

# Day Hospital and Residential Addiction Treatment: Randomized and Nonrandomized Managed Care Clients

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Male and female managed care clients randomized to day hospital ( $n = 154$ ) or community residential treatment ( $n = 139$ ) were compared on substance use outcomes at 6 and 12 months. To address possible bias in naturalistic studies, outcomes were also examined for clients who self-selected day hospital ( $n = 321$ ) and for clients excluded from randomization and directed to residential treatment because of high environmental risk ( $n = 82$ ). American Society of Addiction Medicine criteria defined study and randomization eligibility. More than 50% of followed clients reported past-30-day abstinence at follow-ups (unadjusted rates, not significant between groups). Despite differing baseline severities, randomized, self-selecting, and directed clients displayed similar abstinence outcomes in multivariate longitudinal models. Index treatment days and 12-step attendance were associated with abstinence ( $p < .001$ ). Other prognostic effects (including gender and ethnicity) were not significant predictors of differences in outcomes for clients in the treatment modalities. Although 12-step attendance continued to be important for the full 12 months, treatment beyond the index stay was not, suggesting an advantage for engaging clients in treatment initially and promoting 12-step attendance for at least a year.

*Keywords:* self-help, Alcoholics Anonymous, ASAM patient placement criteria, selection bias, social model

With few exceptions (Finney, Hahn, & Moos, 1996), reviews of treatment outcome studies have found that less costly, intensive outpatient programs may yield substance use and social outcomes similar to those of more costly inpatient programs for all but the most severely medically and psychiatrically compromised patients (Belenko, Patapis, & French, 2005; Miller & Hester, 1986). A landmark study by McKay (McKay, Alterman, McLellan, Snider, & O'Brien, 1995), which contributed to policy makers' decisions

regarding the trade-offs between inpatient and day hospital care, recruited and followed both randomized and nonrandomized male alcoholic veterans to examine treatment effects on outcomes under experimental and nonexperimental designs. Despite its importance, this study has not been replicated with samples that include women and that are more representative of the general population of treatment seekers or with potentially less costly types of community residential treatment that offer fewer structured treatment hours per day than traditional inpatient programs. This article presents findings from a study that addresses these gaps.

In their meta-analysis of relevant research on treatment setting effects, Finney et al. (1996) found that inconsistent findings between inpatient and day treatment programs could be explained largely by factors that had not been adequately explored and had little to do with the treatment setting per se, such as methodological decisions, including exclusion criteria and naturalistic versus experimental research designs. It also is possible that conflicting findings are caused because clients are sent to more intense treatment than is indicated by their problems at intake; treatment-matching studies show that undertreatment predicts poorer outcomes compared to matched treatment and that overtreatment provides no additional benefit (Magura et al., 2003). Since ethical considerations compromise studies' ability to randomize high-severity clients to lower levels of care, creative study designs are

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This study was supported by National Institute on Drug Abuse Grant R01 DA 12297. The opinions expressed here are those of the authors and do not necessarily reflect those of the managed health care organization sites that participated in the study. Requests for further information can be made at [www.arg.org](http://www.arg.org).

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required that address these real world issues of exclusion criteria, selection bias, and undertreatment.

This health services study was designed to examine two types of treatment settings, day treatment and community residential treatment, in an existing health care organization serving commercially insured managed care clients. Both randomized and nonrandomized (i.e., refusing and excluded from randomization) clients were recruited, which thus allowed us the advantages of a randomized clinical trial yet provided an additional opportunity to compare randomized to nonrandomized client outcomes. While randomized designs have the distinct advantage of maintaining treatment groups that are similar apart from random variation, researchers such as McKay et al. (1995) have questioned the external validity of such designs given that clients who agree to or are eligible for random assignment may well be different from the population to whom results are generalized (Institute of Medicine & Division of Mental Health and Behavioral Medicine, 1989; Strohmets, Alterman, & Walter, 1990). This concern is especially heightened in randomized trials that attempt to assign clients to treatments that require different levels of commitment. One is inclined to question whether the prognosis for clients willing to be randomly assigned to either a day hospital or a residential program is the same as the prognosis for clients in the usual care population seeking these treatment services. Further, is there something in the process of making one's own decision about the most suitable or desirable treatment that affects one's prognosis (McLellan et al., 1997; Miller, 1989)?

Moreover, many (if not all) inpatient and outpatient comparison studies have used exclusion criteria to disqualify clients with severe problems, who (conceptually) might especially benefit from the more intense inpatient or residential setting (Humphreys & Weisner, 2000; Tunis, Stryer, & Clancy, 2003). This is understandable in the realm of clinical trials; how can patients seemingly in need of more intensive treatment (e.g., residential) be ethically included in a trial in which they could be randomized to receive the less intensive alternative (e.g., day hospital)? For example, patient placement criteria established by the American Society of Addiction Medicine (1996; ASAM) include consideration of six dimensions for residential treatment, the last being environmental risk; it follows from these guidelines that patients whose home environment places them at (environmental) risk for alcohol and/or drug use should not be considered eligible for potential randomization to a day hospital program. How, then, can researchers establish the efficacy of residential treatment in a randomized trial? One solution is to require that the other five criteria for residential treatment be met, a compromise that brings to the trial a client pool that exceeds the criteria for intensive outpatient or day hospital treatment. This was the approach used in the present research. By design, this study (a) sought to randomize clients who exceeded placement criteria for intensive outpatient treatment and met criteria for all but one dimension (environmental risk) for low-intensity residential treatment (ASAM, 1996); (b) included as nonrandomized study participants those with environmental risk, the sixth dimension for residential treatment; and (c) invited those individuals who were eligible for randomization, but refused, to participate as nonrandomized participants. Both randomized and nonrandomized clients attended the same day hospital and community residential programs.

The first aim of this article was to test the relative effectiveness of day hospital versus community residential programs (chosen for their social model orientation) on abstinence at 6 and 12 months. We hypothesized that higher abstinence rates would follow for clients randomized to community residential programs than for clients randomized to day hospital programs via intent-to-treat protocols. Because community residential treatment has not been broadly evaluated (Borkman, Kaskutas, Room, & Barrows, 1998), especially with nonpublic clients, we draw support for this hypothesis from various sources. Evaluation of community residential programs that adhere to traditional social model ideals (i.e., emphasis on maintaining an environment conducive to recovery; Institute of Medicine, 1990) have reported that the hallmark of this setting is the opportunity for social interactions about recovery-related issues among peers in a homelike setting. This includes frequent on-site presence of alumni and community Alcoholics Anonymous (AA) and Narcotics Anonymous (NA) members, an emphasis on building clean and sober networks, and an ethic of volunteerism, which incorporates program upkeep and service (Barrows, 1998; Borkman et al., 1998). These informal interactions, which may be more constrained at traditional inpatient and outpatient programs by the brevity of contact and professional role (Humphreys & Noke, 1997), may serve as a mechanism through which the benefits from what happens outside formal treatment groups (Finney et al., 1996) are greater than they appear. As supported by prior research, this enhanced opportunity to engage in and practice a 12-step recovery program during treatment should lead to more 12-step involvement in the posttreatment period (McKay, Alterman, McLellan, & Snider, 1993) and, thus, better outcomes (Humphreys, Huebsch, Finney, & Moos, 1999; Magura et al., 2005).

