airway hyperresponsiveness of asthmatics and that of COPD patients is a complex and controversial topic which is discussed in an excellent recent review article.1 4 If Dr. Houben wishes to make a clear distinction between the two entities, he must propose an investigative algorithm that clinicians could use to "exclude" the possibility of asthma in COPD patients with reversible obstruction. Otherwise, the discussion is purely academic and can only confuse clinicians facing the problem of having to select patients for the dipyridamole-thallium test.

Indeed, by requiring that "any doubt about a possible asthmatic pathology be excluded", Dr. Houben unwittingly places far greater restrictions on the test than the simple caution we propose in our article. We believe that if there is any clinical suspicion of coronary artery disease, the benefit from the test in most COPD patients far outweighs the risks, but that aminophylline and an intubation tray must be on hand for the rare occurrence of acute respiratory distress. A prospective trial on the incidence of bronchospasm in COPD patients undergoing dipyridamole-thallium imaging is presently under way at our institution. Preliminary results suggest that mild bronchospasm occasionally occurs (and is rapidly reversed by administration of aminophylline), but we have not encountered any new episode of severe bronchospasm in about 50 COPD patients (out of about 500 total patients) studied to date. Furthermore, Dr. Houben appears to suggest that dipyridamole-thallium imaging is absolutely contraindicated in asthmatic patients. There are a few published reports of severe dipyridamole-induced bronchospasm in asthma patients,1 4 but does this warrant depriving all asthmatic patients of the test, irrespective of the severity of small airway hyperresponsiveness or only those who are theophylline-dependent? Further clinical studies are necessary to address these questions.

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Survival After CPR

To the Editor:

In the September issue of Chest, Drs. Tortolani and co-workers report on the efficacy of norepinephrine and lidocaine infusions during cardiopulmonary resuscitation (CPR) after asystolic cardiac arrest ("In-hospital cardiopulmonary resuscitation during asystole: therapeutic factors associated with 24-hour survival.") Chest 1989; 96:622-26). In their retrospective analysis, survival was significantly greater in patients receiving therapy with norepinephrine, lidocaine, or both infusions together, compared to patients treated according to a standard regimen that included atropine and epinephrine. The authors do not report the circumstances during CPR that may have led to the initiation of lidocaine or norepinephrine infusions. It is quite possible that these infusions were begun after restoration of ventricular electrical activity (ie, as therapy for a new rhythm following asystole). Lidocaine and norepinephrine infusions simply might have identified those patients who recovered ventricular electrical activity after asystolic arrest and thus be expected to have an increased likelihood of survival. It is therefore difficult to evaluate the findings of such a retrospective study, and emphasizes the need for a prospective and controlled evaluation of this problem.

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Hemorrhage After Bypass Surgery

To the Editor:

It was with great interest that we read the recent study by Del Rossi et al entitled, "Prophylactic treatment of postperfusion bleeding using EACA." We agree with their assessment that postoperative hemorrhage after cardiopulmonary bypass is a major cause of morbidity and some mortality. We also congratulate the authors on accomplishing a significant study with a large group of patients. However, there are several points that we wish to discuss.

The changes in coagulation function that occur during cardiopulmonary bypass are undoubtedly complex and probably affect all aspects of the dynamic equilibrium of clot formation and breakdown. Platelet dysfunction and protein coagulant precursor breakdown have been studied previously. It seems certain that platelet dilution, destruction, and dysfunction is the most common defect demonstrable.1 Del Rossi et al point out that plasmogen activation and
primary fibrinolysis often occur. The reported incidence of that plasminogen activation varies widely, dependant upon the study reviewed and the test utilized. What is clear is that the monitoring tests routinely utilized by most cardiac surgical centers do not assess the two major hemorrhagic diatheses just mentioned. Activated clotting time, prothrombin time, partial thromboplastin time, and the remainder of a routine coagulation profile do not examine platelet function and they offer only small amounts of information about fibrinolysis.

