LETTERS TO THE EDITOR

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LETTERS TO THE EDITOR

Environmental impact of ionising radiation

Dear Sir

The publication of the special issue, ‘Framework for assessment of environmental impact (FASSET) of ionising radiation in European ecosystems’ (Williams C (ed) 2004 J. Radiol. Prot. 24 (4A) A1–A157), gives evidence of a substantial amount of thorough research and development work but raises, in my mind, serious reservations on the actual need for this work.

I can appreciate the need to verify the assumption made by the ICRP that recommendations for the protection of man probably guaranteed an adequate protection of the environment but I suspect that a brief study of the available literature would have provided that verification.

The special issue does nothing to alter the reservations I have about this research topic. The radioecological models satisfactorily describing the transport and accumulation of radioactivity in the different compartments of ecosystems were largely developed by the end of the 1990s. The only step required from these models is the assessment of dose rates in the various compartments and several articles in the special issue address this step. But the main reservation that I have concerns the problem of radiation effects on the environment.

The ICRP is concerned with risks of radiation to man and the main risk is, of course, the induction of cancer. I am unclear as to the exact nature of the risk to the fauna and flora in the environment but I doubt that it is the induction of cancer, and the special issue does nothing to resolve my doubts. I have always felt that the first and most important issue which needs to be addressed in any consideration of the environmental impact of radiation is a careful definition of just what risk, or radiation effect, is crucial and needs to be investigated. Until a specific and quantifiable risk to the environment is identified we are surely working in the dark and that means that much work will be useless and much research funding will be wasted. Rather like looking under the light for keys dropped further down the street.

One article in the special issue describes a database (FRED) set up to assemble published information on radiation effects on plants and animals after acute or chronic exposures for ‘umbrella end-points (morbidity, mortality, reproduction and mutation)’. The conclusion of the article suggests that ‘data on the effects of chronic irradiation on non-human mammals included in FRED show that dose rates lower than $10^3\,\mu\text{Gy}\,\text{h}^{-1}$ do not produce clear irreversible deleterious effects on morbidity, mortality or reproductive capacity on this wildlife group’. We should not be surprised as morbidity, mortality and reproductive capacity are all deterministic effects which exhibit a threshold type of dose response for acute exposures and where the threshold will almost certainly extend to higher doses as the dose rate is reduced. However, despite concluding that there were no effects under $1\,\text{mGy}\,\text{h}^{-1}$, the article finishes by suggesting that the experiments done so far do not reflect the conditions arising in the environment and suggests that ‘future research activities ... plan experiments in order to obtain useful results for environmental protection purposes’.

Whoa!

• we do not have a specifically defined risk;
• this is old fashioned radiobiology at best;
• this is a monster programme with monster costs and an uncertain outcome;
• where is the money for such an experimental programme coming from?
• what other, much better defined, research will suffer from a loss of funding as a result?
It is not clear just what the radiobiological qualifications of the people involved in Fasset really are. I suspect the programme is driven by radioecologists, not radiobiologists, and this makes me nervous.

Radiobiology can already provide ways of extrapolating effect–dose responses from acute to chronic dose rates and, ultimately, of deriving effect–dose rate responses. Radiobiology already knows that radiation sensitivity is strongly dependent on genome size and that mammals will be the most sensitive species. The last thing we need is a large experimental programme exposing different organisms in a wide variety of environmentally typical conditions at a variety of different exposure-rates using different plausible cocktails of radioactive isotopes looking for a risk which remains undefined.

I shudder to think what a newly established ICRP Committee to develop guidelines for environmental assessments and environmental risk management will recommend in the way of future research and I fear that large amounts of research funding will be diverted to search for a largely ill-defined and potentially insignificant environmental risk in spite of how laudable the aim appears to be.

I am personally convinced of the validity of the ICRP assumption that if man is protected then so is the environment. I think it is time to pause and rethink and redefine the problem before too much valuable research money is wasted.

