

# Correspondence

## INSTRUCTIONS FOR LETTERS TO THE EDITOR

Editorial comment in the form of a Letter to the Editor is invited; however, it should not exceed 800 words, with a maximum of 10 references and no more than 2 figures (submitted as camera ready hard copy per Journal Guidelines) or tables and no subdivision for an Abstract, Methods, or Results. Letters should have no more than 3 authors. Full name(s) and address of the author(s) should accompany the letter as well as the telephone number, fax number, or E-mail address.

Contact: The Managing Editor, The Journal of Rheumatology, 920 Yonge Street, Suite 115, Toronto, Ontario M4W 3C7, CANADA. Tel: 416-967-5155; Fax: 416-967-7556; E-mail: jrheum@jrheum.com Financial associations or other possible conflicts of interest should always be disclosed.

## The Approaching Crisis in Rheumatologic Care

To the Editor:

The recent article<sup>1</sup> and editorial<sup>2</sup> by Dr. Lewtas on the approaching crisis in rheumatologic care in Canada is of vital interest. In the USA, at least, the crisis is not approaching but is already here, and has been intensifying for more than 5 years. While Canada may have additional problems compared to the US, a major deficiency in both countries is the number of trained rheumatologists available.

During the past 12 years, the number of graduates of American medical schools has not been increased despite a 40,000,000 addition to the population and an almost logarithmic increase in the elderly segment, who are the greatest consumers of medical care. During the last 7 years, the number of rheumatologists graduating from accredited programs has been drastically reduced, resulting in a serious shortage as older physicians have retired or died. It is not uncommon to find active practices essentially closed to new patients or necessitating 6 month delays for an appointment. To not have recognized this years ago and not have taken corrective action is an egregious failure on the part of the leadership of our professional societies.

It should be easy to identify problems as your journal has done. It is more difficult to provide solutions. Some of us, as I have, chose to work far beyond our normal retirement age, but this is a temporary solution. Obviously, we need to train more rheumatologists.

My associate and I offer a rheumatology rotation to residents of our local medical school that has become highly sought after, but available only during their third year of training. Most are fascinated by the clinical practice of rheumatology. Unfortunately, they have already made their career choices by then and are on their way to another speciality. To attract physicians to our field, it will be necessary to expose them at an earlier time in their career.

But even if we are able to fill our current programs next year, it will be 3 years before new rheumatologists are available and many years before the deficiencies of the last decade are reversed. What do we do in the meantime? Allow patients with rheumatoid arthritis to become crippled or die? Allow the many women who are limited by fibromyalgia to suffer? Refuse

to take care of the many diseases we are so capable of treating? Turn over therapy with the new biological drugs to physicians untrained to do so in a qualitative manner?

Faced with this problem, my associate and I recruited and trained a physician extender — specifically a physician assistant — to help us care for and monitor stable patients with an assortment of arthritic illnesses. Since then, we have brought in two additional assistants, one of whom devotes her time to patients with fibromyalgia. This has worked out beautifully and has allowed us to see seriously ill patients immediately and others within a few weeks. There is some limitation to this but we have not reached it yet. Our PA's do an amazing job! We recently gave an internet interview that summarized our experience [www.jointandbone.org/education/rheumanations/rn\_20021212.cfm], and now the American College of Rheumatology wants information on physician extenders, although they summarily rejected an abstract we submitted on this subject for the 2002 ACR meeting.

It is our opinion that clinical rheumatologists could each employ 2 PA's and thus triple their availability for referrals and consultation and increase their availability to those requiring their expertise. The current shortage could be rapidly eliminated or reduced. One can train an extender with 2 years of graduate school, rather than the 10 years necessary for a trained MD rheumatologist. Not only would his education be far less expensive than medical school, but he would be available for a 42 year career, rather than the 33 year career of an MD rheumatologist. Granted, he will require MD supervision, but he can take over many tasks that burden us each day.

Do not be mistaken. I am not proposing this as a temporary expedient but as a permanent method of rendering excellent rheumatologic care to all who need it, in a timely manner, at reasonable cost. For those of you who wish to be critical, try it. It really works.

