Can Sonography of the Inferior Vena Cava Predict More than Just Intraoperative Hypotension?

To the Editor:

With interest we read the article by Zhang and Critchley[1] and sincerely commend them for their effort to further expand the role of ultrasound in the perioperative setting. They find an association between a greater respiratory variation in the diameter of the inferior vena cava (IVC) and the collapsibility index (IVCCI), suggestive of low volume status and the occurrence of hypotension after induction of anesthesia. Clearly, this offers the anesthesiologist another tool to identify patients at potential risk for adverse outcomes during anesthesia. However, a few issues may warrant some consideration before extrapolation of the present results to clinical practice, as the question on the preferred treatment of these patients (*e.g.*, fluid administration) in our opinion still remains.

First, transthoracic echocardiography was performed in spontaneously breathing patients. During respiration, the position of the IVC changes, possibly altering the angle at which the ultrasound plane intersects the vessel and thus altering the measured diameter.[2] Kimura *et al.*[3] showed that depth of inspiration as reflected by diaphragmatic displacement influenced IVCCI. Differences in breathing patterns may in this way be responsible for the moderate reliability of IVCCI to predict fluid responsiveness during spontaneous breathing. For standardization, guidelines from the American Society of Echocardiography[4] recommend performance of a short sniff by patients during scanning of the IVC. In order to translate the current findings into clinical practice, it would be important to specify how spontaneous breathing was standardized between patients.

Another potential confounder in the current observations is the initiation of mechanical ventilation after the ultrasound examination. As airway pressures reverse as compared to normal physiologic conditions, positive intrathoracic pressure during inspiration causes a decrease in venous return that can lead to hypotension in not only hypovolemic but also normovolemic patients.[5,6] Keeping the aforementioned in mind, it would be necessary to standardize ventilator settings and explore whether a relationship between pressure and the occurrence of hypotension in the study population could be substantiated.

Furthermore, the authors state that they did not perform another scan of the IVC after intubation because the change to positive pressure ventilation precludes reliable assessment of IVCCI. We agree with the authors that the collapsibility index cannot be used during mechanical ventilation. However, the distensibility index ([max IVC diameter – min IVC diameter]/max IVC diameter) can be used as an indicator of volume status and fluid responsiveness.[7,8] In contrast, where IVCCI is conceivably an indicator of volume status, some recent evidence shows that it does not reliably predict fluid responsiveness in spontaneously breathing subjects, but has a moderate correlation at best.[9–11] A recent meta-analysis showed that fluid responsiveness was predicted in a more reliable way in mechanically ventilated patients than in spontaneously breathing patients.[12] Accordingly, we would like to argue that another scan would have been of additional value. Besides exploring the complex relationship between mechanical ventilation and the cardiovascular system, it could also indicate which patients would probably benefit from fluid administration.

Nevertheless, this well-designed study elegantly demonstrates another useful application of ultrasound in the perioperative setting. Follow-up studies will undoubtedly confirm the efficacy of this noninvasive measurement of volume status, thereby aiding identification of patients at risk for hypotension after induction. With attention to the above-mentioned points, hopefully this will also translate into improved outcomes by not only detecting these patients but also providing clues to the best possible (fluid) management.

Competing Interests

Drs. Korsten and Bouwman have received consultant fees from Philips Medical Research, Eindhoven, The Netherlands, since 2016. The other authors declare 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.

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I Was There! I Did It!

To the Editor:

I am long retired from the practice of anesthesiology and, admittedly, no longer read our journal, Anesthesiology, from cover to cover. However, from time to time, I look at some of the titles to see what topics may still be of interest to me.

Recently, I found two articles in Anesthesiology, June 2016 (vol. 124, no. 6) that captured my attention and that I read and reread with great interest and genuine pleasure.

The first article is the special article by Drs. Johnstone and Fleischer[1] on a 1966 anesthetic by Robert D. Dripps, M.D. Why did it capture my attention? BECAUSE I WAS THERE! In the early 1960s (1963 to 1965), I was a resident in anesthesiology at the Hospital of the University of Pennsylvania, under the chairmanship of Dr. Dripps. Reading the article brought back many fond memories of Dr. Dripps (respectfully known by many of the residents as The Dripper!) and of the residency he created. It was indeed an "Ivory Tower" of academic anesthesiology, and I am forever grateful to have been at the Hospital of the University of Pennsylvania during those early years.

The second article that I allude to is Epiglottitis: It Hasn't Gone Away, by Lichtor *et al.*,[2] in the Clinical Concepts and Commentary section. I have always had an interest in acute epiglottitis. Why? BECAUSE I DID IT! In 1967, I submitted a case report to the journal *Pediatrics* describing the management of acute epiglottitis with prolonged nasotracheal intubation in a 4-yr-old child.[3] I was then a staff anesthesiologist at the Portsmouth Naval Hospital (Portsmouth, Virginia), fresh out of residency. Little did I know at the time that this would be the first reported use of nasotracheal intubation in lieu of tracheotomy for acute epiglottitis! It is gratifying to know that my case report is still being referenced in today's literature, almost 50 years after its publication.

Competing Interests

The author declares no competing interests.

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Blood Transfusion in Resource-limited Settings

To the Editor:

As French physicians frequently and currently deployed in Theater of Operations in Africa, we read with interest the article by Ariyo *et al.*[1] reviewing the anesthesia care provided at Médecins Sans Frontières facilities between 2008 and 2014. Spinal anesthesia was the most common type of anesthetic technique (34,413 [45.56%]), with cesarean section being the most common type of procedure performed (26,091 [34.54%]). Wound surgeries represented another common procedure type included in this retrospective study (18,547 [24.55%]). In this context, perioperative deaths occurred in 72 (0.21%) cases of obstetrics/gynecology and urology surgery and in only seven (0.06%) cases of wound surgeries. Nevertheless, the authors did not provide any details regarding the causes of death or any details about the incidence of perioperative bleeding and perioperative blood management. Perioperative blood management refers to perioperative blood transfusion and adjuvant therapies.[2] Perioperative blood transfusion addresses the preoperative, intraoperative, and postoperative administration of blood and blood components. Adjuvant therapies refer to drugs and techniques to reduce or prevent blood loss and the need for transfusion of allogeneic blood. Actually, postpartum hemorrhage and trauma-associated hemorrhage are both global public health issues. Indeed, fatal hemorrhage is the leading cause of death in postpartum and trauma contexts, especially in developing countries.[3–5] After many years of experience in