Review of ICRU Report 64: Dosimetry of high-energy photon beams based on standards of absorbed dose to water

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It seemed a strange coincidence when I was asked to review ICRU Report 64 as I was a member of the ICRU committee writing this report for nearly 10 years before resigning in 1997. Under the circumstances, this review will be something of a minority report.

This report deals with the reference dosimetry of photons beams, primarily for radiotherapy purposes. Its stated purposes are to present the status of different methods used in primary standard measurements for absorbed dose to water, to summarize the international comparisons of these standards, to present a formalism along with necessary correction factors and reference conditions for disseminating the measurements made in standards labs to the users' beams and to compare such a formalism to the previous one using air-kerma standards.

The writing of this ICRU report, which started in 1988, was the motivating force which led to the AAPM's TG-51 dosimetry protocol, published in 1999, and the IAEA's TRS-398 Code of Practice, published in 2001. As such, this report has already had a substantial influence on radiotherapy dosimetry since it has led to the near universal change-over from clinical dosimetry protocols based on air-kerma calibrations to those based on absorbed-dose calibrations (although the change-over in the UK and Germany preceded this committee's work).

The report's first section introduces four methods used to measure absorbed dose. The four methods that the report deems as capable of being primary standards are: methods based on graphite or water calorimetry; ionization methods; and chemical methods based on Fricke dosimetry. I have serious reservations about this classification. To measure absorbed dose to water, the Fricke method requires data about the variation with photon beam quality of G, the radiation chemical yield, and these data are based on an absorbed dose to water standard. Similarly the ionization method requires a value of $(W/e)\, s_{gr,air}$ which is predominantly based on measurements made with a graphite calorimeter. Thus the Fricke and ionization 'primary' standards are basically extremely complex transfer methods for the underlying primary standards, namely calorimeters. This does not mean that the Fricke and ionization methods cannot be used as first-rate national reference standards, only that they are not properly 'primary' standards as generally defined.

The second and longest single section of the report presents an interesting didactic review of the interaction of high-energy photon beams in matter with emphasis on fundamental concepts. This section makes many useful points although its handling of the issue of beam quality specification is unbalanced (but I will not get drawn into that debate again). This section suggests that the AAPM's TG-21 protocol used the nominal accelerating potential (in MV) to specify its stopping-power ratios. This is only true of those stopping-power ratios needed for calculations of the rather minor wall correction factor. Stopping-power ratios needed for the dose equation are given explicitly as a function of the measured ionization ratio in figure 2 of the TG-21 protocol and the TG-21 letter of clarification (Med. Phys. 13 (1986) 755)) made it clear that only this figure was to be used. It is unclear why the report should repeatedly (p25 and p29) make this point which is of such little consequence, as well as being wrong.

The third section describes the methods for fundamental measurements of absorbed dose to water, starting with water and graphite calorimetry, including a subsection on the conversion from graphite to water absorbed dose and covering Fricke and ionization methods. This could be the most valuable section of the report but it suffers from several major defects in my opinion. For example, it assigns no uncertainty to the assumption that graphite has a heat defect of zero. The experimental data on this point are at best inconclusive and there is one study which presents data clearly indicating there is such a heat defect until the graphite has received a dose of 5 kGy (ref 142, which in summarizing data from several experiments presents a pooled result of 2±2%, which the ICRU reports, but their data for the temperature rise per unit dose in graphite shows a 1.4±0.6% effect). Given that the uncertainty related to the heat defect in water (which is theoretically zero and which is much more extensively studied) is the largest uncertainty in the water calorimetry method, it is inappropriate for an ICRU report to assign zero uncertainty to the heat defect of graphite, especially since there is a real uncertainty in its value, let alone its uncertainty. If there were a 0.5% thermal heat defect in graphite, quite possibly this would not have shown up in the experiments done so far in photon beams, and yet it would have significant effects on absorbed-dose to water standards based on graphite calorimeters, on air-kerma standards and on the value of (W/e).

The third section of the report also accepts what I consider an unrealistic underestimate of the uncertainty on the gap effect correction for graphite calorimeters (0.12%, given in table 3) and even misquotes its reference 159 which gives an uncertainty of 0.14% (which is still optimistic). Interestingly, the combined uncertainty given in Table 3 appears to include the correct value from reference 159 since the values shown do not correspond to the quadrature sum of the individual values shown.

