

# Deterioration over time in effect of Motivational Interviewing in reducing drug consumption and related risk among young people

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Submitted 8 March 2004;  
initial review completed 24 June 2004;  
final version accepted 24 November 2004

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## ABSTRACT

**Aim** To test whether beneficial effects of a single session of Motivational Interviewing (MI) on alcohol, tobacco and illicit drug use apparent after 3 months were maintained until 12 months.

**Design** Cluster randomized trial, allocating 200 young people in the natural groups in which they were recruited to either MI ( $n = 105$ ) or to an assessment-only control condition ( $n = 95$ ).

**Setting** Ten further education colleges across inner London.

**Participants** Two hundred young people who were current users of illegal drugs (age range 16–20 years) with whom contact was established through peers trained for the project.

**Intervention** The intervention was adapted from MI in the form of a topic-based 1-hour single-session discussion.

**Measurements** Changes in cigarette, alcohol, cannabis and other drug use and perceptions of risk and harm between the time of recruitment and follow-up interviews after 3 and 12 months.

**Findings** A satisfactory follow-up rate (81%) was achieved. After 12 months, 3-month differences between MI and assessment-only groups have disappeared almost entirely. Unexpected improvements by the assessment-only control group on a number of outcomes suggest the possibility of reactivity to the research assessment at 3-month follow-up.

**Conclusion** In the terms of the original experiment, there is little evidence of enduring intervention effectiveness shown by between-group differences after 12 months. Deterioration of effect is the most probable explanation, although reactivity to 3-month assessment, a late Hawthorne effect, cannot be ruled out.

**KEYWORDS** Alcohol, brief intervention, cannabis, cigarette smoking, drugs, Motivational Interviewing, young people.

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## INTRODUCTION

There have now been more than four decades of research in which the 'irrepressible dream of addiction prevention' has been pursued through primary prevention, with a great deal of societal investment (DuPont 1998). Even with the most effective primary preventive interventions, effect sizes are small and many young people will make decisions to initiate and to continue to use drugs

(Coggans *et al.* 2003). For this reason, there is a need to develop secondary prevention among young people already involved in drug use. Brief interventions have been developed for this purpose among adults, most notably in reducing alcohol consumption (Moyer *et al.* 2002).

Motivational Interviewing (MI) is a client-centred and directive counselling style, with an increasingly broad evidence base (Miller & Rollnick 1991, 2002). In 1997, Noonan & Moyers (1997) conducted the first formal

review, in which they examined 11 published controlled trials, all of applications in addiction. Four further reviews have since been undertaken (Dunn, DeRoo & Rivara 2001; Burke, Arkowitz & Dunn 2002; Burke, Arkowitz & Menchola 2003; Burke *et al.* in press), three of which were quantitative studies. In the latest meta-analysis Burke *et al.* (in press) examined 38 controlled trials, 20 of which addressed alcohol problems and six drug problems. The MI interventions under study in these trials are essentially adaptations of MI rather than direct manifestations of the counselling style itself (Burke *et al.* 2003).

Studies of the application of MI with young people and/or for prevention rather than treatment purposes are at a comparatively early stage of development. There has been a treatment preparation study with a teenage clinical population (Aubrey 1998), hospital-based opportunistic interventions targeting cigarette smoking and drinking, respectively (Colby *et al.* 1998; Monti *et al.* 1999), and the targeting of drinking among higher education college students (Marlatt *et al.* 1998; Borsari & Carey 2000; Baer *et al.* 2001). Among these studies, Baer *et al.* (2001) have identified an acceleration of maturational trends towards reduced drinking at the end of the teenage years and into the 20s as a probable mechanism of effect. More recently, indicated preventive interventions have been developed, and are currently under study, which target youth populations already involved in drug use or deemed by other means to be at high risk (Baer & Peterson 2002).

Two particular aspects of the nature of MI effects, as revealed by previous quantitative reviews, are important to note in connection with the current study: (1) Burke *et al.* (2003) identified for the first time a dose–response effect whereby longer MI interventions were found to be more efficacious than shorter interventions; and (2) Dunn *et al.* (2001) found no deterioration in the effect of MI over time, with studies involving shorter-term follow-up assessments having equivalent outcomes to those conducted over the longer term. Baer *et al.* (2001) identified a long-term effect, reduced drinking and related problems, maintained for 4 years in their study of university students.

