**MATTERS ARISING**

**Epidemiology of whiplash**

Space restrictions prohibit a comprehensive refutation of the uneven treatment of the whiplash literature presented by Ferrari and Russell.1 They fiercely interrogate research that does not support their view, yet uncritically embrace literature favouring their pre-conceptions. Central to their argument is the assertion that there are different rates of chronic whiplash in different countries, and that systemic injury related damage cannot account for the wide differences.3

A valid comparison between the prevalence of any condition in two places would require that it is measured in the same way. Balla’s study comparing Singapore and Australia was little more than anecdotal from interviews of selected Singaporean doctors compared with the data from Australia.4 Such data may be fatally corrupted by recall, case selection, sampling, and expectation bias.

Caution should be observed in comparing insurance claim rates between countries. There is no international consistency in notification of accidents or insurance or compensation procedures. Conclusions drawn from such comparisons are unsustainable and subject to the ecological fallacy. The frailty of using insurance claims as a surrogate for the incidence of injury does not seem to have been addressed by Ferrari and Russell. A claim is a behaviour arising from a combination of motivation, enabling circumstances, perceived benefits, costs, social norms, peer and family pressure, and fear of current or future pain and disability—all factors extraneous to the injury itself. The Victorian experience in Australia is particularly pertinent. Fewer claims for whiplash were noted after the introduction of legislation creating bureaucratic barriers, disincentives, and up-front costs for potential claimants. Some then argue that chronic symptoms after whiplash do not occur in communities lacking a compensatory system. This is harder to make a claim, fewer people will venture to raise alarm about whiplash, and reinforce prejudice against genuinely afflicted patients.

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Through their leader, Ferrari and Russell’ve venture to raise alarm about whiplash, repeating the same arguments that they have already raised in two previous editorials and a letter to the editor. But their alarm is overstated and misplaced.

Acute whiplash is not a problem. Even the studies of Radanov et al show that only some 5% of patients have severe symptoms at 12 months.1 Meanwhile, the study of Borchgrevink et al seems to be a breakthrough.1 Most patients can be adequately treated simply by advising them to act as usual. If there is any psycho-social problem with acute whiplash, it is on the part of doctors and therapists who overmedicate patients prematurely.

However, even so, some 10–20% of patients remained symptomatic at six months. Two questions arise: why are these patients symptomatic, and what should be done about them? This approach has been to investigate these patients for a possible source of pain. Under stringent, double blind, controlled conditions we have found that we can pinpoint a source of pain in the zygapophysial joints in some 50% of these patients. Moreover, by surgical treatment we can relieve their pain and their psychological distress and return them to normal life.

These patients may not be typical of acute patients, but they are quite typical of chronic patients. Ferrari and Russell1 contend that zygapophysial joint pain must be rare. Indeed, it is, for it accounts for only half of 5–10% of the original population; but it accounts for 50% of the chronic population. Elsewhere, Ferrari and Russell argue from the general to the specific, that we can pinpoint a source of pain in the zygapophysial joints in some 50% of these patients. Moreover, by surgical treatment we can relieve their pain and their psychological distress and return them to normal life.

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Ferrari and Russell1 argue that there is no persisting lesion, and that psychological and social factors totally explain the chronic complaints of these patients. They can utilise the work of Radanov, by claiming that it “is fraught with at least 15 significant methodological flaws”. They do not enunciate these flaws but instead cite four references, thereby relying on sophistry instead of logic. Readers—if these references are consulted, the last three offer no criticism of Radanov. Only the first, a letter, offers criticism, but cleverly Ferrari and Russell do not inform the reader of Radanov’s rebuttal of these criticisms.

Yet even if we accept that psychosocial factors are important in these patients, Ferrari and Russell do not provide an answer as to what they do about them. They criticise the work of Radanov, by claiming that it “is fraught with at least 15 significant methodological flaws”. They do not enunciate these flaws but instead cite four references, thereby relying on sophistry instead of logic. Readers—if these references are consulted, the last three offer no criticism of Radanov. Only the first, a letter, offers criticism, but cleverly Ferrari and Russell do not inform the reader of Radanov’s rebuttal of these criticisms.
Making selective use of the literature and incorrect quoting of previous research, the January 1999 "leader" insists to intend the view of the whiplash syndrome as malinger- ing. This reply cannot be exhaustive but will address the following: The Ballas paper lacked a definition of the whiplash syndrome and did not describe the assessment of 300 selected cases seen in a single practice. Moreover, selection bias arising among the non-selected sample the authors announced the study in the Medical Weekly Journal and repeatedly distributed letters to primary care doctors." Behind this false reporting is probably the hope that the scientific community will eventually become tired of commenting, which eventually will demonstrate the malingering hypothesis for whiplash injury.

