

科学编辑第一次定稿工作清单			
下载时间	2021-03-03 00:45		
科学编辑签名		日期	
总编辑签名		日期	
手稿总数量	80		
约稿数量	45		
自由来稿数量	35		
科学编辑小结	拟接受手稿数量： 篇 拟退稿手稿数量： 篇 拟转投手稿数量： 篇 退稿率： %		
Copyright © 2021 Baishideng Publishing Group Inc			

序号	手稿编号	同行评议人ID	学术质量评级	语言质量评级	同行评议意见	科学编辑第一次定稿评论意见
1	54974	03604107, 05849395	Grade B (Very good), Grade C (Good)	Grade A (Priority publishing), Grade B (Minor language polishing)	(1)Reviewer 03604107: Thank you very much for the fine contribution; it is a very good and cogent paper. I would have been very happy if you should have included - as a short paragraph - other experiences in the field; i.e. not only US forensic psychiatric perspectives. I know the legal / forensic system differ substantially within countries, but are there similar works in, let's say, similar settings (UK, Canada...) ? Comparing worldwide / multicenter data would enormously enhance the validity of your findings, and why not, increase as well the number of interested readers. A single grammatical error:	The manuscript has been published. https://www.heighpubs.org/jfsr/abstract.php?id=jfsr-aid1020

					<p>---beginat (page 4: begin at)</p> <p>Figure 3 (page 13) is blurred: please submit a high resolution image.</p> <p>(2)Reviewer 05849395: Although this is the second retrospective study of OCRP, this update further confirms the effectiveness of the Washington D.C. model and identifies an optimal length of time for competency restoration. The new update will provide some beneficial reference for the practice and research of similar judicial competence restoration in the same cultural and legal context.</p>	
2	59700	03397272	Grade D (Fair)	Grade D (Rejection)	(1)Reviewer 03397272: English editing needed!	重新送审
3	60416	05182411	Grade C (Good)	Grade B (Minor language polishing)	<p>(1)Reviewer 05182411: In the manuscript entitled: "Intra-abdominal adipose depot variation in adipogenesis, lipogenesis, angiogenesis and fibrosis, and their relationships with insulin resistance and inflammation" the authors analyzed gene expression of adipogenesis, lipogenesis, angiogenesis and fibrosis in visceral ATs and evaluate relationships with IR and inflammation.</p> <p>In their study, the authors found that Although SCD1 mRNA levels were lowest in the ME, expression of DGAT-2 was highest in this depot (P<0.001). ME had the lowest VEGF-α mRNA levels but the highest of leptin (P<0.001). LOX expression was lower in the ME than in other depots (P<0.05) whereas COL6A3 expression was higher in the ME and RL (P<0.001). COL6A3 mRNA levels in the RL were negatively associated with IR indices (0.01<P<0.05), those in the OM with fasting glycemia and HOMA-IR (0.001<P<0.05). VEGF-α expression was positively related to plasma TNF-α and PAI-1 in the RL (0.001<P<0.05) and to PAI-1 in the OM (P<0.05), but negatively with PAI-1 in the ME (P<0.001). Fibrosis gene expression in RL and OM negatively correlated with inflammation (0.001<P<0.05), while positively correlating with PAI-1 in the ME (0.001<P<0.05). The authors concluded that gene expression in fibrotic RL, adipogenic OM and</p>	<p>1 Scientific quality: The manuscript describes a basic science about the relationship of Intra-abdominal adipose depot variation and insulin resistance and inflammation. The topic is within the scope of the <i>WJD</i>. (1) Classification: Grade C; (2) Summary of the Peer-Review Report: Athours achieve very nice results with their adopted protocol. This study requests some revisions before a further re-evaluation of the manuscript. The questions raised by the reviewers should be answered; (3) Format: There are 7 tables and 3 figures; (4) References: A total of 49 references are cited, including 1 reference published in the</p>

			<p>adipogenic and lipogenic ME relates to IR and inflammation in women with severe obesity, suggesting a contributory role for all three depots.</p> <p>Major comments:</p> <p>In general, the idea and innovation of this study, regards the analysis of adipose depot variation in adipogenesis, lipogenesis, angiogenesis and fibrosis, is interesting, because the role of these mediators is quite validated but further studies on this topic could be an innovative issue in this field could be open an innovative matter of debate in literature by adding new information. Moreover, there are few reports in the literature that studied this interesting topic with this kind of study design.</p> <p>The study was well conducted by the authors; However, there are some concerns to revise that are described below.</p> <p>The introduction section resumes the existing knowledge regarding the main aspects linked with the impact of adipose depot variation in adipogenesis. However, as the importance of the topic, before a further re-assessment of the manuscript, the reviewer strongly recommends to update the literature through read, discuss and must cites in the references with great attention of recent interesting articles, that helps the authors to better introduce and discuss the aim of the study in light of also adipose depot variation in inflammation.</p> <p>The authors should be better specified, at the end of the background section, the rationale of the study and the aim of the study with the null hypothesis.</p> <p>The discussion section appears well organized with the relevant paper that support the conclusions, even if the authors should better discuss the importance of early markers in the diagnosis of systemic inflammatory diseases. The conclusion should reinforce in light of the discussions.</p> <p>In conclusion, I am sure that the authors are fine clinicians who achieve very nice results with their adopted protocol. However, this study, in my view does not in its current form satisfy a very high scientific requirement for publication in this journal and requests some revisions before a further re-evaluation of the manuscript.</p>	<p>last 3 years; (5) Self-cited references: There are 5 self-cited references. The self-referencing rates should be less than 10%. Please keep the reasonable self-citations (<i>i.e.</i> those that are most closely related to the topic of the manuscript) and remove all other improper self-citations. If the authors fail to address the critical issue of self-citation, the editing process of this manuscript will be terminated; and (6) References recommendations: The authors have the right to refuse to cite improper references recommended by the peer reviewer(s), especially references published by the peer reviewer(s) him/herself (themselves). If the authors find the peer reviewer(s) request for the authors to cite improper references published by him/herself (themselves), please send the peer reviewer's ID number to editorialoffice@wjgnet.com. The Editorial Office will close and remove the peer reviewer from the F6Publishing system immediately.</p> <p>2 Language evaluation: Classification: Grade B. The authors are native English speakers.</p> <p>3 Academic norms and rules: The</p>
--	--	--	--	---

					<p>authors provided the Biostatistics Review Certificate, the Institutional Review Board Approval Form. No academic misconduct was found in the Bing search.</p> <p>4 Supplementary comments: This is an invited manuscript.</p> <p>The study was supported by Canadian Institutes of Health Research by Pascale Mauriège and Centre de Recherche de l’Institut Universitaire de Cardiologie et de Pneumologie de Québec. The topic has not previously been published in the <i>WJD</i>.</p> <p>5 Issues raised: (1) The title is too long, and it should be no more than 18 words; (2) The authors did not provide the approved grant application form(s). Please upload the approved grant application form(s) or funding agency copy of any approval document(s); (3) The authors did not provide original pictures. Please provide the original figure documents. Please prepare and arrange the figures using PowerPoint to ensure that all graphs or arrows or text portions can be reprocessed by the editor; (4) PMID and DOI numbers are missing in the reference list. Please provide the PubMed numbers and DOI citation numbers to the reference list and list all</p>
--	--	--	--	--	--

						<p>authors of the references. Please revise throughout; (5) The “Article Highlights” section is missing. Please add the “Article Highlights” section at the end of the main text.</p> <p>6 Recommendation: Conditional acceptance.</p>
4	61252	05393105, 05282786	Grade C (Good), Grade A (Excellent)	Grade B (Minor language polishing), Grade A (Priority publishing)	<p>(1)Reviewer 05393105: A pertinent study in COVID times. However the number is far too small and observational for any generalisation. Please augment the discussion with more data of SPECT in liver tumours and why would this combination TARE fare better</p> <p>(2)Reviewer 05282786: The manuscript analyze 14 patients in full pandemic time and is to appreciate the idea of same day procedures and in the same time maintaining the results of therapy. The manuscript is original and well organized.</p>	<p>1 Scientific quality: The manuscript describes a observational study of an opportunity to improve care for patients with liver cancer during the COVID-19 pandemic. The topic is within the scope of the <i>WJGO</i>. (1) Classification: Grade A and Grade C; (2) Summary of the Peer-Review Report: The authors should augment the discussion with more data of SPECT in liver tumor and why would this combination TARE fare better. The manuscript is original and well organized. The questions raised by the reviewers should be answered.; (3) Format: There are 1 table and 5 figures; (4) References: A total of 35 references are cited, including 16 references published in the last 3 years; (5) Self-cited references: There are 4 self-cited references. The self-referencing rates should be less than 10%. Please keep the</p>

					<p>reasonable self-citations (<i>i.e.</i> those that are most closely related to the topic of the manuscript) and remove all other improper self-citations. If the authors fail to address the critical issue of self-citation, the editing process of this manuscript will be terminated; and (6) References recommendations: The authors have the right to refuse to cite improper references recommended by the peer reviewer(s), especially references published by the peer reviewer(s) him/herself (themselves). If the authors find the peer reviewer(s) request for the authors to cite improper references published by him/herself (themselves), please send the peer reviewer's ID number to editorialoffice@wjgnet.com. The Editorial Office will close and remove the peer reviewer from the F6Publishing system immediately.</p> <p>2 Language evaluation: Classification: Grade A and Grade B. The authors are native English speakers.</p> <p>3 Academic norms and rules: The authors provided the Biostatistics Review Certificate, the Institutional Review Board Approval Form. Written informed consent was waived. No academic misconduct was found in the Bing search.</p>
--	--	--	--	--	--

						<p>4 Supplementary comments: This is an invited manuscript. No financial support was obtained for the study. The topic has not previously been published in the <i>WJGO</i>.</p> <p>5 Issues raised: (1) The title is too long, and it should be no more than 18 words; (2) The authors need to fill out the STROBE checklist with page numbers; (3) The “Author Contributions” section is missing. Please provide the author contributions; (4) The authors did not provide original pictures. Please provide the original figure documents. Please prepare and arrange the figures using PowerPoint to ensure that all graphs or arrows or text portions can be reprocessed by the editor; (5) PMID and DOI numbers are missing in the reference list. Please provide the PubMed numbers and DOI citation numbers to the reference list and list all authors of the references. Please revise throughout; (6) The “Article Highlights” section is missing. Please add the “Article Highlights” section at the end of the main text.</p> <p>6 Recommendation: Conditional acceptance.</p>
5	61608	02536288	Grade B (Very good)	Grade B (Minor language polishing)	<p>(1)Reviewer 02536288: Hello</p> <p>Thank you for excellent manuscript.</p> <p>I have only two comments: please include colonoscopy foto as figure 3 (if you made it) and make histological conclusion more detail (not only information about lymphatic follicular hyperplasia and eosinophilic infiltration). Also please write that you checked this patient for worms and protozoa (because they also can cause eosinophylia)</p>	<p>1 Scientific quality: The manuscript describes a case report of the eosinophilic gastroenteritis with abdominal pain and ascites. The topic is within the scope of the <i>WJCC</i>. (1) Classification: Grade B; (2) Summary of the Peer-Review Report: This is an excellent manuscript. The authors should make histological conclusion more detail. The questions raised by the reviewers should be answered; (3) Format: There are 2 figures; (4) References: A total of 9 references are</p>

					<p>cited; (5) Self-cited references: There is self-cited no references.; and (6) References recommendations: The authors have the right to refuse to cite improper references recommended by the peer reviewer(s), especially references published by the peer reviewer(s) him/herself (themselves). If the authors find the peer reviewer(s) request for the authors to cite improper references published by him/herself (themselves), please send the peer reviewer's ID number to editorialoffice@wjgnet.com. The Editorial Office will close and remove the peer reviewer from the F6Publishing system immediately.</p> <p>2 Language evaluation: Classification: Grade B. A language editing certificate issued by Elsevier was provided.</p> <p>3 Academic norms and rules: No academic misconduct was found in the Bing search.</p> <p>4 Supplementary comments: No financial support was obtained for the study. The topic has not previously been published in the <i>WJCC</i>.</p> <p>5 Issues raised: (1) The authors did not provide original pictures. Please provide the original figure documents. Please prepare and arrange the figures using PowerPoint to ensure that all graphs or arrows or text portions can be reprocessed by the editor; (2) The authors need to fill out the CARE Checklist (2016)with page numbers; and (3) Please</p>
--	--	--	--	--	---

						<p>upload the primary version (PDF) of the Informed Consent Form (Surgical procedures or other) that has been signed by the patients in the study, prepared in the official language of the authors' country to the system; for example, authors from China should upload the Chinese version of the document, authors from Italy should upload the Italian version of the document, authors from Germany should upload the Deutsch version of the document, and authors from the United States and the United Kingdom should upload the English version of the document, <i>etc.</i></p> <p>Example: Download our sample of signed informed consent-Case report, at https://www.wjgnet.com/bpg/GerInfo/287.</p> <p>6 Recommendation: Conditional acceptance.</p>
6	61640	03401827	Grade E (Do not publish)	Grade B (Minor language polishing)	(1)Reviewer 03401827: The authors present the clinical course of single patient with CoVID-19 treated with NIV and eventually mechanical ventilation along with prone positioning. There is plenty of literature evidence describing such features in larger cohorts. Therefore, this study adds little to the current knowledge in this field. Furthermore, multiple concepts in the introduction and discussion are outdated (e.g niv might be effective even for prolonged periods; vaccines are now available).	<p>1 Scientific quality: This manuscript is a case report, and it does not reach the publication standard of the <i>WJCC</i>. (1) Classification: Grade E; (2) Summary of the Peer-Review Report: Reviewer 03401827 pointed out that There is plenty of literature evidence describing such features in larger cohorts. This study adds little to the current knowledge in this field. Furthermore, multiple concepts</p>

						<p>in the introduction and discussion are outdated.</p> <p>2 Language quality: Classification: Grade B.</p> <p>3 Recommendation: Rejection.</p>
--	--	--	--	--	--	---

7	61797	03820695	Grade E (Do not publish)	Grade B (Minor language polishing)	<p>(1)Reviewer 03820695: Thank you for giving me an opportunity to review a manuscript entitled “Clinicopathological and prognostic significance of lymphocyte to monocyte ratio in patients with ovarian cancer”. This meta-analysis demonstrated that decreased pretreatment lymphocyte to monocyte ratio (LMR) was associated with poor prognosis and unfavorable clinicopathological parameters in patients with ovarian cancer (OC); however, unfortunately, this result seemed to be lacking in novelty and interest.</p> <p>Major comments</p> <p>1. Considering the results of 11 studies included in in this meta-analysis, LMR has already been shown to be a poor prognostic indicator even in patients with OC; therefore, the result of this meta-analysis seemed to be lacking in novelty and interest. Was there anything special about OC compared to other malignancies?</p> <p>2. The authors demonstrated that low LMR was positively correlated with malignant ascites, lymph node metastasis, higher CA125 level, bigger residual tumor mass, and platinum response. Therefore, one question rises whether LMR is really a prognostic indicator independent of these associated clinicopathological parameters. Another question is whether LMR is more useful in predicting prognosis of OC than other systemic inflammatory indicators such as C-reactive protein (CRP), Glasgow Prognostic Score (GPS), platelet count, neutrophil-lymphocyte ratio (NLR), and platelet-lymphocyte ratio (PLR). Please solve these problems.</p> <p>Minor comments</p> <p>1. There were some mistakes in the text.</p>	<p>1 Scientific quality: This manuscript is a evidence-based medicine, and it does not reach the publication standard of the WJCC. (1) Classification: Grade E; (2) Summary of the Peer-Review Report: Reviewer 03820695 pointed out that LMR has already been shown to be a poor prognostic indicator even in patients with OC; therefore, the result of this meta-analysis seemed to be lacking in novelty and interest.</p> <p>2 Language quality: Classification: Grade B.</p> <p>3 Recommendation: Rejection.</p>
---	-------	----------	--------------------------------	---	--	---

8	61939	05187266, 04623475	Grade D (Fair), Grade D (Fair)	Grade B (Minor language polishing), Grade B (Minor language polishing)	<p>(1)Reviewer 05187266: The authors present a review on the epidemiology of COVID-19 in Libya. Although this work might be worth publishing, there are some major concerns. First, the work is in need of an additional review by a native english speaker. Second, there are a number of repetitions in the text such as mentioning the first confirmed case in Libya. In addition, I believe that at this point in the pandemic there is need for a more depth discussion about the vaccine preparation and distribution and it's effects in countries like Libya. I believe that authors must tackle these issues before proceeding.</p> <p>(2)Reviewer 04623475: This is an original article regarding the epidemiology of COVID-19 in Libya</p> <p>The article is well written without any linguistic problems.</p> <p>It sounds like a narrative review on coronavirus disease.</p> <p>The study methodology is not clear. The authors tell the evolution of the pandemic in Libya. However, the topic is largely out of date and inadequate for a high IF journal such as the world journal of gastroenterology</p> <p>I suggest sending it to a national journal</p>	<p>1 Scientific quality: The manuscript describes a review of the expression of epidemiology and factors that influence COVID-19 pandemic in Libya. The topic is within the scope of the WJV. (1) Classification: Grade D and Grade D; (2) Summary of the Peer-Review Report: The article is well written without any linguistic problems. There is a need of depth discussion about the vaccine preparation and distribution. The questions raised by the reviewers should be answered; (3) Format: There are 6 figures; (4) References: A total of 27references are cited, including 27 references published in the last 3 years; (5) Self-cited references: There are 2 self-cited references. The self-referencing rates should be less than 10%. Please keep the reasonable self-citations (<i>i.e.</i> those that are most closely related to the topic of the manuscript) and remove all other improper self-citations. If the authors fail to address the critical issue of self-citation, the editing process of this manuscript will be terminated; and (6) References recommendations: The authors have the right to refuse to cite improper references recommended by the peer reviewer(s), especially references published by the peer reviewer(s) him/herself (themselves). If the authors find the peer reviewer(s) request for the authors to cite improper references published by him/herself (themselves), please send the peer reviewer's ID number to editorialoffice@wjgnet.com. The Editorial Office will close and remove the peer reviewer from the F6Publishing system immediately.</p> <p>2 Language evaluation: Classification: Grade B and Grade B. A language editing certificate issued by PaperTrue was</p>
---	-------	-----------------------	---	---	---	---

						<p>provided.</p> <p>3 Academic norms and rules: No academic misconduct was found in the Bing search.</p> <p>4 Supplementary comments: This is an invited manuscript. No financial support was obtained for the study. The topic has not previously been published in the <i>WJV</i>.</p> <p>5 Issues raised: (1) The “Author Contributions” section is missing. Please provide the author contributions; (2) The authors did not provide original pictures. Please provide the original figure documents. Please prepare and arrange the figures using PowerPoint to ensure that all graphs or arrows or text portions can be reprocessed by the editor; (3) The authors need to fill out the CARE Checklist (2016) with page numbers; and (4) Please upload the primary version (PDF) of the Informed Consent Form (Surgical procedures or other) that has been signed by the patients in the study, prepared in the official language of the authors’ country to the system; for example, authors from China should upload the Chinese version of the document, authors from Italy should upload the Italian version of the</p>
--	--	--	--	--	--	--

						<p>document, authors from Germany should upload the Deutsch version of the document, and authors from the United States and the United Kingdom should upload the English version of the document, <i>etc.</i></p> <p>Example: Download our sample of signed informed consent-Case report, at https://www.wjgnet.com/bpg/GerInfo/287.</p> <p>6 Recommendation: Conditional acceptance.</p>
9	61948	04068828, 04025443	Grade D (Fair), Grade C (Good)	Grade C (A great deal of language polishing), Grade B (Minor language polishing)	<p>(1)Reviewer 04068828: The authors investigated the esophageal microbiome in esophageal squamous cell carcinoma patients using 16SrDNA Amplicon Pyrosequencing. They found some differences between the ESCC and the HV group, in which relatively large differences were found in relative abundances of Streptococcus, Veillonella, Neisseria at the genus level. The number of samples are reasonably well. However, the description of study subjects background and the process of sampling are described vaguely.</p> <p>Major comments:</p> <p>1. Please clarify which part of the esophageal samples were taken from? Was it from upper or lower esophagus? Was it from cancer or non-cancer lesion in the ESCC group?</p> <p>2. What was the pathologic stage of ESCC in study patients?</p> <p>3. It is assumed that most of the ESCC group was drinker and/or smoker. How much alcohol and tobacco did subjects in each group consume? In other words, the different microbiome between the two groups might come from the different background.</p> <p>4. Page 4, line 13-14. Although the authors said gender and age did not differ between the two groups, supplementary table 1 says P=0.034. Please clarify what the p value is for?</p>	<p>1 Scientific quality: The manuscript describes a retrospective study of t esophageal flora of esophageal squamous cell carcinoma. The topic is within the scope of the <i>WJG</i>. (1) Classification: Grade C and Grade D; (2) Summary of the Peer-Review Report: The number of samples are reasonably well and the described data may bring new to the field. However, the description of study subjects background and the process of sampling are described vaguely. The questions raised by the reviewers should be answered; (3) Format: There are 7 tables and 6 figures; (4) References: A total of 42 references are cited, including 5 references published in the last 3 years; (5) Self-cited references: There is 1s elf-cited references; and (6) References recommendations: The authors have the right to refuse to cite improper references recommended by the peer reviewer(s), especially references published by the peer reviewer(s) him/herself (themselves). If the authors find the peer reviewer(s) request for the authors to cite improper references published by him/herself (themselves), please send the</p>