ALTON J. MORRIS, MD, Arthritis Associates, Kingsport, Tennessee, USA.

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## Dr. Lewtas replies

To the Editor:

Dr. Morris does make an excellent point in his letter. Although we can develop strategies for increasing physician recruitment into the field of rheumatology, we have an immediate problem to be addressed. And these shortages are going to continue to affect our individual practices for at least 5 to 10 years, even if we are successful in attracting students into our post-graduate training programs.

A recent article in the *New England Journal of Medicine*<sup>1</sup> described the trend to increased use of nonphysician health care providers in the United States. Interestingly, nonphysician providers were used by patients for additional care as opposed to replacement of care by their physicians. There is really no threat to our livelihood or profession to add other health care professionals to the team, and as rheumatologists we have been open to a multidisciplinary approach.

Dr. Morris's model is particularly appealing because the physician extender (or assistant) works within the physician's practice. Thus, the approaches and goals for individual patients and/or diseases can be harmonized between the physician, the assistant, and the patient. The rheumatologist maintains some control over the information and treatment plans.

The current specialist shortage will breed creativity, and it is important to hear how individual rheumatologists have developed local solutions. It

would be helpful if there were a forum for sharing this information. We are trying to establish such a forum in Canada at this time.

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### Methotrexate, Hydroxychloroquine, and Intramuscular Gold in Rheumatoid Arthritis

To the Editor:

The article by Hurst, *et al*<sup>1</sup> supports the argument that well performed longterm observational studies have significant advantages compared to randomized controlled trials (RCT) in evaluating outcomes in a chronic disease like rheumatoid arthritis (RA). The study provides information about the comparative effectiveness of different standard treatments in early RA in the real world of usual patient management outside of the artificial conditions of a RCT.

Despite some limitations of the Health Assessment Questionnaire (HAQ) being influenced more by disease activity in early disease and more by structural damage in later disease<sup>2,3</sup>, disability — as measured by HAQ — is the most important outcome from the patients' perspective. The study by Hurst, *et al* calculates annualized area under the curve of the HAQ as a measure of disability averted. With respect to this primary outcome, parenteral gold was found to be the most efficacious therapy compared to methotrexate (MTX) and hydroxychloroquine (HCQ), with 24.1% of possible disability averted with gold, 21.2% with MTX, and 16.0% with HCQ. This finding is in agreement with other comparative studies of parenteral gold and MTX that showed at least similar or even better efficacy of gold on disease activity and on structural damage measured radiographically<sup>4,5</sup>.

Pincus often states that data cannot lie. But there are always several ways to interpret them. Therefore the authors come to a totally different conclusion, arguing that MTX should be the preferred first-line treatment because it is continued significantly longer (3.23 vs 1.96 yrs) than parenteral gold. We cannot follow this argument, as there are several other explanations for the observation of longer treatment duration with MTX. Medicine is not the only field where new developments are considered more attractive than established ways, a phenomenon that can be observed with the new therapies now available for patients with RA, just because they are new. At the time this study was performed, MTX was the new treatment that every modern doctor wanted to offer his patient. Patients preferred the possibility to take tablets once a week instead of getting regular injections. This may already have led many patients and doctors to change from their initial gold treatment to MTX. But the most important reasons for the shorter treatment duration of parenteral gold are the following:

1. Many patients taking parenteral gold experience striking improvement or complete remission with side effects, especially skin reactions evolving at the same time<sup>6</sup>. These patients are not willing to continue their treatment because they don't feel the need to continue a treatment that has obvious side effects. The beneficial effect on disease activity is missed if the time after the cessation of treatment is not taken into consideration. We were able to show that patients who discontinued gold treatment performed much better during longterm followup than those who stopped MTX treatment, in a randomized clinical trial comparing both treatments<sup>8</sup>.
2. On the other hand, strict dosing regimens did not allow adapting the therapy to the dose that maintained clinical efficacy and avoided side effects, as was recommended later for patients with mucocutaneous side effects<sup>9</sup>.
3. Finally, the recommended maintenance dose of 50 mg every 4 weeks is too low in many patients to sustain the therapeutic effect<sup>10</sup>, so that loss of

efficacy was assumed, while higher gold doses of 50 mg every other week or even weekly as used by many rheumatologists in Europe might have worked better.