The direction of this hypothesis additionally draws from a constellation of findings from Project MATCH (Project MATCH Research Group, 1998) and Veterans Administration studies (Moos, Finney, Ouimette, & Suchinsky, 1999) that compared 12-step-oriented treatment programs to other approaches. These studies found that patients in 12-step-oriented programs were more likely to be involved with 12-step (AA and NA) groups and have better outcomes as a result of that involvement, compared to patients in programs with other orientations. While the day hospital programs in our study were also 12-step oriented and encouraged 12-step meeting attendance, the community residential programs, with their social model approach, were an extreme version of 12-step orientation, relying primarily on 12-step principles and the 12-step mechanism of peer helping (including counselors sharing their 12-step experiences; Kaskutas, Marsh, & Kohn, 1998) in their service delivery.

The second aim of this article was to go beyond comparing setting effects (day hospital vs. community residential) to explore research design, client, and program effects such as those described above (Finney et al., 1996; Magura et al., 2003). Because randomization excludes the potential contribution of client preference on treatment involvement and because clients who self-select their treatment may differ along other important dimensions from those who agree to randomization (Timko, Finney, Moos, & Steinbaum, 1993), we tested whether clients who self-selected into day treatment had better (or worse) outcomes than those randomly assigned to day treatment. Both groups met residential treatment criteria for the first five ASAM dimensions and exceeded the

criteria for day hospital and intensive outpatient treatment. Although authors often note self-selection bias as a study limitation, we found little research testing its effect on outcomes (Juster, Heimberg, & Engelberg, 1995; McKay et al., 1998; Rehm, 2005). Unlike McKay et al. (1995), we were unable to determine whether comparisons between day treatment and community residential treatment yielded similar outcomes under randomized and nonrandomized conditions, because almost all clients who refused (but were eligible for) randomization in our study chose to attend day hospital programs. Additionally, because the managed care residential benefit was designed particularly for high-severity clients, who would ethically be excluded from randomization (because they met the environmental risk criteria for residential treatment), we were interested in gauging how well clients directed to residential treatment would fare relative to randomized community residential clients (who exceeded the criteria for intensive outpatient treatment but only partially met the full criteria for residential treatment).

In summary, we compared treatment effects for clients randomized to day hospital versus community residential programs, clients who self-selected versus were randomized to day hospital, and clients who were directed versus randomized to community residential programs. In addition to comparing abstinence outcomes, we examined formal and informal treatment involvement (days) to test dose effects (during treatment and aftercare) on abstinence, and we explored whether outcomes varied by gender and ethnicity in the comparison groups. We also examined possible attrition bias. Study limitations prohibiting adequate measurement of potentially prognostic effects are discussed.

## Method

### Participants

Participants were 733 men and women seeking treatment from three metropolitan-area chemical dependency (CD) programs that are part of a large prepaid health care plan. A private, nonprofit managed health care organization providing integrated care for CD and other health services administers the health care plan. Clients were 53% White, 23% Black, 18% Hispanic, and 6% other ethnicities; their average age was 41 years (range = 19–77 years). Thirty-six percent were women, and 35% were in a married or partnered relationship (see Table 1). The majority had a drug dependence diagnosis, although proportionately more clients were alcohol dependent (66%) than dependent on any other single substance. Crack (21%) and stimulant (21%) dependence were the next most frequently assigned substance use disorders (followed by marijuana and painkiller dependence). Forty percent were dependent on more than one substance.

Between May 2000 and December 2002, 279 clients were recruited from a single CD program that, in addition to providing a continuum of on-site outpatient services to clients in its own service area, also provided inpatient medical detoxification services for two additional affiliated CD programs included in the study. Clients from CD programs in the other two metropolitan areas were recruited between October 2000 and June 2002 ( $n = 210$ ) and between March 2002 and July 2003 ( $n = 244$ ). These two sites provided a continuum of on-site outpatient services but did not provide inpatient detoxification. Rather, clients who required detoxification services were either treated on an ambulatory basis

Table 1  
Baseline Characteristics for All Study Groups

Characteristic	R-DH ( $n = 154$ )	R-CR ( $n = 139$ )	S-DH ( $n = 321$ )	D-CR ( $n = 82$ )	All ( $n = 733$ )
Gender (% female)	39.0	36.0	33.6	39.5	35.9
Ethnicity (%)					
White	51.9	55.4	55.5	44.4	53.0
Hispanic	15.6	19.4	21.1	27.2	17.8
Black	24.7	22.3	17.1	24.7	23.2
Other	7.8	2.9	6.2	3.7	6.0
Age, $M$ ( $SD$ )	39.9 (11.0)	39.0 (10.7)	41.7 (10.7)	42.9 <sup>a</sup> (9.7)	40.9 (10.7)
Married/partnered (%)	38.3	28.8	37.4	35.8	35.5
Dependence (%)					
Alcohol dependence only	29.9	28.1	39.3	38.3	35.1
Drug dependence only	32.5	36.7	29.6	17.3	30.2
Alcohol and drug dependence	35.1	31.7	25.9	44.4 <sup>a</sup>	30.7
Undiagnosed	2.6	3.6	5.3	0.0	4.0
ASI score, $M$ ( $SD$ )					
Alcohol	0.41 (0.33)	0.40 (0.34)	0.43 (0.33)	0.54 <sup>a</sup> (0.32)	0.44 (0.33)
Drug	0.16 (0.12)	0.15 (0.12)	0.13 <sup>b</sup> (0.12)	0.16 (0.14)	0.15 (0.12)
Medical	0.22 (0.30)	0.27 (0.37)	0.25 (0.34)	0.35 <sup>a</sup> (0.38)	0.26 (0.34)
Psychiatric	0.46 (0.23)	0.46 (0.23)	0.40 <sup>b</sup> (0.24)	0.50 (0.25)	0.44 (0.24)
Family/Social	0.42 (0.29)	0.39 (0.28)	0.30 <sup>b</sup> (0.26)	0.37 (0.28)	0.35 (0.28)
Legal	0.14 (0.20)	0.18 (0.25)	0.07 <sup>b</sup> (0.16)	0.15 (0.22)	0.12 (0.20)
Employment	0.43 (0.27)	0.46 (0.28)	0.35 <sup>b</sup> (0.25)	0.55 <sup>a</sup> (0.28)	0.41 (0.27)
Treatment episodes, $M$ ( $SD$ )	3.0 (3.0)	2.8 (2.6)	2.6 (2.3)	4.6 <sup>a</sup> (5.3)	2.9 (3.1)
Found at 6 months (%)	82.5	77.0	79.8	75.6	79.1
Found at 12 months (%)	76.6	79.9	73.8	58.5	73.5

Note. R-DH = randomized to day hospital; R-CR = randomized to community residential treatment; S-DH = self-selected day hospital; D-CR = directed to community residential treatment; ASI = Addiction Severity Index.

<sup>a</sup>  $p < .05$ , pairwise difference between R-CR and D-CR. No significant differences were found between R-DH and R-CR. <sup>b</sup>  $p < .05$ , pairwise difference between R-DH and S-DH.

(under direct CD program supervision) or treated in health care plan hospital beds before starting day hospital or community residential treatment. Physicians specializing in addiction medicine (detoxification unit) and licensed clinical intake staff at the CD programs determined whether clients were eligible for referral to the study, as either randomized or nonrandomized participants.

