We thank the reviewers for their participation in the important peer review process. Below are our by point by point responses to their thoughtful comments.

Answering Reviewers:

Reviewer's code: 03537995

1. "However, because lipid levels after treatment was within the optimal level, irrespective of HCV genotype (Table 2), clinical interpretation should be done with caution."

   We agree that clinical interpretation should be done with caution and peripheral lipid profiles may be altered but not to an extent that may have significant effect on cardiovascular or all-cause mortality. However, it has been seen previously, in Corey et al, Hepatology 2009; 50: 1030-1037 (4) increased cardiovascular risk has been demonstrated. We aim to highlight the fact that long-term, longitudinal outcome studies are needed to address this question as discussed in our conclusion (page 12).

   2. Authors described the study is prospective; however, they also described that "The need for informed consent was waived by our institutional review board." The study design should be specifically described.

   We attempted to clarify this in the paper (pages 5 and 6). This study was reviewed and approved by the Banner University Medical Center – Phoenix Institutional Review Board. While data was collected prospectively, all patients were monitored and treated according to joint American Association for the Study of Liver Disease and Infectious Diseases Society of America hepatitis C guidelines. As there was no deviation from standard of care in our population, the need for informed consent for the prospective study was waived by our

institutional review board. Please see the attached approval letter from our institutions IRB outlining the aforementioned.

3. In the discussion section, authors described that, "In our study, we found that treatment with DAA (without the use of interferon) resulted in increases…"; however, according to table 1, interferon was used in some genotype 1 patients.

   This has been revised. Our primary point is that all patients received a DAA in some form or another, and these patient had the described changes in lipid panels. The absence of interferon is a secondary point that may be extrapolated but not directly tested by our study, thus we elected to remove such phrasing altogether.

4. Abstract results; "19.7% Genotype 3" should be incorrect.

   We thank the reviewer for identifying this mistake. This should be 9.7% and is corrected now corrected (page 3).

5. Other errors in the manuscript should also be corrected.

   Again we thank the editors for their careful review. The manuscript has undergone further proofreading and grammatical review, changes in phrasing, tense, and wording have been highlighted in yellow in the revision.

Reviewer's code: 03538158

1. In the abstract section, page 3, "Among genotypes (GT1/GT2/GT3, p), significant differences were seen in TCHOL." What is "p"

Here we are referring to the mean differences. We have defined mean differences as change in lipid profile component from start to end. The mean differences were then compared between the 3 genotypes with ANOVA analysis, which is the "p" that we refer to. We attempted to clarify this in our abstract (page 3)

2. Authors stressed the association between HCV and lipid profiles. This author wants to know the association between hepatic function and lipid profiles in the study. Authors should show changes in albumin and prothrombin time.

   We thank the reviewer for emphasizing this important point. We were able to go back to our collected data and analyze hepatic function. These may be seen in the new table 2 (page 8). Albumin improved in all genotypes. INR improved only in genotype 2. We interpret this to mean that synthetic function improved across genotypes, and that the changes in lipid panel were not just a function of overall improved hepatic function.

3. In discussion section, page 9, is "Chronic hepatitis C infection…" correct?

   We believe that evidence for an association between HCV and secretory pathway has been characterized in the literature and have provided references supporting this. That said, full characterization has not been achieved and we have amended wording to reflect more is to be characterized (page 9)

Reviewers Code: 02528284

1. In addition, they found that patients with GT3 to have the most profound changes in lipid profile, characterized by a significantly greater increase in total

cholesterol than both GT1 and GT2 across the entire population. This is very interesting and merits to be more discussed.

We thank the reviewer for their response and indeed believe genotype specific differences exist which may influence metabolic consequences and response to antiviral therapy. We believe our current discussion highlights and includes seminal articles in the world literature examining the topic from the interferon era and including the study by Meissner et al (5) which, to our knowledge, is the only other paper examining changes (albeit in genotype 1, page 10).

2. "A second interesting point also to discuss if data are available is to provide some information if some of the sera tested were also….."

   It is not clear to us what specifically the reviewer meant in terms of additional sera testing. But we agree that our presented study forms the foundation for continued study in examining metabolic epiphenomena associated with chronic hepatitis c viral infection and its treatment. Our study was designed to evaluate common laboratory paramaters that would be readily available in clinical practice and easily measurable (peripheral lipid profiles by standardized assay). No doubt, a more sophisticated analysis is warranted looking at elements such as the HOMA-IR as well as other lipid subparticles (e.g. LP(a)). As this was a prospective study of a clinical based cohort out measurements were in line with what was commercial available and free of cost (covered by insurance) in line with standards of "real world" ambulatory clinical care