LETTERS TO THE EDITOR

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Concerning CERRIE’s conclusions and COMARE’s response on doses and risks from internal emitters

Dear Sir

As discussed in the last issue of this journal (Smith 2004, Wakeford, 2004a), two UK government committees have recently published reports on risks from internal emitters. The Committee Examining Radiation Risks of Internal Emitters (CERRIE) was set up in 2001 by the Committee on Medical Aspects of Radiation in the Environment (COMARE) at the request of the Department for the Environment, Food and Rural Affairs (DEFRA) and the Department of Health (DH). CERRIE operated independently from COMARE and produced an extensive report (CERRIE 2004) on which COMARE commented, giving advice to government (COMARE 2004). We wish to draw attention to the wording of the conclusions in the CERRIE report concerning uncertainties in dose and risk estimates for radionuclide exposures in comparison to the wording of the press release that accompanied publication. We also refer to analyses included in the COMARE response, comparing risks of exposure to external and internal sources of irradiation, since the impression may be given that this is the best information available.

CERRIE examined the methodology used by the ICRP to estimate dose and risk from internal emitters, considering epidemiological data, biological mechanisms, and biokinetic and dosimetric models for the behaviour of radionuclides in the body. At the end of this process, most members were prepared to accept that current estimates of dose and risk from internal emitters probably represent reasonable central values. However, all members considered that it would be important to consider uncertainties in these estimates in situations of occupational or environmental exposure in which doses were estimated to be a significant fraction of dose limits or constraints.

More work is required to provide reliable quantitative estimates of uncertainties in dose and risk for a range of important radionuclides. However, current estimates suggest that uncertainties in central estimates of equivalent dose probably vary from around a factor of 2 or 3 above and below the median in the most favourable cases to more than a factor of 10 in other cases, depending on the availability of data and the complexity of the radionuclide’s biokinetics and dosimetry. Such uncertainties are additional to uncertainties in risk factors. CERRIE (2004) concluded that more efforts should be directed to quantification of uncertainties in dose estimates for important radionuclides, with transparent identification of all underlying contributions to overall uncertainties and how to compound them. The view was that doses and risks from internal emitters should be calculated on the basis of best current information using central values but that, ‘where appropriate, these estimates should be accompanied by an explicit statement of the uncertainties involved’. ‘This approach would help identify those situations in which a precautionary approach might be appropriate, and was greatly to be preferred over one in which conservative or pessimistic estimates were arbitrarily introduced at various stages in the calculation’. This conclusion from the CERRIE report became: ‘Report calls for precautionary approach to internal radiation’ as the headline of the press release that accompanied publication (www.cerrie.org).
An important question considered by CERRIE was the equivalence of internal and external sources of irradiation and whether risk estimates based largely on the life span study (LSS) of cancers arising in the survivors of the atomic bombings at Hiroshima and Nagasaki are applicable to internal radionuclide exposures. Chapter 4 of COMARE (2004) presents ‘Additional reviews and analyses requested by COMARE’. The groups considered by COMARE, for comparison with LSS risk estimates, are Sellafield plutonium workers, workers at the Mayak plant in the former USSR, and residents of the Techa River region exposed to discharges from the Mayak plant. Comparisons are tabulated (risk ratios) for leukaemia and solid cancers in all three groups and lung cancer in the Sellafield plutonium workers and Mayak workers. The conclusion reached is that the risks of radiation-associated cancers among groups exposed to substantial quantities of internal emitters are compatible with those observed in the atomic bomb survivors. However, this conclusion is qualified by reference to the substantial uncertainties associated with the risk estimates and the absence of information on internal doses. It is only for lung cancer in the Mayak workers that there are published data on internal doses on which to base a risk estimate; further work is required to improve the reliability of these plutonium dose estimates (Romanov et al 2003, Wakeford 2004b).

