THE LNTH IS DOING MORE HARM THAN GOOD

Dear Editors:

On page 86 of the July, 2003, issue of *Health Physics* (Brooks 2003) Tony Brooks, associated with the DOE Low Dose Radiation Research program, states: "It is interesting to note that the number of observed accidental deaths produced by the clean-up process was greater than the number of lives calculated to have been saved by the remedial action (Church 2001). This of course results in a transfer of (hypothetical) risk from a potentially exposed population to the worker population (parenthetical word added)." The author is describing what has actually happened at clean-up sites in the U.S. Department of Energy programs. Earlier in the paper the author states: "Since following low dose radiation exposure the number of excess cancers cannot be measured, it becomes necessary to use the predicted number of cancers derived by a LNT calculation to determine if a clean-up activity decreases the risk for cancer. Using this methodology the number of excess cancers produced at DOE clean-up sites has been calculated. These are calculated (hypothetical) deaths rather than demonstrable deaths (parenthetical word added)."

Section 1 of the "Objectives and Purposes" of the Health Physics Society states, in part, "The SOCIETY is a professional organization whose mission is excellence in the science and practice of radiation," and "SOCIETY members are involved in understanding, evaluating, and controlling the potential risks from radiation relative to the benefits."

Given the information in the first paragraph, it seems that the Society is not doing a very good job in understanding and evaluating the risks from radiation at DOE clean-up sites relative to the benefits. Neither is the DOE. Many members of the society work for DOE or its contractors. It is to those members that this letter is addressed.

I have read Bruce Church's paper, "Environmental Remedial Action—Are We Doing More Harm Than Good?" (Church 2001), and I am completely persuaded that continued use of the LNT hypothesis is absolutely wrong to arrive at a numerical value for risk at low doses, particularly when we are killing people during clean-up for no measurable benefit. The HPS agrees in its position statement "Radiation Risk in Perspective" where it is stated: "In view of the above, the Society has concluded that estimates of risk should be limited to individuals receiving a dose of 5 rem in one year or a lifetime dose of 10 rem in addition to natural background. Below these doses, risk estimates should not be used; expressions of risk should only be qualitative emphasizing the inability to detect any increased health detriment (i.e., zero health effects is the most likely outcome)."

The Society remains unwilling to state in a position paper that low doses are safe, in the usual sense of that word (not absolutely safe, but acceptably safe). However, it is apparent that simply making such a statement does not result in "excellence in the practice of radiation." Until health physicists both inside and outside government insist on implementing the Society's position set forth above, we, as a Society, will continue in some cases approving killing people with no measurable benefit from their deaths. Use of the LNT hypothesis must stop!

In 1958, Austin M. Brues published in Science (Brues 1958) a "Critique of the Linear Theory of Carcinogenesis." In the summary he states: "Present data on human leukemogenesis by radiation fail to indicate a linear relation between dose and effect. Because data are scanty, such a hypothesis cannot be ruled out statistically but it is less probable than a nonlinear or threshold relation." His arguments are applied to leukemia, but could be applicable to other cancers.

In light of the above information, would those of you in the DOE family care to comment on how you can reasonably effect a change in the manner that DOE evaluates whether a clean-up operation should be done so that real deaths are not created and hypothetical deaths are ignored? Those in the EPA family might also comment. Is there anything more the HPS can do to assist in this matter?

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REPLY TO TSCHAECHE

Dear Editors:

I want to make it clear that I am not working for the DOE but for Washington State University Tri-Cities (WSU). I am part of a WSU group funded by a DOE grant to help facilitate outreach and interactions between radiation research and other scientific fields and to increase awareness of the scientific progress in radiation biology. I am responsible for the scientific content of the WSU Radiation Research Web site. I have nothing to do with DOE funding decisions or policy making. However, as a member of the Radiation Research Society, Health Physics Society, and the NCRP I personally take every opportunity to provide scientific input into matters related to health effects of radiation and radiation protection.

