Record Status
This is a critical abstract of an economic evaluation that meets the criteria for inclusion on NHS EED. Each abstract contains a brief summary of the methods, the results and conclusions followed by a detailed critical assessment on the reliability of the study and the conclusions drawn.

Health technology
A project for the treatment of excessive alcohol use was examined. The project, Trial of Early Alcohol Treatment (TrEAT), was based on a clinician-delivered brief intervention during office visits. The protocol consisted of two 15-minute sessions, one scheduled after the first contact with the general practitioner (GP), and two 5-minute follow-up phone calls from an office nurse.

Type of intervention
Treatment.

Economic study type
Cost-effectiveness analysis and cost-benefit analysis.

Study population
The study population comprised individuals aged 18 to 65 years. The inclusion criteria specified consumption of more than 14 drinks/week (168 g alcohol) for men, more than 11 drinks/week (132 g alcohol) for women, or more than 5 drinks on at least four occasions in the previous 30 days for either gender. The exclusion criteria were pregnancy, recent suicide ideation, symptoms of alcohol dependence, alcohol treatment in the last year, and severe medical problems.

Setting
The setting was primary care. The economic study was conducted in 10 southern Wisconsin counties in the USA.

Dates to which data relate
The dates during which the effectiveness and resource use data were collected were not reported. The price year was 1993.

Source of effectiveness data
The effectiveness evidence was derived from a single study.

Link between effectiveness and cost data
The costing was conducted prospectively on the same sample of patients as that used in the effectiveness study.

Study sample
Power calculations to determine the sample size were not conducted. An initial group of 17,695 individuals, aged between 18 and 65 years, entered their GP office for routine care and completed a self-administered health questionnaire containing questions on smoking, exercise, weight and other health-related issues. Of these, 2,450 screened positive for at-risk alcohol use and were invited to participate in a face-to-face assessment interview. Seventy
per cent (1,705) agreed to participate, and 774 met the eligibility criteria and were allocated to either the intervention group (392) or control group (382). Reasons for exclusion were not meeting the eligibility criteria, particularly in relation to alcohol use.

**Study design**
This was a randomised controlled trial, which was carried out in the offices of 64 community-based primary care physicians from 17 clinics. The method of randomisation was not described and the physicians were unaware of the group to which their patients were assigned. All of the participants received a general health booklet. No further intervention was available for control patients, who were advised to contact their GP to discuss any concerns they had after reading the booklet. The length of follow-up was 48 months. The patients were contacted at 6, 12, 24, 36 and 48 months after enrolment to provide data on the study outcomes. Medical record audits were conducted at 12 and 48 months. All follow-up interviews were conducted by telephone. The follow-up rates were 93% at 12 months, 89% at 24 months, 87% at 36 months, and 83% at 48 months. More patients in the intervention group were unavailable for the final assessment than in the control group. The reasons given were death (n=10), refusal (n=83), and simply lost to follow-up (n=38).

**Analysis of effectiveness**
The analysis of effectiveness was conducted on an intention to treat basis. Missing data were estimated using baseline or post-baseline average data (conservative estimates). The outcomes used were:

- differences in 7-day alcohol use and 30-day binge drinking episodes during the last 30 days between the control and intervention groups;
- changes in the proportions of heavier drinkers (defined as >20 drinks/week for men and >13 drinks/week for women) and binge drinkers (persons who reported one or more binge drinking episode during the last 30 days);
- mortality;
- emergency department visits and days of hospitalisations;
- motor vehicle crash with fatalities, nonfatal injuries or property damage only, operating while intoxicated, or other moving violations; and
- legal events such as assault/battery/child abuse, resist/obstruct officer/disorderly conduct, controlled substance/liquor violation, criminal damage/property damage, theft/robbery, and other arrests.

The authors stated that the study groups were comparable in terms of any baseline outcome measure. However, the demographic and medical characteristics of the sample were not provided. The statistical significance in treatment effect was assessed using a repeated measures analysis of variance.

**Effectiveness results**
For 7-day alcohol use, statistically significant treatment effects were observed for both men and women over the 4-year follow-up:

- for men, alcohol use was reduced from 21.3 to 14.1 drinks/week at 6 months and this reduction was maintained over the 4 years;

- for women, alcohol use was reduced from 14.8 to 8.4 drinks/week at 6 months and this reduction was maintained over all the follow-up points.

