

Topics under Debate

IS IT USEFUL TO ASSESS ANNUAL EFFECTIVE DOSES THAT ARE LESS THAN 100 mSv?

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INTRODUCTION

A primary goal of radiation protection is to ensure that workers and members of the public are not caused significant harm by practices that involve the use of ionising radiation. An important method for achieving this goal for radiation workers involves the use of personal dosimeters providing signals that are proportional to the external dose equivalents received by individuals. The quantity determined from measurements made with personal dosimeters is assumed to be a reasonable surrogate for a desired protection quantity such as equivalent dose.

Two basic parameters involved in the assessment of equivalent dose based on measurements with personal dosimeters are the frequency of the measurements and the lower level of the quantity measured that is of use to radiation protection. The question posed in this debate is whether it is useful to assess annual effective doses that are less than 100 mSv. This question relates not only to the practical capabilities of current dosimeters, but also to the rationale for performing measurements and the implications of such measurements in the assessment of the risk associated with exposure to low doses of ionising radiation.

Our two debaters have been involved with radiation dosimetry practices for many years. Dr Daniel J. Strom worked as a medical and academic radiation safety officer in the 1970s. He was in public health for twelve years, including doing doctoral research on using occupational monitoring records as the dose variable in epidemiological studies. After teaching and research in the Graduate School of Public Health at the University of Pittsburgh, Dan joined the Pacific Northwest National Laboratory in 1991. He is currently a Staff Scientist as well as Technical Network Leader for Human Health and Safety. Dan's active research interests include quantitative risk analysis for radiological and chemical hazards, models relating radiation and detriment (cancer and heritable ill-health), inference of radiation doses from intakes of radionuclides, and applied statistical inference in support of these topics. He continues his teaching and research careers at Washington State University Tri-Cities and at Oregon State University. Dan was certified by the American Board of Health Physics in 1980, is an Associate Editor of *Health Physics*, and a Fellow of the Health Physics Society.

Professor John R. Cameron is well known for his ground-breaking work at the University of Wisconsin-Madison to develop the first practical thermoluminescence dosimetry system more than forty years ago. In the early 1960s, he invented the bone densitometer that provided the first direct measurements of bone mineral to detect osteoporosis. John was the founding chair of the Department of Medical Physics at University of Wisconsin-Madison, and was the recipient of the Coolidge Award for distinguished contributions to medical physics in 1980. In 1985 he founded Medical Physics Publishing, a nonprofit, tax-exempt publisher of books in medical physics and related fields.

FAVOURING THE PROPOSITION: D. J. Strom

Argument

First let us examine the question of the utility of assessing 'small doses', and then address what the level of 'small' is. Most radiation safety programmes monitor employees for exposures to radiation and radioactive materials. Sometimes monitoring is extended to visitors, members of the public, students, minors, and declared pregnant women, as well as off-site public. Ultimately, most programmes perform monitoring and dose assessment because it is required by a regulation or rule invoked by a licence or contract. Although the title of this journal is *Radiation Protection Dosimetry*, there are many reasons for performing personal dosimetry⁽¹⁾ which go beyond demonstrating compliance with regulatory dose limits, and may not even involve radiation protection.

Beyond the 'score-keeping' needs of regulatory compliance, there are at least seven other valid reasons for performing personal monitoring, many of which fall into the category of 'no news is good news,' or more aptly, 'null news, as long as you can prove it, is good news'. Among these are:

1. Modern management methods stress measuring and assessing quality and performance, and, although fraught with problems, quantitative assessments of individual and collective dose have been incorporated into performance measures of radiation safety and ALARA programmes.
2. While it is unethical to use a worker as a 'canary in a coal mine,' analysis of worker intakes, ontakes*, and doses helps diagnose radiation protection problems, and facilitates the design of protection measures.
3. Dose measurements and assessments, and in particular, assessments of annual (not committed) doses to tissues, are needed for worker health surveillance and occupational epidemiology studies.
4. Records of dose assessments and the measurements and methods on which such doses are based are critical for support of litigation, and good measurements may prevent litigation in some cases.
5. Assessments of dose to workers, the public, and the environment are useful for documenting management commitment to workplace safety as part of management-labour relations, as well as

for demonstrating to the public that an operation involving radiation is safe.

