Screening in brief intervention trials targeting excessive drinkers in general practice: systematic review and meta-analysis

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Abstract

Objective To determine the effectiveness of programmes of screening in general practice for excessive alcohol use and providing brief interventions.

Design Systematic review and meta-analysis of randomised controlled trials that used screening as a precursor to brief intervention.

Setting General practice.

Main outcome measures Number needed to treat, proportion of patients positive on screening, proportion given brief interventions, and effect of screening.

Results The eight studies included for meta-analysis all used health questionnaires for screening, and the brief interventions included feedback, information, and advice. The studies contained several sources of bias that might lead to overestimates of the effects of intervention. External validity was compromised because typically three out of four people identified by screening as excessive users of alcohol did not qualify for the intervention after a secondary assessment. Overall, in 1000 screened patients, 90 screened positive and required further assessment, after which 25 qualified for brief intervention; after one year 2.6 (95% confidence interval 1.7 to 3.4) reported they drank less than the maximum recommended level.

Conclusions Although even brief advice can reduce excessive drinking, screening in general practice does not seem to be an effective precursor to brief interventions targeting excessive alcohol use. This meta-analysis raises questions about the feasibility of screening in general practice for excessive use of alcohol.

Introduction

General practitioners are strongly encouraged to identify and intervene with patients whose alcohol consumption is either hazardous or harmful to their health. Screening using standardised questioning and brief interventions consisting of a few minutes of feedback, information, and advice are promoted for that purpose.[1–3] Implementation research has been carried out,[4–7] not without difficulty, and there is clearly much still to learn about the suitability and compatibility of brief interventions used after screening.[8]

Intervention can work and has been reviewed.[9–14] Calculations of efficacy, which compare screening and brief intervention versus screening and no intervention or less intervention, ignore the many patients who are lost.[15] The effectiveness of screening as a precursor to brief intervention has not been systematically evaluated.

Because screening has become part of recommendations,[16] we aim to provide an estimate of the screening effect equivalent to the one introduced for disease screening by Rembold.[17] This estimate relates screening as a case finding approach to experimental events, in our case "clinically important changes"—that is, changes in alcohol consumption from above the recommended levels to below these levels.[18]

This review aims to answer the question, how effective is screening in general practice for locating patients who consume excessive amounts of alcohol and can benefit from brief interventions and change their drinking to within sensible limits?

Methods

We used the basic review and meta-analysis principles recommended by the Cochrane collaboration and the principles of mapping attrition set out by Feinstein.[19 20]

Identifying studies

AB and TT searched the electronic databases, checked reference lists of earlier reviews and retrieved papers, hand searched, and consulted European experts. They then defined a final electronic search strategy (box 1) and criteria for inclusion (box 2). Medline, Embase, PsycInfo, Cochrane, and ETOH databases were searched without time limits for reports in English language on controlled trials.

Assessing validity

Internal validity—We assessed four types of bias in each study included in the meta-analysis (selection bias, performance bias, attrition bias, detection bias; see table 2).[19 20] The validity assessment was used only for the discussion of differences between studies and possible bias of findings.[19]

External validity—We used an adaptation of Feinstein's model (fig 1) to assess losses from screening

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Box 1: Words and phrases used to search databases

Subject or disorder—alcoholism, alcohol drinking, alcohol-related disorders, substance-related disorders, drinking behaviour, drink

Intervention—patient education, counselling, psychotherapy, behavior therapy, patient-centered care, patient acceptance of health care, physician-patient relations, attitude to health, "knowledge, attitudes, practice," health behavior, patient compliance, life style, prevention, advice, intervention

Setting—family practice, primary health care, physicians, nurse practitioners, general practice, general practitioner, physician, nurse

Methodology—randomized controlled trial, clinical trials, controlled clinical trials, intervention studies, outcome and process assessment (health care), follow-up studies, double-blind method, random allocation, treatment outcome, randomised controlled trial, randomised, randomized, random, comparative studies

to follow up.[20] This categorises the excessive drinkers available for screening (users of the healthcare system, those not refusing screening, not physically or psychologically impaired, not illiterates, etc) into five groups.

