

meta-analysis has included very heterogeneous groups of patients, including severely ill patients in intensive care units, many of them with different forms of shock including septic shock. Such a population is at high risk of organ dysfunction, including acute kidney failure, and cannot be compared with elective surgical patients. Also, we do not think that our meta-analysis is comparable with analysis by Gattas *et al.*⁴ In this meta-analysis, the studies showing a higher risk for the need of renal replacement therapy (fig 3, top panel⁴) were all conducted in patients in intensive care unit, with only one exception (Nagpal *et al.*). In the middle panel of figure 3 of our article,¹ trials conducted in surgical patients are presented. These trials were not associated with a higher risk of renal replacement therapy (risk ratio, 0.46; 95% CI, 0.10–2.05; $P = 0.794$). Therefore both meta-analyses either are inadequate to address the clinical question that we wanted to address or found comparable evidence.

Surgical patients were also evaluated by Van Der Linden *et al.*⁵ In their meta-analysis, 2,139 patients treated with tetrastarches were compared with 2,390 patients treated with a comparator. From 39 trials, the authors concluded that tetrastarches used during surgery did not induce adverse renal effects as assessed by changes in serum creatinine or need for renal replacement therapy. The authors reported 21 studies documenting serum creatinine or creatinine clearance after administration of 130/0.4 starch or other tested fluids. One thousand five patients were given a tetrastarch and 1,051 patients were given a comparator. The period for which creatinine was reported covered up to 14 days after administration. All but three studies showed no difference in peak creatinine concentration. Two studies found a statistically better outcome for the tetrastarch. The authors concluded that they could not detect a hint for an adverse signal after the use of modern starch in surgical patients.⁵ The risk of excessive bleeding was out of the scope of our meta-analysis, but the results of the Van Der Linden meta-analysis are reassuring with this regard. Every meta-analysis can only be as reliable as the data available. In this way, it is in fact limited, and this point was emphasized at the end of our discussion. But, even though only two of the trials in the meta-analysis were primarily designed to evaluate the renal effect of hydroxyethyl starch 130/0.4, this side effect of colloids was well known since long and thus was an integral safety parameter in all of these trials.

We are confident that our conclusions are meaningful today and can hold in the light of upcoming evidence. Although Groeneveld *et al.* point out that a retrospective analysis has found an association between hydroxyethyl starch 130/0.4 and renal replacement therapy, we would like to draw the readers' attention to a recently published prospective randomized study in patients undergoing abdominal surgery by Feldheiser *et al.*⁶ demonstrating that a stringent treatment algorithm and an adequate monitoring results in better hemodynamic stability and reduced need for fresh-frozen plasma. This study included a 3-month

follow-up and measured the sensitive renal marker neutrophil gelatinase-associated lipocalin. If older studies might not provide the evidence, we would wish for today, this is also true for all studies that did not use rigorous protocols to identify patients who were in need of volume therapy.

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Epidural and Continuous Wound Infusion in Enhanced Recovery Protocols

To the Editor:

I read with interest the article by Jouve *et al.*¹ comparing epidural analgesia with continuous wound infusion of local anaesthetic after fast-track colorectal surgery, and I would like to commend the authors on their thorough methodology.

An important aspect of this trial is the management of patients within an enhanced recovery program, and the authors cite consensus recommendations, which guided to their management decisions.² There are two areas that I feel the authors did not strictly adhere to the enhanced recovery recommendations. First, the consensus group

recommends that the use of mechanical bowel preparation should be avoided except in the case of low rectal resections where diverting stomas are planned. However, patients receiving stomas were excluded and all patients undergoing left-sided or rectal resections received bowel preparation at the night before surgery. Second, all of the patients recruited to this study received large (19–20 cm) periumbilical midline incisions. Although it is true that the recommendations do not dictate the preference of transverse over midline incisions, they do recognize the findings of a Cochrane review reporting that short transverse incisions were associated with lower postoperative analgesic requirement and reduced pulmonary complications. My concern is that by including only the patients receiving midline incisions, they have selected a group that are more likely to benefit from epidural analgesia but do not represent the surgical population seen in other centers using enhanced recovery protocols.

I also note the authors' recognition of the potential bias resulting from early conclusion of the trial after interim analysis. This is of particular interest because a similar study recently completed in our institution recruited 60 patients, but did not reveal a significant difference in length of stay or dynamic pain scores between either epidural analgesia or continuous wound infusion.

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In Reply:

We thank Dr. Harper for his comments and interest on our recent study published in *ANESTHESIOLOGY*.¹

We fully agree with the author that complete compliance with the enhanced recovery after surgery recommendations would have involved absence of mechanical bowel preparation (MBP) in left-sided colonic surgery. Nevertheless, the largest systematic review on the role of MBP in colonic surgery (including approximately 5,000 patients) failed to demonstrate harmful effects of MBP in terms of anastomotic

leakage.² In addition, two large-scale randomized controlled trials^{3,4} suggested more deep intraabdominal abscesses in the absence of MBP and a significant benefit in patients who had MBP. It has also been shown that preoperative MBP is useful when intraoperative colonoscopy is required to precisely locate small tumors. Finally, although MBP has adverse physiologic effects attributed to dehydration, omission of preoperative fasting and use of individualized goal-directed fluid administration may easily and effectively compensate for this.

We respectfully disagree with the author when stated that midline incision may not represent current daily for colorectal surgery. To the best of knowledge, current recommendations do not advocate use of transverse or oblique incisions for open colorectal surgery. Since the publication of the Cochrane review, the Postsurgical Pain Outcome of Vertical and Transverse Abdominal Incision randomized controlled trial⁵ has shown no relevant difference in pain scores and postoperative morbidity between incision types after major abdominal procedures, whereas more wound infections were seen after transverse incisions. Although problematic, these concerns are not the most important ones. Indeed, as a possible clinical benefit of wound catheters placement in oblique and/or transverse incisions has never been explored during colorectal surgery, any comparison between continuous wound analgesia, as an interventional treatment group in this setting, and epidural analgesia would have been only speculative.

Until more thorough studies addressing the question have been carried out, our opinion is that there is no sufficient evidence to shift from epidural analgesia to continuous wound infiltration of local anesthetics in elective open colorectal surgery. We are convinced that the soon-to-be-published Dr. Harper's study will help provide answers to the many remaining questions regarding the optimal analgesic regimen in open colorectal surgery.

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