				<p>5. Page 10, line 19. Table 4 could not be found in the manuscript.</p> <p>Minor comments:</p> <p>1. Overall, the manuscript contains many typos. Also, it needs grammatical correction.</p> <p>(2)Reviewer 04025443: I read with interest the manuscript by X.B. Liu et al, based on the original study of oesophageal flora in patients with oesophageal squamous cell carcinoma and in mostly healthy subjects of the control group. The research idea seems to be relevant. The conclusions are based on the described results. In general, the described data may bring new to the field. However, there are some flaws that need to be addressed to make the manuscript better.</p> <p>First of all, it seems that the groups differed by more than one factor (the disease presence). It seems that subjects enrolled to the main group were older (though, the statistics on this matter is given poorly); please consider to discuss this in appropriate section of the manuscript.</p> <p>A number of factors, that are not mentioned, could affect study results. Previous studies described dramatic difference in flora composition in rural or urban population, representatives of different ethnic groups, even if they lived on the same area. Flora composition may depend on dietary patterns, however no information is provided on (at least) major dietary factors. Please, consider to add more details about enrolled subjects, like ethnic distribution, body mass index, etc. Please, explain the absence of the prebiotic use in inclusion criteria, point 3. May this affect study results?</p> <p>Oesophageal flora composition may potentially depend on the state of the dental health and salivation. Moreover, the use of chewing gum (especially containing carbamide) could affect the results. Please, add more details if possible, or describe study limitations.</p> <p>There are some factors that may influence oesophageal flora and trigger the disease (for example, smoking). Please, consider to add this information to the the population demography and make additional calculation of the flora distribution</p>	<p>peer reviewer's ID number to editorialoffice@wjgnet.com. The Editorial Office will close and remove the peer reviewer from the F6Publishing system immediately.</p> <p>2 Language evaluation: Classification: Grade B and Grade C. A language editing certificate issued by Essaystar Group was provided.</p> <p>3 Academic norms and rules: The authors provided the Biostatistics Review Certificate, the Institutional Review Board Approval Form and Written informed consent. No academic misconduct was found in the Bing search. (</p> <p>4 Supplementary comments: The study was supported by Scientific research project of Hubei Provincial Department of Education, the 2016 Joint Diagnostic Medicine Research Project of Taihe Hospital and the Foundation of Taihe Hospital. The topic has not previously been published in the WJG.</p> <p>5 Issues raised: (1) The title is too long, and it should be no more than 18 words; (2) The authors did not provide the approved grant application form(s). Please upload the approved grant application form(s) or funding agency copy of any approval document(s); (3) The authors did not provide original</p>
--	--	--	--	--	--

					<p>depending on this factor.</p> <p>There is no information on the stage of the disease. Advanced stage of ESCC and ulceration could lead to non-specific changes in oesophageal flora distribution.</p> <p>Please, give more details in the description of the study population.</p> <p>Were mucosa samples taken from the similar sites in all subjects? Please, add this information to the Methods.</p> <p>Design of the study did not allow to measure changes with time. Therefore the passage on the page 12 (with the use of "increase" or "decrease") requires correction.</p> <p>There is lack of information on the study conduction and the data processing.</p> <p>Please, provide a description about study registration, number of subject enrolled and excluded (with the reasons) to conform Strobe guidelines.</p>	<p>pictures. Please provide the original figure documents. Please prepare and arrange the figures using PowerPoint to ensure that all graphs or arrows or text portions can be reprocessed by the editor;</p> <p>(4) PMID and DOI numbers are missing in the reference list. Please provide the PubMed numbers and DOI citation numbers to the reference list and list all authors of the references. Please revise throughout; and (5) The “Article Highlights” section is missing. Please add the “Article Highlights” section at the end of the main text;</p> <p>6 Recommendation: Conditional acceptance.</p>
10	62254	03890335, 03442364	Grade D (Fair), Grade E (Do not publish)	Grade B (Minor language polishing), Grade D (Rejection)	<p>(1)Reviewer 03890335: The originality of the Figures in the manuscript must be clarified.</p> <p>(2)Reviewer 03442364: This study is not novel enough to be accepted.</p>	<p>1 Scientific quality: This manuscript is a Review, and it does not reach the publication standard of the <i>WJO</i>. (1) Classification: Grade D and E; (2) Summary of the Peer-Review Report: Reviewer 03442364 pointed out that: This study is not novel enough to be accepted.</p> <p>2 Language quality: Classification: Grade B and D.</p> <p>3 Recommendation: Rejection.</p>
11	62297	01293596	Grade C (Good)	Grade B (Minor language	(1)Reviewer 01293596: The authors showed myocardial infarction in a case with twin pregnancy after assisted reproduction. The case was properly diagnosed and treated without any serious complications.	1 Scientific quality: The manuscript describes a case report of the Acute myocardial infarction in twin pregnancy

				<p>polishing)</p> <p>General comment;</p> <p>The strong point of this study was to have an alert for a risk of acute coronary event in pregnant women who had twin pregnancy with assisted reproduction. However, the lack of coronary imaging or follow up coronary angiography made the etiology of coronary events ambiguous in such patients.</p> <p>Specific comments;</p> <p>The discussion is too lengthy, and should be focused more on the strength and limitation of the present report. It is important to accentuate the risk of acute coronary event in assisted reproduction.</p> <p>Figure 1; the leads should be clarified. Noisy parts of ECG should be omitted.</p> <p>Figure 2; CAG was shown in only one direction. At least two directions should be shown with clear information. Furthermore, images of RCA in figure 2 look like TIMI 1 or 2 flow grade.</p> <p>Table 1; in the last 2 cases, the risk factors for coronary events were not determined.</p> <p>Discussion: the authors should discuss more with regard to the safety and risk of medications for the mother and fetus.</p>	<p>after assisted reproduction. The topic is within the scope of the <i>WJCC</i>. (1) Classification: Grade C; (2) Summary of the Peer-Review Report: The lack of coronary imaging or follow up coronary angiography made the etiology of coronary events ambiguous in pregnant women who had twin pregnancy with assisted reproduction. The questions raised by the reviewers should be answered; (3) Format: There are 3 figures and 1 table; (4) References: A total of 15 references are cited, including 3 references published in the last 3 years; (5) Self-cited references: There is no self-cited reference; and (6) References recommendations: The authors have the right to refuse to cite improper references recommended by the peer reviewer(s), especially references published by the peer reviewer(s) him/herself (themselves). If the authors find the peer reviewer(s) request for the authors to cite improper references published by him/herself (themselves), please send the peer reviewer's ID number to editorialoffice@wjgnet.com. The Editorial Office will close and remove the peer reviewer from the F6Publishing system</p>
--	--	--	--	--	---

						<p>immediately.</p> <p>2 Language evaluation: Classification: Grade B. A language editing certificate issued by AJE was provided.</p> <p>3 Academic norms and rules: (1) The “Author Contributions” section is missing. Please provide the author contributions; (2) The authors did not provide the approved grant application form(s). Please upload the approved grant application form(s) or funding agency copy of any approval document(s);</p> <p>(3) The authors did not provide original pictures. Please provide the original figure documents. Please prepare and arrange the figures using PowerPoint to ensure that all graphs or arrows or text portions can be reprocessed by the editor; and (4) PMID and DOI numbers are missing in the reference list. Please provide the PubMed numbers and DOI citation numbers to the reference list and list all authors of the references. Please revise throughout; (5) The “Case Presentation” section was not written according to the Guidelines for Manuscript Preparation. Please re-write the “Case Presentation” section, and add the “FINAL DIAGNOSIS”,</p>
--	--	--	--	--	--	---

						<p>“TREATMENT”, and “OUTCOME AND FOLLOW-UP” sections to the main text, according to the Guidelines and Requirements for Manuscript Revision.</p> <p>6 Recommendation: Conditional acceptance.</p>
12	62402	02694731	Grade B (Very good)	Grade B (Minor language polishing)	<p>(1)Reviewer 02694731: Dear Authors, you did accomplish a good paper on acetabular cups made out of porous metal. Unfortunately, you missed some early publication about cementless porous metal implants. Therefore, the discussion section urges revision in particular you should include and cite some established papers on porosity, pore size and fixation of acetabular components.</p> <p>The other parts of your manuscript do require minor revisions only.</p> <p>You will find my remarks in the revised manuscript where changes are tracked.</p>	<p>1 Scientific quality: The manuscript describes a review of the Off-the-shelf 3D printed titanium cups in primary total hip arthroplasty. The topic is within the scope of the <i>WJO</i>. (1) Classification: Grade B; (2) Summary of the Peer-Review Report: The authors accomplish a good paper on acetabular cups made out of porous metal. The discussion section need a revision. The questions raised by the reviewers should be answered; (3) Format: There are 1 table and 4 figures; (4) References: A total of 33 references are cited, including 18 references published in the last 3 years; (5) Self-cited references: There are 4 self-cited references. The self-referencing rates should be less than 10%. Please keep the reasonable self-citations (<i>i.e.</i> those that are most closely related to the topic of the manuscript) and remove all other improper self-citations. If the authors fail to address the critical issue of</p>

						<p>self-citation, the editing process of this manuscript will be terminated; and (6) References recommendations: The authors have the right to refuse to cite improper references recommended by the peer reviewer(s), especially references published by the peer reviewer(s) him/herself (themselves). If the authors find the peer reviewer(s) request for the authors to cite improper references published by him/herself (themselves), please send the peer reviewer's ID number to editorialoffice@wjgnet.com. The Editorial Office will close and remove the peer reviewer from the F6Publishing system immediately.</p> <p>2 Language evaluation: Classification: Grade B. The manuscript is reviewed by a a native English speaker.</p> <p>3 Academic norms and rules: No academic misconduct was found in the Bing search.</p> <p>4 Supplementary comments: This is an invited manuscript. No financial support was obtained for the study. The topic has not previously been published in the <i>WJO</i>.</p> <p>5 Issues raised: (1) The language classification is Grade B. Please visit the following website for the professional</p>
--	--	--	--	--	--	---

English language editing companies we recommend:

<https://www.wjgnet.com/bpg/gerinfo/240>; (2) The authors did not provide original pictures. Please provide the original figure documents. Please prepare and arrange the figures using PowerPoint to ensure that all graphs or arrows or text portions can be reprocessed by the editor; and (3) Please obtain permission for the use of picture(s). If an author of a submission is re-using a figure or figures published elsewhere, or that is copyrighted, the author must provide documentation that the previous publisher or copyright holder has given permission for the figure to be re-published; and correctly indicating the reference source and copyrights. For example, “**Figure 1 Histopathological examination by hematoxylin-eosin staining (200 ×).** A: Control group; B: Model group; C: Pioglitazone hydrochloride group; D: Chinese herbal medicine group. **Citation:** Yang JM, Sun Y, Wang M, Zhang XL, Zhang SJ, Gao YS, Chen L, Wu MY, Zhou L, Zhou YM, Wang Y, Zheng FJ, Li YH. Regulatory effect of a Chinese herbal medicine

						<p>formula on non-alcoholic fatty liver disease. <i>World J Gastroenterol</i> 2019; 25(34): 5105-5119. Copyright ©The Author(s) 2019. Published by Baishideng Publishing Group Inc^[6]". And please cite the reference source in the references list. If the author fails to properly cite the published or copyrighted picture(s) or table(s) as described above, he/she will be subject to withdrawal of the article from BPG publications and may even be held liable.</p> <p>6 Recommendation: Conditional acceptance.</p>
13	62479	03648016	Grade C (Good)	Grade B (Minor language polishing)	(1)Reviewer 03648016: The article achieved its objective. The experimental design was strictly adhered to and the results were relevant. The findings are of interest to the area.	<p>1 Scientific quality: The manuscript describes a meta-analysis of the effectiveness of SGLT-2 Inhibitors, GLP-1 Receptor Agonists and DPP-4 Inhibitors in patients with non-alcoholic fatty liver disease. The topic is within the scope of the <i>WJG</i>. (1) Classification: Grade C; (2) Summary of the Peer-Review Report: The article achieved its objective. The experimental design was strictly adhered to and the results were relevant. The findings are of interest to the area. The questions raised by the reviewers should be answered; (3) Format: There are 3 tables and 13 figures; (4) References: A</p>

					<p>total of 46 references are cited, including 13 references published in the last 3 years; (5) Self-cited references: There is no self-cited reference.; and (6) References recommendations: The authors have the right to refuse to cite improper references recommended by the peer reviewer(s), especially references published by the peer reviewer(s) him/herself (themselves). If the authors find the peer reviewer(s) request for the authors to cite improper references published by him/herself (themselves), please send the peer reviewer's ID number to editorialoffice@wjgnet.com. The Editorial Office will close and remove the peer reviewer from the F6Publishing system immediately.</p> <p>2 Language evaluation: Classification: Grade B.</p> <p>3 Academic norms and rules: The authors provided the Biostatistics Review Certificate. No academic misconduct was found in the Bing search.</p> <p>4 Supplementary comments: This is an invited manuscript.</p> <p>The study was supported by National Programs for Science and Technology Development of China and National Natural Science Foundation of China</p>
--	--	--	--	--	---

						<p>(General Program). The topic has not previously been published in the <i>WJG</i>.</p> <p>5 Issues raised: (1) The language classification is Grade B. Please visit the following website for the professional English language editing companies we recommend:</p> <p>https://www.wjgnet.com/bpg/gerinfo/240; (2) The title is too long, and it should be no more than 18 words; (3) The authors did not provide the approved grant application form(s). Please upload the approved grant application form(s) or funding agency copy of any approval document(s); (4) The authors did not provide original pictures. Please provide the original figure documents. Please prepare and arrange the figures using PowerPoint to ensure that all graphs or arrows or text portions can be reprocessed by the editor; (5) PMID and DOI numbers are missing in the reference list. Please provide the PubMed numbers and DOI citation numbers to the reference list and list all authors of the references. Please revise throughout; (6) The authors need to fill out the PRISMA 2009 Checklist with page numbers; and (7) If an author of a submission is</p>
--	--	--	--	--	--	---

						<p>re-using a figure or figures published elsewhere, or that is copyrighted, the author must provide documentation that the previous publisher or copyright holder has given permission for the figure to be re-published; and correctly indicating the reference source and copyrights. For example, “Figure 1 Histopathological examination by hematoxylin-eosin staining (200 ×). A: Control group; B: Model group; C: Pioglitazone hydrochloride group; D: Chinese herbal medicine group. Citation: Yang JM, Sun Y, Wang M, Zhang XL, Zhang SJ, Gao YS, Chen L, Wu MY, Zhou L, Zhou YM, Wang Y, Zheng FJ, Li YH. Regulatory effect of a Chinese herbal medicine formula on non-alcoholic fatty liver disease. <i>World J Gastroenterol</i> 2019; 25(34): 5105-5119. Copyright ©The Author(s) 2019. Published by Baishideng Publishing Group Inc^[6]”. And please cite the reference source in the references list. If the author fails to properly cite the published or copyrighted picture(s) or table(s) as described above, he/she will be subject to withdrawal of the article from BPG publications and may even be</p>
--	--	--	--	--	--	---

						held liable. 6 Recommendation: Conditional acceptance.
14	62494	03471292	Grade D (Fair)	Grade C (A great deal of language polishing)	<p>(1)Reviewer 03471292: General Comments</p> <ol style="list-style-type: none"> 1. The manuscript needs substantial English language editing. 2. Authors need to incorporate proper scientific terminology 3. Though the case may be unique in presentation, surgical technique mentioned follows principles already accepted. Authors may discuss what value does this case add to literature. <p>Specific Comments</p> <ol style="list-style-type: none"> 1. Authors have mentioned “A few patients can indeed achieve reduction through traction”. Authors may quote reference and be more specific 2. “Previous studies reported that it can be treated by the front entrance treatment of decompression with release or odontoidectomy combined with posterior fixation and fusion, but the operation is difficult, coming with high risk coefficient and increased risk of infection.” Authors may quote the mentioned references 3. The left muscle strength was graded I. Authors need to mention muscle strength as per ISNCSCI. Hence the strength of lack muscle group needs to be mentioned. 4. He also provided important information that the neck beginning pain was caused after a fall from about 2.0 meters high in September 2014, and the pain was aggravated from January 2018.- Authors may give reasons for the delayed presentation and also discuss implications with references from literature 5. Title mentions atlantooccipital dislocation while manuscript mentions atlantoaxial dislocation- Authors may do needful. 6. Authors have mentioned “Then, the traction weight increased slowly, and the maximum traction weight reached 8 kg”. Authors need to clarify the time duration between every incremental increase of weight. 7. Authors have mentioned “One of the treatments for separation of the anterior and posterior atlantal arch with posterior atlantooccipital dislocation and type II 	<p>1 Scientific quality: The manuscript describes a case report of the Congenital separation of the anterior and posterior atlantal arch with posterior atlantooccipital dislocation and type II odontoid fracture. The topic is within the scope of the <i>WJSP</i>. (1) Classification: Grade D; (2) Summary of the Peer-Review Report: Though the case may be unique in presentation, surgical technique mentioned follows principles already accepted. Authors may discuss what value does this case add to literature. The questions raised by the reviewers should be answered; (3) Format: There are 11 figures; (4) References: A total of 9 references are cited, including 2 references published in the last 3 years; (5) Self-cited references: There are 1 self-cited references. The self-referencing rates should be less than 10%. Please keep the reasonable self-citations (<i>i.e.</i> those that are most closely related to the topic of the manuscript) and remove all other improper self-citations. If the authors fail</p>

				<p>odontoid fracture can be the direct posterior reduction, occipital plate and customized instrumentation, posterior arch resection of atlas using ultrasonic bone, and the procedure management technique---distraction, extension, compression, and reduction (DCER)". Can the authors draw a definitive conclusion based on 1 case.</p>	<p>to address the critical issue of self-citation, the editing process of this manuscript will be terminated; and (6) References recommendations: The authors have the right to refuse to cite improper references recommended by the peer reviewer(s), especially references published by the peer reviewer(s) him/herself (themselves). If the authors find the peer reviewer(s) request for the authors to cite improper references published by him/herself (themselves), please send the peer reviewer ' s ID number to editorialoffice@wjnet.com. The Editorial Office will close and remove the peer reviewer from the F6Publishing system immediately.</p> <p>2 Language evaluation: Classification: Grade C.</p> <p>3 Academic norms and rules: The authors provided the Written informed consent. No academic misconduct was found in the Bing search.</p> <p>4 Supplementary comments: No financial support was obtained for the study.</p> <p>The topic has not previously been published in the <i>WJSP</i>.</p> <p>5 Issues raised: (1) The language classification is Grade C. Please visit the</p>
--	--	--	--	---	---

						<p>following website for the professional English language editing companies we recommend:</p> <p>https://www.wjgnet.com/bpg/gerinfo/240; (2) The title is too long, and it should be no more than 18 words; (3) The “Author Contributions” section is missing. Please provide the author contributions; (4) The authors did not provide original pictures. Please provide the original figure documents. Please prepare and arrange the figures using PowerPoint to ensure that all graphs or arrows or text portions can be reprocessed by the editor; (5) PMID and DOI numbers are missing in the reference list. Please provide the PubMed numbers and DOI citation numbers to the reference list and list all authors of the references. Please revise throughout; (6) The “Case Presentation” section was not written according to the Guidelines for Manuscript Preparation. Please re-write the “Case Presentation” section, and add the “FINAL DIAGNOSIS”, “TREATMENT”, and “OUTCOME AND FOLLOW-UP” sections to the main text, according to the Guidelines and Requirements for Manuscript Revision;</p>
--	--	--	--	--	--	--

						<p>(7) The authors need to fill out the STROBE checklist CARE Checklist (2016) with page numbers; and (10) Please obtain permission for the use of picture(s). If an author of a submission is re-using a figure or figures published elsewhere, or that is copyrighted, the author must provide documentation that the previous publisher or copyright holder has given permission for the figure to be re-published; and correctly indicating the reference source and copyrights. For example, “Figure 1 Histopathological examination by hematoxylin-eosin staining (200 ×). A: Control group; B: Model group; C: Pioglitazone hydrochloride group; D: Chinese herbal medicine group. Citation: Yang JM, Sun Y, Wang M, Zhang XL, Zhang SJ, Gao YS, Chen L, Wu MY, Zhou L, Zhou YM, Wang Y, Zheng FJ, Li YH. Regulatory effect of a Chinese herbal medicine formula on non-alcoholic fatty liver disease. <i>World J Gastroenterol</i> 2019; 25(34): 5105-5119. Copyright ©The Author(s) 2019. Published by Baishideng Publishing Group Inc^[6]”. And please cite the reference source in the references list. If</p>
--	--	--	--	--	--	---