Reductions were also observed in the control group, but these did not reach statistical significance.

Similar results were observed for the 30-day binge drinking episodes during the last 30 days, although more variability
was reported.

The proportion of heavier drinkers changed from 46.7% at baseline to 22.4% at 48 months in the intervention group, and from 49.2% at baseline to 26.4% at 48 months in the control group, (p=0.0005).

The proportion of binge drinkers varied from 85 to 63.8% in the intervention group and from 86.9 to 70.4% in the control group, (p=0.0004).

Three individuals in the intervention group and seven in the control group died due to different causes. This difference was statistically significant at 36 months, but not at 48 months.

There were 392 emergency department visits in the intervention group versus 376 in the control group, (p<0.10), and 420 (intervention) and 664 (control) days of hospitalisation, respectively, (p<0.05).

In terms of motor vehicle events, there were:

0 crashes with fatalities in the intervention group versus 2 in the control group, 20 versus 31 crashes with nonfatal injuries, and 67 versus 72 crashes with property damage only;

25 violations in each group of operating a vehicle while intoxicated; and

169 other moving violations in the intervention group versus 177 in the control group.

In terms of legal events, there were:

8 (intervention group) versus 11 (control group) assault/battery/child abuse events,

8 (intervention) versus 6 (control) resist/obstruct officer/disorderly conduct events,

2 (intervention) versus 11 (control) controlled substance/liquor violation events, (p<0.05),

2 (intervention) versus 1 (control) criminal damage/property damage events,

3 (intervention) versus 3 (control) theft/robbery events, and

5 versus 9 other arrests.

**Clinical conclusions**

The effectiveness analysis showed that the intervention for treating excessive alcohol use was effective in reducing alcohol use (also in the sub-group of heavy drinkers), medical events requiring hospitalisation or emergency department visits, and both legal and motor vehicle events. Improvements were also observed in the control group, but these changes did not reach statistical significance.

**Measure of benefits used in the economic analysis**

The health outcomes were left disaggregated and no summary benefit measure was used. A cost-consequences analysis was therefore conducted when the perspective of the medical care system was adopted. However, in the cost-benefit analysis, the summary benefit measure referred to the economic value of two main components:

avoided services utilisation (hospitalisations and emergency department visits); and

reductions in legal events and motor vehicle accidents. This considered the intangible costs of crime and motor vehicle crashes (e.g. victim's pain, suffering, reduction in quality of life, and work losses) as well as medical care, mental health services, property damage, public services costs and other monetary losses.

Thus, a monetary value was put on intangible benefits associated with legal events, which accounted for almost the
entire monetary benefit estimated in the analysis. Therefore, this represented the main economic benefit, the value of which was estimated from a published study (Miller et al., see Other Publications of Related Interest).

**Direct costs**
The perspective adopted in the analysis of the direct costs was that of the medical care system. Local managers were contacted to estimate provider costs. Economic benefits referring to avoided services utilisation were estimated from a published study (French and Martin, see Other Publications of Related Interest). The health services considered in the calculation were clinic costs (supplies, telephone calls and salaries), overheads, and the patients' travel expenses and time spent attending GP visits. The economic benefits, which would be more appropriately defined as the costs saved due to the study intervention, included avoided services utilisation (hospitalisations and emergency department visits).

Most of the details of the cost analysis were taken from another study by the authors of the present study (Fleming et al.). The resource use data were estimated alongside the clinical trial, but the dates during which the data were collected were not reported.

Patients' time was estimated from occupation data from the Wisconsin Career Information System. The opportunity cost of time for those who were not employed (e.g. students and homemakers) was assumed to have been equal to the average hourly wage rate for all occupations in Wisconsin.

Discounting was not conducted. However, it was unclear whether it was relevant because the costs per patient could have been incurred during 48 months. The unit costs were not reported separately from the quantities of resources used. The price year was 1993.

**Statistical analysis of costs**
Due to skewed data, non-parametric statistical tests were conducted to compare the costs estimated in the two groups.

**Indirect Costs**
The indirect costs were not included in the analysis.

**Currency**
US dollars ($).

**Sensitivity analysis**
Sensitivity analyses were not conducted.

**Estimated benefits used in the economic analysis**
In the cost-benefit analysis, the estimated benefits per patient were $7,985 (intangible losses and medical interventions avoided).

