

Pires dos Santos *et al.* emphasised the limitations of self-reported data. We concur with them, that self-reported data are less reliable. However, oral hygiene self-care does not have any other instrument but self-report. It is unfortunate that these scholars do not recognise the benefits of oral hygiene self-care but only see the deficiencies of self-report. Similarly, physical activities are difficult to assess and especially by self-report.³ However, many studies use the frequency of exercise as a proxy and are accepted as valid.⁴ Now physical activities are recognised as beneficial for health.⁵ The same acceptance should be given to self-reported oral hygiene performance.

We would like to point out to Hujuel and Pires dos Santos *et al.* that the risk reduction of 50% is relative to those who did not 'brush or floss'. It is not an absolute risk reduction. A classic example of differences between 'relative risk' and 'absolute risk' can be found in the JUPITER trial for rosuvastatin [see comment⁶]. The CVD event rate in the placebo group was approximately 3% and the same in the statin group was 1.6%, thus, although this trial reported a highly significant relative risk reduction of 44%, the absolute risk reduction was only 1.4%. Per our calculation: $(251/8901 - 142/8901) \times 100 = 1.4\%$.

We would like to offer a word of caution to Hujuel who wrote 'a failure to take hormone replacement therapy in post-menopausal women caused cardiovascular disease, that insufficient intake of dietary carotenoids caused cancer, and that periodontitis during pregnancy caused adverse pregnancy outcomes'. These are transposition of reported study results. Even if the relationship is causal, increased or decreased risk is not the same as 'disease' or 'non-disease' occurrence. One should not invert the reported results because 'estrogen replacement therapy decreased the risk of CVD' and 'a failure to take hormone replacement therapy caused cardiovascular disease' are two different events in inverse direction as we have explicated.⁷ We also would like to inform Pires dos Santos *et al.* that the ROBINS-E tool is not universally accepted as useful.⁸ Lastly, we thank *BDJ* and its reviewers for giving us the opportunity to discuss these issues openly and fairly.

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Restorative dentistry Somewhat misleading

Sir, we would like to offer some comments in relation to the interpretation of our published research data,¹ as part of a recent article published in the *BDJ*.² With reference to our investigation reporting on the 5.5-year clinical performance of direct composite resin restorations for the full mouth rehabilitation for patients with severe tooth wear, Dr Hassall has stated that, 'Level 1 and Level 2 failures required repair or replacement, while Level 3 failures (small chips) were polished or accepted. Combined Level 1 and 2 failures were high at 32.5% and if Level 3 failures were included, failure rose to 67.6% after only five years.'

As part of our investigation, there were 676 anterior direct resin composite restorations prescribed for the treatment of tooth wear, observed for a mean period of 62.4 months. Failures were described as, either, a 'Level 1' failure that had a severe deficiency and required replacement of the restoration (to include the need for endodontic treatment or a dental extraction – a catastrophic failure), 'Level 2', a type of failure which referred to the presence of localised deficiencies that were repaired, and 'Level 3' failure, denoting the presence of a small material chip, which would require refurbishment by polishing or needed no further intervention. As part of

the data analysis, the following descriptions were applied; 'Level 1' – a catastrophic failure, 'Level 2', combined Level 1 and 2 failures and 'Level 3', all levels of failure observed. In total at the 5.5-year mark, there were 19 Level 1 failures (2.8%), 58 Level 2- failures (8.6%) and 72, Level 3- failures (10.7%). The combined Level 1 and Level 2 failures for the overall anterior restorations were in fact 8.6% (and not 32.5%) and 10.7% (opposed to the quoted 67.6%) for the overall sample, with an overall annual failure rate for all types of failures combined (Level 3-) of approximately 2.2%.

