Facial aesthetics and perceived need for further treatment among adults with repaired cleft as assessed by cleft team professionals and laypersons

Sir,

We read with great interest the recent article by Foo et al. (2013), on ‘Facial aesthetics and perceived need for further treatment among adults with repaired cleft as assessed by cleft team professionals and laypersons’. However, we would like to express the following comments regarding the methodology of this study.

1. The sample of cleft patients used for this study is not homogeneous and included 44 bilateral and 33 unilateral cases with cleft lip and palate, as well as one patient each with isolated cleft lip, isolated cleft palate and a submucosal cleft palate. A study from the Australian Craniofacial Unit has previously concluded that ‘facial growth is more affected in bilateral cleft lip–cleft palate patients than in either unilateral cleft lip–cleft palate or isolated cleft palate patients’ (David et al., 2011). Thus, showing a pooled sample of different cleft types to the raters should have been avoided or else appropriate subgroup analysis should have been performed. As the authors have collected Visual Analogue Scale (VAS) scores for nose, lips, and face separately, the heterogeneity within the sample almost certainly affected the VAS scores. For example, a VAS score for a repaired bilateral cleft lip may not be the same as that for a repaired unilateral cleft lip or a patient with an isolated cleft palate who did not undergo lip surgery.

2. The rating panel consisted of laypersons and professionals; however, there were only nine judges in total. There were two cleft subjects in the rating panel. The authors have correctly identified this shortcoming in their discussion by saying ‘The small number of raters with a cleft meant that their ratings may not be representative of that cohort’. Therefore, the authors should have avoided the generalized statement ‘Among the laypeople, the members with clefts reported higher facial aesthetics ratings and had a lower perception of necessity for further surgery compared with the non-cleft laypeople’ in the Conclusion.

3. The authors stated that it was difficult to recruit raters and acknowledged that the subgroups of raters (e.g. laypersons without cleft N = 2) was small, but they justified this small size by citing the work of others, rather than performing a power analyses or a priori estimation of sample size (Cohen, 1988). Subject recruitment, bias and obtaining a representative sample are major considerations and often the most difficult portions of conducting a project of this type. In addition, it is mentioned that due to logistical challenges, a reliability study was not possible, but given the very small sample size of raters, a determination of intra-rater reliability would have been critical.

While we agree with Foo et al. that it is important to know the differences in how people perceive treatment outcomes, we feel that there were some major limitations to this study, which might have considerable impact on the results. It would be desirable if the authors could consider repeating this study with a sufficient sample size of raters.

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References


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