Anesthesia and Ullrich Congenital Muscular Dystrophy: Comment

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To the Editor:

I read with great interest the short report published in the Images in Anesthesiology section about difficult intubation in a 2-yr-old patient with Ullrich congenital muscular dystrophy.[1] This disease is well known to carry a risk of difficult intubation.[2–6] The authors nicely described how they used a nasopharyngeal airway to administer a volatile anesthetic and oxygen through one nostril while performing nasotracheal fiberoptic intubation *via* the other. I am however surprised that the choice of the anesthetic agent(s) used was not discussed. Ullrich congenital muscular dystrophy indeed belongs to the subgroup of the collagen type 6–related myopathies occurring after a mutation of the COL6A1, COL6A2, or COL6A3 gene. Collagen type 6 is part of the large complex that anchors the basal lamina and the interstitium in muscle cells. The myopathy is probably caused by the muscle membrane fragility and an associated mitochondrial dysfunction, which can be decreased with cyclosporine A. As collagen 6 is close to the dystrophin-glycoprotein complex, this muscle disease could be at risk of anesthesia-induced rhabdomyolysis in the presence of halogenated agents or succinylcholine, as are children with Duchenne or Becker progressive muscular dystrophy. Very few reports on the anesthetic management of patients with this type of myopathy have been published so far and all except two[5,6] report using total intravenous anesthesia.[2–4] Carefully titrated intravenous anesthesia to maintain spontaneous ventilation and using either dexmedetomidine, propofol, and/or ketamine could therefore be a safe alternative.

Competing Interests

Dr. Veyckemans reports a financial relationship with *European Journal of Anaesthesiology*'s Editorial Board and *Pediatric Anesthesia*'s Editorial Board.

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This letter was sent to the author of the original article referenced above, who declined to respond.—Evan D. Kharasch, M.D., Ph.D., Editor-in-Chief.

Goal-directed Therapy and Postcystectomy Ileus: Comment

To the Editor:

We read with interest the recently published study by Arslan-Carlon *et al.* in Anesthesiology.[1] This primary objective of this randomized, controlled trial (N = 283) was to determine whether a goal-directed fluid therapy approach would result in a lower incidence of postoperative ileus compared with a standard fluid therapy approach

in patients undergoing open radical cystectomy. All patients had an arterial line coupled to an advanced hemodynamic monitoring device (EV-1000, Edwards Lifesciences, USA) to monitor advanced hemodynamic variables before, during, and immediately after surgery. No statistically significant difference in this primary outcome was found between the two groups. Interestingly, however, the authors identified a relative negative impact of goal-directed fluid therapy on acute kidney injury (AKI) incidence (secondary outcome) with more patients in the goal-directed fluid therapy group who experienced AKI (56% *vs.* 40%, $P = 0.005$). We do appreciate the statistical insignificance noted after taking into account Bonferroni adjustments ($P = 0.170$) because this is rarely done. We congratulate the authors for having performed such a rigorous study. We have, however, two main comments for any interested readers.

First, although many clinicians throughout the world may use a similar hemodynamic algorithm that calls for fluid, vasopressor, or inotrope administration if stroke volume variation is greater than 12%, mean arterial pressure (MAP) less than 60 mmHg, or cardiac index less than 2.5 $l \cdot min^{-1} \cdot m^{-2}$, there have been at least two large randomized, controlled trials demonstrating that targeting a higher arterial pressure during surgery is associated with less postoperative AKI. The first, from Futier *et al.*,[2] reported a lower incidence of organ dysfunction in the group of patients with a targeted systolic arterial pressure closer to the patient's baseline value compared with the control group. The second, from Wu *et al.*,[3] demonstrated that targeting a MAP level between 80 and 95 mmHg in chronically hypertensive patients reduces postoperative AKI compared with two other MAP levels (65 to 79 and 96 to 110 mmHg). Additionally, French national guidelines recommend maintenance of a MAP target greater than 70 mmHg to prevent AKI in patients with chronic hypertension (which is the case in more than 60% of the current study population).[4] As a result, the MAP level targeted in the current hemodynamic algorithm may have been too low and could be responsible for the higher incidence of AKI.

