**Letters to the Editor**

**Osteotomy and Osteoarthritis**

To the Editor:

In their article in the April 1998 issue entitled “Distal Femoral Osteotomy for Lateral Compartment Osteoarthritis of the Knee” (Orthopedics 21[4]:437-440), Mathews et al reported a spectrum of results for osteoarthritic knees, all with valgus deformity and lateral compartment osteoarthritis, treated with medial wedge femoral varus osteotomy.

Between one and two thirds of the patients had satisfactory results using Hospital for Special Surgery scoring systems. However, they reported a high level of complications (ie, more than half of the cases), and 5 of the 21 patients required total knee replacement.

While the complications of nonunion, infection, and fixation failure may all be related to technical difficulties, the ongoing degeneration and poor outcome in the remaining cases raises questions as to the logic of the chosen osteotomy. Indeed, this issue forms a part of the discussion with respect to the appropriate selection of correction angle and desirable postoperative realignment.

Valgus deformity is associated with lateral compartment osteoarthritis in most cases. Most cases also have an element of bone deformity. Supracondylar osteotomy presumes the bone change is maximal for the femur, but this assumption may not be logical.

Our investigations of the alignment of osteoarthritic knees in a Canadian population revealed that in those with a valgus deformity, most of the bone contribution to valgus came from the tibia. In the analysis applied to varus deformities, we found greater contribution of femoral varus to the deformity than tibia varus.

The definition of femorotibial alignment alone is insufficient to appropriately decide on the nature of osteotomy and the bone to which it may best be applied. An analysis of which bone contributes most to the deformity is needed. In a few instances, correction is best provided at both bones to realign the limb adequately in the presence of unicompartment osteoarthritis.

These comments for realignment choices are not appropriate in cases of advanced arthritis associated with subluxation; such cases may not be amenable to osteotomy.

Careful attention to the overall alignment and definition of bone contributions to the deformity as well as other selective factors such as the extent of local changes, range of motion, and subluxation will improve outcome. The authors have already drawn attention to the importance of using adequate surgical fixation.

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**REFERENCE**


Reply:

In our study, the mechanical and anatomical axes were the two measurements of lower limb alignment used to quantify the degree of preoperative valgus deformity and the amount of postoperative correction that was achieved. Distal femoral osteotomy was chosen because of the widely held belief that the deformity in the valgus arthritic knee more on the femoral than the tibial side. Our article did not seek to compare femoral with tibial osteotomy for valgus osteoarthritis, but to study the outcome of a group of patients treated by femoral osteotomy.

Dr Cooke has carefully studied the lower limb alignments with normal and arthritic knees and has reported that in valgus knees, the deformity is predominantly on the tibial side. We agree that one should attempt to define the site of the deformity by studying the condylar-hip and plateau-ankle angles on standardized standing full-length radiographs. The type of osteotomy and postoperative care regimen used may then greatly influence the overall results of treatment of the valgus osteoarthritic knee.

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OVERUSE TENDINITIS

To the Editor:

The authors of the article entitled “Overuse Tendinitis of the Intrinsic Muscles,” which appeared in the August 1998 issue (Orthopedics (21[8]:891-893), are to be congratulated. They mention “overuse” of the lumbricals and the interossei as the causative factor.

I question the relationship of many hand and wrist complaints and “overuse.” The cause-and-effect connection between the hand and the computer and the development of specific lesions has been assumed by many, but not well-documented biomechanically or anatomically except by coincidence.

Silver and Rozmaryn have recognized a relatively rare syndrome that is usually unrecognized by the first physician who examines a patient with a hand complaint. In my experience, more than 90% of hand pain without a specific known injury is called “carpal tunnel” by the individual not familiar with the anatomy or complex hand pathology.

Watson et al documented anatomic variations or anomalous insertions of the lumbrical muscle in 12 (9%) of 134 intermetacarpal space explorations. Eyler found numerous variations primarily related to the insertion of the interosseous tendons into bone or the extensor hood, with varying ratios of insertion of the tendon into soft tissue or bone.

What is the natural occurrence of this condition in the general population, with or without computers, and what is the natural course of the pathologic process? Also, the authors speculate that causation is related to action of the metacarpophalangeal joints and abdication and that is, intuitively, logical. The long finger has two interossei inserted into the base of the proximal phalanx and at least one lumbral, and the little finger has no interosseous, but does have the abductor mass of muscles.

The authors describe a relative position of intrinsic plus (ie, metacarpophalangeal joints flexed and interphalangeal joints extended) by the operator of the computer. My experience has been that the wrist is usually flexed a few degrees, that the metacarpophalangeal joints are flexed 5° to 10°, and the interphalangeal joints also are flexed. Unusual force is not required to compress the keys. I would not describe this as an abnormal intrinsic plus position, but more as a combined intrinsic minus at the interphalangeal joints and minimal intrinsic plus at the metacarpophalangeal joints. Furthermore, in compressing the keys, extension of the interphalangeal joints by the intrinsic is not required whereas the flexor digitorum profundus and flexor digitorum superficialis are the muscles used most frequently.

The authors reported on 6 (4%) patients of 150 who had the diagnosis of tenosynovitis of the hand. Their case report of the 44-year-old computer software designer shows that his intrinsic tendinitis problem began after a weekend spent working with a caulking gun. My experience has been that the use of the computer was coincidental and that the use of the caulking gun might well have been the inciting factor. After all, he worked with the computer 6 to 8 hours a day for a long time without complaints whereas the caulking gun was an exceptional activity. The fact that he improved with “stretching the intrinsics and contrast baths” indicates that the condition was self-limiting. The fact that he obtained wrist rests at his work station requires reassessment as to the relationship between treatment and “cure.”

Computers are the blessing of the 1990s, but they get too much credit for causing coincidental lesions that are related to other factors in life.

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REFERENCES

Reply:

The points raised by Dr Goldner are central to the worldwide controversy regarding the very existence and nature of “repetitive motion disorders.” One thing is certain—the term “overuse” is itself grossly overused.

To have any scientific meaning, one would have to define a threshold envi-