Osteoarthritis: definitions and criteria

Sir: The American College of Rheumatology (formerly the American Rheumatism Association) subcommittee for classification criteria of osteoarthritis respect the opinions expressed by Drs McAlindon and Dieppe in their editorial.1 They raise important issues about the general and thorough need for osteoarthritis (OA) of the knee in particular.2,4 In their editorial they correctly state the purpose of the criteria for classification: ‘providing a descriptive framework in reporting OA to assure consistency of patient selection and thereby improve communication’. They conclude, however, that the proposed criteria are faulty and therefore should not be applied (and presumably not tested). They also state ‘that should be better ways forward’ but do not state how.

The critique lists the following as ‘shortcomings’:

Choice of controls: Fifty per cent of the control group had rheumatoid arthritis (RA). We consider this a strength rather than a weakness—in a rheumatic disease practice the comparison group if based on consecutive entry or an appropriate control group is helpful when controlling RA. Testing against controls without RA (half the control group), however, resulted in very similar criteria.

Subjects and controls were not matched for age and sex: The protocol required subjects to have knee pain. No other variable was matched as a variable on which one matches cannot subsequently appear as a disease discriminator. To emphasise the deficiency of McAlindon and Dieppe’s idea of matching for additional variables, if matched for joint space narrowing and osteophytes, radiographic findings of joint space narrowing or osteophytes could never appear as criteria. The argument is further neutralised by results of the discriminant trees. Age was not selected as a primary variable, and none of the trees selected sex. McAlindon and Dieppe fail to appreciate the discriminant value of the applied statistical methodology (recursive partitioning).

The use of osteophytes as criteria: The prevalence of osteophytes in the absence of OA is unknown. Similarly, the prevalence of knee pain with osteophytes in the absence of OA is unknown, but we suspect that it is distinctly uncommon.

The criteria are circular: Classification criteria of the rheumatic diseases suffer innately from circularity as the diseases are of unknown cause. We are not aware of another method to develop criteria. Listing the characteristics of the disease is mandatory. Circularity is miti-
gated by testing.

The criteria have not been validated: ‘validation’, if not defined, but Altman et al clearly reference criteria used (other than pain and stiffness),5 which have been validated in the past, including crepitus.

Cartilage damage is not a criterion: McAlindon and Dieppe fail to distinguish clinical disease from pathological changes of cartilage. Currently there is no substitute for tissue to document cartilage damage. Unfor-
entially, it is rarely practical for the clinician to obtain tissue for diagnosis. Medicine is replete with inferences of disease by clinical and laboratory examination. Must one obtain a cardiac muscle biopsy specimen for a diagnosis of a myocardial infarction in the presence of chest pain, electrocardiographic changes, and cardiac enzyme increases?

A recent communication by Dr Dieppe and coworkers on bone resorbing properties of synovial fluid highlights some of the issues of the use of clinical criteria.2 The investigators used the American College of Rheumatology criteria for RA to select their patients with RA. They then stated that patients with OA were selected on the basis of typical clinical and radiologic features. What is meant by ‘typical’? Why the inconsistency of criteria for one disease and vague terminology for the other?

The American College of Rheumatology criteria for OA should be considered as an organised attempt to define clinical variables that separate OA of the knee from other conditions, in the absence of a ‘diagnostic test’. It is felt that the criteria should not abandon our theories lightly, for this would involve too uncrtical an attitude toward us, and would mean that the theories themselves were not tested as rigorously as they should be.2 Karl Popper, a great influence of philosophers, believed a scientific theory must be proved false to be relinquished. Lindblad tested the criteria by arthroscopy on patients in whom the diagnosis of OA of the knee was uncertain—she found the decision trees reliable.6,9 Let us continue to evaluate these proposed criteria and improve them using appropriate scientific methods. Classification criteria are not ‘etched in stone’ as shown by changes in the diagnostic criteria for rheumatic fever, rheumatoid arthritis, and systemic lupus erythematosus.

In conclusion, the OA criteria of the knee should not be condemned arbitrarily and without vigorous testing.

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6 Alwan W H, Dieppe P A, Elson C J, Bradfield K P. Osteophytes in synovial fluids in destructive osteoarthritis and rheuma-

Assessment of rheumatoid arthritis

Sir: Dr Larsen,1 referring to a recent viewpoint article,2 says that some workers have apparently changed their minds about the assessment of rheumatoid arthritis (RA). I think the comment is directed at me and am glad of an opportunity to reply.

Table 2 of the viewpoint article shows the consensus view was that for assessing RA over one to two year periods ‘clinical and laboratory indices of disease activity’ were appropriate. This is not in conflict with views expressed by us,2,3 to which Dr Larsen refers. There are small differences in matters of detail. Dr Larsen specifically mentions ‘clinical score’. We proposed this years ago2 and later showed that there was a high degree of similarity between it and some other clinical measures.4 Which method to use is therefore a matter of personal choice; I still prefer ‘clinical score’, but a consensus meeting is not the place to promote individual preferences. Which laboratory tests to use is, similarly, best decided by personal preference.

Two more difficult problems are how to assess RA over periods exceeding two years and also the place of x rays. With regard to the first, I still suspect that there are few differences between them, but that the approach is an essential background. It must be said, however, that as time goes by some extra guides become increasingly necessary; I was in agreement with the views about this expressed at the meeting and set out in the viewpoint. In particular, the morbidty assessment suggested seems likely to be useful.

On the question of x rays, Dr Larsen must surely agree that there are at least two difficulties; firstly, the x-ray change and change in overall function is not very high; secondly, x rays change in, for example, the hands, do not reflect the impact of changes in a big joint. Hence the comment that this value is not placed on x rays. Nevertheless, I suspect that most people, uncertain about the progression of RA or the effects of a drug in a particular patient, will still be influenced by x ray findings; and I do not think x rays will be abandoned as part of the methodology of trials.

The viewpoint did not, it seems to me, point to changes of mind; the meeting did provide a useful forum where ideas, shaped by experience, could be reviewed, argued, perhaps refined—then submitted for wider appraisal.

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