Dear Editor:

We would like to congratulate Dr Ferretti et al for their article investigating the effect of double-bundle anterior cruciate ligament (ACL) reconstruction on knee kinematics, “Double-Bundle Anterior Cruciate Ligament Reconstruction: A Computer-Assisted Orthopaedic Surgery Study” (April 2008, pp 760-766). Their study demonstrated that addition of a posterolateral bundle reconstruction to an anteromedial bundle reconstruction caused no reduction in internal rotation laxity of the tibia at 30° of knee flexion. However, we have concerns about the authors’ interpretation of this finding, suggesting that the role of double-bundle reconstruction in minimizing the pivot shift and better restoring knee kinematics should be questioned.

The authors have elegantly measured, using navigation, the restraining effect of ACL reconstruction on passive, uniaxial, internal rotation of the tibia. Several studies have shown that the ACL is a secondary restraint to this motion (with the medial collateral ligament complex being the primary restraint). This is because the ACL, being near to the center of the knee, has only a relatively small moment arm about the relevant axis of rotation, and is hence at a mechanical disadvantage. Lane et al. showed that sectioning the ACL had no effect on uniplanar, axial tibial rotational laxity and this may suggest why Dr Ferretti et al found that further reconstruction of the PL bundle, having already reconstructed the AM bundle, had no measurable effect on this type of laxity.

The pivot-shift test, however, is a dynamic, complex, coupled rotational and translational 3-dimensional motion about a helical axis. Lane et al. showed that although the ACL did not play a significant role in limiting uniplanar axial tibial rotation, sectioning the ACL did produce a positive pivot-shift test.

Several in vitro studies have shown that double-bundle reconstructions are more effective at restraining coupled rotatory loads that simulate the pivot shift. Recently, an in vivo study, again using navigation but with software specifically adapted to assess the coupled tibial rotation occurring during the pivot-shift maneuver, provided objective data as to the importance of the posterolateral bundle in restraining the coupled tibial rotational component of the pivot shift. This may be the cause for the improved control of the pivot shift found at 2 years postoperatively in the recent randomized controlled study comparing single and double-bundle techniques.

Dr Ferretti et al did not measure the effect of posterolateral bundle reconstruction on the coupled tibial rotation occurring during the pivot shift and therefore their inferences as to the lack of improved efficacy of double-bundle reconstruction in controlling this movement should be interpreted with caution.

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Authors’ Response: I would like to thank Dr Robinson for his kind letter, which we appreciated very much. To answer the questions arising from his concerns, we reply to the points raised below.

He affirms that we did not find that the ACL reconstruction has a measurable effect on uniplanar internal tibial rotation. In our series of chronic instabilities, as shown in Figure 6, the fixation of the anteromedial (AM) bundle significantly reduces internal tibial rotation of the knee at 30° of flexion with no further reduction after fixation of the posterolateral (PL) bundle, as compared with preoperative stability. Therefore it is the fixation of the PL bundle in addition to the AM bundle that produces no measurable effect on the internal tibial rotation. On this subject, it could be useful to remember that back in 1976, Furman et al., in a cadaver study conducted on 40 knees, showed that 1) cutting the whole ACL results in a significant increase of both internal and external rotation in all knee positions; 2) cutting the AM bundle results in a significant increase of both internal and external rotation with the knee in extension; and 3) cutting the PL bundle results in an increase of both internal and external rotation with the knee in extension and in an increase of only the external rotation with the knee at 45° of flexion. Therefore, it is not surprising that in our study, a significant effect on tibial rotation is observed after the AM bundle reconstruction.

The pivot-shift phenomenon has 2 main components, 1 translational and 1 rotational. In our study, we tested both components separately. Again, while the reconstruction of the AM bundle reduced both of them, the addition of the PL bundle had no further effect. It is hard to suppose that testing the 2 components is the only way to determine the effect of the double-bundle reconstruction.
simultaneously in a coupled rotational and translational motion could lead to significantly different results.

Concerning the recently published article (Robinson et al, 2007) referenced in the letter, we have several concern about the analysis of the results made by the authors. (1) The results obtained in the 2 groups (11 and 10 cases, respectively) were compared with the preoperative data of the entire series of 21 cases. To consider the 2 groups separately, in terms of preoperative and postoperative values, would have been much more effective. In fact, possible differences in preoperative stability of the 2 groups could affect the analysis of the results. Moreover, the use of the Bonferroni index correction affects the statistical power. (2) The authors did not explain how they assigned the patients to the different kinds of reconstruction. (3) Even if we consider the data reported by the authors, while the reconstruction of the AM bundle leads to a reduction of about 42% of the tibial rotation with a statistically significant difference of $P < .001$, the addition of the PL bundle produces a further reduction of only about 3°, with a statistically significant difference of $P = .005$ and a questionable effect on the clinical outcome.