While there are clear excesses of lung, liver and bone cancers in the Mayak workers that are attributable to plutonium (Gilbert et al 2000, Koshurnikova et al 2000, Kreisheimer et al 2003), leukaemia in these workers correlates with external dose and not plutonium dose (Shilnikova et al 2003). That is, it appears that plutonium is a weak leukaemogen, as reported in animal studies (Breckon and Cox 1990, Ellender et al 2001), and consistent with a low incidence of leukaemia in radium-exposed groups (Wick et al 1999, Nekolla et al 2000, UNSCEAR, 2000).

Fortunately, there are better human data providing evidence for equivalence of internal and external irradiation than those presented by COMARE. In a review undertaken for CERRIE, Harrison and Muirhead (2003) compared LSS risk estimates with data on:

- liver cancer and leukaemia in patients given intravascular injections of ‘Thorotrast’, a colloidal thorium oxide preparation (232Th, an alpha emitter), as a contrast medium for diagnostic radiology;
- bone cancer in patients given 224Ra for medical conditions; and
- lung cancer in uranium miners exposed to radon-222 and its immediate alpha emitting progeny, and in plutonium exposed Mayak workers.

While the uncertainties associated with dose and risk estimates for these alpha emitters were emphasised, it was concluded in the review that risk estimates obtained for lung and liver cancer, assuming an RBE of 20 for alpha particles, are consistent with those derived from the LSS data. For leukaemia, the available data suggest that the assumption of an alpha RBE of 20 can result in overestimates of risk. For bone cancer, it also appears that current assumptions may overestimate risks from alpha-particle emitting nuclides, particularly at low doses.

Of course, all these comparisons are based on exposures at high to moderate doses and questions remain concerning their applicability to low and protracted exposures to internal emitters. We welcome the CERRIE and COMARE conclusions that more attention should be paid to uncertainties in dose and risk estimates and, having identified priorities, that appropriate research funding should be made available.

Yours faithfully,

J D Harrison and J P Day
The CERRIE majority and minority reports and the COMARE response

Dear Sir

I would like to draw your readers attention to some points arising from the majority and minority reports issued by the Committee Examining Radiation Risks of Internal Emitters (CERRIE) [1, 2] and from the response of the Committee on the Medical Aspects of Radiation in the Environment (COMARE) [3] to the majority report. I am prompted to do this by the review of the three reports by Smith [4] and by your editorial about CERRIE [5]. I must make it clear at the outset that I am not an impartial observer. I was a member of the CERRIE secretariat but resigned because I did not like the way that the committee and its secretariat operated. I also believe that COMARE has outlived its usefulness and should be replaced. Letters reproduced in the CERRIE minority report [2] contain further details of my views.
My first point is that I find it difficult to understand why COMARE states that ‘CERRIE has made an excellent analysis of the uncertainties involved in making risk assessments’ [3] when so much of the CERRIE majority report [1] is about uncertainties in assessing doses from intakes of radionuclides, not uncertainties in assessing risks. Doses are, of course, only intermediate quantities in estimating risks and uncertainties in risks from intakes of radionuclides will always be larger than uncertainties in doses from these intakes. Furthermore, the definitions of some dosimetric quantities (especially effective dose) are questioned both by those who think that the International Commission on Radiological Protection (ICRP) greatly underestimates risks and by those who think it greatly overestimates risks.

Secondly, COMARE says that ‘we accept that current uncertainties (in risks) from internal emitters may be of the order of a factor of ten’ [3], but the discussions in the CERRIE majority report [1] imply that uncertainties in some risks from intakes of some radionuclides by some people are much larger than this. In particular, Chapter 2 of the majority report [1] implies that uncertainties in risks range from a factor of less than ten either side of a central estimate in situations where data are good (e.g. intakes of some radionuclides by adult workers), to a factor of a hundred or more either side of a central estimate in situations where data are poor. The discussions of the biological and epidemiological evidence in Chapters 3 and 4 of the majority report [1] indicate clearly that it is possible that the risks of some health effects from intakes of some radionuclides by some people could be more than a factor of a hundred higher than the current mainstream central estimate, while risks of other health effects from intakes of other radionuclides by other people could be more than a factor of a hundred lower than this estimate. The general conclusion on uncertainties to be drawn from the CERRIE majority report is surely that the factors by which risks from internal emitters could differ from ICRP’s general estimates range from less than ten to more than a hundred, depending on the radionuclide, health effect and type of person. It would also be possible to conclude from the report that the uncertainties in risks are greater for children and foetuses than for adults and that in these instances it is more likely that the ICRP general risk estimates are too low than that they are too high.

