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LETTERS TO THE EDITOR

Gamma radiation measurement

Dear Sir

I have some observations to make on the recent paper by Ramli *et al* (2005 *J. Radiol. Prot.* **25** 435).

On page 441, Ramli *et al* make a statement that the Ludlum micro-R meter has an almost flat response to gamma radiation. I think this is unlikely, as the instrument is basically a simple sodium iodide scintillator based ratemeter with a fixed energy threshold. Such instruments generally have a response which varies considerably over the range of interest for environmental dosimetry. This ranges from the highest significant energy generated, 2.615 MeV from Tl-208, down to the lowest energy which is likely to escape from bulk material as a consequence of Compton scatter, around 60 keV. As an example, the response of a 38 mm × 55 mm sodium iodide detector varies over a range of 10 from Co-60 (1.25 MeV) to 109 keV when used in the gross count mode. The range varies by a factor of 2 between Co-60 and Cs-137 (662 keV), which are reasonably close to K-40 at 1.46 MeV and the 609 keV line from Bi-214, both of which are, or can be, major contributors to environmental gamma dose rates. This is with a much bigger detector than the one employed by the authors. Moving to a smaller detector, such as the 25.4 mm × 19 mm one in the Thermo 41 detector, the energy response varies by a factor of 30 from 87 keV to 1.25 MeV, with a Cs-137/Co-60 ratio also of 2. The detector in the instrument used is likely to have a range of response somewhere between the two.

There is an additional factor. Sodium iodide detector energy responses, when used in the gross mode, are very susceptible to the setting up process, which is effectively a process of setting the low energy counting threshold. It is possible to achieve the same indication, in dosimetric units, by having a higher energy threshold, and thus a lower sensitivity in counts per microGy air kerma, and combining this with a high gain in the meter drive circuit. This will produce a different energy response than the opposite, with a low energy threshold and a low meter drive gain. Hence, it is important to demonstrate that the detector used has the expected response. For larger detectors, it is sometimes possible to find an energy threshold which will give the same response per unit air kerma for the U-238 and Th-232 series and for K-40 in an infinite matrix. This allows the use of such detectors to determine environmental air kerma rates without taking any account of the relative abundance of the natural nuclides present. The assumption is that this was not done in this case.

Ludlum clearly point out in the catalogue that the instrument is set up for Cs-137 and that it is energy dependent. The 3rd edition of the reference (Knoll), which the authors state supports the statement that the instrument has an almost flat response from 40 keV upwards, does not support it. Figure 10.24 clearly points out that the intrinsic total efficiency for a 25.4 mm thick piece of sodium iodide for radiation normal to its surface increases from about 38% for 1.25 MeV radiation to 100% at about 100 keV. This has to be combined with the reasonable approximation that it's the amount of energy that impinges on a mass of air that determines air kerma, with only a small dependence on individual photon energy (ICRP 74, table A1). Hence, at 609 keV, there are about 2.6 times the number of photons per square metre per Gy air kerma as at 1440 keV. Combining this with the change in detection efficiency of about 25% clearly shows that we should expect the response (in counts per Gy air kerma) to vary by about 3 over that very limited energy range. Hence, I think there are potentially major problems with

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the stated environmental dose rates. One is that the data will be very spectrum dependent and changes in the ground fingerprint will influence the results. The other is that there may be a systematic shift in all the data caused by the use of Cs-137 as the calibration nuclide.

Yours faithfully,

Peter Burgess
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(E-mail: peter.burgess@ukaea.org.uk)

Reply to 'Gamma radiation measurement'

Dear Sir

The following is my response (on behalf of the authors) to the observation made by Peter Burgess.

Basically I do not dispute his comments. I would like to thank him for his contributions and they are certainly very valuable for our research. I would like to offer the background to the mentioned statement; hopefully it may place this issue in its proper perspective.

The statement was a conclusion formed from Knoll (1989 *Radiation Detection and Measurement.* 2nd edition, page 230):

> 'The most notable property of of NaI(Tl) is its excellent light yeild. Its response to electrons (and gamma rays) is close to linear over most of the significant energy range (see Fig. 8.8). It has come to be accepted as the standard scintillation material for routine gamma-ray spectroscopy and can be machined into a wide assortment of sizes ...'

