Review of the report:

"Exposure to Radiation and Health Outcomes" by M. Lemstra; a report commissioned by the Canadian Centre for Policy Alternatives (Saskatoon Office) 2009 June

by

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Review of: "Exposure to Radiation and Health Outcomes" by M. Lemstra; a report commissioned by the Canadian Centre for Policy Alternatives (Saskatoon Office) 2009 June

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Summary

The author is evidently not familiar with the topic of radiation and health effects, judging from the mistakes made in the document. Few papers are selected as the basis for his review, the findings in most of them are misinterpreted, and the limitations and caveats clearly expressed in many of the reviewed paper are ignored. As a result the author gives a completely false impression of the impact of radiation on health. The objective of the report is to provide an evidence-based epidemiological review of the impact of exposure to radiation on subsequent outcomes. The superficiality of the review, together with the errors, misinterpretations of study findings, and failure to take into account the basic considerations of epidemiologists in reviewing evidence for causal relationships, make the document a travesty of an evidence-based review. The Canadian Centre for Policy Alternatives and the Saskatchewan Union of Nurses have not been well served by the document.

Outline of findings

The first three sections of the report deal with the health effects of radiation. The topics covered are cancer and cardiovascular disease in the survivors of the atomic bombs in Japan; cancer in nuclear workers in Canada and internationally; cardiovascular disease in Canadian radiation workers; cancer from plutonium doses in workers at the Russian Mayak facility; leukaemia and cardiovascular disease in the workers who cleaned up at the Chernobyl facility after the accident; thyroid cancer and leukaemia in the public after the Chernobyl accident; cancer and leukaemia in the residents along the Techa River in Russia; and leukaemia in children near nuclear facilities.

This review is on these three sections. The main findings of this review are summarized here with reference made to the paragraphs in the detailed text that follows.

- The introduction to radiation and health effects has errors [2, 5] and does not provide the reader with even the most rudimentary information on the effects of radiation that would provide a basis for understanding the following text [3,4].

- Throughout the text the metric of risk—the excess relative risk at one sievert, or per sievert, or per gray—is misstated as being excess relative risk, thereby giving the exaggerated impression that the total group being studied has that risk. [6,8,18,20,21,22,26].

- The confidence intervals in many of the Tables are misstated as 95% when they are actually 90%, with the result that the significance of estimates is exaggerated [7,8,17,19].

- Values for the incidence of site specific cancers in the study of the Japanese survivors of the atomic bombs are incorrectly and incompletely copied from the reference and unwarranted conclusions are drawn from the data [8].

- The reader is given no information on the relevance of the results from studies such as that of the Japanese survivors and how they can be interpreted for radiological protection purposes. This is a key topic that is discussed at length in one of the references and which should have been discussed in this report [9, 10].

- The discussion of risk to children relative to that to adults confuses two factors – the inherent relative sensitivities of children and adults, and the age-dependence of the biokinetics associated with internal emitters [11].

- In the discussion of the mortality from heart disease and other non-cancer disease after exposure to radiation, the conclusion by the authors of the reference cited—that the data are insufficient to establish a causal relationship—is ignored [12].

- In the discussion of the impact of radiation on the health of nuclear workers, completely ignored are the discussions of the uncertainties in the results of these studies. These uncertainties are discussed by the authors of the cited references and reviewers have expressed doubts about the validity of the estimates of risk, particularly those based on the Canadian National Dose Registry, that are higher than those from the study of the Japanese survivors. The contention that such results warrant revision to radiological protection standards has no substantive basis [14,15,17,18].

- Values of the confidence intervals in a study of the health of Canadian radiation workers are incorrectly stated and the cautions and caveats expressed on the results of this study by its authors and by others are ignored. The assertion that these results should prompt a revision of radiological protection standards is without foundation [20].

- In the discussion of a study of cardiovascular disease in Canadian radiation workers, errors are made in quoting and interpreting risk and dose quantities. The conclusion that the results show an excess risk of disease after exposure to radiation ignores the comment by the authors of the cited paper that their results needed to be interpreted with caution [21].

- In the discussion of a study of the health of workers at the Mayak nuclear facility in Russia there are several inaccuracies; in citing risk values and the risk metric (i.e., confusing excess relative risk and relative risk), in interpreting the results, and in concluding that the increased risk is associated with low doses, which it clearly is not [22,23,24].

- The findings from studies of the incidence of leukaemia and of death from cardiovascular disease in the clean-up workers after the Chernobyl accident are inaccurately stated as demonstrating the impact of chronic low doses of radiation on health. Ignored are the conclusions by the authors of the cited references and others that point to the inappropriateness of such an assertion [25].

- The results from several studies of the incidence of Chernobyl-related thyroid cancer are inaccurately cited and the conclusions drawn—particularly that such cases have been observed as far away as the United Kingdom—are contrary to those of the authors of the cited references [26,27].

