To the Editors:

I am writing in regard to the article, “Treatment of leg length discrepancy with temporary epiphyseal stapling in children with juvenile idiopathic arthritis during 1957–99,” authored by Eerik Skyttä, MD, and coauthors.1 In their introduction they state that “due to allegedly high risk of premature epiphyseal closure, the technique has not gained universal acceptance.” They do not support this statement by a reference to the literature. In my 30 years of experience in children’s orthopaedics and with many epiphyseal staplings, I have never seen a premature closure. I would therefore like to know on which basis the authors made their statement.

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REFERENCES

To the Editors:

We thank Prof. Guy Fabry for his interest in our article “Treatment of leg length discrepancy with temporary epiphyseal stapling in children with juvenile idiopathic arthritis during 1957–99.” There indeed are no premature epiphyseal closures reported in conjunction with temporary epiphyseal stapling. In our institute, we have encountered one such case in the management of knee valgus deformity, eventually leading to knee arthroplasty. We are well aware of the use and acceptance of the method in Nordic countries and northern Europe. The method has not, however, gained universal acceptance in the Anglo-Saxon countries. A major reason for this, in the senior author’s (E.B.) personal communications, has been the risk of premature epiphyseal closure.

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To the Editors:

Levine et al’s article addressing the test characteristics of C-reactive protein (CRP) for pediatric septic arthritis is an important contribution to the growing body of literature regarding the proper interpretation of diagnostic tests.2 The article nicely demonstrates how presenting the results in the form of likelihood ratios (LR) for different cutpoints is preferred over that of traditional dichotomous sensitivity and specificity. Interval LRs allow the clinician to use more of the information provided by a continuous variable such as CRP.1

Unfortunately, the calculations represented in Table 2 are misleading.3 As reported in Table 2, a LR of 0.36 for a CRP < 10.5 would imply a decrease in the probability of septic arthritis rather than an increase. The LR is defined as the probability of obtaining a given test result among those with disease divided by the probability of obtaining the same test result in those without disease. The appropriate calculations for Table 2 are the following:

CRP ≥ 10.5: 2.76 (95% CI, 1.49–5.08)
CRP ≥ 5–10.4: 1.42 (95% CI, 0.71–2.82)
CRP ≥ 1–4.9: 0.60 (95% CI, 0.32–1.13)
CRP < 0.9: 0.36 (95% CI, 0.13–0.95)

This is important when interpreting CRP results in an individual patient. None of these LR values would have a large effect on the posttest probability of disease since they are neither large (>10) nor small (<0.1).2 For example, if the pretest probability is estimated to be 50%, then a CRP ≥ 10.5 would increase the probability of disease to 73%. On the other hand, if the clinical suspicion is low (20%), and the CRP is <0.9, then the posttest probability would be 8%. It appears that in the evaluation of the limping child, the CRP alone is not adequate to rule in, or rule out, septic arthritis.

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REFERENCES
To the Editors:

In their article, “Acute synovitis of the knee resulting from intra-articular knee penetration as a complication of flexible intramedullary nailing of pediatric femur fractures: report of two cases,” Rohde et al.1 presented a previously unrecognized complication of what is rapidly becoming a common procedure. It is reasonable to expect that as the procedure continues to be more widely accepted, more complications will be identified.

There are two standard techniques for dealing with the distal end of the flexible intramedullary nail: leave it flush to the bone, as was done in this study, or bend it over at a right angle to the entry site and cut it deep to the deep fascia. The latter, which I prefer, is less commonly performed because of the concern over hardware bursitis. Although the children do form a reactive bursa, laterally more so than medially, rarely does it cause enough pain to limit activity. It always resolves with hardware removal. Perhaps by using this technique of bending the rods at the insertion site, this new complication can be avoided.

I would be interested in the number of femoral nailings performed over this time frame. The incidence of this complication in this study may be useful.

Lastly, the article should more rightly be titled, “Acute Synovitis of the Knee Resulting From Intracapsular Knee Penetration as a Complication of Flexible Intramedullary Nailing of Pediatric Femur Fractures: Report of Two Cases.” Clearly the nail did not penetrate the articular surface of the distal femur. It was visualized in the lateral gutter of the knee. True articular penetration could create a far more devastating complication than synovitis.

I would like to thank the authors for bringing this previously unreported complication to the literature.

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REFERENCE

To the Editors:

We appreciate Dr. Weisman’s interest in our article as well as his comments. At our institution, the intramedullary flexible nail fixation has become a standard procedure for children aged 7 to 14 years with femoral shaft fractures. Initially, we bent the ends of the nails. Indeed, in one of the cases presented in our paper, the tips of the nails had been bent more than 90°. The other fracture was stabilized with the nails flush. Currently, we leave the ends flush because we think a less bulky construct might create less irritation. Whether any of these techniques prevent intra-articular penetration by the nail is unknown, since the number of cases is too small to draw any conclusion. The incidence of the knee joint penetration is definitely lower than that of bursitis and well under 1%. Regarding the title of our paper, we used the word “intra-articular” because in both cases the nails indeed penetrated the articular cavity. They did not damage the articular cartilage.

We thank Dr. Weisman for his thoughtful comments.

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REFERENCES

To the Editors:

The authors thank Dr. Steven Frick for his comments on the article, “Valgus deformity after fibular resection in children,” published in the Journal. Dr. Frick comments that there are other etiologies that can result in a valgus deformity of the ankle like a “tibiofibular synostosis in children,” resulting in an alter-
ation of the normal growth pattern of distal migration of the fibula relative to the tibia due to the different growth rates of the proximal and distal physis of the tibia and fibula, as described by Kärlholm et al. Their excellent article explains it perfectly.

In fact, valgus deformity of the ankle can result after surgery of the fibula, but not exclusively. There are several other situations in which, without the need for surgery, a valgus deformity is a frequent clinical manifestation, as in congenital pseudoarthrosis of the fibula, fibular hypoplasia, or multiple exostoses where an osteochondroma near the ankle acts like a synostosis.

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REFERENCES

To the Editors:

We would like to commend Hui and Torode1 on their important work concerning the potential for remodeling of the glenoid in children with brachial plexus palsy and shoulder subluxation/dislocation. Several authors have documented and even classified the array of dysplasias that result in the glenoid from the gradual subluxation/dislocation.2,4,5 We believe that the initial improvement of 30% of the glenoid version is the result of reduction into the anterior “chamber” of a biconcave glenoid, and not a rapid mutation over the first year.

Furthermore, we believe that a careful assessment of the amount of or lack of external rotation power in the shoulder often necessitates the transfer of the latissimus or the latissimus/teres major, depending on the degree of imbalance. Without concomitant transfers, many of these patients might re-sublux or dislocate. They make no mention of the role of muscle transfer to address the imbalance.

If we are to extrapolate our knowledge of hip dislocation to this problem with the shoulder, the authors make a step in the right direction in determining if we should be considering the plausibility of glenoid osteotomy.

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REFERENCES

Editor’s note: Ian Torode, FRCS(C) did not submit a response to this letter.