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**ICRP Publication 92,
*Relative Biological Effectiveness (RBE), Quality Factor (Q), and Radiation-Weighting Factor (w_R)—
A Critical Review***

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PROLOGUE

Late in April 2004 the author received a letter dated 16 December 2003 from Professor Roger Clarke suggesting that he might wish to review International Commission on Radiological Protection (ICRP) Publication 92. In view of this reviewer's stated perception of flaws in and expressed concerned dissent from the introduction of the quantity effective Dose, E, in ICRP Publication 60, this was a most generous offer. Unfortunately, due to delays within the postal service and personal circumstances this review has only just been completed.

The reviewer understands that some of his suggestions might be unwelcome to ICRP but respectfully requests that they be given serious consideration.

GENERAL COMMENTS

"Beware you lose the substance by grasping at the shadow"
Æsop

This reviewer had hoped that this ICRP Publication 92 would provide the groundwork for a system of dosimetric concepts that would be universal and unambiguous. It would be integrated and applied to all radiations, whatever their energy and independently of their origin, either outside or inside the human body. It would be rigorous, logically and mathematically coherent, and consistent with physical laws. Finally, because of these characteristics it would be stable, ridding the radiological protection profession of the all-too-frequent changes in names and symbols of its concepts, many times more of shadow than substance. High hopes indeed.

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ICRP 92 has much to commend it. The report accepts that change is necessary in the radiation-weighting system and shows itself willing to make some changes, but sows the seeds of its own failure by constraining its changes to those that represent, in its own words, “minimal departures from the present system.” In consequence the recommendations for change in the report are rather in the nature of a patch on the inner tube of a leaking tyre rather than a brand-new tyre.

Universality. It is disappointing that the authors of the ICRP 92 Guest Editorial seem to dismiss any possibility of a universal dosimetric scheme that encompasses both internal and external exposure by the statement that “we believe that ICRP should continue the use of w_R values that relate, for external radiation, to the incident field. For radionuclide intakes, w_R values should relate to the internal fields that cause the absorbed dose to organs and tissues [*sic*].” Here is an undesirable and unnecessary source of ambiguity.

Duality and operational quantities. There is general agreement that protection quantities such as effective dose equivalent, H_E , and effective dose, E , may be determined, though some would not agree that they may be measured. When a quantity may be determined it may be sufficient to leave it to the inventiveness of the dosimetrists to discover the precise means with which to do so. It would be helpful if the commission could be persuaded to avoid further controversy over the dual system of radiation protection quantities by softening its apparent *imprimatur* of the ambient dose equivalent. It may not be wise to endorse ambient dose equivalent to the exclusion of alternatives. The ambient dose equivalent is of great utility in some aspects of dosimetry but there are significant problems with its application to neutron dosimetry, particularly at high energies (Ferrari 1998, 2004; ICRP 1997; ICRU 1998; Pelliccioni 2004).

Radiation weighting. Some considerable effort is expended in Sections 4 and 6 in modifying recommended radiation-weighting factors for high-linear-energy-transfer (LET) radiations, particularly neutrons. Because of its self-imposed constraints, the report fails to deal with root causes. The results are not altogether logically coherent and the effort might have perhaps been better applied to a revision of the $Q(L)$ - L relationship (see below).

High-energy radiations. A major deficiency of ICRP 92 is that there is no significant discussion of the dosimetric issues for particle accelerator radiation environments. The increasing exposure to mixed radiation fields, often not in radiation equilibrium, from particle accelerators with thin shields as well as in commercial air travel present novel dosimetric problems created by ICRP Publication 60 that require attention in ICRP’s revised recommendations.

The relevance of w_R at high energies. In ICRP 92 a major effort is directed towards the development of an average radiation-weighting factor applicable to the whole body (see above). Even if one accepts (which this reviewer does not) that ICRP 92 achieves this goal, the practical utility of this factor at high energies is nevertheless not clear. The author suggests that greater emphasis needs to be placed on an acceptable definition of the $Q(L)$ - L relationship, from which values of w_R but also, and perhaps more importantly, other parameters that facilitate the determination of effective dose may be derived.

Although some 25 years ago average radiation-weighting factors were of great value to the “critical organ-MADE” system of protection for neutrons, they are of much less interest at high energies or in more sophisticated detriment models, such as effective dose equivalent, H_E , and effective dose, E , which specify tissues and organs that facilitate the use of anthropomorphic phantoms. At high energies, and particularly at accelerator laboratories, there

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is more interest in using conversion coefficients that relate field quantities (e.g., fluence) to determine the radiological protection quantities.

It might be of assistance to ICRP in forming its final recommendations if it were made aware of the needs of, and the body of relevant information largely developed by, the accelerator community.

What follows is not a work of literature. It is a collection of paragraph-by-paragraph comments on ICRP 92. It rambles and it is repetitive in places. There is a need for a crisp summary. The spirit is willing but the flesh is weak. Please accept its faults with kindness.

SPECIFIC COMMENTS

GUEST EDITORIAL

Page 2, paragraph 2: Any possibility of a universal dosimetric scheme that encompasses both internal and external exposure is dismissed in the editorial by the statement that “we believe that ICRP should continue the use of w_R values that relate, for external radiation, to the incident field. For radionuclide intakes, w_R values should relate to the internal fields that cause the absorbed dose to organs and tissues [*sic*].” Here is a serious source of ambiguity.

PREFACE

No comment.

1. INTRODUCTION

Paragraphs 1-3: No comment.

Paragraph 4: Note that w_T and w_R are here introduced as independent quantities; i.e., the value of one quantity is not influenced by the value of the other (see paragraph 289 of the document). This independence is lost in the definition of q_E in paragraph 253.

Paragraph 5: No comment.

Paragraph 6: The paragraph is neither an entirely balanced nor accurate representation of the historical facts. ICRP publication 60 (ICRP 1991) in fact represented a *volte-face* in the commission’s evolution of policy that may be traced through ICRP Publications 4, 15, 21, and 51 (ICRP 1964, 1970, 1973, 1987). In Appendix 2 of Publication 21 ICRP recognised it to be convenient if the $Q(L)$ - L relationship were defined to be mathematically well-behaved and recommended a continuous function “as a common basis for calculation.”

Accepting that the commission’s views on the matter of detail and precision were indeed as stated here in paragraph 6, it is difficult to understand the logical process that simultaneously eliminated the $Q(L)$ - L recommendations of ICRP 21 but introduced an equation for the determination of the radiation-weighting factor for neutrons of energy E :

$$w_R=5 + 17\exp\{-(\ln 2E)^2/6\}$$

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“to provide consistency in calculations” (paragraph A13, ICRP 60) [*sic!*]. See also equation 1.5 in paragraph 12 of ICRP 92.

With the greatest respect, this is a perfect example of the absence of logical rigour that causes confusion and that the commission might consider a serious attempt to avoid in its future recommendations. See also comments on paragraph 9. As the great Yogi Berra remarked, “It’s *déjà vu* all over again.” See section 6.6.4 (paragraph 367).

Paragraph 7: Again with the greatest respect, the commission might be able to avoid further controversy over the dual system of radiation protection quantities by softening this apparent *imprimatur* of the ambient dose equivalent.

There is general agreement that protection quantities such as effective dose equivalent, H_E , and effective dose, E , may be determined, though some would not agree that they may be measured. When a quantity may be determined it may be sufficient to leave it to the inventiveness of the dosimetrists to discover the precise means with which to do so.

