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Interim Methadone Treatment: Impact on Arrests

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Abstract

AIMS—This study examines the frequency and severity of arrest charges among heroin addicts randomly assigned to either interim methadone maintenance (IM) or to remain on a waiting list for methadone treatment. It was hypothesized that IM participants would have a: 1) lower number of arrests at 6 and 12 months and 2) lower mean crime severity scores at 6 and 12 months post-baseline.

METHODS—Available official arrest data were obtained for all 319 study participants for a period of 2 years before and after study enrollment. Crime severity ratings of charges were made using an established measure of crime severity.

FINDINGS—Participants randomly assigned to IM as compared to those on a waiting list had a significant reduction in number of arrests at 6 but not at 12 months from study enrollment. There were no significant differences in whether participants were arrested for a more severe crime but frequency of severe crime was relatively low in both groups. Additional post hoc analyses based on whether participants were in methadone treatment at 4 and 10 months after original random assignment to treatment condition revealed that those participants not in treatment at these follow-up assessment points were significantly more likely to be arrested and to have a higher mean crime severity rating at 12 and 24 months post-baseline assessment.

CONCLUSIONS—IM as compared to the waiting list condition, had a significant reduction in number of officially- recorded arrests from baseline to 6 months post-baseline. Those who were enrolled in methadone treatment at the 4 and 10 month follow-up assessment, regardless of initial assignment, had fewer arrests at 12 and 24 months post-baseline.

Keywords

methadone; interim; crime; arrests; heroin

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1. Introduction

Early studies of methadone maintenance consistently reported that patients treated with methadone not only reduced or stopped heroin use, but also found decreased criminal activity during treatment (Dole and Nyswander, 1965; Dole et al., 1969; DuPont and Katon, 1971). Several large-scale US multi-site studies with pre- and post-treatment designs conducted in the 1970s and 1980s found that individuals enrolled in methadone treatment for more than a few months reported greater decreases in criminal activity during and for some time following treatment as compared to treatment drop-outs (Ball and Ross, 1991; Hubbard et al., 1989; Simpson and Sells, 1983). However, the Drug Abuse Treatment Outcome Study (DATOS) conducted in the early-to-mid-1990s did not replicate these earlier findings of a reduction in self-reported crime among methadone maintenance programs (Hubbard et al., 1997).

While self-reported criminal activity gathered in confidential research interviews is considered reliable and valid by most researchers (Chaiken and Chaiken, 1990; Kinlock and Gordon, 2006) such information does not provide an objective measure of arrest rates and severity of criminal charges. Many policy-makers remain skeptical of self reported changes in antisocial behavior among heroin users who have become known to the criminal justice system, but are less able to dismiss or discount findings based on criminal justice system records. Therefore, although objective data on arrest rates tend to underestimate the number of crimes committed by heroin users, there is nevertheless considerable value in reviewing arrest records. A number of pre- post-treatment studies conducted in the 1970s used official arrest records to estimate the impact of methadone maintenance on criminal activity (Bowden et al., 1978; Cushman, 1972; DuPont, 1972; Haglund and Froland, 1978; Newman et al., 1973). These studies found a decrease in arrests during the treatment and post-treatment period compared to a pre-treatment baseline, but a study of 126 methadone maintenance patients conducted in the late 1990s that also used official arrest records did not find a significant decrease in arrests when the two-year pre- and post-treatment periods were compared (Rothbard et al., 1999). These authors hypothesized that the increase in cocaine use in the 1990s may have reduced the impact of methadone treatment on criminal activity.

Because US federal regulations that became effective in the early 1970s mandated that methadone maintenance programs provide some level of psychosocial services (Rettig and Yarmolinsky, 1995), nearly all of the studies of the impact of methadone treatment on crime were in actuality studies of the combination of methadone maintenance and psychosocial services. In fact, in the entire 40-year history of methadone maintenance, there have been only a few studies in the U.S. of providing minimal methadone services (Glasscote et al., 1972; Gruber et al., 2008; McLellan et al., 1993; Senay et al., 1973; Yancovitz et al., 1991). None of these studies examined the impact of minimal methadone services on official arrest data. Furthermore, reduced funding of treatment programs in the late 1980s also reduced the quality and quantity of psychosocial services available to patients in standard methadone treatment in more recent studies (Etheridge et al., 1995). Thus, the question of the length of time and extent to which methadone without psychosocial services can, under current social conditions, reduce crime remains an open one.