In comparison with other interventions, this latter characteristic appears somewhat surprising. Moyer *et al.* (2002), in a meta-analysis of brief interventions for alcohol problems in non-treatment-seeking samples, found the largest effect sizes in studies involving follow-up periods of up to, and including, 3 months. According to Moyer *et al.* (2002), averaged effects of brief interventions remained significant for up to 12 months, after which time they became non-significant for both alcohol consumption itself and a composite measure of all alcohol-related outcomes. At present it remains unclear whether

with brief applications of MI there is deterioration of effect over time, in much the same way as is already recognized with brief interventions in general.

This paper comprises the findings from a long-term follow-up of young drug users who were originally randomized to either MI or education-as-usual, for which short-term follow-up data have already been reported (McCambridge & Strang 2004a). Consequently, some description of the original study is required. A single session of MI of up to 1 hour duration was compared with a non-intervention education-as-usual control condition among 200 non-help-seeking participants aged 16–20 years (McCambridge & Strang 2004a). Participants were almost all regular cannabis users, with approximately one-third also involved in stimulant drug use, and were recruited in 10 London further education (FE) colleges (McCambridge & Strang 2004b).

At 3-month follow-up, a wide range of benefits were attributed robustly to the intervention including significant reductions in cigarette smoking, alcohol consumption and cannabis use, with the largest effect being obtained for reduction in cannabis use, the most prominent drug discussed. Following the identification of short-term effectiveness (McCambridge & Strang 2004a), although initially unplanned, we decided to embark on a longer-term follow-up study of this sample, in order to study the longevity of the effects observed after 3 months.

## METHOD

### Design, participants and study procedures

A full account of the methodology of the trial has been provided in the original report of the 3-month follow-up study (McCambridge & Strang 2004a). As a result, only a brief overview will be presented here, with attention drawn to those elements particularly relevant to this 12-month follow-up study. A cluster randomized design was used. A cluster was defined in this context as all those recruited by a single individual (see below), and was used as the unit of randomization. Stratification by college was applied in order to control for local variations in drug use. The inclusion criteria were weekly cannabis use or stimulant drug use within the prior 3 months; exclusion criteria were opiate use or injecting drug use. Participants were required on study entry and at follow-up interviews to provide consent to the provision of a hair or saliva sample for biochemical validation, as a bogus pipeline measure (Werch *et al.* 1989; WHO Brief Intervention Study Group 1996).

Participants were recruited by peers with privileged access, a procedure well established in the study of populations of drug users for whom no orthodox sampling

frame exists (Griffiths *et al.* 1993). These peers were trained in recruitment and baseline data collection procedures—checking eligibility, providing information on the project, obtaining consent and in the distribution, assistance with, and collection of the baseline measure (a 20-minute self-completion questionnaire). This brief baseline instrument was designed specifically to minimize reactivity to assessment, a particular problem in the study of brief interventions (Bien, Miller & Tonigan 1993).

The original follow-up assessment was undertaken 3 months later by structured researcher-administered interview (McCambridge & Strang 2004a). Of the two interviewers at 3-month follow-up, one (J.M.) had also delivered interventions and designed the study, while the other was blind to study allocation in order to test for experimenter bias—this was ruled out as a possible explanation for 3-month between-group differences. After 12 months, the 3-month assessment procedure was repeated, this time involving three interviewers (including the previous two and one other, who was also to blind to initial allocation). At both 3- and 12-month interviews, those few items which required identification of original allocation were deferred until the last section of the instrument to safeguard blinding. Participants were not paid upon entry to the study for initial data collection, but were paid £10 for each interview completed.

### Experimental and control interventions

Following Rollnick, Heather & Bell (1992) we developed an application of MI, in which a topic-based format was used to guide discussions with young people about their relationships with drugs, targeting the entire range of drugs being used. Our application was novel in that any aspects of the use of any drug deemed worthy of focus by the young person themselves—either directly, or by negotiation with them—then formed the objects of the intervention (McCambridge & Strang 2003).

