

anaphylaxis, whether it is rocuronium triggered or not [1], acknowledging the lack of existing evidence and practical difficulties of obtaining it.

I would like the authors to clarify the question they were seeking to answer in their paper. They state that they wanted to compare cases treated with sugammadex and subsequently confirmed to be triggered by rocuronium, with those that were not. However, they draw the conclusion that sugammadex cannot be relied upon as an effective treatment in anaphylaxis and does not modify the immunological cascade of a suspected hypersensitivity reaction. Do the authors consider that their study was adequately designed to either confirm or refute this?

Secondly, I question the rigor of the peer review process for the paper. It is clearly stated in the title that this is a case-control study, although the method used is far from that of a case-control study. Indeed, there has been no search and selection of cases to use as controls. Those presumed to be controls are a subset of those identified while searching for cases. This is unfortunate, as a well designed, case-control study would have been a very appropriate methodology (providing one of the higher levels of evidence achievable) to gather evidence on the use of sugammadex in suspected anaphylaxis due to rocuronium, given the low frequency of cases. Have the authors considered identifying a group of suitable controls and using that as a comparison for the cases already identified?

Nevertheless, this case series provides interesting reading in the run up to the imminent NAP6 study on peri-operative anaphylaxis.

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Sugammadex in anaphylaxis – a case-control study? A reply

We thank Dr. Whitehead for his interest in our paper [1] and his acknowledgement that the publication is timely with the 6th National Audit Project (NAP6) study on anaphylaxis underway. However, we disagree with Dr. Whitehead's comments about our design and conclusions.

We compared cases of anaphylaxis treated with sugammadex and subsequently confirmed to be triggered by rocuronium, with those not triggered by rocuronium. The purpose of the comparison was to be able to determine by implication whether sugammadex can be relied

upon as an effective treatment in anaphylaxis by modifying the immunological cascade of a suspected hypersensitivity reaction. As there appeared to be no obvious difference in the frequency of confirmed rocuronium-induced anaphylaxis by response to treatment with sugammadex, we had to conclude that this was not the case.

Our study was a case-control study insofar as we compared patients with a known pre-specified outcome (improvement in blood pressure after the administration of sugammadex (cases) vs. no improvement (controls)), and then determined the frequency of confirmed rocuronium-induced anaphylaxis in these two groups (3/6 vs 5/7, respectively). Dr. Whitehead is correct that the controls were a subset of patients in whom sugammadex had been administered in an attempt to modify the course of rocuronium-induced anaphylaxis. However, we disagree with him that this was an inappropriate methodology. All the anaesthetists referring cases for analysis felt that they were dealing with rocuronium-induced anaphylaxis and administered sugammadex. Some of the anaphylaxis cases were subsequently found to be not rocuronium-based, or not anaphylaxis, and were treated as controls. We are unclear how to improve this methodology to search for and select suitable controls.

We do not agree with Dr. Whitehead's criticism of the peer-review process. We were very appreciative of the peer review we received, including the many helpful comments, and were grateful for the opportunity to publish this

information on the apparent lack of efficacy of sugammadex in the management of rocuronium-induced anaphylaxis. This is consistent with in-vivo experiments and a rational appraisal of the pathophysiology of anaphylaxis [2, 3].

This is the first study that has examined the efficacy of sugammadex in presumed anaphylaxis that includes a control group. As we provided all our data in a summary table, as well as comprehensive case details in the online version of the article, readers will be able to formulate their own conclusions.

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TAP blocks for laparoscopic appendicectomy

The trial by Tanggaard et al. of bilateral dual transversus abdominis plane (TAP) blockade in emergency laparoscopic appendicectomy adds evidence about the management of this common emergency procedure, but we would like to invite the authors to comment on several methodological issues that we would like to raise [1].

Firstly, the efficacy of nerve blockade by sensory testing postoperatively does not appear to have been tested by a blinded observer. How confident are the authors in drawing conclusions about the usefulness of bilateral TAP blocks without assessing whether the block had been technically successful?

Secondly, not all patients in this study had the same operative procedure. A significant proportion underwent diagnostic laparoscopy rather than laparoscopic appendicectomy. In hindsight, do the authors think that these patients should have been excluded?

A further confounding variable relates to the use of surgical infiltration of laparoscopic port sites with 20 ml bupivacaine 0.25% in all patients. This should be superfluous in patients with working bilateral dual TAP blocks [2], and merely exposes the patient to higher doses of local anaesthetic. This approach may also have reduced the likelihood of finding clinically significant differences in analgesia between groups. Did the authors consider using blinded sham infiltration of the port sites in the ropivacaine

group and local anaesthetic infiltration in the saline group?

The side effects of opioids are well documented [3] and this study aimed to demonstrate reduced morphine requirements following bilateral dual TAP blocks. However, both saline and ropivacaine groups were given a large fixed dose of morphine intra-operatively. It is therefore not surprising that no significant differences were observed in time spent in recovery or post-operative nausea and vomiting (PONV) scores. Was morphine also administered pre-operatively, for example, in the emergency department, which may also have affected these results?

Despite PONV being a secondary endpoint of the trial, we note that prophylactic antiemetic treatment was not standardised. Do the authors agree that conclusions cannot therefore be drawn, about PONV from these results?

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