

LETTERS TO THE EDITOR

Should the Population be Offered General Health Screenings?

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Should the population be offered general health screenings? In the Ebeltoft study, Dr Engberg and associates ("General health screenings to improve cardiovascular risk profiles: a randomized controlled trial in general practice with 5-year follow-up," *J Fam Pract* 2002; 51:546-552) answered "yes" to this question and assigned general practitioners a key role in this offer. Their conclusion was based on the demonstration of an average difference of 0.6 kg/m² in body mass index and 0.12 mmol/L in serum cholesterol concentration at a 5-year follow-up. These differences are of the same magnitude as those found in other similar intervention studies.¹⁻³ A Cochrane review concluded that the costs of such interventions are high and that these resources may be better used in persons at high risk of cardiovascular disease in whom evidence of effectiveness is much stronger.¹ Before suggesting that Engberg and colleagues draw more cautious conclusions from their own results, we would ask the authors for some important information:

1. A baseline examination of the control group was omitted in their study. Although good arguments could be made for this choice, the omission makes it difficult to control for imbalance between the 3 groups. However, some baseline information is available on all 1507 participants, and more extensive information about 1370 of these individuals.⁴ Because the results can be completely or partially due to an imbalance, we would like the researchers to document the randomization with more of the baseline information that was not part of the randomization procedure. A possible imbalance does not need to be statistically significant to have an effect on the small observed differences.⁴
2. The most serious potential source of bias in the Ebeltoft study is the qualitative aspects of dropout (ie, the characteristics of those who dropped out), particularly in the intervention group. This dropout can probably explain a large part of the differences between the control and intervention groups.^{2,3} In the Family Heart Study, the proportion of smokers at baseline, for example, was twice as large among the persons who did not attend the final health screening as among those who finished the project, and they were 2 kg heavier. These important analyses from Engberg's intervention group have not been presented; also missing is a summary of reasons for dropout (death, move, exclusion, nonappearance).
3. The main result of the Ebeltoft study is reported using a modified cardiovascular risk score, but have the validity and clinical relevance of this score been investigated? The value of the scale as a measure of effect seems doubtful, because as many as 9 points can be allocated based on circumstances that cannot be affected at all by intervention, namely sex and genetic disposition. The observed difference in score and prevalence proportions can thus be due, for example, to a different gender composition among the patients dropping out of the study in the various groups. We would therefore suggest that Engberg and colleagues present a comparison of the score among the 3 groups, explaining the contribution of the single factors to the total score.
4. The planned health discussions did not seem to increase the effect of the health screening, which is both disappointing and surprising. If this is a case of a spillover effect from the intervention among the 2 intervention groups, it is surprising that this spillover effect did not also occur through the physicians to the control group. In any case, it is impossible to judge a randomized trial with 2 intervention groups without an account of the primary effect measures in each of these 2 groups.

The Ebeltoft study is an important family practice research project. However, the lack of baseline information on the control group demands a much more conservative interpretation than that presented by the researchers. At present, we do not think that the results of the project provide sufficient evidence that general practitioners should use their own resources or those of their staff on population-based screening for risk factors for ischemic cardiovascular disease.

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■ DRS ENGBERG, CHRISTENSEN, KARLSMOSE, LOUS, AND LAURITZEN RESPOND:

We thank Dr Olivarius and colleagues for commenting on our report after a thorough and critical reading. It is, however, a mistake stating that in the paper we answer “yes” to the question “Should the population be offered general health screenings?” We described solely the effects of the intervention; it is Olivarius and colleagues who answer yes to the question after having read the results.

It is correct that the mean effect on body mass index and serum cholesterol is small. The potential variation does not, however, refer to a scale from zero to infinite, nor is the variation irrespective of where on the scale and for who and how many people the reduction is observed. Evaluating only mean values is insufficient and not clinically relevant. The prevalence proportion of persons with an elevated risk of coronary heart disease after 5 years’ follow-up is thus presented for an elaboration of the results, showing that the number of persons with an elevated or high cardiovascular risk is significantly lower in the intervention groups.

Even though the Cochrane review to which Olivarius refers contains few randomized examinations of “health promotion” for persons with a low risk of cardiovascular disease, we agree that interventions for these groups are not as effective as in groups at higher risk. Nevertheless, finding persons to whom a more cost-effective intervention can be offered is necessary. This imperative is mentioned in our report. Regarding Olivarius’ 4 comments:

1. It is an illusion to think that an imbalance between treatment groups can be rejected by comparing different factors between them. There is always a risk for imbalance that may lead to confounding, if not on known factors then on unknown factors. The randomized experimental design used in the Ebeltoft project is the strongest instrument to avoid such an imbalance. But it is a mistake to believe that the groups become identical by a randomization procedure.⁵ Most reviewers and readers do, however, feel more comfortable if no imbalance is apparent for the most basic factors such as sex, age, etc. The report thus presented a few such factors and no imbalance was found between the groups.
2. It is a well-known fact that unhealthy and vulnerable persons are more likely to drop out from a study than are healthy individuals. It is therefore not possible to carry out a follow-up study and avoid the described dropout problem. This pitfall is one of the reasons why evaluating results from nonrandomized studies is difficult. In our study the groups were compared in accordance to the intent-to-treat rule to adjust partly for this problem, but dropout can never be totally avoided in a clinical setting. In a randomized study the biggest problem arises if the dropout is differentiated, meaning if the more unhealthy individuals drop out from one group but not from other groups. In the Ebeltoft project the attendance rate between the control group and the intervention group was not different after 5 years regarding sex, age, baseline smokers, and baseline body mass index.
3. We used a cardiovascular risk score developed by Anggard and associates⁶ based on sex, familial inheritance, tobacco use, blood pressure, serum cholesterol (total) concentration, and body mass index, which also figures in other cardiovascular risk scores and which are commonly accepted factors in the estimation of future risk of heart diseases. As any score is difficult to evaluate, we also presented the relevant clinical outcome measures. The mean value of the cardiovascular risk score is 5.50 (SD 3.18) in the control group and 4.98 (SD 2.80) in the intervention group ($P < .01$) if sex and inheritance are omitted; if smoking is also left out the scores are 4.33 (2.60) and 3.91 (2.34), respectively ($P < .01$).
4. Our study shows that a health screening is suitable to reduce the population’s cardiovascular risk profile, with

or without a planned discussion with the general practitioner. All baseline participants who were offered a health screening and who were warned of an elevated risk of coronary heart disease were, for ethical reasons, encouraged to see their general practitioner even if they belonged to the group that was not automatically offered a health discussion.

Although these consultations probably were not as extensive and detailed as those offered as part of the study, they may confound the difference in the degree of intervention. But it cannot be concluded that discussions with the general practitioner have no effect. The possibility of having a health discussion must exist, but planned health discussions do not add further to the effect. Likewise, the control group participants may have changed their behavior and consulted their general practitioner more often than they had previously. Such behavioral changes and other conditions mentioned in the report indicate that the detected effect is an underestimation of the real effect.

The goal of our study was to focus on the general effect of the intervention. We found that health screenings can improve the cardiovascular risk profile in the general population. We did not consider whether health screenings "should be" offered to the general population, an arena that delves into organizational, political, and economic questions.

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