

same for series with similar rates of critical ischaemia. Is this necessarily the case? Let us study the few surveillance series which quote amputation rates. In the publication of Sayer *et al.*,³ in which all patients are stated to have critical ischaemia, 25 of the 33 (75%) occluded grafts resulted in amputations. Whereas in the article of Berkowitz and Greenstein,⁴ in which all patients are stated to have severely ischaemic legs, only 11 of the 25 (44%) occluded grafts resulted in amputation. Clearly there appears to be variation in the outcome of graft occlusion in the presence of critical ischaemia. Thus any analysis based on the hypothesis that the ratio of occlusion to amputation is equal must be invalid.

As stated in the discussion of our publication, we accept that the summation analysis has a number of difficulties. The results do not demonstrate that duplex surveillance has no role following infrainguinal bypass, but they do indicate that a large randomised trial is warranted to establish that the considerable cost and workload required for surveillance is worthwhile. Surely this is the only scientific way of establishing the role of duplex surveillance rather than any hypothesis that the ratio of amputation to occlusion should be equal for disparate groups or the demonstration that secondary patency is significantly better than primary patency.⁵

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Iloprost

Sir,

Having been involved with the early trials of iloprost as an adjunct to distal bypass surgery, we were naturally disappointed that the multi-centre trial reported in this journal failed to show any benefit of the agent in this role.¹ We believe, however, that the results of the

study should be interpreted with great caution for a number of reasons.

Initial experience with iloprost was carried out using a single bolus dose of the agent via a vein graft to a single calf vessel on completion of the procedure. Addition of intravenous iloprost before and after surgery is without proven benefit and may, as the authors discuss, even be disadvantageous. It is further disappointing that 45 patients failed to receive iloprost as planned.

Our major concern relates to the study design. The initial power calculations for the study were based on the results of patients undergoing arterial bypass to a single calf vessel using long saphenous vein. In the reported study only 73.9% of patients received such a procedure, and the inclusion of patients with composite and prosthetic grafts invalidates the initial calculations. Subsequent subgroup analysis of the different grafts is not valid due to the small numbers and is possibly one of the factors contributing to the surprising lack of benefit found for vein compared to prosthetic grafts.

Patient selection and surgical technique are of fundamental importance in distal bypass surgery, and the variation in number of procedures performed by each centre (one unit contributing only one) suggests considerable differences existed. This point is reinforced by the variable use of dextran, antiplatelet agents, heparin and anaesthetic technique. All of these factors introduce further variables which reduce the power of the study.

It is likely that publication of this study will kill further interest in the role of iloprost as an adjunct to distal bypass surgery. We think that this may be a pity and suggest that what the study most strongly illustrates is the need for robust study design before interpreting the results from multi-centre trials.

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Transcranial Doppler

Sir,

We read with interest the article of Giannoni *et al.*¹ regarding the changes seen on transcranial doppler compared with clinical condition when performing a