Thirdly, the recommendations in all three reports [1, 2, 3] seem to me to miss the point. What is most needed now is what CERRIE failed to provide, namely a thorough quantification of the uncertainties in current estimates of health risks from intakes of radionuclides, carried out jointly by representatives of the various points of view. (For examples of how such ‘joint fact finding’ exercises can be conducted see details of the BNFL national stakeholder dialogue at www.the-environment-council.org.uk.) The objective would be to reach agreement on the range of possible risks to the various age groups from various types of radionuclides. Although use could be made of material in the CERRIE majority and minority reports, and of the work of COMARE, it would be essential to set aside the idea that there is one simple answer as to whether current models and risk estimates are right or wrong. It seems clear that the situation is more complex and that until this complexity is recognised more explicitly there can be little constructive discussion or progress towards reconciliation of very different points of view. When this quantification of the uncertainties in risks is well-advanced it should become possible to devise a research programme to reduce the most important of them. Such a programme would be a better use of resources than following the recommendations in the three reports for new research on a few, rather disparate, topics and for more of the same sort of research as has been carried out in the past.

There then remains the question of what policy-makers and regulators should do while awaiting the results of the quantification of uncertainties and of the research to reduce them. The press release that accompanied the CERRIE majority report called for a precautionary approach but went on to say that the committee found no clear evidence that current mainstream
risk estimates are substantially wrong. COMARE believes that a precautionary approach has been standard practice in radiological protection for some considerable time [3]. The UK government has been committed to a precautionary approach in various areas since the early 1990s and has quite recently reiterated this commitment in the particular case of setting limits on discharges of radioactive effluents into the environment. It could presumably claim that nothing more needs to be done at present. In my view this would not be acceptable. There is a fairly urgent need for the government and regulators to consider how to address the possibility that risks to children from their intakes of radionuclides, and risks to foetuses from intakes of radionuclides by their mothers, might be much higher than mainstream radiological protection views dictate.

Your editorial about CERRIE [5], and the articles and letters in various newspapers, show how tortuous and unsatisfactory the committee’s deliberations were. There are lessons to be learned from the CERRIE experience but I do not agree with you that the lessons are about peer review and the independence of scientific advisory bodies.

Many of the organisations involved in the radiation risk debate self-publish reports without submitting them to peer review. (The National Radiological Protection Board (NRPB) and the Small Area Health Statistics Unit (SAHSU) come immediately to mind.) The difference between them and self-publishers like Chris Busby is that the organisations have the resources to make sure that work is checked by staff who were not involved in actually carrying it out. Lone workers who are unsalaried rarely have the means to employ others to do even the most basic checking. If they also have views that are outside the mainstream, they are unlikely to submit papers to journals only to have them peer reviewed by people who are determined to find fault. The answer to this problem is not to dismiss self-published documents as being automatically less reliable than journal articles, as you suggest. It is to provide lone workers who wish to self-publish with the resources to have their work checked.

I also disagree with your idea that scientists should be allowed to carry out ‘impartial critical reviews’ of the evidence without hindrance from ‘industry, pressure groups, government, politicians and other interested parties’ and then submit their conclusions to ‘policy-makers’. This seems to me to be completely inconsistent with modern thinking about, and practice in, formulating policy in areas where the science is controversial (see, for example, [6]). There is no such thing as an ‘impartial’ scientist because everyone has intellectual baggage, even if they do not support a particular faction in a debate. Nor is it practical or desirable for a group of scientists to work in isolation from the influence of ‘interested parties’. What is needed is for committees that advise governments on scientific topics to be composed of people who fully represent all the points of view and all the relevant disciplines, for all members of these committees to be resourced properly, and for the committees to work in an open and transparent way. In the UK the government has started to use this approach in radioactive waste management (see www.corwm.org.uk). I think it is high time the approach was applied to the health risks of radiation.

