Correspondence

We welcome letters to the Editor concerning articles which have recently been published. Such letters will be subject to the usual stages of selection and editing; where appropriate the authors of the original article will be offered the opportunity to reply.

Letters should normally be under 300 words in length, double-spaced throughout, signed by all authors and fully referenced. The edited version will be returned for approval before publication.

Anatomy of the medial femoral circumflex artery and its surgical implications

Sir,


The authors have attempted to focus attention on the importance of knowledge of the blood supply of the upper end of the femur in surgical practice.

The paper is based on anatomical dissections of 12 fresh cadavers and it is supported by 52 references made in en bloc form. Many of the statements which the authors make, however, are not supported by the work of other researchers, for example, “The deep branch of the MFCA gives rise to two to four superior retinacular vessels and occasionally, to inferior retinacular vessels.” The work of Trueta and Harrison2 was shown to be flawed many years ago by Lagrange and Dunoyer3 whose work is not quoted in the references, and by Crock4 and Chung5.

The fact that multiple retinacular vessels have not been demonstrated by the present authors may indicate that they were not fully injected during the preparation of the specimens, rather than that they are absent.

Another statement made is: “Despite previous descriptions,9,12,14,17,18,20-22,24,49 we found no anastomotic branch surrounding the neck of the femur . . . .” The ten cited references are merely listed, and several are outstanding anatomical works which disprove this claim.

Unfortunately, this paper simply perpetuates fallacies about the blood supply of the upper end of the femur since it is based on the use of inadequate anatomical techniques. Also, the five authors thank six co-workers for technical assistance and friendship, raising serious questions about multiauthorship. For example, who did the dissections, and who accepts the academic responsibility for conclusions drawn from the work?

H. V. CROCK, AO, MS, MD, FRCS, FRACS, FRCS Ed (Hon)
Spinal Disorders Unit
Cromwell Hospital, London, UK.


Author’s reply:

Sir,

The main aim of our paper was to advance knowledge of surgical anatomy in its application to safe and reproducible surgery of the hip.

In his letter, Mr Crock criticises a perceived deficiency in the accuracy of our study because of inadequate vascular perfusion using rubber latex. He has based this on the (wrong) assumption that we were dealing with the entire vessel distribution supplying the proximal end of the femur. It was our intention, however, to describe the applied surgical anatomy of the ramus profundus of the medial femoral circumflex artery along the external rotators.

This is of practical importance and can be seen by the surgeon in his daily work. The study was initiated because most atlases of anatomy and textbooks of approaches in use today give only a minimum amount of information on the anatomy of the medial femoral circumflex artery which is useful surgically. This is especially true of the course of the vessel along the external rotators.

Perfusion with latex rubber has a long and successful tradition in the anatomy laboratory in Paris where we have performed several studies, including the present one.1-3 It can fill vessels with a diameter of surgical interest of 0.5 mm and greater. The same technique was used for data in the books of Tubiana, McCullough and Masquelet1 and Masquelet, McCullough and Tubiana2 on surgical approaches to the extremities. In our present study, we used only specimens in which excellent perfusion could be demonstrated up to the entrance of the vessels into the femoral head. We therefore believe that we have largely excluded any significant vascular or technical variations. Moreover, if arteries were present on surfaces and failed to fill, they would certainly not be as patent or extensive as the adjacent vessels, which filled very well.

In his further critique, Mr Crock expounds on the vascular anatomy of the proximal femur in childhood with his reference to Lagrange and Dunoyer and Chung. Our study examined the vascular anatomy in the adult exclusively. Although this is not mentioned specifically, it is alluded to within the text. There is little doubt that the vascular distribution in early childhood and in the adult have some differences. One example is the regression of the lateral femoral circumflex artery with age, a phenomenon which was mentioned by Ogden1 and which explains why there is no substantial peripheral anastomosis between the medial and lateral femoral circumflex artery in adulthood. This is also stated by Gillot, Sakka and Frota2 in their detailed anatomical study.