6. Assessments of dose are useful in counselling workers (for example, declared pregnant women) about personal health.
7. Providing there is a policy for dealing with false positive results, in the majority of cases, assessment of dose and communication of the results to individual workers enhances their peace of mind by minimising worry, displacing uncertainty with knowledge, and providing reassuring evidence that the workplace is safe.

Certainly it is useful to assess effective doses to both workers and members of the public which are below 100 mSv for reasons other than radiation protection. On the basis of the arguments presented above, I advocate (1) more personal monitoring (external dosimetry, bio-assay, and exposure monitoring) rather than less, and (2) better communication of dose assessment results.

Regarding the second part of the question as to whether 100 mSv is a 'small' effective dose or not depends on what one believes about the health effects of such doses. Recent publications indicate that a single acute exposure to 100 mSv can lead to significant excess cancer risk.

The UNSCEAR 2000 Report⁽²⁾ states:

In general, significant radiation effects can be detected at doses of about 100 mGy (low-LET) and above, although there are some experimental systems for which effects at lower doses have been observed. (para. 536)

For most tumour types in experimental animals and in man a significant increase in risk is only detectable at doses above 100 mGy. An exception is for human exposures *in utero* when a significant increase in tumour induction in children has been found for doses in the 10-20 mGy range (low-LET). (para. 537)

Limiting their analysis to Japanese atomic bomb survivors with doses less than 500 mSv, Pierce and Preston⁽³⁾ show useful risk estimates for doses as low as 0.05-0.1 Sv and place 'an upper [95%] confidence limit on any possible threshold' of 0.06 Sv. Using an innovative, model-free visualisation that abandons models altogether, Chomentowski *et al* have shown positive risk in the Japanese data all the way to zero excess dose⁽⁴⁾.

*By analogy with 'intake,' which means either the process of material entering the body or the amount (e.g., activity) of material entering the body, 'ontake' means either the process of material getting on to the skin, or the amount of material getting on to the skin⁽²⁾.

The fact that *some* cancers, e.g., osteosarcoma and liver cancer, appear to have a threshold for induction does not mean that *all* cancers have thresholds. The fact that leukaemia in the Japanese life span study cohort is better described by a linear-quadratic than a linear dose-response model⁽⁵⁾ in no way contradicts the observation that solid tumours appear to have a linear response down to 5 mSv.

A notion we need to put to rest is that of *extrapolation* from effects at high doses to effects at low doses. We do not extrapolate; we *interpolate* between statistically significant excess risk at high increments of dose above background and the zero excess risk at zero increment of dose above background. When the high dose data form a line or trend that approaches zero excess risk at zero increment of dose with a positive slope, it stretched credulity to imagine a threshold or a benefit at doses in between. Dr Abel González has clearly illustrated⁽⁶⁾ that we are starting neither from zero dose nor from zero effect, but from background dose and background effects, and interpreting data above that point (Figure 1).

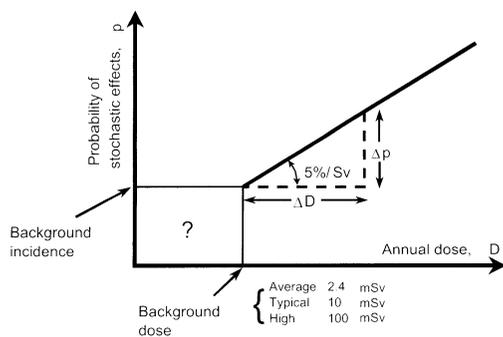


Figure 1. Adapted from an illustration of risk projections by González⁽⁶⁾.

With every likelihood that there is some increment of risk associated with an increment of dose regardless of the size of the dose, at least for some people (children, the immune compromised, those with impaired DNA repair), assessing of 'small' annual doses, certainly well below 100 mSv.y⁻¹, makes sense from a radiation protection standpoint as well as from the standpoint of regulatory compliance.

