material although the subjects did not demand treatment at the baseline. However, the subjects in an earlier study (Kirveskari *et al.*, 1998) had no identifiable external risk to their muscles. Moreover, a strong correlation between the number of interferences and the presence of (mainly muscular) signs of TMD was found in cohorts of healthy children after interferences had been eliminated in a small number of them (Kirveskari *et al.*, 1989, 1992). Lastly, the finding that artificial interferences without a change in the intercuspal position could induce symptoms of TMD in former patients (Le Bell *et al.*, 2002) strongly suggests that interferences can be harmful.

We have repeatedly been unable to falsify the argument that occlusal interferences are a causal factor. The fact that the relative risk in subjects with was significantly greater than that in subjects without interferences suggests to us that we cannot dismiss interferences as a health risk. Before critical tests comfort us, there is reason to suspect that even the changes in occlusion unwittingly caused by dental therapy may in fact, at least in some patients, increase rather than decrease the structural risk from occlusion. However, we have not tried to assess the absolute magnitude of the risk neither in the population at large nor in patients before or after any dental therapy.

Symptoms in the head and cervicobrachial region are common. They are often managed by using symptomatic therapy with only temporary relief. In view of the discomfort and considerable cost the symptoms cause, it seems unwise to ignore interferences as long as the hypothesis of their causal role has not been falsified. The explanation suggested by Drs Schindler and Türp is scientifically sound. However, the hypothesis underlying it would need to be critically tested. A clinical trial testing their hypothesis would not be easy to carry out in practice—it would need to be designed so that the two rival hypotheses (elimination of interferences versus any change in mandibular tooth-to-tooth positions) predict a different outcome.

Pentti Kirveskari and Tapio Jämsä
Institute of Dentistry
University of Turku
Lemminkäisenkatu 2
FIN-20520 Turku
Finland

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Sir,

Drs Schindler and Türp consider the term ‘health risk’ ‘an over-interpretation that is likely to obfuscate a more physiological explanation’. They suggest that any small repeated changes in mandibular position suffice to explain our results. We are pleased to share their opinion that occlusion appears to have an influence on the neuromuscular system.

The alternative explanation for our results offered by Drs Schindler and Türp is an interesting example of the problem of proving hypotheses correct. Results of studies can as a rule be explained in several ways. We agree that small changes in mandibular position can have a short-term therapeutic effect on patients suffering from muscle or temporomandibular joint pain. Work-related muscle pain was probably common in our