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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. If a practice with 3 to 4 hand surgeons collects information prospectively, it would take about 2 to 3 years to complete a large study. Looking retrospectively as we did is easier, but it will only reliably catch major complications such as nerve injury and deep infection. It is also important to distinguish surgery for pain from surgery for triggering, as we did.

In our opinion, saying that 40% of patients have an adverse event after release of a trigger digit is misleading, when this is 1 of the safest and most successful procedures in hand surgery. On the other hand, it is high time that we acknowledge and take a more proactive approach to the human protective response to postoperative pain, even expected pain such as transient scar tenderness. Pain intensity and magnitude of disability after minor hand surgery correlate with symptoms of depression, catastrophic thinking and low self-efficacy, and anxiety in response to pain, with symptoms of depression predominating.[1] Our current research focuses on preoperative screening with 2-question measures of symptoms of depression and pain self-efficacy, looking toward preoperative coaching using either a workbook or a 1-on-1 coach, and postoperative telephone or even Web- or app-based coaching to help limit the normal human tendency to feel protective and prepare for the worst in response to pain. This "human safety system" is particularly problematic for intuitive people (concrete thinkers): People who are used to trusting their feelings are less able to distinguish true and false alarms and are therefore less prepared to manage the natural human protective tendencies.

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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's[1] 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

recurrence rate is higher than that of more invasive treatments.[2] 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 contractures 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.

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In Reply:

We thank the authors of the letter for their constructive criticism and thoughtful comments. The primary objective of our study was to compare flexion deformity in Dupuytren disease patients who underwent percutaneous needle aponeurotomy combined with triamcinolone acetonide injections with that of patients who underwent needle aponeurotomy alone at 6 months. Our data provide short-term evidence that a combination of triamcinolone injections and needle aponeurotomy may potentially have a role in lessening recurrence of joint contracture.

We acknowledge the potential for bias in this study, as pointed out by the authors.

To clarify confusion arising from Table 2,[1] mean total active extension deficit (TAED) of "All Joints" refers to the average TAED of subjects (TAED for each subject is equal to the sum of TAED of each affected joint). The mean for "All Joints" is not equal to the sum of mean TAEDs for affected metacarpophalangeal and proximal interphalangeal joints because the denominators in each category differ, as the result of most subjects having multiple affected joints. This is implied in the text in the Results section: "Groups did not differ significantly for any baseline characteristic (Table 1) or TAED (Table 2)." Nevertheless, this could have been more explicit.

Finally, we have not reported an improvement over time, but rather, significantly less flexion deformity at the 6-month point in participants who received triamcinolone injections. The absence of a significant Group × Time interaction indicates similar trends in TAED over time, which is expected when both groups have undergone an effective treatment. We did not choose a multivariate analysis of variance because percentage correction is a transformation of TAED and not a separate response variable.