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Reply

To the Editor:

We thank Knobloch et al for their comments about the development and validation of the URAM scale for Dupuytren's disease. The URAM scale is indeed a tool to assess patient-reported functional outcome in Dupuytren's disease and we agree that assessing disability is of key importance in Dupuytren's disease. However, we would like to respond to a few points made by Knobloch et al. We emphasize that the validation of a scale for a specific condition has to be done in the disease being considered and not another disease. Specificity here is a question of what disease from which the scale has been developed and in which it has been validated, and of functional limitation due to the considered condition; it is not only a question of side (right hand or left hand). Validation means studying the reliability, the construct validity, and the responsiveness as has been done for the URAM scale. To our knowledge, neither the DASH nor the MHQ have been validated in such a way for Dupuytren's disease. Our opinion is that nonvalidated scales should not be recommended and used to assess disability in Dupuytren's disease.

Nonetheless, we are grateful to Knobloch et al for their German translation of the URAM scale. We compliment them for using the French language version and translating it into the German language version using sound methodology. We encourage using the URAM scale in patients with Dupuytren's disease, including German patients. Indeed, assessment of the disease should not be limited to the flexion contracture. A patient's subjective perception of their own difficulties in daily living is pertinent in current practice and recommended in clinical trials (1,2). The URAM scale is the only scale developed and validated to assess disability in Dupuytren's disease. Therefore, the URAM scale should be part of the assessment of Dupuytren's disease in current practice and in future clinical trials.

Finally, we agree with Knobloch et al that the recurrence rate is also of key importance for assessing effectiveness of treatments in Dupuytren's disease and that a consensus definition of recurrence is needed. Several definitions have previously been proposed and used, including reappearance of cords, nodules, or contracture requiring further operation with followup of a few months to several years (3). A consensus definition of recurrence should therefore include the criteria for recurrence and the time of its assessment.

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Interreader agreement in determining monosodium urate deposition using musculoskeletal ultrasound: comment on the article by Howard et al

To the Editor:

We read with interest the article by Howard et al in a recent issue of *Arthritis Care & Research* on the reproducibility of musculoskeletal ultrasound (US) for detecting monosodium urate (MSU) deposition (1). In this article, the authors found an almost perfect agreement between 2 readers for the US identification of intraarticular tophi and the double contour sign of the hyaline cartilage in the first metatarsophalangeal and knee joints of patients with gout, patients with asymptomatic hyperuricemia, and healthy controls.

In a previous study by Filippucci et al (2), the interobserver exact agreement for the double contour sign of the hyaline cartilage in the knee was 92.7%, while the unweighted kappa value was 0.68, which are much lower than the values reported by Howard et al. We would like to raise some issues that could have influenced these results and, in our opinion, need to be addressed.

First, the US examinations were performed by 1 sonographer and the reproducibility was tested on the ability to recognize US findings in static images. Postacquisition reading of the images is likely to perform better than interpretation during real-time acquisition. The scanning technique may affect reading in several ways, leading to easy recognition of US findings when properly carried out, or conversely leading to misinterpretation due to suboptimal adjustment of the setting parameters and/or the probe positioning. Another issue that needs to be addressed is whether the sonographer was blinded to the patients' status (gout/hyperuricemia/healthy) or not.

Second, the exclusion of patients with arthritis from the control group facilitates the reader's work. Intraarticular tophi, as described in the study, are not always easy to

distinguish from synovitis, which could have similar ultrasonographic characteristics (3).

Third, another aspect that could have influenced the agreement is the US background shared by the 2 readers. Therefore, the following data may be of interest: years of US experience for each reader, US training shared by the 2 readers, and number of US images showing MSU deposition and/or the number of patients with gout examined together before the beginning of the study.

Studies on the use of US in crystal deposition diseases are of great interest and represent relevant contributions to the evidence of US validity in the assessment of findings indicative of MSU deposition. Since we believe that US is an operator-dependent technique par excellence, all of the steps from image acquisition to image interpretation need to be investigated for assessing reliability. Finally, we agree with the authors that US diagnosis of microcrystalline arthritis represents a challenge for all sonographers mainly because of the lack of a noninvasive and universally accepted gold standard imaging tool.

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Reply

To the Editor:

We thank Filippou and colleagues for their comments. We agree that the level of concordance we achieved in our study was strikingly high, and that the reasons for this might provide insight into how best to approach the use of musculoskeletal US for the assessment of gout.

The authors correctly note that our image interpretations were performed on static images and not at the time of acquisition. Separating the activities of scanning and image interpretation may have a beneficial effect on the ability of the reader to apply an objective protocol to interpretation. We agree that standardizing and evaluating the interreader and intrareader consistency of image acquisition are important, but we also believe these would be better served by a separate study. Moreover, the fact that all images were acquired by a single ultrasonographer using uniform patient and transducer positioning likely resulted in consistent views and image quality that allowed for a more homogenous interpretation of images. Our investigators scored all of the images without any knowledge of the corresponding patients' crystalline status (gout/hyperuricemia/healthy) by assigning each patient a number and shuffling the order of images before evaluation. While the ultrasonographer in our study was not blinded to the diagnosis, the standardized positioning of the patients and the probe likely was sufficient in preventing bias in image acquisition that could have altered the results of the study.

The authors also correctly note that our exclusion of patients with other forms of arthritis from the control group may have made it easier for the readers to identify gout, since the task at hand was largely one of determining gout versus no gout (or really, crystal deposition versus no deposition). As this suggests, US may be more useful in gout assessment when the clinical judgment has already been reduced to a simple yes/no determination, rather than asking the ultrasonographer/reader to identify an unknown diagnosis based strictly on clinical appearance. Such an interpretation essentially proves the importance of understanding pretest probability, and in this regard US may be no different than any other modality. One lesson from our study may therefore be applying clinical information to the fullest extent possible in order to increase the power of the US study by narrowing the scope of the question. Conversely, our assessments in this study were related only to urate deposition, not to the presence of synovitis or other features of gout that may readily be found in other diseases. In this regard, the main confounding diagnosis would have been chondrocalcinosis, which also presents as linear hyperechoic (bright) signals along (but below the surface of) the cartilage. Patients with known chondrocalcinosis were excluded from our study; nevertheless, chondrocalcinosis was detected incidentally, and by both readers, with no confusion as to its distinction from gout.

Finally, Filippou et al inquire about the background training of our US readers and suggest that readers with common training and/or experience may be more likely to produce concordant readings; we agree with this suggestion completely. In our study, the 2 readers had both similarities and differences in their skills and experience. One reader had 4 years of experience with musculoskeletal US and has served as an instructor at the American College of Rheumatology biannual US courses designed specifically for rheumatologists. The other reader had <1 year of US experience, but spent time with an expert who has published studies on US in crystal deposition disease in