- The conclusion drawn from the review of papers on Chernobyl-related leukaemia in children is that increases were found across Europe. This conclusion is directly contradicted by the authors of the cited papers [28,29,30].

- An excess relative risk per gray is cited for chronic lymphoid leukaemia (CLL) in the residents along the Techa River in Russia. CLL is well known to be not linked to radiation. The value cited is actually for leukaemia excluding CLL [31].

- The presentation of results from a study of the incidence of breast cancer incidence is completely in error. Numbers of cases are identified as risk values and doses are mischaracterized. The discussion is, therefore, irrelevant [32,33].

- The discussion of leukaemia near nuclear power stations is an inadequate and misleading description of the findings from the cited studies and ignores other extensive studies that have been undertaken related to so-called clusters. The failure to link such clusters to radiation exposure is not mentioned [34,35].

- The conclusions 1–9 that pertain to the sections reviewed here reflect the mistakes and misinterpretations in the body of the report, which are detailed below. In particular, conclusion 6 that nuclear power workers in Canada have a much higher excess relative risk of all cancer mortality than worker elsewhere is not warranted [14–19]. There is no basis for the assertion of conclusion 7 concerning the need to revise current radiological protection standards [18]. The statement of conclusion 8 on the implications of the Chernobyl accident for thyroid cancer and leukaemia in countries thousands of miles away has been shown to be clearly wrong [25–30]. Finally, the assertion of conclusion 9 on the association of living near a nuclear power facility with leukaemia in children is misleading in that though three clusters have been identified, such clustering is not specific to nuclear power sites and is unlikely to be related to radiation [34,35].

Detailed review

This review covers the introductory comment on the search strategy and the Sections A, B and C, page by page. References cited in the report are noted in italics (e.g., *reference 1*). References introduced in this review are given as footnotes.

Search Strategy

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1. There is no indication of how the quality of papers was determined by the author who, from the mistakes made in writing the report, would appear to have little background in radiological health effects. Why were 51 of the 73 articles that were reviewed in detail not accepted? Were they of inferior quality? The footnoted comment that "If a very high quality paper" on a particular topic was accepted, there was no need to accept a "lower quality paper" on the same topic is astonishing. And what about other "very high quality papers" on the same topic? As an epidemiologist, the author would undoubtedly be familiar with the so-called Bradford Hill criteria or considerations[1], which epidemiologists apply in reviewing evidence for causal relationships. Selecting just one paper on a particular topic is hardly compatible with the criterion that in assessing effects and potential causation there should be consistency between the findings of different studies, in different places, circumstances and times.

A. Exposure to radiation and impact on health outcomes

Page 7

2. The first sentence is a misquotation of a sentence from the WHO "fact sheet" on its website (r*eference 1).* The fact sheet has "Ionizing radiation exposure is measured as 'absorbed dose' in gray (Gy)." The report has "Ionizing radiation is measured as absorbed dose in gray (or Gy)." Leaving out the word "exposure" leaves a nonsensical sentence but the WHO description is incorrect anyway. Ionizing radiation exposure is a quantity that expresses the ability of

[1] Hill AB. The environment and disease: association or causation? Proc R Soc Med 58:293–300; 1965.

radiation to ionize air and thereby create electric charges, which can be collected and measured. It has the unit roentgen, equal to 0.000258 coulomb/kilogram air. Correct would have been to write "The amount of ionizing radiation absorbed (the radiation dose) is measured in joules per kilogram. The special unit gray (Gy) is given to 1 joule/kilogram."

3. The next paragraph lists considerations that are taken into account by epidemiologists in designing studies and interpreting the results (though Dr Lemstra refers to them only as being considerations when reviewing the evidence). Though considerations such as these are important in epidemiology (see, for example, the discussion in *reference 2*) is not clear why they are listed here since they are not referred to elsewhere in the report. The "overall scientific quality of the study" is listed, again without any indication of how this is ascertained.

4. Completely missing from this section, or anywhere else in the report, is any discussion of how radiation affects health and, in particular, the various relationships between radiation dose and magnitude of effect or probability of occurrence of effects, or of linear, curvilinear or threshold relationships. Such a discussion is essential in providing a context for a quantitative assessment such as this report. How else, for example, can the reader appreciate the analyses that are undertaken to ascertain whether a threshold or non-threshold relationship is more appropriate, or appreciate the subtle difference between metrics such as excess relative risk per sievert and excess relative risk at one sievert? Any evidence-based review, which this report sets out to be, needs to reflect all these considerations in leading the reader to the best estimate of the risk to health from ionizing radiation at the doses likely to be encountered by workers and the public from nuclear facilities.

