Comments on ICRU Report 64: Dosimetry of High-Energy Photon Beams based on Standards of Absorbed Dose to Water

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Abstract

The ICRU has published a report entitled Dosimetry of High-Energy Photon Beams based on Standards of Absorbed Dose to Water in Journal of the ICRU, 1 (2001) 1. I have been asked to review it for Physics in Medicine and Biology and I have such extensive comments on the report that I am presenting them here in detail, in addition to the much shorter review which has been published in Phys. Med. Biol. **47** (2002) 3663 – 3665 (preprint available on-line). It should be noted that I was a member of the report committee from 1988 to 1997 when I resigned from the committee and thus these comments and the associated review represent something of a minority report.

Section 1: Introduction

1. The report defines calorimetry as well as chemical dosimetry (Fricke) and ionization methods as primary standards or 'basic methods for the fundamental determination of the absorbed dose to water'. I believe this is an inappropriate classification because both the ionization and chemical methods rely explicitly on values of physical parameters (viz the variation in the value of G with beam quality and $(W/e)\, s_{gr,air}$) which are determined using absorbed-dose calorimeters. It is inappropriate to classify these 'non-calorimetric' methods as primary standards in the normal sense of the word.

2. Figure 2, showing the International Measurement System for absorbed dose determination fails to allow for the direct calibration of user chambers by the PSDL which is done by many PSDLs (eg Canada and the UK).

Section 2: Photon beam interactions

3. P19, left column asserts that the data from the NIST codes EPSTAR and ESTAR are based on the data in ICRU Report 37 whereas it is the other way around. The ICRU Report 37 values are based on these programs!

4. P23, left column makes a simple estimate of the effects of pair production on the photon scaling theorem. This ignores extensive work done at NPL on exactly this subject (see, e.g. Burns et al, PMB 39 (1994) 1555-1575).

5. P24, last para: The statement that the quality of the beam is fully described only by the complete photon information ignores the fact that the electron contamination of the beam must also be known in detail to fully describe any radiotherapy beam.

6. P25, right column, near bottom: The statement 'Data in TG-21 for stopping-power ratios were still given numerically as a function of nominal MV' is highly misleading, although strictly speaking correct. What this statement ignores is that the only stopping-power ratio data given in numerical form in TG-21 are for calculations of the relatively insensitive wall correction factors and that the important stopping-power ratios to be used in the dose equation are given graphically directly as a function of the measured ionization ratios. It was made explicit in the TG-21 letter of clarification (Schulz et al, Med. Phys. 13 (1986) 755– 759) that only this figure was to be used for this purpose and using the measured ionization ratio (no mention of MV, albeit TG-21 was vague and the worksheets appeared to allow the values in Table IV to be used - but these differ by up to 1% from the values in the figure, and hence the clarification).

7. P25, right column, near bottom: The statement about the methods used in TG-21 being based on "inaccurate calculations" is not justified. They were based on an approximate calculation method which elsewhere in the ICRU report is described as "sufficiently accurate" (p28) and which in practice gives the same results as the less approximate Monte Carlo calculations.

8. P29, top right column: the assertion that 'another intermediate step was introduced' regarding ionization ratios and nominal accelerating voltage repeats the same misleading idea as discussed in comment 6. It is not clear why such an unimportant and incorrect point is raised twice!

9. P28: The section on Monte Carlo codes fails to mention the EGSnrc code which was released in 2000 and which is now the standard code being used in many PSDLs. The PTB, ENEA, NPL, NRC and BIPM all presented results using this code at the May 2001 meeting of the PSDLs at the BIPM.

10. P29/30: In the discussion of stopping-power ratios I am surprised that the paper of Malamut et al is not mentioned (Med Phys 18(1991) 1222-1228) since it was the first

(only?) paper to demonstrate that including the differences between electrons and positrons made no difference to the calculated stopping-power ratios, despite the fact that the stopping powers for these particles differ significantly and this difference is not modelled in any of the other codes used to calculate stopping-power ratios.