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It surprised me that Tschaeche used my *Health Physics* paper, "Developing a Scientific Basis for Radiation Risk Estimates: Goal of the DOE Low Dose Research Program" as a forum to discuss the problems associated with the LNTH. This particular paper was not intended to defend or refute this hypothesis. However, I have written other papers (Brooks et al. 2000) and helped to organize meetings and workshops that suggest that the LNTH needs continual re-evaluation. Such outreach has been useful to encourage dialogue between the scientists conducting basic research and those involved in decision-making processes. Currently, a great deal of very interesting data are being generated world-wide at the cell and molecular level that I think may impact the validity of the LNTH.

I have no argument with the discussion that is presented by Tschaeche and the paper by Church (2001) suggesting that we carefully balance the risk associated with any activity with the benefit of that activity. However, I do **not** agree with Tschaeche's statement suggesting that there are **no** benefits from current clean-up efforts. Clean-up provides many benefits including changes in public perception, containing radioactive contamination, and doing everything possible to limit even non-measurable human health effects. The serious question is how clean is clean enough? Attempts to clean-up to levels below natural background drives the cost up and I, like any tax-payer, wonder if the benefits are worth the time, energy, and money invested. The use of the LNTH of course supports and drives the perceived risk and the need to clean up to these very low levels.

The Health Physics Society remains a leader in health protection and can be proud of their past record in limiting radiation exposure and adverse health effects. The Society should, as it has in the past, continue to draw from the best science available in generating position statements and optimizing the day-to-day control of radiation exposure. The thrust of my paper in *Health Physics* and my hope for the future is that radiation protection and radiation standards will be based on scientific knowledge and not on paradigms or hypotheses that may not be valid. It may be necessary to discard the LNTH and other established radiation paradigms as we gain mechanistic understanding on how radiation interacts with biological systems.

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COMPUTED MAYAK NEUTRON DOSES

Dear Editors:

IN RESPONSE to the paper by Choe et al., "Calculated organ doses for Mayak Production Association Central Hall Using ICRP and MCNP," that appeared in the March 2003 issue of *Health Physics*, it appears that MCNP-computed fluences were used to compute organ equivalent doses in an incorrect manner. From the paper it appears that the authors used MCNP to compute energy-dependent neutron and secondary gamma-ray fluences in the organs of the phantom created with the BodyBuilder code. Then they apparently converted (or had MCNP via the DE and DF cards convert) these fluence spectra to organ dose equivalents using the ANSI/ANS 6.1.1/1977 conversion coefficients, which are taken from NCRP-38. These conversion coefficients are for computing deep-dose equivalent, or more appropriately maximum dose equivalent (MADE), from free-field fluences, *not* organ dose equivalents from in-phantom fluences. The origin of these conversion coefficients strongly demonstrates their inappropriateness to compute organ dose equivalents.

The NCRP-38 dose conversion coefficients were computed in the following manner. A cylindrical phantom was irradiated by monoenergetic parallel beams of neutrons. The neutron and secondary gamma-ray fluence spectra were computed in tally volumes in the cylinder (on the axis of irradiation and on a transverse cut through the center of the cylinder). These fluences were converted to absorbed dose using kerma factors. The NCRP-38 dose equivalents were computed for parallel, monoenergetic neutron beams of energy E_{inc} by numerically performing the following integrations using computed neutron and secondary gamma-ray group fluences inside the phantom tally volumes, namely,

$$H(E_{inc}) = \int\Phi(E'_n)Q_n(E'_n)K_n(E'_n)dE'_n + \int\Phi(E'_\gamma)\left[\frac{\mu_{tr}(E'_\gamma)}{\rho}\right]dE'_\gamma,$$

where $\Phi(E_n)$ and $\Phi(E_\gamma)$ are the energy-dependent neutron and secondary gamma-ray fluences in the tally volumes in the cylinder, $Q_n(E_n)$ is the energy-dependent neutron quality factor, $K_n(E_n)$ is the neutron kerma factor, and $[\mu_{tr}(E_\gamma)/\rho]$ is the energy-dependent gamma-ray mass-energy transfer coefficient. The first integral is the neutron dose equivalent *for the neutron fluence energy distribution inside the phantom tally volumes.* The second integral is the dose equivalent contribution due to the secondary gamma-ray fluence in the phantom. One is left to assume that the authors used the dose equivalent conversion coefficients found in Appendix H of the MCNP manual. The reference in their paper to quality factors on page 318 is somewhat bothersome as the coefficients already have the quality factor included in them. However, based on the values in Table 1, they apparently did not multiply the dose equivalent by the quality factor a second time.

