Reviewer's report

Title: A community programme to reduce salt intake and blood pressure in Ghana.

Version: 1 Date: 10 October 2005

Reviewer: Laura P Svetkey

Reviewer's report:

General
This is a very interesting study, using a unique strategy for preventing hypertension in Ghana, with possible implications for other Sub-Saharan countries. The authors focus on one non-pharmacologic strategy, namely reducing sodium intake, in 12 villages in Ghana. There was a significant relationship between reduced sodium intake and reduction in BP, and blood pressure trended lower in the villages that received the low salt intervention. The fact that there was no difference in sodium intake between intervention and control villages may simply reflect the limitations of 24-hour urinary excretion as a measure of sodium intake. An important limitation of this study was the focus exclusively on sodium intake. More powerful effects on BP are noted with the DASH dietary pattern and with weight loss in the overweight. Nonetheless, the reduction in population BP associated with a population-wide reduction in sodium intake could lead to significant reduction in hypertension incidence and potentially in BP-related cardiovascular events. In addition, this clever study design could serve as a model for testing other lifestyle strategies, and the authors provide important in interesting details about the challenges of implementing a population-based intervention in Ghana, and presumably in other similar societies in Africa.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)
1. The authors assume that readers are familiar with statistics concerning increasing rates of hypertension and stroke rates in Africa. I think many readers will not be familiar with these data, and the authors should provide them, focusing on Ghana if country-specific rates are available.
2. In "Household survey and population census", the authors should confirm that those numbers are correct: total population of 12 villages combined = 16,965, of whom only 2743 were adults. It may be plausible and true that only 16% of the total population are over 16 years of age, but this statistic should be confirmed.
3. Collecting two 24-hour urine samples is difficult in Western countries, especially in women, and I would imagine even more difficult in rural and semi-urban Ghana. The authors should give more detail about how they were able to pull this off, and what percent of participants actually provided the samples.
4. The intervention is key to replication and implementation of these results; therefore the authors should provide additional detail: who attended? how were they encouraged/convinced to attend? where and when were the meetings held (what kind of setting, what time of day)? what issues were addressed in making it feasible and desirable for villagers to attend these sessions? how were they advertised? was there any incentive? what proportion of villagers actually attended? what percent of men? women? what happened to attendance over time?
5. In the section called "followup", the authors should define "response rates" - is this the % who provide f/u data? If so, the numbers don't match Figure 1.
6. In Results: Since rural vs semiurban status could be an important confounder, the authors should provide the rural/semiurban breakdown by treatment group.
7. Since DASH dietary pattern and particularly potassium intake also affects BP, the authors should report the effect of intervention on urinary excretion of potassium if available. If not available, they should discuss the possibility that the intervention had unintended impact on intake of fruits,
vegetables and other sources of potassium.
8. The authors are more pessimistic about the results than this reviewer. The study was insufficiently powered to detect small reductions in BP that could have public health implications, and yet they showed trends in this direction suggesting that a larger study might show significant benefit.
9. In the "perspectives" section, the results of the DASH-Sodium trial are not accurate. In non-hypertensive participants (a better term than "normotensive" since most non-hypertensives in DASH-Na had prehypertension not normotension), the reduction in BP associated with reducing sodium intake by an average of 38 mmol/day (without the DASH diet) was 3.4/2.0 mmHg (Bray et al. Am J Cardiol 2004;94:222-227). Effects are greater in African Americans and therefore might be expected to be greater in West Africans.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)
1. On page 5, "Randomisation and power calculations", the last line refers to "Table 1", but this is the table of regression coefficients. The tables should be renumbered or the text corrected.
2. On page 7, "relationship between urinary sodium and BP", the authors refer to table 1, but this table should probably be re-labelled table 2 (see #1 above).
3. On page 7, correct typo: "Intention-to-[tr]eat" should be "Intention-to-treat".
4. On page 9, first full paragraph, change "If we accept that there was been contamination..." to "If we accept that there was contamination...".
5. On page 11, first line of "Perspectives", "DASH-salt study" should be changed to "DASH-Sodium study".
6. Clarify title of Figure 2: what does "adjusted for...locality in each village..." mean?

Discretionary Revisions (which the author can choose to ignore)

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:

I declare that I have no competing interests.