The parent study from which this report is drawn involved 319 heroin-dependent adults enrolling on a waiting list for methadone maintenance treatment for a single clinic who were randomly assigned on a 3:2 basis to immediate Interim Methadone (IM) or to remain on the waiting list. Thus, study participation afforded a number of individuals who would have remained on a waiting list the opportunity to enter IM treatment while providing participants with encouragement and referral to apply to other MTPs throughout the city. Interim treatment consisted of up to 120 days of supervised methadone dosing with emergency counseling only, followed by entry into a comprehensive methadone treatment program. Participants were

assessed at baseline, at the time they entered comprehensive methadone treatment (or at 120 days from baseline for those who did not enter comprehensive treatment) and six months thereafter. Participants who were out-of-treatment at follow-up were provided information about how to access treatment in the community. The study was approved and monitored by the Friends Research Institute's IRB.

In that study, we found that participants randomly assigned to IM reported sharp reductions in illegal behavior compared to a control group that remained on a waiting list for methadone treatment (Schwartz et al., 2006, 2007). The purpose of the present study was to determine if there were reductions in the frequency and severity of criminal behavior associated with IM treatment as measured by official arrest records. Here we report on the official arrest data spanning two years prior to and two years following enrollment in the parent study. There were two study hypotheses: Compared to the waiting list group, the IM group would have: 1) a lower number of arrests at 6 and 12 months from study enrollment and 2) a lower number of arrests for severe (violent) crimes at 6 and 12 months from study enrollment.

2. Methods

2.1. Arrest Data

Available arrest data were obtained on all 319 participants from the Maryland Department of Public Safety and Correctional Services through a memorandum of understanding and as proposed in the study design approved by the Friends Research Institute Institutional Review Board and to which all participants consented. Data included the dates of arrest, and the literal description of all the charges for each date of arrest (e.g., "bawdy house-prostitution", "burglary", "controlled dangerous drug possession", "larceny under $100").

Data for the participants were matched using names, social security numbers, and dates of birth. Examination of the arrest data was limited to the time period of two years before and two years after study enrollment for each participant in keeping with the time frames utilized in previous research (Newman et al., 1973; Rothbard et al., 1999). While we had no hypotheses for the data beyond 12 months pre- and post-baseline, we did explore findings with the 24 month pre- post data.

The severity of each crime for this study was rated by adapting the scale developed by Nurco and colleagues (1991) in which offenses involving the infliction of physical harm are considered the most severe (e.g., assault), followed by those involving loss or destruction of property (e.g., theft) and those in which there is no immediate victim (e.g., drug possession). The criterion for severity was based on the reviewers' ratings of the relative aversion average citizens would have to knowing that such a crime was committed in their community. These study participants had committed a total of 359 different types of criminal charges, which were rated independently on a scale from 1 to 7 by three of the authors (JHJ, RPS, and TWK). The scale was then used by a fourth independent rater (MSG) to rate the crimes, in order to replicate our use of the rating scale. Examples of the severity ratings for a variety of the charges are shown in Table 1.

The mean severity scores of the 359 distinct charges rated by the three judges were respectively 3.40, 3.38 and 3.38 while the mean score of the fourth independent rater was 3.45. The mean severity scores of the three raters were not significantly different ($p > .05$). The interrater reliability [*ICC*(2,*k;* Shrout and Fleiss, 1979)] was calculated to ascertain the degree of agreement between the raters. [*ICC*(2,*k*) evaluates the absolute agreement of the raters.] Because there was an independent rater used to provide replication information, two different *ICC*s were calculated: (1) between the original three raters, *ICC*(2,3), and (2) between the linear composite of the original three raters and the replicate rater, *ICC*(2,2). Results in both cases, .

97 and .97, respectively, indicated an extremely high degree of agreement, thus supporting use of the scale as a measure of crime severity.

For some analyses, the crimes committed were further categorized into more severe (5–7) or less severe (1–4) based on whether or not the crime involved violence (e.g., attempted murder) or could lead quickly to violence (e.g., burglary) to a person. In many instances a single arrest resulted in several different criminal charges. In these instances, only the most severe charge was used in the analysis of severity. We chose to classify charges in terms of severity because arrest rates are influenced by police policy. For example, a change in police arrest policy which sharply increases the number of arrests for minor ("quality of life") charges might increase the likelihood of arrest of drug-addicted individuals during that time period.

