Dr. Raschke took a well-deserved vacation, and in his absence we did another quick-fire critical care journal club reviewing 7 articles.


This was a randomized control trial, which enrolled 181 patients from multiple medical-surgical ICUs to receive either nasojejunal or nasogastric nutrition. The number of patients selected for this study provided an 80% power to detect a 12% difference in mean energy delivery. Inclusion criteria for the study were patient that were admitted to the ICU, needing mechanically ventilated, narcotic drips for sedation as well as elevated gastric residuals (>150ml). Patients were excluded if patient had abnormal anatomy or imminent death.

Of the 1453 patients who met inclusion criteria only 181 patients were enrolled in the study. Patients were randomized to both groups via computer-generated randomization schedule. Of the 91 patients randomized to receive nasojejunal feeding, only 76 patients had successful placement of nasojejunal tube placement at 48 hours. The study showed that the estimated energy requirements were 72% for early nasojejunal feeding and 71% for the nasogastric feeding. A blinded panel diagnosed ventilator-associated pneumonia in 18 patients with nasojejunal group versus 19 patients in the nasogastric group. Both these finding were not statistically significant. Despite similar risk for major bleeding of 2% there was a slight increase risk of minor bleeds in patients treated with early nasojejunal feeding.

Overall this was a good study with good internal validity. Limitations identified by this study were the conservative definition of elevated gastric residual and the delay of enrolling patients into the study. These limitations may have indentified only mildly symptomatic patients or patients who were beyond the time of maximal gastric feeding intolerance.

Although based on sound theory of providing jejunal feedings to avoid risk of pneumonia as well as improving absorptive ability, this large RCT study shows conflicting results compared to previous studies. Early nasojejunal nutrition did not increase energy delivery or reduce the incidence of pneumonia in critically ill patients.

Heemesh Seth, DO

Last month the Critical Care Journal Club focused on drotrecogin alfa (Xigris®) and the problems associated with interpreting the data, especially the original PROWESS study, which contributed to Eli Lilly withdrawing the drug (1). Just when drotrecogin appeared to be a dead issue this study was published which performed a meta-analysis of 47,223 patients with sepsis from 9 controlled trials and 16 single group studies. The results showed a significant mortality reduction similar to the original PROWESS trial. However, mortality in patients who were treated with drotrecogin alfa in the 16 single group studies (41.3%) was much higher than that was reported in the PROWESS trial (29.7%). The study concluded that there is a relative risk reduction of 18% in hospital mortality with drotrecogin alfa. This is in conjunction with the original PROWESS trial.

Although this study shows similar results to the original PROWESS trial, I am skeptical that this would change our clinical practice based on the deep distrust that was developed over the course of last decade against the true clinical benefit and the manufacturer of the drug. Inclusion of data that is tainted in a meta-analysis leads to conclusions that are also tainted.

Suresh Uppalapu, MD


I liked this article. Few interventions have proven to improve mortality and outcomes in the ICU over the past several decades, so investigating our current unproven practices for evidence basis seems only logical. This ensures our current practices are actually beneficial, and helps eliminate those that only seem like good options just because they are available.

The authors correctly point out the conflicting data regarding the use of hydroxyethyl starch (HES) in sepsis volume resuscitation, as evident in the Cochrane review 2011 “Colloids versus Crystalloids for fluid resuscitation in critically ill patients” (2). In that review, they found 17 randomized controlled trials with mortality data comparing HES with crystalloids (n = 1172) with a pooled RR = 1.18 (95% CI 0.96 to 1.44). Although not significant, it implies a mortality risk with the use of HES, leading the authors to recommend against the use of HES (or any other colloid) for resuscitation of critically ill patients with trauma, burns or following surgery.

With the concerns over accelerated renal failure with the use of HES in previous trials, a definitive study about the use of HES in sepsis resuscitation was needed.
This trial fills that gap. 804 patients with severe sepsis were randomly assigned to fluid resuscitation with either 6% HES 130/0.42 (Tetraspan) or Ringer’s acetate. Patients with severe sepsis assigned to fluid resuscitation with HES 130/0.42 had an increased risk of death at day 90 and were more likely to require renal-replacement therapy.

I also liked that they used the lower molecular weight HES, which has been referred to as “3rd Generation Starch” implying a better safety profile than previous starches; asked centers to follow “international guidelines” for resuscitation reflecting real-world practices; and allowed patients with acute kidney injury, which better represents actual practices in ICU settings today. The trial was large, multi-centered, randomized and well matched, and blinded. Overall good study design and implementation.

This trial has, for me, clinched the idea that using HES colloid for volume resuscitation is a poor choice compared to alternative crystalloids. The finding that HES increased mortality and worsened renal failure is consistent with previous trials and independent review trends, lending more credibility to the outcomes. We should continue evaluating current practices for evidence-basis to ensure our intended outcomes are the actual outcomes.

Elijah M. Poulos, MD


We were confused about this editorial. Dr. Lee describes a system for improving healthcare where a team decides what patient centered outcome needs improvement, develops an improvement strategy centered on evidence based medicine, and measures the outcome. Although there are several good points within the editorial, details are missing and what is implied is a much rosier version of healthcare decision making than really exists.

