Send your letters to the editor, British Dental Journal, 64 Wimpole Street, London W1G 8YS or by email to bdj@bda.org
Priority will be given to letters less than 500 words long. Letters should be typed. Authors must sign the letter, which may be edited for reasons of space

Conscious sedation

Sir, I have carefully read the original article by Dr Kupietzki, (*BDJ* 2004, **196**: 133) a respected clinician and teacher, and the subsequent correspondence.

I feel that conscious sedation is of little if any value in the treatment of uncooperative or preoperative children since it relies upon the ability of the practitioner to communicate with the child.

Its main value is to assist the child who wishes to have treatment but who is unable to overcome his/her anxiety.

The trained, experienced paediatric dentist will generally be able to overcome such anxieties without recourse to sedation. That is not to say that it has no place in the armamentarium of the less experienced practitioner.

B. Scheer
London

*One of the authors of the subsequent articles on this paper (BDJ 2004, **196**: 138), G. Manley responds: B. Scheer's comments on the use of conscious sedation are curious. He informs us that conscious sedation relies on the ability of the practitioner to communicate with the child, and therefore is of little value in some cases.*

He also informs us that the trained experienced paediatric dentist will be able to overcome children's anxieties 'without recourse to sedation'.

Presumably such a practitioner would need to communicate with the child as part of their strategy in order to achieve success.

Even in the event of providing treatment under general anaesthesia communication with the child is a necessary and essential part of care.

The use of inhalation conscious sedation does of course rely on a degree of compliance from the child and is extremely valuable in the management of the anxious child and also the uncooperative child. It is essential that the technique is carried out with good behaviour management, and to achieve

success in the treatment of the uncooperative or preoperative child, its use may require time, patience, additional expertise and skill.

I feel confident that there are many practitioners in the UK who have had success in treating anxious and or uncooperative children using conscious sedation techniques.

I would also suggest that many might hold the view that the use of effective conscious sedation requires training and experience additional to behaviour management rather than being a prop for the inexperienced.

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Cause for concern

Sir, the letter from Longman *et al* published in the *BDJ* is of concern in itself.¹

The correspondents have failed to read with sufficient care the guidelines published on www.rcseng.ac.uk which form part of a larger document providing recommendations for the prevention, diagnosis and treatment of infective endocarditis (IE) – soon to be published on The British Cardiac Society's website (www.bcs.com).²

Furthermore, they have not taken account of The European Society of Cardiology (ESC) guidelines published in 2003 on www.escardio.org/knowledge/guidelines/Guidelines_Infective_Endocarditis.htm.³

Cardiologists and cardiac surgeons are acutely aware of the devastating consequences that IE may have on those patients with underlying cardiac abnormalities, e.g. congenital heart defects, mitral valve prolapse/regurgitation, prosthetic heart valves, which put them at increased risk of developing the condition if significant bacteraemia is induced by dental, interventional or surgical procedures.

The Advisory Group of the British Cardiac Society Clinical Practice Committee and Royal College of Physicians Clinical Effectiveness and Evaluation Unit,² the European Society of

Cardiology³ and the American Heart Association (AHA)⁴ all consistently recognise moderate/high risk cardiac patients which include not only those mentioned above but those patients with acquired valvular heart disease and those with a previous history of IE.

These three advisory groups advocate antibiotic prophylaxis for such 'at-risk' patients undergoing dental and oral procedures likely to cause bacteraemia and it is misleading to say that the BCS/RCP guidance document has greatly increased this list.

A further issue is the apparent lack of association between dental visits and IE. The emerging view that background or 'everyday' bacteraemia may be a significant cause of IE is insufficiently robust to justify abandoning antibiotic prophylaxis in 'at risk' patients, at least at present.

Absence of evidence is not evidence of absence. There is still a need to proceed with caution and provide antibiotic prophylaxis where appropriate.

However, dental practitioners in various types of practice around the UK have raised questions as to which particular dental procedures warrant antibiotic prophylaxis. We felt that it may be useful to try and offer more clear and specific advice than that previously offered by the ESC and AHA.

The new recommendations on antibiotic prophylaxis for dental procedures were therefore assembled after reviewing the literature on the incidence of bacteraemia after various dental procedures.

The claim that the recommendations '...Differ significantly from previous international and national guidance in that they increase the necessity to prescribe antibiotic prophylaxis' is wrong.

The ESC guidelines recommend antibiotic prophylaxis for all dental procedures, 'the only exception could be procedures without any risk of gingival or mucosal trauma and subsequent bleeding'.

