

tracts obtained with these newer biopsy needles in EUS-guided liver biopsy in specific liver conditions, like staging of chronic hepatitis C. Until then, on the basis of our study, we recommend EUS as a valid approach for performing liver biopsy.

Babu Mohan, MD
Douglas G. Adler, MD, FACG, AGAF, FASGE
Department of Gastroenterology and Hepatology
University of Utah School of Medicine
Huntsman Cancer Center
Salt Lake City, Utah, USA

REFERENCES

1. Knoedler D. Pathologist's perspective on EUS guided liver biopsy. *Gastrointest Endosc* 2019;90:703.
2. Mohan BP, Shakhateh M, Garg R, et al. Efficacy and safety of EUS-guided liver biopsy: a systematic review and meta-analysis. *Gastrointest Endosc* 2019;89:238-46.e3.
3. Rockey DC, Caldwell SH, Goodman ZD, et al. Liver biopsy. *Hepatology* 2009;49:1017-44.

<https://doi.org/10.1016/j.gie.2019.05.021>

Stent as a bridge to surgery for colonic obstruction: Do we really need more systematic reviews with meta-analysis of the same articles?



To the Editor:

The number of systematic reviews published over the past 2 years examining the use of stents for colonic obstruction as a bridge to surgery has increased. However, what has attracted even more attention is the divergent results, even when the same studies were analyzed. This leads us to the question: Do we really need more systematic reviews?¹

In less than 2 years, 5 systematic reviews with meta-analyses examining the subject "stent as a bridge to surgery" in patients with acute obstructive abdomen were published,²⁻⁶ and only 1 study⁷ focused on the use of stents for palliation. Also, a recent narrative review covered this topic.⁸

We read with great interest the article by Arezzo et al,² and we believe that this study was very well conducted. However, we do have some questions about certain aspects of the analysis, which we outline below:

1. The review by Wang et al³ included 9 randomized controlled trials. Why did you include only 8?
2. The review by Yang et al⁶ evaluating the same outcomes had similar results. What is the new and relevant information presented in your study?
3. Primary anastomosis rates: Why was the population analyzed smaller than the population examined in the other articles,⁶ and why was there heterogeneity in the results if the same articles were analyzed?

4. Tumor recurrence rate: Why was this not included in the study by Cheung et al?⁹ Yang et al⁶ demonstrated the tumor recurrence rate to be significantly higher in the stent group.

5. Stoma rate: How do we explain the different results when the same articles were analyzed in the study conducted by Allievi et al⁴ and Yang et al?⁶

Finally, we would like to congratulate and thank the authors on the level of shared evidence. To finalize, we would like to know the authors' opinion on whether new systematic reviews of the same articles could add something new to the literature.

DISCLOSURE

Dr Eduardo de Moura is a consultant for Boston Scientific and Olympus. The other authors disclosed no financial relationships relevant to this publication.

Igor Braga Ribeiro, MD
Gastrointestinal Endoscopy Unit
Hospital das Clínicas
University of Sao Paulo School of Medicine
São Paulo, Brazil

Diogo Turiani Hourneaux de Moura, MD, MSc, PhD
Gastrointestinal Endoscopy Unit
Hospital das Clínicas
University of Sao Paulo School of Medicine
São Paulo, Brazil

Division of Gastroenterology, Hepatology and Endoscopy
Brigham and Women's Hospital
Harvard Medical School
Boston, Massachusetts, USA

Amit H. Sachdev, MD
Division of Gastroenterology, Hepatology and Endoscopy
Brigham and Women's Hospital
Harvard Medical School
Boston, Massachusetts, USA

Eduardo Guimarães Hourneaux de Moura, MD, MSc, PhD
Gastrointestinal Endoscopy Unit
Hospital das Clínicas
University of Sao Paulo School of Medicine
São Paulo, Brazil

REFERENCES

1. Zhang L, Gerson L, Maluf-Filho F. Systematic review and meta-analysis in GI endoscopy: why do we need them? How can we read them? Should we trust them? *Gastrointest Endosc* 2018;88:139-51.
2. Arezzo A, Passera R, Lo Secco G, et al. Stent as bridge to surgery for left-sided malignant colonic obstruction reduces adverse events and stoma rate compared with emergency surgery: results of a systematic review and meta-analysis of randomized controlled trials. *Gastrointest Endosc* 2017;86:416-26.

