Reviewer’s report

Title: The Frequency Of Microscopic And Focal Active Colitis In Patients With Irritable Bowel Syndrome

Version: 2 Date: 18 May 2011

Reviewer: Darren Brenner

Reviewer’s report:

Dear Authors:

Thank you for the submission of your paper entitled: The Frequency of Microscopic and Focal Active Colitis in Patients with Irritable Bowel Syndrome. This is a very interesting and important topic which requires further study. The following are my comments regarding your manuscript.

--->Major Compulsory Revisions

1. Could you please comment whether the study was performed prospectively or retrospectively. I could not find this information.

2. Why did you choose to use Rome II Criteria given that collection of data was initiated in 2007 (after Rome III was released)?

3. The controls that were chosen for comparison turned out to be neither matched for age nor sex. There were significant differences between the IBS and control groups and most importantly, the control group was significantly older. Given that you are making a comparison between IBS and a disorder which is age based and has a higher prevalence in females (collagenous colitis), it appears that you tried to statistically account for these differences using logistic regression? I do not believe this can be done as the comparison group for a prevalence study such as this one should be matched for at least age.

4. Did you control for other factors associated with the development of Microscopic Colitis (Meds/Tobacco use..)

5. Backgroup section: last sentence. I am not sure this is the exact purpose of the study. The goal was to define the prevalence of histopathologic changes consistent with current definitions of MC and FAC in a cohort of patients meeting IBS criteria. Subsets of IBS patients have been shown to have inflammatory changes in colonic biopsies which have not been completely elucidated. There may be some overlap.

6. Why was the celiac assessment performed using Gliaden antibodies when USA and European Celiac Societies recommend tissue transglutaminase. This is important given the increased prevalence of celiac in patients with either IBS or microscopic colitis.

7. The IBS population used in this study was only 55% female. Is this typical of the IBS population in Turkey?
8. As far as I can tell the histologic definition you used for focal active colitis was never described. Please add this to the methods section.

9. Results section: last paragraph: You comment that LC differed among IBS subtypes (p<0.01) was this for IBS-D vs M and C or D+M vs C. Please describe where the significant differences are b/t groups. This also needs to be made clear in Figure II.

10. Discussion: 2nd sentence. This statement needs a citation as does the study by Chey et al which is not cited in the bibliography.

11. Discussion: Paragraph starting with "MC is a rare disease" you comment that Tuncer et al., found a prevalence of MC in 23.3% of IBS patients but the study actually looked at patients with symptoms consistent with IBS. This should be clarified.

12. Discussion section: You must discuss the limitations of your study and including #3 above.

13. Discussion section: Paragraph on FAC. I am not sure of the take home message. Are you stating that all patients should be biopsied b/c there may be microscopic changes. Please clarify further.

14. Conclusion: You comment as you did in the abstract that MC can be identified in pts w/IBS-D and women but I did not see any stats in the article regarding the increased prevalence in women. Please comment or add this data.

15. Table I: Based on the currently available data, this list appears extremely restrictive. Can you comment how you chose these factors to include as "alarm features."

---Minor Essential Revisions---

1. Material and Methods: 2nd paragraph 1st sentence change "Roma II" to "Rome II"

2. Discussion paragraph starting with "MC" I wouldn't use the shorthand at the beginning of a paragraph. Spell out Microscopic Colitis.

3. Conclusion: Instead of stating the "It may be reasonable" consider changing to "It appears reasonable."

4. Tables II and III and Figure I: please change "lymphositic" to "lymphocytic"

---Discretionary revisions---

1. In the conclusion portion of the abstract: you comment upon the fact that MC is more common in older women. I would consider adding some data in the results section to validate this finding.

2. Background Section:

1st sentence: consider changing "disease" to "disorder"

2nd and 3rd sentences: I would consider restructuring these sentences. You could consider something like the following: This disorder is characterized by abdominal pain ro dicomfort associated with altered bowel habits.
2nd paragraph sentence starting with collagenous colitis: I would remove this removing this statement as it comes before your differentiation of MC as 2 subtypes. I think the explanation of the 2 subtypes as you have explained them is sufficient.

3. Results section paragraph starting with "The control group subjects did not have..." Consider removing this paragraph as it is likely unnecessary.

4. Results section sentence beginning with "However, lymphocytic colitis differed..." This flows better if placed before your disussion on FAC as it directly related to the MC portion of the findings. Then the final sentence regarding the incidence of FAC can be added to the paragraph ending with "FAC was found in 6.6%..."

5. Discussion: 2 paragraphs starting with "A retropective study

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

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

**Declaration of competing interests:**

I declare I have no competing interests.