Cricothyroid arthritis in a child with familial Mediterranean fever

Sir: We describe for the first time the occurrence of cricothyroid arthritis in a girl who first presented with migratory polyarticular arthritis but eventually developed the classical features of familial Mediterranean fever.

A 9 year old Palestinian Arab girl was admitted in January 1979 with fever and migratory polyarticular arthritis of the large joints. The heart was normal. The erythrocyte sedimentation rate was 110 mm/h and the antinuclear antibody titre was 400 Todd units. A diagnosis of acute rheumatic fever was made and treatment was started with secondary prophylaxis. During the following six years she had several episodes of arthritis, which were interpreted as recurrence of acute rheumatic fever due to irregular prophylaxis, and occasional fever and abdominal pain.

In January 1983 the girl was admitted with fever and arthritis of both elbows and the right wrist. Her history of the developed arthritis of the cricothyroid joint. The diagnosis was verified by indirect laryngoscopy. She also developed arthritis of the interphalangeal joints of both hands. She became better after five days of aspirin treatment. Two months later she had another similar episode of transient arthritis of the cricothyroid and interphalangeal joints. During the following three years she had several episodes of fever and abdominal pain, with the frequency progressively increasing to one to two attacks a week.

She also developed arthritis of the ankles associated with erysipelas-like erythema. Family history revealed that her mother and a maternal aunt and two sisters had had similar recurrent episodes. Prophylaxis with colchicine was effective in decreasing the frequency of febrile and painful episodes; during the past 12 months she had only three attacks with abdominal attacks and one episode of transient arthritis of the left ankle.

The synovial attack of familial Mediterranean fever typically appears as acute monoarthritis affecting large joint of the lower extremity.1-4 Involvement of the small joints, including the temporomandibular, sternoclavicular, and metatarsophalangeal joints, has been described in patients with familial Mediterranean fever,1-4 whereas involvement of the interphalangeal joints has been reported to be most unusual.4 Cricothyroid arthritis in the course of familial Mediterranean fever has not been previously described.

The presentation with migratory polyarticular arthritis, the involvement of the interphalangeal joints, and the long period before the appearance of the classical manifestations of familial Mediterranean fever are other unusual features in this case.

uestion: What does the text say about the patient's medical history and how does it differ from previous cases of familial Mediterranean fever?

Answer: The text describes a 9-year-old Palestinian Arab girl who presented with migratory polyarticular arthritis and eventually developed the classical features of familial Mediterranean fever. Unlike previous cases, she had arthritis involving the cricothyroid joint, which is typically not seen in familial Mediterranean fever. Additionally, she had several episodes of fever and abdominal pain, which were interpreted as recurrence of acute rheumatic fever due to irregular prophylaxis. The family history showed that her mother and a maternal aunt and two sisters had had similar recurrent episodes. The synovial attack of familial Mediterranean fever typically occurs as acute monoarthritis affecting large joints of the lower extremity, but in this case, involvement of the small joints, including the temporomandibular, sternoclavicular, and metatarsophalangeal joints, was described. Involvement of the interphalangeal joints, which is unusual in familial Mediterranean fever, was also reported. This case is unique because it describes a patient with familial Mediterranean fever who developed cricothyroid arthritis and had multiple episodes of fever and abdominal pain.

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Dr. Doherty's expertise in the field of osteoarthritis and his interest in non-steroidal anti-inflammatory drugs (NSAIDs) is evident. He has previously presented on the subject and his current work continues to highlight the importance of these drugs in the management of osteoarthritis. His research and clinical work on the topic have been published in numerous reputable journals, and he has presented his findings at several international conferences. His expertise in the field is widely recognized and respected by both his peers and the broader medical community. His contributions to the field have been significant and have helped advance our understanding of osteoarthritis and the role of NSAIDs in its treatment. His work is an example of the dedication and commitment that is required to make meaningful contributions to the field of medicine. His focus on non-steroidal anti-inflammatory drugs (NSAIDs) is particularly relevant given the ongoing debate about their effectiveness and safety in the management of osteoarthritis.

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Dr. Doherty's letter raises important considerations regarding the current use of NSAIDs in the management of osteoarthritis. His cautionary stance on the potential risks associated with their use is noteworthy and highlights the need for careful consideration of individual patient circumstances when making treatment decisions. His emphasis on the importance of balancing efficacy and safety is a critical aspect of clinical practice. The ongoing debate about the role of NSAIDs in the management of osteoarthritis underscores the complexity of the issue and the need for continued research to better understand their effectiveness and long-term safety profile.

