In Reply:

We are happy to reinforce the idea that surgery should not be taken lightly—even a trigger release, which is arguably 1 of the simplest and smallest surgeries in any field. On the other hand, our motivation for doing this study was the fact that studies of small series of patients report more minor and major complications than are consistent with our experience. Our concern is that surgeons with a few bad outcomes might decide to publish a small subset of their experience that is neither internally nor externally valid. The data to date are consistent in that most of the adverse outcomes are short-term pain, stiffness, and swelling issues, and major complications such as nerve injury or deep infection are uncommon.

Release of an idiopathic trigger digit is an extremely common procedure, so additional data should be easy to come by. Because most hand surgeons release about 100 trigger digits a year, studies of trigger digit should include a minimum of 500 to 1,000 patients.

Steroid Injections in Combination With Needle Aponeurotomy as a Treatment Method for Dupuytren Disease: Suggestions for Increasing the Research Evidence

To the Editor:

We read with great interest McMillan and Binhammer’s1 article about steroid injections in combination with needle aponeurotomy as a treatment method for Dupuytren disease. As the authors stated, needle aponeurotomy is minimally invasive, but its

REFERENCE

We congratulate the authors for the interesting research they performed, because at first glance this combination of steroid injections with needle aponeurotomy seems to be promising in decreasing the recurrence rate. Nevertheless, the results must be interpreted with care, because in our view the study has some shortcomings.

First, the risk profile of each Dupuytren disease patient was not taken into account when the patients were randomly allocated to the experimental or control group. This might have led to selection bias that could have been prevented by matching.

Second, randomization was done before the treatment. This may have provided an additional source of bias, because the surgeon already knew whether steroid injections were to be used before starting the aponeurotomy. Therefore, we recommend randomization after the aponeurotomy and before the injections.

Third, the same surgeon performed not only the aponeurotomies and injections, but also all measurements. Although the authors recognize the potential bias, we want to emphasize that the lack of blinding during measurements may have been yet another large source of bias, which is especially troublesome because the differences in total active extension deficit (TAED) between groups at 6 weeks, 3 months, and 6 months are so small. An independent researcher could have prevented this.

Fourth, we were confused by the figures of contractions of subjects at baseline presented in the table. When the individual figures for active extension deficit of the proximal interphalangeal and metacarpophalangeal joints are added, the result is not the same as the TAED of these joints that was cited. The outcome is lower in the experimental group but higher in the control group. This deviation needs further clarification.

Finally, we disagree with conducting independent t-tests after repeated-measures analysis of variance. The absence of a significant interaction effect of Group × Time indicates that the combination of aponeurotomy and injections did not provide better results than aponeurotomy alone. A nonsignificant difference between groups at baseline and a significant difference between groups at the end of the study do not equal an improvement over time. We suggest limiting the data analysis of both TAED and percentage correction to a repeated-measures multivariate analysis of variance. We are curious about the outcome of this.

REFERENCES