The extent to which the intervention under study actually represented an intention to apply MI may be assessed by inspection of the published report (McCambridge & Strang 2003). Study of process as rated by the practitioner delivering all 105 interventions (J.M.) was broadly supportive of the general narrative of MI, with any discussion of change during intervention being strongly predictive of 3-month outcome (Strang & McCambridge 2004). Possible reduction in cannabis use transpired to be the most prominent subject in discussions. It was not possible to assess the extent to which it was actually delivered as intended—this trial was designed prior to the current practice of recording of interventions-as-delivered as the gold standard method for ascertaining what has actually taken in place sessions

of MI (Miller 2001). The control group self-completed the baseline instrument and subsequently received researcher-administered interviews of approximately 30–40-minutes duration, at both 3 and 12 months.

### Outcome evaluation and analyses

MI is intended to be highly interactive, with a defining characteristic being the particular nature of the balance struck between client-centredness and directiveness (Rollnick & Miller 1995). The method may be practised in different ways with more and less directive applications towards pre-specified objectives possible (Miller & Rollnick 2002). This latter approach was that taken in development and delivery of MI in this study, although directiveness was evident in both the use of the topic-based structure and in practitioner encouragement of attention to areas of individually assessed risk and the possibilities of change (McCambridge & Strang 2003). Thus while drug use was targeted in general terms, with a view to harm minimization, individualized intervention objectives were set during the session for each individual. It was thus necessary to consider a wide range of outcomes. In light of this approach, we decided to apply the following primary stringent criterion for evaluation: outcomes for the intervention group and control group were compared across the groups as a whole, including non-users, and regardless of whether the outcome in question played any part in the intervention. Some logically required subgroup analyses were also undertaken, for example in consideration of cessation, in which those using a particular drug at study entry were the denominator population (McCambridge & Strang 2004a).

No statistical control for the number of tests being performed was made. Type 1 errors were thus possible, i.e. some of the individually observed differences between the intervention group and the control group may have resulted from chance. An early decision had been taken to adopt a more exploratory attitude to the identification of intervention benefit. It was decided to reject the more conservative outcome assessment entailed by elevating the statistical thresholds for differences to be considered significant (Pocock 1997).

For this 12-month follow-up study, it was decided to repeat the analytical methods used at the 3-month point. The Huber–White sandwich estimator of variance was thus used to control for the clustered nature of the recruitment, using STATA (StataCorp 1998). As many of the outcomes under study were not distributed normally, this technique was additionally helpful in being robust to non-normality. Reported regression coefficients were not adjusted for clustering as this was found not to be important. Linear or logistic regression was used for continuous and binary outcomes, respectively, with the baseline

measure included as a covariate. In addition to consideration of the main effects of intervention, it was decided to test for effect modification in those three outcomes for which these analyses had been undertaken after 3 months—reduced cigarette smoking, drinking and cannabis use. Stepwise backward elimination procedures with a value of  $P=0.1$  with respect to potential confounders were employed to arrive at final outcome models.

In light of study questions identified subsequent to the preliminary assessment of 12-month outcome, a range of additional analyses were also undertaken. These will be described at appropriate points in the Results section of this paper, which comprises two main sections of planned and *post-hoc* analyses. All analyses have been conducted primarily among those for whom outcome data were available: the 162 participants (81%) contacted successfully after 12 months, of whom 158 (98%) had also provided 3-month follow-up data. Of the 162, 149 (92% of those providing 12-month outcome data) completed a face-to-face interview involving the complete outcome data set, while for 13 (8%) others it was not possible to interview them face to face. This group was interviewed by telephone with an abbreviated instrument.

#### Attrition and equivalence

There was no difference in attrition between the intervention group and the control group, for whom 80% ( $n=84/105$ ) and 82% ( $n=78/95$ ), respectively, provided 12-month follow-up data. Attrition from study entry proved to be non-random as follows: men were more likely to be lost to 12-month follow-up [73% ( $n=30/113$ ) retained;  $\chi^2=9.6$ , 1 df,  $P=0.002$ ], as were older participants (mean age 18.6 compared to 18.1 years;  $t=2.31$ ,  $P=0.022$ ); non-drinkers at baseline [66% ( $n=21/32$ ),  $\chi^2=5.9$ , 1 df,  $P=0.016$ ]; part-time and non-students [57% ( $n=12/21$ ) and 73% ( $n=8/11$ ), respectively;  $\chi^2=9.6$ , 2 df,  $P=0.008$ ]; those who had missed more college or work days in the month before study entry (mean of 8.9 compared to 5.7 days;  $t=2.67$ ,  $P=0.008$ ); and also those at study entry who had more interactional problems resulting from their own drug use (mean of 1.1 compared to 0.6 problems;  $t=2.1$ ,  $P=0.036$ ). In addition to these differences, attrition was greater on three other measures which were collected not at study entry, but at 3-month follow-up ( $n=179$ ) among those who had ever been homeless [75% 4 ( $n=18/24$ );  $\chi^2=4.7$ , 1 df,  $P=0.03$ ]; those who had ever been in the care of Social Services [72% ( $n=13/18$ );  $\chi^2=5.0$ ,  $P=0.026$ ]; and among those who had never sold drugs to friends [84% ( $n=16/97$ );  $\chi^2=5.9$ ,  $P=0.015$ ].