The findings of our study are constantly confirmed in our daily surgical practice, including routine dislocation of the hip at operation. They have been reinforced recently by dynamic perfusion profiles of the femoral head during surgical dislocation, using intraosseous laser-Doppler flowmetry. We thus feel that Mr Crock’s reproach that we simply perpetuate fallacies about the
blood supply of the upper end of the femur, is not justified. This paper has information important for hip surgery which is not available from other sources.

E. GAUTIER, MD
Hôpital Cantonal
Fribourg, Switzerland.


Nonunion of the femoral diaphysis

Sir,

I read with interest the article in the July 2000 issue by Giannoudis et al entitled ‘Nonunion of the femoral diaphysis.’ The main finding of their retrospective review of 377 patients with fractures of the femoral diaphysis was the association between delayed nonunion and the use of non-steroidal anti-inflammatory drugs (NSAIDs). This was interpreted as being causative because the authors state that they now exclude the use of NSAIDs in these patients.

Some animal studies support this hypothesis, while others do not. There are few human studies on the effect of NSAIDs on the healing of fractures. Butcher and Marsh’s abstract quoted in this paper as supporting evidence, was also a retrospective study. Davis and Ackroyd found no effect of flurbiprofen on bone healing compared with placebo in a double-blind prospective trial of Colles’ fractures.

An alternative interpretation of the authors’ findings is that patients with delayed nonunion have pain, and this results in their use for our patients with fractures.

If we interpret the odds ratio of Giannoudis et al for the use of NSAIDs conservatively, and assume the risk of nonunion with NSAIDs to be fivefold, to have an 80% chance of demonstrating a fivefold increase in the rate of nonunion at a significance level of p = 0.05, would require a trial of 150 patients.

R. K. PRATT, FRCS
D. J. KRAMER, FCS(SA) Orth
North Tyneside General Hospital
Newcastle upon Tyne, UK.


Author’s reply:

Sir,

We agree that the most powerful way to assess the effect of NSAIDs is by a proper randomised clinical trial and look forward to this being performed. The experience in trauma surgery, however, and in a wide variety of clinical situations, is that bone healing is restricted by NSAIDs. Bone healing is multifactorial and this trial was set up to look at the effect of reamed against unreamed nails because of our previous report describing delay in healing and, more recently, the increased rate of nonunion with unreamed nails noted by the Canadian group. The effect of the NSAIDs was so marked, however, that it effectively swamped the other variables in our groups. We have had our data reassessed by several statisticians and do believe that this is a real effect. The clinical impression after removal of NSAIDs from our practice has been the disappearance of delayed and occasionally, poor healing in fractures of the femoral diaphysis.

R. M. SMITH, MD, FRCS
Trauma Service, St James’s University Hospital
Leeds, UK.


Sir,

We read with interest the article by Giannoudis et al in the July 2000 issue entitled ‘Nonunion of the femoral diaphysis.’
We have some concerns regarding the outcome measures used to define union. The clinical and radiological methods used to define healing are inaccurate.\textsuperscript{2,3} The radiographs were not standardised; this adds further to the inaccuracies of a method relying on the quantification of callus to define union. The need for surgical intervention is dependent on this definition as is the separation into the two study groups. Nonunion was found to occur with a mean time to diagnosis of 11.5 months (7 to 18) in the study group, and 6 months (3 to 10) in the control group. This overlap questions the methods of defining union.

We note the effects of varying factors on union, and question the conclusions drawn since inaccurate outcome measures were used. The deleterious effects of non-steroidal anti-inflammatory drugs (NSAIDs) on healing in the nonunion group are discussed, although the timing of the administration of NSAIDs is not recorded. Patients with nonunion may continue to take analgesics as their fractures are still painful, and they bear weight later at 7.8 weeks compared with 4 weeks. Although this is stated in the discussion, the administration of NSAIDs for their analgesic effect seems to have been ignored. It is therefore difficult to separate the ‘cause and effect’ of the administration of NSAIDs and nonunion.

