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Therapy preference and treatment outcome in clients with mild to moderate alcohol dependence

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Abstract

The Brief Treatment Programme for Alcohol Dependence allocated 122 clients randomly to three different forms of brief therapy. Prior to allocation clients were asked what their preference would have been had allocation not been random. This study posed the question: did clients receiving their preferred treatment have a better outcome than those who did not? Also examined were differences in the treatment process variables of perceived effectiveness, satisfaction, rapport, engagement and number of sessions attended. The results were that there was no difference in either outcome or treatment process according to whether or not clients were allocated to their treatment of preference. It is concluded that these findings reinforce both the ethicality of the randomized controlled trial as a methodology for examining differential treatment outcomes in individual brief treatment of between one and five sessions for alcohol dependence and the validity of these findings as they might relate to real clinical settings. Finally, it is suggested that other researchers consider the inclusion of questions related to client preference. [Adamson SJ, Sellman JD, Dore GM. Therapy preference and treatment outcome in clients with mild to moderate alcohol dependence. Drug Alcohol Rev 2005;24:209 – 216]

Introduction

The role of client choice in psychotherapeutic practice has been identified by Van Audenhove and Vertommen as a highly relevant, but neglected, area for therapists [1]. These authors describe a common clinical process whereby following assessment the client is advised of the appropriate form of therapy. This is seen as undermining the client's sense of responsibility with regard to the therapeutic process and may lead to dissatisfaction, reduced compliance and dropout. Van Audenhove and Vertommen propose that treatment choice may be systematically incorporated into the therapeutic process in the belief that this will enhance outcome.

In contrast to treatment choice, treatment preference has been identified in randomized controlled trials as the choice of treatment a client would have made had they been asked. Torgerson and Sibbald [2] have identified the potential negative impact of

'resentful demoralization' on treatment outcome when treatment allocation does not match treatment preference.

The area of clinical predictors of treatment outcome for alcohol dependence is well studied, with several variables showing reasonable consistency across studies in their ability to predict treatment outcome, including alcohol related expectancies [3,4], self-efficacy [5 – 7], dependence severity [6,8] and motivation [6,9,10]. In contrast, attempts to identify variables to enable the matching of particular clients to particular treatments have been much less successful. This was most clearly demonstrated by the findings of Project MATCH [11], a study of over 1700 alcohol-dependent clients randomized to either Cognitive Behaviour Therapy, Twelve-Step Facilitation Therapy or Motivational Enhancement Therapy. A wide range of proposed matching variables were tested, with few matching effects found. Project MATCH did not investigate client preference as a matching variable, however.

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Empirical literature examining the role of client choice, or preference, in treatment outcome is sparse. Ojehagen and Berglund [12] examined client choice with two consecutive samples from an out-patient alcoholism treatment programme. In the first sample, recruited over a 2-year period, 82 clients were informed about the 2-year 'psychiatric treatment programme', of whom 58 decided to participate. In the second study 72 patients recruited over the next 2 years were assigned randomly to either the psychiatric treatment programme, as for the first sample, or to multimodal behavioural therapy, and to either 1 or 2 years' duration of either, with 30 treatment sessions in all four alternatives. The authors report that the number of treatment acceptors was lower in the second sample, which also suffered a higher rate of treatment non-completion. Both differences were statistically significant. Drinking outcome did not differ significantly between treatment completers in the first sample and psychiatric treatment programme completers in the second sample. Interpretation of these results is hampered by the non-equivalent design employed in that treatment was not contemporaneous, with the treatment for the second sample also being delivered over two different durations, and the fact that the choice faced by clients in the first sample (psychiatric treatment or no treatment) was not equivalent to the choice faced by the second sample (psychiatric treatment or multimodal behavioural therapy). The failure to include non-completers in the analysis of treatment outcome further undermines the clinical applicability of findings.

Sterling *et al.* [13] found almost no difference in treatment retention or outcome in a sample of 127 treatment-seeking cocaine users, just over half of whom were able to choose between individual and group therapy delivered over a 12-week period. The two options were described to participants, but no detail is provided as to the nature of this information. Again treatment was not contemporaneous; rather, the choice group followed immediately on from the randomized group. The shorter duration of treatment (12 weeks) when compared to Ojehagen and Berglund's study may render this less crucial, however. At 9 months' follow-up Sterling *et al.* found no difference in retention, AIDS-related risk behaviour, further addiction treatment, jail, job or school status or scores on the seven problem scales (medical, employment, alcohol, drug, legal, family and psychological) of the Addiction Severity Index [14]. The only difference that did emerge was that those in the choice condition reported fewer days of cocaine use in the last 30 days.