The ambient dose equivalent (ICRU, 1985, 1988) is of great utility in some aspects of dosimetry but there are significant problems with its application in some cases; for example to neutron dosimetry, particularly at high energies (Ferrari 1998, 2004; ICRP 1997; ICRU 1998; Pelliccioni 2004). It may not be wise to endorse ambient dose equivalent to the exclusion of alternatives.

Thus while the ambient dose equivalent is useful the commission might consider it be one weapon in an armourium of many alternatives that may be used as an operational quantity. Other techniques might, for example, include LET-spectrometry (paragraph 278) and neutron- and charged-particle spectrometry (Moyer 1952, 1954; Thomas 2003).

An even better option would be to abandon the dual concept of protection and operational quantities altogether, define only protection quantities, and, has already been suggested, leave it to the ingenuity of dosimetrists to deduce the means of measurement. (See also comments on paragraph 283.)

Paragraph 8: No comment.

Paragraph 9: If properly explained, the difference between precision and absolute accuracy should not present a difficulty. The quotation really is an insupportable straw man. One commissioner has suggested that the $Q(L)$ - L relation was to be eliminated: “A pragmatic recommendation has to be accepted as given or rejected. The ICRP Q - L relationship has been accepted, but has then been taken as a statement of biological truth, which it is not. The ICRP tried to escape from this travesty by introducing the radiation-weighting factor, with values that are adequately protective but obviously crude” (Dunster 1998). This reviewer responded: “(This statement) is a classic example of a fallacy of the false alternative and not supportable from the literature” (Thomas 1998).

Paragraph 10: No comment.

Paragraph 11: No comment.

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Paragraph 12, Table 1.2: If high-energy radiations are to be within the scope of the new recommendations it is necessary and important to specify the limitations on the simplification (definition) $w_R \equiv 1$ for electrons and photons in order to comply with physical laws. Electromagnetic interaction (e.g., photoneutron production) inevitably places a physical limit on this simplification.

When I once discussed this with a commissioner I was told that the simplification was a definition and must be accepted. With respect, I would suggest that even the commission cannot suspend the laws of physics.

Once again attention is drawn to the equation (1.5) and the conflicting logic discussed in the comments on paragraph 6.

Paragraph 13: No comment.

Paragraph 14: Is any useful purpose served by the change in the hitherto generally accepted notation for the ambient dose equivalent set of quantities; e.g., from $Q^*(d)$ to $q^*(d)$? If so it might be helpful to explain it. There is a strong argument for retaining customary symbols also used by other international organisations and for historical continuity.

Paragraph 15: As discussed elsewhere this is another example of imprecise use of terms: w_R and $Q(L)$ are not comparable parameters.

It is evident that the model for the $Q(L)$ - L relationship is itself central to the radiation-weighting system because values for \bar{Q} cannot be determined without first having such a model. If the model for $Q(L)$ - L is “biologically correct” then the laws of physics and mathematical logic lead inevitably to correct values of \bar{Q} under specified irradiation conditions and in defined phantoms.

In this sense the $Q(L)$ - L may be regarded as having primacy over \bar{Q} and, as ICRP Publication 21 recognised, it is convenient if the relationship is defined to be mathematically well-behaved (in particular, a smooth function). It is this relationship (model) that demands the focus of attention and best judgement of those making recommendations of any future radiation-weighting system. The primary scientific issue is whether the existing relationship is the best that Science can give us at this time. ICRP 92 does not suggest change from the recommendation of Publication 60 and, by implication, accepts that such is indeed the case (see Section 3). On no account should any of the particular values of \bar{Q} calculated under such specific conditions be taken to be a substitute for $Q(L)$ - L relationship from which they were generated.

2. THE CONCEPT OF RELATIVE BIOLOGICAL EFFECTIVENESS (RBE)

2.1 Background

Paragraph 16: No comment.

Paragraph 17: The first sentence does not make it clear whether the “somewhat different ways” in which RBE has been used is in both, or between radiobiology and radiation protection.

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Paragraphs 18-19: No comment.

Paragraph 20, first bullet, last two sentences: The point that “. . . it is appropriate that the quality of radiation under study is not substantially altered in the passage to tissues deep in the body. In cell studies and animal experiments, this condition is usually met” (underlining is the reviewer’s) is true only because few relevant experiments have been made in large animals. Is it not somewhat incomplete nor to mention this caveat?

In view of the importance of the size of the experimental animal in determining the quality of the internal radiation field that results from external irradiation exposure and its variation with location, it might have been helpful if this point had been discussed more generally and forcefully here because this issue becomes significant at higher energies in general and for neutron irradiation in particular.

There was after all some confusion in the selection of the values of w_R for low-energy neutrons in ICRP 60. Although the basic nuclear physics was understood at the time of the preparation of Publication 60 (for example from calculations of repose functions for Bonner spheres), its consequences were ignored. This is a matter addressed in section 4.3.

Paragraph 21: No comment.

2.2 Reference radiation

Paragraph 22: See comments on paragraph 12 concerning the physical limitation on the assumption $w_R=1$ for photons and electrons of *all energies*.

This reviewer would quibble with the language of the first sentence. The assumption $w_R=1$ for photons and electrons does not facilitate measurement (and with modern computational methods does not even facilitate the arithmetic).

Paragraphs 23-25: No comment.

Paragraph 26: See comment to paragraphs 12 and 22.

Paragraphs 27-28: No comment.

Paragraph 29: See also comments on paragraphs 12 and 22 concerning the physical limitation on the assumption $w_R=1$ for photons and electrons of *all energies*.

This reviewer would quibble with the language of the first sentence. The assumption $w_R=1$ for photons and electrons does not facilitate measurement (and with modern computational methods arithmetic is no longer a difficulty). However, in some cases, the assumption may facilitate the interpretation of measurements.

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2.2.1 In-vitro studies

Paragraphs 30-39: No comment.

2.2.2 Biophysical considerations

Paragraphs 40-47: No comment.

2.2.3 Information from radio-epidemiology

Paragraphs 48-53: No comment.

2.2.4 The special case of the Auger emitters

Paragraphs 54-55: No comment.

2.3 Different uses of the concept of RBE

2.3.1 RBE as a low-dose equivalence factor

Paragraphs 56-59: No comment.

2.3.2 Dependence of RBE on dose

Paragraphs 60-69: No comment.

2.3.3 Derivation of high-LET risk coefficient

General comment: Starkly contrasting with photon exposure, a fundamental problem for neutron exposures lies in the lack of any adequate human epidemiological data. Until quite recently it had seemed there might be some slight hope that studies of the Hiroshima A-bomb survivors would provide some clue to the risks associated with neutron exposure and of the neutron RBE (Hunter and Charles 2002a, 2002b). Thermal neutron activity measurements had suggested a significant underestimation of neutron doses. These hopes have, however, been dashed by a recent review of the neutron dose data by Straume *et al.* (2003) who confirmed the DS86 calculations that neutrons contributed only 1—2% of that total absorbed dose to the survivors. The conclusion appears to be that no useful information on neutron risks may be obtained from the Hiroshima study (Little 2003).

These developments are not addressed in ICRP Publication 92, but see paragraph 334.

Paragraphs 70-81: No comment.

2.3.4 Derivation of risk under specified conditions

Paragraphs 82-86: No comment.

3. QUANTIFICATION OF RBE

General comment: The organisation of this section might be much improved for the understanding of the non-specialist in radiobiology. This reviewer makes no claim to having any expertise in radiobiology but has had some

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experience in teaching and writing and hopes that these comments will be accepted in the spirit intended, which is in hopes of improving the clarity of the section.