The dose conversion coefficients in NCRP-38 (ANS6.1.1/1977) were created by selecting the maximum dose equivalent from the cylindrical phantom tally volumes for an incident neutron energy and dividing it by the fluence incident on the phantom. Therefore, the conversion coefficients the authors used can only be used to convert free-field fluences to MADE, **not** for converting in-phantom fluences to organ dose equivalents. The same is true of the NCRP/ANSI photon conversion coefficients — they can only be used with free-field fluences. A historical and technical development of neutron dosimetric quantities that might prove useful to the authors, if they want to continue to do work in this area, is available in Thomas (2001).

The appropriate approach to computing organ absorbed doses, the one that the authors should have applied, is to compute neutron and secondary gamma-ray fluence spectra in the phantom organs and fold them with energy-dependent kerma factors. (Incidentally, the F6 tally in MCNP does this for the user.) Keep in mind that this approach only holds when the kerma approximation to absorbed dose is valid. Because of the size of most of the organs considered in the paper, the difference between kerma and absorbed dose would be negligible for neutrons below 10 MeV. Armed with these organ kermas, one is faced with a decision as to which "dose" quantity to compute: organ dose equivalent (ICRP 26) or organ equivalent dose (ICRP 60). The ICRP 60-based quantity is the internationally accepted quantity.

To compute the ICRP 26 quantity, one folds the energy-dependent mean quality factors $[Q_n(E_n)]$ by the energy-dependent neutron kerma spectrum in the organs, not the effective quality factors from NCRP-38, and adds the result to the secondary gamma dose to obtain the organ "dose" value. Examples of this methodology are numerous in the numerical dosimetry literature and the authors are referred with some favoritism to Nabelssi and Hertel (1993). To compute the ICRP 60 "dose" quantity of interest, one merely multiplies the total absorbed dose (neutron plus secondary gamma-ray kerma) in the organ by the ICRP-60 radiation weighting factor for the *incident* neutron energy (E_{inc}). What can be inferred from the brief description of the authors' MCNP calculated organ dose equivalent quantities is that this was not the approach used. Instead they used the NCRP-38 (ANSI 6.1.1/1977) conversion coefficients to convert organ fluences to organ dose equivalent.

A few statements are in order about ICRP Publications 51 and 74 quantities as discussed in the paper. The quantities in these publications are defined differently (Thomas 2001). The organ dose equivalent conversion coefficients in the ICRP 51 are based on ICRP 26 quantities, and the ICRP 74 organ equivalent doses (ICRP 1997) were created using ICRP 60 methodology. The radiation weighting factors (ICRP 60) essentially have the much discussed "factor of two" change in the neutron quality (ICRP 1985) incorporated into them, at least over certain energy ranges, while the ICRP 51 values do not (ICRP 1987). This "factor of two" change is obvious for certain organs and irradiation geometries in the authors' paper, for instance AP irradiation of liver and lungs. Additionally, the values reported by the authors using their MCNP fluences and the NCRP-38 effective quality factors do not have the quality factor doubled (over the certain energy ranges), aside from their inappropriate use with in-phantom fluences. However, the authors apparently did use the ICRP 51 and ICRP 74 conversion coefficients correctly by folding them with free-field fluences.