Yours faithfully,

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Dear Sir

Without wishing to appear sycophantic, may I congratulate you on your editorial, ‘Reflections on CERRIE’ (Wakeford R 2004 J. Radiol. Prot. 24 337–40). As another member of the Committee, I too sat for many hours listening to wild allegations and unjustified assertions. Being an optimist, I was not too dismayed to discover that the CERRIE process was being partly funded by government money that might otherwise have gone into radiation research. After all, part of our remit was ‘to identify any further research that might be needed’. However what did give me increasing cause for concern were the frequent references by Busby to his discussions with Michael Meacher. It was if the prosecuting counsel in a court of law had gone to the jury at the end of each day’s hearing to feed them ‘facts’ which had not been accepted as evidence in open court.

The intention was that the Committee should reach consensus in its views concerning the risks and their uncertainties. Where this was not possible, the Committee was asked to give details of the failure to agree, including the reasons for such disagreement and how they might be resolved in the future. Although most members agreed to compromises in the way in which their views were recorded, Bramhall and Busby resolutely refused to do so. In an effort to break this dead-lock, I suggested at one of the later meetings that we should adopt an unusual format for the printing of the report. In this, the main consensus agreement would appear on one page, with the differing minority view on a particular matter appearing on the opposite page. I thought that this proposal had been accepted, but at the following meeting it was rejected.

My worst fears concerning the Bramhall/Busby/Meacher relationship were confirmed soon after the last formal meeting of the Committee. Even while its Report was still with the printers, Meacher (by now the ex-minister) had rushed to Radio 4 to complain that the two anti-nuclear members had been gagged, and that ‘underhand skulduggery and chicanery’ had been used to suppress their minority report. Immediately after the Report’s publication he had a piece in the Guardian (‘Counting the dead’, October 22, 2004) which had an error of fact in almost every paragraph. These errors were virtually identical to those created by the two...
dissidents which made it impossible for the other members of the Committee to incorporate these views into the Report.

Like you, I am further concerned about the assured independence of scientific advisory bodies. This is because a more sinister aspect of the previous three years’ events emerged in the *Observer* (‘Ex-minister under fire for bullying’, October 24, 2004). In this, Dudley Goodhead, Chairman of the Committee, revealed that Meacher had attempted to manipulate the work of the Committee, an allegation endorsed by Bryn Bridges, Chairman of COMARE. Both pointed out that they were approaching retirement and could therefore afford to reject the pressures on them. ‘However,’ Bridges continued, ‘younger scientists, with careers ahead of them, might well have backed down in the face of such pressure from a minister.’

To appreciate that this is not a fanciful scenario, one must look across the Atlantic. A long feature in the *Times Higher Education Supplement* (‘The right’s hi-jack of US science’, March 12, 2004) gave a detailed account of just such happenings. Two members of the President’s Council on Bioethics were eased out of their positions because they argued in favour of a scientific approach to stem cell research rather than an ideological one. In a very different area, the wife of the Vice-President continues to urge her ex-colleagues on the National Endowment for Humanities to ‘flag up’ certain grant proposals which, it is widely believed, might produce information unhelpful to the Administration’s stance on these matters.

The question, then, is a very real one. Would you, dear reader, risk your career to serve on a committee which might produce findings contrary to the beliefs of a bigoted minister?

Yours faithfully,

J A Simmons

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**LNT hypothesis, thresholds and hormesis**

Dear Sir

There should be a warning sign ‘Beware of slippery data’ for all those who discuss the low dose epidemiological and experimental data and draw conclusions from it with respect to the shape of the risk–dose relationship as dose approaches zero. Simmons [1] has unfortunately missed the sign and, in the name of ‘thresholds’, has crossed swords with a distinguished group of radiation scientists [2] who defined the limits of usefulness of the currently available data and indicated the limitations of ongoing studies but preferred LNT. Irrespective of the difficulties of ascribing ‘average’ doses to different groups exposed at low doses and the influence these ‘averages’ might have on the interpretation of the risk–dose relationship at low doses, the data are just not sound enough below 200 mSv to draw conclusions. The shape inferred for the risk–dose effect relationship below 200 mSv may be LNT, threshold or even hormesis depending on the whim of those wanting to draw conclusions from the slippery data.