**Cost results**
Under the perspective of the medical care system, the direct costs were $166 and the cost-savings per patient were $712. Thus, the net cost-savings were $546 (95% confidence interval, CI: -71 - 1,164; p=0.08) and the benefit-cost ratio was 5.3 (95% CI: 0.6 - 8).

Patient travel and time costs were $39 per patient. Thus, the total costs relevant to the societal perspective were $205.

**Synthesis of costs and benefits**
In the cost-effectiveness analysis, the costs and benefits were not synthesised because a cost-consequences analysis was performed.

In the cost-benefit analysis, the societal net benefit was $7,780 (95% CI: 894 - 14,668; p=0.01) and the benefit-cost ratio was 39 (95% CI: 5.4 - 72.5).

**Authors’ conclusions**  
Brief advice from general practitioners (GPs) was effective and efficient in reducing alcohol abuse and the use of medical services in the long run. Substantial cost-savings were observed when a societal perspective was adopted.

**CRD COMMENTARY - Selection of comparators**  
The authors compared the study intervention with no programme, which was appropriate because no specific intervention is generally used to control excessive alcohol use in primary care. You should decide whether it represents a valid comparator in your own setting.

**Validity of estimate of measure of effectiveness**  
The analysis of effectiveness used a randomised controlled trial, which was appropriate for the study question. The study was conducted in several centres and the method of sample selection was clearly reported. The length of follow-up was stated, as were the reasons for the loss to follow-up. The patients’ demographics were not reported, although the authors stated that the two study groups were well balanced at baseline. The basis of the effectiveness analysis was intention to treat. The methods used to derive the final outcome values from patients with missing data were reported. These issues tend to increase the internal validity of the analysis. However, power calculations were not conducted, although the study samples were large. The method of randomisation was not reported. The authors noted that the use of self-reported data may represent a limitation to the validity of the analysis.

**Validity of estimate of measure of benefit**  
In the cost-effectiveness analysis, no summary benefit measure was used in the analysis because a cost-consequences analysis was conducted.

In the cost-benefit analysis, the benefit measure was estimated as the monetary value of intangible costs avoided, as well as health care resources avoided. This appears to have been appropriate since a societal perspective was adopted. Accordingly, the economic benefits relevant to the entire society were considered. The monetary value of the benefits was derived from a study, a report by the Department of Justice, which estimated intangible costs mainly associated with crimes in the USA. The number of relevant events was estimated in the effectiveness study.

**Validity of estimate of costs**  
Two perspectives were adopted in the economic analysis. It appears that most of the relevant categories of costs have been included. The only possible exception was productivity loss due to morbidity. Details on the unit costs were not reported, although the quantities of resources used were provided. The price year was given, thus simplifying reflation exercises in other settings. The sources used to estimate both the direct and indirect costs were reported. The cost estimates were specific to the study setting and sensitivity analyses were not conducted. Statistical tests were performed to compare the resource use and costs in both groups. Discounting was not conducted, but it may have been relevant since the patients were followed for a long time. The authors stated that the actual cost-savings may be even higher than those estimated in the study because research costs were included in the analysis.

**Other issues**  
The authors compared their findings with the results of other studies that assessed the short-term benefits of the study intervention. The authors noted that their study was the first to estimate the long-term impact of brief physician advice for the control of alcohol use. However, the issue of the generalisability of the study results to other settings was not
addressed and sensitivity analyses were not conducted. Thus, the external validity of the analysis was low.

**Implications of the study**
The study results suggested that brief physician advice led to sustained medical and economic benefits for the individual patient, the health care system, and society as a whole.

**Source of funding**
Supported by the Robert Wood Johnson Foundation (#027204) and the National Institutes of Health, National Institute on Alcohol Abuse and Alcoholism (AA08512-01A).

**Bibliographic details**

**Other publications of related interest**


**Indexing Status**
Subject indexing assigned by NLM

**MeSH**
Adolescent; Adult; Aged; Alcohol Drinking /economics /mortality /therapy; Analysis of Variance; Comparative Study; Cost-Benefit Analysis /methods /statistics & numerical data; Female; Follow-Up Studies; Humans; Male; Middle Aged; Physician-Patient Relations; Research Support, Non-U.S. Gov't; Research Support, U.S. Gov't, P.H.S.; Time Factors; Treatment Outcome

**AccessionNumber**
22002000316

**Date bibliographic record published**
30/04/2004

**Date abstract record published**
30/04/2004