Subsections 3.3 and 3.2 provide a helpful primer on the different approaches that are possible in extracting from experimental RBE data the information most appropriate for application to radiological protection but they could be better organised.

The following sections, which discuss specific RBEs applicable to specific radiations (neutrons [3.3], protons [3.4], and heavy ions [3.6]) lack a consistent structure. The paragraphs vacillate between discussion of the data for experimental animals, biological end points, and biophysical discussions in a manner that is sometimes difficult to follow. Surprisingly there is no comparable subsection for photon/electron irradiation.

3.1. Two approaches to the determination of RBE_M

Paragraph 87: No comment.

3.1.1. The low dose method

Paragraph 88: As a general comment, for reasons discussed in the reviewer's comments on paragraph 211, it would be helpful if precise and correct comparisons between the radiation-weighting factors were made throughout the document. w_R is not comparable with the function $Q(L)$. w_R is an average factor, derived using $Q(L)$ and by the application of physics and mathematics, that applies to the whole body (although the body is ill-defined). w_R may correctly be compared with other average factors such as the mean or effective quality factor for the whole body (however weighted).

Paragraph 89: No comment.

3.1.2. The high dose method

Paragraphs 90-96: No comment.

3.1.3. Relationship to risk estimation for high-LET radiation

Paragraphs 97-100: No comment.

3.2. The use of life shortening in the determination of RBE_M

Paragraph 101, 3.2.1 Advantages, 3.2.2 Disadvantages: No comment.

3.2.1 Advantages

No comment.

3.2.2 Disadvantages

No comment.

Paragraph 102: No comment.

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3.2.3. Determination of RBE_M for life shortening

Paragraphs 103-113: No comment.

3.2.4. Radiological concerns

Paragraphs 114-115: No comment.

Paragraph 116, last sentence: More precisely, in terms of the ambient dose equivalent $H^*(10)$ (or $q^*[10]$ in the revised notation used in this report).

Paragraphs 117-119: No comment.

General comment: Why is there no separate section for photons/electrons?

3.3. Neutrons

3.3.1 Mice

Paragraphs 120-122: No comment.

Paragraphs 123, lines 12-13: “. . . no evidence to suggest that the RBE may differ between lethal and nonlethal tumours in mice.” It would be helpful if this statement were supported with some rational discussion. The entire section reveals high variability in RBEs in mice and rats. The numbers range widely. How is this statement supported? Perhaps it is common knowledge within the radiobiological community?

Paragraph 124, penultimate sentence: Superficially this seems to be a silly sentence. It reminds one of the discussions in school, when we were introduced to the differential calculus and fretted over the concept that quotient $0/0$ was indeterminate. Why are the observations of the health of the mice exposed to low doses insufficient to preclude any chance that RBE_M might be infinite? I assume this means that it cannot be shown how the dose-effect curve approaches the origin at zero dose? It would be helpful to explain the inference of the sentence. Perhaps it should be deleted?

Paragraph 125: No comment.

3.3.2 Leukæmia and lymphoma

Paragraphs 126-129: No comment.

3.3.3 Rats

Paragraph 130: Does the degree of lethality vary between or within species, or both?

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Paragraphs 131-133: No comment.

3.3.4 In-vitro neoplastic transformation

Paragraphs 134-136: No comment.

3.3.5 Chromosome aberrations

Paragraphs 137-139: No comment.

3.3.6 Mutations

Paragraphs 140-142: No comment.

Paragraph 143, penultimate sentence: The numerical data are available, why not give them?: The photon contributions to the total absorbed dose are 50% and 20% and neutron energies of 0.4 MeV and 1 MeV, respectively.

3.4 Protons

Paragraph 144: No comment.

Paragraphs 145 and 146: These paragraphs seem to suggest a causal relationship between the (political?) pressure to reduce the w_R for protons and the estimated fraction of the total dose contributed to aircrew by protons. The proper reason is, of course, consistency in the scientific data. Some rewording would remove this doubt. The ICRP should be clearly seen to be above political pressure. A fuller discussion of the three data sets cited together with a reference to CEC recommendations would therefore be helpful.

Paragraph 147: Change q^* to $q^*(10)$ (three times). See general comments about proper definition of the ambient dose equivalent and changes in symbols.

Paragraph 147, last sentence: Isn't it possible to be certain about the reason for the factor of 0.8? As it stands the text is much too tentative. Change $H^* \Rightarrow H^*(10)$.

Paragraph 148: No comment.

3.4.1 Biophysical considerations

Paragraphs 149, 150 and 150, Table 3.6: It has been customary to designate average Q s by the symbol \bar{Q} . See the general comments on symbols throughout this report. Why unnecessarily confuse the reader?

Paragraph 151, line 3: Typo? What "largest value?"

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Paragraph 152: Why is the change in w_R to the value 2 only for “cosmic ray protons?” Surely one proton is as good as another whatever its source, and it was intended that all protons of like energy were to be included?

The values for w_R at high energies ($> \sim 100$ MeV) of 2 for protons and 5 for neutrons are incompatible.

3.4.2 Radiobiological data

Paragraphs 153- 157: No comment.

Paragraph 158: It is inconsistent for the recommended values of w_R for high-energy protons and high-energy neutron w_R to be 2 and 5 respectively. At high energies they should approach the same value.

3.5 α -particles

Paragraphs 159- 161: No comment.

3.5.1 Lung cancer

Paragraph 162: The phrase “rock or soil with uranium” is peculiar. Uranium-bearing rocks or soil? Rock or soil with high concentrations of uranium?

Paragraphs 163- 172: No comment.

3.5.2. Bone sarcomas-RBE of α versus β rays

Paragraphs 173- 180: No comment.

3.5.3. Leukaemia

Paragraphs 181- 182: No comment.

3.5.4. Liver

Paragraphs 183: No comment.

3.5.5. In-vitro neoplastic transformation

Paragraphs 184- 186: No comment.

3.5.6. Chromosome aberrations

Paragraphs 187-188: No comment.

3.6 Heavy ions

Paragraph 189: Incomplete discussion. An increasing number of humans have the potential to be exposed to “significant” fluences of heavy ions at particle accelerators.

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3.6.1. Tumours

Paragraphs 190-193: No comment.

Paragraph 194: This paragraph once again reveals a basic misunderstanding of the differences between $Q(L)$, \bar{Q} , and w_R . The comparisons between $Q(L)$ and w_R are misleading, if not specious. Both w_R and \bar{Q} “reflect the entire LET spectrum of the particle and its secondaries created within the body.” Naturally any particular value of $Q(L)$ is defined for a particular value of L but \bar{Q} is calculated by integrating over the entire LET spectrum., as is w_R .

Paragraphs 195-204: No comment.

3.6.2. In-vitro neoplastic transformations

Paragraph 205: No comment.

3.6.3. Chromosome aberrations

Paragraphs 206-207: No comment.

3.6.4. Mutations

Paragraph 208: No comment.

4. WEIGHTING FACTORS FOR RADIATION QUALITY

Paragraph 209. There is a direct and clear relationship between the kinetic energy of a charged particle and its linear energy transfer. Thus in principle these two “reference parameters” are not different; one defines the other.

The last sentence leaves the reader hanging because it is not supported by citations to the literature and it is not clear what appropriate “numerical conventions” ICRP have in mind. However, for reasons of necessity, precise dosimetry cannot tolerate any numerical convention that ignores differences of a factor of 2-3 in the value of a quantity determined by alternative methods. Better by far that ICRP should recommend only one method for its determination and one that conforms to the laws of physics and mathematical logic (see comments on paragraphs 7 and 212).