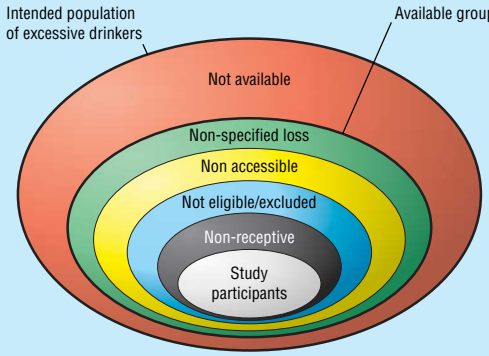
Assessing outcomes

Absolute risk reduction (in this case an increase in benefit) is the difference between proportions of individuals reporting to be drinking below weekly limits at follow up when groups were comparable at baseline. To compute the event rates we used an intention to treat approach with the total number of randomly assigned patients as the denominator.[21] Twelve months' follow up was the typical period, and we used it for comparison purpose in the meta-analysis. CIA software (Wilson method) was used for computing confidence intervals.[22]

Number needed to treat equals 1 divided by the absolute risk reduction. We gave confidence intervals as suggested by Rembold,[17] with negative values indicating harm and positive values indicating benefit. For example, an NNT of +20 means that an extra patient benefits for every 20 patients treated with the "new intervention."[22]

Box 2: Criteria for including studies

- Focus on excessive alcohol use (hazardous or harmful drinking) but not focus on a specific disease and not focus on alcohol dependency
- Recruitment involved screening or a procedure similar to screening
- Brief interventions were studied (minutes (not hours) of interaction) in general practice settings (not in hospital wards, not in emergency rooms, not in ad hoc research clinics)
- A randomised controlled design was used to compare outcome of a brief intervention with outcome of no/less intervention
- Studies were included for meta-analyses if they reported at least one discrete outcome measure reflecting a clinically significant change in alcohol consumption (an experimental event) and reported the number screened to obtain the study sample



- Not available for screening – refuse screening, treated elsewhere, not using health care, etc
- Non-specified loss – no reasons for loss given or reasons poorly differentiated
- Not accessible at some point between screening and follow up – moved away, died, had no access to telephone or no address, etc
- Not eligible or excluded by protocol, typically after a thorough assessment – false positive screening result, superimposed conditions, comorbidity, comedication, non-compliance, etc
- Non-receptive group – refuse further participation at some point between screening and follow up
- Study participants – admitted to study and followed up according to protocol

Fig 1 From intended population to admitted group

Screening effect—To describe the effect of screening we estimated the number of positive events per 1000 patients screened. This figure shows the proportion of patients who will benefit from the programme. The screening effect is estimated as the product of the prevalence of a treatable condition and the absolute risk reduction from treatment.[17] In our case, the prevalence is the number admitted for brief intervention divided by the number screened. These prevalences are kept fixed and not considered to deviate due to chance in our estimations since they are primarily determined by the screening and assessment procedure of the single study rather than by any stochastic variation.

Summarising effects across studies

We used the fixed effect Mantel-Haenszel pooling method and the matching heterogeneity statistic for combining results from the different studies.[23] To test the sensitivity of the pooling method we replicated the estimations using a random effects model. The pooled NNT value and corresponding 95% confidence interval were obtained by inverting the pooled risk difference value and its interval. The pooled screening effect value and its 95% confidence interval were established by multiplying the average weighted prevalence with the pooled absolute risk reduction and its confidence intervals.

Results

Inclusion and description of studies

Of 309 hits on Medline, 19 papers could be included, according for attrition mapping (table 1). PsycInfo (Silverplatter) contributed one additional paper.[24] One of the 20 papers reported long term follow up of a trial already included, and it was not included for further assessment.[25] We mapped the 19 studies according to the different types of attrition from screening to follow

Table 1 Participation and losses from positive screening to follow up in 19 general practice brief intervention studies on patients who screened positive for excessive alcohol use (arranged by size of the available group)

