high-risk and high-needs offenders. The hypothesis for the Admission Phase was that participants in the group receiving an admission incentive will be more likely to show up for admission to the Residential Program than will participants in the group receiving information only. The hypothesis for the Attendance Phase was that participants in the group receiving attendance incentives will have a longer time in treatment than will participants in the group receiving information only.

Method

Setting

The study consisted of two phases: the Admission Phase and the Attendance Phase. The Admission Phase took place in a Substance Abuse Program (SAP) for men operated by HealthRIGHT360 at the California Substance Abuse Treatment Facility (SATF) prison in Corcoran, California. Participants were recruited at SATF from February 2011 through May 2012. The SAP program followed a modified therapeutic community model, and clients participated from three to six months. Clients participated in 20 hours per week of required programming, plus 10 or more hours of optional activities. As a client neared his parole date, he worked with his transitional counselor to develop a parole plan, which usually included a referral to a community treatment program. Clients did not have to accept a referral, nor were they required to follow-up on the referral once released (i.e., community treatment was voluntary).

The Attendance Phase was implemented at the HealthRIGHT360 Residential Treatment Program (the “Residential Program”) located in Los Angeles, CA; and subjects were recruited in this phase from September 2010 through March 2012. The Residential Program served 186 male clients at any one time and had 30–40 admissions per month. Clinical staff members carried caseloads of approximately 15 clients each. Clients attended residential treatment for three months followed by an additional two months of outpatient treatment. Within the overall therapeutic community model, the residential phase of treatment provides services to address a variety of client needs, with an emphasis on employment, to assist parolees in navigating the difficult transition from prison to community living. During their time at the program, residents learn and follow program rules, work on substance abuse and mental health issues including relapse prevention, attend AA or NA meetings, address family and parenting issues, address criminal thinking, and generally learn the skills and attitudes that will help them “beat the streets.” Most of these activities occur within group sessions, although individual counseling is also available as needed.

Participants

To be eligible for the Admission Phase, participants in the SAP at SATF had to meet the following eligibility criteria: At least 18 years of age, English speaking, within 60 days of parole, agreed to attend HealthRIGHT360 Residential Treatment Program after release, able to provide informed consent and complete the baseline interview. Given CDCR criteria for referral to community drug treatment programs, SAP participants who were sex offenders or who were seriously mentally ill were not eligible for referral to the community residential program.

All study subjects from the Admission Phase who enrolled in treatment at the Residential Program were able to volunteer for participation in the Attendance Phase. In addition, all other parolees entering the Residential Program were also eligible to be part of the study. To be eligible for the Attendance Phase, parolees had to meet the following eligibility criteria: At least 18 years of age, English speaking, within two to four days of admittance, had used drugs prior to last incarceration, able to provide informed consent and complete the baseline interview. Thirty-three subjects were in both phases of the study.

Procedure

Treatment program staff participated in the revision of the intervention manual (Prendergast & Hall, 2011), which was initially based on two previous intervention manuals, Budney and Higgins’ “A Community Reinforcement plus Vouchers Approach: Treating Cocaine Addiction” (1998) and “HAART Adherence Strategies for Methadone Clients Who Are HIV-Positive” (Haug, Sorensen, Gruber, Lollo & Roth, 2006). Taking into consideration discussions with program staff and study design, we adapted elements of these manuals to reflect the “real world” of community residential treatment for parolees. For instance, voucher payments were banked according to HealthRIGHT360 procedures and the length of reinforcement was adjusted to reflect the usual length of post-release treatment (22 weeks). Residential program staff received a half-day training session on principles of contingency management from an experienced trainer. We provided additional training on specific study procedures for those staff who were designated to provide vouchers or who were involved in

the program's banking procedures. They also participated in weekly project meetings with research staff.

For Admission Phase recruitment, program staff at the SATF SAP provided a list of potentially eligible study participants to UCLA research staff. Research staff then met with prisoners on the list and provided them with IRB-approved study information. Those interested in volunteering for the study were consented and randomized. A similar procedure took place during the Attendance Phase at the Residential Program where treatment staff members provided a list of potential participants to the research team. Available clients were approached by trained research staff who provided them with IRB-approved study information. The consent, interview, and randomization processes were performed after the clients volunteered to take part in the study.

Within seven days of enrollment in the study, participants completed the baseline assessment interview in a private setting. Admission Phase subjects completed the baseline interview at SATF. Attendance Phase subjects completed the baseline interview at the Residential Program, except for those who came from SATF because their data had already been gathered. Twelve months after the end of the intervention (i.e., 17 months following the intake date at the Residential Program), we attempted to contact all study Attendance Phase participants for a follow-up interview. Because admission to treatment was the only outcome for the Admission Phase participants, there was no need to conduct a follow-up interview with them. Thus, participants in the Admission Phase were not followed-up unless they had participated in the Attendance Phase.