						<p>the author fails to properly cite the published or copyrighted picture(s) or table(s) as described above, he/she will be subject to withdrawal of the article from BPG publications and may even be held liable.</p> <p>6 Recommendation: Conditional acceptance.</p>
15	62515	05021504	Grade C (Good)	Grade B (Minor language polishing)	(1)Reviewer 05021504: The review has surveyed the post-life and fate of patients with gastric cancer. The language literature is good. But I think the writing style is not interesting. I think it would be improved with a few tables or plots.	<p>1 Scientific quality: The manuscript describes a review of the health-related quality of life after gastric cancer treatment in Brazil. The topic is within the scope of the <i>WJCO</i>. (1) Classification: Grade C; (2) Summary of the Peer-Review Report: The language literature is good. I think it would be improved with a few tables or plots. The questions raised by the reviewers should be answered; (3) Format: There is no figure and table; (4) References: A total of 60 references are cited, including 4 references published in the last 3 years; (5) Self-cited references: There are 5 self-cited references. The self-referencing rates should be less than 10%. Please keep the reasonable self-citations (<i>i.e.</i> those that are most closely related to the topic of the manuscript) and remove all other improper self-citations. If the</p>

					<p>authors fail to address the critical issue of self-citation, the editing process of this manuscript will be terminated; and (6) References recommendations: The authors have the right to refuse to cite improper references recommended by the peer reviewer(s), especially references published by the peer reviewer(s) him/herself (themselves). If the authors find the peer reviewer(s) request for the authors to cite improper references published by him/herself (themselves), please send the peer reviewer ' s ID number to editorialoffice@wjgnet.com. The Editorial Office will close and remove the peer reviewer from the F6Publishing system immediately.</p> <p>2 Language evaluation: Classification: Grade B. A language editing certificate issued by AJE was provided.</p> <p>3 Academic norms and rules: No academic misconduct was found in the Bing search.</p> <p>4 Supplementary comments: This is an invited manuscript. No financial support was obtained for the study. The topic has not previously been published in the <i>WJCO</i>.</p> <p>5 Issues raised: The running title is too long, and it should be no more than 6</p>
--	--	--	--	--	--

						words. 6 Recommendation: Conditional acceptance.
16	62516	05329434	Grade E (Do not publish)	Grade B (Minor language polishing)	<p>(1)Reviewer 05329434: This is a retrospective study aimed to compare the radiographic findings of cemented vs uncemented reverse shoulder prostheses (RTSA) for proximal humeral fractures.</p> <p>The topic is interesting, but there are several methodological limits that compromise the scientific quality of this study.</p> <p>It is well known that fixation of the humeral stem is a minor concern in shoulder arthroplasty, particularly in primary implants and in the short term. The authors report the radiographic outcomes of 64 RTSA at a mean follow up of 12 months (range, 3 to 37 months): frankly, this time interval is too short to allow a significant comparison between radiographic findings in cemented and uncemented stems, unless operative complications or mistakes occurred.</p> <p>The problem of stem stability in proximal humeral fractures is mainly influenced by the lack of proximal fixation. Therefore, stem fixation relies on distal fit and this is a critical aspect for uncemented stem, because precise positioning in terms of height and rotational stability is not easy, particularly in case of a thin diaphyseal cortex, as usually happens in osteoporotic elderly patients. The authors didn't provide any data about the quality of the bone stock in this series of patients (selection bias?) and didn't report about the potential proximal fixation achieved by some implants.</p> <p>Radiographic analysis is too inconsistent to provide reliable data about lucencies, subsidence, loosening, stress shielding, etc. A study that is focused on radiographic changes (since clinical results are completely omitted) should be based on reproducible radiograms, but this retrospective study doesn't fulfil this prerogative. Indeed, there is only one figure (Figure 2) showing an example of radiolucency and the absence of explanatory images doesn't allow the reader to understand how the authors compared the radiograms inside and between groups.</p> <p>There is no need to analyze point by point the paper, because the design of the</p>	<p>1 Scientific quality: This manuscript is a Retrospective Study, and it does not reach the publication standard of the <i>WJO</i>. (1) Classification: Grade E; (2) Summary of the Peer-Review Report: Reviewer 05329434 pointed out that the design of the study is too weak, with substantial methodological problems not supporting the reported results. The paper is not suitable for publication on <i>WJO</i>.</p> <p>2 Language quality: Classification: Grade B.</p> <p>3 Recommendation: Rejection.</p>

					study is too weak, with substantial methodological problems not supporting the reported results. I'm sorry to conclude that the paper is not suitable for publication on WJO.	
17	62526	00506472, 03547628	Grade B (Very good), Grade E (Do not publish)	Grade A (Priority publishing), Grade C (A great deal of language polishing)	(1)Reviewer 00506472: No comments. It a very well written original paper for SPARC as a diagnostic and prognostic biomarker for the diffuse large B-cell lymphoma. (2)Reviewer 03547628: 1. The levels of SPARC in DLBCL is well known. 2. The study lacks novelty	1 Scientific quality: This manuscript is a Retrospective Study, and it does not reach the publication standard of the WJCC. (1) Classification: Grade B and Grade E; (2) Summary of the Peer-Review Report: Reviewer 03547628 pointed The levels of SPARC in DLBCL is well known and the study lacks novelty. 2 Language quality: Classification: Grade A and Grade C. 3 Recommendation: Rejection.
18	62743	05225569	Grade B (Very good)	Grade A (Priority publishing)	(1)Reviewer 05225569: Well written article arguing for the use of multi-omics research on the mechanisms of T2DM and providing some research areas for the future.	
19	62760	05038583, 02543955	Grade B (Very good), Grade C (Good)	Grade A (Priority publishing), Grade B (Minor language polishing)	(1)Reviewer 05038583: General comments This is a nice review article about the impact of COVID-19 to HCC. This review is well written and discussing about a timely topic. I do have some comments specified as below. Specific comments At my institution, there were some patients refused to come to the hospital for testing during pandemic to reduce the exposure to themselves, thus delayed screening. Is there any study suggesting about patients' factor? Before pandemic, everyone came to the clinic and got blood test before or after the visit, and ultrasound for HCC screening right away. However, increasing number of telephone or telehealth video visit makes it harder to do these tests right away and some patients got some delay in getting blood draws. Is there any study stating this observation of mine?	

					<p>In the U.S., a lot of liver transplant centers closed earlier in 2020, but our center remained open during pandemic and we actually noted increase in volume of liver transplantation. Was there any study mentioned about these changes?</p> <p>(2)Reviewer 02543955: Very nice manuscript that summerizes the recommendations for treatment of HCC during covid-19. Only some minor comments:</p> <p>- page 10, line 5: recommendation instead of "raccomandation"</p> <p>- page 12, line 22/23: the sentence ".....RT to patients who do not they are suitable or refractory..." does not make sense</p> <p>- table 1, recommendation for liver resection from HC-FMUSP: I assume the number platelets should be 100.000 but not 4100.000</p> <p>- table 1, recommendation for vascular intervention from HC-FMUSP: Is it really "...patients with solitary nodules 43 cm in size...." ? I think 43 cm is not the cut-off.</p>	
20	62798	05085435	Grade B (Very good)	Grade B (Minor language polishing)	<p>(1)Reviewer 05085435: It is a case report, the authors retrospectively analyzed 8 patients definitively diagnosed with CHF by liver histopathology and reviewed the literature to provide some experience for the diagnosis and treatment of this disease.</p> <p>1 Title. Does the title reflect the main subject/hypothesis of the manuscript? yes</p> <p>2 Abstract. Does the abstract summarize and reflect the work described in the manuscript? yes</p> <p>3 Key words. Do the key words reflect the focus of the manuscript? yes</p> <p>4 Background. Does the manuscript adequately describe the background, present status and significance of the study?</p>	<p>1 Scientific quality: The manuscript describes a Case Report of clinical experience with congenital hepatic fibrosis. The topic is within the scope of the <i>WJCC</i>. (1) Classification: Grade B; (2) Summary of the Peer-Review Report: The case report provide some experience for the diagnosis and treatment of this disease. The questions raised by the reviewers should be answered; (3) Format: There are 3 tables and 3 figures; (4) References: A total of 24 references are cited, including 2 references published in the last 3 years; (5) Self-cited references: There is no self-cited reference; and (6)</p>

				<p>yes</p> <p>5 Methods. Does the manuscript describe methods (e.g., experiments, data analysis, surveys, and clinical trials, etc.) in adequate detail?</p> <p>yes</p> <p>6 Results. Are the research objectives achieved by the experiments used in this study? What are the contributions that the study has made for research progress in this field?</p> <p>It is a case report about congenital hepatic fibrosis</p> <p>7 Discussion. Does the manuscript interpret the findings adequately and appropriately, highlighting the key points concisely, clearly and logically? Are the findings and their applicability/relevance to the literature stated in a clear and definite manner? Is the discussion accurate and does it discuss the paper's scientific significance and/or relevance to clinical practice sufficiently?</p> <p>yes</p> <p>8 Illustrations and tables. Are the figures, diagrams and tables sufficient, good quality and appropriately illustrative of the paper contents? Do figures require labeling with arrows, asterisks etc., better legends?</p> <p>yes</p> <p>9 Biostatistics. Does the manuscript meet the requirements of biostatistics?</p> <p>It is a case report</p> <p>10 Units. Does the manuscript meet the requirements of use of SI units?</p> <p>yes</p> <p>11 References. Does the manuscript cite appropriately the latest, important and authoritative references in the introduction and discussion sections? Does the</p>	<p>References recommendations: The authors have the right to refuse to cite improper references recommended by the peer reviewer(s), especially references published by the peer reviewer(s) him/herself (themselves). If the authors find the peer reviewer(s) request for the authors to cite improper references published by him/herself (themselves), please send the peer reviewer's ID number to editorialoffice@wjgnet.com. The Editorial Office will close and remove the peer reviewer from the F6Publishing system immediately.</p> <p>2 Language evaluation: Classification: Grade B. A language editing certificate issued by BioScience was provided.</p> <p>3 Academic norms and rules: The authors provided the Written informed consent. No academic misconduct was found in the Bing search.</p> <p>4 Supplementary comments: The study was supported by the Science and Technology Development Program of Jilin Province and the Youth Development Foundation of the First Hospital of Jilin University. The topic has not previously been published in the WJCC.</p> <p>5 Issues raised: (1) The authors did not</p>
--	--	--	--	--	--

					<p>author self-cite, omit, incorrectly cite and/or over-cite references? References are appropriate.</p> <p>12 Quality of manuscript organization and presentation. Is the manuscript well, concisely and coherently organized and presented? Is the style, language and grammar accurate and appropriate? yes</p> <p>13 Research methods and reporting. Authors should have prepared their manuscripts according to manuscript type and the appropriate categories, as follows: (1) CARE Checklist (2013) - Case report; (2) CONSORT 2010 Statement - Clinical Trials study, Prospective study, Randomized Controlled trial, Randomized Clinical trial; (3) PRISMA 2009 Checklist - Evidence-Based Medicine, Systematic review, Meta-Analysis; (4) STROBE Statement - Case Control study, Observational study, Retrospective Cohort study; and (5) The ARRIVE Guidelines - Basic study. Did the author prepare the manuscript according to the appropriate research methods and reporting? yes</p> <p>14 Ethics statements. For all manuscripts involving human studies and/or animal experiments, author(s) must submit the related formal ethics documents that were reviewed and approved by their local ethical review committee. Did the manuscript meet the requirements of ethics? yes</p>	<p>provide the approved grant application form(s). Please upload the approved grant application form(s) or funding agency copy of any approval document(s); (2) The authors did not provide original pictures. Please provide the original figure documents. Please prepare and arrange the figures using PowerPoint to ensure that all graphs or arrows or text portions can be reprocessed by the editor; (3) Please re-organize the manuscript according to the Guidelines for Manuscript Preparation. The “Case Presentation” section was not written. Please re-write the “Case Presentation” section, and add the “FINAL DIAGNOSIS”, “TREATMENT”, and “OUTCOME AND FOLLOW-UP” sections to the main text, according to the Guidelines and Requirements for Manuscript Revision; and (4) The authors need to fill out the CARE Checklist (2016) with line/page numbers.</p> <p>6 Recommendation: Conditional acceptance.</p>
21	62816	03026171	Grade C (Good)	Grade B (Minor language	(1)Reviewer 03026171: Early and late post-ERCP complications should be discussed	

				polishing)		
22	62825	05665395	Grade C (Good)	Grade C (A great deal of language polishing)	(1)Reviewer 05665395: Good work, please add references 25-30:	
23	62843	03667297, 03656586, 03884279	Grade B (Very good), Grade D (Fair), Grade D (Fair)	Grade A (Priority publishing , Grade C (A great deal of language polishing), Grade A (Priority publishing)	(1)Reviewer 03667297: Well written manuscript. Well described data and figure. What is MC in the figure 2. Colour indicator of UC is missing in figure 3. (2)Reviewer 03656586: This article did a general descriptive study on the incidence of IBD patients in the author's area, and did not put forward its own view which based on the actual situation. (3)Reviewer 03884279: Major concerns are as follows: 1. "Introduction" section, "a stabilization of the incidence in most but also suggest an increase in northern European countries.2-4 On the other hand, there is a trend of increasing incidence in areas with traditionally lower incidence rates, such as the countries of eastern Europe" The incidence of IBD in western European and North American countries should be reported. 2. "Methods" seciton, the criteria of IBD should be reported. 3. "Results" section, the total patients and UC, CD, IC demographic data should be reported, including risk factors and co-morbidity. 4. "Results" section, a logistic regression analysis was preferred to analyze the risk factors of IBD.	
24	62855	05374991	Grade C (Good)	Grade B (Minor language polishing)	(1)Reviewer 05374991: General Impression: The authors reported two cases of luno-capitate joint osteoarthritis. This disease is rare and the reported cases in the literature are very few. The authors provided an adequate presentation of the cases. The title indicated that the authors performed a literature review, but this was not evident in the main text. Nonetheless, the authors discussed beautifully the relevant literature.The abstract is brief and summarizes the work clearly. The cases are well-presented and the patients were followed-up for an adequate time. The language is acceptable. The	

					<p>figures are informative.I would like to stress out some minor issues that should be considered prior to publication.</p> <p>Comments:</p> <p>1) The article does not follow the journal guidelines. The authors should reformat the article according to the guidelines.</p> <p>2) The introduction is very short. I advise the authors to try to give a better background about the disease by discussing the following points nire thoroughly: the disease prevalence, etiology, symptoms, commonly used treatment methods, and last but not least the authors should highlight what do these cases add to the existing knowledge about the disease.</p>	
25	62909	01808895, 03397272, 05493185	Grade B (Very good), Grade C (Good), Grade B (Very good)	Grade A (Priority publishing), Grade B (Minor language polishing), Grade B (Minor language polishing)	<p>(1)Reviewer 01808895: This is a review of the iRECIST response evaluation of cancer patients undergoing immunotherapy with a particular focus on atypical resonse pattern (pseudo-, hyperprogression, dissociated response). The review summarizes the most important findings in different tumor types plus gives a reasonable overview about the potential pathomechanisms of these atypical response types as well</p> <p>-a table summarizing the different findings (percentages) for different tumor-types would be helpful, since this information is very difficult to memorize from the text.</p> <p>-the images in figures 3 and 4 seem to have mixed up: please correctly assign the images to the text</p> <p>(2)Reviewer 03397272: Please see comments in the attached file.</p> <p>(3)Reviewer 05493185: Advantages:</p> <p>1. Immuno-RECIST (IRECIST) standards were introduced in 2017 to better analyze tumor responses to immunotherapy.Even iRECIST standards have been accepted by the world, due to the different mechanism of immunotherapy,</p>	

				<p>clinical experience has shown that the percentage of patients cannot be ignored, is difficult to determine the type of right relief (stability, progress, or complete remission), this review introduces three kinds of "atypical response": pseudoprogression, hyperprogression, dissociated response, summarizes each pattern, wants to make quantitative standard to help clinicians and radiologists to evaluate the effect by immunotherapy in order to help patients get the best treatment plan.</p> <p>2. Most of the references cited are recent two years and the data are relatively novel.</p> <p>3.The article format is clear, first of all, it introduces the different views on iRECIST in the assessment of immunotherapy , then, it respectively makes a detailed introduction for the three categories , finally,summarizing atypical response characteristics again.</p> <p>Insufficiency:</p> <p>Pseudoprogression, hyperprogression and dissociatedresponse sometimes are not easy to distinguish, which affects the judgment of clinical benefit of immunotherapy, and thus affects the choice of subsequent treatment plan.At the same time, it is difficult to capture the tumor growth dynamics before and after treatment in the early stage of disease in practical operation. In clinical practice, in the case of lack of pre-baseline, pseudoprogression will weaken the true positive rate, because it is impossible to get different parameters, it's also impossible to distinguish between pseudoprogression and hyperprogression.</p> <p>Suggestions:</p> <p>1. Pseudoprogression gives a few of reference cases, so it is suggested to refer to more cases. The incidence of Pseudoprogression only describes a few tumors, so the data is not enough.</p> <p>2. It is suggested to adjust the order of paragraphs in writing, because the logic is not smooth enough.</p> <p>3. The frequency of pseudoprogression is lower than that of routine progression,</p>	
--	--	--	--	--	--

					<p>and the decision to move beyond progression makes only in patients with obvious clinical benefit from immunotherapy. It is suggested to further explore the criteria for clinical benefit from immunotherapy in order to determine when the progression can be moved beyond progression.</p> <p>4. Since there is no definition of HPD, how to clarify its incidence and explore relevant predictors? This part of the content is not mature, it is suggested to write down the latest major consensus, more persuasive.</p> <p>5.① In pseudoprogression, the probability data of this phenomenon in various tumors were not available;② In pseudoprogression, there were less references to the two sections of iRECIST Guidelines.</p> <p>6.① There are few examples of Dissociated Response;② Since the standard of Dissociated Response in iRECIST Guidelines is not clear, we can find some guiding literatures for its subsequent research and introduce the possibility of its subsequent development.</p> <p>7. The classification of atypical responses in recent introduce and core tips are different.</p> <p>8. Reducing the frequency of using Consequently (the text appears 6 times), and consider replacing it by using other words;</p>	
26	62980	05077657, 04737441, 00189256, 02445886, 03755224	Grade B (Very good), Grade B (Very good), Grade A (Excellent), Grade B (Very good), Grade B	Grade B (Minor language polishing), Grade B (Minor language polishing), Grade A (Priority publishing) , Grade A	<p>(1)Reviewer 05077657: The topic is very interesting and described a serious problem. Sars-COV 2 infection is a challenging problem for the clinician and the knowledge of clinical manifestations and pathogenesis mechanism is very important in terms of prognosis.</p> <p>In Cai et al. manuscript the English is good and figure are also clear.</p> <p>(2)Reviewer 04737441: This review is very important as it is summarizing a very trendy topic which is liver affection by SARS-CoV2.</p> <p>However, It would be great to provide figure to summarize the mechanism of liver injury.</p> <p>(3)Reviewer 00189256: The review focuses on studying the important problem of hepatology – the liver injury in patients with coronavirus disease-2019 (COVID-19).</p>	

			(Very good)	(Priority publishing), Grade A (Priority publishing)	<p>In this review authors are present an analysis of the clinical features, potential mechanisms, and treatment strategies for liver injury associated with COVID-19. This review would benefit clinicians in devising better strategies for management of patients.</p> <p>The obtained results are discussed in-depth literature analysis on the investigated problem.</p> <p>The authors summarized the latest results in research on determining the clinical features, potential mechanisms, exacerbation of underlying hepatic dysfunction in patients with chronic liver diseases, and treatment strategies for patients with COVID-19.</p> <p>I recommend the article to publication.</p> <p>(4)Reviewer 02445886: The topic of the article is an analysis of the clinical features, potential mechanisms, and treatment strategies for liver injury associated with COVID-19. Although the lung is the main organ that is damaged in COVID-19, approximately 60% of the patients have been reported to develop various degrees of liver injury in several studies. Accumulating clinical data show that liver damage is related to the severity of COVID-19 and is a major cause of death from COVID-19, especially in the presence of hepatic failure. Thus, early detection, effective treatment, and elucidation of the mechanisms underlying the pathogenesis of liver damage are urgently needed for COVID-19 patients.</p> <p>The authors summarize the characteristics of COVID-19-associated liver injury from multiple perspectives, including clinical features (manifestation, laboratory examinations, liver biopsy, etc.), underlying pathogenesis (direct viral cytotoxicity, uncontrolled cytokine storm, drug-induced toxicity, etc.), special population of patients (those with cirrhosis, hepatitis B, liver transplantation, etc.), and clinical management (drugs, oxygen therapy, artificial liver blood purification, etc.).</p> <p>The manuscript is well written, the title reflects the main subject of the manuscript, the abstract and key words reflect the main topics of the entire text. The discussion is informative and helpful. The article cites 84 relevant and</p>	
--	--	--	-------------	--	--	--

