a mapping for any device, and a paper describing this will soon be produced.) Different normalized display devices then become comparable across all images in that no contrast mapping will change the result of the comparison for an image. Two devices using the same scale but in opposite order can be shown to be entirely equivalent after normalization, so the black-on-white against white-on-black controversy can be dispensed with as a badly put question. Finally, given these linear normalized devices, one can confidently design contrast mappings to emphasize a part of the recorded image intensity range in which one knows that intensity changes are more significant or more likely for a particular class of images. This contrast mapping can be applied to recorded images in this class before feeding the result into the normalized device.

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REFERENCE

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Reply

I thank Dr. Pizer for his comments regarding my recent communication (*1*). I appreciate that a comparison will depend on the sensitivity curves of the various displays and that it would be a good idea to normalize these in the way suggested. Unfortunately—at least until Dr. Pizer's work is published—little information is available on how this should be done for TV displays, and attempts to tackle the same problem for film have resulted in controversy (*2,3*). Also, displays that have well-defined discontinuities, like the "geographical," cannot be so normalized.

Because of this, I decided to compare mappings that were pre-set by the manufacturer and therefore in routine use (at least by purchasers of the same equipment). However, I realized that different mappings of the same display would produce different results, and this is why I included a luminance table and the contribution of each primary to white (or grey), thus uniquely defining the mapping used in each case. The fact that, for both organs, "black-on-white" and "white-on-black" produced results that were not significantly different, might suggest that in these cases the manufacturer has used mappings that are not greatly different from the normalized versions.

I feel that the results I obtained are useful to those using similar TV displays, and may be compared with (and possibly explained by) any subsequent publication on display sensitivity curves. I consider the procedure adopted by Pizer and Chan (*4*) to be well conceived, and look forward to reading their forthcoming paper.

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Re: Skin Decontamination of Commonly Used Medical Radionuclides

Skin contamination among nuclear medicine personnel is an important subject, since it appears to occur more frequently than suspected; fortunately, the eventual internal contamination is relatively unalarming (*1*). Careful protective measures to minimize contamination, especially to hands (*1,2*), must be emphasized in nuclear medicine practice. Thus, the recent article by Moore and Mettler (*3*) on this subject is welcome. However, the misleading conclusion in their article could have serious implications to the nuclear medicine community.

We have studied skin decontamination of various Tc-99m-labeled compounds using tap water with and without ordinary soap (*4*). Thus, the following refers to Tc-99m compounds only. The techniques we employed were serial washings and counting the hands in a fixed geometry using a well counter. Radionuclides were spread over the ventral aspect of a finger. Hands were washed for 10 sec without soap and for 15 sec with soap. This was about the length of time spent by people washing their hands in an ordinary situation. Decontamination was tested in three individuals. With all Tc-99m radiopharmaceuticals more effective decontamination was consistently observed with soap, but achievement of 5% of the original level after the fifth washing was not uniformly observed: TcO_4^- 7.3 ± 3.2%, HEDP 10.2 ± 2.5%, and DTPA 5.9 ± 0.5% (mean ± 1 s.d.). Only MAA was washed off to a 1% level (1.0 ± 0.4) after the first washing with soap, and after the fifth washing this had been further reduced to 0.5 ± 0.2%. Sulfur colloid (SC) could be removed effectively with soap, but 2.1 ± 1.2% remained after the fifth washing. In contrast to the above, when soap was not used, the remaining activity after the fifth washing was far higher: TcO_4^- 19.4 ± 15.6%, HEDP 49.4 ± 9.5%, MAA 2.1 ± 0.8%, DTPA 15.7 ± 4.8%, and SC 8.8 ± 4.6%. We agree that TcO_4^- is one of the most difficult agents to remove once it has contaminated the skin, but we disagree strongly with their conclusion: "Little difference was found between the effectiveness of *tap water, soap and tap water. . . .*" (our italics).

The inference from this is that nuclear medicine personnel can eliminate contamination with tap water alone as easily as with tap water and soap. This conclusion is wrong. Their technique using high activity (1.0 mCi or 37 MBq) and a gamma camera is crude, which could account for the disparity in conclusion. Spreading radionuclides over the dorsal surface of the hand and forearm could result from an accidental spill, but a smaller area of contamination on the ventral aspect of the hand is more frequently observed in nuclear medicine practice. Incidentally, both MAA and sulfur colloid are easy to wash off, but the transfer of activity to the opposite hand when soap was not used was 1-1.5% of the original activity after the fifth washing—a potential additional risk. With soap, 0.03% or less was found on the opposite hand. Information supplied by these authors in terms of Tc-99m-labeled compounds (*3*) should be carefully re-examined, since we believe that this is seriously misleading to our profession.

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Reply

We have the following comments to the letter by Nishiyama et al.

In the first sentence of their letter they indicate that the eventual internal contamination resulting from diagnostic nuclear medicine nuclides is "relatively unalarming." They follow this by stating that our conclusion has serious implications for the nuclear medicine community. I am unable to discern from the remainder of their letter exactly what these serious implications are, especially since they regard the eventual internal contamination as unalarming.

They have compared our experiment with their own, which is essentially comparing apples and oranges. They themselves state that their results apply to technetium compounds only, whereas ours included iodine-131 and indium-111 as well. Additionally, they spread the radionuclides over the ventral aspect of a finger, rather than on the forearm or wrist. Clearly, the skin is thicker on the palmar aspect of the hand and, if there is any binding at all to the skin surface, it would be more difficult to remove there. Our washings were for a period of 30 sec, whereas theirs lasted 10 sec without soap.

We find it of interest that they object to the use of a high activity, and state that this may be a source of error. In fact, the random variations incurred with lower amounts of activity are much greater; they indicate, for example, that the value for TcO_4^- is 19.4 ± 15.6%; i.e., in some of their analyses, the apparent standard deviation was approaching 80%.

Overall, we do not believe our conclusions are misleading and, in point of fact, Nishiyama's results may also be correct. One fact that they did not address, and one might assume that they agree with—is that the value of chelating agents is no better than that of soap and water in these instances.

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