Of 3,668 clients assessed during our recruitment period, clinicians excluded 2,117 clients from participation because they did not meet ASAM Level III patient placement criteria (i.e., they needed higher- or lower-level care). Further, 818 clients who met the criteria were excluded because they either (a) refused to participate in the study ( $n = 411$ ) or (b) did not meet research requirements ( $n = 407$ ). This latter group consisted largely of clients ( $n = 205$ ) who were not approached to be in the study because we decided to slow the number of nonrandomized clients recruited during the last months by asking only every 4th client who refused randomization to participate as a nonrandomized participant. Other reasons clients did not meet research require-

ments included refusal to attend one of the study treatment programs ( $n = 142$ ); inability to speak English ( $n = 16$ ); failure to complete the CD program intake process, departure from the detoxification unit against medical advice, or inability to be contacted by research staff ( $n = 31$ ); and miscellaneous reasons ( $n = 13$ ), such as pending legal issues, treatment in the prior 30 days, participation in another CD program study, or involvement in methadone maintenance. A final sample of 733 clients agreed to be in the study (see Figure 1 for a CONSORT flowchart).

This article reports on 696 randomized and nonrandomized clients: Thirty-seven were dropped from this analysis because they were either mandated to day hospital (vs. self-selecting) by an employer or judge ( $n = 28$ ) or self-selected (vs. were directed) to attend residential treatment ( $n = 9$ ). Among clients who agreed to randomization ( $n = 293$ ), 154 were assigned to a day hospital program, and 139 were assigned to a community residential program. Among eligible clients who refused randomization but agreed to be in the study, 321 selected a day hospital program.

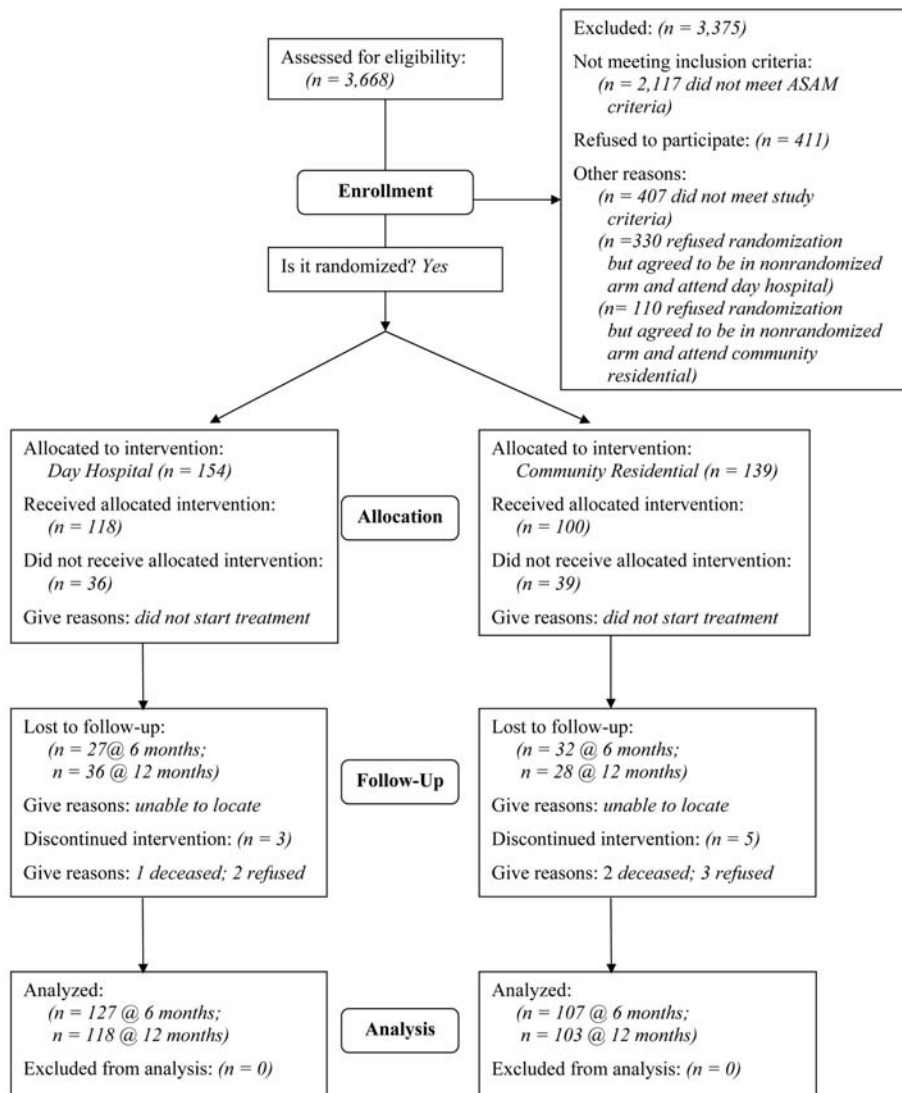


Figure 1. Participant flowchart.

Reasons for refusing random assignment were mainly logistical and included occupation, school, child care, and other family concerns. Among clients who were assessed as ineligible for randomization (but agreed to participate in the study), 82 were directed to attend a community residential program because of reportedly high environmental risk factors that were not supportive of recovery.

### *Design and Procedure*

Physicians and intake clinicians referred clients to the study if they met Level III patient placement criteria for lower intensity residential treatment on at least five of the six assessment dimensions defined by ASAM (1996). Patient placement criteria are specified for 10 types of treatment programs within four broad levels of care: outpatient (Level I), intensive outpatient (Level II), residential (Level III), and inpatient (Level IV). The six dimensions of symptom severity specify criteria for each level of care, with symptoms increasing in severity as the level of care increases in intensity and setting restrictiveness (Rubin & Gastfriend, 2001). In particular, clinicians referred clients who (a) exhibited minimal to no risk for acute withdrawal symptoms (Dimension 1), (b) reported no acute or chronic medical issues (Dimension 2), (c) had no current psychiatric conditions or complications that would distract from recovery (Dimension 3), (d) reported no treatment resistance and had at least minimal recognition of the severity of their problem (Dimension 4), and (e) reported a history suggesting relapse potential at a lower level of care (Dimension 5). Clients who met the criteria for these five dimensions were deemed eligible for randomization. Clients who met the criteria for these five dimensions plus were at high environmental risk of relapse (Dimension 6, Level III) because of social isolation; physical, sexual, or emotional abuse; or endemic substance use in the home environment were referred to the study but were not eligible for randomization. Rather, physicians and intake clinicians directed them to a community residential treatment program.

To retain consistency across sites, physicians and intake clinicians used a decision tree developed by the research team and the CD program medical directors to determine whether clients met Level III patient placement criteria and whether they were eligible for randomization. Usual care procedures were used to assess study eligibility rather than standardized ASAM patient placement criteria decision rules. Although clinical and client perceptions of the substance use problem are somewhat subjective (Deck, Gabriel, Knudsen, & Grams, 2003) and decisions about the appropriate level of care needed can be biased, it was not feasible to institute a protocol to monitor the fidelity or reliability of referral decisions in this health services study.

