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Reply to editor letter☆

Réplica a carta al director

Dear Editor:

We appreciate the opportunity to comment on the Letter to the Editor by Dr Clara Giménez Francés et al. regarding our recently published article on the treatment of cholelithiasis and acute cholecystitis during the initial phase of the COVID-19 pandemic.[1] This letter is an interesting occasion to discuss the results of our survey.

However, the letter is based on a small caseload, where the use of statistics and the conclusions must be considered in their context. The experience presented by the Hospital Universitario Reina Sofía de Murcia confirms the trend observed by the vast majority of Spanish hospitals to suspend elective cholecystectomy surgery during the first phase of the pandemic, as we have observed in our article.[2] As we are seeing in the successive phases of the pandemic, the effect of this strategy has significantly increased the waiting list for surgical treatment of cholelithiasis. The result of a prolonged post-pandemic surgical waiting list to treat cholelithiasis should not lead to a worsening of the quality of life of these patients.

It is essential for hospital administrators to act quickly and efficiently to solve this problem, allocating more resources to the resumed surgical activity while providing the maximum guarantees of safety for patients and professionals.

The series presented in the Letter to the Editor reports that, contrary to the trend described by our national survey, urgent surgical treatment was offered to all patients (3) who presented with acute cholecystitis during the pandemic confinement. Although it is in survey format, our article exposes a situation that has later been confirmed in subsequent publications, including larger numbers of cases.[3,4] We find it interesting that the letter from López Morales et al. states that during the pandemic phase (group 2), a period of 2 months, only 3 patients went to the emergency room for acute cholecystitis. This datum is in line with those of our national study, where 98% of those surveyed have noted a reduction in emergency room admissions due to acute cholecystitis, decreasing even >50% in 34% of the responses.

The survey conducted during the pandemic has also been an occasion to highlight some inadequacies that still exist in the management of cholelithiasis in our setting. For example, there is a need to implement scheduled cholecystectomy in a day surgery regimen, which, according to our survey, is a rare practice and is usually only performed in 38% of hospitals. Its increased use could contribute to improved healthcare during the current pandemic.

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Conversion to gastric bypass in patients with Barrett's esophagus after sleeve gastrectomy☆

Reconversión a *bypass* gástrico en pacientes con esófago de Barrett tras una gastrectomía vertical

Recently, your journal published an interesting case report by V. Lucas et al.[1] on the appearance of Barrett's esophagus (BE) in a patient 8 years after sleeve gastrectomy (SG). This patient presented high-grade dysplasia on a lesion <1 cm and was successfully treated endoscopically. This is where the controversy appears: the authors state that a gastric bypass was also performed.

As described in the letter, in recent years there has been a significant concern regarding the appearance of *de novo* gastroesophageal reflux, esophagitis and BE, with or without symptoms of gastroesophageal reflux disease (GERD), in patients after SG.[2–4] Reports of adenocarcinoma (ADC) of the esophagus in several cases within a few years of SG further added to the controversy.[5,6] This undoubtedly raises concern among surgeons treating obesity and esophageal cancer, given that SG is the most widely used bariatric technique. The IFSO has already determined the optimal endoscopic follow-up after SG,[7] but the question that remains (which they also leave open in the publication) is what to do with post-SG BE.

Regarding the patient who underwent the gastric bypass: what was her BMI? Did she report heartburn or frequent vomiting? Did she take medication for GERD?

I believe that all these data are important when making the decision to reoperate any patient with another surgery that is not without risks. This was already stated in 2020 by M. Guingand et al. in an interesting Letter to the Editor.[8] These authors proposed performing endoscopic antireflux mucosectomy (ARMS) instead of gastric bypass and presented a case with good results.

As a surgeon, and perhaps at odds with gastroenterologists,[9] I think that a patient with BE will benefit more from an antireflux operation than from chronic treatment with proton pump inhibitors (PPI), which only limits acid reflux and not bile reflux. However, a gastric bypass does not have the same morbidity as laparoscopic fundoplication.

What to do with an asymptomatic young woman with BMI <30 kg/m^2 after SG with a diagnosis years later of short-segment BE without dysplasia?

In the first case (and the reason for this letter), I agree that gastric bypass could be the best option due to the appearance of high-grade dysplasia on BE, although endoscopic treatment was effective and other important data are lacking that would help make the decision. However, in the last proposed case, I would be more inclined towards medical treatment of GERD

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