Studies using inaccurate outcome measures should be interpreted with care. The study of the healing of fractures is no exception.

R. H. WADE, FRCS Ed
P. J. ROBERTS, FRCS
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Oswestry
Shropshire, UK.


Author’s reply:

Sirs,

I would like to thank Messrs Wade, Roberts and Richardson for their comments. I am sure that we would all agree that there are some limitations in retrospective reviews, and our study is no different from others. The paper, however, was reviewed by at least two expert statisticians who did feel that it was sound, although the limitations described are indeed there. I also note the difficulty in defining union. This is present in all papers which discuss the union of diaphyseal fractures in the presence of an implant, because all of the measurements of stiffness are useless. It is thus difficult to separate the ‘cause and effect’ of the administration of NSAIDs and nonunion.

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these data. The differences which we observed are not significant and an effect is not going to be proved whatever tests the observations are subject to. We simply wished to illustrate that the data available are poor, yet we are using these data in our day-to-day decisions on whether a patient needs surgery or not.

The correspondents agree that patients should not be subjected to unjustified operations and attendant complications. We would also agree that many patients benefit from surgery to stabilise the spine.

The suggestion that a prospective study could be helpful is never a bad one, although there ought to be considerable retrospective evidence in favour of the proposition before the prospective study addresses how beneficial the treatment is, and for what type of patient, in what circumstances. The fact that there is no evidence that patients benefit neurologically from canal clearance, even in the selected cases, is for the very simple reason that the damage is done at the moment of injury.

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D. LIMB, BSc, FRCS Ed Orth
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Thromboprophylaxis – which treatment for which patients?

Sir,

I read with much interest the Editorial by Prentice1 in the May 2000 issue entitled ‘Thromboprophylaxis – which treatment for which patients?’, and various articles in the journal on the use of thromboprophylaxis after total hip and knee replacements.

Although there is controversy in the literature about the risks and benefits of thromboprophylaxis after TKR and THR, most orthopaedic surgeons use some kind of thromboprophylaxis.2 What was not stressed was the duration of its use. There is evidence that thrombosis and its complications can develop up to three months3 after hip or knee replacement, but the current practice is to use thromboprophylaxis for 7 to 10 days, the duration of the hospital stay. Surely those who believe in the efficacy of thromboprophylaxis should use it in some form for three months.

S. KAPOOR
Glasgow Royal Infirmary
Glasgow, UK.


Author’s reply:

Sir,

Dr Kapoor raises the important point of whether thromboprophylaxis after orthopaedic surgery should continue after discharge from hospital. I agree with him that symptomatic deep-vein thrombosis and pulmonary embolism, both fatal and non-fatal, may occur after the 7- to 10-day period, at which time thromboprophylaxis has usually been stopped. I do not believe, however, that the current evidence is strong enough to support the routine use of heparin on an outpatient basis after hospital discharge. The published studies have used the surrogate endpoints of routine venography and, in some cases, ventilation perfusion scanning, to assess the effect of subcutaneous heparin after discharge.2-3 Although there is a reduction in these endpoints by heparin, it is still not certain whether they can be extrapolated into clinical benefit for the patient. In the only trial to use symptomatic venous thromboembolism as the outcome to assess low-molecular-weight heparin after hip or knee replacement, there was no convincing benefit for extended thromboprophylaxis.3 There is an urgent need to carry out large randomised controlled trials in which short and prolonged prophylaxis are compared using clinical endpoints. The only large study to evaluate this in a controlled manner is the Pulmonary Embolism Prevention (PEP) trial in which aspirin, 160 mg daily, started preoperatively and continued for 35 days postoperatively, was compared with placebo.2 This trial showed that mortality in the aspirin group continued to be reduced, compared with the placebo group, in the interval between discharge from hospital and 35 days, and that the protective effect of aspirin continued from week 2 until week 5 postoperatively. Whether extending thromboprophylaxis after five weeks can produce clinical benefits, however, is still not known.

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