Paragraph 210, last sentence. The difference between precision and absolute accuracy should not present a difficulty provided that it is properly explained.

ICRP recommends a model for the assessment of detriment. Because of intrinsic uncertainties in the fundamental science the model is imperfect. The model has two components: physical and biological. Dosimetrists determine the physical components of this model with high precision and good absolute accuracy. The biological components of the model necessarily have poor absolute accuracy.

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However, the recommendations concerning the radiobiological components of the model made by ICRP must by convention be accepted as perfect for their duration, flawed though they might be revealed to be by the subsequent development of understanding. As Kellerer (1990) has pointed out it is possible, and indeed necessary, to achieve “rigour within uncertainty.” With the greatest respect this should be the goal of ICRP recommendations.

It would have been helpful if ICRP had provided examples of the “certain circumstances” that, in the opinion of the commission, require the assessment of values the protection quantities “with a degree of precision beyond the accuracy of the underlying radiological information”—perhaps ICRP had legal and administrative constraints in mind?

Paragraph 211. This paragraph illustrates a basic logical problem that underlies and bedevils the discussion of the quantities $Q(L)$ and w_R throughout this report. It is essential to realise that $Q(L)$ and w_R are not the same type of concept and therefore the terms are not interchangeable.

The definition of $Q(L)$ makes possible, by the application of physics and the integral calculus, average or effective quality factors, \bar{Q} , for a small volume of tissue, \bar{Q}_T , an entire organ, \bar{Q}_{organ} or indeed the entire human body, \bar{Q}_{wb} . The $Q(L)$ - L relationship is but one of many sets of information required for such a complex calculation.

The $Q(L)$ - L relationship may be used to generate what have been variously called “average,” “mean,” or “effective” quality factors for small masses of tissue, for tissues or organs, or even the whole body.

ICRP defines the tissues and organs, T , of significance for radiological protection and there is a corresponding value of \bar{Q}_T for each and every one. In complex irradiation conditions (high energies, neutrons) these values of \bar{Q}_T may be different for every organ (for example see ICRP Publication 74/ICRU Report 57; ICRP 1997; ICRU 1998).

Thus there is an entire family of \bar{Q} s (“average radiation-weighting factors” if ICRP prefers) whose individual values depend upon many parameters such as phantom, tissue or organ, irradiation geometry, incident radiation type and energy, etc. These parameters must be fully specified in any rigorous dosimetric scheme.

The family of “average quality factors” includes the $Q^*(d)$ ($q^*[d]$ of ICRP 92) of the ambient dose equivalent. Simply put, $Q^*(d)$ is a specific member of the family of mean quality factors, \bar{Q} , and is a rigorously defined \bar{Q} obtained by calculations or measurements in the ICRU sphere. By extension $Q^*(d)$ is defined to be given by $H^*(d)/D^*(d)$, (ICRU 1985, 1988, 1993).

In turn the ambient dose equivalent, with only minor modifications, became the radiation-weighting factor, w_R , of Publication 60. w_R is defined only for the whole body but with no definition of the phantom in which it is to be calculated (but, by inference, the ICRU sphere is recommended as a last resort). In turn w_R must therefore be regarded as a member of “the family of average quality factors.” There is already then an existing indirect and complicated link between $Q(L)$ and w_R but one that cannot be simply expressed in functional form.

With respect to terminology ICRP now seems to prefer the term “radiation weighting-factor” for what in the past has been termed “average,” “mean,” or “effective” quality factor. Whatever term is used it is imperative that a

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clear definition be given as to the manner by which the factor be determined and how it is to be applied (e.g., specific tissue/organ, whole body, phantom, etc.).

As to any possible relationship between any member of the family of “radiation weighting-factors” it is important to reiterate that, by definition, both \bar{Q} and w_R are not direct functions of L . Furthermore, it is clear that no simple or self-evident relationship exists between any member of the family of “average radiation-weighting factors,” including w_R , and some weighted average linear energy transfer of the incident radiation, \bar{L} . Both the definition of such an average quantity is far from clear and its utility for application to measurement is uncertain. In any event, such a relationship will generally be complex, particularly for neutrons and all high-energy radiations. Empirical relationships, if needed, could be derived for specific conditions. In the view of the author it would be better if ICRP were to focus its efforts on clearly defining the concepts necessary to determine such relationships (e.g., unequivocal definitions of effective dose, the $Q(L)$ - L relationship, and the approved anthropomorphic phantoms) and leave it to those whose task it is to determine values of effective dose to decide which such relationships, if any, are needed in practice.

There is one exception to this generalisation. In the special case of low-energy photon irradiation all \bar{Q} s and w_R have been defined to have the value unity. However, at high energies, when photons and electrons have sufficient energy to induce nuclear interactions the assumption $\bar{Q} = w_R \equiv 1$ must be unphysical. This oversight should be corrected.

Given precise definition of the phantom, modern computational techniques make it possible to explore, in an empirical manner, the numerical relationship between \bar{L} and w_R for specific sets of irradiation conditions. As we have seen, such a study is trivial for low-energy photons. For more complicated cases it is probable that a small number of calculations might reveal patterns in the data sets that will facilitate reasonable average values of w_R , however defined, to be selected that may be useful if great precision is not required. If problems similar to those created by ICRP 60 are not to be repeated it is prudent, if not imperative, that such a study be performed before values of w_R are selected.

Paragraph 212: This reviewer finds the dual system of radiation protection quantities unhelpful (Lindell and Thomas 2001; Thomas 1997, 2001, 2003, 2004).

It is respectfully suggested that ICRP avoid controversy by defining only protection quantities, which are determinable (but not necessarily measurable) but not expressing any partiality for particular techniques of determination (see comments on paragraph 7).

4.1. Issues relating to the concepts of w_R and Q

Title: Change Q to \bar{Q} (see general comments on symbols throughout this review).

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4.1.1. Origin of the current choice of $Q(L)$

Paragraph 213: Any incoherence, if it exists, is between \bar{Q} and w_R , both of which have a complicated dependence on \bar{L} (see comment on paragraph 211). For reasons repeatedly stated no coherence or incoherence can exist between $Q(L)$ and w_R because they are not directly comparable.

Paragraphs 214-216: No comment.

Paragraph 217: Does the phrase “in spite of these modifications” of have any value? It has a pejorative flavour— suggest deleting it.

Paragraph 218: The terminology is incorrect. w_R was intended to replace the \bar{Q} s calculated for the “whole body” with the aid of the $Q(L)$ - L relationship. As explained $w_R \neq fn(L)$ (see general comments and paragraph 211). Thus the use of a symbol $w_R(L)$, or similar, implying that w_R is an overt function of L would be misleading.

4.1.2. The need for computation and measurement in radiation protection

Paragraph 219: It really shouldn't be necessary to apologise for the indispensable nature of measurements in dosimetry!

Paragraph 220: This seems to be unnecessarily defensive! However, see the comment in paragraphs 7 and 212.

Paragraph 221: See paras.211 and 218. w_R is not a simple function of L .

4.1.3. A peculiar feature in the definition of w_R

Paragraphs 222-224: No comment.

Paragraph 225: The first step that could eventually have unified the treatment of both “internal” and “external” exposure was taken with the definition of the effective dose equivalent, H_E (ICRP Publication 26, 1977).

Paragraph 225 gives a good description of one the fundamental problems introduced by ICRP 60. If both “internal” and “external” are to be consistent the radiation-weighting factor must be determined at the receptor (tissue or organ).