2.2. Statistical analysis

2.2.1. Predictor Variables—The predictor variables can be considered either treatment, explanatory, or control variables. The treatment variable, of primary interest in this study, was assigned Treatment Condition: IM *v*. Waiting List Control. Five additional explanatory variables were also examined: Age of first heroin use, age of first crime, lifetime number of incarcerations, lifetime number of prior drug treatment episodes, and lifetime number of months of cocaine use (all derived from the ASI and the Friends Research Institute's Supplemental Questionnaire; Nurco et al., 1991). The relationships between each of these five explanatory variables and the outcome variables were also of primary interest in this study because they are a measure of severity of addiction, prior treatment, and treatment history, and so may be associated with prognosis. The control variables were selected demographic and background characteristics of the participants: Gender, Age, Ethnicity (Black|Other *v*. White), Educational Level (i.e., highest grade completed, with grades beyond 12 indicating years in college), and Marital Status (married *v*. not-married).

Thirty-eight participants in response to detailed questions about criminal history denied ever committing a crime other than drug possession, and so information regarding age of first crime was necessarily missing. These missing values were 'plugged' with the appropriate total sample mean, and a coded vector, representing ever committed a crime [yes *v*. no], was included in all analyses to control for this 'missingness' (Cohen et al., 2003). The test of the coded vector was non-significant in all analyses, indicating that these 38 patients did not differ in any significant way from the remaining 279 patients with complete data on the respective outcome measure, so no further mention is made of this predictor variable.

2.2.2. Outcome Variables—There were two major categories of outcome measures, arrests and crime severity. In terms of arrests, outcome variables examined were arrested (yes *v*. no) and number of arrests. In terms of crime severity, the outcome variable examined was crime severity rating. The outcome variables were measured at three time periods: from baseline to 6, 12, and 24 months following study enrollment, respectively.

2.2.3. Analytic Approach—Separate regression analyses were conducted for each of the outcome variables. Poisson regression analyses were conducted for the count variable of number of arrests. (In order to control for under- or over-dispersion in the Poisson models, a scale factor was included in this model, and the standard errors and likelihood ratio tests of significance adjusted accordingly.) Arrested (yes *v.* no) and a committed severe crime (yes *v.* no) were considered binary variables assumed to follow a binomial distribution, and thus logistic regression was utilized, again allowing for a scale factor.

3. Results

3.1. Participants

For the purposes of this analysis, the two participants who had died for reasons unrelated to the study were omitted from all analyses, because they could not necessarily provide complete data over the time period in question. There were 187 (59%) men among the sample and the mean age of the participants was 41.4 (6.0). Of the total sample, 296 (93.4%) were black and 21 (6.6%) were white. There were no significant differences in baseline characteristics between the IM and waiting list control group including: age, gender, race, marital status (19.9% married), mean number of years of education completed (11.6 years), lifetime number of drug treatment episodes (1.7), age of onset of heroin use (22.9) or lifetime number of years of cocaine use (6.7).

3.2. Pre-Baseline findings

In terms of criminal justice history, there were also no significant differences between IM and control group in terms of self-reported months of lifetime incarceration (20 months), and age at first crime (20.7) at baseline. In addition, there were no significant differences between the two treatment conditions for either of the two arrest variables: arrested [yes *v.* no], frequency of arrests or for the crime severity variable: arrested for severe crime [yes *v.* no] for the 6 month [IM: $n = 32$ (16.2%) and Control: $n = 21$ (17.6%) arrested; IM: $M = .20$ and Control: $M = .23$ number of arrests; and IM: $n = 5$ (2.5%) and Control: $n = 4$ (3.4%) arrested for a severe crime, respectively] and 12 month [IM: $n = 60$ (30.3%) and Control: $n = 39$ (32.8%), arrested; IM: $M = .20$ and Control: $M = .23$ number of arrests; and IM: $n = 13$ (6.6%) and Control: $n = 5$ (4.2%) arrested for a severe crime, respectively] periods prior to study entry (all ps > .16).

