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Tooth movement

Sir, studies on overeruption of unopposed posterior teeth are scarce and therefore we read the article by Craddock and Youngson¹ with great interest. The elegant method using 3D scanning of models and a great number of patients promised valuable results. However, there are some shortcomings that reduce the value of their findings. More specifically, there is a lack of information about the age when the antagonist teeth were lost and how long the studied teeth had been unopposed.

After referring to a study reporting significant overeruption of lower second molars after orthodontic extraction of upper second molars in children², the authors wrote “there is no reason to suppose that this movement does not also occur in unopposed adult sites”. This is an unsupported extrapolation conflicting with evidence of greater potential of dentofacial adaptation in children than in adults. In a Swedish study³, molars that lost their antagonists when the persons were above age 26 showed less severe overeruption than those for whom the age at loss of the antagonist tooth was unknown. This study³ reported no overeruption in 18% and ≥ 2 mm in 24% of molars that had been unopposed for more than 10 years. The present study¹ had fairly similar results: 17% no overeruption, 32% > 2 mm. The interpretation of the results differed, however. The British study¹ emphasised the high prevalence of overeruption of unopposed posterior teeth and its negative clinical significance. In the Swedish study³ it was concluded that not all unopposed teeth overerupted. It was also suggested that the finding that 3/4 of the unopposed teeth had overerupted < 2 mm did not give strong support for a general prophylactic replacement of lost antagonists. Support for this opinion can also be found in the literature on the shortened dental arch concept – loss of molar support does not in general lead to impairment of the health of the masticatory system. A retrospective study by Shugars *et al*⁴ (not mentioned in the present article) found that after a mean of 6.9 years the extrusion of an unopposed tooth was < 1 mm in 99% of the cases. The

authors concluded that the great majority of their patients did not exhibit the expected adverse consequences generally assumed to be associated with non-replacement of a single posterior tooth.

Based on recent results, it would seem justified to discuss prosthodontic decision – making for unopposed teeth on an individual level rather than continue the general textbook warning for adverse consequences of not replacing lost posterior teeth.

Finally, a comment regarding “incidence” used both in the title and repeatedly in the text in the paper¹. Incidence and prevalence may be considered synonyms in many dictionaries, but they are not in epidemiology and medicine. Incidence means the rate at which a certain event occurs, e.g. the number of new cases of a specific disease occurring during a certain period in a population at risk (Dorland’s Medical Dictionary, 2000).

G. E. Carlsson, Sweden

S. Kiliaridis, Geneva

1. Craddock H L, Youngson C C. A study of the incidence of overeruption and occlusal interferences in unopposed teeth. *Br Dent J* 2004; **196**: 341-348.
2. Smith R. The effects of extracting upper second permanent molars on lower second permanent molar position. *Br J Orthod* 1996; **23**: 109-114.
3. Kiliaridis S, Lyka I, Friede H, Carlsson G E, Ahlqvist M. Vertical position, rotation, and tipping of molars without antagonists. *Int J Prosthodont* 2000; **13**: 480-486.
4. Shugars D A, Bader J D, Phillips S W Jr, White B A, Brantley C F. The consequences of not replacing a missing posterior tooth. *J Am Dent Assoc* 2000; **131**: 1317-1323.

The authors of the paper HL Craddock and CC Youngson respond: We thank the corresponding authors for their interest in our paper and their already important contributions to research in this area. They raise some interesting points and provide (embarrassingly!) accurate guidance to the correct use of the English language.

However, some comments in their letter may have arisen from a misconception of the purpose of the article. The aim of the study, as stated in the title and text, was to determine the proportion of individuals in the study population having overeruption and occlusal interferences at that time. We

did not try to determine any factors implicated in the aetiology of overeruption. Age of tooth loss may indeed be a factor but, in designing our study, we recognised that we would not be able to accurately determine the age at which each tooth was extracted and so this was (intentionally) excluded. We consider that only a prospective study could do this with accuracy.

A second point that seems to concern the correspondents is our statement, following a discussion of tooth movement in children, that “there is no reason to suppose that this movement does not also occur in unopposed adult sites” and they make the observation that “This is an unsupported extrapolation” We acknowledge this. However, although there is greater potential for dentofacial adaptation in children we are not aware of evidence that suggests that it is completely absent in adults. We are currently investigating this aspect as part of another, larger, study of adults which may provide further evidence one way or the other.