Although Dr. Lee correctly points out that “health care is intended to help people, not just provide a commodity as inexpensively as possible”, details such as who will decide which outcomes need improvement; what strategy should be adopted; and what measure(s) will be used to determine success are either not present or only vaguely described. Increasingly, we have medical decisions from a “team” consisting predominately of nonclinicians. Although Dr. Lee also correctly points out that “simply adapting another institution’s checklist would probably have limited value”, a number of organizations such as the JCAHO, IHI and the Centers for Medicare and Medicaid are mandating exactly that—a checklist whose success is measured by a meaningless surrogate. For example, administration of the 23-polyvalent pneumococcal vaccine is a “quality measure” whose success is measured by the percentage of patients who have the vaccine administered. Yet, pneumococcal vaccine appears ineffective in adults (3). Patient-centered
measures such as the incidence of pneumonia and its resultant morbidity and mortality are not measured. Developing a strategy for improving the frequency of pneumococcal vaccination is straightforward and easy. Improving patient-centered outcomes is much harder. Strategizing from a group of nonclinicians concerned with political gain, or worse, paid bonuses based on the frequency of pneumococcal vaccine administration seems unlikely to adopt or improve patient-centered outcomes.

Unfortunately, Dr. Lee describes an ideal healthcare decision process that does not exist at most institutions. Furthermore, the process described will be corrupted to undermine physician autonomy, encourage the adoption of interventions that are cheaper but inferior or ineffective, and divert blame away from poor decisions made by nonclinicians. This is not a step forward but several steps backwards.

Tonya Whiting, DO
Richard A. Robbins, MD


The authors review the evidence for using hands-only CPR for out-of-hospital cardiac arrest and conclude that for adults; hands-only CPR is the treatment of choice. Most adult out-of-hospital arrests are due to cardiac arrythmias and this approach makes sense since the patient has a “reservoir” of oxygen in the lung and chest compressions provide some ventilation. Ventilation is daunting for most laymen performing CPR and makes them reluctant to initiate the CPR process, especially if it involves mouth to mouth ventilation. The authors review the studies and a meta-analysis which support the use of hands-only CPR only. However, they point out that in children and inpatient adults that asphyxia is more frequent and that assisted ventilation is probably necessary because the oxygen “reservoir” is depleted.

The authors also discuss whether rescue ventilation is necessary for Emergency Medical Services (EMS) providers. The outcomes for out-of-hospital CPR from EMS in Tucson and Phoenix had been dismal. A clue to a potential reason was that the EMS protocols unintentionally lead to interruptions and delays in chest compressions (assessing airway, breathing and circulation, placing the automated external defibrillator pads, waiting for rhythm analysis, endotracheal intubation, and so forth). Replacing these protocols with 200 uninterrupted preshock chest compressions, passive oxygen insufflation and delayed endotracheal intubation resulted in a significant increase in survival from 1.8% to 5.4% (4).

I have often watched as inpatient CPR mimics the prior EMS protocols, i.e., overly focusing on obtaining an airway to the detriment of chest compressions.
Since many of the EMS technicians are fairly good at performing endotracheal intubation and are probably better than many of the inpatient CPR responders, it makes one wonder if a similar protocol should be studied for inpatient CPR.

Richard A. Robbins, MD

**Marik PE, Flemmer M, Harrison W. The risk of catheter-related bloodstream infection with femoral venous catheters as compared to subclavian and internal jugular venous catheters: a systematic review of the literature and meta-analysis. Crit Care Med 2012;40:2479-85.**

This article reviews the incidence of catheter-related bloodstream infections with femoral catheters compared to other sites. Although earlier studies showed a lower risk of catheter-related bloodstream infections when the internal jugular was compared to the femoral site, recent studies show no difference in the rate of catheter-related bloodstream infections between femoral and subclavian or internal jugular sites. This is the same conclusion we came to earlier this year when we published our review of the Institute of Health Care (IHI) guidelines for prevention of central line associated bloodstream infection (5). Use of nonfemoral sites is an integral part of the IHI Central Line Bundle according to their website which still touts nonfemoral insertion (6). It is time IHI revised their guidelines to reflect the evidence.

Richard A. Robbins, MD


Procalcitonin has received much attention as a potential biomarker leading to improved diagnosis of bacterial infection and appropriate use of antibiotics. Studies investigating procalcitonin-guided management of sepsis in the ICU have shown reduction in duration of antibiotics, but mixed effects on ICU length of stay, and no effect on mortality. We previously reviewed one of these articles in the June 2012 Critical Care Journal Club and promised to keep an eye on procalcitonin as more research comes along (7). These authors tested the usefulness of procalcitonin serum level for the reduction of antibiotic consumption in intensive care unit patients. Patients were randomized into two groups: one using the procalcitonin results (procalcitonin group, n=258) and one being blinded to the procalcitonin results (control group, n=251). Surprisingly, knowing the procalcitonin level at the initiation of antimicrobials did not reduce antibiotic consumption in the ICU. As previously promised we will continue to follow the procalcitonin story.

Richard A. Robbins, MD
References