Trained on-site research assistants recruited clients into the study, obtained signed informed consent, and conducted in-person baseline interviews. All recruitment and interviewing were completed by research assistants blinded to the randomized treatment assignment and not affiliated with the CD programs. We instituted a computerized urn randomization process (Stout, Wirtz, Carbonari, & Del Boca, 1994) at each of the three CD program recruitment sites to balance the sample on gender and ethnicity (White and not of Hispanic origin vs. other). Clients received incentives (gift cards totaling up to \$110) for participation at baseline and at the 6- and 12-month follow-up telephone interviews. We at-

tempted to contact all study participants for follow-up interviews and included those found in our analyses, regardless of whether they initiated the treatment or how many days they attended the index program. The internal review boards of the research department for the managed health care organization and the Public Health Institute approved the study.

### *Treatment Programs*

Interviews with program directors and clinical staff (plus tours and treatment observations) informed our decisions on which programs to include as study sites. In addition to obtaining general information about the programs, we asked program directors to complete the Drug and Alcohol Program Treatment Interview (DAPTI; Moos, Finney, Ouimette, & Suchinsky, 1999). The DAPTI, originally created for a national Veteran's Administration study, assesses the distinctive goals and activities that define cognitive-behavioral, 12-step, and eclectic drug and alcohol treatment programs (Moos et al., 1999). The DAPTI data, which were not scored, provided us with detailed information about the programs' staff (credentials and recovery status) and day-to-day therapeutic emphasis. We also administered and scored the Social Model Philosophy Scale (Kaskutas, Greenfield, Borkman, & Room, 1998) at study sites. This is a 33-item multidimensional scale designed to classify the extent to which a given treatment program follows a social model approach to treatment; a score of 75% is considered a cutoff point for true social model programs (Kaskutas, Keller, & Witbrodt, 1999). All community residential programs scored above the cutoff point. Using information taken from the DAPTI and Social Model Philosophy Scale, we sought programs that matched on therapeutic orientation, staffing requirements, and treatment goals and activities (within each modality). Day treatment and community residential programs provided supervised addiction treatment services 20 hr or more per week (minimal standards are set by ASAM). To monitor treatment fidelity and consistency, we observed treatment groups at all programs and documented our observations throughout the recruitment period. Had a program veered from our original assessment of it, we would have substituted another program for it, as we have done in prior studies (Kaskutas, Witbrodt, & French, 2004). Five day hospital programs and seven community residential programs were enlisted as study programs.

*Day hospital.* The five day hospital programs in this study are representative of mainstream private CD programs (Longabaugh & Morgenstern, 1999) that were developed as an alternative to Minnesota model inpatient treatment (Gerstein & Harwood, 1990). Covered under their health care benefit, clients starting in the 2-week or 14-consecutive-day (3 weeks or 21 consecutive days at one CD program) day hospital programs were encouraged to step down to progressively lower levels of care over the course of a year. Ongoing clinical assessment lent itself to individualized treatment planning.

Treatment consisted of didactic and counseling groups in a mixed gender setting, although the programs offered gender-specific groups during the course of treatment. Groups focused on the biological, psychological, and social aspects of addiction. Clients spent 3 to 4 hr a day in groups at the 2-week CD programs and 5.5 hr a day at the 3-week CD program. As a rule, individual sessions occurred as needed. Clients were expected to attend outside 12-step meetings

while attending treatment. Drug testing was random. The CD programs were staffed by psychiatrists, primary care physicians, degreed therapists, registered nurses, and certified/licensed addiction counselors. Fewer than half the staff were in recovery.

*Community residential.* We chose seven community residential treatment programs typical of those historically developed by members of substance use mutual help programs (Borkman et al., 1998). These programs were under contract with the health care organization to provide residential services to its clients. Most health plan members had coverage for up to 60 days of residential treatment. Two of the seven programs provided mixed-gender services; three were male-only programs, and two were female-only programs. Clinical staff monitored a client's need to stay in a residential program on a weekly basis by making calls to the program to assess a client's progress and need for continued stay. Like day hospital clients, residential clients were also encouraged to step down to progressively lower levels of care (day treatment and/or outpatient groups) at a CD program in the weeks following their stay in a community residential program.

Community residential clients attended didactic and process groups and attended 12-step groups or meetings daily (in house and/or in the community). Didactic sessions focused on working a 12-step program. "Big Book" (Alcoholics Anonymous, 2001) study and related writing assignments were also required. Clients partook in recreation and meditation activities most days. Additionally, they were expected to participate in daily living chores. These activities promoted personal responsibility and provided opportunities to apply recovery skills. Clients were drug tested on suspicion of use and, although less frequently, sometimes randomly. The programs were staffed primarily by nondegreed counselors in recovery (some were longstanding graduates of the program). Most programs had several state-certified alcoholism and drug abuse counselors on staff (and at some, all counselors were certified). Programs often relied on volunteers with longer term sobriety to lead recovery-oriented groups.

### Baseline and Outcome Measures

*Addiction Severity Index (ASI; McLellan, Alterman, Cacciola, Metzger, & O'Brien, 1992).* We administered the ASI at the baseline and 6- and 12-month follow-up interviews. The ASI assesses past-30-day problem severity for seven domains: Alcohol, Drugs, Employment, Legal, Medical, Psychiatric, and Family/Social. A continuous composite score for each domain is created from key items (scored 0–1, with higher scores designating greater severity). Baseline ASI measures, along with various standard demographic measures, were used as control variables in statistical models comparing day hospital and community residential treatment on abstinence outcomes.

*Diagnostic Interview Schedule (DIS; Bucholz, Marion, Shayka, & Marcus, 1996).* We assessed baseline substance dependence on any of 12 substances (including alcohol) using a checklist of questions based on the DIS for Psychoactive Substance Dependence. The DIS, which was designed for use by lay interviewers, uses *Diagnostic and Statistical Manual of Mental Disorders* (4th ed., text rev.; American Psychiatric Association, 2000) criteria. We regrouped the clients' diagnoses into one of three mutually exclusive dependence categories: current alcohol dependence disorder, current drug dependence disorder, or both current alcohol and drug

dependence disorders. A small number of participants answered DIS questions such that a dependence diagnosis could not be determined (even though they met dependence criteria as determined by clinicians). We labeled these cases as undiagnosed.

*Formal and informal treatment.* The health plan provided utilization data from its automated databases. We created an unduplicated count of days each client participated in residential, day hospital, group, or individual treatment in each of three reference periods (baseline to 2 months, 2 to 6 months, and 6 to 12 months). The baseline–2-month period is reported as (a) the total number of days spent in the index treatment program (assigned or designated at baseline) and (b) the total number of days spent in other CD treatment, not part of the index program. Thus, a planned day hospital stay ideally included 2 to 3 weeks of day hospital plus 5 to 6 weeks in outpatient treatment, followed by 10 months in aftercare. Clients who started in a residential program followed a similar treatment-planning process. At the 6- and 12-month follow-ups, we also asked clients to report the number of days they received other formal treatment services not paid for by the health plan and the number of days they attended any 12-step meetings (informal treatment) for the prior 6-month period. Self-reported days of treatment utilization outside the health plan were added to those taken from the health plan administrative databases in our multivariate statistical tests comparing client outcomes.

*Abstinence.* We created our outcome measure, abstinence from alcohol and drugs, using ASI Alcohol and Drug Severity questions collected at 6 and 12 months. We dichotomized the number of days the participant used alcohol or drugs into no use (0 days of use in the past 30 days) versus any use (1 or more days of use). This outcome measure is consistent with the treatment programs' goal of abstinence from all mood-altering substances. We also examined ASI Alcohol and ASI Drug composite scores (continuous measures) at follow-up and replicated our analysis with these two outcome measures (summary results are reported as text only).

