OPEN PEER REVIEW

Open Peer Review (OPR)

As a new feature for progressing towards transparency, we decided to open a new window for all of our editors as well as authors titled "**Open Peer Review**". We hope by this new facility, our reviewers will be more motivated and authors will be more satisfied with the review process. We believe that publishing our peer review reports could make a transparent and clear environment for all our efforts within a journal, but not all reviewers tend to publish their comments.

What is "Open Peer Review" process?

An "Open Peer Review" process is making the details of all review process (including reviewers, associate editors, and EICs comments) as "Public" as it is agreed by EIC, Authors, and reviewers.

Advantages of "Open Peer Review" process

- More transparency, constructiveness and tactful comments of the peer review process: leads to an increase in the quality of reviews
- More motivations for all involved roles in the review process
- Authors' satisfactions from the review process: Increases honesty between authors and reviewers
- Education of both authors and new students
- Prevents reviewers from following their individual agendas and leading to the detection of reviewers' conflicts of interests

You can find out more at:
https://sites.kowsarpub.com/kowsar/knowledgebase/category/tree.html#opr.html

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Peer Review Report for "Immunoglobulin G Immunity to Hepatitis A Virus in Liver Transplant Candidates: A Serosurvey From Iran"

Author(s): Kamran B Lankarani, Behnam Honarvar, Mohammad Hassan Zahedroozegar, Alireza Dehghan, Mohammad Reza Rouhezamin, Mojdeh Khorrami, Saeid Amiri Zadeh Fard, Vahid Seifi, Bita Geramizadeh, Heshmatollah Salahi, Saman Nikeghbalian, Alireza Shamsaeefar, Sayed Ali Malek-Hosseini

Review Timeline:

Submit Date: 17 Jan 2021
Revised Date: 21 Mar 2021
Accepted Date: 22 Mar 2021

Revision (0)

Here, you can see the **Reviewers**, **Associate Editors** and **EICs'** comments from the beginning to the end of the revision process.

Heidar Sharafi: Reviewer | **Revision** (0) 31 Jan 2021

Dear AE,

I reviewed the manuscript "Chronic Immunity to Hepatitis A Virus in Liver Transplantation Candidates: A Serosurvey from Iran". The study results are interesting and can contribute in making policy regarding HAV prevention in LT candidates. I have few comments:

1. I recommend authors to add 95% CI for prevalence of HAV IgG in abstract and manuscript results sections.

2. The presented results on seroprevalence of HAV by "water pipe smoking" is confusing. In Table 2, those without water pipe smoking had higher prevalence of HAV IgG than those with water pipe smoking (94.1% vs. 78.8%). While, this means water pipe smoking has inverse association with HAV seropositivity and should have an OR less than 1, the presented OR is 4.32 and water pipe smoking has been presented as a risk for HAV infection in univariate and multivariate analyses.

3. Why more than half of the cases were dropped from the association analysis of autoimmune hepatitis and HAV seropositivity in Table 2?

Regards

REFEREE: Reviewer | **Revision** (0) 1 Feb 2021

Dear editor
Thanks for your invitation. Below you can find my comments:
Abstract
1- I think that results for univariate analysis can be removed from this section.
Introduction
2- Authors briefly reported that we have no previous related research in Iran and mentioned the necessity of their research. Hence, I have no comment on this section.
Methods
3- Settings, type of study, sampling method, ethics, and data analysis has been well presented.
Results
4- Authors should add the odds ratio for multivariable analysis (logistic regression) in the text together with P-value.
5- Figure 2 can be deleted.
Discussion
6- Authors should compare their results with other similar studies in LT patients.
References
7- More updated references should be used.
General Comments:
8- The paper should be revised by a professional language editor.
9- I think the paper should be reformatted as a brief report.

Mohammad Hossein Khosravi: Reviewer | **Revision** (0) 14 Feb 2021

Dear Editor
Thanks a lot for inviting me as the reviewer of the present manuscript, a cross-sectional study in which the authors have tried to provide a report of HAV immunity in Iranian LT patients. This is a really valuable job and I appreciate the effort put to this job by authors. however, there are some points which should be considered.
First, the term "chronic immunity" is not a precise and scientific term. It should be replaced with "Immunity" alone or "IgG serum level" throughout the manuscript.
Another point is the design of this study. It is evident that this a prevalence study and cross-sectional is the most fit design for it. In such a study, sampling method should be concensus as the authors themselves have mentioned it later. So calculation of sample size means that they have limited the samples to a specific number. while, in a cross-sectional design, you should act regarding to a precise time period and include all the samples which are available during this period. I wonder if the author have included all the available patients in their 5-month period of study.
In addition, conclusion section (especially in abstract) is not compatible with the findings.