Section 3: Methods

11. P32, top of left column uses the term 'thermal diffusivity' without any definition and p36 uses the term 'temperature diffusivity', also without definition but presumably meaning the same thing.

12. P32, left column: In this discussion of the heat defects of graphite and water, the report very conveniently takes the heat defect of graphite to be zero, and in Table 3 assigns zero uncertainty for this assumption, despite the fact that there are several papers (albeit quoted in the report) which report non-zero heat defects. The paper by Bewley and Page (ref 142) measured a heat defect of 1.4±0.6% (for the temperature rise per unit dose) and recommended that graphite had to be pre-irradiated to 5 kGy dose to get rid of this effect (due to oxygen in the graphite). Admittedly, in their summary they rolled up the results from several experiments and gave the overall estimate of 2±2% for the heat defect, but for the individual, most relevant experiment, they measured the statistically significant, non-zero heat defect given above and gave a reasonable explanation of the effect. It therefore seems inappropriate to assign an uncertainty of zero to the heat defect in graphite. This is a significant issue since the uncertainty associated with the heat defect in water calorimetry is considered the largest uncertainty in the use of water calorimeter. The heat defect in aqueous systems has been studied with much more care and attention than in the case of the graphite system, and the theoretical models assign a value of zero in equilibrium, yet those using water calorimetry accept that there must still be a significant uncertainty associated with the heat defect.

    The problem is that if one were to assign a credible value to the heat defect in graphite (say 0.5% for graphite which has not been pre-irradiated to 5 kGy), this would not have shown up in the photon beam experiments done to date with graphite calorimeters and it would have dramatic effects on all primary standards based on graphite calorimeters and also all primary standards of air-kerma where the product of $(W/e)\, s_{gr,air}$ is based directly on graphite calorimeters. Similarly, even assigning 0.5% uncertainty to the heat defect of graphite would affect the uncertainty on all absorbed-dose to graphite standards and also on the value and uncertainty on the value of (W/e).

    Nonetheless, it is the role of an organization such as the ICRU to point out such issues and give them a profile. By assigning a 0% uncertainty to the heat defect in graphite, they have legitimized this unjustified assumption.

13. Comment 12 draws attention to the fact that the report gives detailed uncertainty analyses for the graphite calorimeter method, the Fricke method and the ionization method, but avoids doing the same for the water calorimetry method.

14. P32, right column, bottom: The report quotes a 1997 paper by Klassen and Ross (ref 150) that concluded that using the H_2 or H_2/O_2 systems are best in water calorimetry whereas their most recent work no longer recommends H_2/O_2 and in fact N_2 saturated, i.e., pure water, is used for the standard at NRC (reported by Ross et al at the 1999 NPL workshop on calorimetry and more recently by Klassen and Ross, J. Res. of NIST 107 (2002) 171 –178).

15. Table 3, p36: The uncertainty assigned to the vacuum gap correction (0.12%) is unrealistically small, given the inherent difficulty in the measurements and calculations. Furthermore, it misquotes the results of the cited reference which gives a, still very optimistic, uncertainty estimate of 0.14% for the NPL's Domen type calorimeter. Furthermore, Table 3 gives a range of possible values for the uncertainty and the value actually assigned is well outside this range.

16. Table 3, p36: The combined uncertainties do not correspond to the quadrature sum of the values in the table, which should be 0.07 and 0.17 for a total of 0.19 but if one uses the corrected uncertainty estimate for the gap effect(see comment 15), the combined values in the report are actually correct.

17. P37, middle of left column: The text makes the point that thin horizontal foils can be used as barriers to avoid convection in a water calorimeter but then shows a detailed figure (#21) of a design with these thin foils in the vertical orientation. It would make more sense to show the NIST design with a horizontal foil.