3.3 Post-Baseline Findings

a) Arrested [yes v. no]—As shown in Table 2, the majority of participants in the overall sample were not arrested within the two years post-baseline. A total of 262 participants (82.6%) were not arrested during the first 6-month period, 233 (73.5%) were not arrested during the 12-month period, and 186 (58.7%) were not arrested during the 24-month period following study entry. There were no significant differences between the originally assigned study conditions in whether or not participants were arrested during any of the three time periods ($p = .18$, .96, and .75, respectively).

b) Number of Arrests—The number of arrests is a more sensitive measure of criminal activity than the categorical measure of arrest (arrested yes *v.* no) because a single participant may have been arrested multiple times. There was a significant difference between the IM and Control conditions in the mean number of arrests during the 6-month period following study entry, $p < .02$. The mean number of arrests in the interim and control conditions were .20 ($SE = .06$) and .34 ($SE = .09$) arrests, respectively. However, in the intent-to-treat analysis the effect of original assignment was no longer significant during the 12- and 24-month periods following study entry (see Table 2).

c) Arrested for more severe crime [yes v. no]—Table 3 indicates the severity of the crimes charged at arrest for the study participants over the same three time periods post-baseline used to analyze the frequency of arrest above. As noted previously, the majority of the participants were not arrested during any three of the time periods between baseline and 24 months post-baseline. Of those arrested, the vast majority were arrested for non-severe crimes rated from 1–4 on the crime severity scale, described above. The total number of participants in both treatment conditions arrested for a more severe crime from 0–6, 0–12 and 0 –24 months post-baseline were respectively 7 (2%), 10 (3%) and 16 (5%). Results of logistic regression indicated that there were no significant differences between original assignment groups in

terms of whether participants were or were not arrested for a more severe crime (ratings 5–7) at any of the three follow-up points (*ps* respectively .23, .62, 1,0).

3.4. Analysis by Treatment Participation

Because some participants assigned to Interim Methadone dropped out of methadone treatment and some participants who remained on the Waiting List subsequently entered comprehensive methadone treatment, we also conducted an analysis of arrest data based on whether the participants were in treatment at the 4 and 10 month follow-up points, regardless of initial assigned group. This is in effect a survival analysis in that as reported previously, at 10 month follow-up 64.8% of IM and 24.5% of controls were in MTPs. In order to determine the relationship between entry and early retention in methadone treatment and arrest history of participants post-baseline, a secondary analysis was conducted, in which an additional explanatory variable was included in the statistical model: Treatment Retention [in treatment at 4 and 10 months post-baseline (Continuous Treatment) v. not in treatment at these points [Non-Continuous Treatment)]. It should be noted that there are two limitations to these data. First, it is possible that some participants who were coded as enrolled in treatment at both the 4 and 10 month assessment time points were not continuously in treatment; since a participant could have entered and left treatment and then re-entered between the follow-up points. Second, there were no data on their treatment status after the 10 month follow-up. A total of 183 participants were not in treatment at both the 4 and 10 month follow-up evaluations, while 134 participants were in treatment at both those points.

Finally, it should be noted that the results for the explanatory and control variables are reported in this section rather than in the previous section, in order to eliminate duplication of the presentation of this information in the two analyses.

3.4.1. Pre-Baseline findings—During the period prior to assignment, there were no significant differences between the 4- and 10-month Continuous *v.* Non-Continuous Treatment groups for either of the arrest variables: arrested [yes *v.* no], and frequency of arrests; or for the crime severity variable: arrested for more severe crime [yes *v.* no]. Moreover, the Non-Continuous Treatment at 4 and 10 months and Continuous Treatment groups did not differ from each other in number of arrests at 6 [M = .20, (SE = .06) *v.* M = .14 (SE = .05), p = .18] and 24 months [M = .69, (SE = .15) *v.* M = .53 (SE = .14), p = .15]. The sole exception was that during the twelve months prior to study enrollment the Non-Continuous Treatment at 4 and 10 months had a significantly greater number of arrests (M = .39, SE = .09) than did the Continuous Treatment group (M = .24, SE = .07), p = .039. Given the self-selection and these possible differences on pre-baseline arrest data, the corresponding pre-baseline arrest data variable was included as an additional control variable in the respective analyses.

3.4.2. Post-Baseline findings

a) Arrested [yes v. no]: As shown in Table 4, there were significant differences in the likelihood of arrest between the Continuous Treatment group and the Non-Continuous Treatment group at all three time points post-baseline (0–6 months, p = .007; 0–12 months, p < .001; and, 0–24 months, p < .001), with the Non-Continuous Treatment group much more likely to be arrested than the Continuous Treatment group.