First author (year)	No of participants	No of non-receptive patients	No of patients excluded or not eligible	No of patients not accessible	No of patients lost for reasons not clearly specified
Wallace (1988)[38]	748	1761	1664	281	0
Fleming (1997)[41]	723	1096	63	190	853
Ockene (1999)[37]	481	41	945	293	0
Maisto (2001)[35]	250	1045	42	51	0
Richmond (1995)[40]	197	286	64	59	195
Manwell (2000)[36]	174	276	0	0	280
Senft (1997)[27]	414	20	41	84	102
Aalto (2001)[29]	202	362	0	0	94
Fleming (1999)[42]	146	272	232	6	0
Cordoba (1998)[26]	229	0	0	0	317
Anderson (1992)[39]	100	245	40	34	0
Aalto (2000)[28]	78	235	0	0	40
Scott (1990)[24]	50	195	93	14	0
Burge (1997)[30]	175	58	49	44	0
Seppa (1992)[31]	95	87	118	0	0
McIntosh (1997)[32]	143	106	0	16	0
Romelsjo (1989)[33]	72	33	151	1	1
Tomson (1998)[34]	72	27	120	3	0
Heather (1987)[18]	104	?	?	?	?

up and estimated the numbers for each type of attrition.

In general, a high percentage of patients who screened positive were excluded by protocol, refused further participation, or were not included for unspecified reasons during a secondary assessment carried out by a researcher and taking place before the randomisation. Typically, a few patients with severe alcohol problems or false positive screening results were excluded by protocol; the reasons commonly given were low compliance, comorbidity, or were not specified.

Inclusion in meta-analysis

Two of the 19 studies did not report the number screened to obtain subjects for randomisation.[18 26] Another nine (of which seven did not find any statistically significant differences on drinking outcome measures) failed to fulfil the fifth criteria by not reporting an event outcome measure and were not included for meta-analysis.[27–35] The meta-analysis included eight of the largest studies (table 2); one of these was a subgroup analysis that did not contribute to the pooled effect values.[36]

For screening, all eight studies used general health or lifestyle questionnaires that included questions on alcohol consumption. Questionnaires were provided to patients when they came to visit their doctor. Four studies also invited patients by mailing out questionnaires,[24 37–39] and one study telephoned patients.[37]

The interventions that were provided ranged from a 10 minute consultation[24 39] to up to five consultations lasting 5-20 minutes.[40] The intervention protocols all included feedback on present drinking, education on risk and strategies for changing drinking, and the practitioner's advice to cut down on drinking.

Methodological quality

Table 3 shows the four key components of study validity.[19 23] One study randomised four participating practices rather than patients[37] and another one used weekly shifts between intervention and control periods.[40] All studies found sufficient blinding impossible.

In general, follow up rates were high and the US studies all had follow up of 85% or more.[36 37 41 42] The TrEAT group studies[36 41 42] did not state their sample size considerations; their goal for consumption after treatment was more liberal than the criteria for excess drinking at screening, and follow up rates differed considerably between allocation groups in two of these studies (90% *v* 97%; $P=0.0002$[41] and 94% *v* 100%; $P=0.039$[36]; table 3). The large UK study by Wallace et al had a lower follow up rate for the male intervention group (83% *v* 89%; $P<0.05$).[38]

Heterogeneity

The studies varied slightly regarding inclusion criteria and baseline prevalence. However, the outcome results were not significantly different (test for heterogeneity $\chi^2=8.9$, $df=6$, $P=0.18$). As this P value is clearly above 0.10 we accepted the fixed effect approach as sufficient to produce an average measure of treatment effect and reliable confidence intervals.[19 43] We used a random effects model (RevMan software[44]) to investigate the robustness of the fixed effect approach.

Intervention effect and assessment efforts

The pooled absolute risk reduction was 10.5% (95% confidence interval 7.1% to 13.9%) (table 4). A random effects model yielded a similar result: 10% (6% to 14%).

The pooled number needed to treat (NNT) was 10 (7 to 14). NNTs of single studies ranged from 5 to 61 and all results favoured intervention to some degree (table 4). Two studies had notably higher NNTs,[24 37] and the 95% confidence intervals of five studies include the possibility of harm.[24 36 37 41 42]

All NNT values have to be interpreted in the light of the screening and assessment activity that took place to establish the trial sample as well as the character of the outcome in question. Nine per cent of patients (12 327/134 693; range in individual studies, 3.3% to 18%) screened positive; further assessment identified 2.5% (3317/143 693; range 0.9% to 5.4%) who were given brief interventions.

