et al[1] due to several methodological problems including the following:

First, the CRC risk in the obese nonsurgery group did not increase over time as would be expected.[4] The authors report a high percentage of obesity-associated comorbidities such as diabetes, hypertension, or cardiovascular diseases (23%, 30%, and 32%, respectively) for the nonsurgery group but fail to provide mortality data. The possibility that the nonsurgery group died before they developed CRC is not addressed, neither is whether patients of the obesity surgery group lived longer because their metabolic conditions were effectively treated.[2,3] If patients live longer because of a reduction in cardiovascular or metabolic diseases, it may be possible that they have longer time to develop CRC. However, the authors failed to address this in their article.

Second, it is surprising that there was no difference in the CRC risk between the 3 types of surgery [vertical banded gastroplasty, gastric banding, and roux-en-y gastric bypass (RYGB)], which have very different anatomy and underlying physiological mechanisms.[5,6] However, looking at the CRC incidence during the observation period, substantial differences exist: 31 of 3743 vertical banded gastroplasty patients (0.83%), 27 of 3575 gastric banding patients (0.75%), and 12 of 7769 RYGB patients (0.15%). The authors suggest that bile flow, bile metabolism, or alterations in gut microbiota may play a role in the development of CRC, but the authors fail to explain why patients after RYGB (the only surgery where bile flow is altered) did not have a higher CRC incidence. The study appeared underpowered to examine this hypothesis.

Third, any comparison between the obese surgery group and the obese nonsurgery group is problematic as body weights were not reported. Subjects were included if they had a diagnosis of obesity at admission to any hospital, but no information on either the severity of obesity or reasons for hospital admission is provided. The authors stated that they ''never intended to formally compare'' these 2 cohorts directly as it would be ''hazardous'' but then go on to conclude that ''The absolute cumulative incidence of colorectal cancer in the obesity surgery cohort was lower (48 per 100,000 person years) than that of the obese no surgery cohort (91 per 100,000 person years).''

If one assumes that both authors and readers of this study are able to overcome the temptation to compare the obese surgery and the obese nonsurgery group; what conclusion can be drawn from this study? On the one hand there is a group A consisting of obese subjects who received surgery that has a higher risk to develop CRC over time when compared with an age- and sex-, but *not* weight-matched group B that is representative for the normal Swedish population. There is also a group C of obese subjects who despite having significant metabolic disease and morbid obesity did not receive obesity surgery and may also have an increased risk to develop CRC when compared with group B. All groups differ significantly from each other and therefore may not be directly compared. Thus, the conclusion that one group has a higher CRC risk than the other cannot be justified.

The authors did not address the numerous methodological flaws and both the title and the conclusion of the article should have reflected the unanswered questions. As a matter of fact, conclusions of any study should only be directly derived from the results and not from assumptions that were not tested. We agree with DeMeester et al, who recently criticized another study by the same group also published in *Annals of Surgery*, where patients were not stratified for risk factors at baseline when investigating the subsequent risk for esophageal adenocarcinoma after antireflux surgery.[7,8] He wrote: ''The inability to match cases and controls for the prevalence of Barrett's in studies on esophageal adenocarcinoma is like trying to study ovarian cancer without knowing the prevalence of women in the case and control groups.''[8(p584)]

In summary, the questions raised by Derogar et al[1] are valid, but the conclusions as to the risk of CRC after obesity surgery are not supported by the data. Causative relations between an intervention and events should be studied in prospective randomized controlled trials, because epidemiological registry studies that were not designed to answer specific questions have a history of being proven wrong.[9,10]

Disclosure: The authors declare no conflicts of interest.

Dimitri Aristotle Raptis, MD, MSc
Thomas Bächler, MD
Antonio Nocito, MD
Marco Bueter, MD, PhD
Department of Surgery
University Hospital Zurich
Zurich, Switzerland
marco.bueter@usz.ch