Second, because both groups were monitored with an advanced hemodynamic monitor, we would have greatly appreciated summaries of advanced hemodynamic variables to better understand the results. It would be useful if the authors could provide mean MAP, stroke volume, cardiac index, and stroke volume variation during the case and also the percentage of procedure time the patients were hypotensive as defined using their algorithm of MAP less than 60 mmHg and the percentage of procedure time with a stroke volume variation greater than 12%. It is quite evident that clinicians' compliance with the application of hemodynamic protocols remains poor, ranging between 62% and 87%, even within ideal study conditions.[5,6] Compliance of less than 50% to protocols is reported in daily practice across different medical specialties, but at least 80% adherence is required to observe improved clinical outcomes.[7–9] As a result, having hemodynamic data would allow readers to assess protocol compliance and determine whether advanced hemodynamic variables were well optimized throughout the perioperative period.

Competing Interests

Dr. Joosten has been a consultant for Edwards Lifesciences (Irvine, California), Aguettant Laboratoire (Lyon, France), and Fresenius Kabi GmbH (Bad Homburg, Germany). Dr. Legrand has received consulting fees from Novartis (Bale, Switzerland), lecture fees from Baxter (Lessines, Belgium) and Fresenius Kabi GmbH, and research support from Sphingotec (Hennigsdorf, Germany). The other authors declare no competing interests.

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Goal-directed Therapy and Postcystectomy Ileus: Comment

To the Editor:

We read the article by Arslan-Carlon *et al.*[1] with great interest. The authors are to be congratulated for their research on the impact of fluid therapy on postoperative ileus. In this recent randomized, controlled trial including 283 patients, they found no difference in the incidence of postoperative ileus between patients treated with a goal-directed therapy compared with a standard procedure in a homogenous open radical cystectomy patient cohort from a high caseload center.

The pathogenesis of postoperative ileus is clearly multifactorial (fluid overload, opioids, neurohormonal dysregulation, gastrointestinal stretch, inflammatory response).[2,3] In the era of enhanced recovery protocols including multiple preoperative, intraoperative, and postoperative optimization steps, it is not surprising to find that intraoperative goal-directed therapy alone has no impact on the return of bowel function compared with a moderate liberal fluid standard fluid administration. Furthermore, it remains questionable whether postoperative fluid substitution should be uniformly managed in patients who received either an intraoperative goal-directed therapy or a restrictive or relatively liberal fluid administration. Indeed, in this study net fluid during the hospitalization was higher in the standard group (−1,986 ml *vs.* −1,296 ml) but resulted in similar maximum body weight changes (2.7 kg *vs.* 3.0 kg) and could be interpreted as the consequence of a more aggressive diuretic therapy in the standard group postoperatively. This is of importance because postoperative submucosal edema has been postulated as a risk factor for a delayed return of gastrointestinal function.[3] Unfortunately, the authors did not give any information on the postoperative administration of diuretics.

The problem of adequate terminology is another ongoing issue. We were surprised by the authors' comment in the discussion that the term *constipation* was not adequately described in the article by Wuethrich *et al.*, as both the term *constipation* as well as the term *ileus* were defined in the appendix. Constipation was defined as no passage of stool without signs of ileus by postoperative day 5 and could be considered similar to what was considered a primary postoperative ileus in the article by Arslan-Carlon *et al*. This definition was based on the nomenclature resulting from a well-performed case series analysis aiming to standardize complications after cystectomy from the Department of Urology at the Sloan-Kettering Cancer Center (New York, New York).[5] Perhaps the authors could specify why they did not apply their own above-mentioned definition in the present study. We recognize that the publication by Shabsigh *et al*. is more than 10 yr old, but the goal of a good standardized reporting methodology should be its continued long-term applicability. The Clavien Dindo classification remains the best example hereof.[4,6] In the context of prevention of postoperative ileus, it would also be interesting to know which opioid antagonist was administered subcutaneously perioperatively.

Finally, no data are presented about the systemic administration of opioids, a known risk factor for delayed return of bowel function. We only learn that patients received an epidural analgesia with a mixture containing a very low dose of bupivacaine (0.05%) and a relatively high dosage of opioid (8 μg/ml hydromorphone).