Table 2 Characteristics and results of studies of screening and brief intervention in general practice for excess alcohol consumption included for meta-analysis

Study (country; participants)	Screening method and when administered	Definition of excessive drinking*	No screened; screened positive (%); randomised (%)	Mean (SD) drinks per week at baseline	Intervention	Treatment goal (maximum drinks*/week)	Follow up period	Difference between groups for main outcomes as reported in the paper
Manwell (USA; women aged 18-40)[36]	Lifestyle questionnaire administered during consultation	>11/week or ≥5/occasion; 2 positive answers to CAGE	5979; 730 (12); 205 (3.4)	14 (9)	Physician advice (15 min)×2; telephone booster×2; self help material	13	4 years	Change in No of drinks/week: −1.7 (P=0.0039)† ARR for binge drinking: 3.4% (P=0.0021)† ARR for drinking above limits: 11% (P=0.05)†
Scott (UK; women aged 17-69)[24]	Health questionnaire administered during consultation; some patients invited for first screening by mail or telephone	>17/week	11521; 384 (3.3); 104§ (0.9)	30 (10)	Physician advice (10 min)×1; self help material	12	1 year	Change in No of drinks/week: −1.1 (NS) ARR for binge drinking: 1.6% (NS) ARR for drinking above limits: 3.3% (NS)
Fleming (USA; men and women aged over 65)[42]	Health questionnaire administered during consultation	>11/week or ≥4/occasion (men), >8/week or ≥4/occasion (women); or 2 positive answers to CAGE	6073; 656 (11); 158 (2.6)	16 (9)	Physician advice (10-15 min)×2; telephone booster ×2; self help material	20 (men), 13 (women)	1 year	Change in No of drinks/week: −5.3 (P<0.001)‡ ARR for binge drinking: 59% (P<0.005)‡ ARR for drinking above limits: 63% (P<0.005)‡
Ockene (USA; men and women aged 21-84)[37]	Health questionnaire administered during consultation; some patients invited for first screening by mail or telephone	>12/week or ≥5/occasion (men), >9/week (women); or ≥4/occasion or 2 positive answers to CAGE	9772; 1760 (18); 530 (5.4)	20 (15) men; 13 (7) women	Physician or nurse advice (5-10 min)×1; self help material	12 (men), 9 (women)	0.5 year	Change in No of drinks/week: −2.4 (P=0.001) ARR for binge drinking: 5.0% (P=0.32) ARR for drinking above limits: 20% (P=0.01)
Fleming (USA; men and women aged 18-65)[41]	Health questionnaire administered during consultation	>14/week (men), >11/week (women)	17695; 2925 (17); 774 (4.4)	22 (13) men; 15 (10) women	Physician advice (15 min)×2; telephone booster ×2; self help material	20 (men), 13 (women)	1 year	Change in No of drinks/week: −3.5 (P<0.01)‡ ARR for binge drinking: 17% (P<0.02)‡ ARR for drinking above limits: 30% (P<0001)‡
Richmond (Australia; men and women aged 18-70)[40]	Health questionnaire administered during consultation	>29/week (men), >17/week (women)	13017; 894 (6.9); 378§ (2.9)	42 (24) men; 22 (14) women	Physician advice (5-20 min)×1-5; self help material	23 (men), 12 (women)	1 year	Change in consumption: −0.3 (NS) Temporary effect on problem score
Wallace (UK; men and women aged 17-69)[38]	Health questionnaire administered during consultation; some patients invited for first screening by mail	>22/week (men), >13/week (women)	62153; 4454 (7.2); 909 (1.5)	42 (21) men; 24 (12) women	Physician advice (few min)×1-5; self help material	22 (men), 13 (women)	1 year	Change in No of drinks/week: men −6.7 (P<0.001); women −3.5 (P<0.05) ARR for drinking above limits: men 18% (P<0.001); women 18% (P<0.05)
Anderson (UK; men aged 17-69)[39]	Health questionnaire administered during consultation; some patients invited for first screening by mail	>29/week	8483; 524 (6.2); 259§ (3.1)	44 (11)	Physician advice (10 min)×1; self help material	18	1 year	Change in No of drinks/week −5.4 (P<0.06) ARR for binge drinking: 30% (P<0.05) ARR for drinking above limits: 13% (P<0.05)

ARR=absolute risk reduction; NS=not significant; CAGE is a four questions screening test (Cut-down contemplations, Annoyed by criticism, Guilt after drinking, and use of Eye-opener to steady nerves).
*Number of standard drinks (12 g alcohol) per week or per occasion.
†P values based on repeated measures test.
§More than two arms in study.
‡P values apply to tests for difference in means or homogeneity of proportions.

