camera, one or three-detector SPECT systems, PET cameras with no rings or with four or eight rings and septa present or absent (for optimized three-dimensional sampling). Finally, the intervention can be exercise, dipyridamole, adenosine or dobutamine. In this multidimensional decision matrix, there is indeed a pressing need for narrowing the duties of the jury!

Because of my NIH-sponsored research results, I have taken great pains to explore the science behind the detection of viable myocardium with FDG. We have published an editorial that addresses this matter directly and concludes with several scientific reservations on cardiac FDG studies (*2*). Of significance, Gropler and Bergmann (*3*) and Lear (*4*) have recently published editorials in *JNM* in which other reservations are voiced. Without being redundant, our consensus is that FDG does not yet pass scientific tests for being the gold standard for myocardial viability. At the recent meetings of the American Heart Association, Pohost concluded that SPECT scintigraphy is most costeffective for detection of myocardial viability. Among the agents for this purpose are ^{201}Tl, ^{99m}Tc-hexamibi and ^{99m}Tc-teboroxime. If FDG is to compete with SPECT scintigraphy, multicenter trials of the type recommended by the AHA (*5*) are needed to identify the subsets of CAD patients requiring FDG studies. Incidentally, echocardiography with dobutamine is another competing modality since it can also give viability information.

In a previous letter in this *Journal*, I stated that I disagreed with the position of the SNM and the ACNP that PET radionuclides should be exempt from efficacy appraisal by the Food and Drug Administration (FDA). If that were the case, then reimbursement, as discussed in the editorial, would be the only way for efficacy testing outside of the jurisdiction of FDA. That would be unfair to other radiopharmaceuticals, such as monoclonal antibodies, none of which have been approved for human use. Also, it is obvious that trials after reimbursement but without the FDA clinical phases process, might be biased since a $5 million PET investment is a strong incentive for obtaining positive results.

I think it is time for nuclear cardiologists to face the challenge and jointly establish the clinical utility of different modalities for assessing myocardial perfusion and viability.

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Clinical PET: The Debate Continues

REPLY: The dichotomy of opinion of McIntyre et al. and Bianco about our editorial (*1*) highlights the need for a well-defined comparison of these radionuclide imaging modalities. We would like to thank MacIntyre et al. for pointing out an error in our editorial: only initial images are required for the detection of coronary artery disease with myocardial perfusion imaging.

It is clear from their comments that MacIntyre et al. are PET enthusiasts, convinced of the superiority of PET for cardiac applications. Based on theoretical considerations, review of the literature and our own personal experience, we tend to agree with them. However, it seems prudent to withhold our unconditional endorsement of cardiac PET for clinical decision making until irrefutable data on a large number of patients are available. Although measurements of resolution, statistical reliability and lack of attenuation artifacts suggest that PET based myocardial perfusion studies are superior to single-photon methods, the clinical community needs to know whether these technical advantages benefit patient care.

An area where PET could make a major contribution to nuclear cardiology is in differentiating areas of ischemic but viable myocardium from scar. Although we did not cite many references in this area, our discussion made the point that the quantity of supporting data for the utility PET for making this distinction is not overwhelming. Bianco is less enthusiastic about the future of clinical cardiac PET. He questions the use of ^{18}FDG as a "gold standard" for evaluating myocardial viability and suggests that a large multicenter trial will be required to define those clinical situations where PET studies are required. Overall, we agree that only the objective scientific observations of a multicenter trial can establish the clinical utility of PET.

Many questions remain about the use of FDG for quantifying myocardial viability. Should these studies be performed under fasting or glucose-loaded conditions? If glucose-loaded, should it be after a single bolus or under controlled "clamp" conditions? These questions suggest that an alternative to metabolic imaging should be sought. A quantitative myocardial perfusion study may provide information about viability. In this regard, we would like to mention the results of a recent study performed in our laboratory by Gewirtz (*2*). In a group of 28 patients with previous myocardial infarction, we compared the results of quantitative blood flow studies with $^{13}NH_3$ to ^{18}FDG studies (with glucose loading) for detecting viable myocardium. In this group of subjects, myocardial blood flow in regions of previous infarction (MI) was significantly lower than in zones of normal myocardium (NZ): 0.33 ± 0.24 versus 1.02 ± 0.48 ml/min/g, $p < 0.01$. Border zone regions (BZ) showed intermediate reductions in flow: 0.68 ± 0.34 ml/min/g ($p < 0.01$ versus MI and NZ). Mismatches of flow and FDG occurred in only six zones (five patients) of which five (4 BZ, 1 MI) had flows of greater than 0.40 (0.65 ± 0.08). Perfusion and FDG accumulation were well correlated in MI zones. This study suggests that in patients with previous MI, viable myocardium is unlikely to be present in areas in which flow is < 0.40 ml/min/g.