18. P39, left column: The report mentions two semi-formal reports about variations of the Fricke G value but fails to mention the refereed and published full paper of Klassen et al (PMB, 44 (1999)1609 –1624).

19. P39, right column: The report gives an approximate formula for relating the dose in the medium to the dose in the Fricke using the ratio of mass energy coefficients for water to Fricke. In Ma et al's paper (ref 201 in the report), a full Monte Carlo calculation is done which shows a significant difference between the real conversion coefficient (which, in principle depends on the vial shape and beam quality, but the calculations suggest is 1.003 for all vials and energies studied). The difference in values is significant for high-energy beams (1.003 vs 1.001, which is twice the uncertainty assigned to this factor in table 4 of the report).

20. P39, right column, middle: The report cites a paper related to electron beams by Ma and Nahum (ref 200) whereas they meant to reference a paper on photon beams (PMB 38 (1993) 93 – 114).

21. Table 4, p40: This table introduces an uncertainty in the Fricke method due to the uncertainty in the stopping-power ratio, Fricke to water, and yet this quantity does not appear in the equations presented related to this method.

22. P43, 4-th last line states that the uncertainty using equation 21 for a thin walled chamber is 0.15% whereas Table 5 gives 0.20% for the same quantity.

Section 4: Primary standards

23. P45: There is a discussion here about the BIPM standards after there was an extensive discussion on p40. It would be much clearer to have all the discussion in one place - and less repetitive.

24. P45, eqn 22: the quantity k_{cav} is introduced here and despite the explicit reference to the list of quantities on p85, it is not defined there or anywhere in the report (at least that I can find).

25. P46, top left: the report cites a discussion document I presented to the CCRI in 1995 with the statement "The uncertainties of (W/e) and $(W/e)\,s_{gr,air}$ have been discussed by Rogers" and drops it at that. Although the report cited was a discussion paper marked 'Not to be quoted", one of its major results re-iterated a result presented in 1993 to the CCRI in a published NRC Report (PIRS 363, 1993). The point was that the original analysis of Boutillon and Perroche (PMB 32 (1987) 213 – 219, remarkably not referenced in the ICRU report except indirectly through ref 209) was very sensitive to several assumptions. The one indisputable change required from my re-analysis is that one of the important experiments to measure (W/e) relied directly on the half-life of ^{35}S and the modern estimate of this half-life is (a) more accurate than was available when the original data on (W/e) were measured and (b) causes the overall analysis of (W/e) to reduce the best estimate by 0.4% which is 3 times the stated uncertainty. It seems it would have been appropriate to mention this uncontested result, even if some of the rest of the analysis is still considered more controversial (related to estimates of uncertainties on stopping-power ratios).

26. P48: The report spends far more space describing the undeclared NRC water calorimeter based standard than it does describing that which was formally declared as Canada's primary standard in 1998 (not 1999 as stated in the report) and which had been worked on for several years. I think it would also be more appropriate to call it a Seuntjens-Palmans-type sealed-water calorimeter than a Domen-type since it differs in two important ways: it is operated at 4^{o}C and it has a considerably larger inner core. It is unfortunate that no uncertainty estimate is given for this standard since it is considerably more accurate than the undeclared standard.

Section 5: Comparisons

27. P51, left column: There is an unqualified statement that the Fricke vial wall correction 'can vary up to 1.5% in the worst case' whereas the paper cited reports a 2% effect for the standard vials used at NRC at 24 MV and the effect would presumably be even greater at higher energies.

28. P52/53 and figure 28: I find the entire section on absorbed-dose comparisons very confusing and inconsistent. I will give some examples related to NRC since I know these best, but there is similar confusion with the other comparisons as well. The caption of Figure 28 states that the NRC1 result is based on water calorimetry, which is partially true, but it is also based on the assumption that the Fricke G value is a constant from 20 MV to ^{60}Co, which is now known to be wrong, and does not represent the current Canadian standard based for water calorimetry, but this isn't made clear. The caption to Figure 28 states that 'other results' are from graphite calorimeters, which suggests that the NRC2 and NRC3 results are based on graphite calorimetry. No further mention is made of the NRC2 nor NRC3 values in the text, but for certain the NRC3 results are based on NRC's sealed water calorimeter, contrary to the caption. In general I find it very unhelpful to show different results from one lab, especially since only one of these results is considered current.