5. In introducing the life span study (LSS) of the survivors of the two atomic bombs in Japan, the author inaccurately refers to the cohort as "survivors of the atomic bomb". He correctly notes the importance of the study in assessing the risks from radiation but errs in stating that " … with about 5% (479 cases) directly attributable to radiation exposure." What the UNSCEAR text *(reference 2)* actually has is "…about 5% (479) would be attributable to radiation exposure." The addition of "directly" by the report author implies that 479 specific

cases could be identified as being caused by radiation. This is not the case. The attribution can only be made in a statistical sense as an unidentified fraction of the whole cohort.

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6. In the first paragraph, in quantifying the risk from radiation, Dr Lemstra writes "The best estimate for excess relative risk of incidence of total solid cancers (excluding leukemia) after exposure to radiation is 43% (range 35% to 53%) for males and 81% (range 71% to 92%) for females." The omission of any dose quantity here leaves the reader believing that, for example, there was an excess relative risk of 43% for all the males in the cohort. Table 1, which follows this text and from which these numbers are taken, indicates in a footnote that the quantity tabulated is excess relative risk per 1 Sv. This is wrong; the values tabulated in the UNSCEAR report are actually excess relative risk <u>at</u> 1 Sv. This distinction is important since the implication of the former is that there is a linear relationship between excess risk and dose (and hence one can scale down; the risk at 0.5 Sv being half that at 1 Sv for example). In the latter case such a scaling is not valid unless one can show that the relationship is a linear one.

7. Another mistake on Table 1 is that the confidence intervals tabulated are for the 90% values, not 95% as stated. Ninety five percent confidence intervals would have wider bounds and hence estimated values would be judged less significant.

8. These errors (the risk metric and the confidence intervals) also occur in the Tables 2, 3, 4, and 5. In Table 3, which gives the incidence of site-specific cancers in the LSS, several of the entries have been incorrectly copied from the cited UNSCEAR report (*reference 2*; Tables 20, 24, 27, 30, 32, 33, 35, 37, and 40). The estimated excess relative risk at 1 Sv for bone cancer (males) in UNSCEAR Table 30 is 3.34, not 2.24 (or 224% as Dr Lemstra is wont to write); and the values tabulated for urinary bladder cancer are actually those for non-melanoma skin cancer. The correct values (from r*eference 2*, Table 37) are 0.63 (0.17, 1.25) and 1.74 (0.71, 3.22) for males and females respectively. By not showing a complete set of values for all male and female organs as given in the UNSCEAR document, an important perspective on the marginal significance of some of the values is lost. For example, for rectal cancer the missing value of ERR at 1 Sv for males is <0 (<0, 0.28). This is not significantly different from the value for

females (which is included in the Lemstra report), 0.46 (0.08, 0.97). As the UNSCEAR Table shows, the combined value of ERR at 1 Sv for males and females is 0.18 (<0, 0.46). Arguing that there is clear evidence of rectal cancer in females on the basis of these data is unjustified, particularly since, as noted above, the confidence intervals are the narrower 90% values.

9. The author makes no attempt to put the tabulated estimates of ERR at 1 Sv estimated for the Japanese survivors of the A-bombs into context or to show how one sets about deriving risk estimates from these and other data that are to be found in the UNSCEAR compilation (*reference 2*). As is pointed out in that reference (paragraph 545), while the relative risk model is useful for the purposes of modelling cancer risks, it is the absolute risk that is most often of interest to an exposed individual or population. The author completely ignores the need to express the estimates in terms of absolute risk.

10. Derivation of such an estimate is provided in detail in the UNSCEAR report (*reference 2*). The estimated values for lifetime absolute risk of exposure-induced death due to all solid cancers combined (paragraph 593 in *reference 2*) for a populations from China, Japan, Puerto Rico, United States and United Kingdom of all ages, averaged over both sexes, are 3.6%–7.7% per Sv at a dose of 0.1 Sv and 4.3%–7.2% per Sv at a dose of 1 Sv. (That is, the estimated slope of the relationship between risk and dose increases slightly with dose). For leukaemia mortality, the absolute risks are 0.3%–0.5% per Sv at 0.1 Sv and 0.6%–1.0% at 1 Sv. Note that these values in the UNSCEAR report are slightly lower than previous estimates. The International Commission on Radiological Protection, whose recommendations form the basis for radiological protection regulations around the world, has reached a similar conclusion in its most recent report[2] and recommends that the approximated overall fatal risk coefficient of 5% per sievert continues to be appropriate for the purposes of radiological protection.

