Commentary
Recently published papers: We are what we eat?
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> The glutton digs his grave with his teeth
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> *Anonymous English proverb*

Over the past few months yet more information bombards us. Several papers have concentrated on nutrition and markers thereof, in an attempt to make sense of much data.

Current interest in blood glucose levels focuses on examining patients' glucose tolerance to predict outcomes. This is particularly pertinent to high-dependency and coronary care practice, as well as intensive care unit (ICU) work. The DIGAMI study highlighted the long-term post-myocardial infarction (MI) risk in patients with a deranged glycometabolic state [1]. Similarly, the intensive care population has been scrutinized with regard to glycometabolic control in septic critically ill patients. Strict glycaemic control in such individuals is now hopefully commonplace, with the aim of improving survival. This has prompted much work to delineate those individuals with impaired glycometabolic control, and the same group presented further evidence that abnormal glucose metabolism is associated with a high prevalence of acute MI [2]. A total of 181 consecutive nondiabetic patients admitted with acute MI were given standard glucose tolerance testing at discharge and 3 months later. Fewer than 35% of patients had normal glucose tolerance at 3 months of follow up. It would appear likely, then, that early detection of impaired glycometabolic control might improve outcome by allowing introduction of secondary preventative measures. This probably has little immediate relevance to the ICU, but for those of us who are involved in coronary care it is worthwhile bearing in mind that an HbA_{1c} on admission may well indicate long-term risk and is a relatively quick and inexpensive test.

Nutritional support is often regarded as the Cinderella of the intensivist's armamentarium, probably because it does not generate the same excitement as the latest test for inflammatory mediators or suchlike. However, rather like toothache, it is best not ignored. Intense debate continues as to the preferred route of administration. The recent report by Montejo and colleagues [3] does little to help. A prospective randomized study comparing the efficacy and complications of early jejunal versus gastric feeding was undertaken in a sample group of some 101 ICU patients who were deemed to need enteral nutrition for more than 5 days. Interestingly, the sample group comprised fewer than 6% of patients admitted to the 14 centres involved, with more than 12% being given mandatory total parenteral nutrition. Little overall difference between the two groups was identified, which in part may be explained by the small numbers involved. As expected, the jejunal route resulted in significantly lower residual volume, but this did not translate into higher volumes of diet or indeed increased caloric intake, principally because of increased frequency of tube-related complications (i.e. occlusion, withdrawal [accidental] and displacement). For those of us who have struggled with nasogastrojejunal tubes, some solace is found within this paper. Furthermore, there was no difference in the incidence of nosocomial pneumonia in the two groups, again possibly reflecting the numbers studied. The authors point out that the study shows that the transpyloric tube is as useful as a nasogastric tube for early nutrition in the critically ill. One may argue that this implies a lot more effort for little or no benefit. No doubt further studies will address this. The alternative is of course to befriend an amenable surgeon.

An alternative approach to accelerate recovery of gastrointestinal motility may be effective salt and water balance. In a study by Lobo and colleagues [4], prompted by

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APACHE = Acute Physiology and Chronic Health Evaluation; ICU = intensive care unit; MI = myocardial infarction; SMART = Systemic Mediator Associated Response Test.

animal models, 20 patients were randomized to standard postoperative intravenous fluids (at least 1 litre 0.9% NaCl plus 2 litres 5% dextrose per day) or restricted sodium and water intake (2 litres of water plus 77 mmol sodium per day). Those in the restricted group showed significantly improved solid-phase and liquid-phase gastric emptying times on postoperative day 4, as well as a reduction in loosely defined complications and in hospital stay. Although intriguing, that study is open to several criticisms. Those in the restricted group were managed remotely from those offered standard treatment, and fluid input was managed solely by one of the investigators. The standard treatment implied little thought as to volume balance, which I am sure was not the case. Also, the elegant studies on gastric emptying were incomplete in the standard group. It may well be that other units approach the problem of postoperative ileus in a different manner, utilizing epidural analgesia, laparoscopic surgery and early mobilization to improve results. The authors also suggested that moderate restriction of salt and water may benefit some critically ill patients. Although this may be the case in a few instances, there is considerable evidence that early aggressive volume resuscitation in septic patients can have dramatic effects on mortality. As such I would take their last comments with a pinch of brine! For those interested, a balanced view to this work is provided by Heyland and Paterson [5].