29. P53, top left column: An NPL/PTB comparison is referred to but no reference is given.

30. P53/54 and table 8: This discussion refers to a high-energy comparison and is referenced to an un-numbered NPL report from 1991 but the data have been revised since then (apparently by the ICRU committee): "the changes concerning beam quality and adjustments, and the correction for the Fricke ampoule glass wall have had the largest impact". This comment highlights the problem of using $\mathrm{TPR}_{20,10}$ as a beam quality specifier at standards labs. However, the reader is left unsure whether the revisions have included the effects of the variation in ϵG reported by Klassen et al (see comment 18), which, if it needs to be applied, would further improve the results of the comparison.

31. P53, right column: In reporting the NIST/NRC comparisons the report initially refers to a 1994 review paper (ref 242) rather than using the final data presented in ref 254. The report makes it look like the results of the two bi-lateral comparisons changed by 0.6% but failed to point out that the two comparisons were consistent with each other to better than 0.15% once the known changes in the standards were taken into account.

Section 6: Dissemination

32. Table 11 specifying reference conditions, P57: the description of the dose rate as being 'such that dose-rate effects are zero' leaves me wondering if initial recombination is to be corrected for or not? Do we correct for all ion recombination or not? It is not made clear where or how the field size is specified? The footnote could have noted that the 'some countries' using $T_o = 22^o$C rather than 20^oC, include the United States, India and Canada.

33. P59: in the discussion of the polarity effect it would have been helpful to point out that the detector readings are really the absolute values of the detector readings (albeit this should be obvious from the context).

34. Table 12 on p60 of measured k_Q values is very out of date. In particular, many of the values are based on the assumption of a constant ϵG and this means that the k_Q values at higher energies are 0.7% too high. I find it particularly disturbing that the most extensive and accurate set of measurements made to date are not reported (Seuntjens et al, Med Phys 27 (2000)2763–2779) despite the fact that these same data were made available to the report committee in early 2000. Figure 29 would look very different if the new data were included, and the ϵG corrections were made. In particular, the new data make it clear that the $\%dd(10)_x$ is needed instead of TPR^{20}_{10} as the beam quality specifier to get consistency between the measurements made with heavily filtered and lightly filtered beams.

35. P60, footnote: This rather extensive footnote duplicates a discussion already included on p41.

Section 7: Relation to air-kerma procedures

36. Eqn 32, P63: in the definition of g, there is no mention of what material it refers to (wall or cavity?). It should be the cavity.

37. Table 14, P64: This table presents ratios from different PSDLs of D_w determined for various chamber types based on a direct absorbed-dose calibration and on an air-kerma calibration with the application of a protocol. There is a 1.1% range of values which the report points out reflects differences in primary standards and non-uniformity of chambers of a given type. Another useful observation would be that within a given PSDL the values range by more than 0.4% in all cases, and in a recent study there was a 1.1% range in values when different build-up caps were used on the same PR06 chamber (Seuntjens et al, 2000). The significance of these values is that the range of values are independent of the standards used (in contrast to the actual values). The

ranges indicate the inaccuracy of the previously used air-kerma based protocols and demonstrate a significant advantage in using absorbed-dose calibration factors.

Section 8: Conclusions

38. P65: The conclusions state that the deviations between absorbed-dose standards are slightly larger than those of air-kerma standards, but this is made up for because they are more robust and direct. With the improvements in the absorbed-dose standards and their comparisons since the report was written and with the many changes happening to air-kerma standards because wall attenuation corrections are being changed, it is now true that there is better consistency of absorbed-dose standards than of air-kerma standards.

