Soluble TNF receptor treatment does not affect raised TGFβ levels in RA

We read with interest the report by Drynda et al demonstrating that treatment of rheumatoid arthritis (RA) with anti-tumour necrosis factor α (anti-TNFα) induces subtle changes in the cytokine network such as down regulation of proinflammatory cytokines including TNF, IL10, and TGFβ were found to be down regulated. Therefore, pathway signalling of these cytokines including TGFβ may be impaired if their receptors are down regulated.

Altered expression of these genes' function, alone or in combination, may have an impact on the predisposition to infection and tumour defence. Such is the case for the induced TNFα inhibitor down regulation in the expression of C9, B and T cell functions, signaling cascade (J Jun B), adhesion molecules, heat shock proteins, and antigen presentation, and the predisposition to infection. Likewise, TNF antagonists also regulate the expression of oncogenes, such as Jun B, c-myc, fos and ras, which may have an impact on tumour defence.

Therefore, our study with DNA microarray confirms and expands the immunomodulatory functions of TNF antagonists. Data, however, seem to suggest that the increased predisposition to development and altered tumour defence may not be related to increased plasma levels of TGFβ because its expression are regulated, but rather to dysregulation of gene expression of other molecules induced by the, TNF antagonists.1,2

R Cuchacovich, L R Espinoza
Section of Rheumatology, Department of Medicine, LSU Health Sciences Center, 1542 Tulane Avenue, New Orleans, LA 70112-2822, USA

Correspondence to: Professor L R Espinoza; jbascz@lsuhsc.edu

References

Authors’ reply
We read with interest the letter by Cuchacovich and Espinoza commenting on our previous paper,1 which, based on results of DNA microarrays showing that increased plasma levels of transforming growth factor β (TGFβ) persist in the course of anti-tumour necrosis factor α (anti-TNFα) treatment in rheumatoid arthritis (RA), suggests that patients may not have an altered tumour defence.

Complex effects of TNFβ on tumour development and progression as well as cancer metastasis have been demonstrated in numerous studies.2,3 As a result of these studies, raised levels of TGFβ seen in patients with RA are thought to contribute to an altered tumour defence.

In our own additional experiments we monitored changes in the expression profiles of mononuclear cells from peripheral blood in the course of anti-TNF treatment in RA in 10 patients using the same human genome U95a Affymetrix chip. By applying a different experimental setting than Cuchacovich et al.,4 different results were found. Only a small number of genes were found to be regulated in five or more of the 10 patients in either direction after anti-TNF treatment compared with baseline. Among these genes were proinflammatory cytokines, chemokines, apoptosis related proteins, and proteins involved in the cell cycle. Interestingly, different regulation patterns were found in our patients.5 In contrast to Cuchacovich et al.,6 no down regulation was found in receptors for interferon γ, interleukin 10, or in either TGFβ receptors (TGFβRI and TGFβRII) within the first six days of anti-TNF treatment. Expression of oncogenes Jun B, c-myc, ras, and fos remained unchanged as well.

Finally, it should be mentioned that neither mRNA levels nor plasma concentrations of TGFβ completely reflect the real situation in vivo because the biological activity of TGFβ is tightly regulated post-translationally. This includes the proteolytic cleavage of active TGFβ, the down regulation of the active ligand-receptor complex, and the downstream signalling via Smads.7

Further research is mandatory to explain the multiple effects of TGFβ and its role in the complex network of cytokines. Recently developed techniques such as DNA microarrays may help to understand the interactions and regulation of proteins and their biological activity.

S Drynda, J Kekow
Clinic of Rheumatology, University of Magdeburg, Germany

D Koczan, H-J Thiesen
Institute of Immunology, University Rostock, Germany

Correspondence to: Professor J Kekow, Clinic of Rheumatology, University of Magdeburg, D-39245 Vogelsang/Magdeburg, Germany; joern.kekow@medizin.uni-magdeburg.de

References

Usefulness of bone densitometry in postmenopausal women with clinically diagnosed vertebral fractures

We read with interest the article by Nolla et al, which demonstrates that only 3% of women with asymptomatic non-vertebral fractures have normal bone mineral density (BMD). We agree with their conclusions that in this clinical setting measurement of BMD is not required to confirm a diagnosis of osteoporosis before starting treatment.
A large number of studies have shown that a previous history of vertebral fracture increases the risk of future vertebral and non-vertebral fracture, independently of BMD. Vertebral fractures are also associated with significant morbidity, leading to an impaired quality of life and increased mortality. A recent study by Lindsay et al demonstrated the speed of disease progression in osteoporosis, with 20% of patients experiencing a new incident vertebral fracture within 12 months after a vertebral fracture. These data suggest that osteoporosis treatment should be started as soon as possible after a fracture has been diagnosed, as any delay in initiating treatment while waiting for bone densitometry may put the patient at risk of further fractures. The availability of dual energy absorptiometry (DXA) is poor in the United Kingdom in comparison with some other European countries. The Advisory Group Report on Osteoporosis noted that in the UK there were 1.6 DXA machines per million population, compared with 2.9 in the USA and 6.6 in France. The limitation of DXA machine provision in the UK compared with the clinical demand has led to long waiting lists for BMD measurements and a potential delay in starting osteoporosis treatment.