There is, therefore, no reason to expect the organ "doses" computed with conversion coefficients from ICRP 51 and ICRP 74 to agree; in fact, the authors are comparing two differently defined quantities. The authors' statement about good agreement between their MCNP results and ICRP 74 do not seen to be corroborated by the values reported in Table 1, where for some organs the equivalent doses are up to a factor of four apart. What the authors have computed are the values of three different dose equivalent qualities, one of which (the MCNP/Bodybuilder phantom calculations) used the NCRP-38 conversion coefficients incorrectly. So the differences between their three sets of organ "doses" go beyond the geometry changes and organ additions as they indicate in the conclusion. Also the authors' use of the phrase "non-sex" specific phantoms when referring to the ICRP 74 conversion coefficients in the conclusions is termed unisex phantoms in ICRP 74, i.e., phantoms having both male and female organs. However, much of the data used in forming the neutron organ absorbed dose conversion coefficients in ICRP 74 were obtained by averaging the absorbed for male- and female-specific phantoms (see Section 4.4.5 of that publication). So this conclusion is not correct as it does not reflect the sources of the organ dose data used in ICRP 74 and their use to construct organ conversion coefficients. It also contradicts the authors' previous statement, which was more accurate, that "The data in ICRP Publication 74 were based on the male and female MIRD Phantoms . . .".

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REPLY TO HERTEL

Dear Editors:

THE AUTHORS are aware that Hertel's approach is valid and agree that such an approach may be preferred under specific circumstances. In his letter, Hertel expressed concern with the authors' choice of the tally option, which is allowed by the MCNP code, but he incorrectly assumed that the authors were unaware of his alternative approach. In fact, the authors typically perform both approaches in their dose construction activities. Under certain circumstances, dose outcomes are similar—given the uncertainties that are associated with the input information and the assumptions and data processing inherent in the guide documents. Hertel suggested care should be taken when performing any dose calculations to assure users stay within the limitations of the selected approach; the authors heartily agree.

The user of MCNP is faced with a decision well before the completion of the Tally 6 calculations as outlined by Hertel. An approach is influenced in part by the quality of the available input information, MCNP process, and the biases present in the guide documents.

Though NCRP-38 flux to dose conversion factors relate the dose to the free-field flux (flux incident on the phantom), it should be noted that one could place each organ individually in a neutron flux and use NCRP-38 to calculate the maximum dose equivalent, MADE, for that organ. Note that MADE always occurs in the first 6 cm and usually within the first 2–4 cm of the phantom. The neutron flux in the organ is not significantly different to the flux incident on the organ. While Hertel noted the weaknesses of using NCRP-38, he failed to mention the questionable quality of the data and of the assumptions found in the guide documents he cited.

Hertel affirmed in his letter that the authors correctly performed the calculation of organ equivalent dose rates by the ICRP 51 and ICRP 74 approaches, and of course the authors agree. He expressed concern about perceived contradictory statements concerning phantom types used in ICRP 51 and 74. He extracted a statement from the conclusion section, a section that highlighted the differences in phantoms used in ICRP 51 and 74. In context, the authors' statement is accurate. ICRP 74 included data from unisex (non-sex specific) phantoms while ICRP 51 did not include such data.

Hertel repeatedly stated that the authors compared ICRP 51 and ICRP 74, which is not correct. The authors understand these methods are different, even if not explicitly stated, as illustrated in the last sentence of the Introduction of their paper, "Comparison of the MCNP results with a known, accepted industry standard calculation of equivalent dose rates using the ICRP publications offered a way to validate the experimental nature of the simulation created for MCNP." The organ equivalent dose rates derived from MCNP were reasonable given the calculated dose rates derived via 51 and 74, and this fact ensured that the information from MAYAK and the subsequent modeling assumptions were real and portrayed as accurately as possible. The authors' paper is a simple check on modeling accuracy and MCNP ability, not a comparison between two ICRP publications. Thus, the authors stand by their conclusion that the MCNP model showed proper geometric dependence of the work environment.

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EVALUATION OF PATIENT AND STAFF DOSES DURING VARIOUS CT FLUOROSCOPY GUIDED INTERVENTIONS

Dear Editors:

I READ with great interest the article by Buls et al. in the August 2003 issue of *Health Physics* on their experience with patient and staff radiation doses during CT fluoroscopy (CTF) guided interventional procedures. As the authors point out, it is very important that CTF exposure time and tube current (mA) be kept as low as possible and that the real-time mode of image acquisition be avoided to reduce radiation dose to patients and personnel.