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References


Authors’ reply to Drs Barnsley and Bogduk

We thank Drs Barnsley and Bogduk for their comments. Dr Barnsley reiterates a dualistic (mind-body) approach that we have been trying to blur and indeed do away with for various reasons, most notably that dualistic approaches have been largely unhelpful in the past. We do not believe that chronic pain is all in the mind, nor all in the body. We also believe, to use her term, that these patients are "genuinely affected." Dr Barnsley’s comment that a "more sober view" suggests a reduction in insurance claims does not reflect a reduction in symptom prevalence requires proof, and is not in accord with admission of anecdotal reports from Australian rheumatologists, nor with the evidence from Lithuania (she does not quote the subsequent prospective study), Germany, and Greece. Dr Barnsley also suggests that the impressive study presented at the World Whiplash Congress in Vancouver which suggests that changing the claim scheme has dramatic effects on recovery rates, as indicated by various patients.

Both Drs Barnsley and Bogduk have missed the key message in the epidemiologi- cal literature—the rapid recovery rate seen in some countries is not being duplicated in others. The studies in Lithuania, Greece, and Germany cannot rule out the possibility of a small number of subjects having chronic pain and disability, but they do show that recovery (as measured by absence of symptoms and return to normal activities, as well as other patient centred outcomes) occurs in 90–95% of subjects in six weeks or less. It is this fact that compels us to question the basis for chronic pain in say, Canada. We find that whiplash in Canada (and reportedly in many other countries) is a massive economic burden, with more than 50% of accident victims reporting chronic pain six months after the accident.

The patients of Dr Bogduk’s study represent merely the tip of a large iceberg. Thus new paradigms are necessary to understand why some subjects recover within six weeks or fewer and others do not. As no one has suggested that Lithuanians, Greeks, and Germans have a different anatomy, we need to look for an explanation for this difference in recovery rates. It is certainly possible that a small proportion of subjects could have chronic structural damage in countries like Lithuania, as Dr Bogduk suggests, and that current studies with background prevalences of neck pain in the control population of up to 10% are not large enough to distinguish an additional 2–3%. Yet, this additional 2–3% of patients are not the group of patients we are describing—a large 50% of patients recover from pain at six months1 that we are concerned with, and the cervical zygapophysial studies are not relevant for this larger group. Indeed, we were not aware that the subjects of Dr Bogduk’s study had undergone such high acceleration-deceleration (of 30–60 kph) as Dr Bogduk indicates. This fact makes it even less likely that their study group is typical of most patients with chronic whiplash, who instead undergo much lower velocity collisions. Clearly, and for good reasons, Dr Bogduk’s study patient spectrum is very different from the group we are concerned with. Our disagreement is not substantially with the few percent that he may see with facet joint problems, but rather with the rest of the iceberg of chronic pain.

The purpose of our model is to develop discussion on research questions and develop bona fide research efforts to understand what explains different recovery rates, so we can use that understanding in changing both the approach of the therapeutic community and society in response to acute whiplash. Understanding the behaviour that promotes chronic pain is the first, best step to changing it. We agree with Bogduk, once again, that over- treatment and medicalisation are likely to be part of the problem. Yet, until it is thoroughly demonstrated to, and understood by, the therapeutic community and society at

large, that this is part of the problem, this practice is unlikely to change.

By setting forth this model we can now investigate it. We are making efforts to do this, and we hope that quality researchers such as Drs Barnsley and Bogduk will engage in such efforts as well.

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Authors’ reply to Dr Radanov

Dr Radanov’s expressed concerns and cry for autoda-fé* are based on his perception that our biopsychosocial model is one of malinger-
gering as an explanation for the late whiplash syndrome. As we have explicitly stated, in both our current article and in a previous review on this topic, we reject a model based on malingering and we consider this to be a rare or uncommon presentation.† Dr Radanov’s concerns are therefore misplaced. That Dr Radanov is unable to appreciate how our biopsychosocial model presents alternatives to the otherwise unhelpful, unidimen-
sional, and dichotomous approaches taken by himself and others is a problem for him, but one which we cannot ameliorate in the space available. We thus refer him to a more comprehensive resource.‡

