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Underwhelming arguments

Sir, I am concerned by the conclusions drawn in the research paper by Pine *et al.* (*BDJ* 2006; 200: 45–47), which suggest that we should be filling and extracting more deciduous teeth in order to reduce oral sepsis. This does seem to be a rather unsettling idea.

Treatment of the deciduous dentition is problematical and, as the authors state, the evidence for intervention must be weighed against the risks of intervention. No-one, from either side of this argument, has an approach that is going to be successful 100% of the time.

My many years in practice, however, have shown that year on year we are presented with fewer children in pain and fewer children with 'fat faces', and approaches to treatment should reflect these changes.

The old chestnut of 'a chronic abscess can result in damage to the developing permanent tooth' should not worry dentists until proven one way or other. If it happens, how often does it happen, and in what situations? One does see the occasional hypoplastic pre-molar though they are usually second premolars rather than the first premolars one would tend to expect – the first deciduous molars being the ones that abscess more easily. Indeed hypoplastic second premolars can be seen when there has been no caries in the related deciduous molar.

Faced with a 5-year-old with gross caries, I would still prefer to adopt a preventive approach – including the regular application of F varnish and later use of fissure sealants – in an attempt to coax the deciduous dentition along until either the child is older (and less likely to be affected by the procedures), or indeed, as happens in many cases, until the teeth are naturally exfoliated. Some teeth may eventually need extracting but if I can save a 5-year-old child from extraction I would prefer to do so. Many practitioners are also aware that conventional restorations in deep cavities can accelerate abscess formation.

It is the treatment of children as 'individuals' rather than the treatment of teeth that should be paramount – guiding them through their deciduous dentition

with minimal discomfort and building on positive non-interventive visits so that they emerge with their permanent dentition, untraumatised and able to cope with whatever future procedures are necessary. Indeed the child I have treated in this way often has to face the daunting task of having four permanent teeth removed, for orthodontic reasons, with local anaesthetic, as their first interventive procedure. I am still amazed at how well they cope.

I am convinced that this preventive orientated/minimally or non-interventive approach can deliver on most occasions. Others obviously feel that an increased level of fillings and extractions is the way forward. I simply find their arguments underwhelming. The fact that from 1 April 2006 interventive treatment now produces UDAs may well see more interventive treatment being undertaken, though it will be unfortunate that the data collected by the new BSA will not be able to tell us what that treatment is.

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Dubious dataset

Sir, the study by C. M. Pine *et al.* (*BDJ* 2006; 200: 45–47) presents data from 5-year-old children from Scotland. The study population included 3,273 (46.8%) who had no caries experience and 3,691 who had caries of which 224 (6%) had all carious teeth restored. This dataset was used to present evidence that the proportion of children with sepsis increases with the number of untreated carious teeth. There is no problem using this dataset for this purpose because very few carious teeth were restored in these young children. However, the authors also conclude that this disadvantage can be mitigated if more of the caries experience is treated and write in their discussion that the reduced level of fillings and extractions in these children is a significant contributor to their oral sepsis. This conclusion is not and cannot be supported by their data.

The data presented are from a cross-sectional survey conducted in 1999/2000 in Scotland on children with a mean age of 5.3. In this population the proportion of

dmft that is fit is reported to be 6% in children with sepsis and 10% in children without sepsis.

The authors use this dataset to try to persuade the reader that restoring primary teeth reduces the prevalence of sepsis. However, the small number of children with fillings in the dataset and the cross-sectional study design, which the authors correctly state precludes the drawing of causal inferences, make any possible link between sepsis and restored primary teeth impossible to demonstrate. To substantiate the authors' views on restoration a very strong relationship between restoring primary teeth and a reduced prevalence of sepsis needs to be demonstrated but this cannot be done with this dataset because so few children had their teeth restored.

If the authors want to investigate the relationship between filling primary teeth and the prevalence of sepsis it would have been more sensible to use a cohort approach. Why did the authors not follow these children from 1999/2000 to 2004/2005?