### Data Analysis

We made preliminary tests before examining outcomes. First, we examined baseline differences between comparison treatment groups (see Table 1) using analysis of variance (continuous variables) and chi-square tests (categorical variables). Next, we conducted two sets of analyses to examine the effect of attrition on follow-up sample composition. First, baseline characteristics and severity measures were compared within each treatment group for those who were and were not located at the follow-ups. Then, 6-month follow-up measures for clients interviewed at both 6 and 12 months—including ASI severity (seven measures), 12-step days (6 months), index treatment days (8 weeks), and aftercare days (2 to 4 months) combined across the study groups—were compared to those for clients interviewed at 6 months only. We used the Brown–Forsythe statistic to test for the equality of means (Brown & Forsythe, 1974). This statistic is preferable to the *F* statistic because it is robust to groups that are unequal in size and to when the assumption of equal variances is violated. Last, differences in formal and informal treatment utilization were tested between comparison treatment groups with nonparametric Mann–Whitney *U* tests (see Table 2).

Looking at past-30-day abstinence, we first compared unadjusted rates at follow-up points across study groups (Table 3)

using *t* tests to compare groups of interest. We then used longitudinal random effects logit models (Diggle, Liang, & Zeger, 1996) adjusted for important baseline covariates (see the note to Table 4) to estimate the odds of abstinence across time. As only two time points were available for the outcome variable, the random effects were restricted to the intercept (not for the slope or other time-varying covariates). Longitudinal models are of interest here in that one of the key analysis questions was whether odds of abstinence changed over time and whether this change differed between the various treatment groups. Preliminary analyses were undertaken to determine which baseline differences should be controlled in the model; only measures of interest and those that correlated with the outcome were retained in our final model. We created dummy variables for each of our four treatment groups (randomized to day hospital vs. not, randomized to community residential treatment vs. not, selected day hospital vs. not, directed to community residential treatment vs. not). Measures were added to models as sequential blocks, with the first block including ASI severity scores (Alcohol, Drug, Employment, and Legal), gender, ethnicity (non-White vs. White), age, number of prior treatment episodes, recruitment site (outpatient vs. detoxification), treatment group (three dummy variables, comparison group omitted), days in treatment (index period, 0–2 months), time, and Treatment Group (three dummy variables) × Time interaction terms. In a second block, we added days in formal aftercare treatment (2 to 12 months, time varying); in a third block, we added days in 12-step groups (baseline to 12 months; time varying); and in a

fourth block, we added Treatment Group (three dummy variables) × Gender and Treatment Group (three dummy variables) × Ethnicity interaction terms.

Once we had established a beta coefficient for our first comparison treatment group (e.g., randomized community residential treatment vs. day hospital), we then calculated the odds ratios (ORs) and significance values for other comparison groups by testing the appropriate linear contrasts of the model coefficients to test our hypothesized treatment effect. We added recruitment site (outpatient vs. detoxification) to our models to control for regional and CD unit differences; however, we did not add measures to control for potential within-treatment-program effects for the five day hospital and seven community residential programs.

Results

Client Characteristics

No significant differences in demographic characteristics, dependence diagnoses, ASI severity scores, or prior treatment episodes were found between clients randomly assigned to community residential treatment and those randomly assigned to day hospital treatment at baseline (see Table 1). Clients who self-selected day hospital had significantly lower baseline ASI Drug, Psychiatric, Family/Social, Legal, and Employment Severity scores compared to clients randomized to day hospital. A poorer prognosis emerged for clients directed to community residential treatment compared to clients randomized to the same programs.

Table 2  
Days in Formal Treatment and 12-Step Groups

Treatment utilization	R-DH clients ( <i>n</i> = 154)	R-CR clients ( <i>n</i> = 139)	S-DH clients ( <i>n</i> = 321)	D-CR clients ( <i>n</i> = 82)
Index program (baseline–2 months)				
Days, <i>M</i> ( <i>SD</i> )	10.9 (8.6)	22.5 (21.2)**a	10.3 (7.1)	32.1 (23.1)**b
Days excluding no-shows, <sup>c</sup> <i>M</i> ( <i>SD</i> )	14.3 (7.0)	31.3 (18.6)***a	12.2 (5.9)	41.2 (17.5)***b
Attended at least 1 day (%)	76	72	82	78
Other treatment, not index program (baseline–2 months)				
Days, <i>M</i> ( <i>SD</i> )	10.4 (12.7)	8.1 (11.7)	8.7 (10.3)	3.4 (6.5)***b
Attended at least 1 day (%)	73	78	74	49
Any formal treatment (2–6 months) <sup>d</sup>				
Days, <i>M</i> ( <i>SD</i> )	17.7 (32.6)	8.9 (34.9)	15.8 (28.9)	28.6 (42.0)*b
Attended at least 1 day (%)	59	52	53	66
Any formal treatment (6–12 months)				
Days, <i>M</i> ( <i>SD</i> )	11.2 (25.2)	8.8 (22.3)	11.6 (29.9)	6.7 (17.4)
Attended at least 1 day (%)	44	36	38	32
12-step attendance (baseline–6 months) <sup>d</sup>				
Days, <i>M</i> ( <i>SD</i> )	80.0 (61.5)	81.7 (62.8)	60.0 (56.3)**e	81.3 (61.5)
Attended at least 1 day (%)	91	91	86	88
12-step attendance (6–12 months)				
Days, <i>M</i> ( <i>SD</i> )	50.3 (56.6)	45.3 (50.9)	44.1 (53.8)	47.9 (56.1)
Attended at least 1 day (%)	78	74	67	72
Total days (baseline–12 months)				
Formal treatment, <sup>f</sup> <i>M</i> ( <i>SD</i> )	50.2 (51.2)	58.3 (52.2)	46.2 (54.1)	70.7 (63.0)
12-step groups, <sup>f</sup> <i>M</i> ( <i>SD</i> )	115.3 (100.5)	108.9 (99.1)	89.5 (90.7)*e	105.9 (92.4)

Note. R-DH = randomized to day hospital; R-CR = randomized to community residential treatment; S-DH = self-selected day hospital; D-CR = directed to community residential treatment.

<sup>a</sup> Significant difference between R-DH and R-CR. <sup>b</sup> Significant difference between R-CR and D-CR. <sup>c</sup> Mean days when clients did not initiate the index treatment are excluded. <sup>d</sup> The 12-step days and index treatment days overlap in baseline–6-month period. <sup>e</sup> Significant difference between R-DH and S-DH. <sup>f</sup> Total 12-step days do not equal the sum of combined follow-ups because of missing cases at follow-ups.  
\* *p* < .05. \*\* *p* < .01. \*\*\* *p* < .001.

Table 3  
Past-30-Day Abstinence Rates

Abstinence	R-DH		R-CR		S-DH		D-CR		Total	
	%	<i>n</i>	%	<i>n</i>	%	<i>n</i>	%	<i>n</i>	%	<i>n</i>
Abstinent at 6 months	65.9	126 <sup>a</sup>	68.9	106 <sup>a</sup>	64.7	255 <sup>a</sup>	60.0	60 <sup>a</sup>	65.3	547 <sup>a</sup>
Adjusted rates <sup>b</sup>	53.9	154	52.5	139	51.4	321	43.9	82	51.3	696
Abstinent at 12 months	62.4	117 <sup>a</sup>	63.1	111 <sup>a</sup>	59.8	234 <sup>a</sup>	64.6	48 <sup>a</sup>	61.6	510 <sup>a</sup>
Adjusted rates <sup>b</sup>	47.4	154	50.4	139	43.6	321	37.8	82	45.1	696
Abstinent at 6 and 12 months	56.6	106 <sup>c</sup>	60.2	93 <sup>c</sup>	51.4	208 <sup>c</sup>	52.4	42 <sup>c</sup>	54.6	449 <sup>c</sup>

Note. R-DH = randomized to day hospital; R-CR = randomized to community residential treatment; S-DH = self-selected day hospital; D-CR = directed to community residential treatment.