Clients recruited at the Residential Program had a variety of parole statuses. They were: SAP graduate, Non-SAP Graduate (NSG), In-Custody Drug Treatment Program (ICDTP) participant, and Post-Release Community Supervision (PRCS) participant. SAP graduates were parolees who had completed a SAP while in prison. Non-SAP graduates were parolees who did not participate in an in-prison drug treatment program, but who had been referred to post-release drug treatment by their parole officer. ICDTP clients were considered to be in-custody and were not allowed to leave the program premises without a staff member. PRCS clients were prisoners whose most recent conviction did not include a violent crime and were released to community supervision in accordance with California's Assembly Bill 109 realignment legislation.ⁱ

Both phases of the study used random assignment and included one group that received no incentives and another group that received incentives (vouchers) to reinforce the targeted behavior of each phase. Participants were randomized using the Urn Randomization Program (version 1.10, developed at Yale University for Clinical Trials Network studies). Urn randomization is a procedure designed to maintain balance and minimization of bias

ⁱIn April of 2011, California's Governor signed AB109 to reduce the number of offenders in state prisons. This caused programmatic shifts that affected study recruitment. In the initial stage of recruitment, in-prison substance abuse programs (SAPs) were cut in length from 6 months to 3 months. During the last year of study recruitment, the in-prison treatment length returned to 6 months. This resulted in a temporary decrease in the number of SAP graduates to be released to the residential treatment programs and, therefore, a decrease in the potential candidates for this study. To cope with these changes, we made adjustments to study inclusion criteria for the Attendance Phase. Most importantly, we included participants who were Non-SAP Graduates (NSG), in the In-Custody Drug Treatment Program (ICDTP), and in the Post-Release Community Supervision (PRCS) program.

across study groups for selected characteristics expected to be related to outcome (Stout, Wirtz, Carbonari, & Del Boca, 1994). Variables associated with drug use crime outcomes were used in the urn randomization process. These included: ethnicity (African American vs. Caucasian, Hispanic, and Other); risk for recidivism using the score on the Lifestyle Criminality Screening Form (0–7 vs. 8–20); and parole status (SAP, NSG, ICDTP, or PRCS). For the Admission Phase, the research interviewer at SATF randomized using only the first two items because all subjects leaving from SATF to the Residential Program were voluntary SAP graduates.

The Admission Phase consisted of 60 participants, with 31 randomized to the Admission Incentive Group and 29 to the Education Group.ⁱⁱ For the Attendance Phase, 202 participants were enrolled, with 104 randomized to the Attendance Incentive Group and 98 to the Education Group (see Figure 1).ⁱⁱⁱ

For the Admission Phase, subjects assigned to the Admission Incentive Group had the opportunity to receive a \$50 voucher at the Residential Program if they showed up within 2 weeks following release from incarceration, completed the intake process, and remained at the program for at least 48 hours. Each participant assigned to the Education Group received a single 30-minute educational session in which the research assistant reviewed information in a pamphlet that described research on the long-term benefits of attending aftercare and provided information on the HealthRIGHT360 Residential Program. Subjects in the Education Group did not have the opportunity to receive a voucher for enrolling in community treatment.

In the Attendance Phase, participants assigned to the Attendance Incentive Group received monetary vouchers for attendance, as described below. Participants in the Education Group did not have the opportunity to receive vouchers for attendance, but they did receive the same information and pamphlet as was provided to Education Group participants in the Admission Phase.

The Attendance Phase lasted for five months (22 weeks), which included three months of residential treatment and two months of outpatient treatment. During the residential portion, the days of attendance reinforcement were Monday through Friday. During the outpatient portion, clients were expected to attend three days a week.^{iv} Those in the Attendance Incentive Group were provided vouchers for attendance using an escalation-reset schedule (Higgins et al., 1994). Attendance Incentive Group members received \$2.50 per day for the first week (\$12.50 total for the week), \$3 per day for the second week (\$15.00 total), \$3.50 per day for the third week (\$17.50 total), and so on (see Table 1). Voucher amounts increased by \$2.50 per week, each week, if perfect attendance was maintained. Upon successful completion of weeks 5, 10, and 15, clients received a bonus voucher worth \$10.

ⁱⁱ63 were recruited into the Admission Phase, but research staff later found that two did not meet eligibility criteria (sex offender and early parole incentive clients) and one refused to complete the baseline interview.

ⁱⁱⁱThere were 204 participants enrolled in the Admission Phase, but two participants were dropped because their baseline interviews were incomplete. Since these participants did not complete the entire intake process, they were not eligible for randomization.