The resulting groups were non-equivalent in ethnic composition, as they had been at study entry, due to the

limitations of the cluster method of randomization. The intervention group comprised 37% white, 57% black and 6% Asian youth compared to 46% white, 35% black and 19% Asian youth in the control group ( $\chi^2=11.0$ , 2 df,  $P=0.004$ ). Three other potential confounders were also non-equivalent and thus included in initial models along with ethnic group—baseline attitudinal positivity to drug use ( $t=2.86$ ,  $P=0.005$ ), 12-month intention to stop cannabis use ( $\chi^2=5.98$ , 1 df,  $P=0.014$ ) and the total number of drugs intended to be using after 12 months ( $t=3.5$ ,  $P=0.0006$ ). Also considered in 12-month outcome analyses were interviewer (first author  $n=51$ , 31.5%; first blind interviewer  $n=47$ , 29%; second blind interviewer  $n=64$ , 39.5%) and data collection method (face to face and by telephone). Neither of these variables were related to observed outcomes.

## RESULTS

### Results 1: planned analyses of outcome

#### *Change in cigarette smoking*

Mean frequency of cigarette smoking in the intervention group as a whole ( $n=84$ ), having declined from 33.4 to 23.8 cigarettes per week after 3 months, was observed to have increased again to 27.7 cigarettes per week after 12 months ( $t=1.84$ ,  $P=0.07$  for baseline/12-month mean comparison; see Fig. 1). Only when the analysis is restricted to those who were smokers at study entry ( $n=66$ ) is the difference over time between baseline and 12 months statistically significant—the mean number of cigarettes per week in this group declined from 41.0 to 32.3 ( $t=2.37$ ,  $P=0.02$ ).

Mean frequency of cigarette smoking in the control group as a whole ( $n=78$ ) had increased from 37.1 to 39.6 cigarettes per week after 3 months, but was then found to have decreased to 34.2 cigarettes per week after 12 months ( $t=0.57$ ,  $P>0.1$  for baseline/12-month mean comparison). The difference over time between

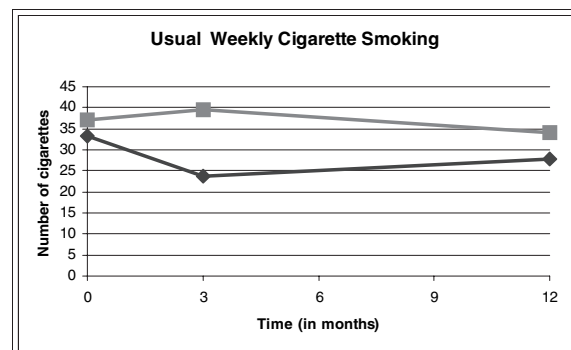


Figure 1 Usual weekly cigarette smoking. ♦, MI; ■, Control

baseline and 12 months was not statistically significant, and remained non-significant even when restricted to only those who were smokers at study entry ( $n = 60$ )—among whom the mean number of cigarettes per week in this group declined from 47.7 to 38.9 ( $t = 1.45$ ,  $P > 0.1$ ).

When all those who were cigarette smokers at entry to the study in both groups ( $n = 126$ ) are examined, there had been a reduction over time in the number of cigarettes smoked per week, from 44.2 to 35.4 cigarettes per week ( $t = 2.53$ ,  $P = 0.013$ ). However, unlike the 3-month assessment (when there was a significant between-group difference), the 12-month between-group difference is not significant ( $t = 1.1$ ,  $P > 0.1$ ) with wide confidence intervals [intervention group mean number 27.7, 95% CI (19.8–35.7), control group mean number 34.2 (25.7–42.8)] resulting from considerable dispersion around the mean, which is also the case when considering only cigarette smokers at study entry.