<sup>a</sup> Number of clients interviewed at that follow-up. <sup>b</sup> Adjusted rates reflect the percentage of participants who were abstinent when missing cases (clients lost to follow-up) were reassigned as nonabstinent; thus, sample sizes reflect rates for all clients in each treatment group at baseline. <sup>c</sup> Number of clients who provided data at both follow-ups.

Directed clients reported greater medical and employment severity, were more likely to be dependent on both alcohol and drugs, and had more lifetime treatment episodes than clients randomized to community residential programs.

*Attrition Bias*

Follow-up rates did not differ significantly for day hospital and community residential treatment clients in the randomized sample (83% and 77% at 6 months, respectively, and 77% and 80% at 12 months, respectively; see Table 1). This pattern held in the self-selecting and directed groups, except at the 12-month follow-up

for clients directed to community residential treatment (59% response rate).

When we compared baseline characteristics, we found that randomized community residential treatment clients lost to follow-up were not different from those found at 6 or 12 months; however, randomized day hospital clients lost to follow-up at 6 months had significantly higher baseline ASI Employment scores ( $M = .5287, SD = .2911$ , and  $M = .4148, SD = .2584; p = .044$ ), and those lost to follow-up at 12 months had significantly higher baseline ASI Drug scores ( $M = .2033, SD = .1148$ , and  $M = .1448, SD = .1187; p = .010$ ) than clients who were found for

Table 4  
Treatment Group Effects on Abstinence

Variable	Coef.	OR	CI
ASI			
Alcohol Severity	-0.82	0.44	0.17, 1.15
Drug Severity	-3.44*	0.03	0.002, 0.45
Employment Severity	-0.96	0.38	0.13, 1.16
Legal Severity	0.87	2.39	0.51, 11.19
Gender (female vs. male)	-0.02	0.98	0.54, 1.77
Ethnicity (non-White vs. White)	0.15	1.16	0.64, 2.12
Age	0.03*	1.03	1.00, 1.57
Lifetime treatment episodes	-0.09*	0.91	0.84, 0.99
Outpt. Recruitment Site B (vs. detox recruitment site)	-0.46	0.63	0.30, 1.32
Outpt. Recruitment Site C (vs. detox recruitment site)	-0.57	0.57	0.26, 1.23
R-CR (vs. R-DH)	-0.45	0.63	0.22, 1.86
S-DH (vs. R-DH)	-0.18	0.83	0.34, 2.02
D-CR (vs. R-DH)	-1.46	0.23	0.06, 0.88
Time (R-DH, reference group)	-0.02	0.98	0.86, 1.11
(R-CR vs. R-DH) × Time	0.07	1.07	0.88, 1.30
(S-DH vs. R-DH) × Time	-0.01	0.99	0.85, 1.17
(D-CR vs. R-DH) × Time	0.18	1.20	0.93, 1.56
(D-CR vs. R-CR) × Time <sup>a</sup>	0.11	1.12	0.86, 1.45
Treatment days, index period	0.04***	1.04	1.02, 1.07
12-step meeting days (across time)	0.02***	1.02	1.01, 1.03
Constant	-0.24		

Note. *n* = 615. Cases with missing data on one or more covariates (*n* = 81) were dropped from the model. All measures included in the final longitudinal model are displayed above. Coef. = beta coefficient (slope); OR = odds ratio; CI = 95% confidence interval; ASI = Addiction Severity Index; Outpt. = outpatient; R-DH = randomized to day hospital; R-CR = randomized to community residential treatment; S-DH = self-selected day hospital; D-CR = directed to community residential treatment.

<sup>a</sup> Values when the reference group is R-CR.

\* *p* < .05. \*\*\* *p* < .001.



these respective interviews. No baseline differences were found for those clients lost to follow-up who self-selected day hospital and those directed to community residential programs. Average days in treatment (baseline to 2 months) did not differ for missed and followed clients within any group at either follow-up interview.

Examining potential attrition bias further, we conducted analyses that compared all clients interviewed at both 6 and 12 months to clients interviewed at 6 months only. Our logic was that if both follow-up groups looked similar on 6-month ASI severity measures, we could be somewhat more confident that our 12-month outcomes were not inflated. As sample sizes were not large enough to make comparisons within treatment groups separately, analyses were performed across the combined treatment groups. We found significant differences in scores for both ASI Alcohol (Brown-Forsythe  $F(1, 107) = 7.4, p = .008$ ) and ASI Drug (Brown-Forsythe  $F(1, 105) = 7.4, p = .014$ ). ASI Alcohol and Drug Severity scores were higher in the group interviewed at 6 months only compared to those for clients interviewed at both 6 and 12 months. For the 6-month-only group, the average ASI Alcohol Severity score was .1831 ( $SD = .2292$ ) and the average ASI Drug Severity score .0561 ( $SD = .0908$ ). For the 6- and 12-month group, the average ASI Alcohol Severity score was .1119 ( $SD = .1811$ ) and the average ASI Drug Severity score .0309 ( $SD = .0632$ ). No other measures were significant.

#### *Days of Formal and Informal Treatment Involvement*

Table 2 shows the average number of days clients attended any type of formal or informal treatment (12-step meetings). For days in the index program, we show data first including all study clients and second excluding no-shows. In addition, we show the percentage of clients who attended at least 1 day in the index program.

*Formal treatment.* The average length of stay among clients who initiated the index treatment program was 14.3 days ( $SD = 7.0$ ) for randomized day hospital clients and 31.3 days ( $SD = 18.6$ ) for randomized community residential clients (see Table 2). Similar to randomized day hospital clients, those who self-selected day hospital stayed in the index treatment 12.2 days ( $SD = 5.9$ ) on average; however, directed clients stayed in community residential programs significantly longer (41.2 days;  $p = .001, SD = 17.5$ ), on average, than their randomized counterparts. About a quarter of the randomized clients (both treatment modalities) and slightly fewer of the nonrandomized (self-selected and directed) clients never initiated their index treatment program. Utilization of services other than at the index program (at 2 months) was similar across groups (8.1 to 10.4 days) for all but directed clients (3.4 days), who would be less likely to seek other CD treatment services given their longer index stay in the community residential program. Just over half the day hospital clients (randomized, 54.7%; self-selected, 52.2%) stayed 12 days or more in the index treatment program; among residential clients, 43.2% of the randomized and 62.2% of the directed clients stayed 30 days or more at the index program.