In this section the report addresses the issue of the G value for Fricke dosimetry but fails to cite the formal refereed paper published in 1999 by Klassen et al, which quantifies, for the first time, the variation in this quantity as a function of photon beam quality (PMB 44 (1999) 1609-1624). These results play a central role in photon beam dosimetry using Fricke dosimetry and have already been used in several papers to correct previously published results, so it is unfortunate this report failed to mention them.

In its discussion of the transfer from dose measured in Fricke solution to dose in water, the report only presents a simple approximation given by the ratio of mass energy absorption coefficients. This ignores the more detailed results available from Monte Carlo calculations (ref 201, published in 1993). This is unfortunate since the more detailed calculations imply that in high-energy beams the correction is 1.003 rather than 1.001$\pm$0.001 as given in the report.

The fourth, rather short section of the report describes the actual primary-absorbed standards at various PSDLs, as opposed to the more general methods described in the previous section. The report is rapidly being put out of date by the changes at the various labs, and so, for example, only 2 of the 6 figures are of standards which are currently being used at the labs which created these figures. Similarly, the report spends much more space describing a previous NRC interim 'research standard' (never used as the basis of a calibration service) than in describing the current declared standard (which was adopted in 1998, not 1999 as stated in the report). For a more up to date description of the standards at many PSDLs, see the 1999 'Proceedings of the NPL Workshop on Calorimetric Absorbed Dose Standards' which are available on-line at http://www.npl.co.uk/npl/rad/publications/calws/.

The fifth section describes comparisons done between various standards labs and the report recognizes the weakness of this section by closing with a statement that "this review can give only a first snapshot". The problem is that there have been so many changes in the last 2 or 3 years that the data presented are confusing, misleading and out of date. The good news is that as the standards have progressed, the agreement between them has continued to improve.

The sixth section presents the well known k_Q formalism for disseminating the unit absorbed dose to water which has been presented in several papers and more importantly two major protocols (TG-51 of the AAPM and TRS-398 of the IAEA). The section also presents a very dated comparison of measured k_Q values for various chambers. Unfortunately, many of the data are based on the uncorrected assumption that the Fricke G value is a constant, which implies many of the high-energy data are high by up to 0.7%. More unfortunate is the omission of the most extensive and accurate set of measured data from Seuntjens et al (Med Phys 27 (2000)2763–2779) which was made available to the report committee in early 2000.

The short seventh section presents a comparison of the air-kerma vs absorbed-dose calibration based formalisms. Table 14 presents an interesting comparison of the dose in a ^{60}Co beam determined either directly with an absorbed-dose calibration factor or indirectly using an air-kerma calibration factor and a dosimetry protocol. The table contains values for many different ion chambers as calibrated at 5 different PSDLs. The table clearly shows variations due to the use of different standards, but the report fails to point out the rather large variation (0.4% to 0.8%) between different chambers calibrated at the same PSDL. These variations are independent of the primary standards being used and are due either to the inaccuracy of the calibrations or, more likely, the inaccuracy of the protocols being used. In similar work at NRC, Seuntjens et al (2000) found a 1.1% variation for one chamber with different build-up caps. These variations are a very strong argument in favour of using absorbed-dose calibrations.

The appendix to the report is a discussion of perturbation effects for ion chambers in photon beams. This entire appendix, the contents of which are available elsewhere in detail (e.g., the IAEA's TRS-398), seems out of place in a report discussing 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!

Overall, despite the fact that creation of this report has led to significant advances in radiation dosimetry protocols, the report itself does not meet its stated goals. There have been significant changes to many of the standards and techniques described in the report so that it fails to present the current status of these methods and it presents many comparisons which are out of date because of changes in the standards. Furthermore, these comparisons have been supplanted by more recent comparisons. Many of the other goals had already been achieved in other documents. Furthermore, I think that the report does a disservice in a variety of ways, e.g., elevating standards based on Fricke and ionization methods (which are acceptable national standards) to the status of primary standards, or assigning a zero value and uncertainty to the heat defect for graphite or giving special status to one type of beam quality specification. Additionally, the report itself is full of a large number of typographical and other minor errors which reduces its usefulness. The ICRU has already published 7 of these and I have identified 19 altogether which are avaliable at http://www.sao.nrc.ca/inms/irs/icru64/icru64.html along with many other specific criticisms of scientific issues in the report.