3. Wang X, He J, Chen X, et al. Stenting as a bridge to resection versus emergency surgery for left-sided colorectal cancer with malignant obstruction: a systematic review and meta-analysis. *Int J Surg* 2017;48:64-8.
4. Allievi N, Ceresoli M, Fugazzola P, et al. Endoscopic stenting as bridge to surgery versus emergency resection for left-sided malignant colorectal obstruction: an updated meta-analysis. *Int J Surg Oncol* 2017;2017:2863272.
5. Foo CC, Poon SHT, Chiu RHY, et al. Is bridge to surgery stenting a safe alternative to emergency surgery in malignant colonic obstruction: a meta-analysis of randomized control trials. *Surg Endosc* 2019;33:293-302.
6. Yang P, Lin X-F, Lin K, et al. The role of stents as bridge to surgery for acute left-sided obstructive colorectal cancer: meta-analysis of randomized controlled trials. *Rev Invest Clin* 2018 27;70:269-78.
7. Ribeiro IB, Bernardo WM, Martins B da C, et al. Colonic stent versus emergency surgery as treatment of malignant colonic obstruction in the palliative setting: a systematic review and meta-analysis. *Endosc Int Open* 2018;6:E558-67.
8. Ribeiro IB, Moura DTH de, Thompson CC, et al. Acute abdominal obstruction: colon stent or emergency surgery? An evidence-based review. *World J Gastrointest Endosc* 2019;11:193-208.
9. Cheung HYS, Chung CC, Tsang WWC, et al. Endolaparoscopic approach vs conventional open surgery in the treatment of obstructing left-sided colon cancer: a randomized controlled trial. *Arch Surg* 2009;144:1127-32. <https://doi.org/10.1016/j.gie.2019.05.036>

Response:



Thank you for giving us the opportunity to reply to this letter¹ about our article.² We agree with Dr Ribeiro and colleagues¹ that the number of systematic reviews published over the past 2 years examining the use of stents for colonic obstruction as a bridge to surgery has increased and that this may produce confusion in cases of divergent results. Nevertheless, our systematic review and meta-analysis appeared in the literature earlier than the other 4 meta-analyses cited.³⁻⁶ Therefore, I would not be surprised if the reviews published later, such as that by Yang et al,⁴ report similar results and conclusions, and no new relevant information.

The authors argue that Wang et al³ included 9 randomized controlled trials (RCTs), whereas we included only 8. It is otherwise interesting that the studies do not overlap. The 9 studies selected in the article by Wang et al, although they addressed all as RCTs, actually are not. Two RCTs are

apparently missing,^{7,8} whereas 3 articles are included as RCTs although they are described as retrospective.⁹⁻¹¹

Although we analyzed the same 8 RCTs, the results by Yang et al⁴ do not completely overlap with our findings. We have double-checked our reported data as is, including the heterogeneity among the included studies. Similarly, we do not see the point regarding tumor recurrence. In fact, we included data from Cheung et al¹² (see Fig. 8 of their article). We acknowledge that we did not include data about recurrence reported by Ghazal et al.⁸ This was clearly a mistake, but fortunately not so important, inasmuch as the new calculation including these data persists in showing a nonstatistically significant difference between groups. In fact, Yang et al,⁴ who collected data correctly, surprisingly used fixed effects for the analysis, despite a reported heterogeneity $I^2 = 53%$, therefore obtaining a statistically significant difference favoring emergency surgery. Correctly, owing to heterogeneity, random effects should be used. With regard to the odds ratio, a value of 2.15 is obtained but with a 95% confidence interval of 0.96-4.85 (Fig. 1), or in case you would like to analyze risk ratio, you obtain a value of 1.65 but with a 95% confidence interval of 0.95-2.89 (Fig. 2), in both cases with no statistical significance. So, the conclusion of Yang et al⁴ seems inappropriate.

Finally, the issue related to stomas. We, unlike Allievi et al⁵ and Yang et al,⁴ considered temporary and permanent stoma rates separately, rather than global stoma rates, which could be confounding. Nevertheless, whether one considers the global incidence of stomas, or the temporary and permanent stoma rates, whether one uses the odds ratio or the risk ratio, an advantage in favor of stent bridge to surgery is always demonstrated.

In conclusion, we would like to express our gratitude for letting us clarify the importance and the correctness of what we reported about 2 years ago on this topic, which is still current. We await further objective data such as the publication of the CREST trial¹³ to discuss further both the short-term and the long-term results of stent placement as a bridge to surgery policy.

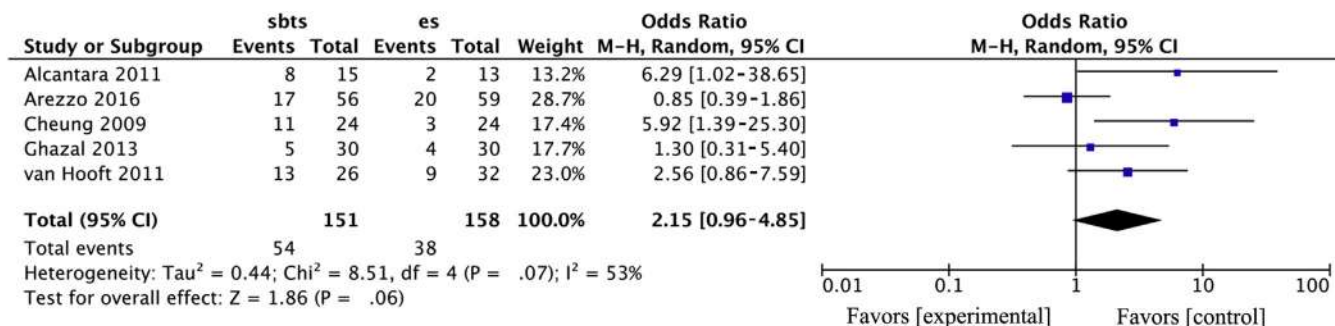


Figure 1. Corrected Forest plot for the tumor recurrence rate considering odds ratio.