An interesting alternative with respect to volume resuscitation was recently described. Horstick and colleagues [6] subjected rats to volume-controlled haemorrhagic shock and examined the mesenteric microcirculation as well as other parameters following resuscitation with similar volumes of either 20% albumin infusion or 0.9% NaCl. A significant improvement in global haemodynamics was found, together with an improvement in the microcirculation, when albumin was used as the resuscitation fluid. Clearly, the albumin story continues to run but, as Horstick and colleagues pointed out, this experimental scenario is vastly different from that which is often encountered clinically, and not surprisingly they suggest a need for further experimentation and clinical studies.

Albumin is also the subject of a recent study reported by Yap and coworkers [7]. They retrospectively studied more than 1000 patients over an 18-month period, their hypothesis being that serum hypoalbuminaemia may be predictive of mortality risk in the critically ill. This was no doubt triggered by the Acute Physiology and Chronic Health Evaluation (APACHE) III as well as the study reported by McCluskey and colleagues [8]. Unlike the latter study, Yap and coworkers failed to show that serial measurement of albumin was as accurate as APACHE II in predicting outcome, although they agreed that serum albumin is associated with acute physiological illness and mortality. They pointed out that their data suggest that serum albumin is a poor predictor of hospital mortality, and the referees should be congratulated on publishing relatively negative results.

Another surrogate of nutritional and overall health status is haemoglobin concentration. An observational study conducted in over 2000 patients who underwent coronary artery bypass surgery [9] examined in-hospital mortality with respect to haemoglobin concentration. The crude mortality rate was five times higher among patients with a haemoglobin of 100 g/l or less. The authors concluded that comorbidity probably had the greatest effect on outcome, as indicated by the reduced haemoglobin levels. There was no evidence to support perioperative correction of anaemia. No doubt studies will soon be focused on the use of erythropoietin preoperatively.

Acute sepsis and markers thereof continue to provide much excitement, although the prognostic value of certain markers such as procalcitonin remain in doubt. Claeys and colleagues [10] studied procalcitonin in septic patients and tried to determine its relationship with more conventional markers, including C-reactive protein and white cell count. Individuals with septic shock ($n = 53$), as defined by the American College of Chest Physicians Consensus, were studied. All patients had elevated procalcitonin and C-reactive protein levels within 24 hours of diagnosis. However, baseline values were of no predictive value. Indeed, single measures of procalcitonin did not discriminate between survivors and nonsurvivors when examined within the first 5 days of admission. There was some evidence that time-dependant changes in procalcitonin may provide an earlier indicator of survival, but equally other nonquoted variables such as inotrope usage, fractional inspired oxygen, or indeed blood pressure may be just as good pragmatically.

Continuing the procalcitonin story, yet another article on proinflammatory cytokine clearance using continuous venovenous haemofiltration was reported [11]. Curiously, this paper infers that procalcitonin is an important clinical prognostic marker in septic patients, which is rather at odds with the findings described above. Using an AN69 membrane, concentrations of the inflammatory mediators measured were significantly reduced, but in keeping with other studies this effect was decreased after 12 hours, probably reflecting progressive membrane saturation. One point that this paper does highlight is that clearance of such mediators is entirely membrane dependent. What implications do those findings have for the use of continuous venovenous haemofiltration in sepsis? My personal view is that the jury is still out. If one views the circulation as the dumping ground for cytokines, one wonders whether such removal has any effect at the cellular level.

Finally, what predictions can be made in critically ill patients? In intensive care we are blessed with a variety of scoring systems of varying complexity, most of which predict mortality with similar efficacy. To date, scoring systems have not been particularly good at predicting the events that determine ICU mortality, such as the development of organ failure. Slotman

[12] described a multivariate predictive model imaginatively described as the SMART (Systemic Mediator Associated Response Test) system. In an impressively thrifty manner, he used the data obtained from the placebo arm of a phase III clinical trial. An impressive array of data had been collected, including interleukin-6, interleukin-8 and granulocyte colony-stimulating factor levels, which were used randomly to develop a training cohort (200), the further 103 data sets being used to provide prospective validation. Through using the training cohort, multivariate models were developed to predict acute respiratory distress syndrome, renal insufficiency, hepatobiliary dysfunction and disseminated intravascular coagulation, as defined according to established criteria. The aim of such a SMART model could be to predict the development of organ dysfunction, and in the future it may enable tailored therapies to be introduced early. The author points out valid criticisms of the model and highlights some of its limitations. However, he should be commended. With the growth of fully automated clinical information systems, it may not be too long before SMART-like models, allied with clinical judgement (hopefully!), become commonplace in ICUs. 'He that would know what shall be, must consider what hath been' (quotation: Fuller T [1654–1734]). Slotman has taken that advice, and we wait to see what the future brings.

Competing interests

None declared.

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