Yours faithfully,

K H Chadwick

The role of COMARE in the light of the CERRIE report and the COMARE response

Dear Sir

A crucial element of the discussions within CERRIE was whether irradiation originating from within the body is equivalent to that received from external sources. Drs Harrison and Day (2005 J. Radiol. Prot. 25 101) rightly assert that there are better human data providing evidence for the general equivalence of internal and external irradiation than those presented in the COMARE response. That evidence was presented to CERRIE and was not further discussed by COMARE as it was fully covered by CERRIE. However, it relates to exposures at high to moderate doses and COMARE was attempting to examine situations where humans were exposed to the sort of lowish doses that should be more relevant to current environmental or occupational exposures. Inevitably the uncertainties inherent in low dose studies mean that the results can never appear as convincing as those of high dose studies. Nevertheless, the analyses done for COMARE were consistent with the concept of general equivalence of internal and external exposure and did not give any indication that current risk estimates were too low by a factor of several hundred as two members of CERRIE asserted.

Ms Hill believes that COMARE has outlived its usefulness and should be abolished. Her stance seems to derive from the fact that COMARE does not contain representatives of all points of view (by which I think she means that it does not contain representatives of all the various lobby groups of industry and environmentalists). Therefore, she asserts, it cannot make any progress towards 'reconciliation of very different points of view'. While reconciliation is desirable, the job of an independent scientific advisory committee is not to pursue that path
but to examine the evidence in as disinterested a way as possible. COMARE is constituted to do just that and, in contrast to her assertion, it does not work in isolation from the influence of ‘interested parties’. In fact CERRIE itself started life as a COMARE consultative exercise designed to interact with all shades of opinion before it was hijacked by Michael Meacher and turned into an independent committee. Even its acronym was originally short for Consultative Exercise on Radiation Risk from Internal Emitters.

Ms Hill has no experience of COMARE and how it works. Nevertheless its members would almost certainly agree with her that there is usually no one simple answer to whether current risk estimates are right or wrong and that the complexities need to be recognised more explicitly. It has been a failing, not only of some anti-nuclear lobby groups, but also of some within the radiological protection field, that they have their noses very close to their special interest and do not deign to consider the bigger picture. COMARE cannot afford to do this. Consider its current discussions relating to its forthcoming report on the distribution of childhood cancer in Britain. Particularly for childhood leukaemia, it is aware of evidence that:

- the degree of affluence is significantly associated with elevated levels in the UK,
- population mixing (assumed to be a surrogate for the involvement of some infectious agent) is significantly associated with elevated levels in the UK,
- pollution may be a significant risk factor in the UK, though harder to detect since sources of pollution are varied and dispersed.

It is also aware that while radiation is undoubtedly a leukaemogenic and carcinogenic agent, the power of the available epidemiological studies makes it hard to discern its contribution to childhood cancer in the UK population (other than resulting from exposure during pregnancy) despite the ascription by ICRP of around one third of childhood leukaemias to background ionising radiation. COMARE has to wrestle with all this and set the effects of radiation in an overall context.

That does not mean that environmental radiation can be ignored, rather that all the evidence has to be weighed for quality and relevance. COMARE, being composed of scientists with an exceptional research record in a wide variety of disciplines, coupled (in my experience) with a decent leavening of common sense, is well suited to its carry out its remit.

Yours faithfully,

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Should the prevalence of obesity be allowed for when assessing cancer risks in radiation-exposed workers?

Dear Sir

There is increasing evidence that particular types of cancer, such as those of the colon and parts of the female reproductive system, occur more frequently in obese individuals [1]. Recently three large studies have reported that a wide range of other cancers, including leukaemia, are
also more common in overweight and obese persons [2–4], and it has been suggested this may account for around 10% of all cancer in white western men [2, 5]. Some earlier, smaller, studies appear consistent with these findings [6–8]. Moreover a recent study of leukaemia has indicated that up to 30% of the Acute Myeloid subtype might be associated with excess weight and obesity [9].

