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PEER-REVIEW REPORT

Name of journal: World Journal of Gastroenterology
Manuscript NO: 35148
Title: Microbial dysbiosis in spouses of ulcerative colitis patients: any clues to disease pathogenesis?
Reviewer's code: 02821664
Reviewer's country: Canada
Science editor: Ze-Mao Gong
Date sent for review: 2017-06-24
Date reviewed: 2017-06-26

CLASSIFICATION	LANGUAGE EVALUATION	SCIENTIFIC MISCONDUCT	CONCLUSION
[ ] Grade A: Excellent	[ ] Grade A: Priority publishing	Google Search:	[ ] Accept
[Y] Grade B: Very good	[Y] Grade B: Minor language polishing	[ ] The same title	[ ] High priority for publication
[ ] Grade C: Good	[ ] Grade C: A great deal of language polishing	[ ] Duplicate publication	[ ] Rejection
[ ] Grade D: Fair	[ ] Grade D: Rejected	[ ] Plagiarism	[Y] Minor revision
[ ] Grade E: Poor		[Y] No	[ ] Major revision
		BPG Search:	
		[ ] The same title	
		[ ] Duplicate publication	
		[ ] Plagiarism	
		[Y] No	

COMMENTS TO AUTHORS

The Editorial is well written and does offer a critical appraisal of the topic covered in the original manuscript (which I did not see, so I base this statement on what I read). Limitations and the need to cautious interpretation are well discussed. However, a few additional points require consideration: General Comments: 1. The original study focuses on UC. I would restrict most comments to UC and not IBD overall. 2. Core tip, page 3: the major finding of the original study appears to be that there is some microbial sharing between spouses and that this alone is likely insufficient to induce disease, as well stated in the Editorial. This would suggest that in relation to the cause vs. effect question, posed at the top of the 'core tip', this study offers more support for effect than cause, although one cannot directly conclude this from this study. It might be a good idea to reflect this point in the 'core tip', obviously with need for caution in interpreting results. 3. The main additional criticism on the original study that I would add

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(assuming this was not done in the original study – I did not receive access to the manuscript the Editorial relates to) is that spousal effect was not controlled for. Beyond the inclusion of local healthy controls, as discussed on the top of page 7, I would have liked to see if spouses of UC patients are more or less similar to their partners than in couples without GI disease. 4. Another important point to consider is whether one partner having UC could impact the spouse's microbiome indirectly, through diet for example. It is possible that when one family member has a chronic intestinal disease the diet of the entire household changes. Specific Comments: 1. Abstract, Page 2: suggest changing 'If this finding impacts..' to 'Whether this finding impacts…' 2. Editorial, bottom of page 3: the statement that 'this symbiotic microbial cell population (or microbiome) outnumbers that of the human host by 10:1' should be modified in light of the following paper: Are We Really Vastly Outnumbered? Revisiting the Ratio of Bacterial to Host Cells in Humans. Sender et al., Cell. 2016 Jan 28;164(3):337-40. doi: 10.1016/j.cell.2016.01.013. 3. When abbreviated after first use Genus names should be followed by a '.'. For example, E. coli, not E coli. 4. Top of page 6 – the first sentence should be rewritten. I think you are trying to say that including a follow up could have clarified whether spousal microbes impact disease course, but the way this is written now the message is not clear. 5. There are several extra spaces throughout the Editorial that should be removed.

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PEER-REVIEW REPORT

Name of journal: World Journal of Gastroenterology
Manuscript NO: 35148
Title: Microbial dysbiosis in spouses of ulcerative colitis patients: any clues to disease pathogenesis?
Reviewer's code: 00004011
Reviewer's country: Greece
Science editor: Ze-Mao Gong
Date sent for review: 2017-06-24
Date reviewed: 2017-06-27

CLASSIFICATION	LANGUAGE EVALUATION	SCIENTIFIC MISCONDUCT	CONCLUSION
[ ] Grade A: Excellent	[ ] Grade A: Priority publishing	Google Search:	[Y] Accept
[Y] Grade B: Very good	[Y] Grade B: Minor language polishing	[ ] The same title	[ ] High priority for publication
[ ] Grade C: Good	[ ] Grade C: A great deal of language polishing	[ ] Duplicate publication	[ ] Rejection
[ ] Grade D: Fair	[ ] Grade D: Rejected	[ ] Plagiarism	[ ] Minor revision
[ ] Grade E: Poor		[Y] No	[ ] Major revision
		BPG Search:	
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		[Y] No	

COMMENTS TO AUTHORS

It is an interesting editorial and well written

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PEER-REVIEW REPORT

Name of journal: World Journal of Gastroenterology
Manuscript NO: 35148
Title: Microbial dysbiosis in spouses of ulcerative colitis patients: any clues to disease pathogenesis?
Reviewer's code: 00381580
Reviewer's country: United States
Science editor: Ze-Mao Gong
Date sent for review: 2017-06-24
Date reviewed: 2017-06-27

CLASSIFICATION	LANGUAGE EVALUATION	SCIENTIFIC MISCONDUCT	CONCLUSION
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COMMENTS TO AUTHORS

This is a thorough editorial. I have several comments and suggestions. 1) Page 5- "fecal samples were collected from 8 UC patients and their healthy partners"- Would specify how many samples and at what time points they were collected as this is a weakness of the study. 2) Page 5- "Microbiota dysbiosis and altered microbial metabolism were detected in both UC patients and their healthy partners, with the most relevant genera in the latter group being Akkermansiam, Bacteroides, Escherichia, Lactobacillales, Klebsiella, and Parabacteroides. " You point out the differences they noted among the two groups but you don't highlight enough what the similarities between the two groups was and this is the thrust of their argument- that the two groups were similar. If you don't think the authors of the study highlighted the similarities enough this has to be mentioned. 3) I think the reader would also want to know how they determined that the healthy control had a "dysbiosis" when they did not use a control population of

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healthy neighbors or others in the same community- also a limitation of the study as you point out. 4) Conclusions- if you are happy that the authors determined that the healthy partners had a "dysbiosis" and did not have IBD, maybe you could comment that further studies are needed but dysbiosis alone is likely not enough to result in IBD which is why the partners are not thought to develop IBD.