In This E-Focus on the Hand

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Kudos to the authors of these 4 case reports. They have collectively made the case—no pun intended—that isolated case reports are indeed worth the read. Indeed, appreciation of their brevity is surpassed only by their educational merit, despite not being "packaged" as part of a "higher level of evidence" study.

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Azar et al have shown that there may be a role for closed reduction of perihamate, transtriquetral fracture-dislocations. Reduction and stability of the triquetral fracture after hamate reduction attest to the ligamentous attachments between these 2 carpal bones, such that reduction of the hamate results in stable reduction of the triquetrum. If we encounter this injury, it appears that an attempt at closed treatment is justified.

Field and Rizzo noted a Vickers ligament at the time of surgical correction of a posttraumatic pseudo–Madelung deformity despite the suggestion of a physeal bar on preoperative magnetic resonance images. Their conclusion that an acquired Madelung deformity may be secondary to a bony bridge or a Vickers ligament is relevant, particularly since their patient had apparently sustained an extra-articular fracture as opposed to a Salter-Harris type of injury. It's worth acknowledging as well that unique anatomic findings may, more often that not, be related to our own surgical misnavigation or misinterpretation of normal anatomy.

Silverman et al report their unusual finding of pisiform-hamate coalition (PH). Their serendipitous finding and instructional literature review remind us that even the most atypical variants have usually been reported before. Though their patient was asymtomatic, they have informed us that this particular condition may result in ulnar neurapathy, and that pisiform excision is the recommended treatment in such cases. We are left to ponder the kinematic implications of PH coalition, however. Radial and ulnar deviation rely on motion between the hamate and triquetrum. In the setting of PH coalition, one might anticipate some degree of restriction thereof, in light of the location of this sesamoid bone within the FCU tendon.

Last but not least, Osuji and McAdams' report provokes thought regarding the relationship between observational findings and conclusions about cause and effect. Y=f(x) implies a simple claim. In this case, the authors opine that a dorso-ulnar ganglion (the dependent variable,Y) was associated with or caused by an os styloideum (the independent variable, x). One cannot dispute their clinical observation. In the absence of simple linear regression based on numerous examples, or consideration of additional independent variables, however, we are hard pressed to assess the statistical validity of possible association/cause. Thus, we do not know from their report whether the os styloideum actually caused the ganglion to develop or whether its removal is critical to satisfactory eradication of the ganglion.

Thank you to the authors for their thought-provoking reports. ■

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