# Authors’ response

## Review #1

We thank the reviewer for their time and the feedback to our manuscript. We attempted to address all comments and answer them in detail below.

This paper examines simulations of the abrupt-solm4p model experiment from four Earth system models that contributed to CMIP6. Specifically, the authors investigate the models’ respond over different timescales to an abrupt reduction of the solar constant. Through the majority of the paper they focus on the climate response on sub-daily, daily, monthly, and centennial timescales. Toward the end of the paper the authors use a linear regression with global mean surface temperature to calculate the effective radiative forcing, and instantaneous radiative adjustment in the top-of-atmosphere radiative fluxes and the cloud radiative effect output of the abrupt-solm4p model simulations. Although many of the results presented in this paper are novel and interesting, there are some deficiencies in this paper that make it not currently suitable for publication. Below I have listed my major comments that need to be addressed before this paper should be published.

We thank the reviewer for their very good summary of our manuscript.

1. The structure of the manuscript is flawed, where the introduction bears little relevance to most of the results shown. The title and introduction are about rapid adjustments and effective radiative forcing, while until Section 3.6 and figure 14 there are no results shown on the rapid adjustments or effective radiative forcing. Instead, nearly the entire paper focuses on the models’ response at different timescales and does nothing to distinguish the rapid adjustments from temperature mediated changes. Due to this discrepancy between the introduction and nearly the entire results section, there is not a cohesive story to the paper. This makes it difficult to understand why the results shown are significant and how they relate to the rest of the paper. I recommend the authors re-organize and rewrite a lot of the material to either be about the different timescales of response to solm4p, or about the rapid adjustments and ERF.

We thank the reviewer for their comment. In light of this comment, we have substantially restructured the manuscript for a revised version and made sure the flow of the descriptions are clearer now.
Differentiating between rapid adjustments and feedbacks in simulations without fixed SST is challenging and we agree with the reviewer that more attention should be given to this point. We addressed this in our revised manuscript and restructured the paper in the following way.

(1) We added a paragraph to the Introduction, which not only mentions previous studies on solar forcing, but also summarizes their main findings *(Lines 80-103 in the new version of the manuscript)*
(2) We shifted the quantification of rapid adjustments via the linear regression method to the beginning of the results section *(now Sect. 3.1)*. This way, the results section starts more

consistently with the approaches of studies discussed in the Introduction, namely the regression analysis and thus classical way to quantify rapid adjustments in fully coupled climate models.

(3) We also added a new section *(Sect. 3.2)* that addresses the reviewer's concern about the link between adjustments and analysis of response over different time scales. There, we show that multiplying the observed temperature change with the climate sensitivity derived from the linear regression cannot explain the change of TOA effective flux simulated by the models during the first three time scales. Only when applying an initial offset (i.e., the rapid adjustments), the long term development is explained this way. These added results demonstrate that the first three time scales are dominated by rapid adjustment processes.

(4) We then continue with the results part as structured before *(Sect. 3.2.1 to 3.2.4)*, analysing the adjustments of different climate variables over these first three time scales. Here we apply a broader definition of rapid adjustments and consider all alterations to the climate system in response to the forcing that are independent of global surface temperature change as adjustments, following the literature.

(5) We removed parts of the former *Section 3.3* "Effects on cloud properties". As the paragraphs on total column integrated cloud liquid and ice water path were mostly of descriptive nature, we decided to omit them in the revised version of the manuscript for the sake of better flow of reading and to avoid unnecessary length of the manuscript.

(6) We added an explicit discussion section *(Sect. 4, more details in answer two second comment)*

2. This paper is missing substantial literature review. This would include summarizing some of the most relevant studies in the introduction and explaining in the conclusions how the present results relate to the previous literature. I believe that this manuscript would benefit from discussion of how the changes in abrupt-solm4p relate to changes from CO2 forcing, and other studies that have looked at solar forcing. I would like to point the authors to a pair of recently published studies on models' response to solar forcing that they might find relevant.