A heroin-using client group was randomized to 'choice' versus 'force' conditions in a study conducted by Kludt and Perlmuter [15]. Clients in the choice group determined the format of counselling (group versus individual) and, to a limited extent, the timing of drug testing. The authors do not provide any details of the options provided to clients. The design was somewhat confounded, however, by utilizing self-monitoring that differed in content across conditions, with the 'choice' group monitoring a range of behaviours each day while subjects in the 'force' group monitored only one behaviour (eating). No significant differences were found in attendance levels, urine results or post-treatment anxiety or depressive symptoms. The study was hampered by small numbers, with only 18 of 51 clients completing the 12 weeks of treatment.

There is a literature on the effects of mandated treatment on treatment process, outcome, and in particular retention. Such research suggests that mandated treatment may lead to improved retention when compared to treatment undertaken voluntarily [16,17], although findings have been inconclusive [18]. Wells-Parker [19] has cautioned, however, that choice to enter treatment should not be thought of in a simple dichotomous fashion as clients not under a court order may be subject to varying levels of coercion by family, friends, employers or other involved agencies. While informative with respect to the importance of choice in entering and completing treatment, it is not clear to what extent this literature sheds light on the role of choice of treatment type within a therapeutic venue.

Only one study was identified which examined treatment preference in a randomized controlled trial rather than attempting to contrast treatment choice and random or mandated assignment. Brown *et al.* [20] reported on the random assignment of 241 clients attending residential treatment for psychoactive substance abuse/dependence to one of two aftercare programmes. The two programmes were relapse prevention and Twelve-Step facilitation, with both consisting of 10 weekly individual sessions. Of the 154 clients successfully followed-up 107 had expressed an aftercare preference, but the authors do not identify when this preference was recorded or how it was elicited. Clients whose aftercare group allocation was consistent with their preference had lower drug use scores and fewer days of substance use at 6 months' follow-up when compared to clients whose preference was not consistent with allocation. There was no difference between groups in time to relapse. The impact of preference – allocation matching on treatment completion is not reported. The lack of detail on how and when treatment preference was elicited is a limitation of this paper, as significant biases may be supposed to have been introduced if preference was elicited after randomization/during aftercare delivery, or if preference was only recorded for those clients

spontaneously expressing a treatment preference. Additionally the authors provide insufficient detail of the preceding residential treatment to clarify whether or not one aftercare option might have been considered more compatible with this prior treatment.

It is in the context of this small number of studies, which suffer a range of methodological flaws, that the current investigation into treatment preference was conducted. The question posed was: does allocation to treatment of preference improve treatment process or outcome for voluntary clients randomized to outpatient treatment for mild to moderate alcohol dependence?

Methods

Participants

A total of 124 clients with a primary diagnosis of mild to moderate alcohol dependence were recruited and initiated treatment in the Brief Treatment Programme for Alcohol Dependence [21,22]. This study was a randomized controlled trial designed to test the efficacy of motivational enhancement therapy (MET), with clients randomized to one of three treatment modalities at the conclusion of a post-assessment feedback and education session. Two of these treatment modalities involved further therapy for which participating therapists had received extensive training with ongoing supervision. All sessions were audiotaped, with a random sample subjected to blind auditing of treatment fidelity. The 6-month follow-up interview was conducted by a research assistant blind to treatment allocation.

The study was conducted at the regional out-patient community alcohol and drug assessment and treatment service. For the purpose of exclusion from this study, 'severe alcohol dependence' was defined as a history of alcoholic withdrawal syndrome as defined by the *Diagnostic and Statistical Manual* version IV (DSM-IV) [23], with symptoms lasting more than 24 hours or current liver damage as indicated by raised GGT, ALT or AST levels. Additionally, those with any other significant health problem that would preclude the offering of controlled drinking as a drinking goal were also excluded.

The mean age of clients was 35.7 years (range 15–59 years). Males made up 57.4% of the sample and 13.9% were of Maori descent, with the remainder of European descent. The sample had a mean of 11.7 years education and a mean onset of alcohol dependence of 27.0 years. A current mood or anxiety disorder was identified in 17.2% of subjects, while the current rate of cannabis abuse/dependence was 13.9% and for conduct disorder/antisocial personality disorder 7.3%. In total 36.1% of the sample suffered from a current disorder comorbid with alcohol dependence. Over the 6 months preceding recruitment subjects drank a mean of 50.4 standard drinks per week (47.8 standard drinks per week for women and 52.4 standard drinks per week for men).