Once again, we reject the view that the chronic pain of whiplash is due to an enigmatic and inexplicable chronic injury, and we simultaneously reject the view that the best explanation (or even a common explana-
tion) for the late whiplash syndrome is malinger-
ging or psychological models that place the pain “all in one’s head”. The biopsychosocial model includes physical sources for pain, and incorporates psychosocial factors to explain both the severity and attribution of the pain, as well as further behaviour enacted upon this substrate of otherwise benign physical sources of pain. Thus we maintain that the most helpful focus of discussion and research should be on identifying how the various ele-
ments of the biopsychosocial model explain the variance in epidemiology of the late whiplash syndrome, and why, even within a given culture some accident victims recover quickly and others do not. Dr Radanov’s views may be coloured by the relatively benign nature of the problem he sees in Switzer-
land. Even with an advertising campaign to recruit subjects, the Swiss outcomes were very much better than those currently being described in North America. We maintain that the Swiss effort at understanding these issues has been a start, but is a mere footstep in a much longer journey of inquisition.

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Rheumatoid arthritis, poverty and smoking

Maiden et al raise a number of important and interesting points in their paper. Does social disadvantage contribute to the excess mortal-
ity in rheumatoid arthritis patients?‡‡ They have observed that mortality in rheu-
matoid arthritis (RA) correlated with social grouping on the west coast of Scotland. Patients with RA of the lowest socioeconomic class have an increased mortality when compared with patients of a higher socioeconomic class. Moreover, RA was more preva-
ent in patients with RA of lower socioeconomic class. We propose that these two important observations can both also be ex-
plained by cigarette smoking.

The authors commented that cigarette smoking was more prevalent in the patients with RA of lower socioeconomic class in their study. In Britain there is a marked difference in smoking prevalence between social classes. In the 1996 census 41% of lower social class men (social class 4) were current smokers, with only 12% of men in the highest social class (social class 1) currently smoking. Cigarette smoking kills 120 000 people a year in Britain.† Most of these deaths are as a result of cardiovascular disease, respiratory disease, and lung cancer. Maiden et al observed that 65% of the deaths in their study occurred as a result of these diseases. Current data show that continued cigarette smoking throughout adult life doubles age-specific mortality rates even in healthy young people. Cigarette smoking is associated with an increased risk of RA in both men and women. The increased mortality seen in patients with RA of low socioeconomic status could be explained in part by cigarette smok-
ing, and that cigarette smoking itself might have contributed to the excess RA seen in the most socially deprived.

Since the poorest in our society appear to have an increased risk of RA, studies designed to identify risk factors for RA may best be focused on those with the highest risk. Ciga-
rette smoking may be especially important to study, because its most powerful effects would be seen in the poorest socioeconomic popula-
tion with RA. Laudable attempts to study the epidemiology of RA in Britain have been set up. One example is the Norfolk Arthritis Register. However, we would suggest such populations, in which there are a large proportion of higher socioeconomic groups, are unrepresentative of the large industrial cities in Britain. In 239 patients with RA in the Merseyside region under hospital follow up, the social class of our patients was identi-

Table 1

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<tr>
<th>Social class</th>
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<tr>
<td>1 – 2 %</td>
<td>3N</td>
<td>4 – 5 %</td>
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<tr>
<td>Social class</td>
<td>Social class</td>
<td>Social class</td>
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<tr>
<td>I &amp; II</td>
<td>III</td>
<td>IV</td>
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Table

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<tr>
<th>Inflammatory polyarthritis cases Norwich and Worcestershire (154)</th>
<th>RA cases Merseyside (239)</th>
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<tr>
<td>51 (33)</td>
<td>73 (47)</td>
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<tr>
<td>28 (12*)</td>
<td>87 (36)**</td>
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*€p<0.0001; †p<0.05.
‡Social class based on the Office of National Statistics classification of occupations.
§N = non-manual; M = manual.
Authors’ reply

We welcome the letter entitled “Rheumatoid arthritis, poverty, and smoking” in response to our article “Does social disadvantage contribute to the excess mortality in rheumatoid arthritis patients?” The importance of looking as a contributor to the influence of socioeconomic deprivation on mortality is rightly emphasised. However, as Black has pointed out eloquently, smoking alone does not account for the excess mortality seen among lower socioeconomic groups.

We observed a higher mortality rate among patients with rheumatoid arthritis (RA) living in deprived areas relative to those living in affluent areas. However, in our methodology we were unable to determine the social class of individual patients according to the Office of National Statistics classification of occupations. Nevertheless, whether measured by income, occupation, educational level, social class, or ecological variables such as the Carstairs score, socioeconomic deprivation has been shown to influence health.7 In addition, we observed that there were more patients with RA living in areas of a lower socioeconomic status than in the general population in Scotland. Although this may result from a higher prevalence of RA among the lower socioeconomic classes, this conclusion cannot be drawn overly from our study. Particularly, the results of studies of inception cohorts in areas of affluence and deprivation would prove valuable in determining the epidemiology of RA.