Treatment utilization following the index period (2–6 months and 6–12 months) dropped substantially across time for all four groups (ranging from 15.8 to 28.6 days at 2–6 months and from 6.7 to 11.6 days at 6–12 months when nonattendees for that period are included). Over half (53%–66%) of the clients received treatment services in the 2–6-month period, and fewer (32%–44%)

received services in the 6–12-month period. Only one significant difference emerged. Community residential clients who were directed to treatment attended more days of treatment than the randomized residential clients did from 2 to 6 months ( $M = 18.9$  days,  $SD = 34.9$ , and  $M = 28.6$  days,  $SD = 42.0$ , respectively;  $p = .035$ ). Because these utilization data assess treatment duration but not treatment intensity, we compared the types of aftercare services (i.e., individual, group, day, and residential) clients sought; no single service type was sought more than another in the between-groups comparisons. Fewer than 3% of all study clients reported entering a sober living or transitional home after their index treatment.

*Informal treatment.* Clients attended more days in 12-step meetings than days in formal treatment over time (see Table 2). Most clients (86%–91%) attended at least one meeting between baseline and 6 months. Randomized day hospital clients attended significantly more meeting days than self-selecting clients ( $M = 80.0$  days,  $SD = 61.5$ , and  $M = 60.0$  days,  $SD = 56.3$ , respectively;  $p = .010$ ). Attendance dropped to about the same level for all groups from 6 to 12 months (about seven to eight meetings a month), as did the number of clients who attended at least one meeting (67%–78%).

#### *Unadjusted Abstinence Rates Among Clients Interviewed at Follow-Ups*

Table 3 displays unadjusted past-30-day abstinence rates for the treatment groups at 6 and 12 months and at both follow-ups. Despite relatively high severity at intake (see Table 1), about two thirds of the randomized day hospital (65.9%) and community residential treatment (68.9%) clients reported abstinence at 6 months. Self-selected and directed groups reported similar rates. Rates dropped slightly at 12 months for all but the directed clients (differences not significant). Just over half the followed clients in all groups reported abstinence at both follow-ups.

#### *Testing Longitudinal Treatment Effects on Abstinence*

Results from our final longitudinal random-effects logit model are shown in Table 4. ASI Drug Severity ( $OR = 0.03, p = .013$ ), older age ( $OR = 1.03, p = .045$ ), and fewer prior treatment episodes ( $OR = 0.91, p = .032$ ) were the only baseline measures associated with abstinence. No main effects were found for gender or ethnicity. None of the Treatment Group  $\times$  Time interactions was significant for our between-groups comparisons—that is, between the randomized day hospital and community residential clients, between the self-selected and randomized day hospital clients, and between directed and randomized community residential treatment clients. In summary, abstinence was not associated with where one sought treatment or one's assignment (randomized or not). By comparing beta coefficients (see Table 4), one can readily see that slopes were not significantly different among the treatment groups.

Two predictors emerged as significant for formal and informal treatment involvement: days of treatment in the index period (significant in each block entry), and the time-varying covariate assessing 12-step (AA and NA) meeting days. Both were associated positively with abstinence. The time-varying covariate assessing formal aftercare treatment (which was significant in the block

without 12-step days added) was excluded from the model shown in Table 4 because it correlated with 12-step days at follow-ups ( $r > .40, p < .001$ ). Gender  $\times$  Treatment Group and Ethnicity  $\times$  Treatment (as well as Gender  $\times$  Treatment Group  $\times$  Time and Ethnicity  $\times$  Treatment  $\times$  Time) interaction terms were not significant, indicating that there was no synergistic effect of gender or ethnicity on program type (by assignment type), and are therefore omitted from the results shown in Table 4. Because days of treatment and days in 12-step meetings related positively to our abstinence outcome, we also ran our final model (using Table 4 measures) without these measures. Results did not change for any study group measures. Additionally, we ran two longitudinal models (similar to that shown in Table 4) using ASI Alcohol Severity and ASI Drug Severity as separate outcome measures. Results were similar to those predicting abstinence.

### *Supplementary Analyses*

Follow-up rates were stable across our study groups (75%–83%), except for directed residential clients at 12 months (59%). Since it is possible that we were unable to locate mostly clients who were drinking and/or using drugs, we conducted a simple (but extreme) post hoc sensitivity analysis. We reassigned all missing cases as nonabstinent and reconducted analyses. Table 3 displays these adjusted rates. Resulting reductions in abstinence rates were therefore proportionate to the respective response rates, with the effect most pronounced for the directed residential clients. Although none of the tests of proportions resulting from inclusion of lost clients as nonabstinent was significant, the magnitude of the difference in the recalculated 12-month abstinence rates for the randomized versus directed residential clients was considerable (50.4% vs. 37.8%) and approached significance (Mann–Whitney,  $Z = -1.235, p = .071$ ).

Our analyses could not control for inherent program differences (e.g., counselor effects, client population) in the five day hospital and seven community residential settings because sample sizes were too small to do so. Rather, the effect of treatment modality (day hospital vs. community residential treatment) was included in our longitudinal modeling procedure as a baseline control, as well as modifying the temporal trajectory of the outcome. Concerned that observations might be correlated within treatment group and not correlated between treatment modalities, we created a model to estimate and examine the residuals. The estimated model indicated no heteroscedasticity in the variances of the residuals between community residential and day hospital treatment and no correlation of residuals within treatment modality. Given that we also included a treatment modality indicator variable to assess main effects between groups, we feel that the assumptions of the model were met and the influence of treatment modality sufficiently incorporated.

Last, because prior research has suggested that the most severely medically and psychiatrically compromised patients may benefit from more intense treatment (Belenko, Patapis, & French, 2005; Miller & Hester, 1986), we ran regression analyses to test whether residential treatment was differentially beneficial for more severely impaired clients in the randomized group. Interaction terms (Treatment Modality  $\times$  Severity) were not significant in logistic regression analyses modeling 6- and 12-month abstinence or in longitudinal panel analyses modeling abstinence across time for

any of the baseline ASI severity scores tested (i.e., Alcohol, Drug, Medical, Family/Social, and Psychiatric).

### Discussion

The primary findings of this study indicate that similar outcomes were obtained for day hospital and community residential treatment clients. In all study groups, past-30-day abstinence rates exceeded 50% at follow-ups (among those interviewed). In addition, length of stay in the index treatment program and 12-step attendance were highly associated with abstinence. However, greater immersion in 12-step ideals at community residential programs (compared to day hospital programs) did not result in greater AA or NA attendance over the course of 1 year. Patient, study design, and program factors that might explain those results offered no substantial evidence to suggest that our findings were otherwise confounded. Several study limitations need to be considered before we can discuss our findings more fully.

### *Limitations*

First, the randomized arm of this study included clients who met only five of the six ASAM criteria for placement in a residential program; thus, our results do not speak to the efficacy of residential versus day hospital treatment among participants who are fully “matched” to residential treatment. Further, all of the randomized clients in this study exceeded the criteria for intensive outpatient or day hospital treatment and thus were a “mismatch” to day hospital as well. However, this limitation should be considered in light of the larger matching literature, which has been based on naturalistic rather than randomized trials (Magura et al., 2003). It also is possible that the placement criteria used to assess study and randomization eligibility were imperfectly applied by the program staff. This limitation is countered somewhat by the strength of external validity lent to the study by its implementation in real world treatment settings.