					<p>important references and gives a complete picture of the topic.</p> <p>I suggest to accept the manuscript with no specific comments.</p> <p>(5)Reviewer 03755224: I would like to congratulate the authors of this paper for the excellent review of the mechanisms of liver injury in relation to COVID-19 infection. The coexistence of a cytokine storm, and the toxicity of the drugs used in the treatment, are adverse factors especially in patients with previous hepatopathies. The present study provides a comprehensive description of the pathophysiological mechanisms of these lesions. Of interest is the analysis of hyperpigmentation due to the involvement of the adrenal cortex when melanin production is stimulated, together with the increased conversion of tyrosine to melanin.</p> <p>One of the limitations is the absence of liver biopsies in early stages of the disease. It is also important to describe the factors involved in the disease, and finally to describe the therapeutic measures used, especially Glycyrrhizic acid, in addition to other supportive therapies.</p> <p>I reiterate my congratulations.</p>	
27	63042	05227913, 03068027	Grade C (Good), Grade E (Do not publish)	Grade B (Minor language polishing), Grade B (Minor language polishing)	<p>(1)Reviewer 05227913: Dear Authors,</p> <p>Congratulations on your successful intervention. Hip dislocation is an important orthopedic emergency usually seen in young patients who have experienced high-energy trauma, often resulting in significant long-term morbidity.</p> <p>1. You mentioned that your patient had multiple rib fractures. Have fixation surgery considered for these fractures?</p> <p>2. In patients with multiple rib fractures, flail chest can be seen and the patient needs intensive care unit support. Has this patient ever needed intensive care? How did you provide respiratory support?</p> <p>(2)Reviewer 03068027: Unfortunately no new concepts of treatment or finding in this case report. I would not recommend publication</p>	
28	63066	03738365, 03538156	Grade D (Fair),	Grade B (Minor	<p>(1)Reviewer 03738365: 1. Slovakia and Croatia are separated by Hungary, the developing level of social economy and geographic region is different between</p>	

			Grade D (Fair)	language polishing), Grade B (Minor language polishing)	<p>the two country. Is there difference of baseline clinical and demographic characteristics between Slovak and Croatian? The authors should provide e comparison in another table.</p> <p>2. According to exclusion criteria, patients who received immunosuppressive or obeticholic acid treatment or liver transplantation after less than 12 months of UDCA treatment were excluded from the study. Did these patients were offered the above treatment due to inefficiency of the first line treatment in the early stage? If so, the authors should include these patients in the study.</p> <p>3. ALT mainly exists in the cytoplasm, and AST mainly exists in the mitochondria. Generally, ALT enters the blood first when liver cells are damaged, AST enter the blood when the liver cells are severely damaged and endanger mitochondria. Thus, it is improper to use a composite variable AT (which was ALT when available; otherwise, AST was used) in original UK study. In the article, the authors using ALT in the place of the composite AT variable is a reasonable improvement. It suggests establishing URS using ALT and AST respectively and estimate discrimination ability of the model.</p> <p>4. For patients with probability of nonresponse, how about the response of second line treatment? These results will show the clinical value of the model.</p> <p>5. Using calibration belts to reflect the agreement between predicted probabilities from the URS model with actual outcomes is insufficient. It suggests using Decision Curve Analysis to demonstrate the net benefit of the model.</p> <p>6. Did any prediction model propose or use previously? The authors should compare the efficiency of URS model with others.</p> <p>(2)Reviewer 03538156: PBC incidence and prevalence are increasing with time. UDCA remains the first-line treatment option for PBC, and OCA has already been conditionally approved in combination with UDCA for patients showing an inadequate response to UDCA. It is important to identify patients suboptimal to UDCA as early as possible, so that we can offer OCA or other new promising drugs still in the early stage of the disease. In the current study, the authors independently evaluated the predictive performance of the URS model which was designed to estimate the baseline probability of treatment response after 12</p>	
--	--	--	-------------------	--	--	--

					<p>months of UDCA therapy. This study is of clinical significance. Some suggestions are as follows:</p> <ol style="list-style-type: none"> 1. Page 8: Does exclusion of liver transplant patients within 12 months of UDCA affect the results? Can it be regarded as suboptimal response or nonresponse? 2. There are many causes of liver dysfunction, such as PBC complicated with viral hepatitis, alcoholic liver disease, metabolic fatty liver disease, drug induced liver injury, overlapping AIH and so on. How to deal with these cases? It should be explained in the article. 3. Line 6 on Page 11: The “form the UK-PBC cohort” should be “from the UK-PBC cohort”. 4. Natural history of PBC has different clinical stages. Among 194 patients, are there patients with liver cirrhosis? Does URS have the same predictive power in PBC cirrhosis? The author should analyze it. 5. Both URS and PBC response criteria in this study are based on Toronto criteria in which the definition of response is $ALP \leq 1.67ULN$ after 24 months of UDCA treatment. But the Paris-2 criteria is more widely used at present. The definition of response includes not only ALP, but also AST and TBIL. AST and TBIL also affect the prognosis of PBC. How to explain it? 6. The discrimination ability was lower in patients with normal baseline AT values compared to that in patients with increased baseline AT values. That means baseline AT values may affect the discrimination ability of treatment response. Please explain it. 	
29	63075	03028174, 00030847	Grade C (Good), Grade B (Very good)	Grade B (Minor language polishing), Grade B (Minor language polishing)	<p>(1)Reviewer 03028174: The study is aimed to review the deep learning for diagnosis of precancerous lesions in upper gastrointestinal endoscopic procedure. The title is “Deep learning for diagnosis of precancerous lesions in upper gastrointestinal endoscopy: A review”.</p> <ol style="list-style-type: none"> 1. This is a review article. 2. Please add more details of the limitations and the disadvantages of the deep learning. 3. What is the new knowledge from this article? 	<p>1 Scientific quality: The manuscript describes a Minireview of the Deep learning for diagnosis of precancerous lesions in upper gastrointestinal endoscopy. The topic is within the scope of the WJG. (1) Classification: Grade B and Grade C; (2) Summary of the Peer-Review Report: The describes</p>

				<p>4. Finally, please recommend the readers “How to apply this knowledge for routine clinical practice?”.</p> <p>(2)Reviewer 00030847: This review article describes AI diagnosis of upper gastrointestinal cancers, in particular the ability of deep learning to diagnose precancerous lesions, and describes selection bias in a retrospective study, making it a well-written article. It is a well-written paper.</p> <p>About Figure 5. What does the orange colour mean? Why is the leftmost case, which appears to be a circumferential Barrett's esophagus, only partially orange? Please explain.</p> <p>Translated with www.DeepL.com/Translator (free version)</p>	<p>selection bias in a retrospective study, making it a well-written article. The author need to add more details of the limitations and the disadvantages of the deep learning. The questions raised by the reviewers should be answered; (3) Format: There are 2 tables and 5 figures; (4) References: A total of 74 references are cited, including 50 references published in the last 3 years; (5) Self-cited references: There are 2 self-cited references; and (6) References recommendations: The authors have the right to refuse to cite improper references recommended by the peer reviewer(s), especially references published by the peer reviewer(s) him/herself (themselves). If the authors find the peer reviewer(s) request for the authors to cite improper references published by him/herself (themselves), please send the peer reviewer ' s ID number to editorialoffice@wjgnet.com. The Editorial Office will close and remove the peer reviewer from the F6Publishing system immediately.</p> <p>2 Language evaluation: Classification: Grade B and Grade B.</p> <p>3 Academic norms and rules: No</p>
--	--	--	--	--	--

					<p>academic misconduct was found in the Bing search. (Bing 检索 -Bing Liu M-1)</p> <p>4 Supplementary comments: This is an invited manuscript. The study was supported by The Science and Technology Development Fund. The topic has not previously been published in the <i>WJG</i>.</p> <p>5 Issues raised: (1) The language classification is Grade B. Please visit the following website for the professional English language editing companies we recommend: https://www.wjgnet.com/bpg/gerinfo/240; (2) The authors did not provide the approved grant application form(s). Please upload the approved grant application form(s) or funding agency copy of any approval document(s); (3) The authors did not provide original pictures. Please provide the original figure documents. Please prepare and arrange the figures using PowerPoint to ensure that all graphs or arrows or text portions can be reprocessed by the editor; and (4) Please obtain permission for the use of picture(s). If an author of a submission is re-using a figure or figures published elsewhere, or that is copyrighted, the author must provide</p>
--	--	--	--	--	--

						<p>documentation that the previous publisher or copyright holder has given permission for the figure to be re-published; and correctly indicating the reference source and copyrights. For example, “Figure 1 Histopathological examination by hematoxylin-eosin staining (200 ×). A: Control group; B: Model group; C: Pioglitazone hydrochloride group; D: Chinese herbal medicine group. Citation: Yang JM, Sun Y, Wang M, Zhang XL, Zhang SJ, Gao YS, Chen L, Wu MY, Zhou L, Zhou YM, Wang Y, Zheng FJ, Li YH. Regulatory effect of a Chinese herbal medicine formula on non-alcoholic fatty liver disease. <i>World J Gastroenterol</i> 2019; 25(34): 5105-5119. Copyright ©The Author(s) 2019. Published by Baishideng Publishing Group Inc^[6]”. And please cite the reference source in the references list. If the author fails to properly cite the published or copyrighted picture(s) or table(s) as described above, he/she will be subject to withdrawal of the article from BPG publications and may even be held liable.</p> <p>6 Recommendation: Conditional acceptance.</p>
--	--	--	--	--	--	--

30	63077	02542439	Grade B (Very good)	Grade A (Priority publishing)	<p>(1)Reviewer 02542439: Title: Efficacy, feasibility, and safety of endoscopic ultrasound-guided fine-needle biopsy for the diagnosis of gastrointestinal subepithelial lesions: A systematic review and meta-analysis</p> <p>Reviewer's comment to the author</p> <p>This review article regarding FNA sampling for GI SELs is statistically well-analyzed and summarized. And also, this result provides us an important information in the preoperative diagnosis of SELs.</p> <p>However, I would like to suggest some issues of this article with several comments and criticisms as following.</p> <p>Major.</p> <p>1. Major guidelines state that the cut-off 2cm of lesion size is clinically important. Authors mention that the number of lesions with a mean size smaller than 20 mm in our analysis was only 77, which was too small to analyze. However, authors need to calculate the diagnostic yield of smaller size than 20 mm even if not statistically significant. This value is grate information for clinicians.</p> <p>Please refer the sentences of a following paper: Endosc Int Open. 2017; 5: E695-E705</p> <p>Major guidelines offer similar recommendations for management of GISTs. The National Comprehensive Cancer Network (NCCN), European Society for Medical Oncology (ESMO), and Japanese guidelines state that lesions ≥ 2 cm could be excised and biopsied [4 – 7]. The NCCN guideline states that the 2-cm cutoff is rather arbitrary, although reasonable [4],</p> <p>2. Authors need to discuss novel EUS scope, called for Forward viewing</p>
----	-------	----------	------------------------	-----------------------------------	--

					<p>echoendoscopes.</p> <p>Please refer the paper; Matsuzaki I, Gastrointest Endosc 2015; 82:287–295</p> <p>3. Authors introduced SINK. In addition, authors need to discuss several mucosal cutting biopsy methods as alternative options while referring the following paper; Osoegawa T, et al. Dig Endosc. 2019 Jul;31(4):413-421. Kobara H, et al. Endosc Int Open. 2017; 5: E695-E705.</p> <p>4. Did authors include comparative studies of EUS-FNB vs. mucosal cutting biopsy method such as the above refs. in this inclusion criteria?</p> <p style="padding-left: 40px;">If they are excluded, authors mention this in the exclusion criteria.</p> <p>Minor.</p> <p>5. References</p> <p>Ref. no 38 and 43 are same paper. Thus, please modify Ref no. thorough the whole text.</p>	
31	63083	03475590, 03018154, 00068559	Grade C (Good), Grade D (Fair), Grade C (Good)	Grade A (Priority publishing (Minor language polishing), Grade B (Minor language polishing)	<p>(1)Reviewer 03475590: In the manuscript, the authors reported a new findings that positivity for both CTLA-4 and PD-L1 predicts a better survival in GC patients. This discovery enriches current theories about the relationship between the immune microenvironment and tumor development by data from real-world. I suggest that the manuscript be published after making the following minor revisions.</p> <p>1. The author reported that for CTLA-4-negative patients, CMT/CRT group has better DFS than those in non CMT/CRT group, while there is no difference in CTLA-4- positive patients. Thus, it would be interesting to conversely analyze the relationship between CTLA-4 expression and GC prognosis in patients of CMT/CRT group and non CMT/CRT group respectively, at least in the supplementary materials. I believe that this to some extent support the role of CTLA-4 in GC treatment and prognosis.</p> <p>2. Please carefully modify the layout of the comparison between groups with different expression of CTLA-4 and PD-L1 in univariate analysis and multivariate</p>	

				<p>analysis in Table 2 to make the statement more accurate.</p> <p>3. How many tumor cores each tissue array block contained? It should be stated in the method section.</p> <p>4. The discussion part is too long and the findings are not prominent enough. It would be better to use appropriate concise language to clearly summarize your findings, and briefly discuss with other literature reports to highlight the key points.</p> <p>(2)Reviewer 03018154: This study examined the expression of CTLA-4 with immunohistochemistry in gastric cancer, and the association between CTLA-4 expression and clinicopathological variables or patients' prognosis was also explored. This study is interesting, and I mainly focus on a few substantive concerns:</p> <p>1. Expression loss of MMR proteins (dMMR) is not equal to MSI, although a high consistence between dMMR and MSI has been validated. Thus, microsatellite markers (such as BAT25, BAT26) should be detected for MSI determination.</p> <p>2. EBV-associated gastric adenocarcinomas also exhibit abundant tumor-infiltrating lymphocytes and expression of PD-L1, thus in this study, the association between CTLA-4 expression and EBV affection needs to be domonstrated.</p> <p>3. Did CTLA-4 positive / CPS positive patients show a better prognosis than all the other patients (including CTLA-4 positive / CPS negative, CTLA-4 negative / CPS positive, CTLA-4 negative / CPS negative patients)? It seems not feasible to evaluate prognosis with two markers in clinical practice.</p>	
--	--	--	--	---	--

					<p>(3)Reviewer 00068559: The authors reported their retrospective study about the prognostic impact of CTA-4 expression in curatice gastric cancer and its association with PD-L1 expression. The research proved that positivity for both CTLA-4 and PD-L1 was an independent factor associated to better survival in GC patients, and may be useful as an additional marker for stratifying patients and predicting prognosis. The discussion interpreted the findings adequately and appropriately. It is helpful to deepen our clinical understanding of gastric cancer. Please check Table 1. the numbers of CTLA-4 Negtive(n=125) in MSI/MSS status, and Supplementary Table 1. the numbers of CTLA-4 positive(n=159) in pT, both negative and positive numbers in Recurrence. If possible, D-L1 immunohistochemical staining figure could be presented.</p>	
32	63086	00482530	Grade C (Good)	Grade C (A great deal of language polishing)	<p>(1)Reviewer 00482530: This clinical case report summarises a case of post-operative ventricular dysfunction in an 18-month-old patient operated on for ASD and VSD repair. After attempting significant inotropic support that failed, the patient was arrest with warm blood cardioplegia for a period (duration is unclear) and subsequent weaning from CPB was successful. It is suggested that a 'hotshot' of blood cardioplegia may be a way to treat these patients when circulatory support is not an option.</p> <p>This is an interesting case report. However, the authors do not report the original cardioprotective strategy, so it is difficult to know whether they considered their original strategy as one that should have provided optimal myocardial protection. The English is rather clumsy and needs further attention from a native English speaker! Also, the videos were not provided to this reviewer.</p> <p>Specific Comments.</p> <p>Abstract:</p> <ul style="list-style-type: none"> • Are the authors suggesting that the only current treatment would be 	

					<p>mechanical circulatory support?</p> <ul style="list-style-type: none"> • 'repair'! • 'weaning'? • 18-month-old? • Was the alternative treatment successful? If so, state it! ie. 'successful alternative treatment'! <p>Introduction:</p> <ul style="list-style-type: none"> • The English is still rather clumsy! • Suggest reversing this sentence to read 'In ventricular failure the only option is to induce mechanical circulatory support.' • '18-month'? • 'hypertension' - spelling! • 'repair'! <p>Case Report:</p> <ul style="list-style-type: none"> • There is no mention of the original cardioprotective strategy for this patient! <p>How was this done? Would it be considered optimal?</p> <ul style="list-style-type: none"> • 'returned to sinus rhythm'? • 'both left and right'? • What is 'hypophysin'? • See earlier comments relating to sinus rhythm! • Check these numbers. The original durations were 56 Xclamp and 65 CPB minutes respectively! Xclamp can't be 404 if CPB is 75! <p>Discussion:</p> <ul style="list-style-type: none"> • Need to discuss the original cardioprotective strategy. Was cardioplegia used? If so, what was it and how was it administered? If not, give details! 	
33	63094	04213276, 05077163, 03765445,	Grade D (Fair), Grade E	Grade B (Minor language	<p>(1)Reviewer 04213276: The manuscript contains too many information regarding the state of endoscopy on periampullary disease, which are much bigger than the sections referring to the AI applications. The article can be shortened by much</p>	

	03725492, 03727239	(Do not publish), Grade B (Very good), Grade E (Do not publish), Grade B (Very good)	polishing), Grade D (Rejection), Grade B (Minor language polishing), Grade C (A great deal of language polishing), Grade B (Minor language polishing)	<p>and focus on AI without general information that are already known to most physicians.</p> <p>(2)Reviewer 05077163: In this review article, the authors explain the current status in the diagnosis and treatment of periampullary disease and argue that AI may be useful in assisting diagnosis and treatment of it. This topic is great interest to the reader of this journal. However, this article has fatal problems for publication.</p> <ul style="list-style-type: none"> ▪ This article contains a large degree of textbook information, on the other hand, contains few information about the topic of AI in peri-ampullary benign and malignant diseases. To compare recent review article, the article contains quite few references. ▪ This article shows some incorrect citations. For example, the sentences about ref no.53 is incorrect. There is a problem with reliability as a review article. ▪ English is problem. 'Nipple' is a bad joke. <p>(3)Reviewer 03765445: Overall, a very good referenced review article with good content. Minor language polishing needed. I have raised a few questions which may help to address a more meaningful future directions.</p> <p>Core tip Line 9-10 ...benign and malignant periampullary disease.</p> <p>Introduction</p> <p>Paragraph 1. So what is AI or AI concept? Line 3...AI technology can make... Line 4...information processing reach or exceed that of ...</p> <p>Paragraph 5. Line 15...has received increasing paid more and more attention.</p>	
--	-----------------------	--	--	--	--

				<p>Paragraph 6.</p> <p>Line 1... diverse diagnosis diagnostic methods,</p> <p>Line 3... of benign periampullary diseases... and the avoidance of ERCP-related adverse events.</p> <p>Line 4...comment: perhaps use periampullary resection or papillary resection rather than nipple resection.</p> <p>Paragraph 7.</p> <p>Line 5...malignant papillary lesions.</p> <p>Paragraph 8.</p> <p>Line 1... With the recent advancement of big data, ...particularly the accumulation of ...</p> <p>Line 2 ...have has promoted the need to the use of AI...</p> <p>Line 7 ...However, the universal...Comment: What are the universal challenges here?</p> <p>Line 10 ...benign and malignant periampullary diseases by performing from a literature ...</p> <p>Line 14 ...have been reconciled consulted to ...</p> <p>Paragraph 9.</p> <p>A repeat. To delete.</p> <p>Application of AI in diagnosis of benign disease.</p> <p>The diagnosis of choledocholithiasis is based on pre-procedural history taking, physical examination, biochemical LFT test, imaging tests such as US scan, CT, MRCP and/or EUS. ERCP and Intraoperative US scan, intra-operative cholangiogram or choledochoscopy also have been used for similar diagnostic purposes. ERCP and ductal stone extraction is the gold standard for both diagnostic and therapeutic purposes. Can you justify why the treatment decision</p>	
--	--	--	--	---	--