The author of the *BDJ* article has referred to an overall rate of failure that was approximately six times greater than the actual finding. This is somewhat misleading. Whilst significantly higher failure rates were observed where anterior veneer restorations required further visits for completion, based on our overall findings, we concluded that direct resin composite, with proper case planning,³ can offer an acceptable medium-term solution for treating severe generalised tooth wear. This included the prescription of posterior direct resin composite restorations, noting, higher-risk patients were not excluded in our full sample of 1,269 restorations. This contrasts with the author's interpretation of our data, and this is of material relevance. The use of direct resin composite applied in an additive, minimally invasive manner has many benefits for the restorative rehabilitation of tooth wear, to include some documented improvements to patients' oral health-related quality of life post-intervention.

We feel the author is incorrectly using our paper to support his point of view and would kindly request an appropriate erratum to the published paper.

S. B. Mehta, London, UK; B. A. C. Loomans, Nijmegen, The Netherlands

Dominic C. Hassall responds: The paper considered¹ presents data for all regions of the mouth including the anterior maxilla for one session and two session direct composite veneers on maxillary anterior teeth for advanced tooth wear.

My paper² selected the two session anterior maxillary data as this is the most aesthetically demanding area and it clearly highlights the limitations of traditional composite techniques.

For two session maxillary veneer placement combined level 1, 2 and 3 failure is indeed very

high at 67.7% over a relatively short study period. Even if the more minor level 3 failures are excluded the failure rate is still high at 32.5%. Although less failure is associated with one session maxillary veneer placement combined failure was also very high at 46.1% and still high at 26.9% if level 3 failures are excluded.

Other areas of the mouth also displayed high failure, for example combined level 1, 2 and 3 failure in the mandibular molar area is 42.8% or 22.5% if level 3 failures are excluded.

Level 3 failures are actually of clinical significance as these small chips further deteriorate and there are time implications if polishing is required or the roughness, sharpness or staining requires attention.

Over many years of using traditional composite techniques I like many other clinicians have experienced these high failure/repair/refurbishment/polishing levels which are disappointing and frustrating for both patients and clinicians. Unlike the study where the treatment was provided free of charge, in many countries the majority of tooth wear is treated privately where the significant time involved has financial implications for patients.

This is why over the last decade fresh approaches such as the Bioclear composite approach and monolithic high strength ceramics have gained worldwide popularity due to their longevity and low maintenance.

It is worth noting that it took up to 15 hours to complete the rehabilitations which is a significant time investment and polishing due to extrinsic staining or surface roughness was not even registered as failure but again has significant clinical time implications.

The use of your data is fully justified to support why a more contemporary composite approach has been adopted by many clinicians worldwide, which in the experience of many of us has dramatically reduced the significant failure and ongoing high maintenance rates presented in your study.

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Clinical research

Research collaboration controversies

Sir, I read with interest the recent publication in the *BDJ* entitled 'Possible malpractice by researchers'.¹

The issues surrounding research collaborations between the so-called global north and south extend far beyond journal APCs. In general, there has been a rise in concern regarding research integrity in recent times. The 7th World Conference on Research Integrity led to the formulation of the Cape Town statement which pertains to fairness, diversity and equity as they relate to research. The need of the statement stems from recognising that collaborators from low- and middle-income countries (LMIC) do not gain equitable benefits

when compared to those from high-income countries.²

This goes beyond a simple capacity to pay for APCs to include authorship, progress in career, number of publications, data/sample ownership and research priorities.² The role of LMIC collaborators, it has been found, can get relegated to merely collecting data, reviewing manuscripts for local propriety while the 'lead' team conducts the actual analyses.² There are other documents which can be referred to in order to address this larger issue which includes the BRIDGE guidelines, Commission for Research Partnerships with Developing Countries and the Global Code of Conduct for Research in Resource-Poor Settings.² An example for necessitating stipulations for diversity may be derived from the EDCTP2 call in 2020.²

Journals, editors and publishers may also take steps to stamp out such practices. *Nature* encourages author disclosures on ethics and inclusion upon submission of manuscripts.² *The Lancet* rejects papers which have data from Africa in the absence of a mention of a single African collaborator.² The 'Open Science' model is cost prohibitory for a number of researchers and perhaps publishers should look into this issue as it pertains to countries such as Brazil, South Africa and the like which do not make the cut for services such as Research4Life.²

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