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## View with a pinch of salt?

The *BDJ* strives hard to achieve the highest standards of publication, both in clinical outlook and statistical content, and yet the statistical analysis and presentation of results in the paper by Delilbasi *et al*<sup>1</sup> on gustatory function in postmenopausal women are largely either inappropriate or inadequate. As a consequence, I believe that the conclusions should be viewed with caution (perhaps with a pinch of salt?).

The authors have made no attempt to justify their choice of sample size using power calculations. It should be remembered that lack of significance, when it arises, may well be a consequence of low power. Although the authors have avoided some biases by randomising the order in which the stimulator solutions were taken and the order of locations for painting, each solution was given in increasing concentrations rather than at random.

Smoking is associated with increased (worse) taste thresholds but no indication is given of the proportions of male and female smokers. The type of analysis of variance used has not been described nor have the circumstances in which it has been employed. The authors regard an 'acceptance of a probability of  $p < 0.05$  as significant' but this is misleading since if  $p < 0.05$  then the null hypothesis is rejected.

The postmenopausal women were compared to age-matched males but the non-parametric Mann-Whitney U test used to investigate differences between the gender groups treats the observations in the two groups as independent rather than paired. And why use a non-parametric test, which is useful for skewed data and essentially compares medians, and then report the result as a difference in means (where is the estimate? for which concentration?) when the mean is not a sensible measure of central tendency for skewed data? The authors indicate, incorrectly, that the relevant mean scores are contained in figures 1 and/or 2. In fact, figure 1 gives the results for each concentration separately and labels the

vertical axis as '% of first identification (mean + se)'.

This is confusing since the percentage of individuals in a particular gender group first identifying a stimulus is a single figure and not a mean. In figure 2 the six oral locations are marked individually on the horizontal axis although the legend explains that the diagram shows the mean taste intensity ratings given to each compound summed over six oral locations.

Does each shaded portion of a bar for a given location then represent the mean taste intensity rating averaged over the compounds (where is its standard error or confidence interval?) for males or females?

The authors report that 35% of the women noticed failure in tasting sweet, salty, sour and bitter substances as strongly as before the menopause, but since taste sensitivity declines with age and complete amenorrhoea time is up to 15 years in these women, this could be a reflection of age and not of menopausal status.

**A. Petrie**  
London

1. Delilbasi C, Cehiz T, Akal U K, Yilmaz T. Evaluation of gustatory function in postmenopausal women. *Br Dent J* 2003; **194**: 447-449.

**Dr. Cagri Delilbasi, one of the authors of the paper responds:** We are thankful to Ms Aviva Petrie for her assessment and advice about our article 'Evaluation of gustatory function in postmenopausal women' in the *BDJ*. There are some points we want to emphasize. In this study, to avoid some biases, the order in which the solutions were taken and the order of the locations for painting were randomized; however, the solutions were given in increasing concentration.

When the physiology of taste is considered, high concentration of a solution may reduce the person's ability to recognize a lower concentration of that solution. When we searched similar threshold studies about taste perception, we noticed that the solutions were mostly given in increasing order. When

conducting such studies, one should consider 'physiologically' not only theoretically. We agree with Ms Petrie that it would be better if figure legends were more descriptive and clear for the reader to better understand what we wanted to say. The statistical methods used in the study were chosen by our statistician. Of course it is open to criticism as there are different ways to analyze the results.

There are many external factors that may affect taste perception such as smoking, dietary habits, denture use and alcohol consumption. In order to make our study more objective, we restricted the inclusion criteria for the study as mentioned in the 'Subjects and Methods' section. However, if there is a small sample size, it is not always possible to rule out all the factors that could influence the study. This is not a matter for only this study. We asked two questions to female subjects to learn their self-assessment of change in taste perception and dietary habits before and after menopause. Although age can influence this assessment besides many factors, the purpose of asking these questions was just to have information about the subjective evaluation of the participants.

The answers supported the results of the taste tests. We want to thank again to Ms Petrie for her careful evaluation of our paper. These kind of critics are very valuable because they help scientists to better prepare future studies.