American and British studies defined sensible drinking differently. The US studies included people who drank less than in the UK studies (table 2). The TrEAT group studies had intervention goals for sensible drinking that were more liberal than the ones they used in their inclusion criteria,[36 41 42] whereas the studies from Britain and Australia all had goals for sensible drinking that were more strict or equal to their inclusion criteria limits.

Giving up binge drinking is another possible outcome of intervention. Table 5 shows the screening effect values for this event in the six studies that reported this outcome. However, a strong interdependence between reduced drinking overall and lack of binge drinking is likely, so that giving up excessive weekly consumption also "cures" binge drinking. Only one of the eight studies reported a combined outcome of "safe weekly drinking and non-binge drinking."[37] In that study giving up excessive drinking reduced binge drinking while the number of "pure" binge drinkers remained unchanged in both groups.

Screening effectiveness and programme outcome

The pooled screening effect was 2.6 (1.7 to 3.4) patients per 1000 screened for achieving sensible drinking (based on the weighted average of admission to brief

Table 3 Quality of studies included in meta-analysis

First author	Sample size	Validity assessment criteria: Randomisation (selection bias)	Blinding (performance bias)	Follow up (attrition bias)	Sample size calculations (detection bias)
Manwell[36]	205	++	–	+ (97%; I<C)	–
Scott[24]	104	++	+ (research staff)	+ (69%; I=C)	++
Fleming[42]	158	+	–	++ (92%; I=C)	–
Ockene[37]	530	– (4 practices)	+ (research staff)	++ (91%; I=C)	+
Fleming[41]	774	++	–	+ (93%; I<C)	–
Richmond[40]	378	– (weekly shifts)	+ (research staff)	+ (69%; I=C)	++
Wallace[38]	909	+	+ (research staff)	– (82%; I<C)	++
Anderson[39]	192	++	+ (research staff)	+ (65%; I=C)	++

Selection bias: Randomisation quality and description of randomisation (– not justified, + fully described or valid procedure, ++ described and valid procedure)
Performance bias: Blinding and description of blinding; includes between groups detection bias (– no blinding, + single blinded, ++ double blinded)
Attrition bias: Follow up rate and differences between intervention group (I) and controls (C): (– <85% and different rates, + ≥85% or no difference, ++ ≥85% and no difference)
Detection bias (regarding multiple outcome measures): Sample size calculated and justified (– not justified, + partially justified, ++ fully justified)

Table 4 Outcomes in studies included in meta-analysis. Values in parentheses are 95% confidence intervals

Study	Intervention group (EER)*	Control group (CER)*	Absolute risk reduction (%) (EER–CER)	Number needed to treat (1÷ARR)	Proportion (%) of screened patients given intervention†	Screening effect‡
Manwell[36]	83/103	71/102	11.0 (–0.9 to 22.5)	9 (4 to –113)	3.43	3.8 (–0.3 to 7.7)
Scott[24]	9/33	10/39	1.6 (–18.1 to 21.9)	61 (5 to –6)	0.90	0.1 (–1.6 to 1.9)
Fleming[42]	66/87	44/71	13.9 (–0.5 to 27.9)	7 (4 to –195)	2.60	3.6 (–0.1 to 7.3)
Ockene§[37]	102/274	66/256	11.4 (3.5 to 19.1)	9 (5 to 28)	5.42	6.2 (1.9 to 10.3)
Fleming[41]	277/392	247/382	6.0 (–0.6 to 12.5)	17 (8 to –171)	4.37	2.6 (–0.3 to 5.5)
Richmond[40]	16/96¶	13/93¶	2.7 (–7.8 to 13.1)	37 (8 to –13)	2.90	0.8 (–2.3 to 3.8)
Wallace[38]	116/448	48/459	15.4 (10.5 to 20.4)	6 (5 to 10)	1.46	2.2 (1.5 to 3.0)
Anderson[39]	14/80	4/74	12.1 (1.8 to 22.4)	8 (4 to 54)	3.05	3.7 (0.5 to 6.8)
Subtotal (pooled estimate)	600/1410	432/1374	10.5 (7.1 to 13.9)	10 (7 to 14)	2.46	2.6 (1.7 to 3.4)