Of the 36 who were not cigarette smokers at study entry, five of the intervention group and six of the control group initiated cigarette smoking during the 12-month study period. Thirteen smokers in the intervention group ( $n = 66$ ) and six smokers in the control group at study entry ( $n = 60$ ), respectively, ceased smoking.

Although the main effects of intervention on cigarette smoking have disappeared, there remain intervention effects on two subgroups as detected by tests for interaction: (a) heavier smokers at study entry ( $P = 0.02$ ) and (b) those who had interactional problems resulting from their own drug use at study entry ( $P = 0.02$  for any problems;  $P = 0.005$  for the number of these problems).

#### Change in alcohol consumption

In terms of past-week quantity/frequency alcohol consumption, the intervention group as a whole ( $n = 84$ ) having reduced from 12.5 to 8.7 units after 3 months, was observed to have increased again to a mean of 11.5 units (95% CI – 8.4–14.6; see Fig. 2) after 12 months. On

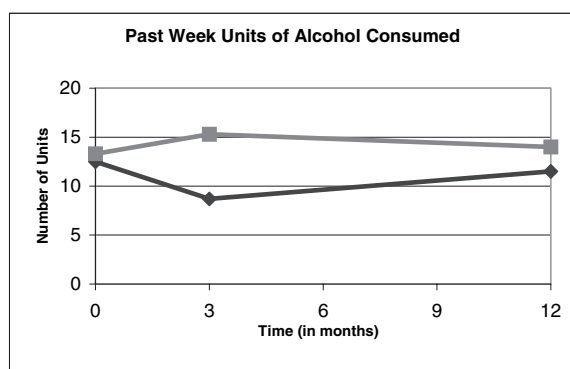


Figure 2 Past week units of alcohol consumed. ♦, MI; ■, Control

this measure, there was little change in the control group ( $n = 78$ ), with mean values reported of 13.3, 15.3 and 14.0 units (10.2–17.8) at baseline and after 3 and 12 months, respectively. Again, unlike the 3-month assessment, the 12-month between-group difference was no longer significant ( $t = 1.0$ ,  $P > 0.1$ ).

Five of eight baseline non-drinkers at study entry in the intervention group remained abstinent, compared to seven of 13 in the control group. Six of the 76 baseline drinkers in the intervention group ceased alcohol consumption during the study period, as did one of 65 in the control group. As with cigarette smoking, while the main alcohol effect has disappeared, there was some evidence of subgroup effect—those who lived in households reliant on state benefits reported drinking less alcohol in the previous week after 12 months than controls (interaction term significance  $P = 0.013$ ).

#### Change in cannabis and other illicit drug use

In the intervention group ( $n = 84$ ), the mean frequency of cannabis use had declined dramatically from 16.2 to 5.9 times per week after 3 months, but then increased to 8.6 times per week by the time of the 12-month follow-up. The intervention group were smoking cannabis less frequently after 12 months than they were at baseline ( $t = 3.12$ ,  $P = 0.003$ ; see Fig. 3). In the control group ( $n = 78$ ) there was an increase in mean frequency of cannabis use during the first 3 months from 12.5 to 16.6 times per week, after which time frequency of cannabis use returned to 11.9 times per week, approximately the level reported at study entry. Across the two study groups there has been a reduction over time in the frequency of cannabis use after 12 months ( $t = 2.47$ ,  $P = 0.015$ ). However, as with change in cigarette smoking, the 12-month between-group difference is not significant as a result of wide confidence intervals [intervention group mean 8.6 (5.8–11.5), control group mean 11.9 (7.4–16.4);  $t = 1.23$ ,  $P > 0.1$ ] and, as with cigarette smoking and

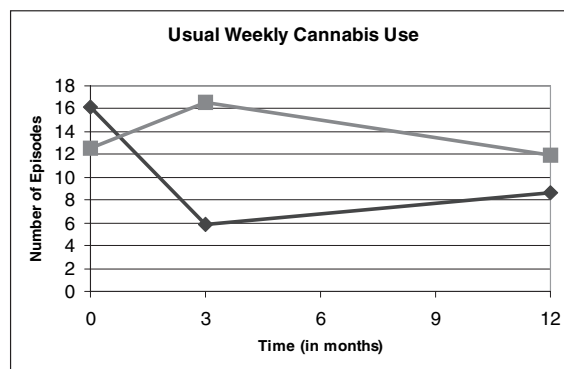


Figure 3 Usual weekly cannabis use. ♦, MI; ■, Control

alcohol consumption, the significant difference seen at the 3-month assessment is no longer present after 12 months.