There is naturally no objection to the recommendation of “approximations” to radiation-weighting factors if they might be helpful but, with respect, the ICRP may wish to enquire of the operational dosimetrists how useful these approximations might be in some cases. There are indications throughout the document, and elsewhere in the literature, that for neutron and other high-LET radiation exposures, “approximate radiation-weighting factors” may be of limited interest, particularly at high energies.

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In any event paragraph 225 makes an excellent point when it comments that tedious arithmetic, which perhaps the “approximate radiation-weighting factors” are designed to avoid, is no longer of any practical concern and therefore makes their recommendation of limited practical value for high-LET and neutron dosimetry.

There also remains the added complication of to which measure of absorbed dose the “approximate radiation-weighting factors” are to be applied. At high energies, particularly when radiation equilibrium has not been established, both the absorbed dose and LET-spectrum vary within the body (see for example ICRP Publication 74, figure 49, and comments on paragraphs 211 and 254).

Paragraph 226: The question that this paragraph attempts to discuss here is an important one but, alas, this reviewer finds its logic convoluted.

The paragraph begins by asserting that “primarily, E is related to the body of a specific person,” presumably some Christopher Robin or Mary Poppins, but ends with a muddled comment that this specific person “is not sex specific . . .” followed by some attempts to explain that female humans have two breasts and male humans almost none (*sic!*). With respect the ICRP should address this important question with vigour and much greater clarity than exhibited here.

This reviewer is sceptical that data obtained from epidemiological studies can correctly be said to relate to a specific person but must surely relate to the stereotypical clone that represents the intended population.

4.1.4. Computation of E or H_T

Paragraph 227: It is worth pointing out that there is also a great body of information on “the equilibrium of the radiation field,” referred to in this paragraph with respect to “aviation altitudes,” which has been accumulated at accelerator laboratories around the world. Indeed, here is a source of information scarcely acknowledged and unused in this report. The ICRP is urged to consider reviewing this material before it issues its final recommendations that will impact neutron and other high-LET radiations and radiological protection over a much broader field than civil aviation alone—at accelerator facilities in particular.

This reviewer would not wish the last sentence of this paragraph to be misinterpreted. Although the “bilocality of H_T ” may not *pose a critical arithmetical problem* it does present *a critical logical problem* which, in this reviewer’s opinion, should be resolved in the new recommendations.

Paragraphs 228 and 229: The accelerator laboratories have found practical solutions to several of the issues discussed here, and an understanding of and reference to this body of work might be worth mentioning with appropriate references here.

The statement on lines 8-10, “However, whenever the external field is composed of different types and energies with different values of w_R , there is still no way to determine E or H_T from the measured data,” is incorrect.

4.1.5 Consequences

Paragraph 230: Excellent paragraph!

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Paragraph 231: The correct symbols are $H_p(d)$, $H^*(d)$.

Paragraph 232: See comments on relation between $Q(L)-L$ and w_R in paragraph 211 and throughout these comments.

4.2 Effective dose: field or receptor quantity?

Paragraph 233: No comment.

4.2.1. Reference to the external field

Paragraph 234, last line: The correct symbol for ambient dose equivalent is $H^*(d)$. Why keep changing established precedents for no apparent reason?

Paragraphs 235-6: No comment.

Paragraph 237: Interestingly the argument against referring the primary dose-equivalent quantity to the external field also may be used in support of unifying “internal” and “external” dosimetry by using the $Q(L)-L$ relationship and the local radiation field resulting from external exposure at the tissue/organ of interest to determine the \overline{Q}_T for every tissue/organ. The logic of the document *vis-à-vis* “external” and “internal” dosimetry is perhaps inconsistent here.

4.2.2 Reference to the internal field

Paragraphs 238-239: Presumably it is suggested w_R is loosely related to the $Q(L)-L$ relationship via the appropriate \overline{Q} (but see paragraph 211 and elsewhere).

The comment in paragraph 239 that “certain required numerical values were not available at the time” (of the preparation of ICRP 60) to obtain coherence between w_R and $Q(L)-L$ serves to reinforce this reviewer’s warning that “the consequence of simultaneous changes . . . , as were made in Publication 60, was unpredictable and this episode should caution against taking important policy decisions without first having the consequences of a proposed policy completely understood before making it public.”

4.3. Neutrons

Paragraph 240, third sentence: Does the “it” refer to “the case of high-energy protons?” If so what is the difference between “merely the choice a realistic RBE value” for neutrons or protons? ICRP knows that this choice is more complex than implied here.

4.3.1 The two components of neutron dose

Paragraph 241: See comments on paragraph 20.

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Paragraph 242: No comment.

Paragraph 243, first bullet, line 1: “Moderated” is rather like “pregnancy”—it cannot be qualified. Thermal neutrons are produced (giving rise to 2.2 MeV photons by the $[n \gamma]$ capture process) even in rather small volumes of moderator. For a thermal neutron the moderation is not “partial” but complete.

Paragraph 244: The discussion of paragraphs 244—250 is helpful in understanding the problems with using the ICRU sphere as a surrogate phantom for the human body. This reviewer believes that the discussion could be improved by addressing the following comments.

This reviewer thinks that the notation D_n for the high-LET charged-particle component of the absorbed dose and to call it the “genuine neutron dose” is highly misleading . This dose is neither more nor less a “genuine” neutron dose than is the contribution from the photons (that is actually deposited by electrons).

It is much simpler to view the deposition of dose by uncharged radiations by the creation of charged particles having a broad LET spectrum.

Paragraph 245: No comment.

Paragraph 246: The correct symbol is for organ-averaged absorbed dose is $\overline{D_T}$.

Paragraphs 247-250: No comment.

4.3.2 The origin of the choice of the radiation-weighting factor for neutrons

Paragraph 251: The paragraph correctly points out that there is no explicit comment on the derivation of the numerical values of w_R in ICRP publication 60. However, the facts are generally known (Thomas 2004) and are as follows:

ICRP recommended an amended $Q(L)$ - L relationship, to account for then-current fission-neutron spectra RBE data and this new relationship was used to determine values of \overline{Q} at a depth of 10 mm in the ICRU sphere ($Q^*[10]$) (paragraph A14, Publication 60). With only minor changes, the values of $Q^*(10)$ were renamed “radiation-weighting factors,” w_R (see also paragraphs 251 and 252 of ICRP Publication 92).

Paragraph 252: With respect to the continuity of the defining function for w_R see comments to paragraphs 6, 9, 282 and 367. ICRP has been around this track a few times now!

It is not surprising that the values of $Q^*(10)$ are similar (almost identical) to those of w_R (see paragraph 251). Neither is it surprising that for neutrons the values of $Q_E(q_E)$ are smaller than $Q^*(10)$ in the 1 MeV energy region: “ H_E took account of the dose distribution of absorbed dose within the human body rather than, as hitherto in the case of external exposure, merely searching for the maximum dose equivalent within a crude representation of the human body. Some assessment of the magnitude of the overestimation that resulted from use of the MADE was

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made possible by comparing the conversion coefficients provided in ICRP Publication 51 (ICRP 1987). For irradiation by neutrons in antero-posterior geometry and within the energy range 10 keV up to 1 MeV, the coefficients for H_E are a factor of 2-3 lower than corresponding values for MADE. Above 1 MeV the overestimation declines until at about 10 MeV the coefficients are equal” (Thomas, 2003).