				<p>of these patients is difficult for clinicians in the past?</p> <p>Application of AI in treatment of benign diseases.</p> <p>Long paragraph ... Should break the long paragraph into a few shorter paragraphs. Can use papilla or ampulla and try to avoid nipple.</p> <p>Line 29-30 ... to give optimal sedation ...make an accurate sedation...</p> <p>Future development trend and challenges in endoscopy-based AI.</p> <p>There are many challenges regarding the utility of AI technology in endoscopy-related diagnosis and treatment. Do we need more studies to validate the current AI technology? If yes, what kind of studies do you propose? These studies may potentially address the safety concerns.</p> <p>Conclusion</p> <p>How do we address the legal and ethical issue of the regulation of AI technology?</p> <p>It is important to point out that we use AI technology to enhance our available diagnostics tools, decision making in therapeutic options and not to allow complete take over without the control of our human-being.</p> <p>(4)Reviewer 03725492: This manuscript reviewed AI application for periampullary diseases. This manuscript includes a lot of contents which should be shortened. Furthermore, this manuscript didn't review properly regarding main topic, which contribute low priority for publication. I suggest some corrections for next submission.</p> <p>Major</p> <p>You don't have to review the role of ERCP, EUS, IDUS, and Spyglass in detail. Most of this article consisted well known fact and results rather than new technique, AI. You should briefly summarize the limitation of current technique not to describe in detail. There are duplications also. I recommend you to check the manuscript totally. This manuscript can be shortened by 30-40%.</p>	
--	--	--	--	--	--

				<p>(5)Reviewer 03727239: This is a review of application of AI in the endoscopic diagnosis and treatment of periampullary disease</p> <p>Comment 1</p> <p>For early ampullary tumors and adenoma-T1 stage cancer, minimally invasive endoscopic papillectomy is preferable to surgical resection. Hence, diagnosis and staging of ampullary tumors is important, as the author has described in this study.</p> <p>The usefulness of endoscopic images with NBI and magnifying observation for diagnosis of ampullary lesions has been reported previously (1, 2); however, CAD using endoscopic images is not yet validated for use in ampullary tumors. On the other hand, among gastric and colorectal cancers, CAD using endoscopic images has already been validated for differential diagnosis and staging in many studies (3-5). I think CAD using endoscopic images might also be useful for use in ampullary tumors. Please consider briefly discussing the potential role of endoscopic images for CAD of ampullary tumors.</p> <p>Comment 2</p> <p>In the introduction section, the last paragraph is overlapping with the previous one.</p> <p>1. Uchiyama Y, Imazu H, Kakutani H, Hino S, Sumiyama K, Kuramochi A, Tsukinaga S, Matsunaga K, Nakayoshi T, Goda K, Saito S, Kaise M, Kawamuara M, Omar S, Tajiri H. New approach to diagnosing ampullary tumors by magnifying endoscopy combined with a narrow-band imaging system. J Gastroenterol. 2006 May;41(5):483-90.</p> <p>2. Park JS, Seo DW, Song TJ, Park DH, Lee SS, Lee SK, Kim MH. Usefulness of white-light imaging-guided narrow-band imaging for the differential diagnosis of small ampullary lesions. Gastrointest Endosc. 2015 Jul;82(1):94-101.</p>	
--	--	--	--	--	--

					<p>3. Le Berre C, Sandborn WJ, Aridhi S, Devignes MD, Fournier L, Smail-Tabbone M, Danese S, Peyrin-Biroulet L. Application of Artificial Intelligence to Gastroenterology and Hepatology. <i>Gastroenterology</i>. 2020 Jan;158(1):76-94.</p> <p>4. Zhu Y, Wang QC, Xu MD, Zhang Z, Cheng J, Zhong YS, Zhang YQ, Chen WF, Yao LQ, Zhou PH, Li QL. Application of convolutional neural network in the diagnosis of the invasion depth of gastric cancer based on conventional endoscopy. <i>Gastrointest Endosc</i>. 2019 Apr;89(4):806-815.</p> <p>5. Tamai N, Saito Y, Sakamoto T, Nakajima T, Matsuda T, Sumiyama K, Tajiri H, Koyama R, Kido S. Effectiveness of computer-aided diagnosis of colorectal lesions using novel software for magnifying narrow-band imaging: a pilot study. <i>Endosc Int Open</i>. 2017 Aug;5(8):E690-E694.</p>	
34	63095	03767650, 03647881, 00034489	Grade E (Do not publish), Grade C (Good), Grade B (Very good)	Grade B (Minor language polishing), Grade B (Minor language polishing), Grade B (Minor language polishing)	<p>(1)Reviewer 03767650: The authors showed no significant relation between markers of early kidney damage and H. pylori infection.</p> <p>In the abstract, this study can not conclude H. pylori is not a major factor affecting the onset of renal damage. This is because there was only no statistically significant difference.</p> <p>Regarding the content of the discussion, this section should first state the conclusions of this paper and its importance, but this paper does not have such contents.</p> <p>(2)Reviewer 03647881: No comments.</p> <p>(3)Reviewer 00034489: The authors report that H. pylori infection does not cause any change in renal function in healthy subjects. It's written very well. The following are the hands that are of slight concern.</p> <p>1) Please indicate the number of H. pylori infections in 67 kidney disease patients excluded from the study.</p> <p>2) "A subject was diagnosed with diabetes mellitus (DM) if fasting blood glucose was 7.0 mmol/L or more, if glycosylated hemoglobin was more than 6.5%, or if there was a history of using a hypoglycemic agent, DM, or both." Are "DM, or</p>	

					both" correct?	
35	63096	05397187, 05710028, 03722267, 02938811	Grade C (Good), Grade C (Good), Grade A (Excellent), Grade D (Fair)	Grade B (Minor language polishing), Grade C (A great deal of language polishing), Grade B (Minor language polishing)	<p>(1)Reviewer 05397187: This is a very general topic and lots of reviews have been published. The authors may re-focus on recent updates and guideline changes, and provide more information on updates on surgical treatments indications / follow ups in tables.</p> <p>(2)Reviewer 05710028: This is a review article regarding GB stones and CBD stones. I think it is well-written, however, I have some concern to be revised.</p> <p>Minor issue:</p> <ol style="list-style-type: none"> 1. Title: The title of this manuscript included only surgical management, however, endoscopic procedures were also included in the main text. Please reconsider your title. 2. Key Words: There are no key words observed. 3. In the section of "RADIOLOGICAL EXAMINATIONS", 4th paragraph, you mentioned "MRC may allow for providing a higher spatial resolution then EUS". Is that true? You mentioned above "This was mainly due to the significantly higher sensitivity of EUS in the identification of small stones. 4. This manuscript should be checked by a native English speaker. <p>(3)Reviewer 03722267: This review article is interesting and well written. It is better to present the indication and contraindication criteria of one-session and two-session treatment.</p> <ul style="list-style-type: none"> ▪ Is one-session treatment contraindicated in patients with severe acute cholangitis? ▪ Is choledochotomy contraindicated in patients with non-dilated common bile duct to avoid postoperative biliary stenosis ? ▪ In patients with primary bile duct stones, stone recurrence may be more than that of secondary stones after laparoscopic stone removal in one-session treatment. Is two-session treatment better than one-session treatment for 	

					<p>suspected primary stones such as cholesterol stones in preoperative diagnostic modality ?</p> <p>(4)Reviewer 02938811: the manuscript describes on of the most important problem in surgery. the topic is not new and it did not add something new. It did not have anything new. the limitations that the study is review without adding anything new and also most of the references used are old and I suspected in such review is to include new references with new suggested techniques for diagnoses and management</p>	
36	63099	03604107	Grade B (Very good)	Grade B (Minor language polishing)	<p>(1)Reviewer 03604107: The paper is well-written, and an important clinical message is transmitted.</p> <p>A few comments:</p> <p>1. You refer to a travel to Nigeria and Dubai. Geographically it makes little sense: Nigeria is a big country with more than two hundred million inhabitants; Dubai is a city. Correct the details: which cities of Nigeria, where, how long... Dubai (UAE) was declared a decade ago as free from malaria, but imported cases are continuously registered.</p> <p>Please showcase this fact: the origin of malaria was Nigeria; not Dubai (or am I wrong)? If transiting at the airport, it makes no difference: the P. falciparum strain was an African one....</p> <p>2. Very few expressions are awkward:</p> <p>--- was diagnosed suspiciously (was suspected ?)</p>	

					<p>---we made the diagnosis in time (was timely diagnosed ?)</p> <p>3. When you state "artemether is 6 times more potent that of artemisinin"; please quote relevant references. Who has so clearly cut the relative potential of one drug vs. the other ?</p> <p>4. Please use throughout the same style of quoting references (some are in superscript, some are not).</p>	
37	63105	01489938	Grade D (Fair)	Grade B (Minor language polishing)	<p>(1)Reviewer 01489938: In the retrospective study of Li Yuan et al. the authors perform a qualitatively and quantitatively evaluation on publications about DNA methylation in reproductive medicine, using a visual exploration tool CiteSpace, to identify emerging trends and transient patterns in scientific literature.</p> <p>As a scientometric analysis, this article is interesting, and it shows that DNA methylation is a hot topic in the field of reproductive medicine, hence it provides valuable information for researchers to find new perspectives of potential collaborators and cooperative institutions and determine the future research direction.</p> <p>In the view of a basic researcher, however, some key point need to be revised or discussed in another angel.</p> <p>I am not sure that the following statements are entirely correct:</p> <p>“However, the annual citation frequency of these productive authors was not included in the top 15, suggesting that these productive authors should consider not only the quantity of articles, but also the quality of their articles.”</p> <p>“demonstrating their critical roles in DNA methylation-related studies in reproductive medicine.”</p> <p>The quality of a publication is not just about which reputable journal it was published in. Many important results are often not accepted for publication by</p>	

					<p>reputable journals because authors have less knowledge or cannot afford horrible publication and color image fees.</p> <p>And many times, the "open access" system can be a barrier to quality against quantity! Many times, uncritically poor-quality materials are published presumably for the purpose of paying publication fees.</p> <p>In the case of the original publications, have the used methods been tested for reproducibility?</p> <p>Has an unbelievable increase in the number of co-authors been investigated? (I mean, that sometimes someone is included in an article just to increase that person's Scientometric parameters, but later he or she is not mentioned in any communication on a similar topic as an author.)</p> <p>I suggest a major revision of the article, before accept it for publication.</p>	
38	63112	05743807	Grade D (Fair)	Grade B (Minor language polishing)	<p>(1)Reviewer 05743807: General comments:</p> <p>The authors (Juan Liu and Guang-Ling Shi) present in their article (Evidence-based practice for the prevention of fecal incontinence-associated dermatitis in ICU patients) a report on evidence retrieval, evaluation and summary of preventive intervention for fecal incontinence-related dermatitis in ICU patients and their implementation in clinical practice.</p> <p>The authors present in their article evidence, which is certainly interesting, however does, in light of the other publications, not provide new information on the topic at hand. Finally, I believe the authors could rework their article into a short report.</p> <p>Comments:</p> <p>- “Incontinent dermatitis” is not a common or internationally used phrasing, nor is “incontinence-related dermatitis”, to the best of my knowledge, a common terminus; the possibilities include “incontinence-associated dermatitis (IAD) or incontinence dermatitis” as stated in the in the “Identifying evidence-based care issues” and “Literature search” sections.</p> <p>- I am sorry for my lack of knowledge; however, I am sadly not familiar with the concept of PIPOST, nor are there any results in PubMed, google or UpToDate. A</p>	

				<p>citation would help the readers to become familiar with this concept.</p> <p>- The search protocol is quite similar to the article from Beeckman et al. (Cochrane database), which found 13 adequate trials for their study in 2016. Also, how did the authors decide which articles are irrelevant or invalid (apart from the exclusion criteria)? I advise presenting the search protocol with the help of a PRISMA study protocol (not included). Moreover, the discussion section omits similar studies, such as:</p> <ul style="list-style-type: none"> o Prospective study on Incontinence-Associated Dermatitis and its Severity instrument for verifying its ability to predict the development of pressure ulcers in patients with fecal incontinence; 10.1111/iwj.12549 o A Survey of Turkish Nurses' Knowledge of Incontinence-Associated Dermatitis; 10.1097/WON.0000000000000567 o Knowledge of nursing professionals about dermatitis associated with incontinence and pressure injury. 10.5935/1415-2762.20190014 <p>- The recommendations are nicely organised; however, these have been to already published in other guidelines and review articles (some of which have not been included in the literature list or discussed):</p> <ul style="list-style-type: none"> o Incontinence-Associated Dermatitis: State of the Science and Knowledge Translation. 10.1097/01.ASW.0000546234.12260.61 (not included in the review). o Incontinence - associated dermatitis in the critically ill patient: an intensive care perspective. https://doi.org/10.1111/nicc.12331 (not included in the review) o A decade of research on Incontinence-Associated Dermatitis (IAD): Evidence, knowledge gaps and next steps. 10.1016/j.jtv.2016.02.004 (not included in the review) o Fecal Incontinence in the Elderly. https://doi.org/10.1016/j.cger.2020.08.006 o Best practice recommendations for the prevention and management of Incontinence-associated Dermatitis. https://www.woundscanada.ca/docman/public/wound-care-canada-magazine/2010-vol-8-no-3/376-wcc-2010-v8n3-best-practice-english/file o Prevention and management of incontinence-associated dermatitis. 10.12968/bjon.2017.26.20.1128 	
--	--	--	--	--	--

					<p>- What is more, what were the defining points or what was the thought process that led to either choosing an A or B evidence level categorisation?</p> <p>- Furthermore, the authors speak of a questionnaire, I believe it would be advisable to include the questionnaire as a supplementary material. Otherwise, the reader is not able to decide whether the questions were topic relevant or not.</p> <p>- Last but not least, the additional supplement data, that was given to me as a reviewer consisted of: Language certificate, PRISMA guidelines, Biostatistics certificate. All of these are merely samples. I suppose the authors should be able to provide these files. Also, I have not seen a ethics statement copy.</p>	
39	63116	02441021, 05242826	Grade C (Good), Grade B (Very good)	Grade B (Minor language polishing), Grade B (Minor language polishing)	<p>(1)Reviewer 02441021: Lot of English language mistakes and needs to be revised by a native English speaker</p> <ul style="list-style-type: none"> • KF not KR ring • Values greater than 250 g/g (4 mol/g) are diagnostics....Values greater than 250 g/g (4 mol/g) are diagnostics • while values less than 50 g/g (0.8 µmol/g)while values less than 50 g/g (0.8 µmol/g) • Finally, metanobactins are a novel approach.....What are your References??? <p>(2)Reviewer 05242826: 1) Title is very uninformative and the reader cannot be sure what is the purpose of the article</p> <p>2) Abstract should contain the description of the article type - in this case it is a narrative review.</p> <p>3) Genetics - authors should mention the current discussion about the penetrance, which may be lower than typically believed 100%, thus increasing the prevalence of asymptomatic forms (see Eur J Clin Invest. 2019 Aug; 49(8): e13147. Published online 2019 Jun 20. doi: 10.1111/eci.13147)</p> <p>4) Authors should briefly also mention epigenetic alterations which may influence phenotype of the disease</p> <p>5) Diagnosis - "Free copper" - at first appearance of this term, it should be explained that it is non-ceruloplasmin (albumin bound) copper, since truly free</p>	

					<p>molecular copper is not present in the plasma</p> <p>6) consider changing very categorical statement about imunological determination of ceruloplasmin, when discussing the calculation of free copper. (E.g. It is preferable to use enzymatically determined ceruloplasmin levels when calculating free copper). Also maybe a wider discussion about poor correlation between ceruloplasmin levels and ceruloplasmin activity can be included - e.g. PMID: 15614251 DOI: 10.1016/j.lab.2004.08.005</p> <p>7) Some efficacy data for DPA and trientine may be included - e.g. DOI: 10.1016/j.cgh.2013.03.012</p>	
40	63118	03060875	Grade B (Very good)	Grade A (Priority publishing)	(1)Reviewer 03060875: The review is comprehensive and interesting. However, the article has to include "perspective" that can further enrich the content.	
41	63576	03477847	Grade D (Fair)	Grade B (Minor language polishing)	<p>(1)Reviewer 03477847: I think, an interesting case is presented in this article. You can find my comments about the article below.</p> <p>Although inform consent is taken from the case, it is not understood due to the fact that the informed consent received from the patient is Chinese.</p> <p>The presentation style of the case is good.</p> <p>The authors classified this article as case report and literature review.</p> <p>However, when looking at the discussion part, it is seen that this part is not sufficient and the literature review is incomplete.</p> <p>Literature comparisons or sampling are not sufficiently included.</p> <p>That's why I think the discussion should be rewritten.</p>	
42	63613	05374991	Grade B (Very good)	Grade B (Minor language polishing)	<p>(1)Reviewer 05374991: General Impression:</p> <p>The authors have reported a case of diffused pleural and pulmonary nocardiosis in a patient that was treated with prednisone for 6 months. The case is interesting and informative. The abstract is brief and concise. The introductions gives a good background about the manuscript's subject. The case presentation is beautifully written in details. The discussion is good and insightful. All the ethical considerations are covered and well-respected. The figures are informative. The</p>	

					<p>language needs to be revised, as the manuscript contains some minor linguistic mistakes. I only have some comments that concern the patient history and the conclusions.</p> <p>Comments:</p> <p>1) I would like to ask: what does the patient work? Or what are her hobbies? Could the authors identify any source of infection? For example working in gardening could increase the exposure to Nocardia and therefore, acquiring the infection.</p> <p>2) Based on the previous comment I would suggest advising the practitioners to advice their patients to avoid the sources of exposure to Nocardia or at least taking some preventive measures when corticosteroids administration will last long.</p> <p>3) I would like to add-up to the conclusions that more severe manifestations of nocardiosis should be suspected when prolonged administration of corticosteroids is present.</p>	
43	63654	03473431, 05770102	Grade C (Good), Grade C (Good)	Grade A (Priority publishing) , Grade B (Minor language polishing)	<p>(1)Reviewer 03473431: The case is very interesting and well written I believe that the knowledge of a possible anaphylactic shock can be of fundamental importance for those who deal with HCC and in particular with the advanced forms.he case is very interesting and well written.</p> <p>The discussion section is complete.</p> <p>(2)Reviewer 05770102: Overall this is an interesting case report. Most reactions to McAb, and more so, in ICIs are likely infusion related reactions rather than anaphylactic reactions.</p> <p>This patient presented with reactions to two different anti-PD(L)-1 agents with severe symptoms, raising questions of this being not just your usual infusion-related reaction.</p> <p>It would be interesting if more could have been done to justify the thought of this being a true anaphylactic reaction or more discussion as to how to differentiate it</p>	

					<p>from just a severe infusion reaction. Was serum tryptase sent? Were there any cutaneous/respiratory/gastrointestinal manifestations? Evaluation by an allergist?</p> <p>Manuscript is legible, but may still need another round of language editing and polishing. References 19 and 21 are identical.</p>	
44	63655	03700188	Grade B (Very good)	Grade B (Minor language polishing)	<p>(1)Reviewer 03700188: The title is appropriate, reflecting well the work done. The summary adequately summarizes the work described in the manuscript. The keywords reflect the work described well. The manuscript adequately describes the history, current status and importance of the study. The manuscript describes methods such as data collection and analysis in adequate detail. The research objectives were achieved by the experiments used in this study. Most of the risk factors related to HCC were already known, but a local study can help us to compare the characteristics of different populations. Discussion: the manuscript interprets the findings appropriately and appropriately, highlighting the key points in a concise, clear and logical manner and and it discusses the scientific meaning of the article to clinical practice. The tables lack the definition of the acronym FBG. The manuscript adequately cites the most recent, important and reliable references in the introduction and discussion sections. And the manuscript is well organized and presented in a concise and consistent manner.</p>	
45	63660	05088164, 02942873	Grade D (Fair), Grade D (Fair)	Grade C (A great deal of language polishing), Grade C (A great deal of language polishing)	<p>(1)Reviewer 05088164: The authors present the case of a child with pulmonary embolism and deep vein thrombosis caused by a novel heterozygous deletion of the PROS1 gene.</p> <p>The title reflects the main subject of this case report.</p> <p>The abstract summarizes the most important aspects of the case presented in the manuscript. Some English language errors in the abstract should be corrected.</p> <p>The key words reflect the focus of the manuscript.</p> <p>The authors should describe the background and present status of knowledge on hereditary protein S deficiency in the Introduction. After 2-3 sentences, they presented the case shortly. This part of the manuscript should be rewritten.</p> <p>The case presentation followed the steps needed to understand the case correctly: admission symptoms, history of the present illness, family history, physical</p>	

