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Barrett's esophagus is a controversial condition

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Dear Editor,

We wish to respond to the comments provided by José Miguel Esteban López-Jamar and Ravishankar Asokkumar (1) about our report in this Journal (2). Some of the comments were perhaps prompted by inadequately explained or understood concepts, since they mention aspects that we never even considered or stated.

1. We never claimed that the article was a "detailed systematic review", neither in the table of contents of the Journal nor in the article text. In the table of contents, our paper appears as a "special article" and was later described as a review. We believe that this special article fits in the category known as a "narrative review" or "brief review", a type of publication that is included in many major international medical journals.

Given the limitations in terms of text length and number of references, such a review cannot be detailed or systematic in any way whatsoever, which does not mean that it is superficial. Furthermore, we do not think that there is a "conflict of interest" involved in reporting our publications. Our Esophageal Disorders Unit has been caring for patients and performing research for over 30 years. As a result, we are highly

experienced in scientific studies concerning nearly all aspects of Barrett's esophagus, with articles published in some of the most significant medical journals dealing with surgery and gastroenterology. In fact, self-referencing is common practice amongst the majority of authors.

The references provided also include benchmark research on Barrett's esophagus by the most respected authors such as Fitzgerald, Pera, Peters, Spechler and Lagergren, among others. Hence, we also disagree with their comment "...neglecting seminal papers on the topic". Indeed, we may have omitted some important reports, but as we have previously mentioned, our paper was not intended to be an exhaustive review.

2. It is clear that no subspecialty called "esophagology" exists. However, the term "esophagologist" is used worldwide in the clinical practice, although it is a somewhat colloquial term to describe a physician (gastroenterologist, endoscopist, pathologist, digestive surgeon or thoracic surgeon, etc.) who specializes in esophageal diseases, including their etiopathogenic, diagnostic and therapeutic aspects. One only has to attend the international meetings of the International Society for Diseases of the Esophagus (ISDE) to confirm this.

The authors never "...focused only on selected risk factors and did not represent the importance of other risk factors." This was not a paper on risk factors for the progression of Barrett's esophagus to malignancy. In fact, it was only briefly mentioned in the introduction, specifically indicating that low-grade dysplasia represents the highest risk. Furthermore, we have studied various risk factors, as shown by our publications.

3. Questioning the importance of gastroesophageal reflux (GER) in the pathogenesis of Barrett's esophagus is contrary to most expert opinions and the medical literature (3). Even though it is based on two references, one about Barrett's esophagus in Asian patients and another retrospective study from 2009 performed in a single hospital where GER diagnosis is based solely on symptoms, without objective findings. As many authors have found, Barrett's esophagus is often oligosymptomatic. These references do not seem to correspond to "seminal papers", as the authors claim.

4. Furthermore, minimizing the relevance of genetic inheritance in the pathogenesis of Barrett's esophagus just does not seem right. There is a subset of patients who do suffer from familial Barrett's esophagus and this subset represents up to 7% of cases according to some studies. These patients also have a higher risk of progression to malignancy (4). The fact that the literature has not attached a greater importance to this subset may result from the difficulty to perform in-depth studies in affected family members.

5. The authors state that "anti-reflux surgery offers no superior benefit compared to proton pump inhibitors in reducing esophageal adenocarcinoma rates", based on a 2003 meta-analysis. This is at least a controversial claim. A more recent meta-analysis (5) found exactly the opposite and this debate is still active. We have not been able to reach a definite conclusion, even after 30 years of randomized studies, as discussed in our report.

6. The authors have incorrectly assumed our opinion about the "lack of emphasis on endoscopic therapies". In fact, the opposite may be assumed from our report. However, that was also not the goal of our research. The expression "a hammer looking for a nail" regarding the use of radiofrequency is not ours, but was taken from Rubenstein JH in a report published in the journal *Gastroenterology* in 2014.

As we discuss in our paper, we use radiofrequency as eradication therapy for Barrett's esophagus. This therapeutic option represents the present rather than just the future state of the art.

To conclude, as stated in the introduction to our paper in *The Spanish Journal of Gastroenterology (Revista Española de Enfermedades Digestivas),* "Barrett's esophagus is a controversial condition". However, only from respectful disagreement and confrontation of opinions will we be able to reach a consensus that benefits patients.

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