REFERENCES

1. Derogar M, Hull MA, Kant P, et al. Increased risk of colorectal cancer after obesity surgery. *Ann Surg*. 2013;258:983–988.
2. Adams TD, Gress RE, Smith SC, et al. Long-term mortality after gastric bypass surgery. *N Engl J Med*. 2007;357:753–761.
3. Sjostrom L, Narbro K, Sjostrom CD, et al. Effects of bariatric surgery on mortality in Swedish obese subjects. *N Engl J Med*. 2007;357:741–752.
4. Bardou M, Barkun AN, Martel M. Obesity and colorectal cancer. *Gut*. 2013;62:933–947.
5. Bueter M, Ashrafian H, le Roux CW. Mechanisms of weight loss after gastric bypass and gastric banding. *Obes Facts*. 2009;2:325–331.
6. Bueter M, le Roux CW. Gastrointestinal hormones, energy balance and bariatric surgery. *Int J Obes (Lond)*. 2011;35:S35–S39.
7. Lofdahl HE, Lu Y, Lagergren P, et al. Risk factors for esophageal adenocarcinoma after antireflux surgery. *Ann Surg*. 2013;257:579–582.
8. Demeester SR. Antireflux surgery and the risk of esophageal adenocarcinoma: an antithetical view of the data from sweden. *Ann Surg*. 2013;257:583–585.
9. Gerstein HC, Miller ME, Genuth S, et al. Long-term effects of intensive glucose lowering on cardiovascular outcomes. *N Engl J Med*. 2011;364:818–828.
10. Manson JE, Hsia J, Johnson KC, et al. Estrogen plus progestin and the risk of coronary heart disease. *N Engl J Med*. 2003;349:523–534.

Laparoscopy in ALPPS Procedure: When We Can Do It?

To the Editor:

We would like to acknowledge the Letters to the Editor written by Machado et al, describing totally laparoscopic ALPPS procedure performed on a patient with bilateral colorectal liver metastases, and to contribute to his proposal with a new concept to accomplish these cases.[1] The goal of this procedure is to avoid adhesions in the first surgery and to facilitate the second-stage hepatectomy. Therefore, a less invasive surgical procedure can be offered in these cases, aiming to reduce the number and severity of postoperative complications. Laparoscopy ALPPS can be planned before surgery for a patient with a small future liver remnant.

However, there are several situations in which we have to decide the ALPPS procedure while an open liver resection is being performed. For these cases, we propose the alternative of performing the second-stage laparoscopically.

To support this letter and proposal, we would like to report the procedure that was successfully performed on a 37-year-old male patient who had a preoperative diagnosis of multiple epithelioid hemangioendotheliomas. The initial proposal was to perform a combined segment II and right posterior hepatectomy. But during the intraoperative ultrasound, more lesions were identified confined to the right lobe and a new surgical master plan decided. The future liver remnant was not adequate to proceed with a formal

segment II plus right hepatectomy. Therefore, we decided to proceed with ALPPS procedure and the anatomical resection of segment II.

The right portal vein was identified, tied, and transected. The right hepatic artery, bile duct, and hepatic vein were isolated and tagged with a polypropylene to facilitate identification during the second stage. Parenchymal transection between right-left hemilivers was done following the demarcation line. Postoperative course was uneventful. After a week, the new liver volumetry showed a 120% increase on the future liver remnant. The second stage was performed laparoscopically on postoperative day 7. Pneumoperitoneum was used to release adhesions between the liver and the diaphragm. The right artery and bile duct were transected with endoscopic linear staples. After this, the right liver was mobilized, the right hepatic vein transected with the same device and finally removed through a partial opening of the previous midline incision. The patient was discharged 3 days later without complications.

Perhaps high morbidity is the major criticism received by the ALPPS procedure.[2] It has been demonstrated that laparoscopic hepatectomy has several advantages over open surgery, reducing the rate of complication and hospitalization time.[3,4] There is scarce literature demonstrating the use of laparoscopic surgery in the treatment of complications after an open surgery.

Laparoscopic or hand-assisted ALLPS have started to be proposed as a valid option to improve outcomes.[5,6] In the case here reported, we proved the feasibility of accomplishing a second stage laparoscopically. No intraoperative or postoperative complications were diagnosed. Neither did previous or recent incisions impact pneumoperitoneum, whereas complications associated with the second stage did not evolve. Larger series will define if the second stage performed laparoscopically would potentially decrease the complication rates in ALPPS but we consider that it could become part of the surgical armamentarium for hepatopancreatobiliary surgeons.

Disclosure: The authors declare no conflicts of interest.