Paragraph 253. The definition of q_E (equation 4.6) now links the parameters w_T and $Q(L)$. In ICRP 60 w_T and w_R , as paragraph 289 confirms, were intended to be independent quantities; i.e., the value of one quantity is not influenced by the value of the other (see paragraph 289 of the document). The independence of tissue weighting and radiation weighting is now lost by the definition of q_E in paragraph 253. At least a conceptual and logical problem is introduced if the “radiation-weighting” factor is dependent upon tissue-weighting factors?

It might (will be) argued that changes in the values of w_T are unlikely to occur frequently or that such changes are unlikely to have large numerical impact, or both. Nevertheless, the desirable goal of “rigour within uncertainty” may only be achieved by the application of rigour to our definitions and this objection does call into question the wisdom of introducing yet another weighting factor to add to the general confusion.

In this reviewer’s opinion, such discussions merely emphasise that the LET-dependent weighting factor $Q(L)$ - L relationship must be clearly identified by ICRP as the primary model (convention), because it is independent of other factors, such as w_T . By contrast, q_E is a derived quantity, which depends on many factors.

Paragraph 254. ICRP Publication 74 provides sufficient data to show that the effective dose per unit fluence is strongly dependent upon the angular distribution of the irradiating neutron field. If w_R is to be defined to be independent of the directional distribution of the radiation field, errors of as much as a factor of 2 in the estimation of E are possible at some energies. It would be helpful if ICRP were to seek advice from neutron dosimetrists as to whether a concept such as w_R , so defined, is of any utility in their work.

Although some 25 years ago average radiation-weighting factors were of great value to the “critical organ-MADE” system of protection for neutrons, they are of much less interest at high energies or in more sophisticated detriment models, such as effective dose equivalent, H_E , and effective dose, E , which specify tissues and organs that facilitate the use of anthropomorphic phantoms. At high energies, and particularly at accelerator laboratories, there is more interest in using conversion coefficients that relate field quantities (e.g., fluence) to determine the radiological protection quantities. (See also paragraphs 211 and 225.)

Paragraph 255, lines 1-3: These lines point out the dangers for ICRP of not obtaining adequate data before making its recommendations. It is now clear that the calculations described in ICRP 75/ICRU 57 should have preceded the publication of, and been used as a guide to shape, the recommendations of ICRP 60. Had this been done perhaps many of the inconsistencies discussed in ICRP 92 might have been avoided. It is to be hoped that the mistakes of the past will not be repeated in 2005. There are probably now sufficient data available to enable wise judgements to be made that will facilitate the development of a logical and coherent system of radiation-weighting dosimetry that will be of utility at all energies.

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Paragraph 255, lines 7-8: These lines reverse the process of logical analysis. If a desired value of q_E is required by the radiobiology (e.g., about 20 for 1 MeV neutrons) then the function $Q(L)$ must be correctly chosen to suit. If this is done values of q_E calculated for an anthropomorphic phantom will, if the arithmetic is done correctly, automatically be acceptable.

4.4. Options for a modified convention

Paragraph 256: This is a crucial issue. Three options are proffered in ICRP Publication 92 but there is a fourth option not addressed here:

- Revision of the $Q(L)$ - L relationship in such a way that values of q_E calculated with its aid are in consonance with current radiobiological judgement.

This fourth option makes possible the development of a universal and coherent system of dosimetry (all radiations, at all energies whether tissue is exposed to ionising radiation originating from outside or inside the human body).

4.4.1 Radical simplification of w_R

General comment: The word radical (from the Latin *radix*, a root) means of or from the roots and, in the sense used here, favouring fundamental change. The root cause of the “difficulties” with w_R and q_E is that the ICRP’s $Q(L)$ - L relationship is not consonant with ICRP’s specific requirements placed on the value of w_R (or q_E) for fission neutrons.

Paragraph 257: Of course! “The system of radiological protection quantities . . . must be stable” but to be blunt the history of the radiological protection profession has been abysmal in this regard. Radiation protection quantities and their symbols have changed with monotonous regularity over the past 50 years. In many cases, as in ICRP Publication 60, these changes have not been warranted by the emergence of any new significant information or by the shedding of any new light.

This reviewer makes this comment, not to cause annoyance, but to place emphasis on the extreme importance of “getting it right” this time around if ICRP is not to lose credibility.

A shift in thinking might be helpful. The telescope should be reversed. Approximations to the simple can only be made if the complex is understood. If a dosimetric system is devised that adequately serves the needs of high-energy, high-LET radiations, then the approximations that may be made to satisfy the simpler demands of low-energy, low-LET radiations will be facile.

Time is surely of the essence. There will inevitably be great pressure on ICRP to complete its revisions as quickly as possible. Nevertheless it is more important to be correct and honest in one’s science than to hurry to a timetable—whatever the external pressure. Radiation protection cannot continue to change its vocabulary every decade or so. Any changes to be recommended by ICRP in 2005 must be based on an adequate database to permit wise decisions and the author believes that sufficient information is available to resolve the issues raised in this review. There are probably now sufficient data available to enable wise judgments to be made that will facilitate the development of a logical and coherent system of radiation-weighting dosimetry that will be of utility at all energies.

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The scientific input to the standards-setting process is necessarily a delicate balance between the ivory tower of academia and the practical concerns of the workplace. Many, including this reviewer, believe that it is the latter that should lead in the development of radiation protection recommendations and standards policy.

Paragraph 258: This reviewer does not understand the logic of this argument. After all, the development of w_R is just such an attempt “to simplify one single aspect of an otherwise complex system”—*n'est-ce pas?*

Paragraph 259. It is argued that “radical simplification of w_R seems impracticable for the reason that it would tend to force tightening of the dose limits in general. If the current value of 20 for fission neutrons were reduced to 10, this would decrease the numerical value of the effective dose from exposure to fission neutrons by a factor of 2. This would amount to a relaxation of limits for neutron exposure, which may meet strong objections and would almost certainly generate pressure to offset the change by a decrease of the effective dose limits, which would then apply to all radiations including photons.” One wonders why a proposed reduction in w_R by a factor of 2 for 20 keV neutrons is acceptable but is so egregious for fission neutrons. ICRP appears to have two determiners of radiation-weighting operating—a sure recipe for ambiguity.

If the primary radiation-weighting generator, the $Q(L)$ - L relationship, is “correct” in the sense that it represents present best judgement, then the laws of physics and mathematical logic will inevitably generate proper values of \bar{Q} , $Q^*(10)$, and q_E under the irradiation conditions specified.

Using the $Q(L)$ - L relationship recommended in ICRP Publication 60, the calculated values of the organ-weighted effective quality factor, q_E , at a neutron energy near 1 MeV is about 65% of values of $Q^*(10)$ and about 60% of the value of w_R given in ICRP 60 (see fig. 4.4). This is thought to be unacceptable to ICRP who apparently wishes to retain a radiation-weighting factor of about 20 at ~ 1 MeV.

There are two possibilities: either the $Q(L)$ - L relationship is incorrect or this insistence on a whole-body radiation-weighting factor of 20 is incorrect.

If it is to avoid ambiguity ICRP cannot have two determiners of radiation weighting. If ICRP chooses the presently recommended $Q(L)$ - L relationship, a value of $q_E=10$ at 1 MeV must be accepted. Conversely if ICRP wishes a value of $q_E=20$ at 1 MeV, it must modify its currently recommended $Q(L)$ - L relationship. The degree to which the radiobiology permits this to be done is a matter for the judgement of ICRP. It may be significant that ICRP 92 makes no such recommendation.

