Reviewer's report

Title: Thyroid cancer risks among medical radiation workers in South Korea, 1996-2015

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Reviewer: Mark Little

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General comments

The paper reports an analysis of thyroid cancer in a cohort of Korean radiation technologists. The findings are largely null, as one would expect given the fact that exposure is in adulthood and the doses are low. The authors make too much at various points (e.g. beginning of the Discussion) of the elevation in SIR, which given the possibility of ascertainment (screening) bias is probably meaningless.

The analysis has been generally well done (but note the additional censoring specified below). However, the presentation of results and the writing are in need of polish. The paper could usefully give more details on the dosimetry. The dosimetry appears to parallel what was done in the USRT cohort (Simon et al 2006 ref 17), but I suspect that the parallel is not exact, and in any case readers should not have to go to another paper to work out details. The language is occasionally opaque, and would benefit from the services of a native English speaker (which I note that the paper has on its authorship).

Specific comments (page/line)

p.4 l.55-57 Does this exclusion of previous cancers include non-melanoma skin cancers (NMSC)? If so, I think there is no good reason to exclude these individuals. Also, is follow-up censored at cancer occurrence after baseline (as it should be, again for all cancers apart from NMSC)?

p.5 l.49-56 This sentence ("Thyroid organ dose … study methods [17].") is incomprehensible, and should be re-written. Also, why is the 2006 report of Simon et al (ref. 17) referred to here? Is there some reason why this outdated methodology, rather than that of the currently used USRT dosimetry report (Simon et al Radiat Res 2014 182 507-28) be referenced?

p.6 l.6-9 What is the meaning of this phrase "which were determined as the log-linear function of time and age"? As it stands, this barely standard English (e.g. "the" should surely be "a").
This ("Organ-specific doses were estimated … [19].") is a carelessly written sentence. As it stands it implies organ doses were estimated by multiplying measured and reconstructed badge doses and some conversion coefficients i.e. [measured dose] x [reconstructed dose] x [conversion coefficient]. What is presumably meant is that measured doses were multiplied by one coefficient, reconstructed doses by another coefficient; as below why this should be should be made clear.

What are the values of these ICRP-provided conversion coefficients? The values should be given here. Also, why are the measured and reconstructed components of dose treated differently?

It is not quite clear what is going on here. I assume that the thyroid will generally be above the apron, but that the badge will sometimes be under the apron, sometimes above, which is why one has to adjust for the probability of wearing the badge in these two locations. Perhaps the introductory sentence ("We also considered …") could be expanded to make this clear.

As above, follow-up should also be censored at cancer occurrence after baseline (for all cancers apart from NMSC).

What is the default lag period if sensitivity analyses used unlagged and 5-year lagged dose? Table 4 indicates that in fact both lag periods were used. However, unlagged analyses make no sense; the dose five or so years before the point at risk is essentially irrelevant. There is evidence from Chernobyl-exposed populations (e.g., Kazakov et al Nature 1992 359 21-2) that at much higher doses that thyroid cancer has a lag period of no more than five years (and probably not much less). So in Table 4 and elsewhere I recommend reporting only 5-year lagged results and confine unlagged and (perhaps) 10-year lagged results to an Appendix.

SIR for an endpoint like thyroid cancer are almost meaningless, as screening effects could easily account for variations by factors of 2, which encompass the range of SIRs shown in Tables 2-3. It is probably better to report an overall SIR, and otherwise give relative risk (RR) e.g. in relation to some low dose group (Table 3) or occupational group (Table 2). A Figure showing the variation of RR with dose would be useful.

As above, SIR for an endpoint like thyroid cancer are almost meaningless. The elevation of SIR is made too much of in this para given this. The authors later rightly qualify this finding, but it is not necessary to raise this straw man at all. Therefore, I would not recommend beginning the Discussion with treatment of this finding.
Somewhere in this para the authors should point out another weakness of the cohort, namely that exposures are entirely in adulthood. As there are remarkably few cohorts, even at much higher levels of dose, that exhibit risk after adult exposure (Mabuchi et al Radiat Res 2013 179 254-6), excess thyroid cancer risk would scarcely be expected in this low-dose adult-exposed cohort.

The authors should perhaps specify what these other risk factors may be, and give references in support. Apart from ionizing radiation exposure in childhood and female sex I would judge that there are remarkably few such, in particular, familial risk, and possibly obesity and cigarette smoking. All of these are unlikely to confound the thyroid cancer radiation dose response in this cohort, and perhaps this should be stated here.

It is reasonably common to report SIR, RR and ERR so the reported findings are not peculiar to this cohort. I would therefore remove this sentence (“Fourth, the comprehensive …workers.”).

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