Our cohort 40% of the most affluent group (Carstairs 1 and 2), 45% of Carstairs 4, 5, and 6, and 65% of the most deprived group (Carstairs 6 and 7) were current smokers; figures much higher than the 1996 census figures of 12% and 41% for social classes 1 and 4 respectively. This difference may reflect the fact that our patients were recruited a decade earlier (1984–85), but there are also social/cultural differences between Scotland and the United Kingdom as a whole. The prevalence of smoking in Scotland from the Scottish Health Survey 1995 was 23% in social classes 1 and 2 and 49% in social classes 4 and 5. Although differences in mortality rates according to social status with RA according to socioeconomic deprivation can be explained, in part, by differences in the prevalence of smoking, the observed influence of deprivation on mortality in RA is less readily accounted for by smoking. Functionality is an important outcome measure in RA and is a predictor of mortality in this disease.7

The Scottish Health Survey 1995 showed that there were differences according to social class in other important determinants of health, including diet, alcohol consumption, obesity, hypertension, lung function, fibrinogen levels, general health perception, and psychological well-being. Further research is required to establish the relative importance of these and other factors in determining the influence of socioeconomic deprivation on outcome and mortality in RA and other chronic diseases. The factors which can be modified most effectively to reduce the inequalities in health outcome also require investigation.

If our findings are supported by further studies, the socioeconomic status of populations should influence resource allocation. In addition, these important factors should assist rheumatologists when deciding which patients with RA should receive more intensive, multidisciplinary intervention.

Diagnostic evaluation of classification criteria for RA and reactive arthritis

We read with interest the recent article by Hülsemann et al, in which the 1987 American College of Rheumatology (ACR) classification criteria for rheumatoid arthritis (RA) were evaluated for their ability to identify patients with a clinical diagnosis of RA among 217 patients referred to an early arthritis clinic. The authors concluded that the 1987 ACR criteria can be used to make a diagnosis of RA in this setting. In the study, the “gold standard” against which the criteria were applied as an “expert diagnosis” was made by one of the authors when the patient was first seen (within one year of symptom onset). However, the main difficulty facing the authors was in distinguishing patients with early disease that patients who ultimately develop RA appear clinically similar to those who have self limiting disease or other forms of inflammatory arthritis. It is therefore too early to make an accurate diagnosis at this stage. More importantly, RA is a heterogeneous disease with a prognosis which varies from complete symptom remission to severe disability. Therefore simply categorising patients into those who do and do not have “RA” is not necessarily important when considering which patients require early treatment. Although the authors made a clinical diagnosis without using the classification criteria, they found that the diagnoses were informed by their knowledge of the individual components of the criteria. Therefore the high sensitivity (90%) they reported means that many of the patients with a clinical diagnosis of RA will have had seropositive, erosive, polyarticular disease with hand involvement. Whereas we have no problem in recognising these patients as having RA, it represents only one end of the spectrum. The proportion of patients with “undiagnostic arthritis” in this study is high (54%), though this has been reported in other ACR series.1 It is likely that many of these patients have atypical RA which may still require treatment with disease modifying antirheumatic drugs. Further, in early disease, patients often do not satisfy one of the criteria (nodules, erosions) which are features of established RA. We therefore think it is misleading to imply that patients who do not satisfy the 1987 ACR criteria (a) do not have RA; and (b) do not require early, aggressive treatment.

We recently evaluated the performance of the 1987 ACR criteria in an unselected cohort of 486 patients newly presenting with inflammatory polyarthritis to the Norfolk Arthritis Register. We considered the practical question of whether the criteria could identify which patients would have a poor prognosis after three years as assessed by (a) persistent synovitis; (b) functional disability and (c) radiological erosions. Although we applied the criteria in a number of different ways, we found they had a low ability to discriminate between patients who developed persistent, disabling, and erosive disease and those who did not. For example, applying the criteria in the traditional “list” format, the positive predictive value for erosions was only 45% and the negative predictive value 67%. In practical terms, this means that patients who did not satisfy the criteria developed erosions. However, given the fact that the 1987 ACR criteria were designed to distinguish between hospital attenders with established RA and patients with early, musculoskeletal conditions, and were never intended to be used as diagnostic criteria, it is not surprising that they do not perform well in this setting.