Test for heterogeneity: χ^2=8.9, df=6, P=0.18; test for overall effect: z=6.03, df=1, P<0.0001.
*Proportions of sensible drinkers at follow up.
†Number randomised÷number screened.
‡Screening effect=ARR×(% given intervention)/1000.
§This trial had 6 months' follow up.
¶Calculated from percentages given in text (not found in tables of article).

intervention of 2.46%). In single studies, values for screening effects varied from 0.1 to 6.2 patients per 1000 screened (table 4).

The average pooled screening effect was sensitive to differences in the prevalence of participants admitted to receive brief intervention. This prevalence varied between 0.9% and 5.4% in the studies we included. With an average risk reduction of 10.5% (7.1% to 13.9%) the corresponding screening effect would be 0.9 (0.6 to 1.3) patients per 1000 screened for the prevalence of 0.9% and 5.7 (3.8 to 7.5) patients per 1000 screened for the prevalence of 5.4%.

Figure 2 shows the results of the single studies and the pooled estimate. All results favour screening and intervention to some degree, the results seem to be quite homogeneous, and the pooled estimate is clearly significant.

Discussion

If a practitioner screens 1000 patients, carries out further assessment in 90 patients (9%) who screen positive, and gives feedback, information, and advice to 25 (2.5%) who qualify for brief intervention, two or three patients can be expected to have reduced their alcohol consumption to below recommended maximum levels after 12 months.

Methodological considerations

We discovered several sources of bias, all tending towards overestimation of effect: impossibility of blinding patients and practitioners, combined with self reported outcome measures[45]; three papers did not report their sample size calculations and redefined excessive drinking at follow up[36 41 42]; one paper, which reporting better results than the others, had a shorter follow up[37]; and the two largest studies may have been affected by attrition bias.[38 41] Self selection (through mailed and telephone invitations) in some studies[24 37–39] may have compromised their external validity.

We assumed that the reasons for exclusion and dropout after a positive result on screening in the studies were similar to the reasons for the practitioner or the patient choosing to undergo no further assessment or intervention—these being previous attempts to give advice, non-compliance with advice, refusal to attend for intervention, or a false positive screening result. In real life, screening could have a side benefit, identifying some cases of alcohol dependency not known to the doctor; some of these patients might be willing to be referred for treatment. That screening itself is

Table 5 Screening effect (per 1000 screened) for non-binge drinking (occasional excessive drinking) in studies that included this outcome measure

First author	Maximum No of drinks per occasion	Number needed to treat (95% CI)	Screening effect (95% CI)
Manwell[36]	4 (women)	7 (4 to 29)	5.3 (1.2 to 9.2)
Scott[24]	11 (women)	7 (–13 to 3)	1.3 (–0.7 to 3.2)
Fleming[42]	4 (men); 3 (women)	7 (–75 to 3)	0.7 (–0.3 to 7.5)
Ockene[30]	5 (men); 3 (women)	21 (–31 to 8)	2.6 (–1.7 to 7.0)
Fleming[41]	5 (men); 3 (women)	10 (6 to 26)	4.6 (1.7 to 7.4)
Anderson[39]	5 (men)	4 (3 to 10)	7.5 (2.9 to 11.6)

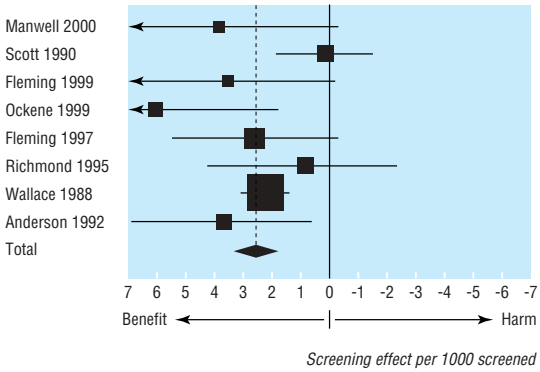


Fig 2 Effect of screening for excessive drinking: number of patients with a positive outcome (reduction in drinking to below maximum recommended limits) per 1000 screened

beneficial is an assumption that remains untried, and counterproductive effects from alienation and badly timed screening have to be considered.