With this in mind, I was surprised to see that the image chosen for the front cover of the journal was from a CTF-guided lung biopsy using real-time CTF *and* that both of the radiologist's hands were in the beam during needle manipulation. I thought that, perhaps, the purpose of this cover picture was to reveal to readers that this practice must be avoided. While the authors later in the article suggest that "it is unacceptable to enter the primary beam with the hands and caution is taken to avoid this at all times," the figure legend states only that the radiologist's hands accidentally entered the primary beam. There is no mention that the radiologist could have used a standoff needle holder device, which is standard practice when using real-time CTF. Although needle holders are awkward, reduce tactile feedback, and can lead to an increased exposure time, not using them leads to unacceptable exposure levels to the radiologist's hands (Kato et al. 1996). I am concerned that needle holders are not being used as often as they should be. While real-time CTF may be helpful in the case of small mobile lesions where respiratory motion is a problem, procedures such as the example depicted on the cover where the lesions are large and fixed in position can be successfully and efficiently performed using the intermittent (or "quick-check") mode of CTF with a significantly lower exposure time (Carlson et al. 2001).

I believe *Health Physics* missed two opportunities when putting this image on the front cover. One was the chance to deny placing it on the cover because "we're in the business of radiation safety" and this clearly is not practicing radiation safety principles. The other was having the image on the cover with an insert stating how this may occur in the radiology practice and that radiation safety specialists must do what they can to minimize or eliminate such practices. I believe that the most important aspect of reducing dose in CTF procedures is educating radiology and

safety personnel on the safest way to use the technology. The authors and editor both missed an opportunity to educate readers on the potential for improper use of the real-time mode and the importance of keeping hands out of the CT beam.

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REPLY TO CARLSON

Dear Editors:

WE THANK Carlson for her comment on our article. First of all we would like to state that the procedure that is shown on the image was not a part of our study. We included the image to the article as it clearly illustrates the potential danger of entering the primary beam during CTF procedures when no proper attention is paid to radiation protection. From this point of view, the image is valuable to the article as it draws the attention to the readers of this potential and encourages them to read the article. Of course, entering the primary beam with the hands is unacceptable as we clearly stated in our text.

Although we agree with Carlson that the use of a stand-off needle holder would prevent a situation as illustrated by the image, we also believe that the use of intermittent fluoroscopy would prevent this. As we have no significant experience with the use of needle holders during CTF procedures, we are not in the position to profoundly evaluate this tool. However, we are not entirely convinced that the use of a needle holder during CTF procedures automatically reduces exposure to the hand of the physician. This strongly depends on the type of CTF scanning applied by the user, namely, the intermittent method or the real-time method. With intermittent fluoroscopy the hand can be completely retracted during scanning, as with a needle holder the potential exists to perform a larger fraction of real time scanning while holding the device. This is for example illustrated when comparing our data to the data from Irie et al. (2001), as also shown in Table 6 of our article. They measured comparable doses to the physician's hand with the use of a 7 cm length needle holder, besides the fact that the integrated tube current-time product (mAs) of their study was more than a tenth lower than in our study. This indicates that the method of scanning plays an important role and also that there is a significant potential for dose reduction when using intermittent CTF with a low mA technique. Moreover, further dose reduction can be achieved by combining this technique with new technical developments such as interrupting the x-ray exposure when the tube rotates above the patient.

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TRANSURANIC ISOTOPES AND ^{90}Sr IN ATTIC DUST IN THE VICINITY OF TWO NUCLEAR ESTABLISHMENTS IN NORTHERN GERMANY

Dear Editors:

Your publication linking the "derived release of alpha emitters" from the Krummel nuclear power plant with the incidence of a leukemia cluster in children (Schmitz-Feuerhake et al. 2003) offers little support for its conclusions.

In summary, the paper concludes that the Krummel nuclear power plant released non-permitted levels of radioactivity into the environment. The underlying radiation event possibly occurred on 12 September 1986 based on local newspaper reports. An investigation of chromosome aberrations in people around the plant indicates a kind of continuous exposure from incorporated radionuclides with long effective half-lives. This radiation exposure is "assumed to have contributed to the induction of a leukemia cluster in children," which was observed near the plant between 1990 and 1996.