^{iv}Although participants remained in residential treatment seven day per week, most treatment activities took place Monday through Friday, and thus the decision was made to provide vouchers during those days. That way, vouchers would be consistently distributed by the same program staff.

Residential Program staff kept track of attendance (with parallel accounting by research staff) and awarded the vouchers since we believed that if CM is to work in typical treatment programs, the program staff need to understand CM procedures and be able to integrate them with other program activities.

Attendance and vouchers were recorded daily. Colorful voucher slips indicating the amount earned were awarded five days per week. During the 22 weeks of intervention, if a client had an unexcused absence, the value of the voucher was reset to the beginning amount (\$2.50 per day) for two weeks. After two weeks of perfect attendance, the client began earning at the level he had previously achieved.

Clients received access to their earnings on the following week, and money dispersal occurred via established Residential Program banking rules. Clients were allowed to request a withdrawal from their bank account twice a week (on Tuesdays and Thursdays). Requests totaling more than \$40 required an explanation and staff approval. Voucher earnings could be accrued over time for a large pay-off at the end.

Over the 22-week intervention, participants in the Attendance Incentive Group could earn a maximum of \$882.50. Since some clients did not attend sessions and others left treatment early, the average amount received was \$392.08.

Residential Program staff who tracked attendance and provided vouchers knew which clients were in the Incentive Group; however, counseling staff were blinded as to whether an individual was in the study and the group to which he was assigned, although they might have found out informally. Research Staff were not blinded to group assignment since they delivered the educational session soon after assignment. At follow-up, interviewers were not blinded to group assignment since the same interviewers conducted baseline and follow-up interviews.

The following boards and agencies approved study procedures: UCLA Institutional Review Board, federal Department of Health and Human Service's Office for Human Research Protections concurrence with IRB determination of 45 CFR 46.306(a)(2)(i) (Prisoner Approval), California Committee for the Protection of Human Subjects, California Department of Corrections and Rehabilitation Research Branch, HealthRIGHT360 Institutional Review Board, and UCLA Integrated Substance Abuse Programs Data and Safety Monitoring Board.

Measures

The baseline and follow-up assessments included questions on demographic characteristics, physical health, mental health, criminal history, drug use history, quality of life, and HIV-AIDS risks; the questions in these domains have been asked in the researchers' previous studies of offenders in treatment. The assessment interview also included a number of standardized psychosocial instruments. The Barratt Impulsiveness Scale Version 11 (BIS-11) was administered because impulsivity has been found to be a significant moderator of relapse, particularly within contingency management studies (Dawe, Gullo, & Loxton, 2004; Reilly, Roll, & Downey, 2000). This scale is a 30-item self-report measure of three aspects of

impulsiveness: attention impulsiveness, motor impulsiveness, and non-planning impulsiveness. It has excellent internal reliability and clinical utility for measuring impulsiveness in inmate populations (Patton, Stanford, & Barratt, 1995).

The Texas Christian University Client Evaluation of Self at Intake (TCU CESI) form (Simpson & Joe, 1993) includes short scales for treatment motivation, psychological functioning, social functioning, and criminal thinking. The motivation scales correspond to the Stages of Change model as delineated by Prochaska and DiClemente (1986). The Problem Recognition scale has good internal reliability (Cronbach alpha = .88) and is a self-assessment of drug use consisting of nine items that are intended to differentiate between those who have begun the contemplation stage and those who remain in the precontemplation stage. The Desire for Help scale has adequate internal reliability (alpha = .77) and consists of seven items assessing the transition from general acknowledgment of a drug problem to recognition of the need for help. Finally, the Treatment Readiness scale has eight items intended to assess willingness to enter and comply with treatment, and represents the completion of the above transition and the beginning of the action stage, and has adequate internal reliability (alpha = .72; Simpson & Joe, 1993; George & Mallery, 2003; Nunnally, 1978).

The Brief Symptom Inventory (BSI) (Derogatis & Melisaratos, 1983) is a 53-item questionnaire derived from the Symptom Checklist-90 that assesses nine medical and psychological symptom dimensions: somatization, obsession-compulsion, interpersonal sensitivity, depression, anxiety, hostility, phobic anxiety, paranoid ideation, and psychoticism, which can be combined to form a Global Severity Index. The BSI has acceptable to good internal consistency (Cronbach alpha = .71 to .85) and moderate to strong test-retest reliability ($r = .68$ to $.91$) across the dimensions (Derogatis & Melisaratos, 1983).

