Authors’ response

Review #1

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

This paper looks in detail at the evolution of the climate system in response to an abrupt reduction in 4% incoming shortwave radiation. The authors have addressed several of the key points from previous reviews, but some minor details should be adjusted before publication.

We are happy to have addressed the intended key points of the first review and tried to also address the new points in the following.

Main Comments:

Figure 1: it appears by eye that the yearly anomalies plotted follow a gradient in color. Based on the plots I can only assume that the gradient corresponds to the year plotted, with darker years being older? This should be clarified in both the caption and potentially a colorbar added to the figure.

We thank the reviewer for pointing this out and added a sentence to the caption of Fig. 1, explaining how the color gradient shows the temporal evolution of surface temperature and TOA budget change.

Lines 225-235: The authors discuss nonlinearities in the response of SW up, a departure from the linear behavior of the first decade. However, these nonlinearities do not jump out from the referenced Figure (1c). Differentiating the first 10 years of the response in the figure could remedy this.

We thank the reviewer for this comment. We shortened the manuscript according to the 2nd reviewer's comments and we decided to not show the linear regressions of the single fluxes in the main part of the paper anymore. Hence, also the discussion of the non-linearites is more brief than before. We added the SST-pattern effect as a possible cause for the observed non-linearity, but as the non-linearities appear on time scales of years while the paper concentrated on adjustments over the first days and months, this effect was not discussed in further detail.

Lines 239-246: The mechanisms behind the counteracting cloud adjustments should be discussed. Are these dominated by changes in BL clouds? Do these adjustments come from reductions in the optical thickness of such clouds, or changes in the areal coverage of clouds, and do the 4 models agree on the origin of these adjustments? As mentioned, perhaps the magnitude differs among the ensemble, but the different contributions to the cloud adjustments may be partitioned similarly across models.

We thank the reviewer for this comment. Attributing contributions of different cloud property changes to the overall cloud adjustment is definitely an interesting endeavour and can improve understanding. However, this was not possible with the methods used for this paper as we can only discuss changes in the cloud properties and hypothesize on their interactions, but we cannot quantify their contributions to the overall cloud adjustments. Nevertheless, we added a sentence discussing how the decrease in boundary layer clouds at 850hPa coincides with strong positive signals of CRE_sw and hence, might be one of the causes of the overall positive cloud adjustments *(Lines 230-233)*.

Sect 3.2.1: The response in the first month of the forcing is interesting, particularly the warming over the Arctic, where it is claimed that warm, moist air intrusions drive increased cloud cover and a reduced of LW radiation emitted to space, warming the local surface temperature. However, in Figs 4b and 5b, it appears that this region is subject to an uncertain response, or that not all models simulate the warming. The authors should discuss why some of the models don't simulate this warming, because presumably all 4 models simulate a reduced meridional temperature gradient, which is put forth as the initial driver of perturbations to the polar vortex. It seems like this gets a very brief mention in line 610, but I think this should be more prominent in the text and not buried in the discussion.

We thank the reviewer for pointing this out. Indeed all models simulate a warming of Arctic regions (single models not shown in the paper). However, the specific location of warm air intrusions strongly depends on the base state of the climate, which is different for all four models. Hence, even though all models simulate an increase in Arctic surface temperature, they do not agree on the location, and thus, in some Arctic regions less than 3 out of 4 models agree on the sign. We added a sentence on this point in the newly revised manuscript *(Lines 370-373)*.

Figure 8: The authors should specify the sign convention for vertical velocity (wap).

We thank the reviewer for making this point, as the sign convention for the vertical velocity can be confusing, especially when looking at anomalies. We added a sentence that clarifies the sign convention and also decided to change the colorbar to a diverging colormap between blue and red. This leads to more intuitive visualizations as blue colors are associated with an increase in upward velocity or a decrease of downward motion. Moreover, we decided to combine Figures 7 and 8 from the previous version (relative humidity and vertical velocity averaged over land and ocean) as the discussion of the underlying mechanisms switches back and forth between the two variables.

Figure 10: The different behavior of the models over time in terms of both LW and SW up is interesting, especially when comparing the timescales at which individual models seem to stabilize, whereas one model exhibits a continually decreasing SW up and increasing LW up

throughout almost the entire duration of the simulation. Do the authors have insight as to this model’s differing behavior?