Under these circumstances, what is the evidence that patients can be treated solely on the basis of vertebral fracture without the need for BMD measurement? The majority of studies have evaluated drug treatment in patients with low BMD alone, or with low BMD and prevalent vertebral fracture. Studies of risedronate, alendronate, and parathyroid hormone have, however, included patients with two or more asymptomatic vertebral fractures in the absence of BMD readings.

In the study by Harris et al of 80% of patients had two or more vertebral fractures, and analysis of this subgroup showed that patients treated with risedronate had a 43% reduction in new vertebral fractures at three years compared with those receiving placebo. A further study of risedronate recruited patients solely on the basis of vertebral fracture history (>2) irrespective of BMD and demonstrated that active treatment reduced the risk of new vertebral fractures by 49% and of non-vertebral fractures by 33% over three years compared with placebo.

Studies of raloxifene and parathyroid hormone also included patients without a vertebral fracture history alone. Although the results of these studies showed an overall reduction in fracture risk, subgroup analysis of the patients with two or more vertebral fractures and no BMD measurement was not performed. It is therefore not possible to determine accurately the effect of treatment in this group.

We feel that the evidence suggests that patients presenting with two or more non-traumatic vertebral fractures should be considered for treatment of osteoporosis without the need for measurement of BMD, after a metabolic or secondary cause of fracture has been excluded. This is reflected in some of the recent guidelines for the management of osteoporosis.

**References**


**Author’s reply**

We thank Dr Moss and Dr Keen for their interest in our article and for their comments, we read with great interest the leader by Gilchrist et al. We feel that the evidence suggests that patients can be treated solely on the basis of vertebral fracture history (>2) irrespective of BMD and demonstrated that active treatment reduced the risk of new vertebral fractures by 49% and of non-vertebral fractures by 33% over three years compared with placebo.

In an increased risk of infection with *S. pneumoniae*, *N. meningitidis*, and *H. influenzae* type B is also seen after surgical splenectomy. Such patients should receive lifelong prophylaxis with penicillin V and immunisation with pneumococcal polysaccharide vaccine. We have previously recommended that patients with SLE and chronic hypocomplementaemia should also receive similar prophylaxis and wish to reiterate the importance of these measures in order to prevent life threatening infection in this disease.
In young age, renal disease, and persistent infections, Mitchell and colleagues suggested in infections, the evidence to support prophylaxis for all doctors caring for patients with SLE and neisserial infections is received.

It is important to recognize cohorts of patients with systemic lupus erythematosus (SLE) who also have hypocomplementaemia, but this should be further investigated with more definitive studies. For now, the best approach for doctors caring for patients with SLE is to immunize them against pneumococcal vaccinations, consider antibiotic prophylaxis in certain situations, and maintain a high degree of awareness for the diagnosis of bacteria and other pathogens, especially those that are prevalent in the community in which you care for the patients.

W R Gilliland, G C Tsokos
USUHS/WRAIR, 503 Robert Grant Road, Bldg 503, Rm 1A32, Silver Spring, MD 20910-7500, USA

Correspondence to: Dr G C Tsokos; gtokos@usuhs.mil

References

Authors’ reply
We appreciate the opportunity to respond to comments generated by our leader entitled “Prophylactic use of antibiotics and immunisations in patients with systemic lupus erythematosus”. Hepburn and Davies address several important issues about the prophylactic use of antibiotics in treating patients with systemic lupus erythematosus (SLE) who also have hypocomplementaemia or functional asplenia, or both. They suggest that the increased incidence of infections with encapsulated organisms in patients with SLE is related to defective clearance secondary to functional asplenia, immunosuppressive treatment, and defective opsonisation. All these potential explanations seem plausible, but it is important to note that not all patients with SLE and neisserial infections are receiving immunosuppressive agents at the time of their infection.

