ever, the tumor did show up as a focal ‘hot spot’ on subsequent PET scanning (Fig 1). A gated MRI of the hot spot gave further, accurate localization. PET scanning therefore localized the tumor when other modalities had failed. In the second case (patient 2), an atypical carcinoid (moderately differentiated neuroendocrine tumor), the tumor was localized with a CT scan of the chest but subsequent PET scan revealed an unsuspected metastasis in the lumbar spine (Fig 2). The lumbar metastasis required orthopedic fixation before the surgery to remove the mediastinal tumor. Octreotide scanning was not performed in patient 2, but MIBG scanning revealed the same distribution of metastases as the PET scan, and the patient received radioactive (I131) MIBG treatment.

As far as we are aware, PET scanning has not previously been described as a useful investigative tool for neuroendocrine tumors of the thymus. However, on the basis of our limited experience with this condition, we would suggest that in addition to CT, MRI, octreotide, and MIBG scanning, PET may be a valuable tool. Where available, it could be used and evaluated further in patients presenting with thymic neuroendocrine tumors. The sensitivity and specificity of PET compared to octreotide and MIBG scanning is unknown and requires further investigation.

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**Blood and Starch in Cardiac Surgery***

To the Editor:

The recent article by Avorn et al (October 2003) purports to show an association between the use of hydroxyethyl starch (HES) and excessive postoperative bleeding after coronary artery bypass surgery (CABG). This article resurrects the unresolved controversy regarding the use of HES in cardiac surgery and the emerging concern of clinically significant bleeding. Several prospective randomized trials, observational studies, and meta-analyses have investigated the suspected association between HES use and bleeding after CABG.

Most randomized studies on HES and bleeding have failed to show any clinically significant bleeding differences. The published retrospective studies showing an increased incidence of blood loss have received the most press but are inherently limited due to study design. Cope et al retrospectively reviewed the use of hetastarch infusion based on perioperative exposure to HES and transfusion requirements during the first 24 h postoperatively. The selection bias of this study favored those with hemodynamic compromise or those with greater severity of illness. The meta-analysis by Wilkes et al shows that the difference in pooled mean blood loss in the albumin group was 487 mL compared with 789 mL in the HES group, a difference of 302 mL.

We believe that the article by Avorn et al fails to show an association between HES and postoperative bleeding, as cited in the majority of related studies. The title leads the reader to believe that hetastarch increases the risk of bleeding, but the authors did not report any single measure of bleeding. They did not account for the number of bleeding episodes nor did they use quantifiable measures, i.e., chest tube drainage volume. Some of the study design limitations were that no clear definitions of nonsurgical bleeding or proper criteria for correction of microvascular bleeding were used. Measurement of hematocrit from the drainage fluid and collection of blood samples for baseline

![Image 1. PET scan in patient 1 showing the thymic neuroendocrine carcinoma at right heart border.](Image)

**Figure 1.** PET scan in patient 1 showing the thymic neuroendocrine carcinoma at right heart border.

![Image 2. PET scan in patient 2 showing the thymic neuroendocrine carcinoma and lumbar spine metastasis.](Image)

**Figure 2.** PET scan in patient 2 showing the thymic neuroendocrine carcinoma and lumbar spine metastasis.
laboratory parameters indicative of blood loss would have been more reliable estimates of perioperative blood loss. These indicators should be considered as one of the primary outcome variables. Although transfusions may reflect bleeding, previous data\textsuperscript{5-8} suggest that transfusing blood in cardiac surgery is behavioral rather than a response to blood loss. Re-operation (exploration for bleeding) in coronary bypass surgery cannot be used as a measure of drug-induced coagulopathy unless one completely excludes other causes for bleeding, i.e., heparin-protamine activity, large-vessel bleeding, and overall patient coagulation status. Simple changes in preoperative, intraoperative, and postoperative hemoglobin and hematocrit values would help clarify their claims, although even these are confounded by volume changes.

The patients selected in the study by Avorn et al\textsuperscript{1} were those who had received \( \geq 3 \) U packed RBCs within 72 h after undergoing CABG procedure; all other CABG patients were control subjects. The higher comorbidity in the cases (HES group) may act as a confounder leading to higher transfusion rates as a result of lower transfusion thresholds for more severely ill patients.

The editorial accompanying this study recommends a change in practice without convincing evidence. Lack of sufficient data should not be replaced with personal interpretation of study results. This invariably can be misleading to the medical community.

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Unresolved Questions With the Use of Linezolid vs Vancomycin for Nosocomial Pneumonia

To the Editor:

We believe several issues must still be addressed in the study by Wunderink et al (November 2003)\textsuperscript{1} published in CHEST. First, it is very unlikely that only uncensored data were used, as mentioned under Fig 2, because not all of the patients died over the follow-up period. Moreover, the \( p \) value reported in the curves appears to have been based on a \( \chi^2 \) test, as suggested by the statistical analysis section, whereas it should have been based on the log-rank test, which appropriately accounts for length of follow-up and censoring over time. Furthermore, it is not clear at what time point the crude death rate was calculated for the logistic regression analysis and why logistic regression was used rather than proportional hazards regression, which would account for the length of follow-up and censoring.

Second, the clinical cure analysis in the methicillin-resistant \textit{Staphylococcus aureus} (MRSA) subgroup could be biased due to the different proportion of missing/indeterminate follow-up patients in each group, especially since those who died were counted as missing/indeterminate. A more appropriate analysis would have been based on cumulative incidence rates, which would account for the length of follow-up, censoring, and competing events such as death or toxicity.

Last, linezolid did not show significant beneficial results in the \textit{S aureus} subgroup. This is odd not only because the intention-to-treat (ITT) \textit{S aureus} subgroup was more than twice as large as the ITT MRSA subgroup (larger statistical power to detect a treatment effect), but also because vancomycin is obviously not an optimal therapy for non-MRSA strains, which made up more than half of the comparator group. Consequently, this potentially further decrease in the efficacy seen in the vancomycin arm could have amplified the treatment effect observed in the linezolid arm.