Finally, we wish to point out that the proportion of patients who satisfy the criteria at one year of follow up varied from 28% if applied “cross sectionally” (on the day of assessment) to 61% if applied “cumulatively” (each criterion satisfied if “ever” positive). Further difficulties are likely to be encountered using the criteria as ascertainment from case note review. It is therefore more appropriate in a group with early synovitis to assess the criteria applied longitudinally at follow up rather than simply at baseline. In the study by Hülsemann et al and Zeidler we were given no information about how or when the criteria were applied apart from that they were applied “retrospectively”.

We agree with Hülsemann and Zeidler that there is a need to “…distinguish RA as early as possible from the often benign and self-limited forms of undiagnostic arthritis, as there is a need for early treatment of RA”. However, we strongly disagree with the use of the 1987 ACR criteria. Until we understand more about the pathogenesis of RA, clinicians will have to rely on clinical judgment and the presence of poor prognostic factors to make decisions about whether to treat aggressively patients presenting with early disease.

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early arthritis clinic, that early diagnosis of RA was not the basis of diagnoses. Trained doctors and the patient to perform, as early as possible, a nosological differentiation between RA and non-RA arthritides. Since the ARA criteria had not been revised. Expert practitioners not trained in rheumatology to make a diagnosis of RA and to differentiate between RA and other forms of arthritides as soon as possible in the course of the disease. Thus by early referral to a rheumatologist an adequate treatment can be started as soon as possible. Even rheumatologists, who are familiar with the criteria used in all controlled trials to establish treatment guidelines, are, in our view, well supported in every day practice by applying the 1987 ACR criteria to differentiate RA from other forms of arthritides, enabling early diagnosis and treatment decisions.

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diagnosis. Making a diagnosis of RA and to differentiate between RA and the whole spectrum of other arthritides and spondarthritides. We have described the incidence of undifferentiated arthritides to be as high as 54%. When patients were seen in this early synovitis outpatient clinic between 1984 and 1986, the 1985 American Rheumatism Association (ARA) criteria had not been revised. Expert diagnosed were made with knowledge of the 1987 ARA criteria for the diagnosis of RA but were not the basis of diagnoses. Trained as clinicians, rheumatologists never used the ACR criteria for diagnosis making. Only in retrospect, were the 1987 revised ACR criteria applied. These criteria are for classification of RA. The intention was to investigate the performance of these criteria in early synovitis with a high proportion of undifferentiated and reactive arthritides. Since the performance of good with a high sensitivity (90%) and a high specificity (90%), we suggested, that these criteria could be used not only as classification criteria but also as criteria for prognosis and diagnosis of RA. Criteria should be applied longitudinally at follow up, rather than simply at baseline. We applied criteria cross sectionally on the day of their first visit. We can not present follow up data on the whole group, but of a subgroup of 28 patients with undifferentiated arthritides. Only two of these patients developed rheumatoid factor negative RA, 15 patients showed complete remission, two showed partial remission that had unchained progression of progressive unclassified arthritis, and one patient had developed anklyosing spondylitis.

In accordance with our experience, van der Horst-Bruinsma et al have shown, in a special early arthritis clinic that early diagnosis of RA is possible and reliable. Compared with routine patient care, 74 patients with definite RA according to the 1987 ACR criteria, diagnosed at two weeks after the first visit, 66 still had definite RA after one year, and in only four patients was the diagnosis changed to systemic lupus erythematosus (one), unclassified arthritis (one), gout (one), and probable RA (one). Two patients had died and two were lost to follow up. This shows, that the validity is high for the 1987 ACR criteria for differentiating between RA and non-RA arthritides in an early synovitis clinic.

We do not imply that patients who do not fulfill the 1987 ARA criteria do not have RA. If they do not fulfill the criteria at this early stage, we classify them as having undifferentiated arthritis. This is a working diagnosis, which can be changed to a definite diagnosis during follow up, but is only rarely necessary, as our experienced rheumatologists show. The 1987 ACR criteria are not valid for prognostic purposes as Horst et al stated. Other prognostic factors exist and can easily be applied to patients with RA. But the ACR criteria for RA are also of great importance as means of helping family doctors and general practitioners not trained in rheumatology to differentiate between RA and the whole spectrum of other arthritides and spondarthritides.

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Authors’ reply

We thank Professor Smith for his interesting comments. Professor Smith refers to an “obvious omission...any doctor or patient derived clinical parameters”.

Clearly, we had measured the knee circumference of the target knee in this situation and we were using knee swelling as a clinical parameter; in table 1 of our paper it can be seen that there was no significant change in the knee circumference in any of the treatment or placebo groups during the study. Although we did not show the data in the results section, we stated that there was no statistically significant improvement in the doctor’s assessment of knee synovitis over the study period. Therefore, we do not suggest that there was a marked clinical response to treatment in these patients. We agree that there was a marked disparity in the baseline CRP levels within the three groups, but this was a result of randomisation and therefore something over which we had no control.