How low should doses be assessed? This is more of an economic question than a radiation protection question, because even small increments of radiation exposure above background, if imposed on people (as opposed to voluntarily accepted), are important to some people. While the dose assessment following an intake of tritium is probably the most precise we have, nonetheless even I do not advocate calculating and recording effective doses in the range of tens of nanosieverts, although it may be an easily accessible

range. Surely assessing, and recording, doses at 0.1 mSv, the traditional cut-off for thermoluminescence dosimeters, is not unreasonable.

Rebuttal

Dr Cameron and I agree that personal monitoring has at least some merits, but we disagree on many other points, especially the selection and interpretation of epidemiological evidence.

In general, the use of epidemiological studies in risk analysis requires one to weigh the evidence from all relevant studies, not just those that support one's conclusions. While the study by Berrington *et al* referenced by Dr Cameron is worthy, so, too are studies of radiologists in the US^(8,9), by none other than Dr Genevieve Matanoski of Johns Hopkins, the same researcher who led the NSW study. These do not exonerate low dose rate radiation, nor do the massive studies by IARC⁽¹⁰⁾ or those of indoor radon (see, for example, the work of Field *et al*⁽¹¹⁾).

Contrary to Dr Cameron's assertion, Dr Taylor's statement that 'No one has been *identifiably* injured...' (italics mine) does not support the inference that no one has been injured. There are two issues with this mistaken inference: (1) the implicit assumption that real excess cancers would necessarily be detected as a signal standing out from an immense background of cancer; and (2) the notion that individual cancers could be unambiguously ascribed to radiation exposure. Dr Taylor's statement is consistent with the knowledge that standard epidemiology is a very blunt tool that can be used neither to demonstrate an excess of even 10% in migrating human populations, nor to identify *who* has been injured in the absence of radiation-specific biomarkers of disease causation by radiation. The whole probability of causation method⁽¹²⁾ can only yield probabilities, not certainties, about the causal role of radiation in specific cases. Whether science will ever have 'a smoking gun with fingerprints on it' linking a given case of cancer to radiation is unknown at this time. Thus Dr Taylor's statement is consistent with an excess cancer risk at low doses delivered at low dose rates.

Dr Cameron ignores the growing body of evidence for inverse dose rate effects for both high-LET⁽¹³⁾ and low-LET radiation⁽¹⁴⁾. Such effects result in larger, not smaller, effects per unit dose at low doses.

The NSW study is characterised by an unhealthy control group, making it one of the very few studies in occupational epidemiology not to find a 'healthy worker effect' (Table 1). This odd finding challenges the consistency criterion⁽¹⁵⁾ (findings should be consistent across studies) and makes the entire study suspect. Comparisons with an unhealthy control group will, of course, show a protective effect!

The final disagreement is with the extrapolation from single dose studies to the claim that a moderate dose rate stimulates the immune system. A well-known adaptive response to ultraviolet radiation is the induction of suntan. A suntan fades with a half-time of the order of a few weeks. Adaptive responses have been shown to fade over hours to months, depending on the

endpoint measured. The notion that an adaptive response that requires 10–100 mGy to induce could be maintained by dose rates of the order of 100 mGy or less per year is mere speculation. At 100 mGy per year, the early radiologist data referenced above tell us that there is observable excess risk.

Table 1. Mortality for selected causes, >0.5 rem group, <0.5 rem group, and non-nuclear workers (NNW): summary of standardised mortality ratios (SMRs) (Table 4.1A from Matanoski⁽¹⁶⁾). The SMRs for the comparison group (NNW) do not show a healthy worker effect, the most striking finding of the study.