The correspondents note that we did not include the article by Shugar *et al*. (2000). That study was not included within our article as their methodology was radiograph-based and observations were made of static relationships between teeth (given the limitations of assessing tilting or rotation of teeth from radiographs). Our study was quite specifically looking at occlusal interferences which are dynamic in nature. As interferences could not be assessed from radiographs we considered that Shugar *et al*’s study was not entirely relevant to our paper.

We agree that many prosthetic textbooks suggest replacement of missing posterior teeth based upon traditional beliefs rather than scientific evidence and certainly do not advocate the mechanistic replacement of every tooth. However, we have noted that there are consequences of leaving an edentulous space with an antagonist tooth. If a prosthesis is being considered we hope that our study would encourage clinicians to examine for RCP and protrusive interferences before undertaking its provision.

Our study aimed, with its recognised limitations, to add to the limited evidence base on the prevalence of overeruption of teeth in adults in the UK and the role of these in occlusal interferences and has formed an early part of an ongoing investigation. We are grateful to the corresponding authors for raising the matters for discussion.

doi: 10.1038/sj.bdj.4812246

Restorative dentistry

Sir, we were surprised to see the paper Partial Caries Removal and cariostatic Materials in carious Primary Molars: A Randomised Controlled Clinical Trial published (*BDJ* 2004, **197**: 697–701) as the results were so poor. In this article the authors noted similar failure rates for all three techniques which led them to the conclusion that partial caries removal and restoration was comparable to conventional restorations. There are a number of issues which can be discussed. The overall failure rate for all three methods of restoration was very high. Out of some 86 restorations available for follow-up after 24 months 31 nearly 40% had failed.

Normally follow-up for such studies is 48 to 60 months to compare with previous similar studies¹. Also, significantly more PCC/GIC and PCC/BCC failed in the first six months compared with CR. In addition some 62% of class II restorations had failed. No doubt the failure rate would have been even higher for a full 60 months study.

These failure rates are unacceptable for restorative treatment in children and would probably have been even higher had the authors included the teeth that had abscessed, or were subsequently extracted, rather than curiously withdrawing them from the study. Abscess formation is a sign of failure and these teeth should be included with the failures reported in the final analysis. Also, it is unclear how many of the children lost to follow-up might have undergone extractions of the restored primary teeth under local or general-anaesthesia by other dentists. Recent studies have shown more acceptable survival rates for restorations in primary teeth, but where restorations were performed using local analgesia, proper isolation and following the well established principles of caries removal and cavity design^{2,3}.

It is not surprising that the authors found that placing a material in an unprepared cavity leads to failure of the restoration in a high proportion of the cases. We should not accept such high failure rates of restorations in primary teeth when such results would have been totally unacceptable for permanent teeth. Repetitive treatment in children has implications for a child's behaviour and

emphasis should be on restorative materials and techniques that seldom fail, such as stainless steel crowns⁴. In addition, due consideration should be given to the diagnosis of the state of the pulp especially for proximal caries. In primary teeth pulp inflammation sets in early preceding the exposure of the pulp⁵.

In our opinion this is yet another article in the *British Dental Journal* reporting on techniques for restoration of primary molars that circumvent the basic principles of restorative dentistry, which would never be considered for the restoration of permanent teeth. A high failure rate of restorations placed in primary teeth of children where the state of the pulp is not taken into account, longevity of restorative materials not considered or principles of cavity design ignored, has given rise to the myth that restorative treatment for primary teeth is futile. This is not true. Excellent success rates have been reported and following some of the principles mentioned above it is possible to achieve excellence in restorative treatment for primary teeth, which children who still get dental disease, deserve from dental health professionals.

M. S. Duggal.

M. E. J. Curzon.
Leeds

1. Papatransiou, A G, Curzon, M E J, Fairpo C G. The influence of restorative material on the survival rate of restorations in primary molars. *Pediatr Dent* 1994;**16**:282-288.
2. Mass E, Gordan M, Fuks A B. Assessment of compomer proximal restorations in primary molars: A retrospective study. *J Dent Child* 1999;**66**:93-97.
3. Fuks A B, Araujo F B, Osorio L B *et al*. Clinical and radiographic assessment of class II esthetic restorations in primary molars. *Paediatr Dent* 2000;**22**:479-485.
4. Randall R C, Vrijhoef M A, Wilson N H. Efficacy of preformed metal crowns vs. amalgam restorations in primary molars: A systematic review. *J Am Dent Assoc* 2000;**131**:337-343.
5. Duggal M S, Nooh A, High A. Response of the primary pulp to inflammation: A review of the Leeds studies and challenges for the future. *Eur J Paed Dent* 2002;**3**(3): 112-117.