As emphasised in our article, we agree that it is important to recognise cohorts of patients who are at risk of developing certain infections. However, in the case of neisserial infections, the evidence to support prophylactic antibiotics for patients with SLE and hypocomplementaemia is not clear. In a small case series of patients with SLE and neisserial infections, Mitchell and colleagues suggested the following possible risk factors: female sex, young age, renal disease, and persistent hypocomplementaemia. Although it is clear that in children with haemoglobinopathies and splenic dysfunction who receive oral penicillin prophylaxis, pneumococcal bacteremia is reduced dramatically, little information supports the use of this strategy in asplenic adults.

In summary, optimal strategies to decrease the incidence of infections should remain a priority for all doctors caring for patients with SLE. However, in those who are asplenic, we reiterate the importance of vaccinations against pneumococcal and Haemophilus influenzae type B. Currently, no data support the role of prophylactic penicillin or other antibiotics in patients with SLE who are asplenic or have persistent hypocomplementaemia, but this should be further investigated with more definitive studies. For now, the best approach for doctors caring for patients with SLE is to immunize them against pneumococcal vaccinations, consider antibiotic prophylaxis in certain situations, and maintain a high degree of awareness for the diagnosis of bacteria and other pathogens, especially those that are prevalent in the community in which you care for the patients.

W R Gilliland, G C Tsokos
USUHS/WRAIR, 503 Robert Grant Road, Bldg 503, Rm 1A32, Silver Spring, MD 20910-7500, USA

Correspondence to: Dr G C Tsokos; gtokos@usuhs.mil

References

Was it a case of Takayasu arteritis?

Recently, the case of a 9 year old boy presenting with cardiac failure was presented in the Annals of the Rheumatic Diseases. It was reported as a case of Takayasu’s arteritis in a child with a CD4+ lymphopenia and dysgammaglobulinaemia. I have a number of problems with this case:

• As presented in table 1 in the letter, this 9 year old child has a normal CD4 cell count with a low total lymphocyte count. Is the table wrong or did this child actually have a normal CD4 lymphocyte count?
• The dysgammaglobulinaemia actually consisted of a modest rise in the IgG level, with a normal IgA, and a borderline low IgM level of rather questionable relevance in such a sick young child.
• The evidence for Takayasu’s arteritis is rather circumstantial, based entirely on magnetic resonance imaging with some suggestive clinical findings in a very sick child presenting with cardiac failure. Surely in such a case, especially when the end result was death soon after initiating immunosuppressive treatment, attempts should have been made to secure a pathological diagnosis, either before or after the final outcome. No mention of this was made in the report.

I remain unconvinced that this was a case of Takayasu’s arteritis and there is no evidence presented to suggest that this child did have a CD4+ lymphopenia.

M D Smith
Flinders University of South Australia

Correspondence to: Dr M D Smith, Rheumatology Research Unit, Repatriation General Hospital, Daw Road, Daw Park, South Australia 5041, Australia; Malcolm.smith@rgh.sa.gov.au

Author’s reply
We thank Dr Smith for his comments and would like to reply to the points he made.

Firstly, we agree that the absolute CD4 number was not correct in the table. It was incorrectly converted in the editorial process from the value/mm³ and should have been 0.2×10⁹/l rather than 2×10⁹/l. We regret that this point was overlooked on the proofs.

Secondly, a polyclonal hypergammaglobulinaemia is present in one third of cases with Takayasu arteritis. The serum immunoglobulin levels of our patient are consistent with Takayasu arteritis. Dr Smith mentioned a modest rise in the IgG level, with a normal IgA level, but our patient had high levels of both IgG and IgA.

Finally, the classification criteria for Takayasu arteritis according to the American College of Rheumatology (ACR) are: (a) age at disease onset in years <40; (b) classification of the arms and legs; (c) decreased brachial artery pulse; (d) blood pressure difference >10 mm Hg, (e) bruit over subclavian arteries or aorta; (f) arteriogram abnormality. Our patients had all six of these criteria. In addition to the ACR criteria, our patient had one obligatory, one major, and five minor criteria for the clinical diagnosis of Takayasu’s disease according to Ishikawa’s criteria. These criteria comprise one obligatory criterion, two major criteria, and nine minor criteria. In addition to the obligatory criterion, one major and two or more minor criteria suggest a high probability of the presence of Takayasu’s disease.

These data prove that there is no reason to doubt the diagnosis of this case as Takayasu arteritis. Additionally, the patient had a low CD4 count associated with hypergammaglobulinaemia.

S S Kiliç
Department of Paediatrics, Immunology Division, Uludag University Medical Faculty, Gürkile Bursa 16059, Turkey

Correspondence to: Dr S S Kiliç; sebnemkili@uludag.edu.tr

Reference

www.annrheumdis.com
Usefulness of bone densitometry in postmenopausal women with clinically diagnosed vertebral fractures

K Moss and R Keen

doi: 10.1136/ard.61.7.667-a