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11. The author, having noted the estimated ERRs for children are higher than for adults for solid cancers and leukaemia (as shown in tables 1, 2, 4 and 5), then jumps to discuss age-related

[2] International Commission on Radiological Protection. The 2007 recommendations of the International Commission on Radiological Protection. Oxford: Pergamon Press; ICRP Publication 103; Ann ICRP 37(2–4); 2008.

factors that determine the uptake and the effects of radionuclides. The discussion confuses two topics. One topic is the inherent relative sensitivity of infants to radiation doses, whether from external sources of radiation or from radionuclides that have been taken into the body. This is reflected in the age-related risk estimates from, for example, the studies of the survivors of the A-bombs. In the paper cited by Dr Lemstra (Richardson, *reference 4*), the author presents a hypothesis for the physiological basis for this inherent sensitivity. The other topic is the age-dependence of the biokinetics of uptake, retention and excretion of radionuclides. This is reflected in the age-dependent dose coefficients, such as those suggested by the International Commission on Radiological Protection, that are used to estimate doses that result from ingestion, inhalation or percutaneous intake of radionuclides[3]. Richardson (*reference 4*) suggests how the ICRP coefficients could be refined, based on an analysis of the biokinetics underlying this age-dependence. Since Dr Lemstra has not provided his readers with any background in basic radiobiology and biokinetics, there is no context for the brief and confusing discussion of this paper in his report.

12. Mortality from heart disease and other non-cancer disease after exposure to radiation is discussed with reference to Table 6, which has been extracted from *reference 3*, another recent UNSCEAR report. As with the previous tables, Table 6 also has the wrong confidence intervals; they are for 90%, not 95% as shown. Dr Lemstra writes "The present scientific data suggests [*sic*] there is a small causal relationship between low doses of ionizing radiation and cardiovascular disease or other non-cancer disease" and indicates the data in Table 6 as supporting this. Not noted is the long discussion in the UNSCEAR document on the uncertainties related to these data. The conclusion in the UNSCEAR document (paragraph 129) is more nuanced: "It is the judgment of the Committee that, given the inconsistent epidemiological data and the lack of a plausible mechanism, the present scientific data are not sufficient to establish a casual relationship between ionizing radiation and cardiovascular disease at doses of less than about 1–2 Gy." Why Dr Lemstra would ignore this conclusion and not be more restrained in his conclusion is puzzling.

[3] International Commission on Radiological Protection. Age-dependant doses to members of the public from intake of radionuclides: Part 5. Compilation of ingestion and inhalation dose coefficients. Oxford: Pergamon Press; ICRP Publication 72; Ann ICRP 26(1); 1996.

13. The author notes the UNSCEAR finding (*reference 3*) that studies on patients receiving radiotherapy for Hodgkin's lymphoma or for breast cancer have demonstrated an increased risk of heart disease. The key point, not noted by Dr Lemstra, is that the radiation doses involved were tens of gray; i.e., the doses were much higher than relevant in worker and public radiological protection.

B. Exposure to radiation and impact on health outcomes to nuclear power workers

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14. There is no basis for the statement at the end of the second paragraph: "Recent studies have confirmed significant excess relative risks of health problems and mortality from chronic exposure to low doses of radiation previously believed to be safe." It is not clear what the author means by or to whom the author is referring with "believed to be safe". If the author is referring to the results of the 15-country IARC study (*references 5 and 6*) in which the estimates of ERR per Sv were higher than estimates from earlier studies, then the substantial doubts about the validity of the estimates expressed by UNSCEAR (*reference 2*), for example, which has been the source of much of the data so far in this report, should have been noted. There are two key points. One is that, without the Canadian data, the ERR per Sv is no longer significantly different from zero. The author does mention this later on page 11. The second point, which he does not note, is that there are substantial doubts about the validity of the Canadian dosimetry data (and, in particular, those for AECL workers) that were provided from the National Dose Registry of Canada. The IARC 15-country study results do not include data from Ontario Hydro; the results for the Canadian cohort are essentially due to AECL workers. As recently pointed out by Wakeford[4] earlier studies that have used dosimetry data directly from AECL records[5,6] have not shown unusually high ERR per Sv values, whereas those that have used

[4] Wakeford R. Radiation in the workplace—a review of studies of the risks of occupational exposure to ionising radiation. J Radiol Prot 29:A61–A79; 2009.
[5] Gribbin MA, Weeks JL, Howe GR. Cancer mortality (1956–1985) amongst male employees of Atomic Energy of Canada Limited with respect to occupational exposure to external low linear energy transfer ionising radiation. Radiat Res 133:375–380; 1993.

AECL data from the National Dose Registry have yielded high values. The study by Zablotska *et al.* (*reference 7*) is an example of such a study. There is clearly a large disparity between the results for AECL workers when the AECL data base has been used and when the records as they appear in the National Dose Registry have been used. Wakeford concludes that this pattern of results "suggests that the AECL data included in the National Dose Registry require investigation and that it would be prudent to consider the ERR coefficient for all cancers excluding leukaemia in the 15-country study to be that obtained from the data base for workers other than those from Canada; namely 0.58 (95% CI: -0.22, 1.55) per sievert.

15. There is a similar conclusion in the UNSCEAR report (*reference 2*, paragraph 113), to the effect that "not too much should be made of the apparent discrepancies [of the risks derived from the 15-country study] with risks observed in other studies, such as the LSS." Dr Lemstra ignores these conclusions and reflects none of these caveats on the results.