Procedure

Once recruited, subjects completed a comprehensive assessment, which included the Diagnostic Interview for Genetic Studies (DIGS) [24] and a 6-month alcohol use interview utilizing a timeline follow-back procedure [25]. The DIGS was administered by clinical psychologists and psychiatric registrars with DSM-IV diagnoses being made based on all available information, including these interviews, in a consensus diagnosis meeting headed by a consultant psychiatrist (author DS). Subjects also nominated a 'significant other' who completed a portion of the baseline questionnaires as a validity check.

Following assessment subjects and their nominated significant other were invited to attend a feedback session. At this follow-up appointment subjects were given a pamphlet educating them as to the national responsible drinking guidelines [26], and were also given written educational material. A summary of assessment results was provided, describing dependence criteria met, other diagnoses and 6-month drinking profile. Subjects were then advised to significantly reduce their drinking either by cutting down to within the responsible limits or by stopping altogether. These two options were presented as equally appropriate for all subjects.

Finally, subjects were randomized to one of three treatment options. These were four sessions of MET, four sessions of non-directive reflective listening (NDRL) and a no further counselling option (assessment, feedback session and 6-week review only). MET in this study was very similar to that used in Project MATCH [27] guided by five key principles (expressing empathy, deploying discrepancy, avoiding argumentation, rolling with resistance and supporting self-efficacy) [28], with minor modifications, described elsewhere [21].

NDRL was a cut-down version of Rogerian counselling designed to be as dissimilar to MET as possible for the purpose of controlling for time spent in a therapeutic venue. To enhance face-validity NDRL was referred to as person-centred therapy throughout the study.

Immediately prior to randomization subjects were asked to indicate what their treatment preferences would have been had they been given the choice. This was achieved by asking two questions. First, subjects were asked whether or not they would prefer to have four counselling sessions and secondly, for those who

indicated a preference for counselling or did not have a preference, whether or not they would prefer four sessions of MET or four sessions of NDRL. Prior to eliciting preferences MET was described as 'more directed by the therapist and focused on the person's drinking' while NDRL was described as 'less directed by the therapist and focused on the whole life of a person'. No further information was provided unless requested by the subject, in which case the clinician provided clarification in a manner which supported the efficacy of all three treatments.

The treatment phase was concluded with a 6-week review. Subjects were re-contacted at 6 months for the collection of outcome data by a research assistant who was blind to the treatment conditions.

Outcome measurement

Treatment outcome was measured with two key variables, one focused on drinking and the other on global functioning. Unequivocally unsafe drinking was defined as consuming ten or more standard drinks on six or more occasions over the 6-month period with this obtained by self-report utilizing the timeline follow-back procedure and confirmed by interviewing a nominated significant other. This definition of unequivocal heavy drinking was chosen a priori to reflect the inclusion of controlled drinking and is set at a level sufficiently above national guidelines for responsible drinking, of which all participants were advised, to represent an unacceptably large departure from those limits. The Global Assessment Scale [29] was also repeated at 6 months and was rated by a group of clinicians utilizing all available information. As has been reported previously [21], overall the sample showed a significant reduction in unequivocal drinking from baseline (90.2%) to follow-up (56.5%) and a significant increase in GAS scores, from 65.0 at baseline to 69.2 at follow-up. Furthermore MET was found to be significantly more effective in reducing unequivocal heavy drinking, to 42.9%, than either NDRL (62.5%) or no further counselling (65.0%) ($\chi^2 = 4.11$, $p < 0.05$), with no significant difference between the latter two.

Treatment process measurement

At the 6-week review clients were asked to rate their satisfaction with the treatment received and perceived effectiveness of that treatment on an 11-point Likert scale (0–10). At the conclusion of treatment for those receiving the MET or NDRL therapists were asked to rate the sessions overall on the extent to which the client was engaged with the therapy, how much rapport they developed with the client, also on an 11-point Likert scale, and number of sessions attended.

Data analysis

Data were analysed using SPSS version 10. Categorical comparisons were conducted utilizing χ^2 for contingency tables. Student's t-test was employed to identify any differences between groups assigned on the dependent variable not eliminated by random allocation. Variables with significance levels less than $p = 0.10$ were entered into univariate analysis with the independent variables to address their possible confounding effects.

Results

Six-month follow-up data were available for 122 of the 124 clients randomized to the three treatment conditions. Of these, 100 were interviewed in person, while for 22 clients the nominated significant other was contacted and reported sufficient knowledge of the client's recent alcohol use. The majority of clients identified a preference for four sessions of counselling over no counselling; with MET the favoured counselling option. Treatment preference was not recorded for four clients. Preferences are shown in Table 1.