Appendix: Perturbation effects

39. It is unclear what the purpose of this section is since it is about ion chamber perturbation effects in photon beams and the purpose of this report is to discuss primary standards for absorbed dose and their dissemination. In principle, the entire point in developing primary standards for absorbed dose is to avoid having to worry about these perturbation effects by using measured values of k_Q. Furthermore, similar reviews are found in many locations.

References

40. I have found the following problems in the reference list just by reading it. They obviously reflect my own, highly biased, knowledge of NRC publications, but suggest that there may be other significant problems in the reference list.

    - Ref 76 is missing one author (J. Wei)
    - Ref 200 has given Nahum an extra initial.
    - Ref 230 shows me as the author but it was written by Ross, Klassen and Shortt. In addition, I cannot find it referred to in the report.
    - Ref 259 is an abstract, not a refereed paper and it should, as a minimum, be cited as an abstract (but in this case it would have been preferable to cite the ensuing full paper). I am not sure how many other references are just abstracts?
    - Ref 121, the third editor's name should be 'Rindi', not 'Rindl'

Typographical and grammatical errors

Note that problems identified in items 1, 4, 6, 7, 13, 14, 15. have been published by the ICRU in Journal of the ICRU, Vol 1, (2001) 94. I will add any further errata that are sent to me.

1. Eqn 2, P16, there should be a bar over the denominator
2. Figure 7 x-axis should be 'electron' not 'electronic' kinetic energy,
3. P21, last line left column makes a reference to page 12 which should be page 24.
4. Figure 13, P26: y-axis should be 'g MeV^{-1}
5. P28/29: The sentence implies that the electron spectrum was produced by 'photons in the detector'. This presumably is just a misplaced clause, but it could be highly misleading since photon interactions in the detector itself (which in the context means the gas in the cavity) imply a breakdown in the cavity theory.
6. Eqn 10, P36: 'm' on the right side should be $\frac{1}{m}$
7. P36, 7 lines below Eqn 10, $k_{el,j}$ should be $k_{ir,l}$
8. P42, right column, middle: 'to correct for some of the assumptions underlying the theorem' should read, for clarity: 'to correct for the breakdown of some of the assumptions underlying the theorem'.
9. P47, top left column: the cross reference to p40 should be p51.
10. Table 8, p54: the B in PTB is missing in the heading of column 9.
11. P60, top right column: "W_{air} is the mean energy", not "are the mean energy".
12. P63, top right column: "a calibration factor" not "an calibration factor"
13. Eqn 31, P63: The Q_o should be a subscript.
14. P64, top left: (V_ρ) should be $(\mathrm{V}\rho)$.
15. Eqn 34, P64 is missing a term on the right hand side, viz M_Q.
16. Table A1, the equation for p_{displ} is wrong, it should be $\mathrm{p}_{displ} = 1 - \delta r$, not $\mathrm{p}_{displ} = 1 - \delta - r$
17. P69, top right column: Table A1.1 should be Table A1.
18. P70, eqn A2: Unless you know this equation already, it would be very hard to interpret: the upper two lines are both part of the numerator on the right side.

19. Two PSDLs changed their names before this report was published and the old names are used throughout the report, viz ARL (now ARPANSA) and LPRI (now LNHB).

20. The word ‘ionization’ is consistently mis-spelled as ‘ionisation’ which is found neither in the Oxford English dictionary’s list of words nor that of Mirriam-Webster (although this is a common problem and a quick search of the PMB web site found 127 occurrences of the spelling with an ‘s’ vs 234 with the correct spelling ‘z’, often both spellings being used in one paper - so at least the ICRU report is consistent). However, the ICRU standard appears to be ‘z’ as evidenced by the title of Report 60: “Fundamental Quantities and Units for Ionizing Radiation”.

