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Letters to the Editor

Re: Pathogenesis of post-traumatic ankylosis of the temporomandibular joint: a critical review

Sir,

We read with interest the paper by Arakeri et al.[1] on the pathogenesis of post-traumatic ankylosis of the temporomandibular joint. In this critical review the authors outlined the management and recognised the difficulties in the management of patients with post-traumatic ankylosis.

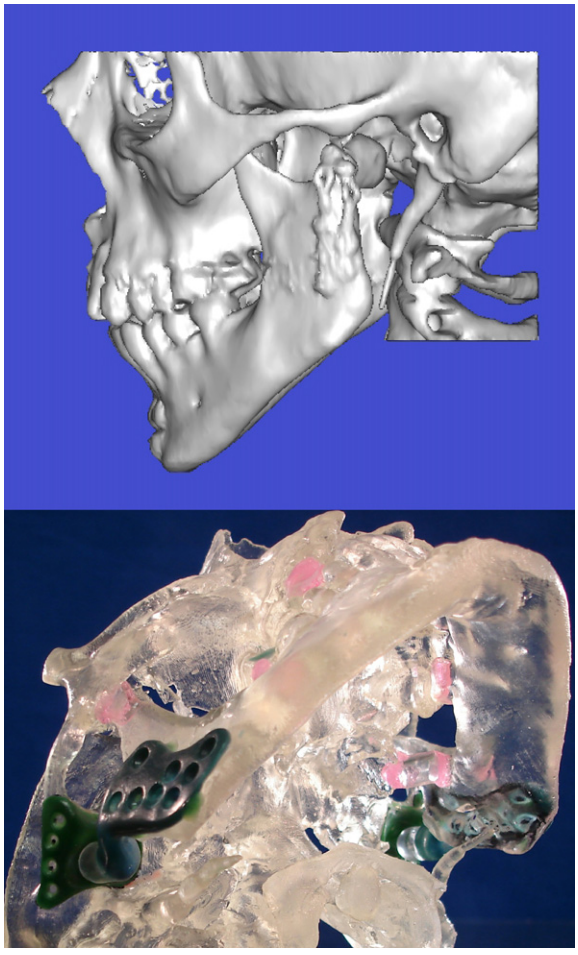

Fig. 1. Ankylosis and a model of a custom TMJ implant as used by the authors.

A variety of techniques have been described[1] including gap and interpositional arthroplasty, osteotomy and excision of the ankylotic mass within the TMJ. As far as the reconstruction is concerned again a variety of techniques that utilise bone or alloplastic materials have been described[1] with variable outcomes. In our experience post-traumatic ankylosis is a challenging problem to correct. We recently examined[2] all our patients with ankylosis and have reported our outcomes.[2] Pain was seen to decrease over time and maximal mouth opening improved for females, males and the overall group only over the entirety of the study period. We do advocate a two stage technique with excision of the ankylotic material and with a second stage reconstruction with Custom joints that allows for changes in antero-posterior and vertical dimensions enabling changes in the occlusion to be made (Fig. 1). It is clear from the paper from Arakeri et al. that in the literature there are several studies that advocate a different and maybe equally successful management of such patients. The patient numbers are small for higher level studies and in the era of evidence based medicine it is time to think about a multicenter randomised trial that if well design will give us answer that can only be of benefit to our patients.

Conflict of interest

The authors have no conflict of interest to declare.

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A.N. Kanatas *
S.F. Worrall
Oral and Maxillofacial Department, Bradford Teaching Hospitals, Bradford Royal Infirmary, Leeds BD9 6RJ, United Kingdom
* Corresponding author. Tel.: +44 07769946105.

E-mail addresses: a.kanatas@doctors.org.uk (A.N. Kanatas), sworrall@doctors.org.uk (S.F. Worrall)
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Diclofenac sodium intramuscularly, or paracetamol intravenously? Question not answered

Sir,

Öncül et al.[1] referred to Precious et al.[2] as stating that a 30% difference in visual analogue scores (VAS) "would be considered as clinically important", and continues with "so 15 (patients) in each group were needed to ensure a type 1 error of 0.05 and type 2 error of 0.20". Precious et al.[2] presented no data that allowed a specific sample size of 15 to be inferred when comparing 2 active analgesic drugs. They categorised mean pain scores (on a 10 cm VAS) of <3 cm as comfort days and ≥3 cm as discomfort days.

"Clinical importance" is based on either observed changes in a population sample or on an external (often subjective) criterion. A 30% reduction in pain has been proposed to indicate clinically important improvement, while ≥70% reduction indicates "much improvement", and less than 30% reduction is "minimal". Öncül et al.[1] present no data regarding calculation of a sample size to clarify the power of the trial.

Trials must show that the drug treatment actually reduces pain and is not a consequence of the natural transitory nature of acute postoperative pain. It implies that there is a reduction in pain after treatment compared with before, and that this effect is different from a placebo effect. To estimate the sample size necessary to identify an effect between 2 active analgesics you need to have a valid estimate or population sample of the actual degrees of pain recorded before and after treatment with an active control drug and preferably a placebo control group as well, in addition to the factors that define the term "clinically important". In the paper by Öncül et al. the test drugs were given during general anaesthesia, which did not allow any comparison between degrees of pain before and after treatment.

Diclofenac sodium given intramuscularly, a comparator drug, was used as the rescue drug on demand after a single dose of the test drug had been given. A survival plot showing the actual times that the rescue medication was given over the whole observation period would add relevant information.

Postoperative facial oedema is one of the longer-lasting postoperative inflammatory symptoms in a large number of patients. The choice of method used to assess oedema may cause bias. An assessment by an observer is neither accurate nor reliable. Conversely, self-assessment by the patient is a sensitive method that can be correlated with objective extraoral measurement.[3]

Clinicians too often jump to conclusions about the efficacy of drugs based on underpowered trials. Trials must be optimally designed to provide robust data that are useful in making clinical decisions.

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Lasse A. Skoglund [a,b,*]
[a] *Section of Dental Pharmacology and Pharmacotherapy, Institute of Clinical Dentistry, Faculty of Dentistry, University of Oslo, POB 1119 Blindern, N-0317 Oslo, Norway*
[b] *Department of Maxillofacial Surgery and Hospital Odontology, Oslo University Hospital (Ullevål), N-0407 Oslo, Norway*
Ellen C. Vigen [c]
Section of Dental Pharmacology and Pharmacotherapy, Institute of Clinical Dentistry, Faculty of Dentistry, University of Oslo, POB 1119 Blindern, N-0317 Oslo, Norway
Per Skjelbred [d]
Department of Maxillofacial Surgery and Hospital Odontology, Oslo University Hospital (Ullevål), N-0407 Oslo, Norway
[*] Corresponding author at: Section of Dental Pharmacology and Pharmacotherapy, Institute of Clinical Dentistry, Faculty of Dentistry, University of Oslo, POB 1119 Blindern, N-0317 Oslo, Norway. Tel.: +47 228 44 672.
E-mail addresses: lasses@odont.uio.no (L.A. Skoglund), ellencv@odont.uio.no (E.C. Vigen), per.skjelbred@ulleval.no (P. Skjelbred)
[c] Tel.: + 47 22 844 671.
[d] Tel.: + 47 22 118 484.

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