Risk for recidivism was determined using the Lifestyle Criminality Screening Form (LCSF; Walters, 1990; 1991; 1998a; 1998b). The LCSF consists of 14 items that factor into four scales related to criminal lifestyle: Irresponsibility (non-support of child, school dropout, longest job held, fired or quit job), Self-Indulgence (drug abuse history, marital background, tattoos), Interpersonal Intrusiveness (any prior intrusive offense, including burglary, robbery, violence against others, arson, and sex offenses, number of intrusive offenses, ever used weapons in an offense, physical abuse of significant others), and Social Rule Breaking (arrest history, age at first arrest, school disciplinary problems). The LCSF scale has acceptable internal consistency (Cronbach alpha = .70) and strong test-retest reliability ($r = .93$). Walters and McDonough (1998) found that the full-scale score to be more predictive of revocations for federal probationers than scores on the individual scales; thus, the full-scale score was used in analysis. It should be noted that we cut one item from the scale, "history of drug or alcohol abuse," because study inclusion criteria required that all our subjects had a history of drug or alcohol abuse and thus would have the same score. As a result, the usual cutoff of 10 suggested by Walters (1998a) moved down to 8 points out of a possible 20. As noted above, the score was used as part of the urn randomization procedure using the cutoff score of 8 (denoting high risk of recidivism).

Anti-Social Personality Disorder was evaluated in the study using the Structured Clinical Interview for DSM-IV-TR. The SCID (First, Spitzer, Gibbon, & Williams, 1998) provides diagnoses of Axis I mental disorders and Axis II personality disorders consistent with the Diagnostic and Statistical Manual of Mental Disorders, text revision (DSM-IV-TR) (American Psychiatric Association, 2000). Inter-rater reliability has been reported in several studies and ranges from adequate to excellent (Lobbestael, Leurgans, & Arntz, 2011). For this study, only the Axis II section on Anti-Social Personality Disorder was administered at baseline. Interviewers received training in the use of the SCID.

Analysis

The primary outcome for the Admission Phase was admission (as defined above) to the Residential Treatment program. The primary outcome of the Attendance Phase was the number of days in attendance at the Residential Treatment program. We tested both hypotheses using an intent-to-treat design. Both outcomes are based on program records. Attendance for this study was counted starting from the first Monday following recruitment. Attendance maxed out on the Saturday of the 22nd week, for a maximum of 153 possible days of attendance. Successful discharges received credit for fully attending the final day of treatment, while unsuccessful discharges did not. If a participant left treatment but returned before the end of the 22nd week, separate treatment episodes were tracked and added together for a final total. Unexcused absences were also tracked and deducted from participants' final totals as needed.

For the Admission Phase, the outcome measure of the two groups were compared using the Chi-square test (SPSS version 21). For the Attendance Phase, a Cox Regression survival model was developed using SAS version 9.3 (calculation) and SPSS version 21 (graphics). To do this, we selected demographic and other variables likely to have a relationship with time-in-treatment as indicated by previous research or baseline bivariate correlations. These potential covariates were: age, age at first arrest, number of arrests, number of incarcerations, race or ethnicity, recidivism risk, impulsiveness, psychological symptom severity, anti-social personality disorder, and parole status. Then, starting with all the potential covariates, we performed manual backward stepwise Cox regressions examining model fit with time-in-treatment. We selected predictors for the final model that had a *p*-value of .2 or less (Hosmer & Lemeshow, 2004). The ASPD variable was retained in the final model because its exclusion altered the model fit for the other covariates. Because of non-normal distribution, we recoded the continuous variable age of first arrest into a four-level categorical variable (1 = 12 or less, 2 = 13 to 15, 3 = 16 to 18, and 4 = 19 or older). Because of a skewed distribution of times arrested, we recoded the values into four categories (1 = 10 or less, 2 = 11 to 20, 3 = 21 to 30, and 4 = 31 or greater). The PRCS parole status category was recoded to the SAP category because there were only four individuals in that group and because all four had completed a prison SAP. The regression model was censored at 153 days—the end of the 22-week intervention. The variables in the resulting model are shown in Table 4.

Results

Participant Characteristics

The study groups in the Admission Phase did not significantly differ on any background variables (see Table 2 for more details on participant baseline characteristics). On average, they were 43 years of age; were nearly 16 years old at the time of their first arrest; and in their lifetimes, 60% had been arrested 11 times or more. Sixty-five percent met DSM-IV-TR criteria for ASPD. Admission clients' CESI Problem Recognition and Desire for Help scores were close to the norms for offenders in treatment (32.46 and 39.05, respectively). Their mean Treatment Readiness score was above the norm of 34.48, and closer to the 75th percentile of 41.25, indicating greater treatment readiness than the offenders on whom the scale was normed, perhaps because they had nearly completed in-prison treatment (Joe, Broome, Rowan-Szal & Simpson, 2002; Simpson, 2004; Simpson & Joe, 1993; Simpson, Knight, & Dansereau, 2004).

