Reviewer's report

Title: Validation of a new predictive risk model: measuring the impact of the major modifiable risks of death for patients and populations

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Reviewer: David Leon

Reviewer's report:

1. Is the question posed by the authors new and well defined?
This paper is derivative of the huge amount of work the lead author and his co-authors put into the GBD 2010 exercise. Building on the GBD’s identification of a set of “risk factors” they apply the estimated effect measures to the population risk factor distribution for the USA (from NHANES). The stated purpose is to develop a risk score for total mortality that could be used in clinical practice to help motivate patients to modify their life style. However, this appears somewhat contrived. I find it implausible that in a clinical setting a doctor would be comfortable to use a risk algorithm for TOTAL mortality, where the whole of the doctor’s training is towards specificity of diagnosis and treatment. The paper therefore comes across mainly as a demonstration that applying GBD risk factors to the US population predicts mortality and it provides an order of magnitude estimate of the improvement in life expectancy that might be expected if risk factor distributions were optimal.

2. Are the methods appropriate and well described, and are sufficient details provided to replicate the work?
At a technical, statistical level the methods are appropriate and are outlined in sufficient detail so that others with access to equivalent data could replicate them. The one thing that is missing (or I could not find) is the size of the RRs associated with each risk factor. This is rather crucial, and without it, others could not replicate the results, or apply them to other populations.

3. Are the data sound and well controlled?
I am prepared to take as correct the NHANES data etc., even though it goes without saying that it is unlikely to be fully representative of the target population (ie the total population of the USA). It is the choice of risk factors that are the most problematic aspect of the paper, particularly given the way in which the authors have framed their work as being a useful tool for encouraging lifestyle modification. The risk factors go from the fully established and undoubtedly causal (the first 7 of Table 1), through physical activity the magnitude of hose effect and how to quantify it is highly contentious, to four dietary elements: fruit intake, vegetable intake, omega-3 and nut intake. The dietary elements are fraught with difficulty. In particular, omega-3 and nuts are really weak. A new meta-analysis of omega-3 supplementation trials has just been published (see Zhang et al. BMC Public Health 2014, 14:204) which fails to find any effect on
total mortality. The nut story is also highly provisional, and there is no trial evidence – the basis for inclusion of this risk factor being almost entirely observational. This is not a good starting point for advising people to MODIFY their diet – where the causal basis of such modification is so uncertain. This also applies to fruit and vegetables, although one could argue that it is “prudent” to encourage a balanced diet. But here we are now moving away from this much more data driven, empirical approach.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
Yes – although see point 2. re : RRs not being reported

5. Are the discussion and conclusions well balanced and adequately supported by the data?
If one is uncritical about the causal provenance (direct and indirect) of the risk factors, then the conclusions are supported, although it is surprising that the authors do not give a much stronger health warning about the fact that their conclusions (gains in life expectancy etc.) are on assumption of associations being causal. However, an uncritical eye here is not really acceptable – especially because of the proposed use of the model to encourage life style modification.

On a slightly different point, I found the emphasis on individual lifestyle modification to be unfortunate, and not adequately counter-balanced by the final paragraph of the paper which does mention structural or upstream determinants of risk factors. The truth is that legislative approaches to control of soft drinks, alcohol and tobacco are essential aspects of improving the health of populations. Largely ignoring this flies in the face of the abundant trial evidence that individual-level lifestyle modification attempts in the workplace or in clinical settings are largely ineffective.

6. Do the title and abstract accurately convey what has been found?
Yes

7. Is the writing acceptable?
Yes

Major Compulsory Revisions
I regard the key points I made above as compulsory for the author's to deal with.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I declare that I have no competing interests