Last but not least, although some authors report better clinical results in double-bundle reconstruction compared with single bundle, in a more recent study, Streich et al$^2$ did not find any significant difference in rotatory stability, evaluated with the pivot-shift test, between single-bundle and double-bundle ACL reconstruction.

We agree that our results should be interpreted with caution. They are only the preliminary results of a study performed with a valuable technique to evaluate rotatory instability in ACL-deficient and reconstructed knees, in which we evaluated only the uniplanar kinematics of the knee at only 30° of flexion. Possibly, in doubled gracilis and semitendinosus reconstruction, in which we evaluated only the uniplanar kinematics, the PL bundle was shown to be the more important bundle in controlling tibial rotation during the pivot shift maneuver.

We are in complete accord with Dr Ferretti that the use of navigation to clinically evaluate ACL reconstructions offers exciting possibilities to investigate how different reconstruction techniques control knee laxity, but that only through good quality prospective clinical studies with long-term follow-up will the question of double versus single bundle really be answered.

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REFERENCES


Author’s Response: We offer the following response to Dr Ferretti’s concerns raised about our article “Influence of Anterior Cruciate Ligament Bundles on Knee Kinematics: Clinical Assessment Using Computer-assisted Navigation” (December 2007, pp 2006-2013).

To ascertain how each of the ACL bundles (AM and PL) acted to restrain tibial rotation and translation, both alone and in combination, our study necessitated 2 reconstruction protocols. In the first (n = 11 patients), knee laxity analysis was performed preoperatively, after AM bundle reconstruction, and then after addition of the PL bundle. In the second (n = 10), measurements were made preoperatively, after PL bundle reconstruction, and then after addition of the AM bundle. Differences between the preoperative and reconstructed states were compared using Student t-tests for paired data. Comparisons were made only between the results in each protocol and not with the entire series of 21 cases as suggested by Dr Ferretti. Thus, the preoperative and postoperative values in the 2 groups were considered separately. Furthermore, to avoid spurious statistical results from multiple comparisons, a Bonferroni correction was used. This lowers the alpha value required for statistical significance from $P = .05$ to $P = .016$.

The patients were not randomized to either protocol. We simply chose to perform the first 11 reconstructions using protocol 1 and the following 10 reconstructions using protocol 2. There were no differences in the preoperative laxity measurements between the 2 groups and therefore it is highly unlikely that this affected the results. We found that isolated PL bundle reconstruction reduced tibial rotation during the pivot shift from 29.3° to 13.8° (55%, $P < .0001$). Isolated AM bundle reconstruction reduced rotation during the pivot shift from 29.3° to 16.7° (42%, $P < .0001$), and additional PL bundle reconstruction further reduced tibial rotation by 3.7° (12%, $P = .0005$). Thus, the PL bundle was shown to be the more important bundle in controlling tibial rotation during the pivot shift maneuver.

We agree that the preoperative data of the entire series of 21 cases and reconstructions further reduced tibial rotation by 3.7° (12%, $P = .0005$). Thus, the PL bundle was shown to be the more important bundle in controlling tibial rotation during the pivot shift maneuver.

Whether the 12% increase in rotational control that we found by adding the PL is clinically significant and is worth the additional surgical time and risks is probably the crux of the double-bundle debate. The discrepancy between the findings in our study and that of Ferretti et al’s might be explained by difference in tunnel placement and it has recently been shown by Zantop et al$^1$ that different PL bundle tunnel placements alter the efficacy in controlling rotatory laxity.

We take note of the explanation given by Dr Robinson of the statistical analysis performed by the authors in their article.

However, as shown in Table 3 of their paper, the comparison between preoperative and postoperative data actually seems to be performed between the total series of patients (n = 21) and the 2 study groups (n = 10 and n = 11, respectively).

Moreover, reading the last letter of Dr Robinson, we noticed that, surprisingly, both groups had exactly the same preoperative tibial rotation mean values (29.3°).

Concerning the suggested explanation of the discrepancy between the findings in our study and that of Dr Robinson to be attributed to the difference in tunnel placement, we note that two of the authors of our paper spent 3 months in Pittsburgh to learn the precise tunnel placement in double-bundle technique by Dr Fu himself.

Anyhow, we believe that AJSM readers have all the elements to evaluate the criticism and possible bias of each paper.

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REFERENCES


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