We have begun routine monitoring of patients with a viscoelastic test of whole blood coagulation, utilizing the Thromboelastograph (TEG), in addition to the ACT and coagulation profile. TEG assesses clot strength from the time of initial fibrin formation through clot development and platelet retraction until eventual lysis. Platelet activity (number and function together), primary fibrinolysis, diffuse intravascular coagulation, and hypercoagulation may all be identified with this technique. TEG provides accurate predictions of postoperative hemorrhage (85 percent predictive), whereas routine coagulation testing does not (≥50 percent predictive). In our series of 1,000 patients there was an 11 percent incidence of post-bypass primary fibrinolysis as monitored by the TEG.

If it is assumed that the 11 percent incidence found in our studies is predictive of postoperative hemorrhage, then Del Rossi et al should have seen 17 or 18 patients in their EACA and placebo groups who might have bled because of primary fibrinolysis. If treatment with EACA was effective only in those 18 patients, that would have been sufficient to show a significant difference in postoperative blood loss and transfusion requirements between groups.

In today's environment of transfusion avoidance due to AIDS and hepatitis, any technique that reduces the need for a transfusion is laudable. The use of EACA for the treatment of primary fibrinolysis may be such a technique. The use of D-8-arginine vasopressin (DDAVP) to enhance platelet and factor VIII activity in cardiac surgery has been advanced by some. Could combinations of both produce even more superior results than those demonstrated by Del Rossi et al?

EACA is not an innocuous drug. In the face of diffuse intravascular coagulation (DIC) it may worsen the clinical picture. DIC, thankfully, is rare in cardiac surgery. However, when it occurs it is not quickly differentiated from primary fibrinolysis by routine testing. TEG may, at least in an animal model, differentiate the two. Universal prescribing of any medication is suspect. We do not agree that routine use of EACA is warranted based upon the results of this study, as there are certainly some patients with undiagnosed DIC who will perish. Del Rossi et al state that patients receiving EACA had fewer major complications than the control group; however, no statistical difference was shown, only trends in very small groups. The claim to fewer complications cannot be validated without supporting statistical data.

In summary, this is a useful study which adds a valuable piece of information to our knowledge about coagulation function during cardiodiopulmonary bypass. We feel that the study demonstrates primary fibrinolysis to be a major cause of clinical hemorrhage. More important, however, the study also demonstrates the inadequacy of intraoperative coagulation monitoring. We feel that a treatment such as EACA should not be advocated for universal application or individually without demonstrable evidence of a disease process (ie, primary fibrinolysis), utilizing appropriate monitoring.

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To the Editor:

We thank Dr. Spiess and his colleagues for their interest in our article, as well as their kind remarks on the importance of the study. They bring up an interesting point regarding hemostatic defects after CPB. Although thrombocyte destruction and dysfunction is commonly seen, this appears more prominently at the end of CPB, and its magnitude is directly proportional to the duration of extracorporeal circulation. Hyperfibrinolysis, on the other side, appears to start with sternotomy and pleural and/or pericardial incision and reaches its peak one hour after the initiation of CPB. Unfortunately, the multiplicity of causes of deregulation of hemostasis and the variable course of these conditions makes clinical assessment of which component has more importance very difficult. Although we acknowledge that the bleeding tendency seen after CPB is multifactorial, there is enough literature support to show that fibrinolytic activity is present in up to 50 percent of patients undergoing open heart surgery with CPB. We believe that if the activation of plasminogen is allowed to proceed, multiple hemostatic changes occur, including thrombocytopenia and platelet dysfunction.

We certainly agree that routine hematologic tests do not target specific hemorrhagic diathesis encountered after CPB. Since there are practical problems related to the rapid laboratory evaluation of fibrinolysis and since EACA in therapeutic doses and in the absence of signs of DIC does not produce any detectable hypercoagulability, it seems logical to use it prophylactically.

Since our study tested only the activity of EACA against placebo, we do not know if there is any synergistic effect of DDAVP or any other compound which has been shown to conserve blood loss. Also, we clearly specified that the differences in major complications are not necessarily attributed to the differences in treatment between the two groups.

We agree with Dr. Spiess and his colleagues that intraoperative monitoring of coagulation would bring valuable information; however, until such practical tests develop and gain clinical acceptance, prophylactic use of pharmacologic compounds that may conserve blood loss after CPB should be considered.

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