Aerenson, T., & Marchand, R. (2024). Cloud Responses to Abrupt Solar and CO2 Forcing: 1. Temperature Mediated Cloud Feedbacks. Journal of Geophysical Research: Atmospheres, 129(12), e2023JD040296. https://doi.org/10.1029/2023JD040296

Aerenson, T., Marchand, R., & Zhou, C. (2024). Cloud Responses to Abrupt Solar and CO2 Forcing: 2. Adjustment to Forcing in Coupled Models. Journal of Geophysical Research: Atmospheres, 129(12), e2023JD040297. https://doi.org/10.1029/2023JD040297

We thank the reviewer for the comment and for providing specific references. We added the adjustment paper to the introduction *(Line 79)*. Furthermore, we added a paragraph in the introduction, which describes the findings of previous studies on solar forcing in more detail than was done in the previous version of the manuscript *(Lines 80-103)*.
The submitted manuscript did not include an explicit discussion section. Based on further literature review and the papers, including the ones kindly provided by the reviewer but also

including several others, we now added an extra discussion section *(Lines 520-621)* that more explicitly discusses the findings in the context of other studies' findings *(Lines 540-601).* Moreover, limitations of this study's approach are now discussed in more detail in the new Discussion section *(Lines 602-621)*.

3. The manuscript is also lacking explanation of how the authors distinguish the forced response from internal variability. On timescales as short as hourly, daily, and yearly one would imagine that the modeled climate response would be susceptible to internal variability. As far as I can tell the manuscript does not include any description of how the authors remove the variability signal to isolate the forced response. Without doing so, one cannot determine if the response is due to changing phases of for example, ENSO or the NAO instead of a response to the change in the solar forcing. If the authors are doing something to distinguish the forced response from internal variability, it needs to be mentioned in the manuscript. Or alternatively, if the authors can show that the internal variability is much smaller than the forced response that would also be adequate but would also have to be shown in the manuscript.

We do agree with the reviewer, that short time scales are especially susceptible to internal variability of the models. Unfortunately, the output of only four models that participated in the solm4p experiment of CMIP6, each providing one run, is available. This is indeed quite sparse to base a detailed analysis of internal variability on. However, due to the strong forcing of 10 W m-2, the forced changes in the atmosphere still may exceed natural variability, as AR6 quantified anthropogenic forcing in 2019 to be of the order of 2.7 W m-2 and Sippel et al., 2021 showed, that this already allows a robust detection of forced warming, significantly exceeding climate variability.
An attempt was made to account for internal variability in our study by only considering areas in the discussion, where three out of four models agreed on the sign of the signal. We do acknowledge that this can not replace a thorough significance analysis, which unfortunately was not possible based on the sparse data available for this study. We agree that this shortcoming was not acknowledged enough in the manuscript. We addressed this in the new discussion section *(Lines 602-613)*.

4. The manuscript mentions “not shown” results a handful of times, nearly all of which had me wondering why it is not shown. Generally, if a result is important enough to discuss it is important enough to show. I understand that this paper is already quite long, so some of this could be provided in a supplement.

We thank the reviewer for pointing this out. As the reviewer supposed, we decided to not show some results, because of the length of the paper, if the results were very much expectable and did not further contribute to the discussion of new findings. Nevertheless, we wanted to mention these results for the sake of completeness. However, since the reviewer kindly made us aware that this might cause more confusion than insight, we decided to take the respective passages out of the revised manuscript. Since the revised manuscript contains two additional parts (TOA budget anomaly from global mean surface temperature change and the new discussion section), which makes the paper even longer, we also decided to leave out the sections on cloud liquid and ice water path *(former Lines 263-299)*, since they were mostly descriptive and did not contribute significantly to better understanding adjustment processes.

## Review #2

We thank the reviewer for their time and their comments on our manuscript. We want to use the opportunity to address the comments in detail below.

This paper analyzes the adjustment of the climate system to a 4% reduction in incoming solar radiation in CMIP-6 abrupt-solm4p simulations. Although the paper does contain some interesting results, a few points require clarification and some further investigations before the study can be accepted for publication.

We thank the reviewer for their concise summary of our study.

Main comments:

1) Internal variability: As far as I understand, the simulations analyzed amount to 8 runs: 4 control runs and 4 experiments. Certainly, there are global phenomena active on that timescale (ENSO) with amplitudes of a few tenths of K, which should partly hide the signature of the adjustment. Could the authors discuss it in more detail?

