For example in their 'retrospective' study publications Nafiu *et al.* (1,2) have chosen to specifically exclude children with Prader–Willi syndrome – certainly a 'high-risk population' among obese children who will present to pediatric anesthesiologists as previously highlighted by Legrand and Tobias (4). In an article titled 'Childhood obesity and anesthetic morbidity' Setzer and Saade (3) concluded (another 'retrospective'study) that '…inhalation induction was not associated with an increase in adverse events in this population' – yet their population of 100 obese children presenting for elective dental procedures included only ASA I and II patients, none of whom was morbidly obese! While the authors clearly stated that 'caution should be used in extrapolating our conclusions to children we consider to be morbidly obese', this was somewhat hidden on the 5th page of the paper and only a discerning reader would realize that a significant subpopulation (i.e. the 'morbidly obese') was omitted from the study population…

Another such example relates to the occurrence of 'bronchial asthma' mentioned by Nafiu *et al.* (1) as a significant comorbidity in the obese pediatric population. In a recent review of pulmonary disease in the obese pediatric population from the United Kingdom, Deane and Thomson (5) discuss in great detail that 'asthma'– by definition –'…is characterized by increased airway responsiveness with chronic inflammation, resulting in reversible airway obstruction', and that the wheezing often noted in obese children is not 'asthma' as such, and that ' …when objective assessments of lung function and airway reactiveness were performed, there was no correlation with obesity'! These authors specifically address children with Prader–Willi syndrome because of their higher risk of obstructive sleep apnea morbidity including sudden death. It seems to me that it is inappropriate to exclude such patient subpopulations when publishing major articles on a topic such as pediatric obesity. Obviously there is too much variation and laxness in our definitions (e.g. obese, asthma, etc) to be able to evaluate manuscripts and compare them one to another; if this persists we will have a 'literature on pediatric obesity' that is worthless or worse, filled with errors and omissions.

While I applaud Nafiu *et al.* and Setzer and Saade for focusing our attention on the obese pediatric population, and identifying such issues as 20% of morbidly obese patients being assigned ASA values of I or II, and the realization that mask inhalational inductions are indeed a necessary part of pediatric anesthesia practice when intravenous access might be difficult or impossible and awake intubation is not really an option, I would urge that subsequent research efforts emphasize prospective studies such as by Kalra *et al.* (6) on obstructive sleep apnea in morbidly obese children. The terminology used must be consistent if at all possible; even the definitions of obesity used by Nafiu *et al.* and Setzer and Saade are not identical!

As more studies are performed we must seek to achieve better uniformity – for example does the patient with BMI of 45 share the same risks of another with BMI of 55? What are the ramifications of implied risk for elective or semi-elective procedures in these 'different' populations, what needs will these patients have with their increased risks of wound infection, fistulae, etc. What staffing and technical considerations (7) must we make in caring for these patients (e.g. increased anesthesia staffing to achieve airway management if two hands are necessary for effective mask ventilation in the difficult airway, prolonged scheduling time for intravenous access, purchasing appropriate sized and strengthened transport stretchers, wheelchairs, etc.)?

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## Author's reply

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SIR—I am grateful for the opportunity to reply to Dr Morrison's letter and particularly for his interest in our work on childhood obesity and anesthetic implications. In one study, we described the secular trend in the obese pediatric surgical population (1), while in the other we examined the occurrence of perioperative complications (2).

Dr Morrison was concerned that we excluded patients with Prader–Willi syndrome (PWS) from our study population and that this was a 'significant' subpopulation of

obese children. While I agree that extreme obesity is one of the classic features of PWS, I think these are the unique group of patients who should be studied separately as detailed by Legrand and Tobias in their case series (3). The focus of our study was primary obesity; to include syndromic and other secondary causes of obesity in the study population will be a fundamental design flaw. This would be akin to including children with diabetes in a study of perioperative hyperglycemia. In a retrospective study (or all study designs for that matter), every attempt ought to be made to exclude potential sources of bias. Therefore, we would strongly suggest that studies on childhood obesity should exclude those with secondary obesity who should be studied as a separate subgroup. We must 'compare apples with apples.'

Dr Morrison was also concerned about the definition of bronchial asthma in our study population. Unfortunately, as practicing anesthesiologists, patients come to us with a 'diagnostic label.' We do not have the luxury of measuring airway reactivity and formal lung function tests. Our definition of asthma based on patient's history and use of medications is a clinically useful and legitimate one. The literature on obesity and childhood asthma is inconclusive; whereas, some authors describe an association, others found none. Our study, only described what we had in our large retrospective database. It would be interesting to study the subject prospectively.

Dr Morrison's points about the role of body mass index (BMI) in defining obesity and the role of morbid obesity and ASA status are interesting. As pointed out in our paper, morbid obesity is *not* a pediatric diagnosis; that data were presented to highlight the difference between how anesthesiologists perceive obesity in the adults vs how it is viewed in children. The debate about whether BMI is a good measure of adiposity is ongoing. Most researchers agree that, while BMI is not the best measure of adiposity, it is by far the most clinically useful and does serve as a useful discriminator. The situation is further complicated in children because BMI varies with age making it impossible to have single cut-off values.

The widely used adult cut-off points—BMI of 25 $kg{\cdot}m^2$ for overweight and 30 $kg{\cdot}m^2$ for obesity are related to health risks (4) but are also convenient round numbers. Such convenient numbers and disease association are unfortunately, not easily discernable in children. Instead age and gender-specific BMI percentile charts are commonly used: 85th and 95th percentiles for overweight and obese respectively (5). Others have suggested using the BMI *z*-score for presenting data on childhood obesity because this allows BMI to be treated as a continuous variable and makes analysis easier. However, this technique has limited clinical applicability for it eliminates useful terms like overweight and obesity, which most clinicians are used to. The rationale behind the definition of obesity used by Setzer *et al.* in their paper (6) is well detailed by Cole *et al.* (5) and we refer readers to this excellent article.

Finally, I could not agree more with Dr Morrison that the subject of 'childhood obesity and anesthetic implications' is in its infancy and efforts should be made to study the subject prospectively. However, well-designed retrospective studies and case series are extremely useful in drawing attention to the subject and often serve as useful take-off points for future research. Any information on the subject will help to bridge the yawning gap between published reports in adult obesity vs childhood obesity.

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## Anesthesia management of a patient with trichothiodystrophy

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Sir—We would like to describe our recent experience anesthetizing a child with trichothiodystrophy, which is a group of autosomal recessive disorders characterized by brittle hair and nails which are sulfur deficient. Phenotypes can vary from brittle hair only to severe mental and somatic growth retardation. The patients usually have short and brittle scalp hair, eyebrows and eyelashes with characteristic facial features of receding chin and protruding ears. Fifty percent of the patients display photosensitivity (1–6).

Our patient was a 6-year old female child with trichothiodystrophy, who presented for tonsillectomy and adenoidectomy for chronic tonsillitis. Her weight was 15.4 kg

 