PDT owe their success largely to the later introduction and use of bronchoscopy at different stages of the procedure; as much as the technique permits. Those problems with the described method mentioned below, which may be linked to the absence of bronchoscopic control, could all be causally interconnected.

Bronchoscopy application permits the precise positioning of the initial needle puncture between the second and third tracheal rings. If a bronchoscope were used, the level of puncture with a needle could be identified without skin incision by simultaneous observation through the bronchoscope and the application of a slight pressure to the skin over the trachea. Without bronchoscopy, the level of the puncture is uncertain, and, as a result, too high a puncture could lead to complications that have been described accompanying high placement of the cannula. Too low a puncture may impose a risk of bleeding. If a large thyroid gland is present, there would be a danger of accidentally cutting across some blood vessels that are positioned beneath, such as the thyroidal arteries, the left brachiocephalic vein, or even the brachiocephalic trunk itself.

With bronchoscopy, not only is the initial placement of the needle fully controlled, but also all steps of the PDT. The permanent observation of the skin surface and, at the same time, the visualization of the inside of the trachea and control of the progress of the tip of the inserted devices (ie, needle, guidewire, and cannula) help to prevent injury, particularly of the membranous tracheal wall. Freedom of movement of those devices introduced into the tracheal lumen is extremely small, and without the use of a bronchoscope the procedure remains largely “blind” and the risk for injury during all stages is high.

In the article under discussion, the initial puncture level may have been incorrect; this could have been avoided if bronchoscopy had been used. The authors stated that they performed a midline, 2-cm-long, incision just above the suprasternal notch. Certainly, it is not clear how a palpation by the finger at such a low level (ie, at the sternal notch) could permit identification of the second and third tracheal rings? Such an extremely low point of incision would seldom allow the performance of a tracheal puncture, as described in the article, between the second and third tracheal ring. In Figure 3 of that article, one can even see that the needle is introduced between the fourth and fifth tracheal rings. However, at that level the normal anatomy looks slightly different than the way it is depicted in Figure 3 of the article. The trachea does not follow the frontal plane of the neck; diverging dorsally, so that to approach for dissection of the trachea at that low level has to be deeper, compared to the one that would be performed at the level of the second tracheal ring. We can only speculate that the increased distance between the skin plane and the tracheal lumen at that low level could have been the cause of the finding that one tracheal cannula was too short in the case of a patient undergoing a Griggs tracheostomy that was described in the article.

In addition, such a low approach may impose a need for extensive surgical dissection accompanied by the above-mentioned complications. This may have disastrous consequences. Bleeding and local infection may by themselves create conditions, in the latter case, for the occurrence of tracheal stenosis. A generous surgical approach, as required in the absence of any bronchoscopic control, may, in addition, be responsible for a loose or badly secured tracheal cannula. This might be one of the reasons for the displacement of the cannula in the first 48 h that was observed in the three patients in the study. When such a deep surgical dissection of the trachea is performed, one should probably not use a commercial and expensive tracheostomy set, but a “percutaneous” tracheostomy, and should instead complete the operation using a routine surgical procedure.

It may be of lesser importance, but it should be mentioned that the enthusiastic conclusion of the authors that the modified percutaneous tracheostomy without bronchoscopic guidance is the simplest and safest method, may be applied to the Ciaglia “Blue Rhino” method only hypothetically since this method was used in only eight patients in the study.

Some drawbacks in the use of bronchoscopy, like carbon dioxide retention, certainly exist, and the use of a bronchoscope would therefore be contraindicated in patients who would not support high CO₂ levels, like those with brain injuries. There are, of course, other circumstances in which the use of modified percutaneous tracheostomy without bronchoscopic guidance would probably be the best choice if the bronchoscope were not at hand. In those circumstances, this technique would be much more suitable than the customary PDT without a bronchoscope. There are however important limits of the method described by Paran et al, and a general dismissal of the usefulness of bronchoscopy would certainly not be warranted.

