Dear Reviewer,

Thank you for your concerns and suggestions, which will help to focus the paper.

Based on the comments of both reviewers, we suggest the following main changes:

- *Change title to "Thermospheric nitric oxide is modulated by the ratio of atomic to molecular oxygen and thermospheric dynamics during solar minimum"*
- *Move Figures 2 (NO timeseries) and 3 (electron densities) of the preprint to a supplement. Only the two extreme cases, WACCMx and EMAC, are shown in Figures 5 and following. Figure 5 with all models will be moved to supplement as well. The discussion of the O/N2 ratio is also moved to the supplement, as it does not provide additional insights but strengthens the conclusions from the discussion of O/O2.*
- *Increase font sizes in all figures*
- *Add table listing advantages and disadvantages of different model geometries to Summary section*

A more detailed response to your concerns is given below. Reviewer comments given in black*, our response in blue.*

This pretty long paper utilized 6 models to check their ability to reproduce the climatology of NO. They also compare the model results with observations in electron density to test the difference in ionization. From my point of view, this, together with the NO comparison, is too much and oversized for a paper. I think a better revision can be carried out by only focusing the NO comparison, and maybe in more detail like NO during solar minimum quiet time and disturbed time. Then a following up paper can focus on the other comparisons.

We do appreciate that the paper is long. However, focusing only on the NO comparison does not make sense at this point in our opinion, as there already was a paper focusing on NO comparisons, Sinnhuber et al., 2022 referenced in the paper. The strong disagreement between different models in the lower thermosphere shown there are the motivation for this follow-up study. Not following up on why the NO differs so greatly from model to model would therefore be of little additional value compared to the previous study, and does not justify a standalone paper. To shorten and focus the paper, we suggest to move the model-observation intercomparison over the whole year (Fig. 2 of preprint) to supplementary material, and only very briefly summarize those results in the paper.

Also, for electron density comparison, I think it is not a good method to quantify why NO comparison has such difference. ….

Electron density stands for too much aspects and may not simply show the ionization.

We appreciate that electron densities can be difficult to interpret. However, both NO and electron densities can be considered indicators of atmospheric ionization, and considering this, we find it remarkable to note, and an important result, that modeled electron densities fall into a much narrower range, and agree much better with observations, than NO densities. However, for the sake of focusing the paper on the

explanations of the large variability of NO, we suggest to move this part to the supplementary material as well.

I think for NO, the author shall present the major terms that determine the NO density, then check these terms in detail.

The reactions governing formation and loss of NO in the lower thermosphere are summarized in the Introduction (Formation: Equations 1.1 to 6, Loss: Equations 7.1 and 7.2). We compared the rates of these reactions as implemented in WACCM and EMAC, and found that these did not differ significantly, with one exception: One significant difference between WACCMx and EMAC is the partitioning between $N(^2D)$ and $N(^4S)$ formation in the dissociation and dissociative ionization of N_2 (Equations 1.1 and 1.2), which is given in Table 1 of preprint. This would favor a higher ratio of $N(^2D)$ to $N(^4S)$ in WACCMx than in EMAC. However, this is not what is observed – comparison of $N(^4S)$ and $N(^2D)$ clearly shows much larger values of $N(^4S)$ in WACCMx than in EMAC, while $N(^2D)$ values are comparable between both models (Figure 5 of preprint). The very high values of $N(^4S)$ lead to a significantly shorter NO lifetime in WACCMx compared to all other models (Figure 5 of preprint) due to the reaction of N + NO (Eq 7.2), and consequently, to the very low values of NO even though the rates of formation of NO might be comparable. The high values of $N(^4S)$ can be explained by the high ratio of atomic oxygen to molecular oxygen as discussed in the paper, as $N(^2D)$ is efficiently quenched to the ground state $N(^4S)$ by atomic oxygen (Equation 3.1). We will clarify this point more in the revised version.