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ESPS Peer-review Report
Name of Journal: World Journal of Gastrointestinal Pathophysiology
ESPS Manuscript NO: 5511
Title: LIVER BIOPSY: ANALYSIS OF TWO SPECIALISTIC TEAMS RESULTS
Reviewer code: 00035193
Science editor: Song, Xiu-Xia
Date sent for review: 2013-09-13 09:29
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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	[Y] Minor revision
		[ ] No records	[ ] Major revision

COMMENTS TO AUTHORS

The study by Anania et al is an interesting one. The authors have reported their experience of liver biopsy based on two different approaches, US assisted performed by the Gastroenterologist (G team) vs. US guided performed by IR (IR), and reports no significant difference in rate of complications, but reports obtaining more adequate samples by the US assisted techniques by use of larger bore needles. The following are my comments on this study. 1.Despite the authors reporting the fact that the two groups of homogenous it is unclear as to why one approach of liver biopsy was chosen over the other, and element of selection bias is conceivable, and the decision for pursuing the type of liver biopsy was possibly led by the comfort level of the ordering physician. Certainly, the IR team performed more procedures as in-patient ($p < 0.0001$), and likely these patients were sicker as compared to those who had liver biopsy as outpatient. Despite rate of complications were not statistically different, numerically the rate of complications were much higher in biopsy by US guided (IR) group [10/59(17%) vs. 16/167 (9.5%), P= 0.15], and this difference probably would have reached statistical significance if two more patients had complications in the IR (US guided) group. As such the conclusion of similar outcomes in the two groups should be taken cautiously. A larger sample size in US guided group would have given us a more robust conclusion, and this limitation should be addressed in the discussion. It will be of interest to know the rate of complications separately in the sub groups with regular admission (US assisted biopsy with regular admission,48 vs US guided biopsy with regular admission, 38) and day hospital admission (US assisted biopsy with day hospital, 119 vs. US guided biopsy with day hospital, 21). This will possibly give us a comparison of two methods of liver biopsy in more homogenous groups of subjects. 2.Based on the AASLD guideline (Hepatology March 2009), an adequate number of portal tracts have been proposed to be

greater than 11 for optimal staging. However the authors choose to report a number of portal tracts as 6 for the current study. It will be of interest to report what % of the biopsy sample had optimal number of portal tracts for adequate evaluation of the liver biopsy sample in both groups, and should be reported. 3.Various complications, such pain, bleeding, organ punctures, etc should be reported in %.

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ESPS Peer-review Report
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CLASSIFICATION	LANGUAGE EVALUATION	RECOMMENDATION	CONCLUSION
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[ ] 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	[ ]Rejection
[ ] Grade D (Fair)	[ ] Grade D: rejected	BPG Search:	[Y] Minor revision
[ ] Grade E (Poor)		[ ] Existed	[ ] Major revision
		[ ] No records	

COMMENTS TO AUTHORS

Anania et al proposes an interesting study comparing the parameters of two approaches of liver biopsy – US assisted and US guided, performed by two teams – one of gastroenterologists (G) and one of interventional radiologist (IR). The study reports no significant differences in rates of complications while reporting more adequate sampling by the G team, who also used a 16 Gauge needle as compared to a smaller bore 18 Gauge needle by the IR team. The article has a very interesting idea behind it; however, it leaves room for improvement in several key aspects. I would recommend a minor revision taking into account the concerns below and a fast resubmission for publication. I would rate the language level as appropriate and needing only minor polishing, therefore acceptable for publication without further linguistic review. 1. By the nature of the study (retrospective analysis), inclusion bias is certain. It is unclear how one type of procedure was chosen over the other. In practice, the chosen approach is based on the performing physician's previous experience. 2. Patients were chosen from day hospital or regular admission care, which imply that some were in worse physical condition than the others, even having similar comorbidities. Even if the IR team performed fewer procedures, they were either carried out in patients needing more attention, or were referred to them on regulatory bases or by convenience (i.e. in some cases, for instance, viral hepatitis treatment can be started by both gastroenterologists and by virologists, both capable of performing liver biopsy; in Italy Internal Medicine physicians may receive patients in need of liver biopsy, therefore referring them to a more convenient service such as IR). The authors may want to approach this idea in the Discussions section, clarifying which is the case. 3. The low number of patients in the IR group makes statistical calculations regarding the number of complications somewhat doubtful. Was there a power analysis performed prior to the study? Is the number

sufficient to draw a definitive conclusion? This is extremely important, as adding a few patients to the IR group might invalidate the conclusion that both groups had similar outcomes. 4. Also, a subgroup analysis based on RA/DH and procedure type should be added and may provide further data for analysis. 5. Perhaps a more detailed description of all complications should be inserted in the results section. Also, an analysis without having "pain" as a complication may prove interesting. 6. The size of the biopsy sample and number of portal tracts is clearly linked to needle gauge, thus adding another factor in the overall equation. Perhaps the authors may add more information in this regard, along with a stratification of data coming from pathology assessment. How many samples were deemed unfit for analysis based on number of portal tracts? As far as guidelines go, either 8 or 11 portal tracts is a minimum for an accurate diagnosis. 7. The author state after declaring their conflicts of interest that all patients expressed consent prior to inclusion. Perhaps a small paragraph detailing this study's compliance to usual international regulations can be inserted in the Methods section. In conclusion, I believe this study is of interest to a wide range of practitioners and can be published after these issues are addressed.