The data at 1 MeV are interesting. In the absence of data calculated in an anthropomorphic phantom, those writing ICRP Publication 60 may have felt that the fair agreement between the peak value of the point function $Q(L)$ - L (about 30), the value assigned to $w_R(20)$, and the somewhat smaller the value of $Q^*(10)$ buttressed one another within the prevailing uncertainties. With hindsight it now seems clear that the influence of averaging was not fully understood. For the organ-weighted value of \bar{Q} for the entire human body, as defined by Kellerer, q_E has a value of about 13. The dilemma for ICRP is that values of q_E appear to be correct at low and intermediate neutron energies. If the values are correct here, one may ask why should they be incorrect at 1 MeV?

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Changing the $Q(L)$ - L relationship to have suitable values around 1 MeV, with perhaps a peak value of 35-40 at about 1 MeV, may not be inconsistent with our present understanding of the radiobiology, and it would take very few calculations to obtain the optimum $Q(L)$ - L relationship required. This proposal would then enable the calculated values of q_E to be used as the radiation-weighting factors, which Kellerer and his colleagues clearly believe to be the correct approach.

The primary issue then becomes this: Is the $Q(L)$ - L relationship needed to obtain suitable values of radiation-weighting factors consistent with the best relationship that judgements of radiobiologists can give us at this time? Only when the answer to this question is “yes” can ICRP be satisfied with its recommendation.

It would be helpful if ICRP were to review the recommended $Q(L)$ - L relationship in the light of the radiobiological data summarised (summarised, for example, in ICRP Publication 92 and NCRP Report 104 [NCRP 1990]). A figure showing the recommended function with estimated uncertainties would serve as a warning to those who might place unwarranted confidence in the recommended function.

Paragraph 260: Foul! The logic becomes confused here by the use of the word “radical” to mean a specific “straw-man” described in paragraph 257.

4.4.2 Modification of w_R to establish coherence with $Q(L)$

Paragraphs 261-265: See the alternative solution for coherence suggested in the comments on paragraph 259.

4.4.3 The proposed modification a moderate change of w_R

Paragraphs 266-272: These are some of the most troublesome paragraphs in the entire document. In the opinion of this reviewer they lay the foundation for dissent and debate in the years ahead if their advice is adopted by ICRP. Equation 4.7 has the appearance and reality of a “fiddle factor” just to make things come out right and will draw justified criticism if adopted by ICRP. Furthermore things don’t come out right at the higher energies for neutrons, where it would be expected that w_R or q_E approaches the value for high-energy protons, selected to be 2 (paragraph 373).

4.4.4. The special case of high altitude and space radiation

Paragraph 273: Neutrons of very high energy are also of particular interest at high-energy accelerator facilities.

Paragraph 274: No comment.

Paragraph 275: Photons and electrons are defined to “pose no problem with regard to the weighting factors” and so the phrase is tautological.

Paragraph 276: No comment.

Paragraph 277: Presumably “cosmic protons” are just the same as any other protons?

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Paragraph 278: Does “the concept of w_R can be unnecessarily complex in high-energy fields” translate to “of practical utility?” If so, many dosimetrists at particle accelerator facilities might agree. In this environment fluence-to-dose conversion coefficients are often of greater utility.

Paragraph 279: No comment.

Paragraph 280: No comment.

Paragraph 281: no comment.

4.4.2. The special case of particle accelerator radiation environments: DOES NOT EXIST

There is a lacuna here. There is no mention of accelerator radiation environments that are just as relevant as high-altitude radiation and for which there is a great body of information. Some solutions to some of the issues raised in this report may be found in these data

4.4.5. The continuous and discontinuous convention

Paragraph 282: This reviewer endorses the common sense of this paragraph. See comments on paragraphs 6, 9, and 367. “*Déjà vu* all over again?”

4.4.6 Role of the operational quantities

Paragraph 283: ICRP recommends dose limits expressed in terms of the protection quantity effective dose, E , and it is generally agreed that E is not “measurable” but it is, nevertheless determinable. ICRP recommends that compliance with limits be monitored by the use of the operational quantities.

The principal objection to the “operational quantities” is that, perhaps inadvertently, a double standard has been established by their introduction (Thomas et al. 2002). This dichotomy has led to some differences of opinion concerning dosimetric issues but a major recent difficulty is that the radiation protection establishment is now defending this double standard. For example, Lindell suggests that this dichotomy was the deliberate wish of ICRP, and that there are indeed two types of “quantity” necessary for radiological protection: one set for operational purposes (which consists of measurable physical quantities) and one set that is used for assessment and as a basis for deriving authorised limits. Furthermore, the uses of these two quantities differ, the former being using retrospectively while the latter is used retroactively. Lindell concludes that E was deliberately and consciously defined in a relaxed manner “so that the calculated magnitude of effective dose is somewhat ambiguous. However, this is in line with the commission’s generous and appropriate recommendations on accuracy” (Lindell and Thomas 2001). Others have the opinion that an administrative system for radiological protection requires protection limits clearly stated and defined in measurable quantities.

This reviewer respectfully submits that this dichotomy is, at best, a distinction without a difference and unnecessarily confusing. Effective dose has been embodied in the legislation or regulations of some countries and

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international organisations. Legislators and the courts do not differentiate between “operational Sieverts” and “assessment Sieverts.”

This represents a serious difference of opinion within the radiation protection profession that urgently needs resolution. It is to be hoped that this may be achieved in the new recommendations of ICRP. The cost of continuing confusion could be a loss of credibility.

It would be helpful if the commission could be persuaded to avoid further controversy over the dual system of radiation protection quantities by softening its apparent *imprimatur* of the ambient dose equivalent. Although the ambient dose equivalent is of great utility in some aspects of dosimetry there are significant problems with its application to neutron dosimetry, particularly at high energies (Ferrari 1998, 2004; ICRP 1997; ICRU 1998; Pelliccioni 2004). It may not be wise to endorse ambient dose equivalent to the exclusion of alternatives.

The commission might wish to consider ambient dose equivalent to be one weapon in an armory that consists of many alternatives, any of which may be used as an operational quantity. Other techniques might, for example, include LET-spectrometry (ICRP 92, paragraph 278) and neutron- and charged-particle spectrometry (Moyer 1952, 1954; Thomas 2003).

Paragraph 284: No comment.

Paragraph 285: For higher energy radiation ICRU does suggest that values of $H^*(d)$ with values of $d > 10$ mm may be appropriate (however, see the publications of Ferrari and Pelliccioni).

Paragraph 286: Several calculations have been made of dose deposition in differing anthropomorphic phantoms and for various irradiation geometries. These calculations have been applied to the determination of coefficients to convert fluence to protection quantities. These data may be of value in guiding the choice of a standard anthropomorphic phantom(s). (For a bibliography see Thomas [2003]).

5. DETERMINISTIC EFFECTS

Read but not reviewed in detail.

5.1. Introduction

Paragraph 287: No comment.

Paragraph 288: No comment.

Paragraph 289: Note the important quotation from ICRP 60: “The w_R values are independent of the organ or tissue and of the stochastic effects considered . . .” See comments to paragraphs 4 and 253.

Paragraphs 289-297: No comment.

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5.2. ICRP Publication 58

5.2.1. Aim of the report

Paragraphs 298-301: No comment.

5.2.2. Linkage of RBE_m to cell killing

Paragraphs 302-306: No comment.

Paragraphs 307 a d 308: Check to see that the mathematical statements are clear here and consonant with equation 2.5.

Paragraphs 309-313: No comment.

5.2.3. Possible complexities

Paragraphs 314-323: No comment.

