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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REFERENCE

1. Skyttä E, Savolainen A, Kautiainen H, et al. Treatment of leg length discrepancy with temporary epiphyseal stapling in children with juvenile idiopathic arthritis during 1957–99. *J Pediatr Orthop.* 2003;23:378–380.

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.[3] 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 $\geq$ 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 $\geq$ 10.5: 2.76 (95% CI, 1.49–5.08)

CRP $\geq$ 5–10.4: 1.42 (95% CI, 0.71–2.82)

CRP $\geq$ 1–4.9: 0.60 (95% CI, 0.322–1.13)

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 $\geq$ 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

1. Brown M, Reeves M. Interval likelihood ratios: Another advantage for the evidence-based diagnostician. *Ann Emerg Med*. 2003;42:292–297.
2. Jaeschke R, Guyatt GH, Sackett DL. Users' guides to the medical literature. III. How to use an article about a diagnostic test. B. What are the results and will they help me in caring for my patients? The Evidence-Based Medicine Working Group. *JAMA*. 1994;271:703–707.
3. Levine MJ, McGuire KJ, McGowan KL, et al. Assessment of the test characteristics of C-reactive protein for septic arthritis in children. *J Pediatr Orthop*. 2003;23:373–377.

To the Editors:

We thank Dr. Brown for his letter concerning an error in our paper addressing the test characteristics of C-reactive protein (CRP) for pediatric septic arthritis.[1] He is correct that column 4 in Table 2 should have the stratum-specific likelihood ratios inverted, as reported in his letter. As he notes, in this situation there is very little effect on the post-test probability. Furthermore, we certainly agree with his assessment that the "CRP alone is not adequate to rule in, or rule out, septic arthritis." Based on our paper we do believe that the CRP is a better negative predictor of disease than a positive predictor, and we believe CRP is a useful test in the evaluation of suspected septic arthritis. Indeed, if a patient has a CRP of <1.0 mg/dL, the probability that the patient does not have septic arthritis is 87%. Once again, we thank Dr. Brown for his kind letter.

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REFERENCE

1. Levine MJ, McGuire KJ, McGowan KL, et al. Assessment of the test characteristics of C-reactive protein for septic arthritis in children. *J Pediatr Orthop*. 2003;23:373–377.

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

1. Rohde RS, Mendelson SA, Grudziak JS. 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. *J Pediatr Orthop.* 2003;23:635–638.

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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To the Editors:

I read with interest the article, "Valgus deformity after fibular resection in children," by Gonzalez-Herranz et al,[2] and agree with their conclusions that fibular resection during growth is not a harmless procedure. Their discussion of the alterations in future leg growth did not mention another possible etiology: that nonunion or resection of a significant portion of the fibula disrupts the normal growth pattern of distal migration of the fibula relative to the tibia. My coauthors and I reviewed a series of cases of iatrogenic synostosis of the tibia and fibula following leg surgery in children and found that cross-union disrupted the normal distal movement of the fibula relative to the tibia, resulting in shortening of the lateral malleolus and ankle valgus, as well as prominence of the fibular head at the knee.[1] We hypothesized that as the proximal fibular physis grows faster than the proximal tibial physis, it provides a proximal to distal thrust that moves the lateral malleolus distally with growth. We believe that this growth mechanism is disrupted in patients with fibular nonunion as well, leading to ankle valgus and shortening of the fibula, but not to proximal migration of the fibular head. As the normal growth pattern of distal sliding of the fibula relative to the tibia is frequently not noted in articles dealing with fibular nonunion or fibular resection in children, we sought again to bring it to the attention of the readers of the *Journal*. I believe it adds further information regarding the possible cause of ankle valgus deformity following fibular resection or nonunion in children.

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REFERENCES

1. Frick SL, Shoemaker S, Mubarak SJ. Altered fibular growth patterns after tibiofibular synostosis in children. *J Bone Joint Surg [Am]*. 2001; 83:247–254.
2. Gonzalez-Herranz PG, del Rio A, Burgos J, et al. Valgus deformity after fibular resection in children. *J Pediatr Orthop*. 2003;23:55–59.

To the Editors:

The authors thank Dr. Steven Frick for his comments on the article, "Valgus deformity after fibular resection in children,"[2] 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"[1] 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.[3] 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

1. Frick SL, Shoemaker S, Mubarak SJ. Altered fibular growth patterns after tibiofibular synostosis in children. *J Bone Joint Surg [Am]*. 2001; 83:247–254.
2. Gonzalez-Herranz P, del Rio A, Burgos J, et al. Valgus deformity after fibular resection in children. *J Pediatr Orthop*. 2003;23:55–59.
3. Kärlholm J, Hansson LI. Selvik G. Longitudinal growth rate of the distal tibia and fibula in children. *Clin Orthop*. 1984;191:121–128.

To the Editors:

We would like to commend Hui and Torode[1] 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.[3] 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

1. Hui JHP, Torode IP. Changing glenoid version after open reduction of shoulders in children with obstetric brachial plexus palsy. *J Pediatr Orthop*. 2003;23:109–113.
2. Pearl ML, Edgerton BW. Glenoid deformity secondary to brachial plexus palsy. *J Bone Joint Surg [Am]*. 1998;80:659–667.
3. Price A, Tidwell M, Grossman AIG. Improving shoulder and elbow function in children with Erb's palsy. *Semin Pediatr Neurol*. 2000;7:44–51.
4. van der Sluijs JA, van Ouwerkerk WJR, de Gast A, et al. Deformities of the shoulder in infants younger than 12 months with an obstetric lesion of the brachial plexus. *J Bone Joint Surg [Br]*. 2001;83:551–555.
5. Waters PM, Smith GR, Jaramillo D. Glenohumeral deformity secondary to brachial plexus birth palsy. *J Bone Joint Surg [Am]*. 1998;80:668–677.

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