Pooled numbers needed to treat derived from meta-analyses can be misleading.[46] For NNTs to be comparable, they must define patients' condition and its severity and the intervention, its outcome, and the setting, which they did in the present analysis. Nevertheless cultural, age, and sex differences should be taken into consideration, and single studies might contribute valuable information that should not be eclipsed by pooled estimates. Although the study by Anderson and the TrEAT trial both attempted to exclude people who were highly dependent on alcohol, the nature of the "events" might differ between men who drink heavily (included if >29 drinks; mean consumption 44 drinks, goal 18; screening effect 3.7 per 1000)[39] and less heavy drinkers (included if >14 drinks; mean 22, goal 20; screening effect 2.6 per 1000).[41] Positive net benefit is still possible when the screening effect is 2.6 per 1000 screened. The TrEAT study group has published an economic analysis showing a benefit-cost ratio of 5.6 (0.4 to 11.0).[47] Aside from methodological problems and the crudeness of such a measure, one crucial problem remains. Screening for excessive drinking is in keen competition with other proposals for screening and prevention. A recent paper points out that a doctor in primary care needs 7.4 hours per working day to provide the preventive services recommended by US Preventive Services Task Force.[48] Family doctors would have to give up other activities to free resources for a programme that would result in safer drinking habits for only a handful of their patients.

Drinking and general practice

The literature on brief intervention documents that a patient who is drinking excessively may reduce their alcohol consumption once a practitioner speaks to them. But if change occurs in only two or three of 90 patients who screened positive and were assessed further, the practitioner who screened 1000 patients to find those 90 could perceive this as a 97% disappointment rate.

Conversations about drinking may take place in many ways in general practice. Sensitively raising the subject, or facilitating patients' initiatives, may be a key characteristic of good clinical practice. Future research might focus on how a well established helping relationship can cover drinking related problems and risks in a way that benefits the patient and appeals to the practitioner.

The Medical Research Council suggests a stepwise framework for developing and evaluating randomised controlled trials for complex interventions to improve health.[49] This approach includes modelling and exploratory research phases defining constant and variable elements of interventions, as well as evaluating the effectiveness of programmes in actual practice.

In the meantime, we propose a focus on the fact that information and advice is sometimes helpful, especially when rapport has been established and the agenda agreed on. Assessment of drinking, not only in groups with obvious alcohol related problems but also in groups of patients with symptoms correlated with high consumption (hypertension, dyspepsia, depression, injuries, social problems, etc), lies within the doctor's role. To help patients change their lifestyle, practitioners need and want supplementary training. These processes of improving good clinical practice could also be considered complex interventions, and trials could be developed accordingly.

Conclusions

Although even brief advice can make a difference, this review calls into question the model of universal screening in general practice as a case finding approach. Alcohol screening, assessment, and intervention are laborious and time consuming activities that only two or three people out of 1000 screened will benefit from. A practitioner who experiences such a low ratio of success to workload is bound to be disappointed and reluctant to engage any further even though the screening approach is consistent and utilitarian by nature and may seem beneficial from a population perspective.

Future research should focus on other ways than systematic screening of addressing excessive drinking

What is already known on this topic

Even a few minutes of feedback, information, and advice by a general practitioner can make some excessive drinkers change to alcohol consumption within sensible consumption limits

General practitioners are strongly encouraged to screen their patients and intervene with those whose alcohol consumption is either hazardous or harmful to their health

What this study adds

The internal and external validity of trials of screening based brief interventions is questionable

Only one in four patients who screen positive for excessive drinking qualify for brief intervention after further assessment

Only two to three patients per thousand screened will benefit from the laborious activities entailed in screening

among patients in general practice. More attention should be paid to the preconditions of and skills for successful interviewing, exchange of information, advice giving, and counselling.

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