For the purpose of comparing clients who did receive their preferred treatment and those who did not it was necessary to identify a subsample who had an unambiguous preference. Thus the 16 clients who stated no clear preference when asked to choose between four sessions of counselling and no counselling were excluded, and so were the 15 who preferred counselling but then had no clear preference for either MET or

Table 1. *Preference for counselling/type of therapy*

Quantity of therapy	N	Type of counselling		
		MET	NDRL	No Preference
Four sessions	90	45	30	15
No counselling[a]	12	-	-	-
No preference	16	8	6	2

MET = motivational enhancement therapy, NDRL = non-directive reflective listening. [a]Clients expressing a preference for 'no counselling' were not subsequently asked which form of counselling they would then prefer.

Table 2. *Relationship of treatment preference-allocation matching to treatment process and outcome*

Measure	Matched			Not matched			F/χ^2	p
	M[a]	SD	n	M[a]	SD	n		
Process								
Satisfaction	7.84	3.83	27	7.44	3.35	45	0.47	0.496
Perceived effectiveness	7.02	4.28	27	6.59	3.75	45	0.44	0.511
Rapport	5.87	5.55	25	5.38	4.78	35	0.33	0.568
Engagement	5.83	5.76	25	5.46	4.96	35	0.18	0.675
Treatment attendance	2.90	2.81	25	3.04	2.42	35	0.09	0.769
Outcome								
Unequivocal heavy drinking	53.3%		30	59.6%		57	5.47	0.486
Change in GAS	2.18	15.12	30	0.78	14.28	57	0.40	0.530

[a]Corrected means.

NDRL. This left a sample of 87 clients with clear treatment preferences—45 preferring MET, 30 preferring NDRL and 12 preferring no counselling beyond the feedback session. Random treatment allocation ensured that the correspondence of preference to allocation was as expected by chance, with 30 clients receiving their preferred treatment and 57 receiving either of the treatments not corresponding to their preference. These formed the two experimental groups for the purposes of the current investigation.

The two groups were compared on a range of baseline variables. This revealed no significant difference for gender, education, employment status, motivation, severity of dependence, alcohol-related problems, current cannabis use disorder, current mood disorder, current anxiety disorder, number unequivocally unsafe drinking days at baseline ($p = 0.056$), marital status or ethnicity ($p = 0.058$). However, those matched to their treatment of choice were older with a mean of 39.7 years compared to 33.4 years ($t = 2.61$, $p = 0.011$), were less likely to have a current diagnosis of conduct disorder or antisocial personality disorder (0% compared to 14.0%, $\chi^2 = 4.64$, $p = 0.031$), and had lower mean baseline GAS scores of 62.3 compared to 65.4 for those not matched ($t = -2.54$, $p = 0.013$). Given these differences across groups, outcome data were analysed with number of unequivocally heavy drinking days, ethnicity, age, current conduct disorder or antisocial personality disorder and baseline GAS as covariates. Corrected mean differences are displayed.

There was no significant association between treatment preference–allocation correspondence and any of the treatment process or outcome measures. As shown in Table 2, both those receiving their preferred treatment and those not were equivalently satisfied, perceived the treatment to be equally effective and attended the same number of sessions. Therapists rated the establishment of rapport and engagement in the treatment process as equivalent for both groups. Drinking outcome at 6 months was not significantly different across groups nor was global functioning at 6 months as measured by GAS scores.

Discussion

For this sample of clients receiving treatment for mild–moderate alcohol dependence, receiving treatment of preference had no measurable impact on treatment outcome, either for drinking behaviour or general functioning. Receiving treatment of preference had no measurable impact on treatment process, utilizing client-rated (satisfaction and effectiveness), clinician-rated (rapport and engagement) and objective (number of sessions attended) measures. A larger sample size would have allowed for the potential identification of small differences between groups, but the sample size employed here provided adequate power to detect moderate to large effects.

This study has a number of methodological advantages over studies described earlier. The three treatment conditions were contemporaneous; treatment preference was elicited prior to randomization and after clients had received a standardized description of the treatment conditions. Confounding factors have been identified and controlled for. Finally, clients were included in analyses on an intention-to-treat basis.

Interpretation of these results must be tempered with an awareness that this study measured treatment preference, not the exercise of actual choice, and the fact that two of the three treatments to which participants were allocated were substantially similar. These points are discussed in greater detail below. These points notwithstanding, the negative findings of the current study suggest a number of research and clinical implications.