Why should we suggest vaccination and impose additional financial burden to healthcare system while we have an immunity rate of 92.4%? Do vaccines act better?
Also authors should reconsider the keywords as they are not suitable for indexing.

Best regards,
Reviewer

REFEREE: Associate Editor | **Revision** (0) 14 Feb 2021

Dear Editor-in-Chief
Thanks for inviting me as associate editor to handle the manuscript NO.113001 entitled "Chronic Immunity to Hepatitis A Virus in Liver Transplantation Candidates: A Serosurvey from Iran".
I appreciate the authors' work to collect and report these important data. The subject is timely and novel. According to the reviewers' comments, the manuscript need minor revision before publishing. See the reviewers' comments attached.

Regards
AE

Seyed Moayed Alavian: EIC | **Revision** (0) 14 Feb 2021

Dear Authors,

Thanks for submitting your manuscript in Hepatitis Monthly. Your manuscript can be considered for publication after receiving the revision.

Regards,
EIC

OPEN PEER REVIEW

Revision (1)

Reply to Reviewers

Ideally, the reviewing process can significantly improve the submitted manuscripts by allowing the authors to take into account the advice of reviewers. Author(s) must reply to all reviewers' comments in a separate Word file, point by point. A "**Reply to Reviewers**" document is submitted along with revised manuscript during submission of revised files, summarizing the changes that the authors made in response to the reviewers' comments. The responses to reviewers' comments specifies how the authors addressed each comment the reviewers made.

You can read the authors' responses to the reviewers' comments in the next page.

Dear EIC and Dear Reviewers

Thank you for reviewing this article and your time spending. We read the comments precisely and our responses will be presented as below:

Reply to reviewers :

Reviewer 1

Abstract

-C1- I think that results for univariate analysis can be removed from this section.

- Response to Comment 1: Univarate analysis findings were removed from the abstract.

Introduction

-C2- Authors briefly reported that we have no previous related research in Iran and mentioned the necessity of their research. Hence, I have no comment on this section.

- Response to Comment 2: No comments.

Methods

-C3- Settings, type of study, sampling method, ethics, and data analysis has been well presented.
-Response to comment 3: No comments.

Results

-C4- Authors should add the odds ratio for multivariable analysis (logistic regression) in the text together with P-value.

-Response to comment 4: odds ratio and Confidence interval were added

-C5- Figure 2 can be deleted.

-Response to comment 5: Results of multivariate analysis were transferred from figure 2 to the body of manuscript and this figure was deleted.

Discussion

C6- Authors should compare their results with other similar studies in LT patients.
Reponses to comment 6: As it was explained in discussion part; lines number (115-116) , no similar literature in Liver transplant candidate was found, so we compared the results with studies which were conducted in patients with chronic liver disease.

References
C7- More updated references should be used.

Response to comment 7: We performed a comprehensive review of literatures and included all related and credible references in this manuscript.

General Comments:
C8- The paper should be revised by a professional language editor.

Response to comment 8: it was re-editted .

C9- I think the paper should be reformatted as a brief report.

Response to comment 9: This time-consuming and nearly novel study was considered by authors to be published as original article and was it not submitted in another journal before submitting in this Journal.

Reviewer 2

-C1-First, the term "chronic immunity" is not a precise and scientific term. It should be replaced with "Immunity" alone or "IgG serum level" throughout the manuscript.

-Response to comment 1: chronic immunity was changed to HAV-IgG immunity throughout the article.

-C2- Another point is the design of this study. It is evident that this a prevalence study and cross-sectional is the most fit design for it. In such a study, sampling method should be consensus as the authors themselves have mentioned it later. So, calculation of sample size means that they have limited the samples to a specific number. while, in a cross-sectional design, you should act regarding to a precise time period and include all the samples which are available during this period. I wonder if the author has included all the available patients in their 5-month period of study.

Response to comment 2: As it was mentioned in method part (lines 32-33),” we used the convenient type (census) of patients' recruitment in this study due to logistic difficulties in access to the patients. A three-person team, including one physician, one health technician, and a laboratory expert, gathered the data from all patients in a period of five months”. It means that the better form of sampling could be a random type of sampling during a longer period of time but as we mentioned due to logistic shortages and poor condition of referred patients from different parts of Iran, we were forced to have a census type of sampling during a shorter time (5 months).