Another limitation of this health services study is treatment intensity. Not all study programs offered identical doses of structured treatment hours. This issue is consistent with prior research, which found major within-level variation by hours per day and by number and type of skilled treatment services across 12 treatment units (Levine, Turner, Reif, Janas, & Gastfriend, 2003). One of our day hospital programs was designed to last 3 weeks (with somewhat longer days), while the others lasted 2 weeks. Moreover, while all programs provided requisite services to meet ASAM qualifications, we proposed a priori that informal interactions with recovering peers and alumni might be as beneficial for community residential clients as time spent in structured groups. Last, in our longitudinal model testing the effect of days in treatment on abstinence, we used a count of total days in any treatment, such that 30 days in a residential program was not distinguished from 30 days in any other combination of treatments.

We did not verify self-reported substance use at follow-up with collaterals or biological tests. However, other studies have found high concordance between self-report and biological or collateral measures (Babor, Steinberg, Anton, & Del Boca, 2000), and comparable studies in which self-report was validated have reported similar rates of abstinence (McLellan, Grissom, Alterman, Brill, & O’Brien, 1993; Weisner et al., 2000). In our randomized sample,

we assume that any potential reporting error was distributed equally in the two treatment modalities; thus, any bias would result in a conservative finding.

One should also consider other factors when interpreting the findings—namely, 26% of the sample refused to be in the study or failed to keep their enrollment appointment, and, among those who consented to be in the study, about a quarter never initiated their index treatment. Moreover, attrition analyses showed that clients interviewed at both 6 and 12 months displayed better 6-month ASI Alcohol and Drug Severity scores than clients interviewed at 6 months only; thus, the 12-month abstinence outcomes may be somewhat inflated. Unfortunately, sample sizes were not large enough to test whether this attrition effect differed by treatment group and thus whether those directed to community residential programs, where 12-month follow-up rates were the lowest, suffered the most from nonignorable attrition bias (Little & Rubin, 2002). Further, some of our analyses included smaller samples and thus limited our ability to detect effects. However, a definitive trial of inpatient–outpatient treatment, which found results similar to ours, had even fewer enrolled participants (McKay et al., 1995, 1998).

Last, residential programs in this study were chosen because of their social model orientation, which carries a very strong emphasis on environmental influences on relapse. Other residential programs, such as therapeutic communities (De Leon, 2000), may be more beneficial for some groups of clients. Further, we do not know whether our residential findings apply to a more indigent population. The generalizability of our findings may be limited to an insured population. Our multisite design, however, allows us to generalize our findings to a more heterogeneous population.

### Research Findings

Our main hypothesis, that randomized day community residential treatment clients would have higher abstinence rates compared to day hospital clients, was not supported in time-specific bivariate or multivariate longitudinal analyses. This hypothesis was tested on a randomized sample that proved to be balanced on a comprehensive set of baseline measures, with response rates comparable to those of studies conducted with similar managed care samples (Weisner et al., 2000; Weisner, Mertens, Parthsarathy, & Moore, 2001). Further, response rates in the randomized groups were not statistically different between the two treatment modalities at any follow-up, which minimized bias due to imbalance. Comparable findings emerged in gender and ethnicity analyses. Other than the veterans study conducted with men, this is the only randomized clinical trial we have found that reports on differences between day hospital and community residential programs among diverse treatment-seeking groups.

Null results were also obtained in our analyses with the non-randomized groups, but the low response rate for the directed community residential clients at 12 months tempers the conclusions that can be drawn with this sample at 12 months. Although no baseline differences were found between those lost versus followed, it is probable that clients not interviewed at 12 months were those with the poorest outcomes. Here, we are guided by our sensitivity analysis, in which participants who were missing to follow-up were recoded as nonabstinent. While the difference in 12-month abstinence rates between the randomized and directed

residential clients did not achieve statistical significance, it was of large magnitude and in the direction favoring the randomized residential clients over the directed residential clients.

Although all study clients were screened according to the same criteria, distinct baseline severity differences emerged across our groups. Directed clients (who met all six ASAM criteria for residential treatment) displayed greater medical and employment ASI severity than those randomized to residential treatment (who met only five criteria), whereas clients who self-selected day treatment presented with less severe scores than the randomized day treatment clients on five of the seven ASI domains. Despite these differences, abstinence outcomes were similar when we compared clients who self-selected versus were randomized to day hospital treatment and when we compared the directed versus randomized residential clients (with the above caution about the 12-month results in the directed group). These findings emerged in a context in which no significant differences in outcomes were obtained between residential and day hospital clients in the randomized arm. Another perspective is that the criteria used to assess client need did an imperfect job of capturing the underlying need that the similar outcomes belie. Toward this end, we are encouraged that research has continued to be focused on testing the reliability and utility of criteria that will place clients in the most efficacious level of care (Gastfriend et al., 2003).

### Implications

It is important to highlight our finding that length of stay in formal treatment was significant only for the index treatment episode; treatment beyond that initial 2-month window was not. In contrast, 12-step meeting attendance continued to be important for the full 12 months. These results spotlight important areas of emphasis for treatment providers: Keep clients engaged in treatment during the first few months, and heavily promote and facilitate 12-step meeting attendance for at least a year. Further, although attending formal aftercare treatment may indeed be important, our data suggest that this may be trumped by 12-step meeting attendance.

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Received June 19, 2006

Revision received September 18, 2007

Accepted September 18, 2007 ■

### New Editors Appointed, 2009–2014

The Publications and Communications Board of the American Psychological Association announces the appointment of six new editors for 6-year terms beginning in 2009. As of January 1, 2008, manuscripts should be directed as follows:

- *Journal of Applied Psychology* (<http://www.apa.org/journals/apl>), **Steve W. J. Kozlowski, PhD**, Department of Psychology, Michigan State University, East Lansing, MI 48824.
- *Journal of Educational Psychology* (<http://www.apa.org/journals/edu>), **Arthur C. Graesser, PhD**, Department of Psychology, University of Memphis, 202 Psychology Building, Memphis, TN 38152.
- *Journal of Personality and Social Psychology: Interpersonal Relations and Group Processes* (<http://www.apa.org/journals/psp>), **Jeffrey A. Simpson, PhD**, Department of Psychology, University of Minnesota, 75 East River Road, N394 Elliott Hall, Minneapolis, MN 55455.
- *Psychology of Addictive Behaviors* (<http://www.apa.org/journals/adb>), **Stephen A. Maisto, PhD**, Department of Psychology, Syracuse University, Syracuse, NY 13244.
- *Behavioral Neuroscience* (<http://www.apa.org/journals/bne>), **Mark S. Blumberg, PhD**, Department of Psychology, University of Iowa, E11 Seashore Hall, Iowa City, IA 52242.
- *Psychological Bulletin* (<http://www.apa.org/journals/bul>), **Stephen P. Hinshaw, PhD**, Department of Psychology, University of California, Tolman Hall #1650, Berkeley, CA 94720. (Manuscripts will not be directed to Dr. Hinshaw until July 1, 2008, as Harris Cooper will continue as editor until June 30, 2008.)

**Electronic manuscript submission:** As of January 1, 2008, manuscripts should be submitted electronically via the journal's Manuscript Submission Portal (see the website listed above with each journal title).

Manuscript submission patterns make the precise date of completion of the 2008 volumes uncertain. Current editors, Sheldon Zedeck, PhD, Karen R. Harris, EdD, John F. Dovidio, PhD, Howard J. Shaffer, PhD, and John F. Disterhoft, PhD, will receive and consider manuscripts through December 31, 2007. Harris Cooper, PhD, will continue to receive manuscripts until June 30, 2008. Should 2008 volumes be completed before that date, manuscripts will be redirected to the new editors for consideration in 2009 volumes.