16. In the second paragraph, introducing the IARC 15-country study (*references 5 and 6*), it is stated that: "Ninety percent of the workers received doses below 50 mSv (or 50 mSv below current International Commission on Radiological Protection standards)." There would appear to be a misunderstanding of one or other of these doses. The worker dose is the dose accumulated over a lifetime. The ICRP "standard" being referred to is the recommendation that doses be limited to 100 mSv over 5 years with no more than 50 mSv in any one year. This is a limit in the current CNSC radiation protection regulations.

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17. In Table 7, which has data extracted from Table 1 of *reference 5*, the incorrect confidence intervals are given—the values presented are for 90% not 95%. The 95% confidence intervals, which are given in *reference 6* (Table 3), are, of course much wider. For example, for all cancer mortality excluding leukaemia they are (0.14, 1.97) compared with the 90% values (0.27, 0.18);

[6] Cardis E, Gilbert ES, Carpenter L, Howe GR, Kato I, Armstrong BK, Beral V, Cowper G, Douglas A, Fix J, Fry SA, Kaldor J, Lavé, C, Salmon L, Smith PG, Voelz GL, Wiggs LD. Effects of low doses and low dose rates of external ionizing radiation: Cancer mortality among nuclear industry workers in three countries. Radiat Res 142:117–132; 1995.

and for solid cancer mortality (0.03, 1.88) (i.e., barely significant) compared with 90% values (0.16, 1.71).

18. Given the doubtful validity of the Canadian data in the IARC 15-country study, and the resulting doubtful validity of the estimates of ERR per Sv, as discussed above, there is no real basis for the author's conclusion that there is new evidence that should prompt a review and possible revision of current radiological protection standards. However, it is worth pointing out that the comparison of doses and risk quantities by the author is misleading. In paragraph 2, below Table 8, Dr Lemstra notes that the average worker exposure [*sic*; it should be dose] was 19.4 mSv. Then the text continues: "At 100 mSv, the excess relative risk for all cancer mortality for nuclear power workers is 151% (Table 9)." Table 9 has data extracted from *reference 5*, the IARC 15-country study. A reader would conclude that a worker receiving 100 mSv would have his or her risk of cancer increased by 151%. In fact the estimated value of 151% is the excess relative risk per Sv. Hence, were this value of ERR per Sv to be valid for a dose of 100 mSv, the excess relative risk at such a dose would be 15%. Indeed, the same table in *reference 5* gives the relative risk at 100 mSv as 1.15; i.e., an ERR of 15%. As has been pointed out above, the more appropriate metric is the estimated absolute risk per Sv. The value recommended by the ICRP as appropriate for protection purposes is 5% per Sv, which corresponds to an increased risk of cancer of 0.5% for a dose of 100 mSv. This is somewhat different from the 151% highlighted in the report as the relevant risk value .

19. Table 9 shows the mortality rates for nuclear power workers by different dose levels in the IARC 15-country study (*reference 5*). The confidence intervals are, again, mistakenly given as 95%; the values tabulated are actually for 90% levels. The values of ERR per Sv for the doses up to 100 mSv and up to 150 mSv are marginally significant at the 90% level; significance would be lost with the wider 95% values. Also, not all the entries in the IARC table are included in Table 9. Left out is the value of ERR per Sv for doses up to 400 mSv which is 1.18 (90% CI: 0.37, 2.11); i.e., the point value of ERR per Sv is lower than those listed for lower doses although the confidence interval overlaps. These two observations should temper any interpretation of this table.

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20. A study of the health of Canadian radiation workers by Zablotska *et al*. is cited (*reference 7*), and the results from it are given in Table 10, albeit incorrectly. The confidence intervals all have errors. For all solid cancer mortality the interval is (-0.038, 7.13) not (0, 7.13); for lung cancer it should be (-0.193, 12.7) not (0, 12.7); for colon cancer it should be (<-2.08, 48.4) not (<0, 16.5); for rectal cancer it should be (1.41, 165) not (1, 165.1); and for leukaemia excluding CLL it should be (0.205, 291) not (3.97, 225). In the evaluation of this study Dr Lemstra does not indicate that the Canadian cohort in the IARC 15-country study is a sub-set of this cohort with the doses being from the National Dose Registry, so the results of the two studies are not independent. The difference between them is that the IARC 15-country study as reported in *reference 5* excludes Ontario Hydro workers from the analysis because of what was seen as insufficient data for stratification on socioeconomic factors, whereas the study by Zablotska *et al.* (*reference 7*) includes those workers. When the Ontario Hydro workers are included in the IARC 15-country study, as reported in *reference 6*, the estimates of ERR per Sv from the two studies are not appreciably different. Hence, the same caveats to the results from the study cited in this paragraph, stemming from potential issues involving the records for AECL in the National Dose Registry, apply here just as they did to the results from the IARC 15-country study. Zablotska *et al.* note in their discussion that the confidence bounds on the estimates of ERR per Sv are sufficiently large that any differences from the results of other studies such as the LSS could be due to chance. Dr Lemstra ignores these cautions and considerations in his assertion, in paragraph 3, that these results should prompt a revision of radiological protection standards. There is no substantive basis from these results for this assertion.

