Long-term anticoagulation after acute thromboembolic limb ischemia

In their paper,[1] Forbes and associates evaluate the benefit of long-term anticoagulation after thromboembolectomy in patients without either atrial fibrillation or a cardiac thrombus. In this observational study, with retrospective and prospective components, 3 aspects of study design and data analysis deserve to be addressed.

First, not all patients in the study used anticoagulation on a long-term basis. At the time of follow-up, only 79% of patients with atrial fibrillation or a cardiac thrombus (group 1) and 39% of patients without these conditions (group 2) were still taking anticoagulants. Therefore, the groups included a mix of long-term and short-term users of warfarin, and this mix was different in the 2 groups. Such a mix could have introduced a statistical bias in the comparisons by causing a dilution of the effect being studied.

Second, in observational studies like this one, it is imperative that the 2 groups be comparable, except for the risk factor under study. In this study, group 2 includes 10 patients (out of 31) with malignant disease, whereas group 1 includes none. Cancer patients are at higher risk of death and thrombotic disorders.[2] They should therefore be excluded to make group 2 more comparable to group 1.

Finally, the outcome of amputation described in Table 3 occurred in 4 patients who "underwent lower extremity amputation during the initial hospitalization for acute ischemia." Therefore, this outcome did not occur during follow-up but rather before the exposure being studied (warfarin treatment at the time of discharge). The epidemiologic principle of directionality requires that the outcome be observed after the exposure, so these subjects should not have been included in the analysis.

To reduce these sources of bias, it would be helpful if the data could be shown for the 2 groups after removing from group 2 the 10 patients with malignant disease and the 4 patients who underwent an amputation during the initial hospitalization. Showing the results only for the long-term users of anticoagulation treatment would be also useful.

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References

1. Forbes TL, DeRose G, Harris KA. Is long-term anticoagulation after acute thromboembolic limb ischemia always necessary? *Can J Surg* 2002;45:337-40.
2. Sallah S, Ahmad O, Kaiser HE. Pathogenesis of thrombotic disorders in patients with cancer. *In Vivo* 2000;14:251-3.

(Dr. Forbes replies)

We welcome Mr. Suissa's comments on our paper regarding the role for long-term anticoagulation after acute thromboembolic limb ischemia.[1] He makes good points, outlining some of the weaknesses of all retrospective studies, which we recognize in our paper. Although in principle I agree with his general epidemiologic arguments, I do not agree that patients with malignant disease or those who underwent amputation should be removed from our analysis.

The purpose of our study was to analyze the natural history of patients who suffered from acute thromboembolic limb ischemia in the presence or absence of certain risk factors and long-term anticoagulation. Although patients in group 2 did contain a significant number of patients with malignant disease, these patients should be included in the outcome analysis because this is a recognized predisposing factor for recurrent venous and arterial thrombosis. In regard to the inclusion of the 4 patients who underwent early extremity amputation, it is important that these patients be included as they are at risk for recurrent arterial events, which was one of our main outcome variables.

Suissa's comments are appreciated and serve to reinforce the limitations of all retrospective studies. However, these studies can propose trends that can be subsequently explored with prospective, randomized studies.

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Reference

1. Forbes TL, DeRose G, Harris KA. Is long-term anticoagulation after acute thromboembolic limb ischemia always necessary? *Can J Surg* 2002;45:337-40.

Subcapital hip fractures: the Garden classification

It takes only brief perusal of the article by Beimers and associates[1] to realize that, the authors are really suggesting a collapse of the Garden classification. As with anybody who is suggesting something they consider to be new, they misrepresent the old and underestimate the difficulties of the new. Although the illustrations in the original Garden article are anteroposterior views of the hip, displacement in all directions is discussed.

The suggestion that stable versus unstable subcapital fractures is an easier classification is deceptive. In order to function worldwide, one would then have to subclassify each of these categories into what one considers to be stable and what is not stable. This, I suggest, is much more complicated than the relatively simple Garden classification.

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Reference

1. Beimers L, Kreder HJ, Berry GK, Stephen DJ, Schemitsch EG, McKee MD, et al. Subcapital hip fractures: the Garden classification should be replaced, not collapsed. *Can J Surg* 2002;45:411-4.