This is interpreted as any procedure that causes mucosal trauma and bleeding requires antibiotic prophylaxis. If this guideline were followed it would increase enormously the number of patients requiring prophylaxis.

This is because many oral/dental procedures cause mucosal trauma and bleeding, including rubber dam placement, matrix band and wedge placement, periodontal probing and placement of gingival retraction cord.

The BCS/RCP recommendations limit dental prophylaxis to those procedures that have been shown to produce significant bacteraemia with or without discernible bleeding – fewer patients than when applying the ESC guidelines.

The final version of the BCS/RCP guidance document recommends intravenous (IV) antibiotic prophylaxis for those patients with a previous history of IE, since the consequences of further IE usually has grave implications. Other high risk patients are recommended oral amoxicillin.

The RCS website document (www.rcseng.ac.uk) will be updated accordingly. Other groups requiring IV prophylaxis include some patients undergoing general anaesthesia or who are unable to take oral medication.

In our view, the use of IV antibiotic prophylaxis is not a barrier to patient care by dentists with high standards of clinical practice. Generally speaking, it will be required infrequently.

For those who take the opposite view, they should refer to the BCS/RCP document or a textbook of cardiology to remind themselves of the many and varied, serious cardiac and extracardiac complications that lead to a high morbidity and mortality should IE occur.

It is not true to say or indeed representative of the views of physicians, cardiologists and cardiac surgeons that current antibiotic prophylaxis regimens are too stringent nor that the IV regimens are too complex.

To say that one could only accept that bacteraemia was a risk factor for IE if it had been reported in the literature as a case report, case study or controlled trial is naïve.

Although it is fair to say that episodes of transient bacteraemia are probably frequent during dental and other interventional or surgical procedures, the majority do not cause clinical sequelae.

However, bacteraemia is essential for IE to develop in the first place and so any bacteraemia-producing procedures should be protected by antibiotic prophylaxis in patients deemed to be 'at moderate or high risk' of IE because of an underlying cardiac lesion. We do not regard this as overuse or abuse of antibiotic therapy.

G. J. Roberts

V. S. Lucas

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1. Longman L P, Martin N V, Field E A et al. Cause for Concern? (Letter). *Br Dent J* 2004;**197**:115.

2. Advisory Group of the British Cardiac Society Clinical Practice Committee and Royal College of Physicians Clinical Effectiveness and Evaluation Unit. Guidance on the Prophylaxis and Treatment of Infective Endocarditis in Adults 2004.<http://www.bcs.com> In Press.
3. Horstkotte D, Follath F, Gutschik E et al. The Task Force on Infective Endocarditis of the European Society of Cardiology. Guidelines on Prevention, Diagnosis and Treatment of Infective Endocarditis. Full Text. *Eur Heart J* 2004;00:1-37. www.escardio.org
4. Dajani A, Taubert K, Wilson W et al. Prevention of bacterial endocarditis. Recommendations by the American Heart Association. *Circulation* 1997;96:358-366.

Evidence based dentistry

Sir, whilst the concept of evidence based dentistry is indeed an honourable one, the reality is that it can become very confusing for the practising dentist when different studies ask the same question but come up with different answers.

In the *BDJ*, Moore *et al*¹ investigated the possible relationship between periodontal disease and adverse pregnancy outcome. They could find no relationship between periodontal disease and pre-term birth or lower birth weight.

The very same month the *Journal of Clinical Periodontology* Radnai *et al*² asked a very similar question and came to the conclusion that periodontitis was an important risk factor for pre-term birth.

To my eyes both appear to have been first rate studies and as both journals are refereed I must not have been the only one. So what conclusions can I, as the practitioner come to? Maybe one of the studies is flawed and neither I nor the referees have spotted it.

Could it be that because the two patient populations were different, Moore *et al* studied a multi-cultural south London population with few health exclusions. Radnai *et al* studied a Hungarian population free of systemic disease.

If the difference in the outcomes of the studies is due to the difference in the populations studied then, surely it raises some questions about the validity of clinical trials in general?

J. Ahearne

Poole

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1. Moore S, Ide M, Coward P Y, Randhawa M, Borkowska E, Baylis R, Wilson R F. A prospective study to investigate the relationship between periodontal disease and adverse pregnancy outcome. *Br Dent J* 2004; **197**:251-8
2. Radnai M, Gorzo I, Nagy E, Urban E, Novak T, Pal A. A possible association between preterm birth and early periodontitis. A pilot study. *J Clin Periodontol* 2004, **31**:736-41.