

**DISCUSSION ITEMS FOR JUNE 18TH CONFERENCE CALL PERTAINING TO
JUNE 10, 2009 PUBLIC REVIEW DRAFT**

Section 2.4, page 8:

Section 2.4, Blue Book Overview: p. 8: It would be helpful to include a reference to “equivocal evidence.” Dr. Preston responds that while some - - notably Miller and Boice [J.D. Boice, Jr. and R. W. Miller, Childhood and adult cancer after intrauterine exposure to ionizing radiation. *Teratology* 59, 227-233 (1999)] are skeptical whether or not in-utero exposure can cause cancers other than leukemia, even they acknowledge that such exposures are likely to increase risks of childhood leukemia. However, what many would regard as the best contemporary review of the evidence is by Doll and Wakeford [R. Doll and R. Wakeford, Risk of childhood Cancer from fetal irradiation. *Br. J. Radiol* v70, 130-139. (1997).]. Dr. Preston does not think that they conclude that the evidence for increased risks of childhood cancer is “equivocal.”

Section 3.2.2, p. 11, lines 40-42:

Dr. Roessler believes that the statement pertaining to risk coefficients derived from studies of cohorts medically irradiated with x-rays needs more explanation to tie it to the RBE discussion.

Section 3.4.3, p. 14, line1:

Dr. Gilbert observes that BEIR VII has no model for NMSC.

Section 3.4.5 Lung, p. 15 1st paragraph:

Dr. Gilbert asks where this comes from. She observes that Gilbert et al (2004) presents an estimate of the RBE with confidence interval (CI), but does not present sex-specific results.

Section 3.4.5 Lung, p. 15 2nd paragraph:

Dr. Roessler observes that the last sentence in the second paragraph sort of hangs there and leaves one with the question as to whether a threshold exists.

Section 3.4.6 Leukemia, p. 15, after 1st paragraph:

Dr. Roessler observes that the section above on leukemia leaves her hanging, and that perhaps it needs a “wrapup” sentence.

Section 4.2.4, Additional Comments on Risk Transfer, p. 20, lines 23-25:

Dr. Gilbert asks why this is in the uncertainty section? Is this material needed? She does not recall that there was adequate discussion and consensus on the points made in this section. She suggests that perhaps only the last 3 paragraphs should be retained since these pertain to

uncertainty. She also asks if this statement is correct, observing that EPA models give greater weight to ERR models than to the EAR model noted here.

Section 4.2.4, Additional Comments on Risk Transfer, p. 20, 1st paragraph:

Dr. Matanoski observes that the first paragraph under Section 4.2.3, above, the assumption of similarity of baseline rates reflecting an ability to assume that Japanese data are directly comparable to U.S. data is probably a good fit, *if the cell types of the cancers of that site are the same for the two populations*. She notes that may not be true for all sites, and she is not sure that all cell types have the same sensitivity to radiation. She notes further that Shore apparently says it is not true for the skin. She concludes that certainly for years, we have assumed that the cell types of leukemia have different sensitivity to radiation.

Section 4.2.4, Additional Comments on Risk Transfer, p. 21, 1st paragraph, line 3:

Dr. Gilbert observes that she does not know of any instance where the tumor sites with different frequency of background occurrence has been a consideration in the choice of ERR or EAR models. She observes that BEIR VII based its weights largely on what is known about interactions of risks from radiation and other factors as well as whether factors that account for differences in baseline risks are initiators or promoters.

Section 4.2.4, Additional Comments on Risk Transfer, p. 21, 2nd paragraph:

Dr. Gilbert observes that this discrepancy should be reduced with the use of the arithmetic instead of the geometric mean.

Section 5.2.3 Radiogenic Thyroid Cancer, p. 23, 1st paragraph:

Dr. Roessler observes that the NCRP Report 159, “Risk to the Thyroid from Ionizing Radiation” is now available. She suggests that in view of this, perhaps the RAC should recommend that EPA should include it’s evaluation in its approach, since the Report uses the most recent epidemiology and has a significant conclusion which follows: “For the population at greatest risk (ages 0 to 14 y), NCRP Report No. 159 preferred model predicts a lifetime risk that is up to 1.5 times greater than that in NCRP Report No. 80. For the entire population, the risk is less in the new Report.”

Section 5.4.1 Low-Dose Protracted Exposure, 24, end of 2nd paragraph:

Dr. Gilbert observes that Mayak is not a low dose cohort. Accordingly, she deleted Gilbert et al 2004, Shilnikova et al 2003 and Gilbert et al 2000.

Section 5.4.3 Cancer Subtypes, p. 25, 1st paragraph:

Dr. Davis wonders why there is no reference to brain tumors in this document. She advises that the human evidence has accumulated quite nicely and there is a consensus among those working in this area that IR is a known risk factor for this tumor. Perhaps the RAC could encourage expanding the discussion of issues relating to brain tumors (cancer) under the Section 5.4.3 Cancer Subtypes and at least add to the list of sites under point (c) asking for comment on why they have not included these site specific risk estimates.