Authors of the paper, J Foley, DJP Evans

and A Blackwell respond: We refer to our recent publication (*BDJ* 2004; **197**:

697–701) and to Professors Duggal and Curzon's letter in response to our study.

Firstly, in relation to the failure rate, whilst the authors acknowledge the high failure rate of this study, this is primarily attributed to the poor performance of the PCR:BCC restorations, with 37% of restorations being lost by six months and 51% by the study censor date; the reasons for the failure rate of this restoration type are explored fully within the paper's text. Regarding the failure rate of the other restoration types, i.e. PCR:GIC and CR, 16% and 19% of restorations had failed for PCR:GIC restorations at six – and twenty-four months respectively, whilst for CR restorations, 5%

and 12% of restorations failed over the same time periods; this is comparable to other studies where a plastic restoration has been used to restore a primary molar tooth^{1, 2} and hence, to consider the results 'so poor' is, we believe, invalid. In relation to those teeth where abscess formation was noted, the authors note the ambiguity of the text and wish to clarify that these teeth were included in the statistical analysis and it was the patients who were withdrawn from the trial. Concerning those patients lost to follow-up, unfortunately, this is an inevitable difficulty with any trial which is clinically-based.

With regard to conventional restorative treatment techniques, the authors fully acknowledge that this is currently accepted best practice, in line with BSPD guidelines³ and effective, particularly when used in specialist practice⁴ and dental hospitals⁵; the difficulty, however, appears to be that such techniques are not popular with general dental practitioners working within Primary Care, where over 90% of child dental care is provided and indeed, recent surveys throughout the United Kingdom have determined that less than 15% of cavities in primary teeth in five-year-olds are being restored.^{6,7} Furthermore, the care index (the proportion of carious primary teeth which have been treated restoratively) for five-year-olds in Scotland has fallen from 20% in 1983 to 9% in 2003.⁸ Perhaps one of the conclusions which can be drawn from this relatively small prospective study is that there is both a need to train further Specialists in Paediatric Dentistry to work within the Primary Care setting, in addition to the overall expansion of the discipline within the Hospital Dental Service?

1. Qvist V, Laurberg L, Poulsen A and Teglers P T. Longevity and cariostatic effects of everyday conventional glass-ionomer and amalgam restorations in primary teeth: three-year results. *J Dent Res* 1997; **76**: 1387-1396.
2. Welbury R R, Shaw A J, Murray J J, Gordon P H, McCabe J F. Clinical evaluation of paired compomer and glass ionomer restorations in primary molars: final results after 42 months. *Br Dent J* 2000; **189**: 93-97.
3. Fayle S A, Welbury R R, Roberts J F. British Society of Paediatric Dentistry: a policy document on management of caries in the primary dentition. *Int J Paediatr Dent* 2001; **11**: 153-157.
4. Roberts J F and Sherriff M. The fate and survival of amalgam and preformed crown molar restorations placed in a specialist paediatric dental practice. *Br Dent J* 1990; **169**: 237-244.
5. Welbury R R, Walls A W, Murray J J and McCabe J F. The 5-year results of a clinical trial comparing a glass polyalkenoate (ionomer) cement restoration with an amalgam restoration. *Br Dent J* 1991; **170**: 177-181.
6. Pitts N B, Nugent Z J and Smith P A. Scottish Health Boards' Dental Epidemiological Programme. Report of the 1999-2000 survey of 5 year old children. University of Dundee: 2000.
7. Pitts N B, Boyles J, Nugent Z J, Thomas N and Pine C M. The dental caries experience of 5-year-old children in England and Wales. Surveys co-ordinated by the British Association for the Study of Community Dentistry in 2001/2002. *Community Dent Health* 2003; **20**: 45-54.
8. <http://www.dundee.ac.uk/dhsru/ndip/index.htm>.

doi: 10.1038/sj.bdj.4812247