In addition, age was a significant predictor of arrest at 6 [*OR* = .93, (*95%CI*: .87, .99)], 12 [*OR* = .94, (*95%CI*: .89, .99)], and 24 months [*OR* = .94, (*95%CI*: .89, .98)] post-baseline, while lifetime number of incarcerations was significant only at 6 months [*OR* = 1.01, (*95% CI*: 1.00, 1.02)]. [Significant *OR*s for the lifetime number of incarcerations in all analyses were quite close to 1; however, due to a small standard error, the resulting confidence interval for the estimate did not include 1, as in the present case.] These results indicate that, in each case,

the likelihood of any arrest [yes *v.* no] decreased with increasing age and increased with increasing number of prior incarcerations.

b) Number of Arrests: As shown in Table 4, the mean number of arrests was twice as large in the Non-Continuous Treatment than in the Continuous Treatment group across all three time periods, that is, at 6, 12 and 24 months post-baseline. This difference remained steady across all three time periods at a greater than two-fold difference. The mean number of arrests in the Non-Continuous Treatment group during the 12 and 24 months pre-baseline went up in the corresponding time periods post-baseline (.39 *v.* .40 and *.69 v.* .77, respectively). In contrast, the mean number of arrests dropped for the Continuous Treatment group (.24 *v.* .18 and .53 *v.* .37, respectively).

Moreover, age and lifetime number of incarcerations were significant predictors of number of arrests at 6, 12 and 24 months post-baseline, while number of previous drug treatment episodes was significant at both 12 and 24 months. These results indicate that, in each case, the number of arrests decreased with increasing age while increasing with an increasing number of lifetime arrests and number of previous treatment episodes.

c) Arrested for more severe crime [yes v. no]: Table 5 indicates the severity of the crimes charged at arrest for the study participants from baseline to 6, 12, and 24 months based on whether or not they were in treatment at the 4 and 10 month assessments. There was only 1 participant in the Continuous Treatment group arrested for a more severe crime compared to 6 participants in the Non-Continuous Treatment group at 6 months post-baseline. This ratio increased to 1 *v.* 9 and 3 *v.* 13 for two respective groups at 12- and 24-months post-baseline, respectively. This is so even though the groups were initially comparable in terms of severe crime arrests. However, logistic regression indicated that these differences did not reach the 0.05 level of significance (all *p*s > .064).

4. Discussion

The present study provides the first data in the US of which we are aware examining the impact of methadone treatment without counseling on crime and arrest. Analysis of the official State of Maryland arrest records of heroin-addicted adults receiving only IM (methadone and emergency counseling) found a significant reduction in the number of arrests for the IM group as compared to the Waiting List group from baseline to 6 months post-baseline. Since the likelihood of arrest is influenced by local police policy, this difference is noteworthy. From 2000 to 2005, the years for which participants' arrest data was obtained under a policy of arresting for quality of life crimes, the number of arrests in Baltimore City increased by 26.8%, from 77,314 to 98,083 (Janis, 2007). We would have expected increased arrests for all study participants during this time period, since heroin-dependent individuals are frequently considered "likely suspects."

That differences in arrest rates do not persist beyond the 6-month period in the intent-to-treat analysis is not surprising. Participants who were assigned to remain on the waiting list, unlike a group assigned to placebo in a pharmacological clinical trial, were given referrals to other treatment programs and had intention and encouragement to seek treatment elsewhere. Therefore, the original group assignments in our study were a transient grouping because there were drop-outs from IM treatment as well as entry into regular methadone treatment by those assigned to remain on the waiting list. In our intent-to-treat analysis these changes in group membership diluted the impact of IM treatment, and by 12 months post-baseline, there were no differences in arrest rates despite significant differences in treatment entry and retention at the 10 month follow-up (64.8% of IM and 24.5% of controls were in MTPs).

These findings show that for at least the first six months, methadone alone as compared to waiting list placement has substantial benefits for the community as well as for the heroin-addicted individual seeking treatment (Schwartz et al., 2006, 2007). We cannot assert that if study participants had remained on Interim Treatment for two years their arrest rates would have declined, because current US regulations do not permit Interim Treatment for more than 120 days.