				<p>examination, laboratory and imaging results, diagnosis, treatment, and outcome. Some changes need to be done in this part, starting from a check for the use of the English language, use of abbreviations, phrases (...the results of auxiliary inspection results...???), repeated information in the first two sentences in Imaging examinations, to mention some of these changes.</p> <p>In the Discussion, the authors interpret the findings of this case appropriately, highlighting the essential aspects. The authors discussed the case aspects (genetics, treatment) in corroboration with data from references. The references are not so many, and just two are more recent (for the last three years). The figures are of good quality and appropriately illustrative of the case presentation contents.</p> <p>According to manuscript type and the appropriate categories, the authors prepared their manuscripts based on CARE Checklist 2016.</p> <p>The manuscript meets the requirements of ethics, as presented by the authors. I would include this mention in the text core, not in Acknowledgment.</p> <p>Overall, the manuscript should be better presented, as the style, language, and grammar should be improved. The authors should correct the English language before taking into consideration the publication of the paper.</p> <p>(2)Reviewer 02942873: The topic reported in the case report presented by Feng-Qin Liu et al. it would be very interesting. Unfortunately, the description made by the authors is very bad. In particular, the appearance of symptoms is often reported in an unclear sequence. Figure 3 helps a little to understand the sequence of events but it is incomplete and, at times, imprecise. The authors report that the treatment with VKA proved to be clinically ineffective but in reality the drug, as far as we can tell, never reached the therapeutic dosage. During the relapse, was the patient still treatment with LMWH or had it already been erroneously suspended? There are numerous syntax and spelling errors throughout the manuscript which contribute to making it difficult to read. I wonder what the measurement of plasma protein S levels might be in a patient</p>	
--	--	--	--	---	--

					undergoing VKA therapy. A risk factor for thrombosis is immobilization, not reduced mobility related to quarantine from COVID (assuming that the child was not bedridden during this period).	
46	63661	04409361	Grade D (Fair)	Grade B (Minor language polishing)	(1)Reviewer 04409361: Though this article raises some important challenges in the psychiatric field, there is no evidence of causal-relationship and need to be improved logically as a case report.	<p>I just submitted the manuscript and it appears as a “case report” which is not correct</p> <p>I wonder whether I did the wrong selection or there was no place to submit it as "Opinion Review” as we agreed</p> <p>Can you fix this?</p> <p>Manuscript details are:</p> <p>Journal title: World Journal of Psychiatry</p> <p>Manuscript NO: 63661</p> <p>Thanks and best regards</p>
47	63671	05186196, 03026651	Grade B (Very good), Grade D (Fair)	Grade B (Minor language polishing), Grade B (Minor language polishing)	<p>(1)Reviewer 05186196: This is an interesting topic highlight. I have some minor suggestions for improvement.</p> <p>1. The paper needs a minor polishing of the English language. Use the free version of Grammarly at least since there are some typos, e.g. white moss instead of white mass. I also think that in some parts of the paper you could have used some abbreviations since the text seems a little bit wordy.</p> <p>2. H. Pylori infection is also associated with a high glycemic index of the diet which may also explain why subjects with a higher BMI had these lesions: https://www.sciencedirect.com/science/article/abs/pii/S089990072030352X</p> <p>3. The adipose tissue is a metabolically active organ and a source of reactive oxygen species and pro-inflammatory adipocytokines, especially in subjects with obesity, diabetes, diabetesity and their associated complications/comorbidities. Maybe it would be interesting to briefly discuss this hypothesis too. See: https://www.wjgnet.com/1948-9358/full/v11/i5/193.htm</p>	

					(2)Reviewer 03026651: Thank you for the interesting review	
48	63679	00034151, 04870360, 05038583	Grade D (Fair), Grade D (Fair), Grade D (Fair)	Grade B (Minor language polishing), Grade C (A great deal of language polishing), Grade C (A great deal of language polishing)	<p>(1)Reviewer 00034151: The authors performed a systematic review and meta-analysis to evaluate the effects of microbiome-targeted therapies (MTT) on HVPG in cirrhosis persons and PH.</p> <p>It is an interesting study. However, major point should be further clarified.</p> <p>- The authors mixed antibiotics-based studies and probiotics-based studies into one category, as MTT.</p> <p>However, antibiotics-based studies and probiotics-based studies have quite different mode of actions. Thus, it should not be categorized in single therapy as MTT. The results from antibiotics-based studies and probiotics-based studies should be demonstrated separately not as subgroups of MTT.</p> <p>(2)Reviewer 04870360: The manuscript entitled “Gut microbiome-targeted therapies on hepatic venous pressure gradient in cirrhosis: a systematic review and meta-analysis” is a systematic review and meta-analysis study that contains a potentially new idea that is relevant to some point. However, several concerns are raised.</p> <p>1- Extensive English language polishing is required. The manuscript contains a lot of typos and grammar mistakes.</p> <p>2- The abbreviations must be mentioned in full name at the first mention in the manuscript. Please revise all abbreviations.</p> <p>3- Inclusion and exclusion criteria should be revised.</p> <p>4- Scopus database is missed; why?</p> <p>5- Sample size calculation is missed.</p> <p>6- Statistical significance and P-value should be included in the tables.</p> <p>(3)Reviewer 05038583: General comments</p> <p>This is a meta-analysis to evaluate the effect of microbiome-targeted therapies on hepatic venous pressure gradient. Microbiome related studies are increasing interest in current era, and I am not aware of similar meta-analysis in the past, so</p>	

				<p>this can be relevant. Unfortunately, there are a lot of points needs to be addressed before consideration for publication as listed below which would require at least major revision.</p> <p>Specific comments</p> <p>Abstract: Please define LBP, SMD, and TNF as this was not defined on abstract.</p> <p>Too many abbreviations on the manuscripts.</p> <p>Language needs to be improved before consideration for publication.</p> <p>“on-selective beta-blockers (NSSBs) are the only available medical treatments for PH and show a sufficient decrease of HVPG[6, 7].” TIPS placement can decrease portal hypertension and can normalize portal pressure.</p> <p>“Antibiotics represented as rifaximin and norfloxacin selectively decontaminate the intestines, which could lead to hemodynamic change[17, 18]. ‘ It is not clear what it means by hemodynamic change.</p> <p>Author did not mention about scopus. Was this database not searched?</p> <p>“All the studies assessed PH though HVPG measurement, four[27-30] of which required patients should show HVPG >10mm Hg(even more,>12mm Hg),” This sentence is not clear. Please rephrase this so that I can understand what your are implying to.</p> <p>“The probiotics group showed a numerically greater HVPG reduction (-1.98mm Hg, 95%CI: -4. 12, 0.16; P=0.07) than the antibiotics group (-1.01mm Hg, 95%CI: -2.14, 0.11 mmHg; P=0.08), without revealing statistical significance.” This is miss leading as there was no statistical significance.</p> <p>Discussion</p> <p>“The results of our meta-analysis mean that MTTs may have beneficial effects on reducing HVPG in patients with cirrhosis and portal hypertension.” Please see the comment on figure 2.</p> <p>It is not clear to me that reducing 1-2 mmHg of portal pressure has significantly alter the clinical outcome.</p> <p>“Comparison may not be consistent with rigorous scientific analysis.” If authors</p>	
--	--	--	--	--	--

					<p>say this, I am not sure if they should include this paragraph in the discussion section.</p> <p>Table 2.</p> <p>Please define VLS. In addition, CFU is not defined.</p> <p>Figure 1.</p> <p>It is not clear that why animal experiment was not excluded in the initial screening process.</p> <p>Figure 2.</p> <p>What does Gupta (P) and (R) means? Why the same study included in the analysis twice? Is this affecting the p value?</p>	
49	63688	04022782	Grade C (Good)	Grade A (Priority publishing)	<p>(1)Reviewer 04022782: This manuscript presents a survey for research on gastroenterology and hepatology using artificial intelligence techniques. Based on the title, the emphasis is focused on challenges and opportunities. The use of AI in the field of gastroenterology has become more and more evident. This is a clear and well-structured review, which summarizes the advances and limitations of the application of AI in gastroenterology and hepatology.</p> <p>However, I found that the manuscript has some limitations. I break these down into 2 issues in the following section.</p> <p>(1) The opportunities of AI applications were not extensively discussed. There were a lot of bullets on opportunities. However, there is no connections to previous literatures discussed earlier. These sections should be discussed in more details since it is one of the major focuses of this article. Moreover, in section 4 and 5, there is so much information about previous literatures. The authors should summarize them into tables, which will give the highest benefits to readers.</p> <p>(2) Technical accuracy and clarity.</p> <p>[2.1] In section 3, the author stated that ANN is a supervised ML ANN is just another name of neural network and can be used for both supervised and unsupervised learning.</p> <p>[2.2] The authors separately mentioned Deep Learning (DL) and Deep Neural</p>	

					<p>Network (DNN). In fact, they are very much the same. I would suggest using only one term to avoid confusion. Otherwise, the authors should clearly state their differences.</p> <p>[3.3] The last sentence of section 3 is a bit confusing because of the terms “before been fed to the CNN”. Normally, the convolutional process is an integral part of CNN.</p> <p>So far, I really enjoy reading section 6 the most. However, in section 6.2, I cannot really relate cybersecurity in the second paragraph to specific AI applications. Section 6.3 (IP) is also an important issue that was nicely covered by the authors.</p> <p>In general, I agree with the publication of the article after minor revisions.</p>	
50	63691	03765445	Grade D (Fair)	Grade B (Minor language polishing)	<p>(1)Reviewer 03765445: Thank you for inviting me to review this article. My comments as per below:</p> <p>Core tip:</p> <p>Line 2 ... remains as are the main ...</p> <p>Line 3-4 ...Esophageal manifestations such as strictures and stenosis (strictures and stenosis has the same meaning, do you mean occlusion?)</p> <p>Line 8 ...available as treatment options.</p> <p>Introduction</p> <p>Paragraph 1 What is the link between PV and clinical esophageal manifestations?</p> <p>Paragraph 2 Similarly, what is the link between MMP and esophageal pathology?</p> <p>Paragraph 2 Line 1 ...not a single disease, but rather it represents a group ...</p> <p>Paragraph 3 Line 16 What is the association between the sentence and Figure 1?</p> <p>Paragraph 3 Line 18-19 ... Consider Rarely ... bleeding may rarely occur</p> <p>Paragraph 5 Line 4 Can the author clarify the obstruction? I assume Incomplete, partial and not complete or total obstruction?</p> <p>Treatments</p>	

				<p>The authors described pharmacologic therapy can be used. Can the authors justify this as the first line treatment and what are the evidence in the literature?</p> <p>And as to what degree of esophageal symptoms and signs or objective parameters like amount of weight loss that determine endoscopic therapy for esophageal stricture?</p> <p>Dilatation</p> <p>What is the percentage of significant bleeding and perforation risk from esophageal dilatation therapy in these group of autoimmune bullous disease?</p> <p>Surgery (this is not endoscopic treatment technique)</p> <p>Is esophagectomy warranted in this systemic autoimmune disease?</p> <p>What is the controversy in choosing the conduit to replace resected esophagus?</p> <p>Rendezvous</p> <p>What is the indication to perform retrograde dilatation and the contraindication of antegrade esophageal dilatation? If the patient has no PEG tube access, how does the procedurist obtain the access for retrograde dilatation?</p> <p>Some of the endoscopic treatment and references cited for indicated for reflux-related or radiation related stricture and not related to autoimmune bullous esophageal stricture.</p> <p>Endoscopic stenting</p> <p>SEMs are not the appropriate permanent treatment in a benign esophageal disease. Are there any case reports describing the use of dissolvable plastic stents in bullous esophageal stricture or temporary removable SEMs?</p> <p>Discussion</p> <p>Esophageal dilatation is the most appropriate treatment for suitable cases of bullous esophageal stricture. Have the authors explore the role of fine bore NG</p>	
--	--	--	--	--	--

					feeding tube or permanent PEG tube feed as the alternative in the literature?	
					I would suggest the authors focus only on endoscopy therapy for autoimmune bullous-related esophageal stricture and not other aetiology to reflect on the title.	
51	63692	05040484, 03905597, 05238087, 05232723	Grade D (Fair), Grade B (Very good), Grade C (Good), Grade B (Very good)	Grade B (Minor language polishing), Grade B (Minor language polishing), Grade B (Minor language polishing)	<p>(1)Reviewer 05040484: Of course, cirrhosis is accompanied by the development of infectious complications and that some of them can be prevented by vaccination. However, in clinical practice, we rarely decide whether it is necessary to vaccinate patients with cirrhosis, doing it on a general basis. Unfortunately, this review does not answer the question of how vaccination against certain infections affects the survival of patients. This review would be useful for vaccination professionals, but it is unlikely that hepatologists will get the data they need from it.</p> <p>(2)Reviewer 03905597: This review summarizes the efficacy of vaccination for liver cirrhosis (LC). As is clearly written in Conclusion, the recent electric healthcare system is expected to improve vaccination coverage for LC patients. For this purpose, healthcare physician education is important. This review is well written and helps increase awareness of vaccinations to LC patients. I agree with the authors. In my opinion, this review will be acceptable for publication after minor revision. Please consider the following minor points</p> <p>Minor points</p> <p>1) English : OK.</p> <p>2) Abstract: OK</p> <p>3) Keywords: Please add some words, such as preventive medicine, viral infection.</p> <p>4) Core tip: OK</p> <p>5) Please consider the following points in each vaccination</p> <p>In terms of vaccination dose, effects and side effects,</p> <p>a) any difference between chronic hepatitis and liver cirrhosis ?</p> <p>b) any difference between HCC (+) and HCC(-) cases ?</p> <p>c) any change according to causal factor (viral hepatitis, alcohol abuse,--)?</p>	

					<p>6) Please indicate the contraindication of each vaccination, if there is, to provide a quick reference for vaccination plan tailoring.</p> <p>7)Please make a list of abbreviations, such as SD, RR, VZV, etc.</p> <p>(3)Reviewer 05238087: Because of the increased risk of infection of bacteria and viruses in patients with liver cirrhosis, which leads to increased mortality, it is of great significance whether vaccination can reduce this risk. The topic of this review is very meaningful. But there are some problems in writing.</p> <p>1. The article should be written closely around the reports on the vaccination of patients with liver cirrhosis, rather than listing the existing vaccines on the market, while some vaccines are not used in patients with liver cirrhosis, such as diphtheria, tetanus and pertussis vaccination; measles, rubella and mumps vaccination; human papillomavirus vaccination, etc.</p> <p>2. It is suggested to summarize the types, safety, effectiveness and current situation of vaccination in patients with liver cirrhosis.</p> <p>3. The covid-19 vaccine should be written together with other vaccines and should not be listed in the penultimate paragraph of the article. And it should be supplemented with the latest development of covid-19 vaccine.</p> <p>4. It is suggested that the name of vaccine should be written in the front and the type of vaccine should be written in the back in Table 1. "HVA vaccine" and "HVB vaccine" were wrongly written.</p> <p>(4)Reviewer 05232723: In this review, the authors discussed the vaccine approaches available for cirrhotic patients, based on international recommendations and available scientific evidence, hoped to help vaccination plan tailoring, especially for gastroenterologists and primary care physicians, to organize appropriate vaccination schedules and stimulate future research in this field. This idea is very interesting, novel and meaningful, and the summary is quite in place. I think it meets the publishing requirements, and I agree to publish it in our journal.</p>	
52	63710	05840259	Grade D	Grade B	(1)Reviewer 05840259: This study revealed a weak association between the	

			(Fair)	(Minor language polishing)	<p>high-sensitivity C-reactive protein-to-prealbumin ratio and no-reflow after primary percutaneous coronary intervention. The area under ROC curve is only 0.563, with a sensitivity of 26.6% and a specificity of 14.1%. The performance of hsCRP/PAB of predicting no-reflow after PCI is not excellent.</p> <p>Additionally, authors may replenish their work refer to the following comments:</p> <ol style="list-style-type: none"> 1. Prealbumin is not a routine examination in emergency department in many medical centers in China, which may limit the generalization of the study. 2. If it is possible to make sex- and age- matched comparison. 3. Please present the performance of CRP, and compare the performance of CRP and hsCRP/PAB if possible. 4. Please improve statistical analysis, especially logistic regression analysis. 5. Please enrich the discussion. 	
53	63762	03261315	Grade C (Good)	Grade B (Minor language polishing)	<p>(1)Reviewer 03261315: nice work, well written</p> <p>please correct the references , there are some pages which are missing.</p>	
54	63866	05077783	Grade D (Fair)	Grade C (A great deal of language polishing)	<p>(1)Reviewer 05077783: The authors report an unusual clinical presentation of cervical spondylotic myelopathy, closely resembling cervical spondylotic radiculopathy. The case is illustrated by high quality images. which add value to the manuscript. However, major revision is necessary regarding writing in the english language. There is also a contradiction in the discussion, when it is stated that "there were (...) a small number of cases (..) developing radiation pain." and a few lines below: "the radiating pain (...) hasn't been reported before." It would important to address these issues in the manuscript.</p>	
55	63905	05067878	Grade C (Good)	Grade B (Minor language polishing)	<p>(1)Reviewer 05067878: The article is nicely describes the time course of CT changes in the COVID 19 patients. However, the CT chest findings in COVID patients have already been extensively described. This article is not adding much to the literature.</p>	
56	63956	05225448,	Grade A	Grade A	<p>(1)Reviewer 05225448: The authors report the review of latest topics of</p>	

		05080641, 04469475, 03806663	(Excellent), Grade B (Very good), Grade B (Very good), Grade B (Very good)	(Priority publishing , Grade B (Minor language polishing), Grade A (Priority publishing , Grade B (Minor language polishing)	<p>preparation of colonoscopy, endoscopic resection and AI system in colonoscopy.</p> <p>I found this paper to be well-written and engaging; however, some points require clarification. The issues that are listed below must be addressed before the paper can be considered for publication.</p> <ol style="list-style-type: none"> 1. You need to correct the references 66-78, 80-86, 89 – 91, 97-98, 2. You need to delete (20) in P8, L18 3. There is a manuscript reporting the efficacy of continuous administration of anticoagulants with cold snare polypectomy. Please refer this article (Takeuchi Y et al. Ann Intern Med. 2019 Aug 20;171(4):229-237. doi: 10.7326/M19-0026.) 4. There are some latest reports of the prediction of deeply submucosal invasive carcinoma with use of AI. You may add these topics. Please refer these articles (Ito N et al. Oncology. 2019;96(1):44-50. doi: 10.1159/000491636. Epub 2018 Aug 21. Lui TKL et al. Endosc Int Open. 2019 Apr;7(4):E514-E520. doi: 10.1055/a-0849-9548. Epub 2019 Apr 3.) <p>(2)Reviewer 05080641: The author queried and read a large literature.The article lists the treatment methods of early intestinal neoplastic lesions in more detail.This article can give readers a more comprehensive understanding of endoscopic treatment of early colorectal neoplastic lesions.However, the format of some references is not correct, and some references only have the beginning page number without the final page number.</p> <p>(3)Reviewer 04469475: This well written manuscript addresses an important topic exploring diagnostics of coloncancer.</p> <p>The study is well done, the material is large enough and the methods look reliable. The study is based on extensive and recent literature, gives some new information and this warrants its publication.</p> <p>Although several reviews about the diagnostics of colon cancer have been already published, the discussion on the markers of colon carcinoma in this paper seems</p>
--	--	------------------------------------	---	--	--

				<p>to be original. However I have the following suggestions/comments and hope the authors can address them in the review.</p> <p>Minor revision</p> <p>1. Some authors showed that the ADH/ ALDH activities are higher in tumor cells than in normal colon tissue, suggesting that isoenzymes of ADH may play an important role in carcinogenesis. Among all tested classes of ADH isoenzymes, only class I had higher activity in the serum of patients with colon cancer:</p> <ul style="list-style-type: none">• Jelski Wojciech, Zalewski Bogdan, Chrostek Lech, Szmitkowski Maciej: The activity of class I, II, III and IV alcohol dehydrogenase (ADH) isoenzymes and aldehyde dehydrogenase (ALDH) in colorectal cancer. Dig. Dis. Sci 2004, 49(6), 977-981• Jelski Wojciech, Zalewski Bogdan, Chrostek Lech, Szmitkowski Maciej: Alcohol dehydrogenase (ADH) isoenzymes and aldehyde dehydrogenase (ALDH) activity in the sera of patients with colorectal cancer. Clin Exp Med. 2007, 7, 154-157.• Jelski Wojciech, Mroczko Barbara, Szmitkowski Maciej: The diagnostic value of alcohol dehydrogenase (ADH) isoenzymes and aldehyde dehydrogenase (ALDH) measurement in the sera of colorectal cancer patients. Dig Dis Sci. 2010, 55, 2953-2957 <p>Please discuss (5-6 sentenc</p> <p>(4)Reviewer 03806663: This topic is very interesting, this review contain a huge amount of data, we can called it huge review, it begins discussion from the bowel preparation til managemnt of patient complications after treatment, also the topic of artificial intelligence is discussed. some few comments are mentioned below:</p> <p>1- Is there a role for EUS in the diagnosis and staging of colorectal lesions.</p> <p>2- What about the use of the knife type (speed boat knife), also they use a different apparatus from that of ERBE.</p> <p>3- What about the use of (over the stitch device) in the management of colorectal perforations.</p> <p>4- Is it important to give a reference for the table and the figures. do you have</p>	
--	--	--	--	--	--