There are also methodological issues of which readers should be aware. The authors ignore two obvious sources of confounding. The first is that children with filled or extracted teeth must come from families that have visited a dentist whereas children with no restorations or extractions might never have been to a dentist. It is possible that attendance at a dentist and the preventive care administered might lead to a reduction in sepsis prevalence. The second is that children with a large number of carious teeth are less likely to have all their teeth restored than those with a small number of carious teeth. This leads to a large and important bias because children with multiple carious teeth are many times more likely to have sepsis. To allow the reader to assess these potential biases the authors should now at minimum describe in relation to the total number of carious teeth the proportion of children who had ever visited a dentist and the number of teeth each child had restored. This is important because the published finding that one in 10 children with untreated decay had sepsis whereas only one in 100 children with all their teeth restored had

sepsis is, I believe, misleading and an artefact of the authors inappropriately ignoring obvious confounding from the fact that children with many carious teeth are more likely to have sepsis but much less likely to have all their carious teeth restored. Young children with many carious teeth are unlikely to have been taken regularly to a dentist and if they are taken to a dentist, the dentist is much more likely to advise diet control, oral hygiene instruction and fluoride rather than attempt to restore four or more carious teeth.

The second set of methodological issues relate to the undertaking and presentation of the stepwise logistic regression model. When undertaking such an analysis it is usual to include all independent variables that might be important in explaining the observed occurrences of the dependent variable. Those independent variables which do not improve the fit of the model to the data are rejected based upon predefined limits. In this paper I believe the authors have failed to present sufficient data to enable a detailed assessment of their method or the final logistic regression model. For example, it would be helpful if the authors described which statistical package they used, the models that were tested and if they presented the statistics about the goodness of fit of these models. The authors might also want to comment and present the statistical reasons why the presence or absence of plaque was not included in the final model.

The data in this paper suggest that five years ago in Scotland too many 5-year-old children had caries and that this was leading to sepsis and presumably pain. The obvious conclusion from this data is that efforts were needed to reduce caries. Water fluoridation might have been discussed. For reasons best known to the authors this paper instead focuses on restoration and ignores prevention. While it is possible that restoring primary teeth might reduce sepsis the authors should acknowledge that this dataset cannot and does not support this view.

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To fill or not to fill

Sir, Pine *et al.*'s paper (BDJ 2006; 200: 45–47) concludes on the filling side of the 'to fill or not to fill' deciduous teeth argument. Only 4.8% of those surveyed (n = 6,964) experienced sepsis. Eleven per cent of those experiencing sepsis were from most deprived backgrounds with 2% from least deprived backgrounds. This supports the work of others in that caries in 5-year-olds is found mainly but not exclusively in

deprived populations.¹

I totally agree with the authors if the only aim from care is to eliminate sepsis in 5-year-olds. However, as a GDP caring for a socio-demographic profile with large deprived and affluent population sub-groups.² I fear attempting to fill all deciduous teeth would be counterproductive.

Parents from deprived and affluent backgrounds with caries active 5-year-olds have one thing in common – a particular mindset for childcare that supports caries active oral environments in their children.

The mean dmft for those with dental sepsis is 6.3 suggesting that this group of children would require extensive restorative work. There is a risk that embarking on such treatment would alienate the client group with possible failure to complete treatment thus increasing unsupervised sepsis. This risk, in my experience, is real when attempting such an approach to care although I have no evidence to support this statement.

An alternative behavioural approach to care would be to focus on developing ongoing continuing care with appropriate, realistic, measurable, positive, important, time-related and specific goals.

The primary goal is access and regular attendance so that if sepsis is observed appropriate treatment can be instigated. During continuing care non-verbal communications should communicate self-responsibility for disease inactivity with specific advice as to how to achieve this. There is a danger that filling deciduous teeth in children with disease active oral environments communicates inappropriate responsibility for oral health. That is the belief that it is normal/okay to have disease active oral environments and when disease appears the dentist fills teeth.

Another goal is to engage with adequate numbers of patients from backgrounds reflecting the social profile of the surrounding community. Practice protocols can deploy resources so as to target disease active sub-populations. Such an approach has been reported in the literature.³

Clearly the 'to fill or not to fill' debate is too simplistic to be based on a two-dimensional medical model of health.

W. Richards

1. Tickle M. The 80:20 phenomenon: help or hindrance to planning caries prevention programmes? *Comm Dent Health* 2002; **19**: 39–42.
2. Higgs G, Richards W. The use of Geographical Information Systems in examining variations in sociodemographic profiles of dental practice catchments: A case study of a Swansea practice. *Primary Dent Care* 2002; **9**: 63–69.
3. Richards W, Ameen J R M, Coll A M. The community general dental practitioner. *Br J Health Care Management* 2005; **11**: 308–312.