Because of the changes in group membership (described above) following random assignment, we also conducted an analysis of arrest data based on whether the participants were in treatment or were not in treatment over the first 10 months of the study, regardless of initial group assignment. While this approach has its limitations, given the self-selection in terms of treatment entry, the corresponding pre-baseline arrest data variable was included as an additional control variable in these analyses. These analyses provide an additional perspective on the impact of entering and remaining in methadone treatment. According to the regulations governing IM, after the first 4 months of IM, participants assigned initially to IM treatment were admitted into regular methadone treatment where counseling (usually group counseling) was available and required. The greater than two fold difference in the mean number of arrests between the Non-Continuous Treatment and the Continuous Treatment groups was consistent at each of the follow-up points (6, 12 and 24 months).

The requirement that psychosocial services must be available to all patients at all times, when treatment resources in this country have been constrained, has had the effect of denying methadone treatment to those heroin-addicted individuals who seek it and could benefit from methadone alone. In some areas, where for-profit MTPs predominate, methadone treatment is available only to those who have insurance or who can pay for it. Many patients must choose medically supervised withdrawal when they can no longer pay for treatment in regular methadone programs. However, detoxification is not without risks. Recent studies indicate that opioid detoxification, whether as a part of treatment or a result of incarceration, is associated with an increased risk in overdose death in the month following detoxification (Davoli et al., 2007; Farrell and Marsden, 2008; Strang et al., 2003), a risk that is actually reduced by participation in treatment with methadone.

The findings have several implications for the criminal justice system. First, it should be noted that the majority of individuals were not arrested despite their self-report of committing crimes on a regular basis (Schwartz et al., 2006; 2007), generally consistent with previous findings indicating that only a small percentage of offenses committed by heroin-addicted individuals result in arrest (Inciardi, 2001; Nurco 1998). Thus, it would seem that providing IM treatment to satisfy demand and eliminate waiting lists might be a better crime reduction strategy than arresting large numbers of heroin–addicted individuals for non-violent crimes, regardless of their addiction and treatment status. Second, the low number of severe crimes was not significantly impacted by the provision of IM. It has been found that individuals with an early onset of criminal behavior may constitute a subtype of the heroin-addicted population that has a greater likelihood of committing severe crimes whether in or out of treatment (Hanlon, Bateman & O'Grady, 1999; Nurco, 1998). Criminal justice approaches to this population may be more fruitful than standard or IM treatment.

In the US, regulations present substantial barriers to providing IM treatment, including the requirement to obtain permission from the Director of the State Health Department to administer methadone on Sundays, Federal holidays (such as Christmas) and times when a clinic would otherwise be closed (such as during a snowstorm), and to transfer patients to comprehensive methadone treatment within 120 days of enrollment in interim treatment.

When the regulations that would permit IM were first proposed by the FDA they were opposed by many methadone treatment providers on the grounds that methadone alone was of little or no benefit (Rettig and Yarmolinksy, 1995). Vincent Dole (1991), one of the holders of the Investigational New Drug permit (IND) for the study of IM by Yancovitz and colleagues (1991), originally argued for the need for psychosocial services but in an editorial he came to believe that in the face of resource limits, IM should be made available. Further, where methadone treatment has been implemented in countries such as the UK, Australia, and elsewhere, the requirement for counseling and other services was not made mandatory, and reports from these countries indicate that methadone alone reduces not only heroin use but also drug-related deaths and self-reported crime (Bell et al., 1992; Gossop et al., 1999; Keen et al., 2000; Lewis and Bellis, 2001).

There are several limitations to the present study that should be mentioned. First, the participant sample consisted largely of African Americans from a moderate-sized US city. The extent to which the findings generalize to other populations is not known. Second, as mentioned above, Baltimore City had an aggressive arrest policy during the time of the study and therefore the findings may not generalize to cities with different police approaches.

This study, showing reduction in official arrest rates during the first six months post-baseline along with prior findings showing the benefits to individuals (Schwartz et al., 2006, 2007), strengthens the argument that the barriers to providing IM should be revisited. Changes in arrest rates are an important outcome variable to supplement self-reported criminal behavior because they may be more persuasive than self-report to some policy makers. We saw this effect on arrests despite the fact that only approximately 40% of the participants at 4-month follow-up had cocaine negative drug tests. Revisiting the barriers to IM is most critical for those communities were there are few subsidies for treatment (such as in some areas of California) and where many people cannot afford to pay for it (Kwiatkowski et al., 2000; Murphy and Rosenbaum, 1988; Rosenbaum et al., 1996). Programs already accredited to provide methadone treatment could treat additional patients with IM at less than 1/3 the cost of what is now mandated by accreditation standards.

