Obstructive sleep apnoea and perioperative complications in bariatric patients

Editor—I read with great interest the article by Weingarten and colleagues. The authors reported data on 797 patients undergoing bariatric operations and concluded that the severity of obstructive sleep apnoea (OSA) is not associated with the rate of perioperative complications. I would like to point out two limitations of the study that were not discussed but may have had significant impact on the results. First, the non-uniformity of surgical procedure may have influenced their results, although the hospital length of stay among groups was similar. This only indirectly indicates that a similar number of patients underwent banding, sleeve gastrectomy, or gastric bypass surgery. The second point has direct implications on the authors’ conclusion. The utilization of continuous positive airway pressure (CPAP) increased significantly with the severity of OSA, indicating that patients who were more likely to experience a subjective benefit from the treatment. The clinical goal of CPAP treatment is to reduce the apnoea–hypopnoea index (AHI). A patient with severe OSA without CPAP may only have an AHI indicative of mild or no OSA with CPAP. Obviously, an increase in the AHI of patients treated for OSA with CPAP do not show an increased risk of postoperative complications.

Conflict of interest

P. Z.-G. is Speaker for Cadence, Speaker for Baxter, Shareholder Cadence, and Johnson & Johnson.

Reply from the author

Editor—We wish to thank Dr Ziemann-Gimmel for his interest in our study examining the relationship between obstructive sleep apnoea (OSA) and complications in obese patients undergoing bariatric surgery. He raises two important questions. First, the overall number of patients undergoing laparoscopic gastric banding and sleeve gastrectomy surgeries comprised only 2.5% of our entire cohort (19=bandings and 1=sleeve). The length of stay after these two procedures was 1.6 days. In response to this letter, we performed an additional analysis of the length of stay among patients who underwent laparoscopic surgery excluding those patients who underwent banding and sleeve procedures. Mean length of stay with the new analysis was 3.9 days, compared with 3.7 days reported in our initial study. As with our initial reported analysis, length of stay did not vary by OSA severity (P=0.27).

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Secondly, we completely agree with Dr Ziemann-Gimmel that our results are not to be interpreted that untreated or unrecognized OSA is not associated with increased risk of complications. Our manuscript explicitly states this point. Our results are only applicable to those obese patients evaluated before bariatric surgery by polysomnography and their obesity-related sleeping disorder managed accordingly in the postoperative period.

Conflict of interest
None declared.

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Efficacy of pregabalin in acute postoperative pain: a meta-analysis

Editor—We read with interest the meta-analysis on the efficacy of pregabalin in acute postoperative pain.1 However, we would like to highlight some of our concerns about the study.

Although the authors have mentioned about the limitations in their study, it would have been perhaps better if they had performed a subgroup analysis on morphine consumption, depending on the different types of surgery in which pregabalin has been used, because not all operations have the same opioid requirement after operation. In the studies where intraoperative opioids have been given,2–7 the authors did not provide a subgroup analysis of whether there was a reduced requirement for intraoperative opioid in the group of patients having had preoperative pregabalin. We found it surprising that the authors chose to analyse opioid consumption where pregabalin had been administered both 1 h before operation and 12 h after operation6 along with studies2–5,7 in which pregabalin was only administered 1 h before operation. Certainly, these cohorts of patients would have had varying postoperative requirement for opioids.

We noted that studies have been included where intraoperative opioids,3–5,7 acetaminophen,8 and non-steroidal drugs2,4 have been given either before operation or as an infusion after operation4 and yet a subgroup analysis has not been undertaken to elicit an influence of these analgesics on the efficacy of pregabalin.

The authors did not take into consideration the use of ondansetron, droperidol, and dexamethasone,2,6 while considering the effect on postoperative nausea and vomiting of pregabalin, when all three drugs are known to reduce postoperative nausea and vomiting.9

The number of patients in the control group in Figures 3 (24 h morphine consumption), 4 (VAS postoperative pain intensity), and 7 (nausea, vomiting, dizziness and headache, and visual disturbance) have been duplicated thereby creating a unit-of-analysis error. This could have been avoided by either splitting the shared group resulting in a smaller sample size and including two or more comparisons, by combining groups to create pair-wise comparisons, or by undertaking a multiple treatment analysis.10

Conflict of interest
None declared.

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doi:10.1093/bja/aer208


doi:10.1093/bja/aer207