General Comments:

I appreciate the authors' efforts to revise the manuscript based on the previous feedback. Although some points have been improved, there are still areas that have not been fully resolved, which are critical for the overall quality of the paper.

Specific Comments:

Major:

1. Title and introduction seem to have been appropriately revised.

2. Method (Line 115-125): Although the authors stated that exercise training program was controlled, I think it is not enough to prove that the actual exercise content and the physiological changes that accompany it are equivalent between the placebo and H2 conditions. Was the high-intensity interval training conducted on the rugby field rather than training gym? If do so, the absolute exercise (running) volume or intensity would be changed depending on the athletes' physiological state, which are influenced by H2 gas inhalation. Thus, it is difficult to demonstrate that the same exercise load was imposed on the body, even though the same training program was provided. Thus, I think the lack of information about the detail data of training program and physiological responses to each training are critical limitations.

3. Method: Although the authors stated "research shows that supplementing specific hydrogen before exercise may improve the endurance and recovery of long-term or high-intensity exercise", the direct evidence proves it was not cited in the author response. Moreover, in the Hori et al. (2020), the participants inhaled hydrogen "during" exercise. While I understand a limitation of actual training field, only this point cannot cover the lack of the rationale of H2 gas preconditioning. Thus, I think the authors has not explained rationale of H2 gas inhalation before exercise yet.

4. Results (overall): Although I know the descriptions of statistical results are important, the redundant texts discourage (at least) me for reading. The try to put some information in the supplementary files is so good, and if there are readers who want to read the details, I believe they will also access the supplementary materials. However, the author is not doing anything wrong, so I would like to leave the decision to the editor.

5. Results (overall): I understand your statistical analysis using absolute values are valid. However, if the differences between the individual's baseline conditions are left as they are, it is thought that the results will be affected by the residual factors that have not been fully controlled between the conditions. Since the diet and training programs have been well controlled in the study, it is thought that it would be good to have data on the amount of change (rate) in addition to the absolute value evaluation.

6. Results (overall): The explanation given by the author regarding the correlation analysis between NO and oxidative stress markers is difficult to understand. While I understand that correlation does not prove causation, the correlation between these parameters could raise the possibility that the reduction in oxidative stress caused by H2 gas inhalation may have promoted NO production. These findings could be useful for the development of this research field. For this reason, the authors should at least consider the correlation and present the results, whether or not they are included in the paper. If the authors do not assume that the increase in NO levels is related to the decrease in oxidative stress markers, I think they have not clearly explained why NO levels increased due to H2 gas inhalation.

7. Discussion (line 365-370 Figure 1) : The authors did not explain why did increase NO levels at D7 by H2 gas inhalation compared to Placebo. I understand that H2 gas prevents the consumption of NO during exercise, but I still don't understand why it increases above the baseline levels after one night rest. This point should be discussed.

8. Discussion (line 341-344): In an animal study, H2 gas inhalation during exercise changes some oxidative stress markers (Nogueira et al., 2018; 2021), which can enhance the authors' discussion. Moreover, the discrepancy between oxidative stress markers responses to H2 gas inhalation would enhance the discussion.

Ref. 1) Nogueira et al. FRBM, 2018, PMID: 30243702

2) Nogueira et al. Can J Physiol Pharmacol, 2021, PMID: 33356867

Minor:

1. Methods (Line 109-114): Thank the authors for their efforts to describe the double-blind methods. However, it is hardly to distinguish the term of research team leader and researchers. Was the team leader a third person who did not participate in exercise experiments? Please more clarify.
2. Results (Line 194-198): It is not considered necessary to explain the sentences in this paragraph, because the same detailed information is reported in each of the following sections.