As regards the changes in MRI measurements, and the quantitative maps showing the reduction in gadolinium uptake, we believe that the trend towards the dose response across the three groups was clearly the most important interpretation of these results. We do not agree, however, with the reader's interpretation that a possible range of change of 25% is small, especially as the patients had longstanding, resistant disease. The mean duration of disease for these patients was about 12 years and they had undergone multiple treatments with disease modifying antirheumatic drugs.

Professor Smith’s final point about anti-CD14 antibodies, which label macrophages as well as T cells, we clearly discussed in the third paragraph of the discussion—“There are a number of possible explanations for this apparent reduction in the number of CD14+ cells, which may represent a reduction in T cells or macrophages...”

In summary, we believe that this was an important study, firstly, as a proof of concept for the conjugation of peroxidase and the monoclonal antihuman-CD36 antibody (Boehringer Mannheim-Germany) diluted to 3.5 mg/ml and the monoclonal anti-human-CD14 antibody (DAKO-Denmark) diluted 1:10 in TRIS-HCL buffer. In brief, specimens were air dried, fixed with acetone and then stored at −70°C until processing. The specimens were incubated for 60 minutes at room temperature with the primary antibody. For the conjugation of peroxidase an En Vision+TM Kit (Dako) was used. The monolayers were then incubated for five minutes with a prediluted diamino-benzidine solution (Dako) and countercoloured with Mayer’s hematoxylin. All incubation steps were preceded by washes in 0.1 M PBS (five minutes × three). The slides were examined at 400× magnification.

Omission of primary antiserum, use of normal rabbit serum, or one of subsequent steps in the staining method were included as controls for specificity.

Macrophages as well as Reiter cells could be observed on MGG stained slides. Reiter cells were more abundant in synovial fluids from patients with seronegative spondyloarthritides and crystal induced arthritis compared with synovial fluids from RA (table 1).

On immunohistochemistry preparations, no obvious omission...any doctor or patient derived clinical parameters... was noted in the clearance of apoptotic PMN during synovial inflammation. In vitro data have shown that thomboembolins receptor and CD14 are some of the most important adhesion molecules involved in cell clearance. The expression of the thomboembolins receptor turns an amateur phagocyte into a professional one. It has been hypothesised that dysregulation of this receptor and the ensuing impairment of inflammatory cell elimination could play a part in inducing chronicity as well as tissue damage and scarring. Recently, CD14 has been demonstrated to mediate recognition and phagocytosis of apoptotic cells. This interaction depends on a region of CD14 that is supposed to be identical to a region that binds bacterial lipopolysaccharide, triggering the release of proinflammatory cytokines from macrophages. On the other hand, the interaction with self components acts as an initial step leading to apoptotic cell elimination. A major role for CD14 in the uptake of apoptotic neutrophils has been recently hypothesised, but it seems likely that microenvironmental modifications could promote the switch from a CD14 dependent pathway to pathways using other adhesion molecules such as CD14. The removal of inflammatory PMN is mediated by several surface molecules and modulated by microenvironmental modifications; it seems to be a crucial, although only partially understood event for the control and resolution of inflammation. Our results suggest that CD14 and CD36 could be involved in the adhesion of the macrophage to the apoptotic cell, the first step of

LETTERS TO THE EDITOR

CD36 and CD14 immunoactivity of Reiter cells in inflammatory synovial fluids

Reiter cells are macrophages containing ingested polymorph nuclei that are commonly found in most inflammatory synovial fluids. Available data indicate that CD36 and CD14 on human monocyte derived macrophages are adhesion molecules involved in several biological processes. Of interest, their role in the process of adhesion and phagocytosis of apoptotic cells has been recently demonstrated.

Jones and colleagues demonstrated reduced Reiter cells in the synovial fluids from patients with rheumatoid arthritis. This observation is consistent with the hypothesis that Reiter cells play a regulatory part in preventing autoxidation of polymorphonuclear neutrophils (PMN) and thus local tissue damage. The purpose of this study was to evaluate by histochemical technique whether Reiter cells express CD36 and CD14 in inflammatory synovial fluids.

