Letter to the editor, Roberts et al.2014

“It is vain to do with more when less will suffice”. The authors should be applauded for advocating this principle of parsimony. Regretfully, they have used Ockhams razor imprecisely: relevant issues are cut away, whereas irrelevant ones are spared.

First, they have reduced the problem of age estimation based on multiple indicators to the mere calculation of a point estimate, hereby ignoring that the prediction interval (“the range of possible values”) is an essential element in age estimation.

Second, the authors have explored various weighting schemes, but restricted the evaluation to the quantification of the bias (i.e. the mean difference between chronological and dental age). Systematic information on the precision (as visualized in the Bland Altman plot, Fig.3) is missing for mostweighting schemes.

Third, there is no need of contrasting the “weighted” with the “meta-analytic” averages, since the latter are also nothing more than weighted averages. When multiple estimates are combined into a single estimate, the classical solution is a fixed-effects meta-analysis where each estimate is given a weight inversely proportional to its variance (Sutton 2000, Section 5.2). Roberts et al. woulddenote this weight as *1/var-tds*. For the random-effects meta-analysis two sources of variance determine the weights: the *1/var-tds* as in the fixed-effects meta-analysis and the variability between the various age estimates (Sutton 2000, Section 5.2).Clearly, not only the fixed-effects, but also a random-effects meta-analysis can be implemented in Excel, thus there is no need at all to switch to a statistical package as STATA.

Fourth, the classical weight*1/var-tds* is even not amongst the considered ones. Instead, many of the proposed weighting schemes do not merit investigation in the first place. What can be the logic behind the use of the standard deviation (*sd-tds*) or the standard error (*se-tds*) as weights? Using these as weights will favor age estimates with the largest uncertainty. Further, the use of *1/se-tds* as weightshould not be allowed. Suppose an uninformative age indicator, but based on a large reference dataset compared to the other age indicators. As a result, this age estimate will have a small standard error, hence a large weightinappropriately dominating the final solution. Roberts et al. justify the choice for the use of the standard error ‘because it incorporates sample size”. The correct way to do this is not to use the observed variability (*sd*, *var*) in the weights, but the variability of the individual predicted value, *varp=var*(1+1/n)*. Thus, in a large reference databases *var* will approximate *varp*. In summary, to obtain a combined point estimate, a fixed and/or random-effects meta-analysis with 1/varp as weights can be considered. But as mentioned before, the proposed method of weighted averages does not help in obtaining the requiredinterval estimation.

Finally, apart from the criticism concerning the content, the paper contains calculation errors. Table 1 presents results which are not possible. For example, the weighted average with 1/sd-tds as weight results in a weighted mean (7.85 years) which is even lower than the lowest of the separate age estimates (7.87 years for UL7).

References:

Sutton AJ, Abrams KR, Jones DR, Sheldon TA, Song F. *Methods for Meta-analysis in Medical Research*. Chichester (UK): John Wiley & Sons, 2000.