**C. Delilbasi**

By email

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**David Moles Statistical Advisor to the *BDJ* responds:** Aviva Petrie's letter lists a series of criticisms of the paper by Delilbasi *et al*. In addition to the issues that are specific to the paper, Ms Petrie's letter also serves to stimulate consideration of the more general aspects of quality control in scientific publication. Ms Petrie criticises the Delilbasi paper in three areas: study design, analytical methods, and presentation of results.

**Study design**

The authors did not report the justification

for their chosen sample size. If comparable studies have been published then it should have been possible to determine in advance a sample size that would provide an adequate chance of detecting clinically relevant differences between groups if they do in fact exist.

This is not to say that the authors did not do this, merely that they did not report it. If the authors were unable to undertake a sample size calculation because the requisite data were not available, then this study should perhaps have been more appropriately described as 'a pilot'. The authors administered the taste stimuli in increasing concentrations in order to elicit the threshold for their detection. Ms Petrie suggests that it would have been better if the concentrations had been given in a random order.

As the authors state in their reply, although this is theoretically true, it is not always appropriate to do so. If, for example, the administration of a particularly intense taste stimulus prevented or reduced a subject's ability to detect a subsequent lower intensity stimulus then it would not be prudent to administer the stimuli in a random order due to the 'carry-over effect'.

Determination of whether the stimuli should be given at random or incrementally requires an appreciation of the physiology of taste and is not solely a statistical consideration. As such the BDJ must rely on the guidance of its expert referees in this respect. Clearly the failure to consider the potential confounding effect of smoking on taste perception is a weakness in the study.

#### **Analytical methods**

Since the women under investigation had age-matched male controls, Ms Petrie is indeed correct in suggesting that the data would have been more appropriately analysed using a method that treats the observations as being paired rather than independent.

#### **Presentation of results**

There are several criticisms of the way the results were presented and the use of language. I agree that it would have been an improvement if the results were presented in the manner suggested by Ms Petrie. I could also offer some additional suggestions.

Indeed there is almost always room for improvement in presentation and clarity. However, there is a dilemma here in that journals must decide how prescriptive they wish to be in defining the format of acceptable presentation. Subtle rewording of the title to figure 2 would have reduced the potential confusion identified by Ms Petrie. It seems that the title needs to read with that 'pinch of salt'.

As suggested by Ms Petrie the BDJ does indeed strive hard to achieve the highest standards of publication. All manuscripts are reviewed by at least two referees who are chosen for their expertise in the particular subject area. Either the scientific or clinical editor as appropriate also reviews the manuscripts. Any of the people involved in the refereeing process may request that it be sent for statistical review.

On this occasion all of the referees were satisfied and so a statistical opinion was not sought. This raises the question as to whether a statistician should routinely review all manuscripts. This is a perpetual dilemma in scientific publishing. To do so has the potential to improve the standard of some publications, but there would be other consequences including that it would take longer for all manuscripts to complete the review process. These and related issues are being discussed and considered by the BDJ.

It is my personal perception that the quality of research in dentistry has improved steadily in recent years. Certainly more research is being undertaken in collaboration with multidisciplinary colleagues, including statisticians.

This is a positive trend that can only be beneficial; especially where there is genuine 'collaboration' rather than just 'consultation'. The BDJ will continue to promote the dissemination of high quality research and to learn the lessons of experience.

Until now the policy of the BDJ has been to attempt to avoid being overly prescriptive in its requirements of authors. In addition to the general instructions to authors, the BDJ has published comprehensive guidelines regarding good practice in both statistical conduct and the presentation of results<sup>2</sup>. These are based very closely on the excellent suggestions from Altman et al<sup>3</sup>. I should like to encourage prospective authors to give due consideration to these guidelines, and I thank Ms Petrie for giving me the opportunity to remind authors of their existence.

**D. Moles**

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By email

1. Delilbasi C, Cehiz T, Akal U K, Yilaz T. Evaluation of gustatory function in post menopausal women. *Br Dent J* 2003; **194**: 447-449.
2. British Dental Journal Statistical guidelines. Available online at [http://www.nature.com/bdj/about/submission\\_statistics.html](http://www.nature.com/bdj/about/submission_statistics.html)
3. Altman D G, Gore S M, Gardner M J, Pocock S J. Statistical guidelines for contributors to medical journals. In Altman D G, Machin D, Bryant T N Gardner M J (eds). *Statistics with confidence*. 2nd ed. pp 171-190. London: BMJ Books, 2000.