These results could lend support to a previous observation that men with leukaemia are more likely to have had particular diseases, including diabetes, infectious hepatitis, heart disease, psoriasis, and eczema more than a year before diagnosis [10, 11]. At least the first few of these disorders are associated with obesity either prior to or consequent upon the disease—as are hypertension, depression, respiratory problems and skin disorders [12, 13]—and lack of exercise in otherwise healthy individuals.

If a substantial association between excess weight and cancer does indeed exist, this may have implications for the assessment of cancer mortality and morbidity in radiation workers. In particular it would provide a direct link between cancer and readily measurable physical parameters including height and weight, blood pressure, physical agility and exercise tolerance, and hence could provide a mechanism by which self-selection or medical-selection of physically fit individuals might result in lower cancer morbidity and mortality in the future. Any widespread exclusion of common obesity related disorders might also affect the prevalence of obesity, and indirectly contribute towards lower cancer rates. In the UK classified radiation workers are required to undergo annual medical assessment which must specifically consider their medical fitness regarding, the wearing of protective clothing (including respiratory equipment), skin disorders (for those working with unsealed radioactive materials), and the presence of mental disorders (in relation to radiation safety) [14]. Although very few diseases would necessarily exclude an employee from undertaking radiation work, specific restrictions might be imposed [15]. Whatever the cause, it is certainly worth noting that within a given cohort, mortality from particular disorders can be much less than expected—as demonstrated by the significantly low mortality from diabetes, endocrinial and nutritional diseases, mental disorders, respiratory system diseases and ischaemic heart disease in radiation workers, but not in non-radiation workers, at the Springfields uranium production facility, for example [16] (table 1). It would therefore be interesting to enquire whether obesity rates within any particular cohort could likewise show substantial variation from expectation.

Table 1 shows an apparent ‘healthy worker’ effect that is more pronounced amongst the radiation workers than amongst the non-radiation workers for that cohort. The disorders listed include ‘life-style’ diseases which for example may have obesity and/or smoking among their contributory causes. The difference between radiation and non-radiation workers is likely to be in part a reflection of the differential selection procedures at employment and partly a consequence of the radiation workers maintaining a healthy lifestyle (perhaps by not having the same opportunities as the non-radiation workers to smoke whilst at work for example). A further component of the ‘healthy worker’ effect is the ‘healthy worker survivor’ effect, a consequence of self-selection out of the workforce due to ill health, resulting from early manifestation of the effects of the ‘life-style’ diseases. This may well have implications for the assessment of mortality and cancer morbidity in radiation workers because ‘healthy working lifestyle’ and ‘healthy worker survivor’ effects are time-dependent, and dose–response estimates could be biased in the presence of time-dependent confounders. It might also be argued that the dose–response estimates could be biased most for those diseases frequently manifesting before retirement and with short latent periods, and perhaps leukaemia should therefore be considered separately to solid cancers.

Despite the above discussion, no direct evidence has been advanced here to demonstrate that obesity in radiation workers is in any way different to that of the general population.
However prevalence might also vary between different groups of radiation workers depending on physical activity. One study of obesity has found that among men who undertake little or irregular recreational exercise, those in occupations with a high level of physical activity are much less likely to be obese than those in occupations with a low level of physical activity [17]. The former occupation category might well include underground metal-ore mining, especially for the most physically active subgroups. We therefore suggest that it could be worthwhile, for physically active or rigorously screened cohorts, to confirm that obesity rates do not differ substantially from expectation across the relevant follow-up period and within each exposure category, and to consider possible adjustments to expected cancer rates should this assumption fail to hold true.

Yours faithfully,

J Eatough and B Epps

References

Comment on papers on NRPB study of the UK test veterans

Dear Sir

Notwithstanding the undisputed eminence of the authorship of the two papers in question (Kendall et al 2004, Muirhead et al 2004), I wish to raise serious criticisms on the methodology used in this important study. I attended the meeting in 1983 which instigated this study,
which was designed to respond to concerns expressed, at that time, by test veterans that their service in Australia and the Pacific, where they might have been exposed to radioactive fallout, had compromised their health. There was no pretense that an epidemiological study of the test veterans would return a result of scientific value; the issue was purely one of providing assurance that the official position of the Ministry of Defence (MoD), that no health detriment was to be expected from service at the atomic weapons test sites, was correct.