We only want to comment very briefly on the authors' conclusion that gold as second-line treatment or a second course of gold were found to be dramatically less effective. These statements are based on the evaluation of only 33 and 6 patients, respectively, thereby substantially questioning the validity of this hypothesis, which is in contrast to other results<sup>11</sup>.

Summing up these considerations, and again supported by the data presented by Hurst, *et al*, there is enough reason to prefer parenteral gold with more individualized dosing options as the first-line disease modifying antirheumatic drug in patients with RA, especially early RA, as still practised in some rheumatology centers around the world, including ours.

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#### Drs. Albert, *et al* reply

To the Editor:

We thank Drs. Wassenberg and Rau for their comments and their interest in our paper<sup>1</sup>.

They apparently concur that disability averted is a meaningful outcome measure for disease modifying antirheumatic drug therapy. We agree that, per year of exposure, the disability averted by gold therapy is at least as large as that achieved with methotrexate (MTX). We observed that the total disability averted is greater with MTX than gold because the average length of therapy on MTX is longer. These conclusions do not directly bear on the

choice to use one or the other of these medications, since cost, toxicity, availability, patient preference, and other factors were not analyzed. We agree that gold may be underutilized, but again, we did not analyze this. Nor did we examine the cause for shorter treatment intervals with gold versus MTX, so we cannot answer this issue directly. We hope to address these issues in a forthcoming cost-effectiveness analysis.

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#### Magnetic Resonance Imaging Criteria to Differentiate Inclusion Body Myositis from Polymyositis

To the Editor:

It was with great interest that I read the article by Dion, *et al* about the differences seen by magnetic resonance imaging (MRI) between inclusion body myositis (IBM) and polymyositis (PM)<sup>1</sup>. The article emphasizes the value of MRI in differentiating between IBM and PM.

There are, however, several items of concern. First, in any study that examines the value of a diagnostic test, the issue of the gold standard arises. For IBM and PM there is no clearly defined gold standard. Therefore, complex diagnostic criteria have been drawn up, usually during consensus meetings. The Bohan and Peter criteria used by Dion, *et al* are outdated and were defined at a time when IBM was not fully recognized to be a separate disease entity<sup>2</sup>. Following these criteria most patients with IBM will be diagnosed as having PM. Especially in a study that wants to differentiate between IBM and PM, the use of the Bohan and Peter criteria is not very wise.

IBM was diagnosed by Dion, *et al* according to the preliminary criteria published by Calabrese, *et al* in 1987<sup>3</sup>. These criteria have never been accepted internationally and have been replaced by better defined criteria that distinguish between sporadic IBM and hereditary IBM (which the Calabrese criteria do not, even though Dion, *et al* present them as criteria for sporadic IBM) and that strongly emphasize the specific clinical syndrome of IBM<sup>4</sup>.

Second, the authors report that no difference in muscle strength was observed between PM and IBM, and that muscle strength was not correlated with MRI findings. Unfortunately, the authors measured strength of the proximal muscle groups only. IBM is characterized by a predominantly distal muscle weakness<sup>4</sup>. If proximal and distal muscles had been measured, a difference between IBM and PM might have been found, and there might have been a correlation between MRI findings and muscle strength.

Third, I would like to stress that IBM is characterized by a very specific clinical syndrome that usually can be distinguished from PM by experienced clinicians based on clinical signs and symptoms, electromyographic abnormalities, and muscle biopsy findings. In only a few cases can a clear distinction not be made, and it is in those cases that we might need the extra information that MRI can provide. It is therefore interesting to consider what the clinical characteristics are of the one patient described by Dion, *et al* in their Table 2 with PM that has fatty infiltration exclusively in the anterior muscle groups and an asymmetrical distribution of the fatty infiltration. My hypothesis is that this patient has steroid-resistant PM — in other words, a probable case of IBM.

Nevertheless, I thank the authors for conducting this time-consuming study, which can serve clinicians in situations of doubt, and which emphasizes the potential value of MRI in neuromuscular disorders.

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