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Table 1

Severity Rating of Arrest Charges

Severity Rating	Examples of Verbatim Description of Crime
1	Baudy House-Prostitution, Possession of Marijuana, Consuming Alcohol, Speeding Ticket
2	Pimping, Trespassing, Failure to Obey, Disorderly Conduct, Malicious Destruction of Property Less than $ 500
3	Theft Less than $500, Malicious destruction of property more than $500, Forgery, Uttering (writing a bad check)
4	Theft greater than $500, Carrying a handgun, Drug distribution or manufacturing
5	Burglary, Second degree assault, battery
6	Robbery with a deadly weapon, Assault first degree, kidnapping
7	Attempted first degree murder, first degree murder, rape

Table 2

Number of Arrests during the 6-, 12-, and 24-Month Post-Treatment-Entry Periods: Interim Maintenance (n = 198) *v.* Control (n = 119)

Number of Arrests	6 Months Post-Treatment-Entry		12 Months Post-Treatment-Entry		24 Months Post-Treatment-Entry	
Condition:	**IM**	**Control**	**IM**	**Control**	**IM**	**Control**
0	167 (84.3)	95 (79.8)	145 (73.2)	88 (73.9)	121 (61.1)	65 (54.6)
1	24 (12.1)	15 (12.6)	34 (17.2)	16 (13.4)	41 (2.7)	27 (22.7)
2	5 (2.5)	5 (4.2)	14 (7.1)	9 (7.6)	16 (8.1)	13 (10.9)
3	1 (0.5)	4 (3.4)	1 (0.5)	4 (3.4)	11 (5.6)	6 (5.0)
4	1 (0.5)	0	3 (1.5)	1 (0.8)	6 (3.0)	5 (4.2)
5	0	0	0	1 (0.8)	2 (1.0)	1 (0.8)
6	0	0	1 (0.5)	0	0	2 (1.7)
7	0	0	0	0	0	0
8	0	0	0	0	1 (0.5)	0
M (*SE*)	.20 (.06)	.34 (.09)	.33 (.09)	.39 (.11)	.61 (.14)	.76 (.18)

Cell entries are *n* (%) with the exception of the last row, which are model-derived exponentiated Means (*M*) and their Standard Errors (*SE*).

There were no Treatment Group differences for arrested (yes *v.* no) during the 6-, 12-, and 24-month post-treatment-entry periods, all *p*s > .18.

Mean number of arrests was significantly higher in the Control than in the Interim condition in the 6-month post-treatment-entry period, $\chi^2(1) = 5.56$, $p < .02$; this difference did not persist into the 12- and 24-month post-treatment-entry periods, both *p*s > .16.

Table 3

Severity of most severe arrest charge during the 6-, 12-, and 24-Month Post-Treatment-Entry Periods: Interim Maintenance (*n* = 198) *v*. Control (*n* = 119)

Crime Severity	6 Months Post-Treatment-Entry		12 Months Post-Treatment-Entry		24 Months Post-Treatment-Entry	
Condition:	**IM**	**Control**	**IM**	**Control**	**IM**	**Control**
No Arrest Charge						
0	167 (84.3)	95 (79.8)	145 (73.2)	88 (73.9)	121 (61.1)	65 (54.6)
Non-severe Arrest Charge						
1	12 (6.1)	8 (6.7)	23 (11.6)	11 (9.2)	34 (17.2)	27 (22.7)
2	3 (1.5)	1 (0.8)	4 (2.0)	2 (1.7)	5 (2.5)	2 (1.7)
3	2 (1.0)	5 (4.2)	6 (3.0)	6 (5.0)	12 (6.1)	9 (7.6)
4	8 (4.0)	9 (7.6)	13 (6.6)	9 (7.6)	16 (8.1)	10 (8.4)
Severe Arrest Charge						
5	0 (0)	1 (0.8)	1 (0.5)	2 (1.7)	2 (1.0)	2 (1.7)
6	6 (3.0)	0 (0)	6 (3.0)	1 (0.8)	8 (4.0)	4 (3.4)
7	0 (0)	0 (0)	0 (0)	0 (0)	0 (0)	0 (0)

Cell entries are *n* (%).