We agree with the reviewer that the different behaviour of models for the long term time scale is surprising and raises new questions. However, for this paper we concentrated on the short term time scales and thus, decided to not go into detail on this long term deviation. Therefore, apart from the differing climate sensitivities of the four participating models, we unfortunately cannot give a more specific explanation as to what are the reasons for the long term behaviour. The only thing that we can point out is that the model with the strongest surface temperature response (CanESM5) is also the model that simulates the strong decrease in CRE_lw change. Therefore, this is more likely to be cloud masking effect, rather than overall stronger changes in cloud properties, as the model did not systematically show the strongest changes in other cloud properties. We added this discussion to the revised manuscript *(Lines 336-339)*.

Line 344: How do the estimates of surface RH changes in the tropics from Cao et al (2012) compare in terms of magnitude to those in this study?

We thank the reviewer for asking for clarification on this point and added the magnitude of Cao et al., 2012 findings to our revised manuscript and also elaborated more on the differences between their findings and ours *(Lines 434-438)*.

Line 650: Please contextualize the Smith (2018) results by providing the percentage by which RA reduce the initial radiative forcing, if possible. The comments from previous reviewers noting the non-negligible role of internal variability remain applicable. The discussion section has helped clarify some of the shortcomings of the relatively small ensemble used here. The justifications in the review response by the authors should perhaps be included in the main text.

We thank the reviewer for their comment and added specific numbers on Smith et al., 2018 findings to the conclusion *(Line 571)*. We also included the justification that we provided to the reviewers in the first revision round to the paper and thank the reviewer for this idea *(Lines 342-348)*.

Typos:

We thank the reviewer very much for carefully studying our revised manuscript and pointing out typos, which we corrected accordingly.

Line 28: “number of studies on the subject have been conducted”

We corrected this in *Line 26*.

Line 70: While it can be inferred that the abrupt-solm4p experiment is a simpler analogue of volcanic eruptions, the transitions between these two paragraphs should make this more explicit and clear

We added a sentence in *Lines 61-62* to clear this up.

Line 81: “to decreased temperature of the troposphere…”

We corrected this in *Line 75*.

Lines 95-100: In reporting the results of other literature, the authors state that SW and LW effects tend to cancel each other out, with SW effects slightly stronger. The next sentence then states that SW effects dominate the overall cloud adjustments. Can the authors make clear how this is the case, when it is stated that the SW and LW tend to counteract each other and cancel?

We thank the reviewer for their comment. The wording was indeed unclear in the manuscript and we adapted the sentence in order to avoid confusion *(Line 95)*.

Line 108: change development to evolution

We adapted the sentence accordingly in *Line 111*.

Line 117: “that all rapid adjustments happen while the global mean..” (no comma needed)

We deleted the unnecessary comma in the sentence *(Line 119)*.

Line 119: “while global mean surface temperature has already begun to change”

We changed the sentence in *Line 121*.

Line 120: “On the 120 other hand, the fixed surface temperature method (Hansen et al., 2005) or rather fixed-sea-surface temperature (SST), which is easier to implement in global climate models (Forster et al., 2016), is widely used and has the advantage of suppressing feedbacks.” Suggest moving Forster citation to the end, and remove clause about ease of implementation. It is somewhat implied in the wide use of this method that it is simple to implement.

We thank the reviewer for their comment and implemented it in the suggested form in *Lines 122-123*.

Line 125: “or cooling cannot be simulated in this kind of setup”

We corrected the typo in *Line 127*.

Line 134: The meaning of the sentence leading with “Thermodynamical and dynamical” is unclear, suggest rewording.

We thank the reviewer for pointing this out and revised the sentence. We also restructured the Introduction section to improve clarity and avoid redundancy. Hence the paragraph on types of adjustment signals was shifted to an earlier point in the Introduction *(Lines 78-80)*.

Lin 208: “slope than in the long term” “developing for as long as a decade”

We corrected the manuscript accordingly in *Line 206*.

Line 218: “budget, the yearly mean”

We revised the sentence in *Line 213*.

Line 402: “indicating a reduction in the amount of longwave radiation lost to space”

We changed the wording according to the reviewer’s suggestions as it is clearer in meaning *(Line 271-272)*.

Line 531: “temperature mediated”

We corrected the typo in *Line 472*.

Line 533: I’m not sure what the authors are trying to convey with the sentence leading with “This corresponds to the time”, suggest rewording to emphasize the role of high inertia components of the climate system

We added more information to make the meaning of the sentence clearer *(Lines 472-476)*.