We analysed the synovial fluids obtained from the knee joints of 10 patients suffering from inflammatory joint diseases of recent onset (< 6 weeks). Three patients had seronegative spondyloarthritides (two reactive arthritis, one psoriatic arthritis, one enterorheumatic arthritis) and three patients had crystal induced arthritis (two cases of acute gout and one case of acute pseudogout). Synovial fluids were processed within one hour of aspiration. Two slides were stained with May-Grunwald-Giems (MGG) reagent. Reiter cells were counted on the basis of the first 500 cells encountered on MGG stained slides. In addition, two cytocentrifuged monolayer preparations were processed for immunohistochemistry using the monoclonal anti-human-CD36 antibody (Boehringer Mannheim-Germany) diluted to 3.5 mg/ml and the monoclonal antihuman monocyte CD14 antibody (DAKO-Denmark) diluted 1:10 in TRIS-HCL buffer. In brief, specimens were air dried, fixed with acetone and then stored at −70°C until processing. The specimens were incubated for 60 minutes at room temperature with the primary antibody. For the conjugation of peroxidase an En Vision+TM Kit (Dako) was used. The monolayers were then incubated for five minutes with a prediluted diamino-benzidine solution (Dako) and countercoloured with Mayer’s hematoxylin. All incubation steps were preceded by washes in 0.1 M PBS (five minutes × three). The slides were examined at 400× magnification.

Omission of primary antiserum, use of normal rabbit serum, or one of subsequent steps in the staining method were included as controls for specificity.

Macrophages as well as Reiter cells could be observed on MGG stained slides. Reiter cells were more abundant in synovial fluids from patients with seronegative spondyloarthritides and crystal induced arthritis compared with synovial fluids from RA (table 1).

On immunohistochemistry preparations, no obvious omission...any doctor or patient derived clinical parameters... was noted in the clearance of apoptotic PMN during synovial inflammation. In vitro data have shown that thomboembolins receptor and CD14 are some of the most important adhesion molecules involved in cell clearance. The expression of the thomboembolins receptor turns an amateur phagocyte into a professional one. It has been hypothesised that dysregulation of this receptor and the ensuing impairment of inflammatory cell elimination could play a part in inducing chronicity as well as tissue damage and scarring. Recently, CD14 has been demonstrated to mediate recognition and phagocytosis of apoptotic cells. This interaction depends on a region of CD14 that is supposed to be identical to a region that binds bacterial lipopolysaccharide, triggering the release of proinflammatory cytokines from macrophages. On the other hand, the interaction with self components acts as an initial step leading to apoptotic cell elimination. A major role for CD14 in the uptake of apoptotic neutrophils has been recently hypothesised, but it seems likely that microenvironmental modifications could promote the switch from a CD14 dependent pathway to pathways using other adhesion molecules such as CD14. The removal of inflammatory PMN is mediated by several surface molecules and modulated by microenvironmental modifications; it seems to be a crucial, although only partially understood event for the control and resolution of inflammation. Our results suggest that CD14 and CD36 could be involved in the adhesion of the macrophage to the apoptotic cell, the first step of

**Table 1** The number of Reiter cells calculated on the first 500 cells encountered on May-Grunwald-Giems stained slides

<table>
<thead>
<tr>
<th>Sample</th>
<th>Reiter cells (n)</th>
</tr>
</thead>
<tbody>
<tr>
<td>RA (n=3)</td>
<td>1 1</td>
</tr>
<tr>
<td>2 1</td>
<td></td>
</tr>
<tr>
<td>3 3</td>
<td></td>
</tr>
<tr>
<td>SsA (n=4)</td>
<td>1 3</td>
</tr>
<tr>
<td>2 2</td>
<td></td>
</tr>
<tr>
<td>3 5</td>
<td></td>
</tr>
<tr>
<td>4 3</td>
<td></td>
</tr>
<tr>
<td>CIA (n=3)</td>
<td>1 4</td>
</tr>
<tr>
<td>2 3</td>
<td></td>
</tr>
<tr>
<td>3 3</td>
<td></td>
</tr>
</tbody>
</table>

RA: rheumatoid arthritis, SsA: seronegative spondyloarthritides, CIA: crystal induced arthritis.
We thank Dr Nicolò Pipitone for reviewing the manuscript, and Ms Eleonora Franceschini for technical assistance.

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8 Fadok VA, Warner ML, Bratton DL, Henson PM. CD36 is required for phagocytosis of apoptotic cells by human macrophages that use either a phosphatidylserine receptor or the vitronectin receptor or the vitronectin receptor (alpha v beta 3). J Immunol 1998;161:6250–7.