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Sparing children pain

Sir, I read the paper by C. M. Pine *et al.* (BDJ 2006; 200: 45–47) with interest. As a Consultant in Paediatric Dentistry in Scotland, working with high caries risk and socially deprived children, the findings in this paper are familiar. However, surely this paper tells us nothing new other than reiterate a truth all dentists already know in their hearts: that the more untreated carious teeth are present, particularly in the primary dentition, the greater the probability of dental sepsis occurring. In my experience this also leads to an overuse of antibiotic prescribing. Clearly, there is a role for stabilisation of dental caries and an equally important place for prevention and early caries detection and restoration. The rights and expectations of those parents who genuinely bring their children for six monthly check ups in the belief that this will prevent dental sepsis, but who are unaware that restorative dental treatment is being withheld, must be considered. In our national audit to determine the reasons for choice of anaesthesia in dental extractions for children across Scotland¹ we found that one third of the children referred presented with toothache on the day of the DGA. Furthermore, 39% of parents had reported that the toothache had caused loss of sleep. Worryingly, using a conservative classification of attendance, over a third of the children in the sample as a whole were classified as regular attendees. Every dentist knows that caries in primary teeth, particularly primary molars, quickly leads to pulpal inflammation. However, we also know that the pulp has a capacity to heal given the correct set of circumstances and we all wish children to be spared the pain of toothache. Surely, in the twenty-first century dentists in the UK can do better?

M. T. Hosey

Glasgow

1. Macpherson L M D, Pine C M, Toner C *et al.* Factors influencing referral of children for dental extractions under general and local anaesthesia. *Comm Dent Health* 2005; **22**: 282–288.

Professor Cynthia Pine responds to the above four letters: My co-authors and I welcome the interest in our paper that was published recently in the BDJ. We note that we have received three letters from colleagues linked to the same Manchester-based research group and one from a consultant paediatric dentist in Glasgow who sees children with the type of conditions described in the article on a daily basis. As space does not allow us to address each point in turn, we have picked out some of the main issues raised.

In his letter to us, Dr King notes that we 'suggest that we should be filling and

extracting more deciduous teeth in order to reduce oral sepsis'. In the next sentence he notes that, 'This does seem to be a rather unsettling idea'. Perhaps this extract more than any other goes to the heart of the matter. He correctly concludes that it is our opinion that rather than observe a child's experience of caries being allowed to progress to such a severe extent that the child is suffering pain and sepsis, that the disease should be actively managed. We agree with our letter writing colleagues that preventing the occurrence of caries in the first place is ideal. However, to say that 'Faced with a 5-year-old with gross caries, I would still prefer to adopt a preventive approach', as advocated by Dr King, ignores the disease that is already present. Clearly we would agree that this child should receive secondary prevention. However, secondary is the key word, as there remains a duty of care to provide some form of treatment for the primary disease condition.

Dr Richards considers that parents of children with caries have a 'particular mindset for childcare that supports caries active oral environments in their children'. Further, that as dentists we should consider that 'There is a danger that filling deciduous teeth in children with disease active oral environments communicates inappropriate responsibility for oral health'. It seems odd that we do not consider that filling the teeth of adults with caries communicates inappropriate responsibility for oral health.

Dr Threlfall considers it contentious that we say that 'the reduced level of fillings and extractions in these children is a significant contributor to their oral sepsis'. It seems to us indisputable that one consequence of untreated gross caries can be oral sepsis. In this matter, I bow to my consultant colleague, Dr Hosey who confirms my understanding from the letter she has written, namely, that 'the more untreated carious teeth are present, particularly in the primary dentition, the greater the probability of dental sepsis occurring'. Dr Threlfall goes on to suggest that we should consider, 'sources of confounding. The first is that children with filled or extracted teeth must come from families that have visited a dentist whereas children with no restorations or extractions might never have been to a dentist'. The contention that these children have never been to a dentist does not match the experience of those paediatric dentists who see children attending for extractions under GA every week. As noted by Dr Hosey, in a recently published study, over a third of these children were regular attendees.

A more representative insight to the barriers dentists perceive to providing care for young children with caries can be gained from

the views of over 2,000 colleagues in 14 countries.¹ Two factors were consistently identified, first the age of the child and secondly, personal feelings dentists have about providing care for young children, that this can be stressful and troublesome and that they felt time constrained. The majority of dentists disagreed with statements suggesting that there was little value in restoring decayed deciduous teeth. Sadly, in contrast to most other countries, dentists in the UK identified most aspects of our dental healthcare system as being significant barriers to providing care for young children with caries.