In conclusion, the saying "one size does not fit all" can also be applied to fluid therapy. This study is of importance because it shows no benefit of goal-directed therapy in terms of reducing gastrointestinal-related complication rates. A more selective and differentiated approach in fluid management is needed. In some cases, restrictive fluid therapy may be beneficial, and in other cases, a modestly liberal fluid administration resulting in a postoperative weight gain of approximately 2 to 3 kg would make no difference in outcome. Fluid management is only part of a complex battery of interventions affecting outcome after open radical cystectomy.

Competing Interests

The authors declare no competing interests.

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Goal-directed Therapy and Postcystectomy Ileus: Reply

In Reply:

We thank Joosten *et al.*[1] for their letter to the editor. We read their comments with interest.

We realize that at the time of publication, work had been published on the association between acute kidney injury (AKI) and blood pressure targets in the operating room; however, when the study was designed, this association had not yet come to light. For this reason, we did not incorporate hemodynamic parameter analysis in our primary outcome.

In response to the second comment, the protocol was executed by a limited number of anesthesiologists, all of whom had contributed to the design of the protocol, which should have assured a greater compliance. But we will have a more definitive answer on compliance rate after we have finished our in-depth analysis of all hemodynamic measures.

Additionally, we thank Wuethrich *et al.*[2] for their letter to the editor. We read their comments with interest.

We agree with Dr Wuethrich that postoperative ileus is multifactorial, of which perioperative fluid balance may only be one component. As described in the Methods,[3] we used the same terminology and definitions for complications used in Shabsigh *et al.*[4] We apologize for mischaracterizing the Wuethrich *et al.*[5] article as lacking adequate definition of the terms *ileus* and *constipation*, as they are indeed provided in the appendix. However, because that definition of ileus does not completely correspond with the definition provided by Shabsigh *et al.*,[4] there is still some difficulty in aligning the Wuethrich *et al.* study's results with other studies. Specifically, the Wuethrich *et al.* article did not include the second half of the Shabsigh *et al.* definition of ileus: "or the intolerance of oral intake by postoperative day 5 resulting in patient fasting with or without nasogastric tube placement or antiemetics."

In response to the other comments: diuretics were not used routinely; they were used only at the discretion of the surgeon for patients who clinically appeared fluid-overloaded based on weight gain, peripheral edema, brain natriuretic peptide, and/or chest x-ray or in patients who were on routine diuretics preoperatively for other medical reasons. Methylnaltrexone was used perioperatively as an opioid antagonist. For the administration of systemic opioids, we believe that randomization should maintain similarity between the arms, thus we did not standardize administration, but we suspect that impact should be the same in each arm. We have found at our institution that we have fewer episodes of hypotension with first out-of-bed trials if the bupivacaine concentration used for epidural analgesia is kept low, without compromising pain control.

Competing Interests

The authors declare no competing interests.

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Using the Tubing Clamp to Prevent the Dislodgement of a Double Lumen Endotracheal Tube: Comment

To the Editor:

In the recent article from Hargrave *et al.*,[1] their figure 1 illustrates how a clamp is commonly applied to a double lumen endotracheal tube. The authors state: "Supporting the antiviral filter prevents dislodgement of the double lumen tube or bronchial blocker."[1] Although supporting the antiviral filter may help prevent dislodgement of the double lumen endotracheal tube, the weight of the tubing clamp as illustrated may be a factor in dislodging the double lumen endotracheal tube. Also, if the airway gradually narrows as one moves deeper into the bronchial system, the inflation of the bronchial cuff would tend to create a force that works to push the double lumen endotracheal tube out.

An alternative technique is to apply the clamp so that the finger rings are directed downward and toward the patient. A drape clamp can be used to secure the finger ring portion of the clamp to either the bed sheet or the head support. This can create a dislodgement stop and/or a small vector force directed to pushing the double lumen endotracheal tube inward. It is doubtful this force will contribute significantly to the pressure exerted by the bronchial balloon on the airway mucosa, particularly if the bronchial balloon is reinflated after applying and securing the clamp. Anecdotally, since using this technique, I have not experienced a double lumen endotracheal tube dislodgement.

Competing Interests

The author declares no competing interests.

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This letter was sent to the author of the original article referenced above, who declined to respond.—Evan D. Kharasch, M.D., Ph.D., Editor-in-Chief

Cryoneurolysis and Peripheral Nerve Stimulation: Comment

To the Editor:

We read the excellent review "Cryoneurolysis and Percutaneous Peripheral Nerve Stimulation to