It is a general statement. It did not mention any crystal size qualifications. As such, we took it in good faith that the statement also applied to the detector we intended to use. As pointed out, we might have taken the statement out of its context.

We were aware that the NaI(Tl) scintillator used is energy sensitive as stated by the manufacturer. We had to decide, within the limited resources available, what was the optimum instrument to be used that would serve our purpose. We decided not to adopt the metrological approach, as it was beyond us from practical point of view. The question then was whether the readings obtained through Ludlum 19 fairly represent the actual environmental gamma dose rate or not.

We have conducted a statistical test for correlation, that is to test if there is a correlation between the measured dose rate using Ludlum 19 and the calculated dose-rate. The dose-rates were calculated from soil activities due to U-238, Th-232 and K-40. The soil activities due to U-238, Th-232 and K-40 were obtained by using hyperpure germanium gamma spectrometer. The conversion factor given in UNSCEAR 1988 was used to obtain the calculated dose rate. Quindos *et al* (1994 *Health Phys.* **66** 194–200) have used a similar approach, but by using the conversion factor given by Leung *et al* (1990 *J. Environ. Radioactivity* **11** 279–90). 128 soil samples were used. They were taken from locations with various values of measured dose rates and with various soil types and geological backgrounds.

The Pearson coefficient of correlation (R) value obtained was 0.938. Given the statistical nature and the exteremely low level of radiation dose rate being measured, the magnitude of uncertainties involved in gross environmental measurements, the degree of averaging of dose rates measured to obtain a representative reading for a particular location and the various other factors that may greatly affect an environmental reading, we considered the correlation was good enough for our purpose. That is, the Ludlum 19 readings do 'fairly' represent the 'actual' environmental dose rate. We do understand that this approach is unsatisfactory from a metrological point of view.

We do intend to carry out a more comprehensive statistical investigation on the extent, the limitations and the nature of the validity of the above correlation, possibly basing it on and with respect to different geological background and soil types, that is to take into account the effects of the variations in radionuclide composition.

The use of a gamma spectrometer capable of *in situ* radionuclide analysis was found to be cumbersome, impractical, not cost effective and unnecessary for this type of natural environmental work, especially in some tropical jungles! Whatever improvement that was obtained in the improved 'accuracy' was overwhelmed by the uncertainties and the averaging involved. We reserved radionuclide analyses for laboratory work, and they were done on a sampling basis.

In short, we do not dispute the observations, but in these circumstances we have taken a different approach.

Yours faithfully,

Ahmad Termizi Ramli
Universiti Teknologi Malaysia

Cancer risk among nuclear workers

Dear Sir

I read with interest your editorial (September 2005 issue, page 225) on the above topic with particular reference to the recently published study in the *British Medical Journal*. This would appear to confirm the current exposure risk estimates at low doses and low dose rates. It seems to me that in an effort to determine the relationship between radiation exposure and cancer mortality amongst nuclear workers an important aspect has been overlooked. That is the life expectancy of these people.

We are all going to die, the question that concerns us most is when. If the expectancy for nuclear workers, for whatever reason, be it radiation, better working conditions, safer work place, etc, is longer than for the general population, then this might affect the interpretation of the analysis. For example cancer is an age related disease; the older we are the greater the risk. If nuclear workers lived longer because of their occupation, then their cancer rate might be expected to be higher. Further more, it is likely that when summing over a large population, the longer people had worked in the industry, the greater would be their accumulated radiation dose, and the longer would be their life expectancy. This would lead to an apparent relationship between radiation dose and cancer mortality. Perhaps such a possibility is too remote to consider?

Be that as it may, as a retired radiation worker from the nuclear industry, my concern is more with my overall life expectancy than with what will ultimately kill me. If my death from cancer risk has increased, but all other factors remain the same, then my life expectancy should have been reduced. Has it, or has my radiation exposure, or working conditions, reduced my risk of dying of other causes and increased my life expectancy?

Yours faithfully,

Barrie Skelcher
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UK