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ESPS Peer-review Report

Name of Journal: World Journal of Gastroenterology

ESPS Manuscript NO: 5109

Title: Distance Management of Inflammatory Bowel Disease: systematic review and meta-analysis

Reviewer code: 02445103

Science editor: Qi, Yuan

Date sent for review: 2013-08-16 10:20

Date reviewed: 2013-08-26 10:14

CLASSIFICATION	LANGUAGE EVALUATION	RECOMMENDATION	CONCLUSION
<input type="checkbox"/> Grade A (Excellent)	<input checked="" type="checkbox"/> Grade A: Priority Publishing	Google Search:	<input type="checkbox"/> Accept
<input type="checkbox"/> Grade B (Very good)	<input type="checkbox"/> Grade B: minor language polishing	<input type="checkbox"/> Existed	<input type="checkbox"/> High priority for publication
<input checked="" type="checkbox"/> Grade C (Good)	<input type="checkbox"/> Grade C: a great deal of language polishing	<input type="checkbox"/> No records	<input type="checkbox"/> Rejection
<input type="checkbox"/> Grade D (Fair)	<input type="checkbox"/> Grade D: rejected	BPG Search:	<input type="checkbox"/> Minor revision
<input type="checkbox"/> Grade E (Poor)		<input type="checkbox"/> Existed	<input type="checkbox"/> Major revision
		<input type="checkbox"/> No records	

COMMENTS TO AUTHORS

Certainly the question of whether IBD patient self management can be optimized through "distance" techniques is a worthy topic for consideration. This paper is well written and the methodology for the most part was spot-on. A few theoretical concerns that weaken my enthusiasm: 1. Distance management was narrowly defined, which is a strength on one hand but a weakness on the other. Many IBD providers provide "distance management" through email, telephone etc. as part of standard care. Was there any attempt to determine that standard care conditions excluded these possibilities or that self-management studies that were considered for inclusion/exclusion did not have this component? 6 studies for systematic review/meta-analysis is hardly notable. furthermore, the absence of findings results in what is likley a premature conclusion about the benefit of these interventions as telehealth for a variety of other conditions has significant effects. i am not a meta-analysis expert, but it would be interesting to compare the 6 distance management trials to other in person self-management programs as a way of determining whether its self-management or distance self-management that is useful. similarly, i would probably have included the non-RCTs in this to increase the number of studies reviewed.



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ESPS Peer-review Report

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ESPS Manuscript NO: 5109

Title: Distance Management of Inflammatory Bowel Disease: systematic review and meta-analysis

Reviewer code: 02445089

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CLASSIFICATION	LANGUAGE EVALUATION	RECOMMENDATION	CONCLUSION
<input type="checkbox"/> Grade A (Excellent)	<input checked="" type="checkbox"/> Grade A: Priority Publishing	Google Search:	<input type="checkbox"/> Accept
<input checked="" type="checkbox"/> Grade B (Very good)	<input type="checkbox"/> Grade B: minor language polishing	<input type="checkbox"/> Existed	<input type="checkbox"/> High priority for publication
<input type="checkbox"/> Grade C (Good)	<input type="checkbox"/> Grade C: a great deal of language polishing	<input type="checkbox"/> No records	<input type="checkbox"/> Rejection
<input type="checkbox"/> Grade D (Fair)	<input type="checkbox"/> Grade D: rejected	BPG Search:	<input checked="" type="checkbox"/> Minor revision
<input type="checkbox"/> Grade E (Poor)		<input type="checkbox"/> Existed	<input type="checkbox"/> Major revision
		<input type="checkbox"/> No records	

COMMENTS TO AUTHORS

The authors present a review and meta-analysis of studies on distance management of inflammatory bowel disease. The authors conclude that the studies indicate that distance management of IBD decreases clinic visit utilization, but it does not significantly improve patient's quality of life, relapse rates, or hospital admission rates. This conclusion is based on the data, and I do not have significant comments pertaining to the analyses. I would find it useful if the authors provided some additional information in the Study Characteristics. For example, when describing the Elkjaer study, the authors point out the disease severity, but did not mention disease severity for any of the other studies (it is listed in Table 1, but if information is provided for one study in text, it should be provided for all studies in text). Finally, I would like the authors to clarify whether a reduction in clinic visits would be useful for the patient (and society). Even though fewer clinic visits might free up resources for new/other patients, are there any risks to reducing clinic visits for patients?