First, it serves to reassure that randomized controlled trials are ethical in that they do not, by the very nature of their requirement to remove choice, impair treatment outcome. This assertion comes with caveats, however. Deprivation of choice may be seen to be ethical only when the process of informed consent is observed and when the treatments to which clients are randomized are all believed to be effective, or if any delay in receiving treatment is judged to be brief enough so as to do no harm. This would ordinarily occur only in circumstances where the condition being treated is a minor one or where declining to participate in the study would have resulted in an equivalent or greater delay in treatment and where there is provision for adequate monitoring of clients.

Another important conclusion to be drawn with regard to the main outcome findings of the Brief Treatment Programme for Alcohol Dependence [21], and for the use of the randomised controlled trial (RCT) is that removal of choice does not diminish the clinical applicability of findings. The ability of RCTs to inform clinical practice is tempered by the degree to which the treatment as provided may be seen to be related to such practice. For example, one of the strengths of the Brief Treatment Programme for Alcohol Dependence lies in the fact that the treatment found to produce superior outcomes, MET, may be readily delivered by a large number of adequately trained clinicians, just as it was delivered by real clinicians in a real clinical setting for the study. If removing choice alters treatment outcome then it becomes more difficult to relate research results to clinical settings where choice is a standard component of treatment.

This study should not be taken to mean that treatment planning is not a process of negotiation. The treatment options in this setting were tightly controlled and relatively similar. In a real clinical setting a much broader range of options are available. Clients need to be made aware of all reasonable options, with the treatment plan being the consequence of such discussion rather than being finalized before such consultation. Treatment planning should also be an ongoing process, with reviews and re-negotiations. Not providing reasonable choice in a clinical setting, particularly when there are few options for the client to initiate treatment elsewhere, is not ethical. This study has shown that the inevitable divestment of client of choice, within the checks and balances of a randomized controlled trial, is ethical. The New Zealand Code of Health and Disability Services Consumers' Rights (1994) specifies that consumers have the right to be provided with an explanation of the options available for their condition and have the right to make an informed choice, with exceptions occurring in clearly defined and limited circumstances such as under the

New Zealand Mental Health (Compulsory Assessment and Treatment) Act (1992) or Criminal Justice Act (1985). Conversely, clinicians should be wary of vesting too much control to clients. To hand the choice of treatment over entirely to the client is to abrogate clinical responsibility, failing to recognize that some treatments may be insufficient or excessive for the problem being treated. It would also be naive to suggest that in routine clinical practice a client will be entirely unaffected by the clinician. Client choice is often influenced strongly by their clinician and level of rapport may have an important impact on the strength of this influence and also on outcome. Transference and countertransference issues will also serve to influence client choice.

Caution should be used in interpreting the results of this study. In particular two points should be made. First, this study examined the relationship between treatment preference–allocation concordance and treatment outcome and between that concordance and treatment process. This is not a direct investigation of treatment choice. While those in the matched group did get their preferred treatment what is missing is the sense of control over their own treatment that would have been experienced had subjects actually been able to choose. Furthermore, by eliciting choice before randomization we may have accentuated any potential 'resentful demoralization' among those allocated to treatment that was not their preference. These factors could be overcome by doubling the total sample size and creating a genuine choice stream and a randomized stream (without eliciting treatment preference). As treatment preference was not the primary aim of the Brief Treatment Programme for Alcohol Dependence this modification to the study design could not be justified in this instance.

Secondly, the results must be interpreted in light of the range of treatments available. This study found no difference in treatment outcome when comparing those who received their preferred treatment and those who did not. The available treatments were limited to three options. All three had identical assessment, feedback and follow-up conditions. All subjects were encouraged to try self-help groups in exactly the same way. MET and NDRL were both comprised of four sessions of one-to-one counselling by an experienced therapist in a confidential setting, so the range of treatment options was narrow. Residential treatment was not discussed, nor were day programmes. Medication was not mentioned and family members were not invited to participate in the treatment beyond attending the feedback session. Furthermore, clients were provided with only a brief description of MET and NDRL. Thus, while these results provide good evidence that lack of choice does not alter the outcome for fairly similar treatments, of

which clients had been provided a brief outline only, it cannot be assumed that this would hold for more disparate treatment options or following a more in-depth description of the treatments available. Randomized controlled trials comparing treatments of greater contrast would benefit from asking for client treatment preferences, as we have here, to clarify the impact this may have on reported outcomes.

Acknowledgements

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