21. The fourth and fifth paragraphs describe a study of cardiovascular disease in Canadian radiation workers whose dose records are in the National Dose Registry (*reference 8).* In the latter paragraph, the excess relative risks are inaccurately described; "per Sv" has been left out so again, the reader has the impression that the ERR applies to the whole cohort. (In fact, the reader could mistakenly take the value as indicating overall mortality in the cohort since "from cardiovascular disease" is omitted.) Some of the results from the study are given in Table 11 though inaccurately. The table is headed "mortality rates for Canadian workers exposed to radiation by different dose levels". In the table the values at a series of doses are given as

"excess relative risk". In fact, the table is of the value of excess relative risk per Sv for the first entry labelled "all mortality" (actually, all cardiovascular mortality, from Table 4 in *reference 8*) and, for the other entries, the values are the observed relative risk for given dose ranges 9 (from Table 3 in *reference 8*). Also, the dose values are given incorrectly. The line labelled 10 mSv is, in fact, for the range 10 mSv up to 20 mSv. The line labelled 20 mSv should be labelled 20 mSv up to 50 mSv etc. The top value should be > 400 mSv. There is an incorrect equating of the line labelled "100 mSv" (which is actually 100 mSv up to 200 mSv) with the ICRP standards. Again there seems to be confusion between a lifetime accumulation of dose (the table value) with the 5 year limit of 100 mSv. Dr Lemstra states (*page 13*) that the authors of the study conclude that "there is an excess risk of disease after exposure to doses of radiation that were previously considered safe." In fact, the statement quoted is an introductory comment in the abstract that is given as the reason for the study that is being reported in the paper. There are no supporting citations. The authors' conclusions are far more circumspect: "Caution needs to be exercised in interpreting these results, due to the potential bias introduced by dosimetry uncertainties, potential record linkage errors, and especially, by the lack of adjustment for non-radiation risk factors." It should also be noted that the mortality rates for males and females respectively in the cohort are 40% and 50% lower than those in the general Canadian population and substantial confounding by these non-radiation risk factor would be expected.

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22. The final study on the health of workers discussed is that on the workers at the Mayak nuclear facility in Russian who had substantial doses from plutonium. In his summary of the overall results, Dr Lemstra has several inaccuracies. The third mean plutonium dose cited is to bone surfaces; not bone—the distinction is important for dosimetry. The values of risk cited are ERRs per gray—the "per gray" has been omitted, leaving the mistaken impression that, for example, males for whom the mean lung dose was 0.19 Gy had an excess relative risk of 7.1.

23. The results of the Mayak study are said to be discussed in Table 12 but it is just a table; there is no discussion. The data are given inaccurately—again. The table indicates that the risk metric for all values is excess relative risk. It is not. For lung, liver and bone cancer mortality for males and females the metric is ERR per Gy, taken from Table III in *reference 9*. The risk

metric for specific dose ranges is relative risk for the specific sub-cohorts, taken from Table II in *reference 9*. The difference is that ERR per Gy is an estimate of the trend of risk with dose; with the other metric (relative risk in a given dose range) the values reflect the numbers of cases of the particular cancer in the group of workers with doses in the given ranges compared with the referent group with essentially zero plutonium body burdens, the appropriate adjustments being made for age, gender, and birth cohort or age, gender and calendar period.

24. The doses in Table 12 are organ doses from plutonium in grays. In commenting on their implications, Dr Lemstra does not point out that a substantial radiation weighting factor needs to be applied to give a metric appropriate for comparing risks since the doses are predominantly from alpha particles. The lowest organ dose where there is a relative risk significantly greater than one at the 95% confidence level is 0.2–0.3 Gy to lung (Table II in *reference 9*). Weighting for the effectiveness of the alpha energy would give a equivalent dose to the lung of 4–6 Sv. The key point is that they are large doses, not low. This is the group of nuclear workers in which the plutonium doses have been sufficiently high for risks of lung, liver and bone cancer to be estimated. Even with this study however, only for lung cancer could a relationship of risk with dose be quantified; for liver and bone cancers, although values of ERR per Gy could be estimated, there is no direct evidence of increase risk below 3 Gy for the former and 10 Gy for the latter. It is clear from the discussion here that it is completely wrong to state, as does Dr Lemstra, that "Once again, low doses of exposure are associated with excess relative risk of cancer mortality."