					<p>permission to use it?</p> <p>5- What about annotations, scale bar, magnifications, type of dye when needed in the legend of figures.</p> <p>6- Language and grammar need polishing. Some corrections are present in the uploaded file.</p> <p>7- Please choose one style for your references.</p> <p>Finally, this review can used as a reference for all doctors in the field of advanced GI endoscopy.</p>	
57	63992	02943442, 03538036	Grade B (Very good), Grade C (Good)	Grade B (Minor language polishing), Grade B (Minor language polishing)	<p>(1)Reviewer 02943442: The title of the study is needed to be revised. The methodology section is lacking, some more improvement is needed. There are some grammatical, punctuation, and syntax issues. References styles are not according to the journal. The study design is good, I recommend to increase the sample size.</p> <p>(2)Reviewer 03538036: I read this manuscript and found it of certain interest. However, it needs revision:</p> <p>1. This study's main problem is the number of studies; the number of studies is relatively small and with highly heterogeneous results.</p> <p>2. The study methodology is NOT well described. The Materials and Method sections are incomplete. The inclusion and exclusion criteria should be more precise.</p> <p>3. The English of the manuscript needs revision. There are many grammatical and syntax errors in the manuscript.</p> <p>4. The references need to be adjusted according to journal style.</p>	
58	63995	03647881	Grade B (Very good)	Grade A (Priority publishing)	(1)Reviewer 03647881: No special comments.	
59	64001	03769687	Grade E (Do not publish)	Grade A (Priority publishing	<p>(1)Reviewer 03769687: A control group is missing. The readers cannot judge what effect the drugs actually have.</p> <p>Further, not only a single drug is under scrutiny but a group of drugs. Please</p>	

)	<p>specify.</p> <p>95% of the study population is under other lipid lowering medication. Again, the effect of the investigational drug is unclear.</p> <p>The dropout rate is quite high. The FOURIER trial included >20.000 patients. I doubt that any meaningful data can be obtained from this study with 45 patients.</p>	
60	64006	05449007	Grade B (Very good)	Grade C (A great deal of language polishing)	<p>(1)Reviewer 05449007: Comments to the Author:</p> <p>The authors must be congratulated for doing a phenomenal job in reviewing the literature in depth and highlighting the role of circRNA in gastrointestinal malignancy. The manuscript is highly relevant to current clinical practice and ongoing translational research in the field of gastrointestinal oncology.</p> <p>However, following are the recommendations/ edits that I would like to make:</p> <p>1. This is a non-systematic review of literature as the process of article selection, methodology of inclusion and exclusion criteria have not been described.</p> <p>2. The section 1 and 2 are too detailed and long with significant redundancy and take away and provide significant distraction away from the major portion of the manuscript that is the section 3. Section 1 and 2 look more to highlight just circRNA and its potential role in oncology but it can be cut short significantly only to describe its origin, pathophysiologic role and potential use in oncology. Cut down these two sections atleast by 1000 words.</p> <p>3. The grammar needs significant revision and editing. There is significant type-setting errors and grammatical mistakes that need to be reviewed by editing services proficient in medical English Grammar.</p> <p>I have attached the revised manuscript with some changes tracked to demonstrate the example of need for grammar and type-setting correction.</p>	
61	64012	02734287, 02954463	Grade D (Fair), Grade C (Good)	Grade C (A great deal of language polishing), Grade A (Priority	<p>(1)Reviewer 02734287: Appendicitis and other gastrointestinal complications of foreign body ingestion have already been extensively studied. I agree that a chicken bone is not a very frequent cause and that brings a certain value to another case report dealing with the topic. The paper is well structured and there are some sound and important points made. Also, the review of the literature may be useful for the reader interested in this pathologic entity. However, the language requires some more editing, preferably by a native speaker.</p>	

				publishing)	(2)Reviewer 02954463: THIS IS A CASE REPORT WITH LITTLE NOVELTY. THE ISSUE HAS BEEN WELL EXPOSED, BUT THE PAPER HAS NOT BEEN STRUCTURED INTO THE SECTIONS OF THE SCIENTIFIC PAPER. AFTER INTRODUCTION, THERE SHOULD BE THE CASE REPORT AND THEN THE DESCRIPTION OF HOW THE REVIEW OF THE LITERATURE HAS BEEN PERFORMED (IT DOES NOT BELONG TO THE DISCUSSION). DISCUSSION SECTION FOLLOWS. MORE DETAILS SHOULD BE PROVIDED ABOUT THE LAPAROSCOPIC TECHNIQUE, IF THE BOWEL WAS EDEMATOUS AND THERE FORE WHAT KIND OF TECHNIQUE WAS USED TO CUT THE APPENDIX AND TO CLOSE THE HOLE IN THE CECUM. WAS A DRAINAGE PUT IN PLACE?	
62	64021	05839630	Grade E (Do not publish)	Grade D (Rejection)	(1)Reviewer 05839630: I found your submission very emotional.	
63	64032	02468626, 05002519	Grade B (Very good), Grade C (Good)	Grade B (Minor language polishing), Grade B (Minor language polishing)	(1)Reviewer 02468626: The authors are to be commended for providing an excellent state of the art review of therapeutic EUS procedures that can replace some major surgical interventions, with minor complications and lower costs. A few remarks are to be raised. Introduction 1.1 Please replace “encouraging development” with “effective technique” 1.2 Last but one paragraph: please delete “between” 1.3 Last paragraph: please delete “further regarding” 1.4 The following references should be added: -Fusaroli P, Kypraios D, Eloubeidi MA, Caletti G. Levels of evidence in endoscopic ultrasonography: A systematic review. Digestive Diseases and Sciences 2012;57:602-609. -Fabbri, C., Luigiano, C., Lisotti, A., Cennamo, V., Virgilio, C., Caletti, G., Fusaroli, P. Endoscopic ultrasound-guided treatments: Are we getting evidence based - A systematic review. World Journal of Gastroenterology 2014;20:8424-8448.	

				<p>EUS-guided biliary drainage</p> <p>2.1 The following references should be added:</p> <p>-Guo J, Giovannini M, Sahai AV, Saftoiu A, Dietrich CF, Santo E,et al. A multi-institution consensus on how to perform EUS-guided biliary drainage for malignant biliary obstruction. Endoscopic Ultrasound 2018;7:356-365.</p> <p>-Fusaroli P, Serrani M, Lisotti A, D'Ercole MC, Ceroni L, Caletti G. Performance of the forward-view echoendoscope for pancreaticobiliary examination in patients with status post-upper gastrointestinal surgery. Endoscopic Ultrasound 2015;4:336-341.</p> <p>2.2 Page 6, line 6: please delete “2-years”</p> <p>Pancreatic Pseudocysts and Walled-Off Necrosis</p> <p>3.1 Page 13, line 16: please replace “conquer” with “overcome”</p> <p>3.2 Page 13 line 19: please replace “ends which can improve the migration process” with “ends which can prevent migration”</p> <p>3.3 The following reference should be added:</p> <p>-Guo J, Saftoiu A, Vilman P, Fusaroli P, Giovannini M, Mishra G, et al. A multi-institutional consensus on how to perform endoscopic ultrasound-guided peri-pancreatic fluid collection drainage and endoscopic necrosectomy. Endoscopic Ultrasound 2017;6:285-291.</p> <p>EUS-guided Gastroenterostomy (EUS-GE)/Jejunostomy</p> <p>4.1 Page 17, line 2: please replace “implicated” with “reported”</p> <p>(2)Reviewer 05002519: The authors showed a review article that compered therapeutic interventional endoscopic ultrasound (EUS) with surgical treatment. The article is interested. However, there are some adjusting points.</p> <p>Major comments:</p> <p>1. The authors treated percutaneous procedure with surgical treatment in the manuscript. However, I thought that percutaneous procedure was not surgical</p>
--	--	--	--	--

					<p>treatment because the degree of invasion is different between these two procedure. The authors should revise the main text and the title, for example Surgical approach to Surgical/ Percutaneous approach.</p> <p>2. The main point of this article is comparing therapeutic interventional EUS with surgical treatment. However, regarding EUS rendezvous technique, the authors just listed only each concrete method in page 4 and did not showed comparing the therapeutic interventional EUS with surgical treatment. The authors should modified the paragraph.</p> <p>Minor comments:</p> <p>1. The authors wrote "Similarly, high success rate was also demonstrated by another meta-analysis, with higher-rate of overall procedure-related complications (18.04%) in EUS-BD procedure performed in patients with inoperable malignant biliary strictures who failed an ERCP procedure" in page 6, line 1. The authors should replace the words "higher-rate of overall procedure-related complications" with "low-rate of overall procedure-related complications".</p> <p>2. In main text, there is no word "Figure 1". The authors should write words "Figure 1" in the proper place in the main text.</p> <p>3. EUS-guided gastroenterostomy/jejunostomy are not common yet. The authors should add these figures.</p>	
64	64033	02942798, 03475479	Grade D (Fair), Grade D (Fair)	Grade B (Minor language polishing), Grade B (Minor language	<p>(1)Reviewer 02942798: Dear sir, thank you to select me to review manuscript: Zunirah A et al. Viral Hepatitis: The entire Alphabet From A-E. It is interesting topic, but the novelty is lacking in some parts of the article. I recommend shortening or deleting the chapters about HAV, HCV and HEV, I also recommend shortening of part about epidemiology of HBV and HDV.</p> <p>My recommendations are bellow:</p> <p>1) Please add a figure with the HBV replication cycle and potential targets for the</p>	

				polishing)	<p>treatment of HBV infection.</p> <p>2) Please add the similar figure for HDV infection.</p> <p>3) Discuss potential HBV treatment options in detail in the body manuscript.</p> <p>4) Explain role of molecular scissors in perspective HBV treatment (CRISP-CAS9)</p> <p>5) If you don't delete the table about HCV therapy, please delete the sofsobuvir/ledipasvir treatment data, this treatment is no longer recommended according to the current EASL guidelines.</p> <p>My recommendation is major revision.</p> <p>(2)Reviewer 03475479: Authors reviewed previous information and recent advances in viral hepatitis A to E. This review included general and comprehensive findings. But novel findings are limited. Authors should describe unresolved issues more detail. Authors should mention also about extrahepatic manifestation in these hepatitis virus. Miss-spellings are found.</p>	
65	64036	03742189	Grade E (Do not publish)	Grade A (Priority publishing)	<p>(1)Reviewer 03742189: Your team tried to find the role of preoperative NLR in prediction of recurrence risk in GIST patients, using a meta-analysis. Final 22 articles were included and you get a conclusion that an increased preoperative NLR was associated with a higher risk of recurrence in patients with GIST. In addition to already mentioned several limitations of your study in your manuscript. another several limitations are present.</p> <p>1. The regional and racial bias should be considered.</p> <p>2. GIST datas are insufficient to evaluate the risk groups. The tumor size is mostly around 5cm; No mitotic count; No primary tumor site.</p>	
66	64041	00503175, 05821532	Grade C (Good), Grade B (Very good)	Grade B (Minor language polishing), Grade A (Priority publishing)	<p>(1)Reviewer 00503175: According to me authors correctly perform this mini-review article.</p> <p>(2)Reviewer 05821532: Hyperkalemia is a potentially life-threatening complication including heart transplantation, in fact, it is well studied and managed in most cases, authors review it for heart transplantation and address it, it is good for clinicians. But for a review, it seems a little concise.</p>	
67	64046	03477174	Grade B	Grade B	(1)Reviewer 03477174: The article is well prepared, I have no additional	

			(Very good)	(Minor language polishing)	suggestions.	
68	64048	00504362, 05121477, 04031726	Grade B (Very good), Grade B (Very good), Grade C (Good)	Grade B (Minor language polishing), Grade B (Minor language polishing)	<p>(1)Reviewer 00504362: This is a very interesting manuscript, showing important data to compare with other experiences world-wide. This pandemic not only has collapsed the health systems anywhere, but also other important points such as the perioperative morbidity and mortality in both asymptomatic or symptomatic COVID-19 must be be considered and analyzed as priorities. Urgent or emergent surgeries should continue despite adversity, not only as a health problem but also with an important ethical component. Therefore, this reviewer considered that this kind of communications are welcome, and should be share with medical communities.</p> <p>(2)Reviewer 05121477: The content is substantial and can be discussed in further depth</p> <p>(3)Reviewer 04031726: This is a literature review discussing the recent factors associated with surgical complications and mortality in COVID-19 patients.</p> <p>Some considerations:</p> <p>1) Abstract:</p> <p>- Highly repetitive. Only "Covid 19" was spoken 6 times, and we need to improve it.</p> <p>- This is a copy of the introduction. Please do not do that.</p> <p>2) The article is very repetitive and is “tired” reading. I suggest improving your writing.</p> <p>3) The authors did not read the submission instructions. The Article is completely outside the standards required by the WJGS. Please respect the invitation sent</p>	

					and resubmit following the rules.	
					4) In the topic: "Preadmission and admission considerations in COVID-19 pandemic" I recommend the discussion with the article PMID: 327855700	
69	64051	03022002, 05402068	Grade C (Good), Grade C (Good)	Grade B (Minor language polishing), Grade C (A great deal of language polishing)	<p>(1)Reviewer 03022002: This research indicates that changes of GAS6 plasma levels found to be correlated positively with the changes of colorectal cancer markers through evaluating the plasma GAS6 levels in colon cancer patients. According to the results GAS6 may be a good indicator for the evaluation of the response in the treatment of colon cancer. In general, this is a somewhat innovative study, but there are also some minor problems.</p> <p>1.Please keep the writing format consistent, such as: GAS6 or GAS 6; Growth ArrestSpecific 6 or Growth arrest-specific6.</p> <p>2.There are some minor errors in the writing of the article, especially the use of spaces and paragraph alignment.</p> <p>3.Please pay attention to the uniformity of the reference format.</p> <p>4.This research is relatively simple and plain, and needs more in-depth study.</p> <p>(2)Reviewer 05402068: It is interesting to know hat plasma GAS6 levels were lower in cancer group than control group and were elevated after surgery, which means this can be a potential marker for diagnosis and monitoring. However, the manuscript itself need revision, especially the discussion section which is too simple. Also I would suggest authors to polish the language of the manuscript.</p>	
70	64238	03501077, 05758726	Grade C (Good), Grade B (Very good)	Grade B (Minor language polishing), Grade B (Minor language polishing)	<p>(1)Reviewer 03501077: The paper is well written and the theme is clinically relevant.</p> <p>Statistical analyses have been well conducted</p> <p>My most significant concern is the very low homogeneity of patient population, being several studies evaluated together.</p> <p>I think Authors should clarify inclusion and exclusion criteria of the considered studies.</p> <p>(2)Reviewer 05758726: 1. The autors made a PMC search to find recent trend of</p>	

					<p>patients with prior liver transplantation and subsequent immunotherapy. They conclude rejection risk is the major obstacle to immunotherapy use.</p> <p>2. The level of description is very good with some exceptions (below).</p> <p>3-1. The study include 9 patients with melanoma, therefore the title "Immunotherapy for Post-Liver Transplant Hepatocellular Carcinoma Recurrence: Where are we now?" is innapropriate.</p> <p>3-2. The authors should include the key words in PubMed Search so that the reader can reconfirm.</p> <p>3-3. Facing the immune-related adverse event, we often continue IO-drug with corticosteroids. That is, immunosuppression and immunostimulation are performed at the same time. It will be helpful to the reader to describe the mechanism at that time.</p> <p>3-4. Despite the description of research limitations, the authors addressed figures of a comparison of two groups with different backgrounds (Pembro.'s PFS and OS are significantly better than Nibo.). It may be misleading to the reader, so it is better to delete the figures and keep it as a reference in the text, or to make a comparison using propensity score matching.</p>	
71	64271	04279936, 03549625, 03122873	Grade D (Fair), Grade E (Do not publish), Grade A (Excellent)	Grade B (Minor language polishing), Grade D (Rejection), Grade A (Priority publishing)	<p>(1)Reviewer 04279936: The subject can serve as an approach to understand the effects related to the risk of resumption of graft after ACL reconstitution and lead to a better diagnosis in order to optimize the surgical acts for a safe return to sports activities.</p> <p>In this work, the authors first focus on the quantification of risk factors such as the effects of demographic, anatomical and environmental factors. They then proceed to a critical analysis of the surgical procedures and the consequences of errors made on athlete activity while inferring that despite significant biomechanical data, the protective effects on ACL reconstruction have yet to be clinically demonstrated. The Quadriceps Tendon (QT) way as well the Bone-patellar tendon-bone (BPTB) autograft technique takes a place in the discussion by shedding light on the lack of long-term trialson and on the limitations related to the donor site morbidity, patellar fractures risk, extension deficit and the fixed graft length that may result in graft-tunnel mismatch.</p>	

					<p>According to the authors, the final decision to authorize RTS is a psychological and partial cross-parameter. It should strike a good balance between the athlete's desire to savor the playing field and the risk of injury again.</p> <p>However, below you will find some comments to improve, in my opinion, the overall content of the article to make it suitable.</p> <ol style="list-style-type: none"> 1. Review the optimization of the numbering of references in the text as there must be confusion on both their placement and their follow-up. 2. The given background is based, in part, on works and references which are no longer relevant for many days (e.g. Ref [143]). 3. The manuscript describes a limited manner the surveys. 4. Anatomical factors sentence: <ul style="list-style-type: none"> • Introduce a schematic illustration to explain the intercondylar notch characteristics • Idem for the lateral femoral condylar offset <p>In conclusion, I give an unfavorable opinion and I not recommend its publication.</p> <p>(2)Reviewer 03549625: The purpose of the paper has certain value for clinical treatment. The authors reviewed more than 250 papers for this review. However, the evidence level of the paper is relatively low due to the method used for analysis and the conclusion. I am sorry that this work does not meet the criteria for publication.</p> <p>(3)Reviewer 03122873: It is an informative review basing on extensive literature search. Congratulations. Nevertheless, the conclusion is far too short. Authors could briefly summarize their objective observations resulting from their literature review: which areas of vulnerability deserve more attention etc.</p>	
72	64278	03721849	Grade B (Very good)	Grade A (Priority publishing)	<p>(1)Reviewer 03721849: 1 Title. Does the title reflect the main subject/hypothesis of the manuscript? Yes</p> <p>2 Abstract. Does the abstract summarize and reflect the work described in the manuscript?</p>	

				<p>Yes</p> <p>3 Key words. Do the key words reflect the focus of the manuscript?</p> <p>Yes</p> <p>4 Background. Does the manuscript adequately describe the background, present status and significance of the study?</p> <p>Yes</p> <p>5 Methods. Does the manuscript describe methods (e.g., experiments, data analysis, surveys, and clinical trials, etc.) in adequate detail?</p> <p>N/A</p> <p>6 Results. Are the research objectives achieved by the experiments used in this study? What are the contributions that the study has made for research progress in this field?</p> <p>N/A</p> <p>7 Discussion. Does the manuscript interpret the findings adequately and appropriately, highlighting the key points concisely, clearly and logically? Are the findings and their applicability/relevance to the literature stated in a clear and definite manner? Is the discussion accurate and does it discuss the paper's scientific significance and/or relevance to clinical practice sufficiently?</p> <p>Yes</p> <p>8 Illustrations and tables. Are the figures, diagrams and tables sufficient, good quality and appropriately illustrative of the paper contents? Do figures require labeling with arrows, asterisks etc., better legends?</p> <p>N/A</p> <p>9 Biostatistics. Does the manuscript meet the requirements of biostatistics?</p>	
--	--	--	--	--	--

				<p>N/A</p> <p>10 Units. Does the manuscript meet the requirements of use of SI units?</p> <p>Yes</p> <p>11 References. Does the manuscript cite appropriately the latest, important and authoritative references in the introduction and discussion sections? Does the author self-cite, omit, incorrectly cite and/or over-cite references?</p> <p>Yes</p> <p>12 Quality of manuscript organization and presentation. Is the manuscript well, concisely and coherently organized and presented? Is the style, language and grammar accurate and appropriate?</p> <p>Yes</p> <p>13 Research methods and reporting. Authors should have prepared their manuscripts according to manuscript type and the appropriate categories, as follows: (1) CARE Checklist (2013) - Case report; (2) CONSORT 2010 Statement - Clinical Trials study, Prospective study, Randomized Controlled trial, Randomized Clinical trial; (3) PRISMA 2009 Checklist - Evidence-Based Medicine, Systematic review, Meta-Analysis; (4) STROBE Statement - Case Control study, Observational study, Retrospective Cohort study; and (5) The ARRIVE Guidelines - Basic study. Did the author prepare the manuscript according to the appropriate research methods and reporting?</p> <p>N/A</p> <p>14 Ethics statements. For all manuscripts involving human studies and/or animal experiments, author(s) must submit the related formal ethics documents that were reviewed and approved by their local ethical review committee. Did the manuscript meet the requirements of ethics?</p> <p>N/A</p>	
--	--	--	--	--	--

