ml for "moderate to large." In addition, Table 2 displays information on laterality: Pleural effusion was bilateral in 17/79 (21%) patients with weaning success and in 12/57 (22%) patients with weaning failure. As per request by Dr. Iwasaki *et al.*, we provide here the height of our patients, which was 168 ± 14 cm in patients with "moderate to large pleural effusion" and 168 ± 24 cm in patients with "no or small pleural effusion." Later, Dr. Iwasaki *et al.* suggested that the impact of pleural effusion might differ according to the postextubation ventilation strategy: non-invasive ventilation, high-flow oxygen, or standard oxygen. Although we definitely share the concerns raised, we are unable to address this issue. A comprehensive understanding of the interaction between postextubation ventilation strategy and the impact of pleural effusion would require specific measurements of breathing pattern and lung mechanics. Given that our study was mostly observational, we did not aim at investigating this question. Dr. Iwasaki *et al.* suggested that our findings would have been different if, rather than comparing weaning success *versus* weaning failure, we had compared success *versus* failure of spontaneous breathing trial. In response to this comment, we reassessed our data and found a "moderate to large" pleural effusion in 7/45 (16%) of patients who failed the spontaneous breathing trial and in 11/91 (12%) of patients in whom the spontaneous breathing trial was successful ($P = 0.60$).

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Competing Interests

Research contracts with Medtronic (Dublin, Ireland; to Dr. Dres), Maquet (Rastatt, Germany; to Dr. Dres), and Philips (Amsterdam, The Netherlands; to Dr. Dres). Dr. Dres also has received personal fees from Medtronic (Dublin, Ireland), Maquet (Rastatt, Germany), and MSD (Courbevoie, France). Dr. Demoule received personal fees from Pulsion Medical System (Feldkirchen, Germany) and Astra Zeneca (Cambridge, United Kingdom).

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One Size Fits All for Stress-dose Steroids

To the Editor:

Liu *et al.* provide a thorough review of perioperative steroid replacement and make evidence-based recommendations to help clear up the "confusing" recommendations about who needs "stress-dose" steroids, what agent to administer, and how much to administer.[1] They report that there is limited evidence that such supplementation is necessary, but continue on to provide an algorithm for how much hydrocortisone to give at-risk patients based on anticipated surgical stress. They also point out that mineralocorticoid deficiency does not occur in secondary adrenal insufficiency (*i.e.*, due to chronic exogenous steroid administration). They also indicate that administration of hydrocortisone can result in excess mineralocorticoid activity with resulting (and undesirable) fluid retention and hypokalemia.

The lack of evidence, clinical confusion, and adverse effects of hydrocortisone seem to beg for a simpler solution. As it happens, there is one: dexamethasone 4 (or 8) mg. The 30+ fold glucocorticoid potency compared with hydrocortisone, absence of mineralocorticoid activity, and longer half life seem to make it a superior agent for perioperative supplementation for any level of stress. Unlike the limited evidence of need for stress-dose steroids, or for an antiemetic effect of hydrocortisone, the evidence of efficacy and safety of dexamethasone for prevention of postoperative nausea/vomiting (PONV) is extensive.[2,3] Since most of our patients have one or more risk factors for PONV, administering dexamethasone is usually indicated even without a question of adrenal insufficiency. Therefore, administering a PONV prophylaxis dose of dexamethasone seems like a simple, one-size-fits-all algorithm for dealing with any concern about secondary adrenal insufficiency.

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In Reply:

We thank Dr. Wax for his response to our recent article on perioperative steroid management.[1] Since the publication of our article, we have received several queries regarding the use of dexamethasone as a perioperative stress-dose steroid and appreciate the opportunity to further address this topic. As Dr. Wax aptly notes, dexamethasone has significantly more glucocorticoid potency than hydrocortisone, has no mineralocorticoid effect, and can be clinically effective in the prevention of postoperative nausea and vomiting. Indeed, the recommended antiemetic dose of dexamethasone (4 mg) has at least the same glucocorticoid equivalence as the recommended intraoperative stress dose of hydrocortisone (100 mg) for patients at risk for adrenal insufficiency undergoing major surgery.[1] The available literature on perioperative steroid supplementation provides dosing guidelines based on hydrocortisone, which has a shorter, more predictable half life compared to dexamethasone and is thus more easily tapered to the usual daily dose in patients requiring continued postoperative supplementation based on surgical stress. However, the literature on patients with *secondary* adrenal insufficiency does not make any specific recommendation as to what is the "best" stress-dose steroid to administer. Dexamethasone is not appropriate for patients with *primary* adrenal insufficiency or critically ill patients, both of whom require mineralocorticoid supplementation.[2,3] While we agree that the use of dexamethasone may be a reasonable approach for many patients with *secondary* adrenal insufficiency, with additional benefit in the prevention of postoperative nausea and vomiting, we caution against a "one-size-fits-all algorithm," especially in critically ill patients.

Competing Interests

The authors declare no competing interests.

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Volume Responsiveness Alone Is Not an Indication for Volume Administration!

To the Editor:

It is with intrigue that we read Gómez-Izquierdo *et al.*'s paper demonstrating the lack of effectiveness of goal-directed fluid therapy (GDFT) in reducing ileus after elective laparoscopic colorectal surgery.[1] We congratulate the authors for a well-done study and Anesthesiology for publishing an important negative trial. There are a few points we would like to discuss.

First, these authors join an increasingly large number of research groups whose results call into question the value of GDFT in mitigating complications and reducing hospital length of stay or cost after elective surgery. Specifically, several previous reports, and now that of Gómez-Izquierdo *et al.*, collectively force us to critically examine the *general applicability* of GDFT in today's surgical patients. Although GDFT has been shown to mitigate postsurgical complications in studies spanning three decades,[2] its effectiveness in reducing postsurgical morbidity in patients on enhanced recovery pathways appears limited.[3] Additionally, traditional proponents of GDFT recently have questioned its value within enhanced recovery.[4,5] Even staunch proponents of standardized, best-evidence clinical pathway design and implementation have questioned the acceptance of all enhanced recovery elements without continued individual element evaluation.[6,7] To be sure, the laparoscopic approach, avoidance of dehydrating bowel preparations, and clear liquid consumption until 2 h before surgery all play important roles in reducing the volume shifts that were typical of traditional surgical procedures. To these points, we agree with Gómez-Izquierdo *et al.* that important advancements in perioperative care have diminished the positive impact of GDFT.

Second, the implemented GDFT approach is not in line with the referenced perioperative fluid therapy consensus statement, which details a logical two-step rationale for intraoperative fluid administration. "First, determine if the patient requires hemodynamic support or augmentation of cardiovascular function. Second, if the need is apparent and the patient is fluid responsive, fluid bolus therapy should be considered."[8] As recently penned by Takala, "giving volume