Answers to Reviewer's comments

Reviewer 1:

Q1 Please indicate the p-value with "=" "<" or ">" in the results section.

Thank you for mentioning this. Yes, we have made the appropriate changes.

Q2. The objective is not very clear; I suggested rewriting it.

We have rewritten the objective in such a manner that it aligns with the results of the study and our conclusion as follows," We plan to investigate further the mechanisms underlying SARS-CoV-2 infection-induced hyperglycemia, particularly the rationale of cytokine-induced hyperglycemia hypothesis by evaluating the association between inflammatory markers and new onset hyperglycemia in non-diabetic patients with Covid-19 infection".

Q3. Explain more extension on the results "Additional analysis showed significantly higher mortality (24.2%vs.9.1%, p-0.001; OR-2.528, p-0.024)".

We have added further statistical information on the prognostic indices that we assessed as below, "We further analyzed the prognostic indices like mortality rate and length of stay between the two groups. There was significantly higher mortality (24.2% vs. 9.1%, p=0.001) and length of stay (8.89 days vs. 6.69 days, p=0.026) in patients with hyperglycemia compared to patients with normoglycemia. Further analysis with binary logistic regression shows an increased risk of mortality in patients with hyperglycemia (OR=2.528, p= 0.024). "

Q4. Suggested finding more correlation between 1) Hyperglycemia and COVID-19 infection and 2) COVID-19 infection and association with inflammatory markers.

More discussion and relevant literature on the association of hyperglycemia & COVID-19 infection and Inflammatory markers and COVID-19 disease have been added to manuscript. Thank you for the valuable suggestion.

Q5. For the binary logistic regression analysis to predict Hyperglycemia, there was no difference in LDH levels between the two groups (OR-1.623,p-0.256); any more remarkable discovery from the two groups. If yes, please explain in the study -results section.

We also found an interesting observation based on remdesivir administration between the two groups that we have discussed as below, "The administration of remdesivir initially showed a statistically significant difference in patients with hyperglycemia (59.7% vs. 44.6%, p=0.04) but did not show any significant difference in binary logistic regression (OR =1.620, CI: 0.882 - 2.974). It has also showed association of higher mortality with hyperglycemia (OR=2.528, p=0.024)."

Q6. Suggested adding the section on future implications and the limitation.

We have added sections as suggested and elaborated on the same.

Reviewer 2:

1. The manuscript concludes that there is no association between inflammatory marker levels and new-onset Hyperglycemia in non-diabetic patients with Covid-19 infection. Based on this conclusion the authors make the following statement, "thus questioning the validity of the Covid-19 cytokine storm-induced stress hyperglycemia hypothesis". While their study was focused on assessing inflammatory markers, those are not really the ideal components to assess the cytokine storm that patients with COVID-19 develop, but rather actual inflammatory cytokines such as IL-6, IL-1b, IL-8 etc. etc., thus the authors cannot really question the validity of the Covid-19 cytokine storm-induced stress hyperglycemia hypothesis, unless they perform measurements of an structured inflammatory panel including some of the above mentioned cytokines and demonstrate that they find no correlation at all. Thus, I suggest that the authors remove every statement related to this from the manuscript or reword it based on their findings.

First, I would like to thank the reviewer for his well-thought-out comment. I highly appreciate the review. Although there exists a milieu of inflammatory markers, some of the markers like IL-6, and IL-1b have been shown to strongly correlate with the level of inflammatory cytokine storm, these are routinely assessed in every patient admitted with COVID-19 disease necessitating the use of other inflammatory markers that are cost-effective and can be utilized to assess the severity of the disease. This led to the use of other inflammatory markers like C-reactive protein, ferritin, LDH, and D-dimer which literature such as that by Malik et al (10) clearly demonstrates the utility of these biomarkers in correlating with the severity of the disease. Hence, we decided to use these biomarkers. However, we do accept that these are not gold standard indices reflective of the underlying cytokine storm, hence we have reworded the aim and every statement in the manuscript based on our findings.

2. Having said that, the aim of the study should be completely reworded.

We have rewritten the objective in such a manner that it aligns with the results of the study and our conclusion as follows," We plan to investigate further the mechanisms underlying SARS-CoV-2 infection-induced hyperglycemia, particularly the rationale of cytokine-induced hyperglycemia hypothesis by evaluating the association between inflammatory markers and new onset hyperglycemia in non-diabetic patients with Covid-19 infection".

3. The discussion of the manuscript does not provide a discussion of the author's findings, but rather provides a literature review of several theory mechanisms of hyperglycemia induced by COVID-19 among others. The discussion should focus strictly on discussing every of their findings in relation to what is already published, and possible reasoning of discrepancies found. I did not see any discussion related to the markers the authors used for instance, nor any discussion of their results in relation to the outcomes found in these patients.

Thank you for this review, we have rewritten the discussion to inculcate all the above comments and remarks mentioned and made it concise and focused only on the results of the study.