In the Attendance Phase, the groups did not significantly differ on background characteristics (see Table 2). Clients had a mean age of 44 years, and they reported an average age of first arrest of 16. In their lifetimes, 69% had been arrested 11 times or more. Sixty-nine percent of Attendance Phase subjects met DSM-IV-TR criteria for ASPD. Nearly half of them had received substance abuse treatment in prison (SAP), and it is likely that as a result of that prior treatment, their Problem Recognition and Treatment Readiness scores were elevated (near or above the 67th percentile) compared to other criminal justice clients, indicating that they had better problem recognition and treatment readiness than other criminal justice populations (Joe et al., 2002; Simpson, 2004; Simpson et al., 2004).

Admission Phase Results

There was no main effect for incentives in the Admission phase of the study. Of the two Admission groups, 60% of the Admission Incentive subjects and 64% of the Education subjects arrived for admission to the Residential Program ($p=.74$).

Attendance Phase Results

At the end of the study intervention period (i.e., five months after treatment entry), 22% of the Attendance Incentive Group were still in treatment compared with 27% of the Education Group ($p=.46$). For days in treatment (Table 3), we found that the total sample had a mean of 80 days in treatment (SD 58.90); the Attendance Incentive Group had a mean of 79 (SD 59.34) and the Education Group a mean of 82 (SD 58.70, $p=.84$). Using Cox regression with covariates of Group (Incentive vs. Education), age, age at first arrest, ASPD, recidivism risk (LCSF score), and type of parole status, we found that age, first arrest age, and type of parole status predicted time in treatment ($p < .05$), but Group did not (see Table 4). Time to treatment dropout was predicted by the model as a whole (likelihood ratio chi-square statistic $[9] = 28.4$, $p=.0008$). In this model, for each year's increase in age, our participants' hazard of dropping out of treatment during the study period decreased by 2%—thus, older participants stayed in treatment longer.

Participants who were first arrested before age 13 were more than twice as likely to drop out of treatment during the study period compared to those who were first arrested at age 19 or later. Also, compared to those who went through the SAP program, Non-SAP Grads and ICDTP participants were at increased hazard to drop out of treatment early; with Non-SAP Grads twice as likely and ICDTP participants about 1.5 times as likely to drop out early. Figure 2 presents the survival curves associated with the Cox regression analysis of time to treatment drop out.

Discussion

This randomized clinical effectiveness study was intended to test whether providing incentives to parolees for admission to and attendance in the “real world” setting of a community substance abuse treatment program (consisting of residential and outpatient phases following prison treatment) would (1) increase the likelihood of showing up for treatment, (2) increase the length of time of participating in the program, and (3) improve post-program outcomes. The current analysis focuses on the impact of incentives on admission and length of attendance. The Admission phase of the study was fairly simple—participants in the Admission Incentive Group would receive a \$50 voucher for enrolling in a treatment program to which they said they had already planned to go. Even though the amount of the voucher was suggested by program staff as an appropriate incentive for their clients, we found that it did not make a difference in post-prison treatment admission.

In the Attendance phase of the study, participants in the treatment group received a voucher redeemable for cash for every day that they were in the program, with the amount of the voucher increasing in value each successive week in attendance. Someone who continuously attended 22 weeks of residential and outpatient treatment would have received \$882.50. Participants in the Education Group did not receive vouchers for attendance. Despite the regular reinforcement of attendance and despite the potential large payout, the number of days that the participants in the Attendance Incentive Group stayed in treatment was virtually the same as that of the participants in the Education Group. Similarly, time to dropping out of the program was not statistically different between the two groups. The average amount received by the treatment group was \$392.08.

Given the overall strong research base for contingency management with clients in substance abuse treatment programs, we had hypothesized that contingency management would also be effective with an offender population, with only the magnitude of the effect being in question. That there was no statistically significant effect on either target behavior (admission or attendance) was unexpected. Although the results of this study do not weaken the accumulated evidence in support of contingency management as an effective technique in treating substance abuse, they do raise questions about whether there may be certain populations or settings where it might not be effective or much less effective. Following the model developed by Cartwright and Hardie (2012) on evidence-based policy implementation, we can say that contingency management has been shown in numerous studies to work “there” (i.e., in substance abuse treatment programs) to change the behavior of clients with drug abuse problems. Why did it not work when we tried it “here”—within a residential treatment program for parolees with the intent to lengthen time in treatment?

What was different about “here” that resulted in an evidence-based treatment not producing the intended outcome? (To simplify the subsequent discussion, we will focus only on the attendance phase of the study.)

The study was designed to determine whether providing incentives to parolees for attendance would improve their length of stay in treatment compared with those who received the same treatment but without incentives. Various considerations need to be addressed before it can be concluded that this study was a fair and rigorous test of incentives with this population. These considerations fall under three categories: theory, design, and implementation.

