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Kopie von e-mail von Frau L.Walsh PhD vom 4.5.2021 in Word-Dokument (resp. .pdf)

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19_Han_2011_Cancer incidence among residents of the Three Mile Island accidentarea 1982–19951-s2.0-S0013935111002039-main.pdf
151 KB

23_Davis et al (2105) Solid cancer incidence in the Techa River Incidence C....pdf
248 KB

24_Hsieh_17_BJC_Ca incid after residential rad, Taiwan.pdf
108 KB

29_Ahn_08_JOEM_Rad workers & Ca incid & mort, lodose.pdf
138 KB

30_Muirhead_09_BJC_Mort & Ca incidence in UK Rad Worker Registry.pdf
127 KB

31_Jeong_10_REB_Rad & Ca incid, Korean NPP nuc workers, rad occ lodose.pdf
272 KB

36_Schubauer-Berigan et al. 2015. Cancer mortality through 2005 among a poo....pdf
503 KB

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Sehr geehrte Frau Bürgler,

Vielen Dank für Ihre Email. Ich hoffe, Sie entschuldigen meine Antwort per Email und nicht per formellem Brief. Für einige Antworten ist es besser, ENGLISCH zu verwenden, um Fehlinterpretationen zwischen Antworten und der veröffentlichten Literatur zu vermeiden. Nachfolgend finden Sie meine Antworten auf die 3 IPPNW-Fragen. Die Publikationen-pdfs finden Sie in der Anlage.

a) In Erinnerung an Ihr instruktives Referat an der Sitzung des TFK/ENSI vom 23.11.2018 möchten wir Sie höflich anfragen, ob sich aus Ihrer Sicht am Konzept, das Sie in Ihrem damaligen Vortrag vorgestellt haben (siehe attachment) aufgrund der erwähnten nun neu vorliegenden Metaanalyse des NCI etwas ändert?

REPLY: I would of course update the power-point presentation to include references and critique for new studies (e.g., the NCI-meta-analysis by Hauptmann et al 2020), but I would not change the concept. I would explain that a meta-analysis summary risk can be seen, in the simplest terms, conceptually, as a weighted mean of published risk results. The Hauptmann et al 2020 all solid cancer meta-analysis for adults in their figure 1 included overlapping data (for Korea, refs 29 and 31 in the Hauptmann et al paper) and a risk factor that did not reflect the originally published risk by a factor of 10 (Three mile Island, ref 19 in Hauptmann et al). After discussions between myself and Prof. Hauptmann and some re-calculations by him, it became clear that, because these 3 studies had very low weighting in the Meta-analysis in figure 1 of Hauptmann et al, the overlapping data and the factor of 10 did not affect the summary risk in this figure 1 to any important extent. Furthermore, although it is true that eligible studies had a mean cumulative dose of less than 100mGy, the risks were derived from doses partially much higher than 100 mGy. So again, for the all solid cancer meta-analysis for adults in this figure 1, 4 studies contributed ~95% of the total weighting:

1) Techa River (ref 23 in Hauptmann et al): %over 100 (mGy or mSv) =11 (TRDS-2000), doses ranged up to 960 (mGy or mSv).

2) Taiwanese residents (ref 24 in Hauptmann et al): %over 100 (mGy or mSv) =9, dose ranged up to 2363 (mGy or mSv).

3) UK NRRW (ref 30 in Hauptmann et al): %over 100 (mGy or mSv) = 6, and these 6% contributed 59% of the collective dose, highest dose category 400 (mGy or mSv) or larger.

4) US nuclear workers (ref 36 in Hauptmann et al): %over 100 (mGy or mSv) ~ 5 (including zero doses) or ~12 (excluding zero doses), highest dose ~700 (mGy or mSv) – NOTE: I estimated % numbers by printing out and measuring the bars in the frequency distribution in Fig 1 of ref 36 in Hauptmann et al.

Please note that the higher doses over 100 mGy or mSv will influence the summary risks per unit dose obtained from these 4 datasets to an extent that can only be known if these datasets are combined into one dataset and then the risk analysis is restricted to doses under 100 mGy. This meta-analysis of Hauptmann et al (figure 1) does not provide strong evidence of a statistically significant summary risk on the 0-100 (mGy or mSv) range, as IPPNW seem to be claiming. Even if the meta-analysis did provide strong evidence of a statistically significant summary risk on the 0-100 (mGy or mSv) range, the current radiation protection guidelines, using the LNT model, do account for risks under 100 mSv, but warn against calculating risks in the very low dose range (see ICRP report number 103 from 2007).

I have attached all the papers cited in the Hauptmann et al 2020, relevant to the points made above. Please examine these issues directly using the attached original publications, to verify the scientific points made above. I assume that these were the points also made by Frau. Dr. Sardella at the last ENSI TKF which I could not attend.

Please kindly note that if the highly qualified IPPNW members are not prepared to carefully examine the scientific points directly to back up their claims, in an unbiased way, and just wish to act as go-betweens in an open "formal letter" exchange between Prof. Hauptmann and myself and other parties, then I do not wish to be further involved, at such a "go-between" level.Thank you in advance for your understanding in this point.

b) Ist es möglich, dass eine Revision der Richtlinie ICRP-103 aus dem Jahre 2007 nun zügiger vorankommen könnte - und somit nicht erst ca. 2028 publiziert würde?

ANTWORT Ich kann nicht für die ICRP sprechen. Ich habe mich nie für eine Nominierung in eines der ICRP-Komitees beworben. Ich bin nur Mitglied einer Task Group, auf Einladung.

c) Spricht aus wissenschaftlicher Sicht etwas dafür, an solchen Formulierungen festzuhalten?

ANTWORT Hier möchte ich auf die KSR verweisen.

Mit freundlichen Grüssen,

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