"fool" the patient. Govind *et al* seem to have already decided that this is not possible, a convenient assumption.

Further, we are concerned that Govind *et al* state categorically that "among patients with whiplash injuries, third occipital headache is common". The study group from which they determine this prevalence has been reviewed elsewhere, and is wholly inappropriate for a prevalence estimate, being best described as an unusual, highly select, and heterogeneous group of subjects.[3]

It is of note that, in regard to validated therapies for whiplash patients, the current study would have been rejected by the criteria of the Quebec Task Force on Whiplash Associated Disorders.[4] We suggest that an invasive procedure should not be advocated until it has been subjected to proper study. Fortunately, we are aware that others are undertaking a properly controlled trial of this form of therapy.

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References

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2 **Lord SM**, Barnsley L, Wallis BJ, *et al*. Percutaneous radio-frequency neurotomy for chronic cervical zygapophysial-joint pain. *N Engl J Med* 1996;**335**:1721–6.
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4 **Spitzer WO**, Skovron ML, Salmi LR, *et al*. Scientific Monograph of the Quebec Task Force on Whiplash Associated Disorders: redefining "whiplash" and its management. *Spine* 1995;**20**(suppl 8):1S–73S.

Authors' reply

Our study reported an audit of outcomes for a treatment of a condition for which there is no other treatment available. It showed what proportion of patients obtained complete relief of pain, and for how long. Readers who wish to adopt this treatment for their patients can do so. If not, they should explain to their patients that they, personally, cannot offer them any treatment that is known to work; but they should not claim that there is no treatment. Our study shows that there is an option.

A placebo controlled trial would not prove that this treatment does not work. The outcomes should be the same as the benchmark established by our study, unless the operators perform the procedure poorly. A placebo controlled study could only show that all or part of the outcome is attributable to non-specific effects.

We consider this to be an unlikely outcome for we have never encountered in any of our own studies, nor in the literature, results showing that 86% of patients obtain complete relief of spinal pain following a sham procedure. Radiofrequency neurotomy has been shown to be associated with placebo responses in only a small proportion of patients, and for a limited duration.[1] They claim that responses to third occipital neurotomy is only a conjecture. In principle it is worthy of testing, but in practice it cannot be tested.

The precepts of informed consent require that participants in a randomised controlled be informed of all the consequences and potential complications of a procedure. Numbness in the territory of the third occipital is an unavoidable side effect of third occipital neurotomy. It is a sign that the target nerve has been coagulated. It is an essential requirement for the procedure to work. The numbness lasts as long as the pain relief lasts. In a double blind trial this side effect cannot be masked. Therefore, patients who underwent a sham procedure would automatically know that they did not have the real treatment. Thereby the patients would be unblinded. Any placebo controlled trial which suffered unblinding would be fatally flawed and, therefore, unacceptable.

Any study that used a control short of a sham procedure would also be flawed, and would not escape criticism. Pundits would argue that patients would recognise that simply blocking the nerve, or simply inserting the electrode without mimicking the two hour procedure assiduously, is an obvious sham, and that any patient so treated would exhibit a nocebo effect.

For these reasons we did not venture to conduct a placebo controlled trial. If Dr Kwan and Dr Friel can show that a sham procedure on the third occipital nerve succeeds in achieving complete relief of pain in 86% of their patients we will gladly convert to their sham procedure.

We recognise it as a pity that our study would not be accepted by systematic reviews; but that is a problem for those who rely on reviews as the only source of evidence. In that regard we stand in good company. Were we to rely only on systematic reviews, radiofrequency neurotomy for trigeminal neuralgia would not be an accepted treatment; nor would we be allowed to perform appendicectomies.

While others are satisfied to deny care to patients while they engage in purist debates about levels of evidence, we are rewarded with patients grateful for the relief that they obtain, and who report: "you must repeat the procedure because I am never going back to suffering headaches again". If someone devises a better treatment for third occipital headache, we will adopt it. In the meantime we feel it would be dishonest of us to tell our patients there is nothing we can do for you.

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1 **Lord SM**, Barnsley L, Wallis BM, *et al*. Percutaneous radio-frequency neurotomy for chronic cervical zygapophysial joint pain. *N Engl J Med* 1998;**335**:1721–6.

CORRECTIONS

In the neurological picture of the June issue (Komotar JR, Clatterbuck RE. Coccidiomycosis of the brain, mimicking en plaque meningioma. *J Neurol Neurosurg Psychiatry* 2003;**74**:806) the initials of the first author were reversed; his name should read as Komotar RJ.

The ordering of the authors in the letter by Soragna D, Tupler R, Ratti *et al* in the June issue (An Italian family affected by Nasu-Hakola disease with a novel genetic mutation in the TREM2 gene. *J Neurol Neurosurg Psychiatry* 2003;**74**:825–6) is incorrect, it should be as follows: D Soragna, L Papi, MT Ratti, R Sestini, R Tupler, L Montalbetti.

The ordering of the authors in the letter by De Tiège, Laureys, Goldman, *et al* in the July issue (Regional cerebral glucose metabolism in akinetic catatonia and after remission. *J Neurol Neurosurg Psychiatry* 2003;**74**:1003–4) is incorrect, it should read as follows: X De Tiège, JC Bier, I Massat, S Laureys, F Lotstra, J Berré, J Mendlewicz, S Goldman.

In the June issue of JNNP fig 1 of the paper by Cagli S, Oktar N, Dalbasti T, *et al* (Failure to detect *Chlamydia pneumoniae* DNA in cerebral aneurysmal sac tissue with two different polymerase chain reaction methods. *J Neurol Neurosurg Psychiatry* 2003;**74**:756–9) was incorrect. The following figure is the correct image that should have been published.

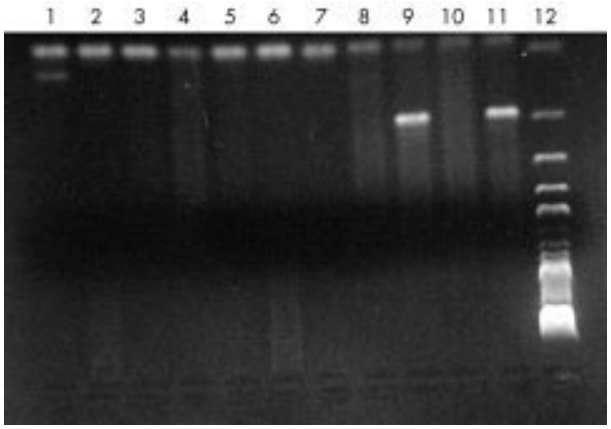

Figure 1 *C pneumoniae* TETR PCR of clinical samples. Lanes 1 to 3, 5 to 7 clinical samples. Lanes 4 and 8 negative control (water). Lanes 9 and 11 positive control (C pneumoniae 4×10^{-1} and 4×10^{-2} CFU). Lane 10 water. Lane 12 DNA molecular weight marker (XIV; 100 bp ladder, Roche Diagnostics). (Correction to *J Neuro Neurosurg Psychiatry* 2003;**74**:756–9.)