optimal treatment strategy when these results appear to be valid in further studies. Screening for thrombosis will probably result in the lowest mortality, which would be a further reason to validate the results of Hull et al.

3. The morbidity rates in thier article are determined only by the number of patients receiving anticoagulant therapy. Venography has a higher morbidity than angiography (10% versus 5%) simply because more patients with thromboembolism are detected. In 47% of all patients, venous thrombosis is found by venography (about 54% of those also having pulmonary embolism (PE)) and only in 36% of all patients is PE found by angiography (about 71% of those also having thrombosis). So, one can then calculate that if all patients are examined with angiography only, thrombosis in not less than *22%* of the total patient population remains undetected and untreated (22% = 47% − (36% × 0.71); 0.71 = the fraction of patients with PE with simultaneous thrombosis receiving anticoagulation therapy because of PE). Not detecting and not treating thrombosis in this large patient population would undoubtedly result in cases with a higher risk of progressive thrombosis and future PE in addition to a much higher mortality rate than that assumed in Quinn and Butler's article.

Our conclusion is in sharp contrast to the results quoted by the authors from the study of Novelline et al. (*3*) on the excellent prognosis of patients with a normal angiogram. What is the cause for this great discrepancy? It is because the results of the study by Novelline et al. cannot be used in this analysis because a selected patient population with a low prevalence of thrombosis was studied. In the Novelline et al. study, there were few patients with thrombosis (41 of 180 patients had lower extremity complaints, but only 3 of the patients who had received anticoagulation therapy, had thrombosis proved by venography). The study by Novelline et al. does not therefore permit the conclusion that it is safe not to treat patients with venous thrombosis and a normal pulmonary angiogram. It therefore cannot be used in comparison with an unselected patient population suspected of PE with a high frequency of thrombosis (47%) (*4*).

The low frequency of thrombosis in the retrospective study of Novelline et al. is probably caused by a normal selection bias that results in a patient population in whom pulmonary angiography is performed: patients without symptoms of venous thrombosis. In clinical practice, many patients with thrombosis and an indeterminate probability lung scan will receive anticoagulant therapy anyhow, and there is less urgency to confirm PE angiographically.

We therefore think that Quinn and Butler's analysis is incorrect for the angiography branch. The mortality rate will be much higher if the thrombosis remains undetected and untreated. If thrombosis is detected (e.g., because of a venography study after obtaining a normal angiogram or a venography study in all the patients with some clinical suspicion of thrombosis), the morbidity rate will be as high as that in the venography branch. Mortality will increase only about 0.2% [22% × 0.990 (the risk of anticoagulant treatment)].

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REPLY: Drs. Kengen and Piers raise several points in regards to our paper (*1*). Our responses are as follows:

1. We agree that in comparing the various morbidity and mortality results that the confidence limits are important with regard to statistical significance. We believe, however, that clinical decisions are currently being made by use of these data. Although these decisions may be based on small numbers, there are no other figures available in the medical literature. We have also tried to improve the significance of the results by using the most favorable data available for the sensitivity and specificity of contrast venography as well as the most conservative figures for morbidity and mortality of anti-coagulation.

We do not feel that listing confidence limits improves the clarity of the analysis presented in our paper, because each decision was calculated using the highest sensitivity and specificity as quoted in the available literature.

2. We agree that the study of Hull et al. (*2*) is interesting. We wonder, however, whether these findings will be validated by other researchers.

We further wonder whether any such validation would be applicable to predominantly inpatient groups with a high prevalence of deep venous thrombosis.

We feel that until these results are validated, further speculation is not of value.

3. Our paper was designed to examine the optimal investigation and management of acute pulmonary emboli. Drs. Kengen and Piers have suggested that since 22% of patients with a normal pulmonary angiogram will have deep venous thrombosis these patients should be included in the morbidity and mortality figures. We believe this is inappropriate, because this does not represent a realistic clinical scenario.

We believe that their suggested alteration to the decision tree does not represent the normal clinical situation and, again, it may reflect the patient population studied by Hull et al. (*5*). If patients with known deep venous thrombosis diagnosed by contrast venography were included in his study population, then this would explain the high numbers of patients without pulmonary emboli who had deep venous thrombosis. We would presume these patients would be anticoagulated.

We wish to thank Drs. Kengen and Piers for their interest in our paper.

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Fast Diagnosis of Abdominal Infections with Technetium-99m-HMPAO-Leukocytes

TO THE EDITOR: I have read with great interest the article by Lantto et al. published in the *Journal* (*JNM* 1991;32;2029–2034). The labeling of leukocytes with ^{99m}Tc-HMPAO has attracted considerable attention and acceptance in recent years because of its high sensitivity, specificity and convenient availability. Image resolution, rapid and accurate diagnosis, especially for inflammatory bowel disease, are now being made accurately within 1–2 hr postinjection.

Our ongoing studies with ^{99m}Tc-HMPAO-labeled leukocytes correlate and readily support Lantto et al.'s observation that most abdominal infections are visualized at 1 hr postinjection. The cells were labeled by employing the modified Hammersmith protocol. This enables greatly increased labeling efficiency while reducing the volume of blood required for the procedure. Of the 32 patients studied, 25% showed RLQ activity at 1 hr, and 75% showed RLQ activity at 4 hr postinjection. The nonspecific activity primarily in the ascending colon precludes the usefulness of imaging for abdominal abscess at 4 and 24 hr.

At this institution, ^{111}In is used to label WBCs for studies of the abdomen, and images are collected at 1 hr for evaluation of inflammatory bowel disease and at 4 and 24 hr to detect abscesses. In the event that ^{111}In-oxine is not available, we label WBCs with ^{99m}Tc-HMPAO and image for 1–2 hr only.

We have elected to continue to use ^{111}In-oxine in order to avoid the chance of false-positive findings, until such time as its replacement by ^{99m}Tc-HMPAO can be justified on the basis of availability or other factors.

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REPLY: We would like to thank Dr. Karalasingam for his comments on our recent publication and are interested to know that he has observed that most infections are positive with ^{99m}Tc-HMPAO-leukocytes within 1 hr after reinjection. We also agree that the nonspecific bowel activity in the right lower abdomen hinders the correct interpretation of scintigrams when patients are imaged only at 4 and 24 hr. Therefore, we routinely use serial imaging before 4 hr to avoid misinterpretations. The possible activity in the intestinal background at this point is rarely seen and easily distinguished from true inflammatory activity. The frequency of RLQ activity at 1 and 4 hr reported by Dr. Karalasingam is surprisingly high and disagrees with our experience and results (*1*). We suppose that this might be due to some differences in the labeling technique and patient population.

At our institution, a great number of patients are imaged for suspicion of an acute abdominal infection, for which the early results are important. Infections and inflammations are visualized much earlier with ^{99m}Tc-HMPAO-leukocytes than with ^{111}In-leukocytes due to the better imaging characteristics of ^{99m}Tc, greater injection doses allowed by the lower radiation burden of ^{99m}Tc and the presence of plasma during the labeling procedure. Furthermore, according to our results, the duration of symptoms does not lower the sensitivity of early images. Previously, Schmidt et al. (*2*) have also reported that the speed of granulocyte accumulation is not affected by the duration of infection. So we have found that late images (after 4 hr) are useless and thus avoid unnecessary false-positives.

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