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 analyses the rapid adjustments to a 4% abrupt reduction of the solar constant across four global climate models, analysing the responses on timescales from hours to years. The analysis seems carefully done, and the focus on the time evolution of the adjustments is novel and useful. This paper can therefore be a useful addition to the literature, although I would request major revisions to address the shortcomings described below.

We thank the reviewer for their summary of our manuscript and address the requested revisions in the following.

Main comments:

My main criticism is that the presentation of the results can be improved: the paper is rather lengthy, lacks focus on the key results and could do a much better job highlighting the novel aspects of the findings. I would encourage the authors to try and identify 2-3 “key points”, and make sure that these are highlighted in the abstract, results and conclusions, with a consistent narrative.

We thank the reviewer for their comment. In light of this remark, we worked thoroughly on the presentation of the results. We now shift the paper’s focus stronger to cloud adjustments and characteristic patterns of change, which we motivated via the variability of TOA budget change and CRE change. Further following this comment, we also restructured the manuscript by shifting the global mean temporal development plots of TOA budget change and CRE after the linear regression section. We hope that this responds to the reviewer’s remark to provide key findings and allows for a clearer narrative on why adjustments of atmospheric temperature, humidity and clouds are crucial to better understand the underlying mechanisms and are of interest, even though this method does not allow a quantification of the single variables to the total rapid adjustments at TOA. We expect to find the identified fingerprints also in more realistic forcing scenarios like volcanic eruptions. In those cases, it can help to decide, whether a signal is an actual adjustment signal or only climate variability, as the patterns identified here, were found for four models, which each started from a different base climate.

The issue starts with the abstract, which is too long (I counted ~280 words, which exceeds the APC word count limit of 250) and contains some repetitive points about the significance of rapid adjustments. The first two paragraphs need to be shortened and the abstract should then quickly get to the key points. The subsequent summary of the results is also too vague – for example the final sentence reads “On longer time scales we find robust changes of cloudiness”. Either this is an important result and the text should then specifically describe what these changes are and their implications, or I would leave this out from the abstract. Similar comments apply to the conclusions section.

We thank the reviewer for pointing this out and shortened the abstract accordingly. We now pay closer attention to the cloud fraction changes that were found in the analysis.

Given the paper is about rapid adjustments, it's a bit surprising that no numbers are provided in the abstract at all. The conclusions provide some numbers, but only in the fourth paragraph and almost as a casual side point. I would suggest stating these numbers near the start, and briefly comparing with prior work.

We thank the reviewer for their comment and quantified the rapid adjustments found via the linear regression method in the abstract. Moreover, we restructured the conclusion, paying closer attention to these findings and providing rapid adjustments found by Smith et al., 2018 for comparison *(Line 571)*.

As mentioned previously, the paper feels rather long. In highlighting the key results, the authors could also choose to shorten or cut other parts of the results section, including some of the figures. My overall impression from a quick read of the manuscript was that a lot of results were described whose significance was not immediately clear.

We thank the reviewer for their remark. With the restructuring of the paper described above, we decided to omit the global distribution plots of TOA budget change and CRE. These plots did not add substantially to the understanding of the underlying mechanisms of cloud adjustments, which we chose to be the main focus of the revised paper. Nevertheless, some interesting signals were found, like the partial compensation of short- and longwave cloud effects. Hence, we decided to provide the plots in the Supplements.

Finally, I would recommend that the authors perform a quick spelling and grammar check, as there are quite a few minor (but slightly annoying) mistakes. Please also check punctuation, particularly where relative clauses are involved (e.g. "we show, that" → "we show that"; "in cases, where" → "in cases where", etc.).

We thank the reviewer for taking the time to carefully read our manuscript and pointing out spelling and grammar mistakes. This helped a lot to further improve the text. We addressed all following comments by adapting the text accordingly.

Other minor comments:

- Why only consider the m4p experiment? Perhaps motivate this briefly in the introduction.

The aim of future work is to compare the findings of the solm4p experiment to modelling of the pinatubo eruption (VolMIP, volc-pinatubo-full). Since the stratospheric volcanic aerosol layer scatters part of the incoming shortwave radiation, we expect similar rapid adjustments in both

cases, although the different nature of the forcing (non-instantaneous and located in the stratosphere, rather than at TOA) will surely lead to some differences as well. Analysing similarities as well as differences between the responses to the two forcings could further expand our understanding of the underlying mechanisms.
Moreover, as Aerenson et al., 2024 showed in their work, solm4p and solp4p do not lead to the same adjustments of opposite signs, but there are significant differences due to non-linearities in the adjustment processes. Hence, also using p4p might not increase statistical reliability, but rather add new questions, which were not the focus of this study.
We added a sentence addressing this topic in *Lines 102-106*.

