LETTER TO THE EDITOR

Response to Dr Baverstock's letter to the editor

Letters

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Letter to the Editor

Response to Dr Baverstock's letter to the editor

Dear Editor,

We are grateful for the opportunity to respond to some of the issues raised by Dr Baverstock. First of all, we wish to clarify that our review was NOT investigating the efficacy of the current WHO guidelines on iodine prophylaxis. As can be seen from the title, and upon reading the text, our review deals with the effects of iodine blocking thyroid cancer, hypothyroidism and benign thyroid nodules following nuclear accidents. We note that this systematic review is a basis for the ongoing revision of the 1999 WHO guidelines in which Dr Baverstock was involved.

We performed this review in accordance with guidelines of the Cochrane Collaboration, using precisely defined inclusion and exclusion criteria, and a search strategy co-developed by us and an information scientist. In contrast to Dr Baverstock's assumption, we were well aware of the difference between iodine prophylaxis for goitre prevention, and potassium iodine blocking. In light of the explicit title of our review it should be clear to readers that the term 'iodine prophylaxis' was used in the context of the response to a nuclear accident. We have to state, though, that the studies we read did not always allow a clear-cut distinction between the two approaches as they investigated the use of iodine (whether as potassium iodine thyroid blocking (KITB) or goitre prophylaxis) only as a side-issue or used the respective information in the context of dosimetry. The fact that the actual details of the intervention were not at all well reported is clearly highlighted several times in the paper.

The Zarzycki *et al* study (1994) was concerned with 'the estimation of the effects, possible side-effects and immunological reactions after the mass iodine prophylaxis following the Chernobyl nuclear disaster' (compare original work from Zarzycki *et al* 1994). Thus, and in contrast to Dr Baverstock's opinion, it was clearly relevant for our assessment, even though some outcomes of interest were not included in the study.

Our study was not restricted to children only, again erroneously surmised by Dr Baverstock. We agree that small numbers of thyroid cancer in children provide an inadequate evidence base for conclusions, but this is not the point of our work. The study by Bandurska-Stankiewicz *et al* (2010) includes 297 case patients with thyroid carcinoma and 589 healthy control subjects and thus represents an important evidence base. We are puzzled by the crude estimations performed by Dr Baverstock as a basis for effect assessment. Evidence-based medicine and Public Health uses different approaches.

What our work shows is that a very plausible and—based on biological mechanisms and physiological considerations—effective intervention such as KITB is hard to assess under an evidence-based framework which looks at empirical results showing that the intervention works as intended. The fact that these results are scarce is the downside of the fortunate fact that there are very limited occasions to actually use KI in affected populations and study its effects. Whether the aftermath of the Fukushima accident represents such an occasion is contested given the estimated thyroid doses for the vast majority of the population.

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To put Dr Baverstock's concerns at rest, we confirm that we (repeatedly) read all papers that we found. The style of this final comment in his letter, however, speaks for itself. Finally, we wonder whether Dr Baverstock has arrived in the 'new world' of evidence-based guideline development.

Best wishes,

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