

Letter to the Editor

Data, methodological and referencing concerns in a cross-sectional study of oral health among preschool children attending Anganwadi centers

Sir,

The cross-sectional study by Pullagurla et al, published in the International Journal of Community Medicine and Public Health, examined oral health status, dietary habits and parental perception among 202 preschool children attending Anganwadi centers in Vikarabad district.¹ The topic is genuinely important: dental caries remains the most prevalent chronic disease of early childhood and data from rural Integrated Child Development Services settings are scarce. However, a close reading of the tables, methods and reference list reveals several internal inconsistencies that should be corrected before the findings inform practice or policy.

The gender distribution in (male 103, reported as 51.1%; female 99, reported as 48.7%) does not match the underlying counts. Correctly calculated, 103/202 equals 50.99% and 99/202 equals 49.01%; the reported figures neither reflect these values nor sum to 100%. A more consequential issue lies in the diet-caries cross-tabulation. For “coffee with sugar,” children who consumed the item frequently showed 0% caries prevalence (0/23), against 16.8% among infrequent consumers (30/179), a statistically significant but biologically implausible reversal of the dose-response relationship between fermentable sugar exposure and caries that the cariology literature otherwise describes.^{2,3} The Discussion nonetheless groups this result with the milk-with-sugar and snack-food findings as mutually “consistent,” without flagging that its direction is reversed. A related concern affects the “potato, chips/snacks/peanuts” analysis: the infrequent-consumption stratum contained only two children, both with caries. A p-value generated from a 2-versus-200 comparison is statistically fragile and presenting it as evidence of association without a small-cell caveat overstates its reliability.

The methods describe Anganwadi centers as sampling clusters, yet the procedure, selecting a center “from the available list” and enrolling “all eligible children present on the day of examination,” more closely resembles convenience sampling within a single center than a randomized multi-cluster design. If cluster sampling was genuinely intended, the sample-size formula should have incorporated a design effect derived from the intra-cluster correlation coefficient; the reported calculation, based solely on the pilot prevalence of 16%, does not appear to

do so, which would understate true sampling error and produce falsely narrow confidence around the prevalence estimate.⁴ Reliability is asserted (“intra-examiner reliability was acceptable”) without a reported kappa coefficient, the accepted metric for quantifying agreement beyond chance.⁵ Because a single examiner used only WHO 2013 visual-tactile criteria without radiographic or ICDAS-based confirmation, early non-cavitated and approximal lesions were likely missed, which would underestimate true caries prevalence.⁶ Finally, diet-caries associations were tested only with univariate chi-square comparisons; no multivariable model adjusted for age, sex or household-level confounders that plausibly influence both diet and caries risk.

Two problems affect the bibliography’s internal consistency. First, the Discussion attributes a finding on dental-service utilization to “Priya et al” and cites reference 14, but reference 14 in the list is Petersen PE’s 2003 World Oral Health Report, not a Priya et al. Publication, the in-text citation and the reference-list entry do not correspond and should be reconciled before indexing. Second, reference 9 (the WHO oral health assessment form for children) links to a URL hosted on a third-party document-aggregator domain rather than who.int, which is neither a stable nor an authoritative source for a WHO instrument and should be replaced with the official WHO citation.

Several established determinants of early childhood caries were not captured. Nocturnal or prolonged bottle and breastfeeding, a well-documented risk factor for caries in this exact age group, is absent from the questionnaire, despite meta-analytic evidence linking bottle feeding to elevated caries risk.⁷ Household socioeconomic status and parental education, both established effect-modifiers of the diet-caries relationship, were not collected. Caries severity (dmft/dmfs) was not reported, only presence/absence, discarding clinically meaningful information on lesion burden that the WHO Basic Methods protocol itself supports collecting.⁸ Inter-examiner reliability is not applicable given the single-examiner design, but this constraint limits generalizability and should be stated explicitly as a limitation. Finally, because sampling appears confined to very few centers, the claim of representativeness across “Vikarabad district” is not adequately supported and should be tempered in the abstract and conclusion.

This study addresses a genuine public health gap in rural preschool oral health surveillance, but several issues limit confidence in its findings as presented: the gender-percentage miscalculation, the unexplained reversal in the coffee-with-sugar analysis, small-cell instability in the snack-food analysis, the cluster-sampling/design-effect mismatch, the unreported kappa statistic, the reference-14 mismatch and the non-official source for reference 9. Incorporating feeding history, socioeconomic status, caries-severity indices and multivariable confounder adjustment in future iterations of this research would substantially strengthen the evidence base for oral health interventions delivered through Anganwadi centers.

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