Theory

Contingency management is well grounded in behavioral theory (Bigelow & Silverman, 1999) and has been extensively evaluated in well-designed studies (Higgins & Silverman, 1999; Higgins et al. 2002; Petry et al., 2005; Roll et al., 2006). It has consistently been found to improve outcomes, with effect sizes higher than most other treatment approaches (Lussier et al., 2006; Prendergast et al., 2006). In addition, some studies examining the use of CM to promote treatment program retention indicate that reinforcing attendance does lead to longer time in treatment (Carroll et al., 2006; Helmus et al., 2003; Elk, Mangus, Rhoades, Andres & Grabowski, 1998; Jones et al., 2001; Ledgerwood, et al. 2008). Behavioral theory would suggest that positive reinforcement (the theoretical foundation for contingency management), when administered in accordance with standard procedures, results in desired behavior change, regardless of the population. But despite theory, it remains to be determined empirically whether the outcomes predicted by the theory extend to particular populations, in this case parolees.

Design

The study allocated participants to the treatment or the control conditions using a computer-based urn randomization program. There were no known allocation errors. For the Attendance phase, the intended (and final) sample of 200 was large enough to detect a medium effect size. The two groups were similar on all measured demographic and background variables. The contrast between the Attendance Incentive Group and the Education Group would seem to be strong since the latter consisted of only a single 30-minute educational session plus a brochure on the importance of attending aftercare. The study eligibility criteria placed few restrictions on who could participate. Apart from the incentives provided to the treatment group, all study participants received the same treatment services from the same clinical staff. Based on the incentive levels in Lussier et al. (2006), our incentives were moderate on a per day basis. The study participants were under different types and degree of supervision from parole and other criminal justice agencies, but these differences were included in the Urn randomization and evenly distributed between the two study conditions. Finally, since the attendance outcome was based on program records, the full sample was available for analysis. Given the nature of the study, research and clinical staff and clients were not blind to the study conditions. Finally, the analysis used a conservative intent-to-treat approach. The results do not appear to be weakened by a faulty or poorly executed research design.

Implementation

The findings might also have been influenced by how well the intervention was implemented. We developed a detailed manual of the CM procedures for research and clinical staff and information materials for study participants that explained the procedures for receiving and redeeming the vouchers. Because this was an effectiveness study, we had the voucher procedures carried out by clinical staff rather than by research staff (as is often done in CM efficacy studies). Research staff monitored on a weekly basis the records maintained by clinical staff and provided feedback when they were departing from the protocol. Still, the regular duties of clinical staff sometimes made it difficult to strictly adhere to CM principles. In particular, a staff member was not always able to provide vouchers to clients at the end of each day in attendance--a departure from the principle that a voucher (reinforcer) should be provided immediately after the target behavior is exhibited. But even if the client did not receive the paper voucher, the voucher value for the day was recorded in the client's record and could be redeemed according to program rules. Turnover in staff may also have affected how faithfully the protocol was carried out since new staff needed to be trained in the study procedures. Overall, the intervention was carried out with a high degree of fidelity to the original protocol, probably higher than would be the case in a typical program due to the presence of researchers who monitored clinical staff activities. In short, there was no apparent serious theoretical, design, or implementation weakness that would undermine the validity of the findings.

As we noted, the population for this study can be characterized as consisting of high-risk and high-needs offenders. This study, combined with our earlier findings (Prendergast et al., 2008; Hall, et al., 2009) and those of others (LeTourneau, McCart, Asuzu, Mauro & Sheidow, 2013; Marlowe et al., 2008) involving a variety of CJ populations, seem to indicate that CM does not result in sustained abstinence or improved attendance with offenders. In a study evaluating the use of incentives in a prison-based treatment program (Burdon, De Lore, Dang, Warda & Prendergast, 2013), the results showed little evidence of a consistent and strong impact of incentives on during-treatment measures of psychosocial functioning and treatment participation. The results of that study indicated that the intervention affected female and male subjects differently and with some variables in the opposite direction of what was expected. By contrast, Carroll et al. (2006) found that therapies with CM added to reinforce attendance and drug abstinence were more effective in increasing retention than the therapies alone; however their population was very specific -- young adults, 18–25 years of age, who were marijuana dependent--and was not typical of most CJ populations. In addition, the Carroll et al. (2006) study involved very high-value vouchers (a maximum of \$880 in just eight sessions), which are unlikely to be adopted in treatment programs for offenders.

Petry, Alessi, and Ledgerwood (2012) recently reported on an effectiveness study in which clinicians in methadone maintenance clinics delivered CM and obtained significantly longer retention for the CM group. An important design difference from the current study was Petry's prize-drawing design (Petry, 2000), which provides variable-ratio reinforcement as opposed to the continuous reinforcement of the current study. Whether a variable-ratio

reinforcement CM protocol (rather than an escalation-reset protocol) would be effective with a high-risk parolee population would require further research.