Pablo Barros Schelotto, PhD
Gabriel Gondolesi, MD
Instituto de Trasplante Multiorganico
Hospital Universitario Fundacion Favaloro
Buenos Aires, Argentina
pbarros@ffavaloro.org

REFERENCES

1. Machado MA, Makdissi FF, Surjan RC. Totally laparoscopic ALPPS is feasible and may be worthwhile. *Ann Surg*. 2012;256:e13–e19.
2. Schnitzbauer AA, Lang SA, Goessmann H, et al. Right portal vein ligation combined with in situ splitting induces rapid left lateral liver lobe hypertrophy enabling 2-staged extended right hepatic resection in small-for-size settings. *Ann Surg*. 2012;255:405–414.
3. Medbery RL, Chadid TS, Sweeney JF, et al. Laparoscopic vs open right hepatectomy: a value-based analysis. *J Am Coll Surg*. 2014;218:929–939.
4. Long-term survival analysis of pure laparoscopic versus open hepatectomy for hepatocellular carcinoma in patients with cirrhosis: a single-center experience. *Ann Surg*. 2013;257:506–511.
5. Nguyen KT, Marsh JW, Tsung A, et al. Comparative benefits of laparoscopic vs open hepatic resection: a critical appraisal. *Arch Surg*. 2011;146:348–356.
6. Buell JF, Cherqui D, Geller DA, et al. The international position on laparoscopic liver surgery: the Louisville statement, 2008. *Ann Surg*. 2009;250:825–830.

Is Multimodality the "Third Way" in the Challenge Robot Versus Laparoscopy for Liver Resections?

To the Editor:

We have read with great interest the article by Tsung et al[1] from the University of Pittsburgh Medical Center. It definitely contributes to the knowledge on minimally invasive liver resections. In particular, the study develops the question whether robotic surgery might overcome some limits of the laparoscopic approach.

This specific subject has been recently investigated in other series, showing that robotic and laparoscopic surgery obtain comparable feasibility and safety.[2–4] The matched comparison by Tsung et al confirms these findings, describing one of the largest experiences to date, made in one of the most important centers worldwide. In addition, Tsung et al have observed that the robot has delivered no significant postoperative benefit, although has allowed completing more operations in a minimally invasive fashion. In fact, although simple, these are very important conclusions that have triggered immediate technical discussion by Montalti et al from the Ghent Group.[5]

Those positive conclusions in favor of the robot might represent further incentive for centers like ours that are exploring robotic approach[6] to provide benefits for their patients despite higher costs. Although simple, these are very important conclusions on a relevant matter, on which we feel the need to comment further.

From a purely surgical point of view, the robotic resections described in the study were partially laparoscopic, as laparoscopy was systematically utilized in the first part of each procedure. Conversely, the laparoscopic group included hand-assisted and hybrid lap-assisted open procedures. These multimodal techniques are clearly well respected and are also advocated by other teams[3,7] but introduce difficulties in this specific comparative analysis. The 2 laparoscopic and robotic groups in fact overlap and include hybrid open procedures, generating a fundamental selection bias at risk of invalidating the study results.

In addition, Tsung et al acknowledge a certain degree of selection bias for the comparison of the 2 groups inevitably caused by the single-surgeon intention whether to proceed to laparoscopic versus robotic surgery. In this sense, more information regarding this particular decision-making process and regarding the patients' eligibility would be very helpful to understand the Pittsburgh group's experience.

In the absence of dedicated statistical matching methodologies (ie, Propensity Score Matching), the matching procedure in our opinion might hide additional bias. It is not clear from the text if that was performed by human selection by a computer routine, and how and when the diagnosis of liver disease was made. BMI was considered at low priority; here it did not show significant differences overall, but interestingly the obese patients with BMI more than 30 kg/m^2 in the robotic group were roughly twice the laparoscopy (38.6% vs 21.9%). Tumor location was not included in the matching, as well as the year (or period of years) of surgery; these 2 variables are not mentioned in the study, again potentially undermining both its internal and external validity.

The potential benefits yielded by the increasing experience in early versus late robotic resections are the object of one of the multiple partial/nested and satellite comparisons presented in the article. This plentiful additional information in our view does not help toward the manuscript's original aims. Conversely, it provides ground for redundancy and inconsistencies. For instance, the further subdivision into minor/major resection is occasionally confounding; with the implicit exclusion of the planned hybrid laparoscopic-open procedures, a similar conversion rate (7% vs 8.8%, $P = 0.67$) is reported, in contrast with the main study conclusions of robotic technique allowing more minimally invasive completions. Nonetheless, this conclusion is in agreement with the general opinion from advocates of robot liver surgery on the potential benefits such as the enhanced vision and dexterity, occasionally allowing meticulous and peculiar surgery.[8–9]