Finally, Dr Threlfall notes that 'in Scotland too many 5-year-old children had caries and that this was leading to sepsis and presumably pain. The obvious conclusion from this data is that efforts were needed to reduce caries. Water fluoridation might have been discussed'. Whilst we heartily support efforts to extend water fluoridation in the UK, I am minded of my own personal experience working for this very aim, over 25 years ago as a PhD student. I remember sitting next to my distinguished supervisor Phil Holloway as we attended a series of public meetings in local councils, libraries and health centres, making the case. I can hear an echo of the same words then as we now wait again for water fluoridation, and ask, how many more generations of children need to experience pain and suffering whilst, in this instance, we discuss whether caries leads to sepsis, matters of confounding and whether our profession is going to treat the disease? Though it is often interesting to participate in academic debate, for the children's sake, let's hope this letters page is not witnessing the same discussion as many years hence.

1. Pine C M, Adair P, Burnside G et al. Barriers to the treatment of childhood caries perceived by dentists working in different countries. *Comm Dent Health* 2004; 21 (Supplement): 112-120.

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A more comprehensive view

Sir, readers of the *BDJ* may wish to have a more comprehensive view of the evidence on the possibility of links between sugar consumption and diseases other than dental caries than that offered by Gill and Gill.¹

Comprehensive reviews on diet (including specific consideration of sugars) and obesity, diabetes, cancer and a range of other diseases have recently been conducted by expert groups for the World Health Organisation and the UN Food and Agriculture Organisation,² The Institute of Medicine³ (as the basis for the five year review of Dietary Guidelines for Americans), and in the UK a particularly thorough review on cancer was conducted by COMA.⁴

None of these reviews have concluded that a credible body of evidence exists to

support the suggestions put forward by Gill and Gill, still less that the strength of any evidence is sufficient to merit advice to the public in relation to cancer.⁴ Particular uncertainties bedevil the interpretation of evidence on cancer.⁴

It is difficult enough to persuade today's young people to limit the frequency of intake of sugars and other carbohydrate-based snacks in order to sustain the substantial improvements in oral health that have resulted from fluoride toothpaste. Experience suggests that they will not be much influenced by shroud waving about health issues much later in life, especially if these are not firmly based on sound evidence.

R. C. Cottrell

1. Gill S K, Gill D S. Sugar and cancer. *Br Dent J* 2005; 200: 31.
2. Joint WHO/FAO Expert Consultation. Diet, Nutrition and the Prevention of Chronic Diseases. WHO Technical Report Series 916. Geneva: WHO, 2003.
3. Food and Nutrition Board. Dietary Reference Intakes for Energy, Carbohydrates, Fiber, Fat, fatty Acids, Cholesterol, Protein and Amino Acids (Macronutrients). Washington: Institute of Medicine. National Academies of Science, 2002.
4. Department of Health. Nutritional Aspects of the Development of Cancer. Report on Health and social subjects No 48. London: The Stationary Office, 1998.

D. S. Gill and S. K. Gill respond: We agree with Dr Cottrell that there is no conclusive evidence to link carbohydrate consumption with the development of cancer. We wrote our original letter to inform readers of the *BDJ* that there was some evidence to suggest a possible link between carbohydrate consumption and the development of cancer. This followed an earlier letter outlining the possible link between the consumption of sugar-containing drinks and the development of obesity and diabetes in later life.¹ We felt that dentists, as healthcare professionals, should be aware of such research as they are actively involved in advising patients about the harmful effects of excessive carbohydrate consumption and should therefore have knowledge of the wider developing issues surrounding sugar consumption and health. Although we do not recommend that mentioning this possible link with cancer should be a main focus of any dietary counselling episode, we feel that dentists have some responsibility in informing the public about wider health issues, and research reported on these, as long as this is kept in context.

As Dr Cottrell correctly states, it is difficult to convince people, particularly the young, to change their behaviour. We feel that if there is evidence to suggest that sugar consumption may have a negative impact on wider health, it should be highlighted as it can only provide added impetus for change.

1. Gill S K, Gill D S. Sugar-sweetened drinks. *Br Dent J* 2004; 197: 520.

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