The first prerequisite for such a study should have been as complete independence from the Ministry of Defence (MoD) as possible, who are seen by the veterans as having caused the claimed health effects and who are responsible for compensation were it to be demonstrated that the effects were due to radiation. It was, therefore, unfortunate that the NRPB relied so heavily on MoD records to compile the study population and for dosimetric data. This is especially so as the MoD records turned out to be incomplete to quite a serious extent. The NRPB has consistently maintained that this incompleteness does not compromise the study from a methodological point of view. I disagree.

My contention is not that the NRPB study is biased but that it cannot be guaranteed to be unbiased and that this is important in the context in which the study was commissioned. The concept of carrying out an epidemiological study on a sample of a potentially exposed population is not of itself wrong. However, there are certain provisos which must be satisfied in order for it to be a valid approach. The most important of those is that the population selected for inclusion in the study should be free of any form of bias. This can be quite a difficult condition to guarantee and, therefore, it is considered preferable, if practicable, to ascertain the affected population completely. If the NRPB study were to have been made using the general population as the control population there would be no doubt that incomplete ascertainment of the order of 15% of the total population would seriously compromise the study. The NRPB’s defence, therefore, relies on the inclusion of the control population in their study. To guarantee a lack of bias under these circumstances requires that the study and control populations are constructed in an identical manner. In the case of the NRPB study both populations are drawn from a single population of men serving in the armed services between certain dates. The NRPB, in effect, asserts that the test veteran population is a randomly selected 85% of the total population of veterans. However, this population is more accurately described as the total number of test veterans for which the Ministry of Defence still had records available in 1983. The NRPB’s control population is comprised of a similar number of servicemen, approximately matched for a number of parameters, such as age, service, etc, who served in the tropics but did not attend the weapons tests.

What is clear is that the criteria for inclusion of subjects for the two study populations, potentially exposed and control, are not identical since the control population is not a sample of any pre-defined population, as is the veteran population. Therefore, the two populations are not strictly comparable. Whether this biases the outcome depends on whether or not the missing veteran’s records are missing as the result of any reason associated with their health.

Prior to 1983, when the study populations were compiled, there had been considerable concern about health issues amongst those servicemen who attended the tests and this may have resulted in differences in the way the records were handled by the MoD between the time they were compiled and 1983. It would not be difficult to imagine, for example, that amongst the veterans there was a greater need to access the records in relation to claims of health detriment than for the servicemen who constituted the control population since there was no specific concern for their health. For example, it is stated (Kendall et al 2004) that ‘prior to 1976 it had been the practice to send army records to DHSS [when a claim for compensation was made] without keeping a note of their removal or seeking their return’. The authors claim that excluding the army records from the analysis returned a similar result to the population as
a whole and conclude that the practice did not ‘appear to have introduced a detectable bias’. However, the practice does indicate the attitude taken by the MoD to these records up to 1976, namely that they were not considered of great importance. Once again I stress I am not saying that this study is biased, simply that a plausible argument to support the possibility of bias can be made; at least as plausible, I contend, as the claim made by the NRPB that they were successful in eliminating bias.

The point is that an under ascertainment of 15% (that is nearly 3770 missing veterans) is enough to conceal a substantial differential in the health of the two populations, particularly for comparatively rare diseases. For example, deaths from leukemia, excluding CLL, are in statistically significant excess with only 40 deaths over the whole follow-up period out of total cancer mortality of 1546.

For this situation to have arisen does not necessarily imply any malfeasance; it could simply result from inefficient, or one might say normal, handling of records that were not considered vital up to 1983. From the point of view of the test veterans though, for whose peace of mind this survey has been carried out, the possibility that ‘inconvenient’ records were deliberately lost by the MoD cannot, and should not, be ignored. It is notable that the MoD has used the NRPB survey as evidence that there are no health consequences as a result of attending tests in the case of individual compensation claims. This, in any case, is a misuse of epidemiological data which says nothing at all about the causes of ill-health in an individual. Thus, the NRPB’s reliance on MoD records for the compilation of the test veteran’s population has, in more ways than one, potentially compromised their study.