There were no Treatment Condition differences for being charged with a more severe crime (yes *v*. no) during the 6-, 12-, and 24-month post-treatment-entry periods, all *p*s > .2.

Table 4

Number of Arrests during the 6-, 12-, and 24-Month Post-Treatment-Entry Periods: Continuous Treatment (*n* = 183, 57.7%) *v*. Non-Continuous Treatment Groups at 4 and 10 month follow-up (*n* = 134, 42.3%) *

Number of Arrests	6 Months Post-Treatment-Entry		12 Months Post-Treatment-Entry		24 Months Post-Treatment-Entry	
Group:	**Continuous Treatment**	**Non-Continuous Treatment**	**Continuous Treatment**	**Non-Continuous Treatment**	**Continuous Treatment**	**Non-Continuous Treatment**
0	122	140	112	121	95	91
1	10	29	16	34	27	41
2	0	10	4	19	6	23
3	1	4	0	5	3	14
4	1	0	1	3	1	10
5	0	0	0	1	1	2
6	0	0	1	0	0	2
7	0	0	0	0	0	0
8	0	0	0	0	1	0
M (*SE*)	.14 (.05)	.29 (.07)	.18 (.06)	.40 (.10)	.37 (.10)	.77 (.16)

Cell entries are *n* (%) with the exception of the last row, which are model-derived exponentiated Means (*M*) and their Standard Errors (*SE*).

* Note: See text on Page 11 for the definition of Continuous and Non-Continuous Treatment Groups.

The Non-Continuous Treatment group had a greater likelihood of being arrested than did the Continuous Treatment group in the 6-, 12-, and 24-month post-treatment-entry periods, $\chi^2(1) = 7.16$ (*OR*=2.98, *95%CI* = 1.34, 6.61), $p = .007$, $\chi^2(1) = 12.81$ *OR*=3.41, *95%CI* = 1.74, 6.68, $p < .001$, and $\chi^2(1) = 12.17$, $p < .001$ *OR*=2.81, *95%CI* = 1.57, 5.03, respectively.

Mean number of arrests were significantly higher for the Non-Continuous Treatment group than for the Continuous Treatment group in the 6-, 12-, and 24-month post-treatment entry periods, $\chi^2(1) = 8.31$, $p < .005$, $\chi^2(1) = 13.21$, $p < .001$, and $\chi^2(1) = 14.65$, $p < .001$, respectively.

Table 5

Severity of most severe arrest charge during the 6-, 12-, and 24-Month Post-Treatment-Entry Periods: In-Treatment (n = 183) *v*. not -In-Treatment at 4 and 10 month follow-up (n = 134)

Crime Severity	6 Months Post-Treatment-Entry		12 Months Post-Treatment-Entry		24 Months Post-Treatment-Entry	
Group:	**In Treatment**	**Not In- Treatment**	**In- Treatment**	**Not In- Treatment**	**In Treatment**	**Not In- Treatment**
No Arrest Charge						
0	122 (91.0)	140 (76.5)	112 (83.6)	121 (66.1)	95 (70.9)	91 (49.7)
Non-severe Arrest Charge						
1	8 (6.0)	12 (6.6)	13 (9.7)	21 (11.5)	21 (15.7)	40 (21.9)
2	1 (0.7)	3 (1.6)	2 (1.5)	4 (2.2)	3 (2.2)	4 (2.2)
3	1 (0.7)	6 (3.3)	4 (3.0)	8 (4.4)	8 (6.0)	13 (7.1)
4	1 (0.7)	16 (8.7)	2 (1.5)	20 (10.9)	4 (3.0)	22 (12.0)
Severe Arrest Charge						
5	0	1 (0.5)	0	3 (1.6)	0	4 (2.2)
6	1 (0.7)	5 (2.7)	1 (0.7)	6 (3.3)	3 (2.2)	9 (4.9)
7	0	0	0	0	0	0

Cell entries are n (%).

There were no significant In- *v*. Not-In-Treatment group differences for a more severe arrest charge (yes *v*. no) during the 6-, 12-, and 24-month post-treatment-entry periods, all *p*s > .064.