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ESPS Peer-review Report

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Title: Distance Management of Inflammatory Bowel Disease: systematic review and meta-analysis

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CLASSIFICATION	LANGUAGE EVALUATION	RECOMMENDATION	CONCLUSION
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<input type="checkbox"/> Grade B (Very good)	<input type="checkbox"/> Grade B: minor language polishing	<input type="checkbox"/> Existed	<input type="checkbox"/> High priority for publication
<input checked="" type="checkbox"/> Grade C (Good)	<input type="checkbox"/> Grade C: a great deal of language polishing	<input type="checkbox"/> No records	<input type="checkbox"/> Rejection
<input type="checkbox"/> Grade D (Fair)	<input type="checkbox"/> Grade D: rejected	BPG Search:	<input type="checkbox"/> Minor revision
<input type="checkbox"/> Grade E (Poor)		<input type="checkbox"/> Existed	<input type="checkbox"/> Major revision
		<input type="checkbox"/> No records	

COMMENTS TO AUTHORS

This is an interesting and well-written systematic review and meta-analysis on a relevant and current topic. Please see my comments to clarify/strengthen the manuscript below: 1. Plagiarism - the box for "Is there plagiarism in this manuscript?" is checked yes. Is this true? If so this manuscript should not be considered for publication. 2. Given that this review focuses on quality-of-life as a primary outcome, it may be pertinent to state the reason that manuscripts concentrating on stress and lifestyle management were excluded. Did these reports lack "distance" management, or was "distance" management limited to stress and lifestyle techniques? Please clarify. If there are studies involving "distance" management for stress and lifestyle, it may be interesting to include the results. 3. Reference 28 refers to a trial published by Kennedy et al. in 2003, yet the Figures and Tables reference Kennedy 2004. It seems Reference 28 should be changed to the trial by Kennedy et al. published in 2004. Please clarify. 4. For completeness, it would be appropriate to note in the text that patients with indeterminate colitis were grouped with patients with ulcerative colitis, especially given the focus on subgroup analysis. 5. It would be interesting to expand upon what was generally involved in management of patients within the comparator groups for each trial. For example, given that the main outcome showed a difference in clinic utilization, it would be worthy to note how often patients were generally seen in follow-up in the comparator groups. Also, were laboratory tests and phone-call follow-ups without clinic visits generally part of management within the comparator group? If this information can be extracted, it would be interesting to include. 6. The extreme heterogeneity renders the results somewhat difficult to interpret. Were the results for the overall group (rather than subgroups) not provided due to this heterogeneity? Even within subgroups, there were differences in specific techniques of web management and patient-directed open access clinic



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management, which can complicate extrapolation to real-life circumstances. Although this variability is unavoidable, this, along with the paucity of significant findings, make it difficult to say with any degree of certainty that the results of this study support the rationale for distance management of IBD patients (as stated in the first paragraph in the discussion). Rather, the concluding statements in the last paragraph of the manuscript seem more reasonable. In addition, some of the subgroup analyses include only a single study, and it may be worth noting where/if these results differ from those of the original trial (ie - in the Cross et al. trial, difference in quality of life between groups was only noted after adjustment for baseline disease knowledge). Finally, it may be interesting to explore in the discussion whether a decrease in clinic visit utilization is beneficial for the patient and/or overall health-care resource allocation, given that significant changes in relapse rates and quality-of-life were not seen (and given that the Elkjaer et al. trials favored the control group in terms of relapse rate). It would also be interesting to include whether adherence to medication rates, baseline medication regimens, or duration of relapses differed between groups. If these latter issues cannot be explored in this study, they may be interesting questions to tackle in future trials. 7. The attrition bias is appropriately noted in the manuscript. It may be helpful to briefly include reasons for withdrawal from the study, and to explore whether this may limit practicality of distance management if adherence rates are low. 8. There are just a few details in this manuscript that seemed discrepant to me: 1) The text states that in the Cross et al. study, 19 patients completed the trial in the BAC group, whereas Table 1 states that 18 patients completed