					<p>Specific Comments To Authors:</p> <p>I read with great interest the editorial entitled "Current state of medical tourism involving liver transplantation - The risk of infections and potential complications" and submitted to the World Journal of Transplantation. The manuscript is well-written and discuss an important issue about transplantation tourism, what is not well reported in the previous literature.</p> <p>I would only suggest to shorten a the text avoiding unnecessary paragraphs (like paragraphs 1 and 2, for example).</p>	
73	64286	01221666, 05467547	Grade D (Fair), Grade C (Good)	Grade B (Minor language polishing), Grade B (Minor language polishing)	<p>(1)Reviewer 01221666: This case report illustrates the course of a complication of hepatic vein puncture-through which occurs during percutaneous transhepatoc procedure for tube exchanging and conservatively managed successfully.</p> <p>Comments</p> <p>1. The vital signs other than temperature right at the fever episode (40 degrees) in the section of "Treatment" is missing. Did the patient experience shock? Did the patient have hemobilia?</p> <p>2. The authors suggested that fever was caused by bile flux into hepatic vein but if it happened, hemoglobin should not be as low as 75 g/L. Please explain.</p> <p>3. TAE is indicated for treatment of hemobilia by puncturing through intrahepatic artery. What about uncontrolled venous hemorrhage in this case? Is there any aggressive treatment option for biliary-venous fistula?</p> <p>4. The conclusion is that complication can be reduced under guidance of ultrasound and fluoroscopy but it seems that this case is already doing under the guidance. Does the conclusion support the case report?</p> <p>5. This case report of complication is supported by National funding? Is it justified?</p> <p>(2)Reviewer 05467547: The PTBD stand for should be describe in its first appearance</p> <p>The phrase "high cost performance ratio" regarding the ultrasound in discussion heading, should be described more clearly. It could be compare with the</p>	

					fluoroscopy cost.	
74	64289	04761604	Grade E (Do not publish)	Grade D (Rejection)	<p>(1)Reviewer 04761604: I thank the authors for their research effort. Although the topic is interesting, I could not recommend it for publication due to the reasons listed below:</p> <ol style="list-style-type: none"> 1. Writing quality of the entire manuscript is poor. 2. The study lacks methodological rigor 3. Both in the abstract and introduction, the rationale and background information were inadequate; it couldn't convince me why the study should be done. <p>Thanks.</p>	
75	64290	02895728, 03490863	Grade B (Very good), Grade D (Fair)	Grade A (Priority publishing) , Grade A (Priority publishing)	<p>(1)Reviewer 02895728: This review is important as it describes an addition to glycemic, Blood pressure control and use of ACE inhibitor/ ARB in Diabetic kidney disease.</p> <p>I have some suggestions</p> <p>Differences between Finerenone and other MRA can be mentioned especially its effect on inflammation and fibrosis</p> <p>Since FIDELIO DKD is the main trial for basis of this article dissection of composite outcome and main outcomes responsible for the conclusion can be given. At what time interval was beneficial effect seen.</p> <p>Description of adverse event like hyperkalemia can be more especially how many required potassium binding resins to continue this drug</p> <p>Finally authors could give a statement about whether in their opinion it is worthwhile to use this drug for its extra 3.4% benefit considering the real problem of hyperkalemia</p> <p>(2)Reviewer 03490863: In this manuscript, S Veneti and K Tziomalos provide a summary of finerenone in diabetic kidney disease. The review is written concisely and clearly, but in fact it is a mini-review. Several review articles on this issue, including the systematic one, have been published in the past two years. These reviews consider the problem in much more detail. Thus, in order to be competitive, it should be recommended to expand the manuscript providing more detailed information on the renal effects of finerenone, comparisons with</p>	

					other drugs in this class, etc. For readability, it is recommended to accompany the manuscript with figures and tables.	
76	64297	00503405, 05272486	Grade B (Very good), Grade A (Excellent)	Grade B (Minor language polishing), Grade A (Priority publishing)	<p>(1)Reviewer 00503405: In the original article of Cococcia et al. the authors aimed to validate an Italian translation of the IBD perceived stigma scale (PSS) in relation to patients' resilience.Since there are very limited data regarding perceived stigma in IBD, and no validated translation of the PSS scale into Italian is available, moreover, perceived stigma in Italian IBD patients has never been assessed the study is of great clinical importance.</p> <p>The validation process of the original PSS score is well designed and well presented, the involved patients' characteristics are representative regarding the main aims of the study.</p> <p>Also, to fit the translated questionnaire according to cultural features is also an important step.</p> <p>They found a moderate level of resilience in their cohort, also they shown for the first time that, in IBD patients, higher levels of resilience correlate with lower levels of perceived stigma and, to better QoL.</p> <p>The discussion is correct, their results, the boundaries of the study, are discussed appropriately and moderately.</p> <p>Minor comment:</p> <p>I suggest to include the original and Italian translation of the PSS scale into the manuscript as a supplemental data.</p> <p>After this minor correction, I definiety suggest to accept the manuscript for publication in WJG.</p> <p>(2)Reviewer 05272486: This in an interesting well written original research article on an often neglected topic in IBD patients.</p> <p>I have some minor comments, mainly regarding the study population</p> <ul style="list-style-type: none"> - 157 IBD patients were included but it is not stated in the article how this number was reached, how many patients were screened and how? Is this a random sample of the total IBD population followed at this hospital? - Is it known why the UC population is significantly older and has significantly more severe disease? 	

					<p>- I am a bit surprised by the high percentage of UC patients' that had previous abdominal surgery, especially since only 4% of patients has a pouch. I would explain this in the introduction as well.</p> <p>"Our findings suggest that downstream public health intervention that focus on patients' resilience may reduce the level of perceived stigma and consequently the patients' QoL. " I would advice to rephrase this sentence, I think the authors mean a health intervention will increase the patients' QoL and not reduce it.</p> <p>- Will the scales itself be included in the final article?</p>	
77	64300	03051573	Grade E (Do not publish)	Grade B (Minor language polishing)	(1)Reviewer 03051573: This manuscript intends to review the link between epigenetics and ferroptosis. There is a substantial lack of direct correlation between the described references and both processes. The manuscript described the different mechanisms underlying the epigenetic regulation of genes and described some of the epigenetic changes reported in cancer cells c. Some of the genes are sometimes proteins involved in ferroptosis. Still, it is not clear based on the manuscript that this regulation could lead/modulate to the activation or inhibition of ferroptosis.	
78	64307	02446483, 00504192, 03479389, 03478568	Grade C (Good), Grade C (Good), Grade D (Fair), Grade D (Fair)	Grade C (A great deal of language polishing), Grade A (Priority publishing) , Grade B (Minor language polishing), Grade A	<p>(1)Reviewer 02446483: The authors review the association of acute pancreatitis (AP) and COVID-19. The authors suggest that AP may be a consequence of direct pancreatic damage by the virus, because pancreatic acinar cells contain ACE2 receptor proteins. They argue that SARS-CoV-2 can bind to these receptors causing pancreatic injury. The review is comprehensive of case reports and case series affecting patients with AP and COVID-19. The English is not satisfactory, because the construction of some sentences is awkward and some grammar mistakes are still present. The manuscript should go through a professional English editing.</p> <p>(2)Reviewer 00504192: The paper is an interesting review about the association of Corona Virus-19 infection and clinical development of acute pancreatitis. The Authors carefully analyze several fields of the topic. The selection of the literature is well done and complete.</p>	

				(Priority publishing)	<p>Bibliographical references are recent and well selected.</p> <p>The importance of the paper and the significance of its contents are relevant because of the big interest and clinical impact of the CoViD-19 issue.</p> <p>(3)Reviewer 03479389: This is a review summarizing the association between COVID-19 infection and acute pancreatitis. This paper listed case reports. It is necessary to indicate the age characteristics, the period from the onset of COVID-19 infection to acute pancreatitis, the severity of pancreatitis, the treatment method and the prognosis in a table.</p> <p>(4)Reviewer 03478568: Basically, the topic about the relationship between COVID-19 and AP is interesting and has practical significance. But the literature study is too primary. It is better to conduct active analysis in details based on the data from individual case reports and cohort studies. Otherwise it is difficult to get a clear conclusion. In addition, the future works and the limitation of this studies were not mentioned.</p>	
79	64311	03486791, 05194798, 03478404, 05382551, 05230210	Grade B (Very good), Grade D (Fair), Grade C (Good), Grade C (Good), Grade B (Very good)	Grade B (Minor language polishing), Grade B (Minor language polishing), Grade B (Minor language polishing), Grade B (Minor language polishing)	<p>(1)Reviewer 03486791: This is an interesting topic.</p> <p>(2)Reviewer 05194798: This manuscript is a retrospective cohort study that evaluated an association between endoscopic intervention and subsequent gastrointestinal bleeding (GIB) in patients with left ventricular assist devices (LVAD) using multivariate logistic regression. The authors found that GIB resolved on its own by discharge in 70% of all 235 encounters, while recurrent GIB occurred in the majority of patients. Furthermore, the authors showed that endoscopic interventions did not significantly decrease the odds of subsequent GIB in patients with LVAD.</p> <p>This study was conducted well, and the methods are appropriate. The topic of this study was unique and interesting, and the results will be of interest to clinicians in the field.</p> <p>However, the following major and minor issues require clarification:</p> <p>Major</p> <p>1. In some guidelines for obscure GI bleeding, a high-resolution</p>	

			<p>Grade A (Priority publishing)</p> <p>contrast-enhanced CT scan is recommended as the first step in the diagnosis of a patient to detect abnormalities in the intramural and extramural structures as well as extra-intestinal lesions. The authors should include the indication and the data of CT scan, and discuss the availability of CT scan as well.</p> <p>2. The authors should also evaluate factors associated with rebleeding, based on general condition, underlying cardiac disease, and cardiac function.</p> <p>Minor</p> <p>1. Please describe possible causal relationship between LVADs and GI bleeding in Introduction section.</p> <p>2. (P8L19) Please describe other overt causes in more detail.</p> <p>3. (P8L20) Please add the range of hemoglobin level.</p> <p>4. (P10L7) Why was the total number of encounters which the source was identified in not 9 but 10?</p> <p>5. (P15L8-9) The authors should correct this sentence as “no significant difference” doesn’t necessarily mean “same as”.</p> <p>6. I recommend the authors also investigate the association between rebleeding and proton pump inhibitor administration for prevention of upper GI bleeding.</p> <p>7. (Table 2) The authors should show the data of “culprit lesion”, followed by those of “type of intervention”.</p> <p>8. Please use flowcharts for overviewing the flow of the patients.</p> <p>(3)Reviewer 03478404: In this manuscript, the authors showed, after an in-deep analysis of 295 patients with LVAD and 238 hospital encounters, that a high proportion of gastrointestinal bleeding stopped without endoscopic therapy and that endoscopic intervention did not prevent subsequent bleeding. Even given the retrospective nature of the study, the overall findings appear very useful for practice and they prompt for prospective studies on this topic. The manuscript is written in an elegant manner, easily to be followed. The overall structure is respected and paragraphs are written in detail. There is a plethora of</p>	
--	--	--	---	--

				<p>results in this manuscript, which should be emphasized.</p> <p>Comments/suggestions:</p> <p>1. Title: I would suggest replacing “review” by “study”, as it was a study and not a review.</p> <p>2. Since this is the first study (powered enough) to evaluate whether endoscopic intervention reduces the risk for subsequent GIB or not, it should be mentioned both in the Abstract and Core tip, to emphasize the importance and impact of the research.</p> <p>3. Abstract: Please remove “using multivariate logistic regression” from “Aims” and add it to Methods. Also, the authors could include here all secondary aims of the study (i.e. – “describe GIB presentations and sources identified, and determine risk factors for recurrent GIB”), as they included the frequency of GIB (and this is also a secondary aim). Results could also include ALL the corresponding findings, which are of interest. I would suggest correcting “other overt causes (21.4%)”, when describing “presentation of GIB”, as this is not about causes, but about type of bleeding (besides mentioned melena and occult bleeding). Sources of bleeding could be inserted, as written above, as well as risk factors for recurrent GIB.</p> <p>4. Materials and Methods: Study population – please mention the place where the study was performed (not only “at our large academic institution”); please state clearly that 319 patients were found with LVADs (or with other VAD too). Please describe BIVAD, before using the abbreviation. Instead of writing “type of LVAD implantation”, the authors should mention “type of device implantation” (as there were also BiVAD and other types of interventions etc). In “Independent (exposure) and dependent (outcome) variables” – please write “For the secondary aims...”, as they were three.</p> <p>5. Results start with the “Frequency of GIB in patients with LVADs” paragraph. Although this has perfect logic, this is not the primary aim. Please rephrase and re-arrange according to the Aims. Please mention in detail what you mean by “other overt causes (21.4%)” - page 8 (this should not be about causes, but type of bleeding) - how many with hematemesis, hematochezia etc? Please rephrase</p>	
--	--	--	--	---	--

				<p>“Three patients expired during active GIB”. Table 1 – please insert after “sex” – Male. From Table 1, it would be important to mention in the main text - the significant difference of LVAD exposure (days) (IQR). The source of bleeding was detected in only 54.6% encounters, which is pretty low, given that “the median number of endoscopic procedures done per encounter was 2” and maximum 8, but the authors nicely addressed this issue in Discussion. Table 2 : Please delete « Sum of percentages is greater than 100% as some procedures involved multiple interventions. », as it is repeated twice. Also, please describe here more about “non-specific oozing” and “others”. Please also double check: in the main text is written: “An endoscopic intervention was performed in 34.8% (71/204) of encounters”, while Table 2 contains 72 lesions (if too lesions in 1 encounter, please mention). With endoscopic procedures, it is clear. Also, please correct the following: “Of 22 cases of recurrent bleeding when the prior GIB source was deep small bowel, the current source was also in the small bowel in 18; the other 4 encounters sourced the bleed in the duodenum.” since duodenum is also small bowel; please insert “deep” before small bowel above. Table 3 has to be corrected – “Had a subsequent GIB” = 97, and not the other way around. Otherwise, both columns are wrong. Please revise the entire Table.</p> <p>6. Discussion paragraph is scientifically well addressed, including the limitations of the study. Reference about Welden et al is missing – page 14 – please insert: “Welden CV, Truss W, McGwin G, Weber F, Peter S. Clinical Predictors for Repeat Hospitalizations in Left Ventricular Assist Device (LVAD) Patients With Gastrointestinal Bleeding. Gastroenterology Res. 2018 ;11(2):100-105. doi: 10.14740/gr972w. PMID: 29707076.</p> <p>7. Minor revision of the English language is required (grammar [verb use, concordance], syntax and punctuation - missing commas etc).</p> <p>8. There are no « Conflict-of-Interest Disclosure Form » and « Copyright License Agreement ». Contribution of the Authors is not written. There are no ORCID numbers of the other authors, except for the Corresponding Author. Please insert.</p> <p>9. The manuscript is not written according to the requested format of the WJG, including references. Please correct.</p>	
--	--	--	--	---	--

				<p>10. STROBE statement was not checked (page number not inserted), just added as a file.</p> <p>(4)Reviewer 05382551: The authors present a study on the association between endoscopic intervention and subsequent gastrointestinal bleeding using multivariate logistic regression.</p> <p>The article is well written and structured. The subject described is within the scope of the journal. His reading is fluent.</p> <p>The results presented constitute an important advance in the study area.</p> <p>(5)Reviewer 05230210: I would like to thank the authors for their important work.</p> <p>Notes on the manuscript are highlighted</p> <ul style="list-style-type: none">• Title: could the authors add BIVAD?• Abstract: why not present data in Hazards ratio or Risk ratio?, Instead of odds ratio.• Introduction: References for bleeding with LVAD could add the following: Baumann Kreuziger LM. Management of anticoagulation and antiplatelet therapy in patients with left ventricular assist devices. J Thromb Thrombolysis. 2015 Apr;39(3):337-44. doi: 10.1007/s11239-014-1162-6. PMID: 25549823• Study population:	
--	--	--	--	--	--

				<p>1- If you included BIVAD what is the percentage of patients? And why not mentioned in the title/abstract?</p> <p>2- "We excluded patients with temporary devices implanted (CentriMag, Thor BIVAD, Total Artificial Heart), leaving a total of 295 patients.">> so why not mention this in the abstract, that your included patients were permanently using the LVD.</p> <p>3- The authors mentioned:" VAD implantation until death," >>so why Kaplan Meier curve was not done, along with survival analysis?</p> <p>4- The authors stated that: "We classified the GIB presentation as overt versus occult". >>Could the authors clarify if they did tests for occult bleeding, or other lab assessment, and how they dealt with the issue that most tests will get false positive with anti-platelets as aspirin?</p> <p>5- the authors state " All encounters with procedures for non-bleeding related indications or iatrogenic bleeding were excluded from analysis." >>>Could the authors explore on that part?, as most of the GIB in LVAD is related to the anti-coagulation, and anti-platelet medications the patients receive as a protective measure from thrombosis.</p> <p>6- "power calculation:" this is a post hoc power calculation, and in my humble opinion this calculation carry a little value and better omitted, since this is a retrospective study and there is a debate about the usefulness in this case. Added to this; it is mainly used for comparison between two independent groups, and this is a one group cohort (LVAD patients) there is no control group. Reference: Zhang Y, Hedo R, Rivera A, et al. Post hoc power analysis: is it an informative and meaningful analysis?. General Psychiatry 2019;32:e100069. doi: 10.1136/gpsych-2019-100069</p>	
--	--	--	--	---	--

				<ul style="list-style-type: none">• Results: 1- there is no mention of octreotide or PPI use, as a control measure especially for patients who had delay in their endoscopic procedure for up to five days, and if these medical treatments ameliorated their condition or not. Also, no relation is mentioned between the type of medical intervention received and the time of stopping of bleeding? ref: Molina TL, Krisl JC, Donahue KR, Varnado S. Gastrointestinal Bleeding in Left Ventricular Assist Device: Octreotide and Other Treatment Modalities. ASAIO J. 2018 Jul/ Aug;64(4):433-439. doi: 10.1097/MAT.0000000000000758. PMID: 29406356. 2-could you replace "expired">> with "died" <ul style="list-style-type: none">• Discussion: 1- Why no medical measures was discussed as preventive measure for subsequent episodes? eg. mucosal coating medications as local measures to stop bleeding (sucralfate, antacids), PPI and H2 blockers, octreotide, etc. 2- the authors recommend " we propose that the VCE be performed urgently in the acute setting while awaiting normalization of the INR, and possible endoscopy.">> but could this be of benefit , if the patient is bleeding, which could mask the visualization of the lesion by digested or coagulated blood that attaches to the lens, best scenario will be 50% yield? and also cost effectiveness, please discuss. ref: Nadler M, Eliakim R. The role of capsule endoscopy in acute gastrointestinal bleeding. Therap Adv Gastroenterol. 2014 Mar;7(2):87-92. doi: 10.1177/1756283X13504727. PMID: 24587821; PMCID: PMC3903085. 3- The recommendation of the authors could be modified to >> prohibit "secondary diagnostic" endoscopic procedures, not primary (as diagnosis is	
--	--	--	--	---	--

					<p>important in the first episode).</p> <p>4- Another recommendation that the authors could discuss, if most of the lesions are angiodysplasia, would a baseline capsule endoscopy before or synchronously with the LVAD insertion be of prognostic benefit for those patients?</p> <p>• Tables and figures:</p> <p>1. It would be better if the type of analysis is mentioned at the table title (eg logistic regression, spearman correlation, etc).</p> <p>2. It is recommended to add a figure representing the survival or time to surgery in the cohort studied.</p>	
80	64313	03544596, 04671880	Grade B (Very good), Grade C (Good)	Grade A (Priority publishing), Grade B (Minor language polishing)	<p>(1)Reviewer 03544596: Thanks to the authors for the extensive review about IBS and probiotics.</p> <p>The manuscript cites appropriately the latest and important references.</p> <p>I do not have any criticism.</p> <p>(2)Reviewer 04671880: This manuscript is an opinion review that well focuses on the statu quo of the overall research in the field of probiotic and IBS. The Authors highlight the most important research topics, the problems that have now been resolved and remain to be resolved, and the future research directions that may maximize the practical impact on the field of IBS and probiotics. The manuscript is an essay in which the author discusses this core issue in a reasonable and thorough manner.</p> <p>In the core tip the Authors suggest the need to design a clinical study where the IBS subgroups are initially defined to understand if all IBS subtypes are treatable with the same probiotic or combination of probiotics.</p> <p>In the conclusion set however The Authors seem to to suggest that such a type of study is quite not feasible at the moment.</p> <p>May be this issue could be more deeply discussed to avoid denying what the core tip stated.</p>	