C. Exposure to radiation and impact on community residents

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25. In the opening paragraph Dr Lemstra introduces studies of the impact on health of the accident at Chernobyl, referencing the fact sheet on the WHO web-site (*reference 1*), and notes that the 240,000 workers responsible for the clean-up (liquidators) had the highest levels of exposure, which resulted in a doubling of their incidence of leukemia and an increased risk of death from cardiovascular disease. There are no references to support these points in the fact

sheet. From the full WHO report on which the fact sheet is based[7], which would have been a more appropriate reference, it seems that the first point is based on a study that found a two-fold increase in the incidence of leukaemia (excluding CLL) among Russian liquidation workers with estimated total doses of 150–300 mGy, compared with incidence in the general population of Russia. However, as Wakeford has noted[4], there is no dose-related trend which indicates that the observation may be largely due to ascertainment bias. Increased incidence and mortality from cardiovascular disease has been observed in the clean-up workers but, as the authors of the WHO report[7] and of the UNSCEAR report (*reference 3*) point out, it is not at all clear that the increases are related to radiation dose and they may well be a result of more traditional risk factors. For example, it was observed in the Russian clean-up cohort that the incidence of cardiovascular disease was driven primarily by hypertensive diagnoses and, paradoxically, the incidence of ischemic heart disease and acute myocardial infarction, which might be expected to correlate with mortality, did not increase with dose.

26. In the second paragraph, studies of the incidence of Chernobyl-related thyroid cancers are discussed. Dr Lemstra indicates that "significant increases in thyroid cancer were not only found in the Ukraine but also in Belarus, Russia, Czechoslovakia and as far away as the United Kingdom." He indicates that the increase in Belarus was 484%, with a range 96% to 1630%, citing Moysich *et al.* (*reference 10*), which is a review of Chernobyl-related ionizing radiation exposure and cancer risk. What group in Belarus this 484% is referring to is not given by Dr Lemstra. It is, in fact (from Moysich *et al.*) the increase in risk for the highest dose category (>1 Gy) versus lowest dose category (< 0.3 Gy). Note, 1 Gy is not considered a low dose. It is surprising that Dr Lemstra would refer to only this one publication on thyroid cancer, given the extensive and more recent perspective on the topic in his *reference 2*, the UNSCEAR report.

27. The comment concerning the observation of thyroid cancer related to Chernobyl "as far away as the United Kingdom" misrepresents the finding of a study carried out on children and young adults in the North of England. The findings (discussed in *reference 10*) were the observation that, in the study area, there were 26 cases in young adults in the 19 years before

[7] Bennett B, Repacholi M, Carr Z (editors). Health effects of the Chernobyl accident and special health care programmes. Report of the UN Chernobyl forum expert group "Health". Geneva, WHO Press, World Health Organization; 2006.

1986 (the year of the accident) and 30 in the 11 years after. There were three cases in children less than 15 years old before 1986; 4 cases after, in the corresponding intervals. The authors of that study note that for iodine a greater effect in the younger group would have been expected and that various factors, including improved ascertainment and earlier detection of tumours could have contributed to the increased incidence. The authors conclude that it seems doubtful that these results reflect exposure to iodine from Chernobyl. Dr Lemstra reflects none of these caveats in his discussion.

28. In mentioning leukaemia in children in the areas contaminated by the Chernobyl accident, Dr Lemstra writes "As well, increases in leukemia in children were found in contaminated areas across Europe including the Ukraine, Belarus, Russia, Turkey, Greece and Germany (a 350% increase in Ukraine)." The citations are Moysich *et al.* and Davis *et al.*, *references 10 and 11* respectively). They directly contradict Dr Lemstra's statement.

29. Moysich *et al.* state in their review of the most comprehensive study then published (the European childhood leukaemia–lymphoma incidence study): "no associations between excess leukaemia risk and ionising radiation dose were apparent." They note a similar result for a follow up study: "…there was no evidence that the excess in leukaemia rates was more pronounced in areas that were most affected by Chernobyl-related ionising radiation exposure." Citing further studies, the same authors note that there was little evidence for an increase in rates of childhood leukaemia in Ukraine, Belarus, Finland, Sweden, or Greece after the Chernobyl accident. They note that "furthermore, there was no association between the extent of contamination and the increase in risk in these countries." Their overall conclusion is that the existing evidence does not provide support for the suggestion that childhood leukaemia rates in Europe had risen as a result of the Chernobyl accident. They note that results with respect to infant leukaemia are difficult to interpret: although several studies have shown a possible association, the rate increases are either not statistically significant, or did not follow the contamination patterns. Furthermore, haematological malignant diseases are rare in children and as many of the reported estimates are based on very small sample sizes, they should be considered unreliable. Finally, Moysich *et al*. point out that in most of these studies, rates could have been greatly affected by the addition or subtraction of two or three cases of leukaemia.

30. Davis *et al.* in the other paper cited (*reference 11*) as supporting Dr Lemstra's statement explicitly concludes with respect to leukaemia in European countries related to Chernobyl: "…this study provides no convincing evidence of an increased risk of childhood leukaemia as a result of exposure to Chernobyl radiation." That Dr Lemstra would make the contrary assertion is baffling.

