Reviewer’s report

Title: Limited utilization of serologic testing in patients undergoing duodenal biopsy for celiac disease

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Reviewer: PAOLA IOVINO

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In this paper, Wiland et al. retrospectively evaluate the use of celiac serology in a cohort of patients undergoing endoscopy who had been evaluated for duodenal biopsy. Although authors are aware that guidelines suggest, in presence of a clinical suspect of celiac disease, to perform sampling after/along with serological tests, their conclusions were that most duodenal samples has been sent without a prior evidence of positive tests that is in turn an evaluation of the adherence of their institution to guidelines

Comments:

There are several missing information on the biopsy sampling. Could the authors clarified if the biopsies were oriented and how many fragments were taken from each patient and from which part of duodenum? In order to reduce confounding factors, I would also suggest to use a standardized classification to categorize duodenal histology (e.g. modified Marsh or Corazza Villanacci Classifications).

At the end of the introduction (Page 4), the authors state that they “retrospectively examined the utilization of celiac serology in a cohort of patients who had been sent for duodenal biopsy”. It is relatively unclear if the authors focus their work only on the low adherence of their institution to published guidelines (data that is already known in literature, Parakkal D. et al Do gastroenterologists adhere to diagnostic and treatment guidelines for celiac disease? J Clin Gastroenterol. 2012) or also on the presence of other diseases in non duodenal samples (page 5) regardless of celiac serology.

As they state in the discussion, other non-duodenal histological findings are common in CD patients, but it’s known that it is not mandatory multiple sampling. Thus, could the authors better explain the value of those data? are they suggesting to perform more biopsies in serological positive patients?

Next they examine the importance of “other” duodenal findings (non specific duodenitis and partial villous atrophy and increased intraepithelial lymphocytes – PVA IEL-) in serology negative patients. Again, could the authors please clarify what they mean with “partial villous atrophy and increased intraepithelial lymphocytes”? A partial villous atrophy can be classified according to Marsh classification of CD lesions, as modified by Oberhuber et al, as type 3a lesion (e.g. destructive lesion, characterized by mild villous flattening, with an increase of IEL greater that 40 per 100 lymphocytes and crypt hyperplasia) or with error sampling if the biopsies were not oriented.
I would suggest to review the paper according to the suggestions and reevaluate more specifically their data.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I have no competing interests