The following additional errata have been brought to my attention by J. E. Burns of NPL (retired). Bob also noted many of the other errata noted above. This was added Oct 24, 2002.

1. P12, Fig 1: Under ‘calorimetric method, graphite’, add $(\mu_{en}/\rho)_{w,gr}$, [see Eqn 18]. Above ‘Calorimetric method,water’ add C_p [see Eqn 7]. Under ‘ionisation method’ change $S_{w,m}$ to $S_{m,air}$ or $(S/\rho)_{m,air}$ [see Eqns 15 and 21] and change p_w to k_p or p_Q [see Eqn 22 or A3].

2. P22, Fig 8: TMR for the 10 MV beams is mis-labelled as 4 MV photons.

3. P24, Fig 10: Caption - all linear dimensions for graphite radiations were scaled as the **inverse** ratio of the electron densities of graphite to water.

4. P27, second column, line 18. Asterisk refers to the footnote on this page, not on page 21 as stated.

5. P40: The definition of the correction factor $\Psi_{w,gr}$ (Eqn 16) is unclear. Presumably Ψ_w refers to the photon energy fluence at P in figure 22(d), but does Ψ_{gr} refer to that quantity at P in Fig 22(a), (b) or (c)?

6. P40: Table 4. Subscripts differ from those used in the text. Also, $(s/\rho)_{Fr,w}$ does not appear in the discussion.

7. P42: Three lines and six lines after Eqn 18, μ and μ_{en} should have overlines.

8. P46: Table 6. Some of the symbols are not discussed in the text.

9. P54, Table 9: The dose rate for the sealed calorimeter should be 1.6447.

10. P63: The second sentence in the footnote seems to imply that $N_{D,w}$ refers to the absorbed dose to water in the cavity.

11. P68: Table A1: It would be clearer to express δ as a simple multiplier rather than a percentage multiplier, eg 0.00040 mm^{-1} rather than 0.40% mm^{-1}.

12. P70: Eqn A1: First term inside the square brackets should be $\alpha s_{wall,air}(\mu_{en}/\rho)_{w,wall}$.

13. P85: The symbol for change in temperature is ΔT, not Δt [Eqn 6].

14. P85: The symbol for specific heat capacity at constant pressure is c_p, not C_p [Eqn 6].

15. P85: The correction factor for air density is k_ρ, not k_p [Eqn 26].

16. P85: The symbol k_h is not used to correct for the humidity (in this report) but it is used in Eqn 7 for the correction for heat defect in a calorimeter.

17. P85: The symbol k_p is also used for the correction for polarity, not k_{pol} [Eqn 27].

18. P85: The symbols k_{ir} and k_{el} are used in Eqn 10, k_{ps} and k_{pf} are used in Table 6, k_{cav} is used in Eqn 22 and k is used in Eqn 25, but they are not listed here.

19. P85: The symbols k_e and k_{pol} are not used in this report.

20. P85: The symbol for air pressure is P, not p [Eqn 26].

21. P85: The symbol p_Q is also used for the correction for total perturbation [Eqn A3] as well as the symbol k_p [Eqn 22].

22. P85: The symbols p_Q, p_{disp}, p_{wall}, $p_{fluence}$ and p_{cel} are used in the Appendix, but not listed here.

23. P86: The symbol δ is used in the calculation of the displacement factor in Table A1, as well as for the relative heat defect [Eqn 8].

24. P86: The symbol used for path length is l, not ℓ [Eqn 11].

25. P86: The symbols $\overline{\theta}^2$, η_B and $\Re$ are not used in this report

In all previous ICRU Reports the various chapters, sections and subsections were numbered in the usual way, e.g. Chapter 4, Section 4.6, Subsection 4.6.2 etc. In this Report not even the chapters are numbered. Reference to particular points raised in the report has to be made by page numbers, column numbers and line numbers. This is very inconvenient for something that should be treated as a reference document, as are most ICRU Reports.