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Prevention of Venous Thromboembolism
To the Editor:

We thank Dr. Lotke (June 2005) for raising a number of important issues about the development of guideline recommendations for thromboprophylaxis. While Dr. Lotke contends that internists undervalue postoperative bleeding related to anticoagulant prophylaxis, we are not aware of any evidence that supports this statement. In contrast, the American College of Chest Physicians (ACCP) thromboprophylaxis guideline process placed...
a high value on minimization of bleeding complications when making its recommendations.2 The chapter authors included three surgeons, two of whom are practicing orthopedic surgeons. Furthermore, before finalizing the orthopedic surgery sections, we obtained formal reviews from 16 experts who were external to the ACCP thromboprophylaxis committee: 10 of these were orthopedic surgeons. Thus we believe that the values we assigned to bleeding complications and prevention of deep vein thrombosis (DVT) are representative of the orthopedic community and their patients.

As an example of the priority we placed on avoiding bleeding due to thromboprophylaxis, the use of low-molecular-weight heparins (LMWHs) was rated over fondaparinux in hip and knee arthroplasty because of slightly greater bleeding with the latter (despite an overall 50% relative risk reduction for DVT prevention with fondaparinux compared with LMWH).3 We also ranked extended prophylaxis with LMWH over vitamin K antagonists based on greater bleeding risks with the latter agents.4

We do agree that anticoagulant prophylaxis should be used cautiously in patients at higher risk of bleeding than those included in clinical trials. However, there is little evidence that age is an independent predictor of bleeding in such patients, and age was not an exclusion criteria in most of the clinical trials. While thromboprophylaxis should attempt to keep the risk of patient-important bleeding to a minimum, we should not lose sight of the need to prevent DVT in major orthopedic surgery patients and thereby to reduce the complications of acute DVT and pulmonary embolism as well as the long-term sequelae of chronic venous insufficiency and pulmonary hypertension.

Asymptomatic DVT is frequently used as a surrogate for patient-important outcomes in thromboprophylaxis trials.3 Clearly, symptomatic thromboembolic events are less common than asymptomatic DVT if a highly sensitive diagnostic test such as contrast venography is used to routinely screen patients. We agree with Dr. Lotke that the ratio of asymptomatic DVT to symptomatic venous thromboembolism (VTE) varies somewhat from one patient group to another, although this relationship has been shown to range from 5:1 to 10:1 across a spectrum of patient groups.2

Dr. Lotke is, however, incorrect in his view that reduction in asymptomatic DVT is not correlated with reduction in pulmonary embolism (PE). This issue is discussed in detail in sections 1.2 and 1.4.2 of our chapter. Although most thromboprophylaxis trials are underpowered to show a statistically significant reduction in symptomatic VTE, large trials and metaanalyses demonstrate similar risk reductions for this outcome compared with those observed for asymptomatic DVT.

For example, nine randomized trials, including a total of 4,000 patients, compared in-hospital thromboprophylaxis with prophylaxis extended to approximately 1 month after discharge following hip or knee arthroplasty.6 The relative risk reductions with extended prophylaxis were 52% for venographic DVT and 62% for symptomatic VTE. In the Pentasaccharide in Hip-Fracture Surgery Plus trial,7 extended thromboprophylaxis in hip fracture patients was associated with a 96% reduction in venographic DVT and an 89% reduction in symptomatic VTE, both statistically significant benefits of prophylaxis. Although we support the design and conduct of large randomized trials of thromboprophylaxis using patient-important outcomes, the prevention of asymptomatic DVT is highly correlated with prevention of symptomatic PE. The attempt by Dr. Lotke to relate the incidence of asymptomatic DVT in high-risk patients receiving no prophylaxis to fatal PE rates in patients receiving prophylaxis is misleading and does not assist the discussion of these issues.

The ACCP Antithrombotic Therapy Guidelines Group continue to work with the orthopedic community to develop guidelines that are both evidence-based and useful for practicing orthopedic surgeons. The principal challenge for all of us is to develop effective strategies to implement such guidelines universally and, thereby, to provide the most effective, safe, and cost-effective protection for our patients.

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