Limitations

The findings need to be considered in light of several limitations. First, the sample was more heterogeneous with respect to criminal justice status than was originally planned. In the study design, it was expected that all study participants at the Residential Program would come from the Substance Abuse Program (SAP) at the Substance Abuse Treatment Facility (prison). Because of budget cuts at the California Department of Corrections and Rehabilitation, the size of the SAP was reduced and fewer clients were being referred to the community residential program. As a result, parolees with other types of criminal justice supervision were recruited into the Attendance Phase of the study. The characteristics of clients with these varying criminal justice statuses might have influenced the results, however, they were balanced between the two treatment groups. A second limitation might have been that the value of the incentives was too low or that the “rules” for receiving them were too complicated. It is difficult to determine what the optimal incentive value is for a given setting and population, but the higher it is, the less likely that programs would be able to adopt the use of incentives. In focus groups, participants did not appear to have difficulty understanding how the incentives worked and what they needed to do to earn them. Because this was an effectiveness study, the incentives were administered by program staff rather than by research staff. That meant that the actual administration of the incentives did not always strictly adhere to one of the standard contingency management procedures; namely, that incentives be provided when the behavior is demonstrated and as soon after the behavior as possible (Petty, 2000). In other respects, however, the incentive protocol was carried out in accordance with recommended CM procedures. A third limitation was that the outcome (retention) was determined from program records, which are subject to error, although it is unlikely that any error that was present would differentially affect one group more than the other. Fourth, the voucher incentives were competing with other incentives that, at least for some participants, were more powerful. For instance, as with most residential programs, participants could gain privileges as they positively progressed in the program. Similarly, they lost privileges for breaking program rules. Still, the point of the study was to determine whether voucher incentives were effective in promoting attendance within the context of a typical treatment program with all of its competing priorities and incentives. Fifth, it was not possible to keep participants and staff completely blind to study assignment, making it difficult to rule out the possibility that the behavior of either participants or staff (or both) was influenced, positively or negatively, by knowledge of which condition a participant was in. Finally, given the setting of the study (a residential treatment program in a large city) and the sample (high-risk parolees who volunteered for the study), caution is called for in how widely the findings can be generalized.

This study was focused on whether incentives would increase admission to and retention in a residential treatment program for parolees. The design of the study, using random assignment, balanced the study groups on characteristics and conditions other than the treatment, thereby limiting the likelihood that other factors were primarily responsible the outcomes. Given the lack of effect on parolee behavior from incentives, a number of other

options are open to policy makers and program staff to try to improve treatment participation, including client motivation (Hiller et al., 2002), therapeutic alliance (Cordess, 2002), and treatment matching using the principles of risk, needs, and responsivity (Andrews, Bonta, & Wormith, 2006).

Conclusions

In this effectiveness study of using incentives to improve admission to and attendance in community substance abuse treatment for parolees, providing incentives did not increase the likelihood that parolees would enroll in community treatment. Similarly, incentives did not improve retention in community treatment over providing a brief educational intervention. Of several background variables, only age, criminal history, and parole status predicted time in treatment. In light of other studies with similar findings, it is clear that more research on the use of CM with criminal justice clients is warranted in order to better understand how and with whom CM might be targeted to be effective. Taking into account this uncertainty, practitioners in CJ settings who are considering the use of incentives, at least as used in this study, should be aware that they may not produce the desired outcomes.

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Figure 1.
Research Design

Figure 2.
Days until treatment dropout: Cumulative survival function at mean of covariates

Table 1

Attendance Incentive Schedule

Intervention Week	Amount per day when vouchers are awarded 5 days/week	Amount per day when vouchers are awarded 3 days/week	Amount per Week Possible	Cumulative Earnings (if perfect attendance)
1	\$2.50		\$12.50	\$12.50
2	\$3.00		\$15.00	\$27.50
3	\$3.50		\$17.50	\$45.00
4	\$4.00		\$20.00	\$65.00
5 *	\$4.50		\$32.50	\$97.50
6	\$5.00		\$25.00	\$122.50
7	\$5.50		\$27.50	\$150.00
8	\$6.00		\$30.00	\$180.00
9	\$6.50		\$32.50	\$212.50
10 *	\$7.00		\$45.00	\$257.50
11	\$7.50		\$37.50	\$295.00
12	\$8.00		\$40.00	\$335.00
13		\$14.17	\$42.50	\$377.50
14		\$15.00	\$45.00	\$422.50
15 *		\$15.83	\$57.50	\$480.00
16		\$16.67	\$50.00	\$530.00
17		\$17.50	\$52.50	\$582.50
18		\$18.33	\$55.00	\$637.50
19		\$19.17	\$57.50	\$695.00
20		\$20.00	\$60.00	\$755.00
21		\$20.83	\$62.50	\$817.50
22		\$21.67	\$65.00	\$882.50

* Weeks 5, 10 and 15 are bonus weeks. During these weeks clients received bonus vouchers of \$10 upon successful completion of the week without any absences.

