Response to Comment on "Screening for Abdominal Aortic Aneurysm and Overall Mortality in Men"

Dr. Koelemay must be acknowledged for his careful review of the meta-analysis, which has revealed a citation error in the manuscript, for which we apologise. The data used in the original and revised long-term meta-analyses are not from the preliminary 10-year report from the Viborg study, but from the complete 7-year report, as for the original and revised long-term meta-analyses are not available instead of after 15 and 12 years, respectively.

For interpretation, one has to remember the effect of screening and the data used in the calculation of odds ratios. The effect is a delay of death but in the end, we are all going to die. Thus, the most relevant statistics to use are survival analyses. However, this would require merging of results from all the randomised trials. This has been attempted but without success. Consequently, we can only use meta-analysis to address this question. However, calculation of the pooled odds ratio is based upon the number of deaths in the invited group versus the control group and ultimately the numbers will be equal in the two groups, and the OR will become 1.00. The modified calculation seems just to be a manifestation of that progression.

The existing meta-analysis still shows that screening reduces overall mortality, and the risk association would probably have been even stronger if seven-year results from the Chichester and Australian Studies had been available instead of after 15 and 12 years, respectively.

References


publish in order to avoid redundancy. Manuscript reviewers should also be vigilant about this. In 2006 Dr. Murie, Editor-in-Chief of the British Journal of Surgery, addressed a similar dilemma before,\(^4\) to emphasize the responsibilities of authors, reviewers, and editors regarding this matter.

References


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Response to Letter to the Editor

Sir,

Ubbink et al. have published a systematic review on negative pressure wound therapy in the British Journal of Surgery in the beginning of 2008 along with a double publication in the Cochrane Library. They consider that our paper later in 2008 in the European Journal of Vascular Surgery poses a double publication of their work as the original trials are very much the same.

We consider the points raised by Ubbink et al. unjustified, unscientific and unethical. Objectivity and repeatability of the findings is a fundamental principle in science and this principle encompasses both original studies and systematic reviews. We found ten previous systematic reviews published on negative pressure wound therapy, the review by Ubbink et al. being one of those reviews. We have referred to these publications in our paper. The need for new reviews is based not only on repeatability of findings but on different study questions in the previous reviews and in other differences in the review protocols.

The suggestion of a double publication is entirely groundless. We did plan our study protocol already in 2006 and our data for the meta-analysis was collected by an informatician at the Finnish Office for Health Technology Assessment in July 2006 and the search was revisited in January 2008 as stated in our materials and methods. Our first submission to EJVES was dated in March 31st, 2008 and our final revision was submitted in June 3rd, 2008. Thus our paper was entirely independent of the work by Ubbink et al. We find it extraordinary that Ubbink et al. consider that we should not have had the right to publish our paper. We think that this is unjustified and violates the principles of scientific publication.\(^1\)

Redundant or duplicate publication is considered when the same data is used to generate several publications and most often when the same author or group reports their data in different papers. Ubbink et al. refer to an editorial by Murie et al., in which a case of redundant publication on a series of laparoscopic cholecystectomy complications is discussed. In this case the two original publications included overlapping cases and the authors discussed this being due to changes during the peer-review process. This discussion should not be confused with the current one where there were two entirely separate scientific publications.

The efficacy, effectiveness and safety of negative pressure wound therapy is and continues to be of high relevance as the technology diffuses rapidly into the health care without sufficient evidence of its effectiveness, but a high level of economical interests. Further trials have been and will be published on negative pressure wound therapy in the coming years and we hope that the body of evidence will grow.

We look forward to new systematic reviews on this important topic.

Reference

\(^1\) International Committee of Medical Journal Editors. Uniform Requirements for Manuscripts Submitted to Biomedical Journals: Writing and Editing for Biomedical Publication, [http://www.icmje.org; October 2008.](http://www.icmje.org)

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