Reviewer's report

Title: Physicians' Ability to Predict the Risk of Coronary Heart Disease

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Reviewer: Richard J McManus

Level of interest: not specified

Advice on publication: Other (see below)

Thank you for the opportunity to review this paper which I previously reviewed in a similar form for a different journal. I recommended its publication then and hope that it will now be published, particularly given the fact that the work was started in 1998. It describes the results from a survey of residents, fellows, attending physicians at three university sites asking them to attribute CHD risk and treatment effect in 12 primary prevention scenarios as well as give a treatment recommendation (or not).

Are the questions posed new and well defined?
The risk estimation aspect of the study using scenarios has been done else where but with fewer case vignettes. This work therefore builds on that done previously. The part of the study dealing with estimation of treatment effects is original and important.

Methods
The very nature of the work (theoretical case scenarios rather than observed practice) leads to limitations, but the authors clearly acknowledge these and have done the work in an appropriate manner.

Data
The data appear sound and well controlled. The results and table contain relevant information.

Reporting
The paper reports the relevant data appropriately. Table 1: column 3 title has an asterisk: is a footnote missing?

Discussion and conclusions
1. The discussion is sound. Three minor comments:
   No reference has been made in the introduction or discussion to Mongomery et als paper which found that clinicians under estimate risk in a study of real hypertensive patients.(1) Do the authors think that risk estimation in real patients might be systematically different to that in scenarios?

2. The discussion might raise the issue of observed under treatment of patients (in terms of blood
pressure, lipid lowering therapy etc) despite evidence of over estimation of risk. This may be more the case in the UK rather than the US.

3. The authors justify their use of 5 year risk but do not discuss whether or not they think its choice over and above 10 year risk may have influenced the results. Might this have confused respondents are 10 year risks more commonly used in the US? (they certainly are in the UK). For example recent US guidelines use 10 year CHD risk.(2) This might be important as if physicians are more familiar with 10 year risks then they may mistakenly "thinking" in10 year risks even though a 5 year risk is required.

Title, abstract and writing
These are all acceptable.

Revisions
All my comments fall into the "discretionary" category other than the asterisk in table one (!).

References:


Competing interests:
None declared.