Two other cannabis consumption variables were collected only at 3- and 12-month follow-up (i.e. not at study entry)—weekly quantity of cannabis consumed and number of abstinent days in the month prior to interview. On the former measure (quantity per week), the 3-month between-group difference of approximately one-eighth of an ounce is maintained until 12 months, but is no longer statistically significant [intervention group mean 0.21 (0.14–0.27), control group mean 0.30 (0.17–0.42);  $t = 1.31$ ,  $P > 0.1$ ]. On the latter measure (abstinent days per month), the between-group difference remains of similar magnitude as reported at 3 months and also remains statistically significant [intervention group mean 17.8 abstinent days (15.6–20.0), control group mean 13.7 (11.1–16.3);  $t = 2.37$ ,  $P = 0.02$ ]. When modelled, this between-group difference proved to be robust to potential confounding [ $B = 4.1$  (0.6–7.6),  $P = 0.025$ ].

After 12 months, 14 cannabis users in the intervention group (17%) and six users in the control group (8%) had ceased their use of this drug ( $\chi^2 = 3.0$ , 1 df,  $P = 0.083$ ), although only five cases in the intervention group and two cases in the control group were stable non-users at both follow-up points. There was no evidence of any residual subgroup effect on reduced cannabis use. As at 3 months, patterns of consumption of stimulant drugs were similar in both groups. There were similar numbers and proportions using stimulant and other illicit drugs, usually infrequently, with little capacity to detect significant differences in levels of consumption.

#### *Change in perceptions of drug-related risk and harm*

Evidence of efficacy after 3 months was more pronounced for the above changes in actual drug consumption than for change in perceptions of drug-related risk and harm. In addition to changes in cigarette smoking, alcohol consumption and cannabis use, statistically significant effects on 16 other outcomes were reported (McCambridge & Strang 2004a). Of these, only one between-group difference remained significant at the 12-month follow-up: subjects in the intervention group ( $n = 6/84$ , 7%; control group  $n = 14/78$ , 18%) were less likely to be in the presence of someone smoking heroin ( $\chi^2 = 3.0$ , 1 df,  $P = 0.034$ ). This difference was robust to potential confounding [OR = 0.35 (0.16–0.77),  $P = 0.009$ ]. Heroin use had been one of the exclusion criteria for study entry, and after 12 months, two of 78 of the intervention group and five of 70 of the control group have actually themselves initiated heroin use during the

study period (Fisher's exact test = 0.26; data not collected by telephone for 14 participants).

#### **Results 2: unplanned analyses of outcome**

Having undertaken planned analyses and observed a dramatic deterioration of the initial intervention effect between 3- and 12-month follow-up points, the following three sets of additional analyses sought to both confirm and explore the nature of this change over time. In the first instance the experimental contrast was manipulated *post hoc*, so that only those in the intervention group who had discussed a particular drug during the course of the intervention, as the most prominent or second most prominent drug, were compared with the control group. No differences were detected in this way. Considering only the intervention group, these data were also examined to see whether discussion of a drug was related to 12-month outcome, and again no relationships were detected.

A second set of analyses involved a revised approach to outcome evaluation. Rather than consideration of effects solely on individual drugs, potential effects were conceptualized as the aggregate of effects on different drugs. In addition to cessation data already presented, seven stimulant users in the intervention group and five in the control group ceased their use of these drugs during the 12-month study period. The use of other illicit drugs was deemed to be too rare and infrequent to be considered in this way. Although the numbers of individuals are small in each case, the full cessation data are presented in Table 1, with both groups having been equivalent in respect of the use of each of these drugs at study entry. These data suggest that the intervention group were approximately twice as likely to have ceased the use of any drug during the 12-month study period. This outcome is confirmed when controlling for potential confounders [OR = 2.55 (1.23–5.25),  $P = 0.012$ ].