In my view the test veterans have every reason to be disappointed with the NRPB. The failure to fully ascertain the involved population and, therefore, to provide the best objective assessment of the health of the veterans as is possible, is difficult to justify as is the failure to acknowledge that their survey is potentially flawed because of this.

Yours faithfully,

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References


Reply to ‘Comment on papers on NRPB study of the UK test veterans’

Dear Sir

Dr Baverstock raises a number of issues.
(1) ‘There was no pretence that the study would provide a result of scientific value’

Whatever was found the study would have been of scientific value if carried out scientifically: either by showing that the exposed men suffered no detectable risk or by showing that they had, whether from ionising radiation or from some other cause. As made clear in the review papers (Kendall et al 2004, Muirhead et al 2004), it was always apparent that, if the recorded doses to test participants were approximately correct and established radiation risk factors apply to this population, it would not have been feasible to have detected a raised risk of mortality or cancer incidence associated with radiation exposure. However, if the dose estimates were substantially in error, or if test participants were exposed to some other risk factor, the study that was undertaken would have detected any resulting unusual patterns of mortality and cancer incidence.

(2) The study should not have relied on records from MoD

There was no alternative to using MoD records for establishing a substantially complete cohort and obtaining estimates of radiation dose. All other sources would have been far less complete and most would have certainly been biased in terms of the health experience of those included. Nevertheless, attempts were made to collect as much data as possible from other sources, both to assess the completeness of coverage of the study population and, as discussed below, to examine the possibility of bias.

(3) Exclusion of 15% of test participants seriously compromises comparisons with the general population

Interpretation of comparisons of test participants with the general population is complicated by selection effects (for example the ‘healthy soldier effect’). But the fact that only a sample of test participants were studied does not preclude comparisons with the general population.

(4) It is plausible that the health status of the 15% of test participants who were not included in the study differed from the 85% who were so included

The question of potential bias of the study population derived from MoD records is an important one and we were alert to it from the start. The work that was undertaken to investigate such a possibility is discussed in detail in the report of the first analysis (Darby et al 1988a, 1988b) and summarised in Kendall et al (2004).

Lists of test participants were assembled from multiple sources. This meant that even if, for example, dosimetry records had been discarded for an individual who recorded zero dose, he would still have been included if he was listed in a ships ledger.

Sample checks were carried out at the DHSS archives of claims for disability pensions. As noted (Darby et al 1988a, 1988b, Kendall et al 2004), this revealed that, before 1976, records for some Army personnel who had made disability claims might not have been returned to the normal records office. However, excluding Army participants made almost no difference to the results of the first analysis.

We also assembled lists of test participants from as many sources as possible that were independent of MoD. These lists were, as anticipated, heavily biased towards those whose health had suffered. But the subsequent health experience of these independent responders who were not in the main cohort was similar to the health experience of those who had also been identified for the main cohort (the 85% of test participants) (Darby et al 1988a, 1988b, 1993a,
This provides powerful evidence against the idea that individuals were significantly less likely to be included in the study population if their health had suffered.

It is also important to note that any bias that might have operated in the early period could not plausibly have affected the later analyses. Most of the deaths and cases of cancer in the cohort have occurred in these later periods. Long term bias could not have arisen unless it were possible to predict the health of individuals ten or more years in the future.

The way that the study was designed and executed is described in detail in the published reports and papers, as referenced by Kendall et al (2004). The need to guard against the risks of bias was always prominent in our minds. We do not believe that Dr Baverstock’s criticisms take account of the way that the study was conducted or the tests made to detect any bias that might have occurred.

Yours faithfully,

G M Kendall, S C Darby, C R Muirhead and R Doll

References

Darby S C et al 1988b Mortality and cancer incidence in UK participants in UK atmospheric nuclear weapon tests and experimental programmes NRPB-R214 (London: HMSO)