Cause	SMR	95% CI	SMR	95% CI	SMR	95% CI
	>5 mSv		<5 mSv		NNW	
All	0.76	0.73–0.79	0.81	0.76–0.86	1.00	0.97–1.03
Leukaemia	0.91	0.56–1.39	0.42	0.11–1.07	0.97	0.65–1.39
LHC	0.82	0.61–1.08	0.53	0.28–0.91	1.10	0.88–1.37
Mesothelioma	5.11	3.03–8.08	5.75	2.48–11.33	2.41	1.16–4.43
Lung cancer	1.07	0.94–1.21	1.11	0.90–1.35	1.15	1.02–1.29

LHC = lymphatic and haematopoietic cancers

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OPPOSING THE PROPOSITION: J. R. Cameron

Argument

The critical word in the proposition is ‘useful.’ The supporter of the statement is thus obligated to demonstrate that such a relatively low dose rate has demonstrated negative health effects. I know of no such evidence. The best human epidemiological evidence that high dose rates do not cause life shortening is from the 100 year study of British radiologists⁽¹⁾. I will argue that dose rates of 100 mSv.y⁻¹ are almost certainly beneficial to the health by stimulating the immune system, reducing mortality from all causes⁽²⁾.

By opposing the proposition it does not mean I am against all personal monitoring of radiation workers. I feel the current monthly monitoring of radiation workers is a waste of money. I doubt if the monthly monitoring of millions of radiation workers in the last half-century has resulted in saving one life. I believe that each radiation worker should wear passive dosimeters, which can be read at any time to determine if he or she has had a significant over-exposure. With the current concern about nuclear terrorism, consideration should be given to including several passive dosimeters in each individual’s national identification card.

The proposed limit of 100 mSv.y⁻¹ is significantly lower than the first occupational radiation limits recommended by the ICRP and NCRP in 1934. The ICRP recommended 0.2 r.d⁻¹. The NCRP chose 0.1 r.d⁻¹ (Note: older units used). In either case the annual limit was greater than the proposed 100 mSv. In 1980 Dr Lauriston Taylor wrote concerning the safety of these early limits⁽³⁾:

No one has been identifiably injured by radiation while working within the first numerical standards set by the NCRP and the ICRP in 1934. The theories about people being injured have still not led to the demonstration of injury and, if considered as facts by some, must only be looked upon as figments of the imagination.

The implication of Dr Taylor’s statement is that our present very low annual limits were not determined by evidence of harm to radiation workers. The limits were

lowered based on non-scientific influences on radiation regulations.

There is no doubt that the very large doses to early radiologists in Britain significantly increased their cancer mortality⁽¹⁾. We do not know their annual dose rate but it was likely much larger than 100 mSv.y⁻¹. British radiologists who joined a radiological society between 1897 and 1921 had a 75% greater cancer mortality rate than all male physicians in Britain. While this suggests a significant health risk, it ignores the health benefits of the radiation. These early British radiologists had a 14% lower ($p < 0.05$) death rate from causes other than cancer than all male physicians in Britain. I suggest that this health improvement was due to stimulation of the immune system, which cancelled the excess cancer deaths⁽²⁾.

The early British radiologists had a slightly lower death rate from all causes than all male physicians. Since there was no life shortening from their high doses, the early radiologists had no increased health risk from their high radiation exposures.

In 1920 the British X ray safety committee became active and encouraged radiologists to lower their occupational doses. The committee did its job well.

If a moderate dose rate of radiation stimulates the immune system and a high dose rate increases cancer mortality, then there must be an optimum dose rate for good health. The British radiologist study suggests that British radiologists who joined a radiological society between 1955 and 1979 had close to the optimum dose rate, even though we do not know what that dose rate was. Their death rate from all causes was 32% lower ($p < 0.001$) than all male physicians in England and Wales.

These and other data in the British radiology study make clear that a moderate dose rate of radiation is beneficial to the health.

The nuclear shipyard worker study (NSWS)⁽⁴⁾ shows similar health benefits from low dose occupational radiation. In 1980, the US Department of Energy (DOE) gave a contract to the School of Public Health at Johns Hopkins University to study radiation risks to nuclear shipyard workers. The study, which extended

for more than a decade, cost the taxpayers \$10 million. This was the world's best epidemiological study of nuclear workers. Unfortunately, the study has yet to be published more than 13 years after its completion in early 1988. Although the nuclear shipyard worker data have not been published, the study had excellent peer review during its duration. The DOE contract provided for peer review twice a year by a panel of eight scientists, comprising the Technical Advisory Panel (TAP), with expertise relevant to the research. The scientists who served as members of the TAP were Arthur Upton, (chair); Gilbert Beebe, John Cameron (the author of this article), Carter Dennison (who resigned in 1983), Merrill Eisenbud, Philip Enterline, Philip Sartwell and Roy Shore. The TAP met twice a year to review data, question the scientific staff and make suggestions. Early in 1988, the TAP approved the draft of the final report. The summary in the final NSW report (p. 393) states: 'Therefore this is an ideal population in which to examine the risks of ionizing radiation in which confounding variables can be controlled.'