- L210: “inertia of the ocean” – this is vague. My feeling is that there is probably an “SST pattern effect” involved, as this is known to cause changes in the slope of N versus T over time.

We thank the reviewer for pointing this out. We added the SST pattern effect as a possible explanation of the observed non-linear behaviour and reworded the sentence to avoid being too vague *(Lines 205-212)*.

- L210: Can’t the IRF be calculated directly? It’s a 4% solar constant reduction, so the IRF should be 0.04*(solar constant)*(planetary albedo).

Theoretically, it would be possible to calculate the IRF directly using the solar constant and the planetary albedo. Then the IRF would be

0.04*(0.25*solar constant)*(1-planetary albedo)

However, this requires knowledge of the current planetary albedo at the moment of forcing. Since three out of the four models only provide monthly data, calculating the planetary albedo directly for the 01/01/1850 is not possible in an exact way. Because assumptions would have to be made either way, we decided to approximate the IRF by the values of the first month after the onset of forcing. The advantage of this approach is its applicability to the single radiative flux components as well as to the CRE. We acknowledge, that this approach is not exact, but as the variability between the models is much higher than any uncertainty that would be introduced by e.g. taking the mean albedo between December and January of the piControl run and calculating the IRF via the equation given above, we decided to rather use the first month response as an approximation of IRF.

- L256: I don’t understand this sentence. Why “more than 1K”? And I think you mean “changes in the TOA radiative imbalance”, not “forcing”

Thank you for this comment. The wording of the sentence was indeed confusing and we revised the paragraph *(Lines 286-291)*.

- L270: That’s plausible, but hard to tell from the figure because with just four realisations (one per model), the timeseries are probably dominated by internal variability. A more convincing argument (in my view) would be that the lambda*T term (which is not shown, but can be inferred from the evolution of the red and blue curves) is comparatively small in the first three panels. As an aside, it's a bit surprising that internal variability isn't mentioned at all in the discussion of this and other results (at least not before the discussion section towards the end of the paper).

We thank the reviewer for their comment. Internal variability is one of the major challenges when investigating rapid adjustments, especially when looking for characteristic local patterns rather than at global means. When using climate models, this can be addressed by running ensembles rather than single runs. However, as the reviewer pointed out, in the case of the solm4p only four models participated, each contribution only one realisation. This makes for less statistical significance than would be desirable.
However, we argue that the initial forcing of 10 W m^-2, is strong enough to trigger adjustments that exceed internal variability. This is based on findings of AR6, which 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 for a robust detection of forced warming, significantly exceeding climate variability.
We decided to discuss this issue already in Section 3.2 when discussing different time scales of adjustments *(Lines 306-312 and 342-349)*.

- L315: This could also be an effect of land surface temperature change, since land surface temperatures are interactive in fixed-SST runs.

We thank the reviewer for pointing this out. Possible influence of land surface temperature changes were not discussed when mentioning the findings from Salvi et al., 2021. We added a sentence about this topic, discussing how land surface temperature can change in fixed SST-experiments. However, the effects are small due to the coupling of ocean and land surface temperature via the atmosphere. Hence, we argue that, although possibly influencing the results, land surface temperature probably are not the main cause of the signal found by Salvi et al., 2021 *(Lines 396-399)*.

- L365: I would explain this more simply as a lowering of the tropopause.

We thank the reviewer for their comment. Since the tropopause does not descend everywhere, originally we decided not to use this explanation, but if specified for the high latitudes, it is correct and a more intuitive explanation than the one given before. We changed the text accordingly *(Lines 405-407)*.

- L393: The text mentions cloud water path here and elsewhere, but I don't think it's shown in any of the figures – consider cutting, or add "not shown".

We deleted this paragraph as it does not fit the new structure of the revised manuscript.

- Table 2: It would be good to report the CRE adjustments (and their range) with the cloud masking removed.

We thank the reviewer for this idea and added another column to Table 2 with the respective contribution to adjustments.

- Fig. 3: The caption doesn’t describe the red shading. The individual curves for the ERF case are all blue, instead of using the colours corresponding to each model.

By reorganising the Figures, we address this now in the caption of the Fig. 3, which shows the temporal development of TOA radiative fluxes.
“Shading around the multi-model mean shows multi-model standard deviation.”