-C3- ***In addition, conclusion section (especially in abstract) is not compatible with the findings. Why should we suggest vaccination and impose additional financial burden to healthcare system while we have an immunity rate of 92.4%? Do vaccines act better?***

-Response to comment 3: Considering that about 7000 liver transplantation has been done in Iran, several thousands are on waiting list of transplantation and evidences about high mortality of LT candidates who were infected by HAV(due to decreasing immunity), therefore as WHO also

recommended, it seems that by proper selection of patients and HAV immunization in them, related mortality and its huge costs could be reduced. Secondly, as we mentioned in introduction (line14-15) the HAV immune status of these patients maybe transient and become vanished due to immuno-compromise status.

-C4-Also authors should reconsider the keywords as they are not suitable for indexing.

-Response to comment 4: we reconsidered some of them

Reviewer 3

-C1*. I recommend authors to add 95% CI for prevalence of HAV IgG in abstract and manuscript results sections.*

-Response to comment 1: *Cl 95 % for Ig G were added to abstract and result*

-C2. The presented results on seroprevalence of HAV by "water pipe smoking" is confusing. In Table 2, those without water pipe smoking had higher prevalence of HAV IgG than those with water pipe smoking (94.1% vs. 78.8%). While, this means water pipe smoking has inverse association with HAV seropositivity and should have an OR less than 1, the presented OR is 4.32 and water pipe smoking has been presented as a risk for HAV infection in univariate and multivariate analyses*.*

-Response to comment 2: Thank you, you are right. we rechecked and rewrote related sections.

-C3. Why more than half of the cases were dropped from the association analysis of autoimmune hepatitis and HAV seropositivity in Table 2?

-Reponses to comment 3: At the time of conducting this study, only causes of nearly half of the ESLD patients were diagnosed so these data were only included.

Thanks again for yours constructive comments.

OPEN PEER REVIEW

Revision (1)

Here, you can see the **Reviewers**, **Associate Editors** and **EICs'** comments from the beginning to the end of the revision process.

Hamidreza Karimi-Sari: Associate Editor | **Revision** (1) 13 Mar 2021

Dear Chief Editor
Thanks for assigning me to handle the revised version of manuscript NO.113001 entitled "Ig G Immunity to Hepatitis A Virus in Liver Transplantation Candidates: A Serosurvey from Iran"

I appreciate the authors' work to revise their manuscript. All of the reviewers' comments are addressed in this revision. I have only three minor comments.

1- This revision of manuscript does not have an abstract. Please include your abstract again.

2- Please use the "immunoglobin G" instead of "IgG" in the title.

3- The figure.1 is not necessary. Please remove this figure and mention the proportion of "Etiologies of Liver Disease of Liver Transplant Candidates" in the results' text (both frequency and percentage for each etiology).

Regards
Associate editor

Seyed Moayed Alavian: EIC | **Revision** (1) 13 Mar 2021

Dear Authors,

Associate editor has asked for more minor corrections. Please submit a revision.

Regards,
EIC

OPEN PEER REVIEW

Revision (2)

Reply to Reviewers

Ideally, the reviewing process can significantly improve the submitted manuscripts by allowing the authors to take into account the advice of reviewers. Author(s) must reply to all reviewers' comments in a separate Word file, point by point. A "**Reply to Reviewers**" document is submitted along with revised manuscript during submission of revised files, summarizing the changes that the authors made in response to the reviewers' comments. The responses to reviewers' comments specifies how the authors addressed each comment the reviewers made.

You can read the authors' responses to the reviewers' comments in the next page.

Dear EIC, editors and reviewers

Thank you for reviewing this article and your time spending. We read the comments precisely and our responses will be presented as below:

Associate Editor's Comments:

Associate Editor 1:

C1- This revision of manuscript does not have an abstract. Please include your abstract again.

A-1Abstract was included in new revision manuscript as you said.

C2- Please use the "immunoglobin G" instead of "IgG" in the title.

A-2 IgG replaced with immunoglobin G as you recommended

C3- The figure.1 is not necessary. Please remove this figure and mention the proportion of "Etiologies of Liver Disease of Liver Transplant Candidates" in the results' text (both frequency and percentage for each etiology).

A-3 We removed figure 1 as you said and all frequencies and percentages of etiologies were added to manuscript.

Also, I should note that new changes highlighted with purple color and previous reviews changes have green highlights.

With regards
corresponding author

OPEN PEER REVIEW

Revision (2)

Here, you can see the **Reviewers**, **Associate Editors** and **EICs'** comments from the beginning to the end of the revision process.

Seyed Moayed Alavian: EIC | **Revision** (2) 22 Mar 2021

Dear Authors,

I am delighted to inform you that your manuscript is accepted for publication.

Regards,
EIC