The NSW results support the hypothesis that a moderate dose rate of radiation is beneficial to the health. The 28,000 nuclear workers with the largest cumulative doses (>5 mGy) group had a death rate from all causes 24% lower than 32,500 age-matched and job-matched controls. That is, their death rate was 16-standard deviations lower than the controls ($p < 10^{-16}$). In addition the cancer mortality of the exposed group was significantly lower ($p < 0.001$) than the unexposed controls.

There is other evidence of health benefits of low doses. Feinendegen *et al*⁽⁵⁾ showed that an acute dose of 100 mGy was about optimum for stimulating the immune system.

If the aim of NSW had been to look for health

benefits of ionising radiation, it would have been a huge success. As a study to find radiation risks, it was an abysmal failure. This may explain the reason the study has yet to be published. I published a brief summary of the results in 1992, shortly after the final report was submitted⁽⁶⁾. I know of no other publication providing the critical data of this important study. The nuclear shipyard worker study supports the hypothesis that an increased amount of radiation stimulates the immune system. The failure of the US DOE to insist that the data from this \$10 million study be published is inconsistent with their duty to responsibly manage radiation work.

Rebuttal

I see no data in Strom's main argument which indicate that real lives are saved by monitoring doses less than 100 mSv.y⁻¹. Mountain states in the US have a background dose rate three times that of the Gulf States but their cancer mortality is 25% lower in the mountain states⁽⁷⁾. There is no indication that Drs González or Strom have considered the situation in the real world rather the theoretical linear, non-threshold world.

Radiologists have had the highest occupational doses, at least for the first half of the 20th century⁽¹⁾. Table 2 is extracted from Table 2 of that article⁽¹⁾ to show that radiologists did not suffer any life shortening in comparison with all male medical practitioners in England and Wales. That is, monitoring their doses would not have saved any lives. One can make a case for current radiation regulations being a health hazard by reducing radiation doses below the optimum needed to stimulate the immune system. From Table 1, it appears that the optimum annual dose rate is about equal to that received by British radiologists who entered the field in 1955–1979.

Table 2. Deaths of radiologists by cause and year of first registration. Standardised mortality ratio (SMR) calculated using rates for all male medical practitioners in England and Wales. Adapted from Table 2 of Berrington *et al*⁽¹⁾.

Cause of death	Year of first registration							
	1897–1920		1921–1935		1936–1954		1955–1979	
	Deaths	SMR	Deaths	SMR	Deaths	SMR	Deaths	SMR
All causes	290	0.97	271	0.92	368	1.00	113	0.68*
All cancers	60	1.75*	51	1.24	83	1.12	32	0.71
All non-cancers	230	0.86†	219	0.86†	278	0.95	77	0.64*

* $p < 0.001$.

† $p < 0.05$.

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SUMMARY

Questions such as ‘How small is small?’ and ‘How low is low enough?’ have long plagued radiation dosimetrists and risk management personnel. Unfortunately, our knowledge about the biological effects of low levels of ionising radiation is scarce and uncertain. If we look to the results of epidemiological studies, we find that it is not easy to arrive at firm conclusions. However, some current radiobiological experiments using microbeams of various radiations, along with improved theoretical models of radiation action, may shed new light on the effects of low levels of ionising radiation. What shall we do in the meantime? Both of our debaters agree that monitoring of radiation workers is necessary, yet careful consideration must be given to the rationale for providing personal monitoring. There is no question that we have done a good job of protecting radiation workers for many years, but we also must be aware of the many implications of our efforts.