5.3 RBE for lens opacifications and cataracts

Paragraphs 324-329: No comment.

5.4 Non-cancer late effects

Paragraph 330: No comment.

6. CONCLUSIONS

6.1. Problems with the concept of RBE

Paragraph 331: No comment.

Paragraphs 332-333: Are these a veiled rejection of the concept of RBE-LET or $Q(L)$ - L relationships or merely a warning caveat?

6.2. Need to invoke experimental data

Paragraph 334: See general comments on section 2.3.3.

Paragraphs 335-336: No comment.

6.3. Two approaches towards the determination of RBE_M

Paragraph 337: No comment.

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6.3. 1. The low-dose method

Paragraphs 338-341: No comment.

6.3.2. The high-dose method

Paragraphs 342-343: No comment.

6.4. Other uses of RBE

Paragraphs 344-346: No comment.

6.5 Radiation-weighting factor-conceptual problems

Paragraphs 347 and 348: Again the confusion between $Q(L)$ and \bar{Q} (see general comments). Either a properly defined \bar{Q} or a properly defined \bar{Q} or w_R are needed for the translation of measurements to the required protection quantities E or H_E . Both H^* and H_p are incompletely defined here

6.5.1 Need for rigorous definitions and coherent concepts

Paragraph 349: No comment.

Paragraph 350: Amen!

Paragraph 351: Amen! Amen!

6.5.2 Impracticability of a radical simplification

General comment: the logic of this section seems confused and perhaps designed to support a policy of minimal change rather than radical change. See general comments on Section 4.4.1.

Paragraphs 352-354: No specific comments.

Section 6.5.3. The need for a formal relationship between w_R and $Q(L)$

Paragraph 355: This paragraph illustrates a troubling and consistent misuse of terms throughout the document. This reviewer submits that $Q(L)$ and w_R are horses of a different colour and, to mix his metaphors, no more comparable than is an egg to a chicken.

The $Q(L)$ - L relationship is, crudely speaking, a model defining an RBE for radiation protection as a function of L . By the use of some physics and mathematics and with the definition of phantoms, the $Q(L)$ - L relationship may be used to generate what have been variously called average, mean, or effective quality factors for small masses of tissue, for tissues or organs, or even for the whole body. Indeed the relationship yields the $Q^*(d)$ (q^* of ICRP 92) of the ambient dose equivalent that gave birth to the young chick who, after minor cosmetic surgery, became the radiation-weighting factor, w_R , of Publication 60.

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While the chicks are comparable they do not resemble the egg even though the egg is crucial to their existence. If I am correct this may represent a serious problem with ICRP thinking. In any event it is more serious than a minor semantic issue.

Paragraph 356: The symbols H^* and H_P are incomplete

6.6. Proposed convention for neutrons

6.6.1. The intended relationship between w_R and $Q(L)$.

Paragraph 357: See comments on paragraph 251.

Paragraphs 358-359: The argument embedded in this paragraph is perfectly correct. Two questions arise.

- First why weren't the "numerical data for q_E " obtained for the preparation of Publication 60? They easily could have been calculated. This point is raised not to annoy but to urge that a similar mistake, of making recommendations on the basis of insufficient data, not be made in 2005.
- Second, and more importantly, why weren't values of q_E adopted in this report (ICRP 92) if the $Q(L)$ - L relationship recommended by ICRP is correct?

6.6.2. Proposed modification of w_R

Paragraph 360: The logic here appears legalistic and obtuse. It seems to suggest that poor judgements cannot be remedied because they have been incorporated into "regulations." This devalues both the recommendations of ICRP and the regulations as being inconsequential.

Paragraph 361: The recommendations of Figure 6.1 for high-energy neutrons would bear scrutiny. The asymptote of $w_R=5$ may be too high.

Paragraph 362: ICRP might wish to consider carefully whether the heavy emphasis in this report on average radiation-weighting factors is of significant practical utility at high energies. Has the concept outlived its usefulness and does it still have any practical value? (These issues are hinted at in paragraphs 278 and 364. Paragraph 278 comments ". . . the concept of w_R can be unnecessarily complex in high-energy fields.")

The concept carries with it the burden of history. It derives from the necessarily rather crude concept of maximum dose equivalent, H_{max} , defined by

$$H_{max} = \bar{Q}D_{max}$$

where an assessment of the maximum absorbed dose was modified by a crude estimate of a radiation-weighting factor. With the definition of quantities such as effective dose equivalent and effective dose, the development of anthropomorphic phantoms and the facility of computational techniques more sophisticated models of the human body the function of w_R it is not clear. There also remains the added complication to which measure of absorbed

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dose the “approximate radiation-weighting factors” should be applied. At high energies, particularly when radiation equilibrium has not been established, both the absorbed dose and LET spectrum vary within the body (see for example ICRP Publication 74, figure 49, and comments to paragraphs 211, 225, and 254). To which measure of absorbed dose should the “approximate radiation-weighting factors” be applied?

6.6.3. Neutrons at aviation altitudes and in space

Paragraph 363: 100 MeV has not been referred to as a “very high energy” for several decades. It would be well to use the terms accepted by the physics community.

Paragraph 364: $w_R=6$ for 100 MeV neutrons is too high (see paragraphs 152 and 158).

Paragraph 365: However, there are radiation fields to which humans are exposed that are not in equilibrium; e.g., around particle accelerators, which are not adequately dealt with in this report.

Paragraph 366: No comment.

6.6.4. Adoption of a continuous dependence of w_R on neutron energy

Paragraph 367: Back to the ideas expressed in ICRP Publication 21 (1973)!!!

6.7. Proposed convention for heavy ions

It is not usual to refer to protons or alpha-particles as HEAVY ions. It would be well to use the terms accepted by the physics community.

6.7.1. w_R for protons

It is not usual to refer to protons as HEAVY ions.

Paragraph 371: No comment.

Paragraph 372: The ideas in this paragraph are poorly expressed. The fact that: “With $w_R=5$, the protons can contribute a major part of the effective dose” is not of itself a reason to lower the value of w_R . The germane issue is “what is the correct assessment of the relative contributions of the components of the irradiating field?” As this paragraph is presently written it gives an impression that ICRP is yielding to influences from interested parties, rather than making an objective assessment.

Paragraph 373: This is very unclear. There is a shuffling between the resulting mean quality factor, effective quality factor, and w_R leaving the reader perplexed.

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Paragraph 374: More detail would aid comprehension by the reader. Two different radiation fields appear to be discussed: “free” and “trapped.” These terms need explanation. Presumably the radiation belts are being discussed here when the term “trapped” is used?

6.7.2. Heavier ions

It is not usual to refer to alpha-particles as HEAVY ions. It would be well for ICRP to use the terms accepted by the physics community.

Paragraphs 375-376: No comment.

Paragraph 377, last five lines: It would, for reasons already made clear, be unwise to introduce the factor 1.6 (equation 4.7). This factor is required because the $Q(L)$ - L relationship used to determine q_E is inconsistent with the imposed value of w_R for ~1MeV neutrons to have a value of ~20 (see comments on paragraph 266-272).

6.8. Radiation weighting for deterministic effects

Paragraph 378: Surely in the *argot* of radiological protection it is the case that the absence of deterministic effects caused “by ionising radiation below certain dose thresholds” is a definition rather than an assumption?

Paragraphs 379-381: No comment.

6.8.1. Deterministic effects due to cell killing

Paragraphs 382-385: No comment.

6.8.2. Lens opacification

Paragraphs 386-388: No comment.

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