To end the speculation about PET, we need to work with funding agencies to organize and perform a multicenter trial to define the clinical utility of PET imaging procedures.

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The Role of Gallium Scanning in Staging Lymphoma

TO THE EDITOR: Fox et al. (*1*) suggest that routine ^{67}Ga-citrate imaging in staging untreated patients with lymphoma is not currently justified. In reaching their conclusion, these authors have focused on test sensitivity; however, the critical issue should be whether management strategy is modified due to abnormalities on the ^{67}Ga scan which are not prospectively detected by other investigations. In this regard, data are emerging which illustrate the contribution of ^{67}Ga scintigraphy to treatment plans in some patients with Hodgkin's disease.

Jochelson et al. (*2*) demonstrated that ^{67}Ga imaging optimized radiotherapy treatment plans in 3 of 26 (12%) patients with Hodgkin's disease, by providing information incremental to chest radiographs and CT scans. In a preliminary report from my institution (*3*), a similar proportion [2 of 13 (15%)] of newly presenting patients with Hodgkin's disease had initially intended treatment altered because of lesions prospectively identified by ^{67}Ga scan alone. Other investigators have suggested that the combination of ^{67}Ga scintigraphy and CT scanning may be valuable in reducing the need for staging laparotomy in selected patients with Hodgkin's disease (*4*). In contrast to early publications quoted by Fox et al. (*1*), these recent series are characterized by high dose ^{67}Ga imaging with modern gamma cameras and improved collimation.

In non-Hodgkin's lymphoma, I agree that the ^{67}Ga scan has less influence on initial treatment decisions (*3*), probably because precise anatomic delineation of disease is not as important as pathologic status (*5*).

In summary, recent reports indicate an important adjunctive role for ^{67}Ga scintigraphy in staging Hodgkin's disease. Some patients are "upstaged" as a result of the ^{67}Ga scan, a feature which may justify its routine use. Further studies examining the impact of ^{67}Ga imaging on treatment decisions are warranted.

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REPLY: Dr. Larcos has suggested a greater role for gallium imaging in the pretreatment evaluation of lymphoma patients than our paper suggests (*1*). Several references are provided to defend this position. In once series, pretreatment gallium scanning altered radiation treatment planning on 3 of 26 patients with Hodgkin's disease, but did not alter clinical staging (*2*). In another series, 16 Hodgkin's patients with normal abdominal staging CAT scans and no gallium uptake in the abdomen proved to have negative abdominal staging laparotomy (*3*).

The staging evaluation of Hodgkin's disease is laborious, essential and controversial. The role of staging laparotomy has been argued for years; the role of bipedal lymphangiography is not fully resolved despite its continued, routine use at our institution for almost twenty years.

We believe that all patients with Hodgkin's disease should undergo, when feasible, pretreatment gallium imaging. But under what circumstances will a patient be truly "upstaged," and will such "upstaging" reliably alter treatment planning? For example, will a patient with clinical Stage IA disease in the neck, whose mediastinum and abdomen are normal by plain radiographs, CT scans and lymphangiogram, have his further workup altered by gallium positivity in the mediastinum? The answer is no, as staging laparotomy is still indicated, in our opinion. If the same patient were to show gallium positivity only in the abdomen, would we have enough confidence to obviate laparotomy, and to "upstage" the patient to IIIA and possibly commit him to chemotherapy? The answer, in our opinion, is no.

Gallium scanning has not yet worked its way into the staging algorithm for Hodgkin's disease—we fully agree that its value can only be determined by further study. We advocate pretreatment gallium imaging in all patients with Hodgkin's disease to help gather this data.

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