The simple fact is that statistics preclude the acquisition of any epidemiological or animal radiobiological data at very low doses which will resolve the issue of the initial shape of the risk–dose relationship at very low doses and put an end to the totally worthless discussion between the hormesis lobby, the threshold lobby and the LNT lobby. The epidemiologists have recognised this and have clearly indicated the limits of the current data.
However, there is a scientific pathway with considerable experimental support which can provide evidence on the shape of the risk–dose relationship at doses down to zero dose. That pathway encompasses the development of a comprehensive understanding of the mechanism of radiation action from the molecular damage, through cellular effects to the induction of cancer, the main radiation risk. This understanding should lead to a coherent explanation of cellular and animal radiobiology as well as the epidemiological data. My colleagues and I have proposed just such a pathway [3] and implied that the radiation-induced double strand breaks in DNA are the crucial initial lesions responsible for chromosomal aberrations and mutations at the cellular level which ultimately lead to radiation-induced cancer. The biophysics of radiation interaction tells us, unequivocally, that all types of ionising radiation induce DNA double strand breaks in direct proportion with dose at very low doses. On this basis I have to conclude that the shape of the risk–dose relationship is strictly linear at low doses from zero dose up. The important parameter in this linear relationship is, of course, the slope which puts a numerical value on the risk and this is where I take issue with the LNT hypothesis as recommended by ICRP [4].

The LNT hypothesis is a simplified, non-mechanistic interpretation of radiobiological and epidemiological data available in the 1980s and essentially provides a single value on which all radiation risk is based. This is entirely reasonable in a document published in 1991 and it has to be said that these ICRP Recommendations have provided the basis for a widely respected radiation protection philosophy and practice. However, there is good evidence now to suggest that the risk–dose relationship at higher acute doses is linear–quadratic with saturation, i.e. a right-leaning, flattened ‘S’ shape which all too easily approximates to a straight line through the origin. It is, however, the initial slope of the ‘S’ curve which is relevant for risk. Other evidence implies that radiation risk is dependent both on age and, in a complicated way, on the natural incidence of a cancer type such that rare natural cancers will have a much lower radiation risk than, for example, leukaemia. A more refined approach to the derivation of risk should be possible.

The ICRP currently proposes to maintain the LNT hypothesis but ‘above (doses of) a few millisieverts per year’ [5]. I appreciate that this is a simple expedient which avoids the misuse of the LNT hypothesis to calculate collective doses for very large populations over long periods of time and thus derive ‘alarming’ levels of harm but, because of my conviction that risk is linearly related to dose from zero dose up, I have to be concerned by these new proposals. If the ICRP is not prepared to take a more refined approach to the derivation of risk at this stage, it is probably better to leave well alone.

It is time to bring the fruitless discussion of hormesis, thresholds and LNT to an end and move on to a more scientifically based approach to radiation risk.

Yours faithfully,

K H Chadwick

References

Radioactive waste management

Dear Sir

Can anyone join in the ‘radioactive waste management’ roundabout? My experience in radioactive ‘waste disposal’ from very low to very high levels, before retiring, leads me to agree that John Dunster’s analysis (‘Solid radioactive waste: a confused mixture of responsibilities and discussions’ J. Radiol. Prot. 24 179–80) is accurate if restrained.

How could one take Nirex seriously when Angela Rippon was appointed to their Board? She may well have attributes that commend her status as a television celebrity, but what could she contribute to the resolution of the issues surrounding radioactive waste?

Finally, the plethora of spawned committees supposed to solve the problem is a reward for failure and has confirmed my experience of committees, i.e. they are composed of individuals who can do nothing but who collectively decide nothing can be done!

Yours faithfully,

Tony Freke