Response to Reviewer Comments

# Reviewer 1

The article is devoted to the study of the role of the association of cholesterol and calcium in the blood in the development of cardiovascular diseases. The article made a strange impression on me. The authors analyzed data obtained on mice, including those on a high-fat diet. The authors compared data obtained in mice with data obtained in humans. Already in the Summary, as well as in the Discussion (for some reason, the authors called this section Conclusion), the authors themselves indicate that the results they obtained on mice fully correspond to the results obtained in many multicenter cohort studies on humans. It is known that cohort studies in humans are, today, the standard for obtaining results on risk factors and predictors of various diseases. At the same time, the results of animal studies help to understand the mechanisms of disease development. The authors do not talk about the mechanisms of development of cardiovascular diseases in their results. Then why did they do this research? To validate data from human cohort studies? It is very strange. I believe that the article and the purpose of the study need to be deeply rethought, to formulate the purpose in a different way, to recalculate and rewrite in a different non-scientific key.

**Response Figure 1: Forrest plot of the associations between cholesterol and calcium in human epidemiological data.** A meta-analysis was performed on studies that evaluated the associations between calcium and cholesterol in a total of 88939 human subjects. In the case of several of these studies data was only presented stratified by subgroups so these are denoted with m, f, f-pm (post-menopausal), or m20-39/m40-69 (males sub-grouped by age). Random effects modelling shows a significant association between calcium and cholesterol with an estimated correlation of r=0.17 (95% CI 0.13-0.20)

We thank the reviewer for their thought-provoking comments about the premise of this research. While the reviewer is correct to point out that our results correspond with the human data, these human data are a) not appreciated in the literature and b) less rigorous in design than our mouse data.

To the first point, while many human epidemiological studies have suggested an association between cholesterol and calcium in blood, to our knowledge there is both not well appreciated in the field of lipidology. Our manuscript is the first to rigorously bring together these often-ignored findings and generalizes them not just to humans but also to experimental animals, reducing the potentiality of residual confounding. To our knowledge, while there have been no systematic reviews or meta-analyses of these human findings. To demonstrate the variability in the human data, and to clarify this point, we performed our own meta-analysis of these associations (Figure 1 of this response), summarizing studies that tested this association [1–9]. Note that several other studies demonstrated significant positive associations, but were analyzed by quantile regression [10–13] or only reported LDL-C not cholesterol [14] so could not be integrated in this meta-analysis. We feel that the work reported in this manuscript in experimental animals is strongly supportive of this human data and we look forward to publishing this meta-analysis of the human associations separately.

In terms of experimental rigor, our approach has several advantages over the human observational studies including known and controlled diets, and environmental conditions. This is noted in the discussion on lines 188-191:

**We present data on a large number of mice roughly equally divided between sexes and two diets and find consistent results across all groups. We have exceptional control of confounders such as diets, environment, activity levels, and other exposures that could affect the interpretation of the human studies**

As an example, calcium and lipid intake varies widely among humans, and diets high in calcium and lipid foods could result in confounded human data. This is noted as a limitation in most of the human studies. This is not a concern in our studies where the diets are entirely controlled. This reduces variability and increases the rigor of the association between calcium and cholesterol. To demonstrate this, the estimated association in humans is r=0.17 (Response Figure 1), but in mice it is 0.39-.048. We believe that this is due to the increased dietary and environmental control, and therefore the reduced variability in the mouse studies. As such this increases the confidence of the generalizability of our findings.

Finally, the extension of these associations to experimental mouse systems (which this study is the first demonstration) enables dissection of the causal mechanisms that both our team and the reviewer are eager to delineate. Without the knowledge that cholesterol and calcium levels are associated in mice, it would be premature to test how calcium regulates cholesterol (and vice versa), so these data are critical to those efforts.

We decided not to over-reach and extend our findings to discussions of cardiovascular disease, as this was not observed in our animal models. This is added as a limitation on line 211-212

**Finally, as cardiovascular disease is extremely rare in mice of this age and these strains we did not assess cardiovascular disease as an endpoint in this study.**

# Reviewer 2

Thank you for your careful reading of our manuscript. We have added the additional information as noted below, and present in the revised manuscript (verbatim text in **red**).

Line 63: it is not clear which details are included in the previous publication. Please specifiy:

Some typos or grammar errors are present all along the text**:**

The manuscript has now been gone through and typos and grammatical errors have been identified and corrected.

Data have to be reported as mean and standard deviation. Standard error is not the correct way to express the variability within the groups:

We revised our data presentation to show standard deviation as requested. This affected Figure 2C, Supplementary Figure 1 and multiple locations in the results file.

Figure 1: the sum of reported n in the box is 818 and not 822 as stated in the legend. I could not find any information on the body weight:

The n of 822 was from a previous version of the dataset, where the n of 818 is from the updated dataset. Thank you for noticing this and we have corrected the number stated in the legend.

# References

Numbering is for the response document only. These numbers are different in the main manuscript file.

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