Basic nuclear engineering and radiation protection science requires that for the Krummel nuclear power plant to be the source of transuranic nuclides emitted into the environment, the plant would have had to have operated with major fuel cladding failures. Under these conditions, other fission products such as ^{137}Cs in the gaseous effluents would dwarf the levels of transuranic nuclides.

These prerequisite conditions would have been unmistakably present in the plant's effluent and environmental programs. The presence of ^{137}Cs in the people living around the plant would be readily measurable with whole body counters even years after exposure.

In the absence of these prerequisite conditions, this paper seeks to prove its intended conclusion by radiological analysis of attic dust. But even in the attic, with the exception of a single

measurement, the difference between the five "selected" sample locations and five control locations is not remarkable. The possibility of finding an elevated measurement at a sample location was enhanced by systematic bias. That bias resulted from pre-screening twelve sample locations for elevated levels of gamma activity prior to further analysis. No such treatment was afforded control locations.

This paper appears to begin with a predetermined conclusion. It continues by selection of data to support this conclusion while ignoring all other facts that refute it. For these reasons, this paper fails by a wide margin to meet the minimum requirements of a scientific investigation.

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Schmitz-Feuerhake I, Mietelski J, Gaca P. Transuranic isotopes and 90Sr in attic dust in the vicinity of two nuclear & establishments in Northern Germany. Health Phys 84:599–607; 2003.

RESPONSE TO M. J. RUSSELL

Dear Editors:

WE REGRET that Russell misunderstood one of our main conclusions. In contrast to his assumptions, we derive in our paper that the nucler power plant Krümmel cannot be the origin of the observed transuranic contamination because the high proportion of ^{241}Am relative to plutonium is not compatible with the inventory of a light water reactor.

The described investigation was one single step — even though a relevant one — in a series of scientific efforts to detect the causes of the childhood leukemias. The observed cluster in the proximity of the Geesthacht nuclear establishments is unique in its spacial and temporal concentration (Kaatsch et al. 1996) and has caused considerable public concern in Germany. The cases and the living conditions of their families were carefully studied by the authorities. No common risk factor could be identified other than the local affinity to the potential sources of radioactivity.

Early certainty about an exposure of the population far above the permitted limit was achieved by us by the mentioned chromosome study (Schmitz-Feuerhake et al. 1997) because the dicentric assay can be regarded as radiation-specific (Hoffman and Schmitz-Feuerhake 1999). Whole body counting was not considered because the investigations began several years after a putative emission and the biological half-life of ^{137}Cs is assumed to be only 100 d (such studies were nevertheless done by others and were negative).

The dust samples were taken in those parts of the community where the leukemia cases had occured and the rate of dicentric chromosomes in the adult inhabitants — 7 of them parents of leukemia children — was high. It was not the aim of our study to compare mean concentrations of radionuclides in the suspicious region with concentrations elsewhere, but rather to seek for abnormal contaminations — perhaps occuring as "hot spots." The control samples were taken after the first Elbmarsch (screening) measurements had shown surprisingly high levels of ^{241}Am in order to assure the expected combination of transuranic nuclides from nuclear weapons fallout because reference values in attic dust had not been published before. The mean concentration in Elbmarsch and controls was not stressed to draw conclusions; therefore, there was no bias due to the selection of samples.

In the meantime, another expert group (ARGE PhAM, Weinheim) chaired by Arthur Scharmann, Giessen, has also found transuranic nuclides in the same region. This group detected, moreover, different fractions of heavy metal microspheres in soil and dust samples (diameters 2 to more than 100 μm) consisting of alleged special fuels which they assign to nuclear experiments with hybrid systems (fusion + fission). Up to now, we are not sure if these were released at the September 1986 event. This event was not only registered in the local newspapers — as Russell notes — but also reflected in the environmental monitoring data of both the establishments.

Links between leukemia and radioactivity of a nuclear facility were shown in several case-control studies (Gardner et al. 1990; Morris and Knorr 1996; Pobel and Viel 1997). These previous findings rather than a "predetermination" have led us to conduct this study.

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