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ESPS Peer-review Report
Name of Journal: World Journal of Gastroenterology
Ms: 3681
Title: Effects of Fufang Biejia Ruangan Pian on hepatic fibrosis in vivo and in vitro
Reviewer code: 00227406
Science editor: Gou, Su-Xin
Date sent for review: 2013-05-14 15:54
Date reviewed: 2013-05-25 07:28

CLASSIFICATION	LANGUAGE EVALUATION	RECOMMENDATION	CONCLUSION
[ ] Grade A (Excellent)	[ ] Grade A: Priority Publishing	Google Search:	[ ] Accept
[Y] Grade B (Very good)	[ ] Grade B: minor language polishing	[ ] Existed	[ ] High priority for publication
[ ] Grade C (Good)	[Y] Grade C: a great deal of language polishing	[ ] No records	
[ ] Grade D (Fair)		BPG Search:	[ ]Rejection
[ ] Grade E (Poor)	[ ] Grade D: rejected	[ ] Existed	[Y] Minor revision
		[ ] No records	[ ] Major revision

COMMENTS TO AUTHORS

The paper by Yang et al describes the anti-fibrogenic effects of a Chinese herbal medication Fufang Biejia Ruangan Pian (FFBJRGP). The data strongly suggests that FFBJRGP may be therapeutically useful adjunct in patients with hepatic fibrosis. I have a few comments- 1. The paper has not been presented according to the journal recommendations. The tables and figures should appear separately when submitted for review. 2. The English requires significant improvement and I would suggest that a native English speaker proof reads the text and makes the necessary adjustments. 3. the number of rats experimented on is hidden away in the methods and materials section and should be included in each of the subsequent tables. 4. Why were there only 6 control rats and 12-14 rats in the model and treatmant groups? 5. There is no indication of the likely active ingredients within FFBJRGP that might be contributing to its anti-fibrogenic activity. This would be useful either in the Introduction or Discussion sections of the manuscript. 6. Colchicine is used as a comaparator by which FFBJRGP is assessed. Their effectiveness in preventing/ reducing hepatic fibrosis appears equivalent from the paper presented. However human trials of colchicine in hepatic fibrosis have been disappointing. Do the authors believe that FFBJRGP could be potentially useful in human hepatic fibrosis. 7. Following on from the previous comment, the discussion should include a note about the potential human application of FFBJRGP. Clearly management of hepatic fibrosis should attempt to reverse the underlying fibrogenic aetiology. How do the authors think the FFBJRGP could be useful in human subjects- as treatment or maybe even to prevent the development of hepatic fibrosis? I think the paper is worthy of consideration for publication, but I would suggest that the above points are addressed. I would be happy to re- review following re-submission with the aforementioned changes.

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ESPS Peer-review Report
Name of Journal: World Journal of Gastroenterology
Ms: 3681
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CLASSIFICATION	LANGUAGE EVALUATION	RECOMMENDATION	CONCLUSION
[ ] Grade A (Excellent)	[ ] Grade A: Priority Publishing	Google Search:	[ ] Accept
[ ] Grade B (Very good)	[Y] Grade B: minor language polishing	[ ] Existed	[ ] High priority for publication
[ ] Grade C (Good)	[ ] Grade C: a great deal of language polishing	[ ] No records	[Y]Rejection
[ ] Grade D (Fair)	[ ] Grade D: rejected	BPG Search:	[ ] Minor revision
[Y] Grade E (Poor)		[ ] Existed	[ ] Major revision
		[ ] No records	

COMMENTS TO AUTHORS

It's quite interesting investigation. Anti-fibrotic molecule is still main theme of chronic liver disease. Authors investigated the anti-fibrosis effect and potential mechanisms of FFBJRGP on hepatic fibrosis. Liver histology finding is most important finding in this study. Histologic improvement in In Vivo must be a primary end point in this study. However there is no describes how they measure the changes of degree of intrahepatic fibrosis. Changes of intrahepatic fibrosis is more important than other supportive surrogate markers (TGF-β, smad2/3, collagen deposition, etc). Quantitative data should be needed. Authors used colchicine group as a positive control. However, most of clinical data and systematic review did not recommended colchicine as anti-fibrotic drug in patients with cirrhosis any more. Smad 3 expression used as surrogate marker of intrahepatic fibrosis, but phosphorylated form is more important than total smad 3. It is not clear authors checked total smad3 or p-smad 3. In Vitro study MTT assay was done using single concentration of active ingredient. 0.55g/kg was treated in LX-2 cell. Please check the concentration of active ingredient and colchicine. Is 0.55g/kg right?

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ESPS Peer-review Report
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CLASSIFICATION	LANGUAGE EVALUATION	RECOMMENDATION	CONCLUSION
[ ] Grade A (Excellent)	[ ] Grade A: Priority Publishing	Google Search:	[ ] Accept
[ ] Grade B (Very good)	[Y] Grade B: minor language polishing	[ ] Existed	[ ] High priority for publication
[Y] Grade C (Good)	[ ] Grade C: a great deal of language polishing	[ ] No records	
[ ] Grade D (Fair)		BPG Search:	[ ]Rejection
[ ] Grade E (Poor)	[ ] Grade D: rejected	[ ] Existed	[ ] Minor revision
		[ ] No records	[Y] Major revision

COMMENTS TO AUTHORS

1) Provide information for the time-point when liver biopsies were performed (before, after treatment or both?) 2) There is discrepancy between statistical results (tables 1-3, tables 5 and 6) and conclusion. 3) Reference to the scepticism of traditional Chinese medicine in order to explain the pathophysiological mechanism of the substance under investigation is inappropriate for the international peer-reviewed scientific journal. 4) Avoid phrases like "induced by some critical cytokines", provide precise pathological mechanism, supported by bibliography