Epidemiology should not be forgotten in osteoarthritis imaging

We read with interest the article by Hayashi et al. 2011. In their review titled ‘Osteoarthritis year 2010 in review: imaging’, a major focus of criticism was the choice of magnetic resonance (MR) pulse sequences for imaging of joints. Indeed, in their summary they suggest that ”the peer-review process of osteoarthritis (OA) imaging in any journal should involve musculoskeletal radiologists experienced in OA research to ensure the publication of papers with scientifically sound contents”.

This is an interesting notion and, if valid, should it be extended to imaging in all areas of rheumatology research? One may argue that there are no specific complexities that relate to OA that would not also be present in other diseases, such as rheumatoid or psoriatic arthritis or even more broadly to musculoskeletal research. Also, by extension, perhaps all biomarker work should be assessed by a biochemist and all joint mechanics by a biomechanist?

We would argue that such an approach is narrow. It does not take into account the issues that are critical in developing a diagnostic method. When a diagnostic method is developed, whether it be an imaging or serum biomarker or a clinical test, the first step is to optimize the method and then to validate it. Optimizing the method with MR imaging must take into account a number of competing issues. It involves matching the image sequences with the structure(s) being assessed. There are also the significant resource issues, since the true cost of MR is directly proportional to scan time. This is a major issue, particularly for publically funded studies. Thus a balance is required between the sequences used (and thus imaging time and cost) and the structures to be assessed. Then there is the issue of whether contrast agent should be used, adding more serious risks to the participants (e.g., nephrogenic fibrosis). These issues are significant in large community based studies, yet the authors of the review have only focussed on the imaging sequences.

Any method must then be validated. Simply having MR sequences that “look” better according to an expert in the field is not enough. There are other aspects that need consideration. What makes a method valid? Showing that a measure actually measures the tissue of interest is important. For example, to validate the use of MR derived cartilage volume measurement, the volume of cartilage obtained from MR imaging has been compared to that obtained by volume displacement of cartilage dissected from anatomical specimens, as has been done by us and others. However, this is not possible for all joint structures. The reproducibility of the method is important to ensure high quality data and consistency over time and between readers. The clinical validity is important. Clinical validity incorporates whether the measure predicts clinically important outcomes. For example, our methods to assess cartilage volume, cartilage defects, bone marrow lesions and subchondral bone cysts, have all been shown to be reproducible and to predict the clinically important outcome of joint replacement.

The area of MR imaging is very exciting. There is the issue of developing new methodologies to examine different aspects of joint structures. Whilst these may provide improved tissue contrast or capture a new facet of tissue quality, whether these methods are reproducible and predict clinical outcomes or structural progression needs to be studied, thus validating these new measures. Indeed, in a recent review on imaging in OA the authors of this current review describe interesting new developments. However when they summarize these new developments, they point out that many have not been shown to be clinically useful. In OA research, as with any other area, there is a need for an objective, scientific, assessment of the data. For example, if a finding in a study is based on a less sensitive imaging method, this does not mean that this result is invalid, but that if a more sensitive imaging method were used, a smaller study population would have been required to show the same effect. This is because using a less sensitive imaging method is likely to result in non-diagnostic misclassification which will tend to underestimate any effects being examined.

We agree with the authors that the peer-review process of OA imaging in any journal needs to ensure the publication of papers with scientifically sound content. However we would argue that this needs to be objective and based on sound scientific and epidemiological principles. We suggest that the cause of OA research will not need to be the case.

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References


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