**Table 1** Drug cessations within 12 months.

	Intervention <i>n</i> = 84	Control <i>n</i> = 78
Cannabis only	6 (7%)	3 (4%)
Cigarettes only	7 (8%)	3 (4%)
Alcohol only	3 (4%)	1 (1%)
Cannabis and cigarettes	3 (4%)	2 (3%)
Cannabis and alcohol	2 (2%)	0
Cannabis, alcohol and cigarettes	1 (1%)	0
Stimulants only	3 (4%)	4 (5%)
Stimulants and cannabis	2 (2%)	0
Stimulants and cigarettes	2 (2%)	0
Stimulants, cigarettes and cannabis	0	1 (1%)
Cessation of any drug	29 (35%)	14 (18%)
No cessations	55 (65%)	64 (82%)

The final set of analyses addressed the unexpected performance of the control group, where increases in the first 3 months were subsequently reversed by the time of 12-month follow-up. At 12-month follow-up, both groups were asked whether their participation in the study had affected their use of each drug on a seven-point scale ranging from 1 (not at all) to 7 (very much). There were clear differences in the reported influence of study participation between intervention and control groups on all drugs: on a seven-point scale, intervention group mean scores were 3.3, 2.4 and 3.5, respectively, for self-reported influence of study participation on cigarette smoking, alcohol consumption and cannabis use compared to control group mean scores of 2.1, 1.3 and 1.9 ( $t = 3.1, P = 0.003$ ;  $t = 4.1, P < 0.001$ ;  $t = 4.9, P < 0.001$ , respectively). These were then evaluated in relation to 12-month outcomes. For both cigarette smoking and cannabis use the main effect was statistically significant [B = 3.25 (0.91–5.59),  $P = 0.008$  for influence on cigarette smoking; B = 1.25 (0.09–2.46),  $P = 0.044$  for influence on cannabis use] and the interaction term was not, indicating that for both drugs, both study groups reported being equivalently influenced by study participation and that this report was associated with behavioural outcome. For alcohol consumption, neither main effect nor interaction term was significant, indicating no relationship between reported influence of study participation and outcome.

## DISCUSSION

### Deterioration of effect

Significant between-group differences were observed after 3 months with reduced consumption of cigarettes, alcohol and cannabis (McCambridge & Strang 2004a). These have largely disappeared by 12 months. There has also been unexpected change in the control group, among whom there have been reductions in actual consumption of two of the three main drugs and a levelling of the third (see Figs 1–3). A wide range of other effects observed at 3 months have almost all disappeared, and those remaining may be chance artefacts of multiple statistical testing. The extent to which there has been a fading away of the impact of MI is not entirely certain.

### Hawthorne effect?

The possibility of a late Hawthorne effect needs to be considered seriously. We deliberately omitted any extensive interview and data collection at baseline so as to avoid the possibility of an inadvertent 'brief intervention' effect arising out of the assessment itself. Instead, we conducted

this more extensive interview and data collection at 3-month follow-up point. However, might this substantial follow-up interview at 3 months have exerted a beneficial effect, and thereby reduced the difference between the intervention and control groups at subsequent longer-term follow-up? The duration of follow-up interview was not measured as not being an object of study. These usually took between 30 and 45 minutes and were thus of similar duration to the original intervention.

Unintended reactivity to research conditions has bedevilled the study of brief interventions from the outset. The seminal report by Russell *et al.* (1979) contained the overlooked finding that more participants, for whom abstinence was biochemically validated after 1 year, actually quit smoking in the month immediately preceding the research interview than had quit in the month following brief intervention. When dedicated studies have been undertaken to estimate the size of these effects in treatment studies they have been found to be large (Clifford *et al.* 2000). More than a decade ago, Bien *et al.* (1993), proposed the employment of Solomon four-group designs in order to separate and estimate precisely the effects of assessments and brief interventions. To our knowledge, this call has gone unheeded. Studies of these types are essential if we are ever to be able to estimate the real world impact of brief interventions, i.e. without confounding by research conditions.