Table 1 Questionnaire on lower extremity complaints

| A | Have you ever had foot or leg pain events after prolonged standing and/or bus travel lasted more than six hours? If the answer is yes, |
| B | Has it been existed since childhood or adolescence? Does it occur mostly during episodes of prolonged standing or sitting? Does it occur mostly bilateral? Does it persist at least 30 minutes after rest? If all of the answers are yes, then the case was considered to be positive. |

Non-periodic leg pain in patients with familial Mediterranean fever

Familial Mediterranean fever (FMF) is characterised by recurrent bouts of fever, peritonitis, pleuritis, arthritis or erysipelas-like skin disease. Between the episodes, FMF patients are free of symptoms and appear healthy. However, interestingly we observe leg complaints after prolonged standing or sitting, or both, in FMF patients, who usually experience these painful manifestations during evenings or after long distance bus trips. Thus we conducted a questionnaire study on 40 FMF patients (age, mean (SD): 21.2 (1.8) years; F: M: 2: 38) and 180 healthy male subjects (age, 21.3 (0.2) years) to ascertain the frequency of these complaints, and some of FMF patients were also included in a test to provoke these symptoms. Table 1 shows the questionnaire. Positive cases were also questioned for the presence of swelling or redness during these periods, and whether these complaints followed by an episode. Although 14 of the 180 healthy subjects responded positively to the first question (question A), none of them were considered to be positive after further questions (questions B). All FMF patients reported foot or leg pain after prolonged standing periods (first part of question A). They described, that at the onset, the pain was merely confined to mid-foot, however other sites (e.g., ankles, the calves, the knees or even the thighs) were involved in an additive manner as the intensity of pain increased unless resting ensued. Thirty-five FMF patients have experienced foot pain (with or without subcutaneous swelling) during or after long distance bus travelling and they also described an area of redness, which typically located on the swollen region on those occasions. Thirty-five patients defined a period of fatigue accompanied a low grade fever subsequent to the episodes with severe lower extremity symptoms.

In a provocative test, 30 volunteer male FMF patients (age, 21.2 (1.8)) without proteinuria and 30 volunteer male healthy subjects (age, 21.1 (0.8)) were kept in an upright position (standing, walking or doing light sitting) for six hours. At the beginning, all participants were symptom free and none of them had any other disorder that may cause foot pain. Thirteen FMF patients were receiving colchicine treatment. Bilateral ankle and the knee circumference were measured from the marked points at the onset and the termination of the test. The mean change in circumference per measurement site (mean (SD)) was 3.0 (2.7) mm and 1.3 (1.5) mm in the patient and the control group, respectively. Although the comparison was statistically significant (p=0.014; Mann Whitney U), we think that our method was not reliable to detect those small changes precisely.

At the end of the provocative test, none of the healthy controls had lower extremity pain or tenderness. Apart for one patient, all FMF patients had intense foot or calf pain, which interfered with walking. Tenderness was so profound that it could be elicited even by a gentle touch. Widespread tenderness was detected in 12, whereas localised tenderness was detected in 17 of the patients. Although swelling was not noticed in anyone, focal erythematosus areas (not erysipelas) were seen in five patients. After five hours of resting, palpation showed that tenderness was sustained (14 widespread and 16 localised). A localised pain and tenderness was also developed in the symptom free patient. Colchicine use did not change the results of provocative test (p=0.240; Fisher’s test).

Although leg pain induced by exercise or prolonged standing has already been discussed in FMF patients, we are unaware of any report about leg pain and swelling episodes after prolonged sitting in these patients. Increased hydrostatic pressure in the lower extremities may be the main factor responsible for those symptoms experienced during bus trips.

It was suggested that FMF is related to catecholamine metabolism as metaraminol infusion may provoke an acute episode, and episodes may be prevented by prazosin hydrochloride, as reported recently. Leucoocytes may need adequate perfusion (driving) pressure to pass through capillaries in microcirculation. These findings raise the possibility that catecholamines may increase the hydrostatic pressure of capillary bed, which may be an inciting factor for episodes. Our findings show that an inflammatory process involving lower extremities occurs after prolonged standing and sitting periods in FMF patients. We think that genetically low level of inhibitory activity (that is, mutated pyrin) may not be able to compenate the inflammatory reaction that is probably initiated in a stressful microenvironment caused by not only microtrauma, but also increased hydrostatic pressure.

I am greatly indebted to Professor Hasan Yazici for constructive criticism and help in preparation.

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Subclinical gut inflammation in spondyloarthropathy patients is associated with upregulation of the E-cadherin/catenin complex (P Demetter et al Ann Rheum Dis 2000;59:211–16). We regret that the references in this article are incorrectly numbered. Owing to the splitting of reference 7, references numbered from 9 onwards in the text are listed as 10 onwards in the reference list.