Reviewers report

Title: Towards new recommendations to reduce the burden of alcohol-induced hypertension in the European Union

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Reviewer: Tony Blakely

Reviewer's report:

Re: Towards new recommendations to reduce the burden of alcohol-induced hypertension in the European Union

This is an interesting paper, merging all of consensus, guidelines and modelling approaches to public health policy.

In the Introduction the authors talk about 'best buys' recommended by WHO: "taxation increases, restrictions of availability and a ban on marketing for alcohol use as a risk factor". The paper, though, is 'just' on "screening and brief intervention for hypertension and harmful alcohol use in primary health care". This is a good objective. But the reality is, for the rational policy maker, that the 'best buys' recommended by WHO are (vastly) superior in both health gain and cost effectiveness than anything done in primary care. For example, see this Australian analysis that shows a massive difference in health gains for policies ranging from prevention to high risk to reduce alcohol-related harm.(Cobiac, Vos et al. 2009) and sections within (Vos, Carter et al. 2010) Whilst - of course - analyses of primary care and screening interventions alone are useful, they need to be interpreted against 'what else could be done'. I am sure that the 'what else could be done' in Europe, just as in Australia, would see a much greater (and more cost-effective) impact for what WHO call 'good buys'. Not only does this broaden the scope of what should be done next, but (again, for the rational decision maker) suggests primary care interventions should only be considered once these more fundamental interventions are done first..... generating an intervention pathway or optimal sequence. It is possible to run a counter argument that this current study only considers primary care interventions, or only those interventions that are politically feasible or achievable by the target readership (i.e. guideline audience) - and this is a valid argument to some extent. But for this type of consensus paper, I believe the results and options should be carefully and explicitly contextualized within the full range of options.

p.17. The authors state: "However, in view of the significant evidence for the health harms of heavy drinking and hypertension, combined with the potential gains of the proposed interventions, Europe should not wait for the results of these studies before moving forward with implementation of these recommendations. The precautionary principle, as indicated by the WHO European Region, implies a responsibility to protect the public from exposure to harm where there is a scientifically plausible risk [70, 71]."
This is an unacceptable policy recommendation. Why? Because actual health gain compared to other possible health interventions, and opportunity costs, and cost effectiveness are not considered [see SPECIFIC comments below, and regarding Appendix].

First, as above (and below w.r.t. Appx) the CRA analysis is good, but does not actually tell us the healthy life years gained (or QALYs gained, or DALYs avoided). To do this requires a simulation model that does allow for morbidity (a person with cirrhosis has a lower quality of life, and high competing mortality and morbidity risk).

Second, there is no mention of cost anywhere in the paper. Diverting primary care time to alcohol screening will have an opportunity cost; something else is not done, or a new cost to the State and taxpayer is imposed. A (rational) policy maker needs to know what the cost effectiveness of this package of interventions might be.

Thus I suggest the authors' recommendations are over-reach, and should be framed in terms of:

a. These interventions are likely cost effective [I suspect they will be], but need to be assessed by way of:

a. Simulation models now given what we know (see Australian study for example)

b. Cost-effectiveness models attached to the proposed RCTs.

I understand why the authors invoke the precautionary principle, but I think it is a slight misuse. A better framing, perhaps, might be that they recommend these policies are implemented, but carefully evaluated as implemented (especially for cost effectiveness) so that they can be fine-tuned or abandoned as necessary. That is, policy-generating-evidence is probably acceptable here - but it must be framed that way.

SPECIFIC COMMENTS:

p.5, lines 12-22: "There are a number of European studies planned or being conducted to examine feasibility and/or effectiveness of these recommendations. However, given large deficits in dealing with both conditions and the existing evidence base for the recommendations, efforts to foster routine screenings and other recommendations should start immediately." The last sentence of the abstract seems premature, unless the interventions are feasible, effective and cost effective; the Abstract does not make this clear enough to the reader to have the second to last and last sentence juxtaposed as it is now.
p.8. It might be useful to give PAR% or population impact fractions from the GBD on the theoretically possible reduction DALYs if alcohol shifted to TMREL in European countries - so long as DALYs expressed in some meaningful unit (e.g. per capita, or as % of all DALYs lost in Europe, etc). It would also provide context for the authors' CRA.

p.8 line 57 to top of p.9. But what is prevalence of AUD in non-hypertensive? (Which relates to above suggestion of PAR%.)

p.9 line 16. What was comparator group for OR? Presumably just a dichotomous split AUD vs non-AUD?

p. 9, line 46. "Over five weeks of measurement during 2013-2014, out of about 900,000 adult consultations (about 1200 consultations with 746 providers), only 1.4% of adult consultations resulted in patients being screened and given advice for their heavy drinking [41, 42]." I am not sure I agree. Given the huge diversity of presentations to primary care, that each person has multiple consultations with a GP (or other primary care provider) over time (i.e. more than this cross-sectional slice), 1.4% may not be bad going.

Table 1 footnotes. Was it greater than 140mmHg systolic to be HT, or both 140 systolic and 90 diastolic?

p.10 to p.11. The table captions say 40-64 year olds, but I think the reader needs reminding in the text too.

Table 3. [See comments below on method in Appx.] CRA methods will overestimate the deaths averted. This is not too severe for CVD (as timelags short), but is more of a problem for liver cirrhosis. Yes, one sees pretty rapid changes in liver cirrhosis deaths with changes in alcohol consumption - but the full benefit (i.e. that implied by the RR which reflects longer term differences in alcohol consumption) will not be realized for some years, given cirrhosis is a chronic condition. This needs acknowledging as a limitation.

APPENDIX

p.20. Why a belly curve? It reads as though it is a log-normal distribution. A distribution very common in biology and public health. That said, the belly curve seems to work. I do, and I assume the reader too, will be curious as to why a log normal distribution was not used though.
The Method is a comparative risk assessment (CRA) method; i.e. estimate change in BP distribution for a given intervention, integrate before and after with RR function, and thereby estimate proportion of deaths avoided. An extension of PAR% methods. This is appropriate, but also has limitations as applied here that need acknowledging:

a. As with many models of this 'cross-sectional' nature, deaths prevented are estimated as though the change in risk factor had the immediate impact suggested in meta-analyses. For BP and stroke/CHD, this is probably not too far out (time lags are not long - unlike cancer aetiogenesis). Nevertheless, time-lags are not included.

b. A grave limitation of the CRA method to deaths averted is the failure to capture the years of (healthy) life saved. It sounds useful to say that X deaths averted, but at what age? How much life gained? Etc. For a publication in high impact journals, I think some estimate of (healthy) life years gained is required.

c. The cross-sectional CRA approach does not allow for attrition of intervention effect. Thus, whilst (say) 50% of target population are assumed covered (good), there is no allowance for attrition of intervention effect (e.g. people ceasing medication, relapsing to high alcohol consumption, etc). This will - in the real world - result in much less health gain than suggested in the analysis. That is the results in Table 3 are optimistic, although the authors do frame it in terms of "within 12 months" without falsely implying this amount each year for many years - which is a sensible framing.


Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Unable to assess

Are the conclusions drawn adequately supported by the data shown?
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