Table 2

Descriptive statistics for the Incentive and Education groups and total sample

	Admission Phase					
	Incentive N=31		Education N=29		Total N = 60	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>p¹</i>	<i>SD</i>
Age at baseline	43.45	9.82	42.94	11.74	.86	43.40 10.59
Age at first arrest	15.47	4.27	16.18	4.80	.55	15.65 4.53
Motivation						
Problem Recognition	33.63	9.47	30.99	10.79	.33	32.36 10.12
Desire for Help	38.67	7.53	38.45	8.30	.92	38.56 7.85
Treatment Readiness	39.46	5.68	40.67	5.77	.42	40.04 5.71
	%		%		<i>p²</i>	%
Race-Ethnicity					.46	
African American	66.67		60.71			63.79
Hispanic	23.33		14.29			18.97
Caucasian	6.67		17.86			12.07
Other	3.33		7.14			5.17
Number of lifetime arrests					.31	
<11	38.71		41.38			40.00
11 to 20	22.58		31.03			26.67
21 to 30	9.68		17.24			13.33
>30	29.03		10.34			20.00
Meets ASPD criteria	63.33		66.67		.79	64.91
Attendance Phase						
	Incentive N=104		Education N=98		Total N = 202	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>p¹</i>	<i>SD</i>

Admission Phase							
	Incentive N=31		Education N=29		Total N = 60		
	M	SD	M	SD	p ¹	M	SD
Age at baseline	44.05	10.17	43.18	10.51	.55	43.63	10.33
Age at first arrest	16.47	7.49	16.30	6.37	.86	16.39	6.95
Motivation							
Problem Recognition	36.53	9.01	36.90	9.23	.77	36.71	9.10
Desire for Help	40.41	7.34	41.41	6.23	.31	40.89	6.83
Treatment Readiness	39.03	6.83	39.79	6.50	.50	39.40	6.67
	%		%		p ²	%	
Race-Ethnicity					.27		
African American	56.73		44.90			50.99	
Hispanic	26.92		34.69			30.69	
Caucasian	10.58		16.33			13.37	
Other	5.77		4.08			4.95	
Number of lifetime arrests					.12		
<11	32.69		28.57			30.69	
11 to 20	27.88		33.67			30.69	
21 to 30	10.58		19.39			14.85	
>30	28.85		18.37			23.76	
Meets ASPD criteria	65.69		73.20		.25	69.35	
Parole status					.34		
SAP	51.92		41.84			47.03	
ICDTP	30.77		38.78			34.65	
NSG	17.31		19.39			18.32	

Note: ASPD = anti-social personality disorder; SAP = in-prison substance abuse program graduate, ICDTP = in-custody drug treatment program, NSG = Non-SAP graduate.

¹Continuous variables – independent samples t-test

²Categorical variables – Pearson chi-square

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Table 3

Admission and Attendance Phase Outcomes

Admission Phase						
	Incentive	Education		Total		
	%	%		<i>p</i>	%	%
Arrived for admission to treatment	60.00	64.29		.74	62.07	
Attendance Phase						
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>p</i>	<i>M</i> <i>SD</i>
	79.19	59.34	81.69	58.70	.84	80.41 58.90
Days in treatment						

Table 4

Cox regression results for time-in-treatment

Label	β	SE	p	HR	95% CI
Treatment Group: Education (2) vs. Incentive (1)	-0.13	0.17	.43	0.88	[0.63, 1.22]
Age at intake	-0.02	0.01	.03	0.98	[0.97, 1.00]
Age of first arrest					
< 13 vs. >18	0.73	0.29	.01	2.07	[1.18, 3.63]
13-15 vs. >18	0.43	0.26	.10	1.54	[0.92, 2.56]
16-18 vs. >18	0.18	0.27	.50	1.20	[0.71, 2.04]
Meets ASPD criteria	0.28	0.20	.16	1.33	[0.90, 1.96]
Lifestyle Criminality Screening Form	-0.06	0.04	.08	0.94	[0.88, 1.01]
Parole status					
ICDTP vs. SAP	0.48	0.19	.01	1.6	[1.11, 2.34]
NSG vs. SAP	0.69	0.23	.003	2.00	[1.27, 3.15]

Note. SE = standard error; HR = hazard ratio, CI = confidence interval, ASPD = anti-social personality disorder, ICDTP = in-custody drug treatment program, SAP = in-prison substance abuse program graduate, NSG = non-SAP graduate.