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PEER-REVIEW REPORT

Name of journal: *World Journal of Clinical Cases*

Manuscript NO: 91615

Title: Discontinuation of therapy in inflammatory bowel disease: Current views

Provenance and peer review: Invited Manuscript; Externally peer reviewed

Peer-review model: Single blind

Reviewer's code: 01115220
Position: Editorial Board
Academic degree: BSc, FEBG, MD, MRCP
Professional title: Assistant Professor, Doctor
Reviewer's Country/Territory: United Kingdom
Author's Country/Territory: Croatia

Manuscript submission date: 2023-12-31

Reviewer chosen by: AI Technique

Reviewer accepted review: 2024-01-02 08:44

Reviewer performed review: 2024-01-02 13:44

Review time: 5 Hours

Scientific quality	[ ] Grade A: Excellent [ ] Grade B: Very good [Y] Grade C: Good [ ] Grade D: Fair [ ] Grade E: Do not publish
Novelty of this manuscript	[ ] Grade A: Excellent [ ] Grade B: Good [Y] Grade C: Fair [ ] Grade D: No novelty
Creativity or innovation of this manuscript	[ ] Grade A: Excellent [Y] Grade B: Good [ ] Grade C: Fair [ ] Grade D: No creativity or innovation

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Scientific significance of the conclusion in this manuscript	[ ] Grade A: Excellent [Y] Grade B: Good [ ] Grade C: Fair [ ] Grade D: No scientific significance
Language quality	[ ] Grade A: Priority publishing [Y] Grade B: Minor language polishing [ ] Grade C: A great deal of language polishing [ ] Grade D: Rejection
Conclusion	[ ] Accept (High priority) [ ] Accept (General priority) [Y] Minor revision [ ] Major revision [ ] Rejection
Re-review	[Y] Yes [ ] No
Peer-reviewer statements	Peer-Review: [ ] Anonymous [Y] Onymous
	Conflicts-of-Interest: [ ] Yes [Y] No

SPECIFIC COMMENTS TO AUTHORS

I enjoyed reading this narrative review of the outcomes, pros and cons of treatment de-escalation in IBD. In general, this is a well-presented and readable review. Whilst it does not add a substantial amount to the already large body of literature in this area, it is a useful and concise update as to the current situation. This obviously remains an important and somewhat contentious area and the author's conclusion are balanced. There are several areas that may benefit from further detail or amendments. 1. The abstract is very vague on exact details. Could more specific details on risk factors or relapse rates with individual agents be included? This is likely to entice more readers. 2. Throughout the paper, the authors accept that reintroduction of therapy is effective in recapturing disease control. However they quote success rates of ~ 66% to support this. It could easily be argued that one third of patients with chronic debilitating failing to recapture remission is actually a substantial proportion and retreatment may not be as effective as the authors would perhaps have us believe? 3. In the modern era, where sequencing of advanced therapies is often the reality and many patients will have received more than one advanced therapy, may clinicians are likely to be interested in

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how this disease behaviour may alter decisions about de-escalation, do the authors have an advice or data on this cohort? Most of the data seem to relate to TNFi as 1st advanced therapy. Are there data specific to de-escalation of subsequent lines of therapy? Many clinicians would probably assume that these are higher-risk patients, based on duration or behaviour and perhaps the same parameters apply? It would be helpful to try and separate if different pathways should be applied for subsequent lines of advanced therapies? 4. The figures given in the introduction of only onset IBD are confusing. If ¼ are diagnosed before 20, is the 1/5 referred to at aged < 10, one fifth of the 25% (so 5% overall) or 20% overall (which seems to only leave 5% in the adolescent group (which seem incorrect?). 5. The inference on the section about 5ASA therapies, especially "topical" therapies would seem to be about 5ASA as sole therapies, not when already combined with other agents? It would be worth being really explicit here about which studies are sole therapy compared to combo-therapy 6. I think on page 7, the authors mean rectal therapies when covering "topical" therapies? All 5ASA drugs probably work topically whether given rectally or as controlled-release oral forms. Please clarify. 7. Use do not rather than didn't -page 10 line 12. 8. On page 13, there is a paragraph seemingly about ustekinumab therapy. This appears to concern the use of ustekinumab in psoriasis rather than IBD? The long-term behaviour of psoriasis is likely very different from IBD and hence I am not sure what relevance this has to IBD. Either this section should be removed or some comparator data included on the rates of psoriasis disease recurrence with other agents (TNFi, ciclosporin etc). 9. The authors correctly identify the use of fecal calprotectin as a marker to predict relapse of IBD. However, they noticeably fail to provide any guidance on what action should be taken on these results? It is perfectly reasonable to monitoring the calprotectin but should treatment be re-instated just for the raising calprotectin.

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Reviewer's code: 02445675
Position: Peer Reviewer
Academic degree: MD, PhD

Professional title: Assistant Professor, Doctor

Reviewer's Country/Territory: Italy

Author's Country/Territory: Croatia

Manuscript submission date: 2023-12-31

Reviewer chosen by: AI Technique

Reviewer accepted review: 2023-12-31 19:52

Reviewer performed review: 2024-01-06 21:54

Review time: 6 Days and 2 Hours

Scientific quality	[ ] Grade A: Excellent [ ] Grade B: Very good [ ] Grade C: Good [Y] Grade D: Fair [ ] Grade E: Do not publish
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Creativity or innovation of this manuscript	[ ] Grade A: Excellent [ ] Grade B: Good [ ] Grade C: Fair [Y] Grade D: No creativity or innovation

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Re-review	[Y] Yes [ ] No
Peer-reviewer statements	Peer-Review: [Y] Anonymous [ ] Onymous
	Conflicts-of-Interest: [ ] Yes [Y] No

SPECIFIC COMMENTS TO AUTHORS

The article is not original per se but it is quite interesting. It is a generic review on the important topic of discontinuing the maintenance therapy in IBD Clinical settings. The quality of the article is overall low, but with some improvements it may be considered for publication. Please clarify: in the biologic therapy chapter (as well as in the Fig. 1), you mention low serum anti-TNF levels are linked to decreased risk of relapse. Did you intend low serum anti IFX antibody instead?