### The nature of the original effect

How do the original effect sizes compare with other studies? For reduced drinking, the 0.37 (0.15–0.6) effect size obtained in our study after 3 months was strikingly similar to that obtained by Burke *et al.* (in press) from 15 other studies—0.35 (0.25–0.46). This is lower than that reported by Moyer *et al.* (2002) for three comparable brief intervention studies with follow-up periods of 3 months or less [0.67 (0.39–0.95)], and larger than the pooled effect of 11 studies with 3–6-month follow-up excluding severely problematic drinkers [0.21 (0.14–0.27)]. This study was similar to the previous two MI cigarette smoking cessation studies for which a combined effect size was non-significant (Colby *et al.* 1998; Butler *et al.* 1999;  $P = 0.056$ ). The effect was similar in magnitude, however, to that obtained in a previous meta-analysis of brief interventions for smoking cessation in general (odds ratio of 2.8 compared to 2.4 for counselling in excess of 10 minutes as calculated by Wetter *et al.* 1998). We did, however, achieve an impact on reduction in cigarette smoking which was very similar to that obtained for reduced drinking [0.34 (0.09–0.59)].

The reduced cannabis effect size [0.75 (0.45–1.0)], which was similar to that observed in the adult cannabis MI treatment study by Stephens, Roffman & Curtin

(2000), was just within the 95% confidence interval calculated by Burke *et al.* for drug use [0.56 (0.33–0.79)]. In our study, this did not include assessments of effects on other drug consumption which proved non-significant and which, if combined, would deflate the estimate of effect. Finally, the impact on various measures of drug-related problems in our original study was rather patchy, due possibly to floor effects arising from the relatively minor nature of problems in our study population compared to those found in clinical populations.

Where comparisons are possible, the size of the effects obtained after 3 months are broadly in line with those identified in meta-analyses of intervention studies targeting single drug use behaviours and their consequences in diverse, but usually adult populations. It is difficult to assess congruence with maturational trends in the present study as this study population is younger than that studied previously (Baer *et al.* 2001), and trends in cigarette and cannabis smoking are highly probably distinct from those for alcohol. In our study these effects were achieved simultaneously. What of the nature of these effects, irrespective of their size? This study mirrors the limitations of the wider MI literature (with notable exceptions) in its incapacity to examine the detail of exactly what has taken place during the intervention (Burke *et al.* 2002).

Content analyses of the participants' answers to the question; 'What effect, if any, did the interview have on you?' at the 3-month interview revealed three categories of response (McCambridge 2002): (a) drug consumption assessment benefits were those which may be expected to be associated with any carefully focused reflection on levels of consumption; (b) risk recognition effects, in which the receipt of new information or the achievement of a new perspective on drug use led to altered risk management strategies; and (c) life context evaluation of drug use approximating most closely the distinctive MI aspiration of the intervention. Thus it may be that regardless of both the pre-intervention intentions and the actual conduct of the practitioner, participants have derived for themselves different types of effect, which may presumably vary in longevity. In this study, the more predominant the former types of effect, the more likely it is that their effects will be indistinguishable from the research interview of similar duration 3 months later.

### Implications for further study

MI led to important changes in drug use behaviours which are clearly detectable at 3 months but which have largely, but not entirely, faded by 12 months. Perhaps booster doses would enhance the benefit. Methodological lessons can also be learned from this study. Beyond the need to record intervention sessions, and to seek to study

and minimize reactivity to research assessments, the issue of outcome evaluation for MI holds particular challenges in view of the need for the discussion material, and consequently intervention objectives to be chosen by, or at least negotiated with, participants.

One final important conclusion can be reached. On the basis of the above findings, young people are as receptive to initial MI prevention impact as other populations, although it is possible that this effect may fade significantly over time, notwithstanding that Baer *et al.* (2001) have already demonstrated the potential for long-term impact on drinking behaviour. Indeed, we are not aware of any evidence in variability of MI, or indeed any brief intervention, effect by age. Perhaps, therefore, we should not be excessively concerned about the impact of age on receptivity to intervention, and seek to study instead the longevity of effects. The translation of this highly promising treatment technology into a secondary prevention intervention has not yet reached the point where there are data which can permit a fuller exploration of these features. Further studies of MI will thus identify more precisely the nature of the intervention itself, the distinctive features of prevention application with young people, as well as the durability of effects.

### Acknowledgements

The original trial was undertaken in the context of a Research Training Fellowship awarded by the NHS Executive (London/South Thames) to the first author under the supervision of the second author. We are grateful to Action on Addiction for additional funding to enable the longer-term follow-up study reported here, and to Eleni Varsimou and Jenny Abbey for assistance with interviewing. We are also grateful for helpful comments from anonymous reviewers.

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