We thank the reviewer for pointing this out. Indeed, addressing internal variability is an important point when discussing rapid adjustments and proves especially challenging in simulations that do not use fixed sea-surface temperatures. However, especially those can provide new inside in rapid adjustments in more realistic scenarios and are hence of interest to the scientific community. As the reviewer pointed out, this study was based on the solm4p experiment by CMIP6, for which only four different models provided output data, each with only one run. This is, as the reviewer rightly points out, a very sparse base to address internal variability. However, due to the strong forcing of 10 W/m-2 we expect forced alterations of the atmosphere to exceed natural variability considerably, as AR6 quantified anthropogenic forcing in 2019 to be of the order of 2.7 W/m-2 and Sippel et al., 2021 showed, that this already allows a robust detection of forced warming.
An attempt was made in the manuscript to account for internal variability, by only considering areas where three out of four models simulated the same sign of signal. Nevertheless, we acknowledge that this cannot be considered a full significance analysis and means that the uncertainty of the results needs to be considered. We added a new discussion section to the revised manuscript, which addresses this shortcoming more clearly than it was done in the previous manuscript *(Lines 602-613)*.

2) Model uncertainty: There is a significant intermodel spread in the longer-term climate response (in temperature, humidity, etc.). However, the pattern of the spread, as well as

possible reasons, are not discussed.

We agree with the reviewer, that especially cloud related variables showed strong inconsistencies in their long term behaviour between the four participating models. However, since this study concentrated on the short term adjustments, which overall showed more consistency between the models. We assume, that long term differences are a among others a result of different cloud parametrizations, tuning and resolution between the models. One effect that we found, which strongly influenced the global mean response of cloud variables, was a reemerging contrast between tropics and high latitudes. Depending on which effect dominated in the respective model, the global mean could show different signs. Unfortunately, addressing these differences was out of the scope of this study and we hope for future research to shed more light on this issue. Nevertheless, we also addressed this disparity in the new discussion section *(Lines 614-621)*.

Minor comments and typos:
Figure 1, caption of panel 3: modelmean -> model mean; xlabel: month -> monthS

Thank you for pointing this out, we changed the caption accordingly.

line 495: A parallel is drawn between the rapid adjustment to a reduction of the solar constant and natural perturbations of the stratospheric aerosol layer, induced by, e.g., a volcanic eruption. However, the timescale of dispersion in the stratosphere is several months, such that the rapid adjustments are unlikely to be relevant. This should be considered in the text.

We thank the reviewer for this comment. The overall aim of this work is to learn more about rapid adjustments. Since in most realistic situations, the forcing is neither instantaneous nor constant, examining rapid adjustments is a big challenge, as the reviewer rightly points out. This we plan to address by starting with very simplified experiments, like the reduced solar constant and then gradually moving onto more realistic simulations. As pointed out by the reviewer, volcanic forcing is changing over the course of months, when the stratospheric aerosol is distributed over the globe and after a few years goes back to zero, when the aerosol deposited and/or was washed out.
We now specified in the respective text passage, that in order to build a link from this idealised simulation to a realistic one considering volcanic aerosol, one has to consider the different time scale of the evolution of the forcing and with it of the adjustments *(Lines 658-662)*. We added a new section to the revised manuscript, in which we show that temperature mediated TOA budget change cannot explain the simulated change in TOA budget for the first three time scales of hours, days and months *(Sect. 3.2)*. In a similar way, it might be possible to show that also in case of volcanic eruptions the first months are dominated by adjustment processes, rather than temperature mediated processes. Since, also significant global mean surface temperature change requires the stratospheric aerosol layer to cover a considerable amount of the Earth, we still expect to see a time delay between adjustment and feedback processes. In

this case, we would not so much be interested in aerosol cloud interactions that happen quickly after a volcanic eruption, but rather in larger scale adjustments of circulation, cloud variables and surface temperature patterns, as we found in this study. We plan to address this comparison in a future study and hope that it helps to further elucidate how the Earth’s climate system adjusts to more realistic forcing scenarios.