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Dr. Doherty's letter is a valuable contribution to the ongoing discussion about the use of NSAIDs in the management of osteoarthritis. His expertise in the field is evident, and his thoughtful considerations regarding the potential risks associated with their use add valuable context to the ongoing debate. The letter highlights the importance of balancing efficacy and safety and underscores the need for continued research to better understand the role of NSAIDs in the management of osteoarthritis. His work is a testament to the dedication required to make meaningful contributions to the field of medicine.
and magnetic resonance imaging techniques.\(^7\) Eventually the accuracy and the methodology associated with these techniques will improve sufficiently to allow their routine clinical application, but in the short term we can only rely on data generated from animal studies to guide us in selecting the drugs of potential clinical interest.

Although we all agree that animal models of osteoarthritis are imperfect, they do permit a direct assessment of a drug's effect on a variety of joint features which are relevant to the human condition. These effects include not only changes in cartilage integrity but chondrocyte metabolic activity, subchondral blood circulation, osteophyte formation, synovial cell metabolism, and biosynthesis of hyaluronic acid, all of which should be included in any definition of chondroprotection.\(^8\)

Furthermore, most drugs used in clinical medicine today were selected from animal studies in which the drugs showed activity which may, or may not be directly applicable to human disease. If this practice is to be criticised the criticism should perhaps be directed at those pharmaceutical companies that have been reluctant to deviate from traditional methods of drug discovery, for there exists a plethora of 'super aspirins' and it is more by luck than design that some of these molecules seem to show chondroprotective activity.

For more than 80 years the medical community has been content to accept the products provided by the pharmaceutical industry for the treatment of musculoskeletal disorders. Today, as a result of the debate stimulated by laboratory studies, this attitude is changing and doctors are rightly questioning the long term efficacy of their NSAIDs, particularly the deleterious side effects which may accompany their use.

The influence of the advertising industry notwithstanding, the choice of an NSAID should be made on the basis of a judicious evaluation of the laboratory and clinical data available at the time. Although these data may be imperfect they can provide the stimulus for further investigations and it is only by this means that we can hope to generate the ground swell of opinion necessary to change prevailing attitudes and promote new therapeutic advances in this much neglected field.

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Sirs: I am sure that Dr Ghosh and I are in general agreement about the many issues relating to the effects of non-steroidal anti-inflammatory drugs on osteoarthritic and normal joints, and I welcome his comments on this subject.

I was educated in the 1989 leader,\(^1\) in vitro and animal work have given valuable insight into possible mechanisms of joint injury and repair, and stimulated interest in the effects, either detrimental or beneficial, of currently available drugs on the joints (not to mention the gut) of our patients. Nevertheless, I would reaffirm the need for caution in extrapolating too rigidly such laboratory derived data to the clinical situation of human osteoarthritis.

When considering the natural history or modification by health intervention of any disease process, it is important to distinguish process from outcome measures.\(^2\) Process measures, clinical or investigative, primarily relate to mechanisms of disease causation and tissue response, reflecting such aspects as inflammation, immune reaction, tissue damage/synthesis/repair, and structural change. Outcome measures, by contrast, relate more to the meaningful effects of disease on the individual, reflecting such aspects as impaired function, suffering, morbidity, and mortality; such measures by their nature are predominantly clinical.

Although process and outcome measures may correlate positively, the former cannot be used to predict the latter.\(^3\) In osteoarthritis, particularly, there is marked discordance between symptoms, signs, and radiographic or pathological abnormality: an association between any process markers that we have and outcome remains to be established. Although I share Dr Ghosh's enthusiasm for continuing work investigating biochemical markers of joint damage/repair and improved assessment of structure, we must remain aware of the limitations of such (predominantly process) measures. Again as previously discussed,\(^4\) undue emphasis on cartilage (cf bone, capsule, ligament, muscle) may prove inappropriate. Although common sense dictates that cartilage loss is bad, this is not an isolated change within the joint and need not be the crucial factor determining outcome. For example, we know that despite gross cartilage loss most osteoarthritic joints, especially in the hand,\(^5\) function normally with minimal or only periodic symptoms. In respect of intervention in osteoarthritis the whole joint (not just selected, individual biochemical or structural change) and whole patient need to be considered.

From a clinical standpoint, therefore, symptoms and functional status remain the standard by which to judge long term success or failure in osteoarthritis. Investigation of accompanying structural, physiological, and biochemical changes (in animals and man) may improve our understanding of its process, and perhaps suggest means of prevention. If found to equate with outcome, such process changes may additionally prove useful in monitoring effects of intervention.

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