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31. In turning to the health of residents along the Techa River in Russia, Dr Lemstra states that the residents had 360% excess relative risks per gray in chronic lymphoid leukaemia. This is clearly wrong since chronic lymphoid leukaemia (CLL) is well known to be not linked to radiation. (See, for example, extensive discussion in Dr Lemstra's *reference 2*.) The value quoted from *reference* 14, a case control study, is actually a conversion from the estimated odds ratio for leukaemia excluding CLL.

32. Table 13 is presented as showing excess relative risk (in %) for breast cancer incidence in residents along the Techa River in Russia. Cited is Ostroumova *et al.* (*reference 13*). The data in Table 13 are derived from Table 4 in that reference, values in Table 4 such as "0.3 (0.04–0.6)" being converted by Dr Lemstra to excess relative risk of 30% with confidence interval (4% to 60%.). These are similar to the conversions made in the other tables in this report, from the usual fractional values for ERRs per unit dose and CIs to the values expressed as percentages. However, in this case the values in Table 4 are not ERRs per unit dose. The table caption and accompanying text have been misread. The values given are numbers of cases estimated to be radiation-related in the various dose categories after adjustment for the effects of age, number of children, time of arrival on the contaminated territory (before and after 1953) and linear birth cohort effect. In effect, the estimated total number of radiation related cases were distributed over the dose intervals according to the characteristics of each dose sub-cohort. For example, in the dose range 25–49.9 mGy the number of observed cases was 19. Two cases (CI; 0.4–4.1) were attributed to radiation. Most (9.1) of the estimated radiation-related cases (13.5) were in

the > 50 mGy group. Dr Lemstra has interpreted the numbers of cases as being estimates of ERR. The data as presented in Table 13 are, accordingly, nonsensical.

33. The overall conclusion by Ostroumova *et al.* (*reference 13*) is that their results are consistent with the hypothesis of linearity of radiation dose-response for breast cancer and that their point estimate of excess relative risk is higher than reported by others but, because of the wide confidence intervals it is consistent with many other studies and, a final point, the results are based on small numbers and require cautious interpretation. Nothing of this discussion and caveats appears with Dr Lemstra's incorrect tabulation of the results.

34. Dr Lemstra introduces a discussion about leukaemia in children near nuclear power facilities by asserting that for such people "The only known health concern is leukemia in children." This is obviously wrong — people have been concerned about any potential impact on health and there have been many epidemiological studies looking for such impacts, though without any findings of significant relationships between radiation from the normal operation of such facilities and disease, including solid cancers and leukaemia. Dr Lemstra cites Laurier *et al.* (*reference 15*) as concluding "there is a range of increased risk of leukemia from 0% to 119% between countries for youth aged 0–24." (The 119% comes, not from that reference but from Grosche, *reference 16*.) This is a completely inadequate and misleading description of the findings from the studies of leukaemia in young people near nuclear sites.

35. Laurier *et al.* (*reference 15*) review of several hundred published documents on the risk of leukaemia in young people aged < 25 years around nuclear sites. They distinguish two types of study: descriptive studies that sought to estimate the frequency of leukaemia and possibly to detect excess risk within a population; and analytical studies that sought to find potential factors explaining excess risk of leukaemia within a population. In the descriptive studies, Laurier *et al.* find that among the 198 individual nuclear sites reported in 10 different countries, there were three where clusters could be confirmed. One was near the Sellafield reprocessing plant in England where 5 cases were observed in 1984 when only one was expected; another was near the nuclear reprocessing plant at Dounreay in Scotland where 5 cases were observed in 1986 with less than one expected; and the third was near the Kruemmel power plant in Germany where

there were 5 cases with less than one expected. Nothing could be said from these studies about the factors likely to explain such clusters. Further, after reviewing 25 different studies of multiple sites in eight countries, the authors conclude that though there were the few instances of clusters near individual sites, globally, the multi-site studies did not show an increased risk of leukaemia in children and young adults close to nuclear sites. They note that local excesses of leukaemia had been shown in zones with no nuclear sites. They also note that despite many studies there had been no evidence to ascribe a casual relationship between such clusters and radioactive or chemical discharges, and that another hypothesis that the cause was the fathers' occupational exposure to radiation had been rejected after many studies. The most convincing explanation, according Laurier *et al.*, is that the clusters are related to infection linked to population mixing. UNSCEAR (*reference 2*) adds some credence to this idea, citing a critical review of the evidence for the role of infectious exposures in the aetiology of childhood leukaemia that concluded that the observed space-time clustering and seasonal variation in the appearance of childhood leukaemia supported the idea that the clustering was a result of an infectious agent. Dr Lemstra's few sentences completely fail to reflect the evidence on this topic.