

## General comment

*We are pleased that the manuscript obtained positive reviews and thankful for the constructive comments. We implemented most of the recommendations of the reviewers and we believe that the text has now much improved. Below is a reply to all comments.*

### Anonymous Referee #1

Received and published: 28 March 2013

The authors present a comprehensive and well written analysis of how the energy budget and hydrological cycle respond to contrasting radiative forcings (solar or CO<sub>2</sub>) of the same magnitude. This builds upon a body of literature, yet some novel insights and important key findings and recommendations are outlined (in particular relating to the influence on meridional temperature gradient and large-scale rainfall) and so it is my assessment that the manuscript should be published with relatively minor amendments. My main concern is that while this is exciting new science, the impact of the study may be enhanced by reducing the length and discursive nature of the text but this is a minor criticism. I outline specific points below.

1) Abstract "...mean precipitation, in simulations of transient CO<sub>2</sub> concentration, increase..." (add in another ", " after concentration). This sentence is rather long.

*No, here we really meant "simulations of transient CO<sub>2</sub> concentration increase" as a whole.*

2) Abstract "On the other hand, lower tropospheric water vapor increases more in simulations with CO<sub>2</sub> compared to solar forcing increase of the same magnitude." for the same radiative forcing or same temperature change? Is this due to high vs low latitude warming?

*Thanks for pointing this out, this was not correct. We changed the sentence to:*

*"On the other hand, lower tropospheric water vapor increase is similar between simulations with CO<sub>2</sub> and solar forcing increase of the same magnitude."*

3) p.395, line 4 - many aerosols also absorb radiation

*"..., although some of them also absorb radiation" added.*

4) p.396, line 10, I suggest changing "as this is what is occurring in the real world" to "since this is more relevant for adaptation strategies."

*Changed as requested.*

5) p.396, line 19 "mean surface temperature neither exactly doubles" the temperature is not doubling - I guess you mean the temperature difference.

*Thanks, changed as requested.*

6) p.396-397 - I found the discussion of linear additivity to be rather verbose and difficult to penetrate and is repeated in Section 3.1. Could this be written more concisely? Also, I think the work of Good et al. (2012) Climate Dynamics ("A step-response approach..." doi 10.1007/s00382-012-1571-1) may be relevant.

*We partly rewrote this paragraph (also taking into account the comment of the fourth reviewer) as well as the first paragraph of Section 3.1.*

*The Good et al. (2012) paper is indeed highly relevant but was not published at the time when the first draft was written, and we forgot to include it afterwards, thanks for this comment.*

7) p.398 - I find the definitions of the scenarios confusing since "74" could be confused with years which are sometimes also quoted. I suggest using S3.7, S7.2, C2X, C4X and C2X-S7.2 to signify the

experiments.

*Thanks for this suggestion - we changed the labels in the text and in the figures.*

8) p.399, line 21, the "rho" symbol should be defined.

*Done.*

9) p.401, line 13, what is the physical mechanism which explains larger than expected responses for larger forcings. One possibility is the increased LW emission level with increased CO2 levels and the reduced Planck function response at colder temperatures (e.g. Good et al. 2012).

*We did not investigate in detail why the response to larger forcings is larger than expected but the mechanism proposed in Good et al (2012) is certainly relevant, we added a sentence in the text:*

*"This is in line with e.g. Good et al. (2012), who showed that with increasing CO2 levels, the longwave emission level raises, implying a colder emission temperature and therefore a reduced Planck function response."*

10) p. 402, line 5-6 - I suggest "...since this is more relevant for the real world, the climate system never reaching a true equilibrium." Also, line 9 "response...does" or "responses...do"

*Changed as requested.*

11) p.403, line 5 - I suggest "...reduced (as occurs in warming scenarios) the..."

*Changed as requested.*

12) p.403, line 12 - 15-50 degrees latitude does not seem well described as midlatitudes. It also includes the sub-tropics.

*Thanks, we modified the sentence:*

*"... the sub-tropics and mid-latitudes (50S-15S and 15N-50N), which will be referred to as mid-latitudes for simplicity in the rest of the text,..."*

13) p.405, line 4 "is balanced" → "is offset" (since it is too small to balance)

*Changed as requested.*

14) p.405, line 9 - LW is also increases due to the warming of the atmosphere. The mechanism applies to CO2 and Solar so I object to the use of the word "contrast". See also Allan (2006) JGR doi:10.1029/2006JD007304

*We agree with this comment and removed "in contrast".*

15) p.405, line 15, could the strong negative LW cloud feedbacks also be influenced by the fast cloud adjustments to the CO2 forcings e.g. Gregory and Webb (2008)?

*This could be one reason but we have not calculated specifically the fast cloud adjustments. We completed the sentence with:*

*“, which might be due to the fast cloud adjustments to CO2 as shown by Gregory and Webb (2008)”.*

16) p.406, line 8, note that the increases in CO2 do not have a substantial direct effect on surface LW in the tropics due to strong water vapor absorption across the LW spectrum for high column integrated water (e.g. Allan, 2006).

*Reference and comment added in the text.*

17) p.406, line 11 I suggest "...causes a larger increase in water vapor and consequently larger back radiation." since it is the larger water vapor amounts that produce stronger water vapor continuum emission to the surface in the LW window region of the spectrum.

*Thanks for this remark, we changed it in the text.*

18) p.406, line 26 - this discussion is interesting but what determines the portion of available energy

that goes into evaporating water, heating the surface or sensible heating? For example, if more energy is available for evaporation, this increased evaporation rate can only be sustained if the evaporated water vapor is removed from the boundary layer by convective processes, such that the atmosphere and surface energy budgets must be considered together.

*We partly rewrote this paragraph, according to your suggestions and to comments from the fourth reviewer, to highlight that surface and atmosphere energy budgets are closely linked.*

19) p.407, line 23, although high cloud cover changes are small, more critical to cloud LW effects are the cloud top temperature. Zelinka and Hartmann (2010, JGR DOI: 10.1029/2010JD013817) for example show that the relatively small changes in cloud top emission temperature with warming cause positive LW cloud feedback in CMIP3 models.

*Thanks for this suggestion, we added the following sentence:*

*“However, Zelinka and Hartmann (2010) showed that small changes in cloud top emission temperature are more critical for the LW cloud feedback than changes in high cloud cover.”*

20) p.408, line 25 "by up to" → "by as much as"

*Done.*

21) p.409-410 discussion is very interesting and novel I think. line 13 - does the unusual NH response in MTG link to the fast responses of land (which dominate the NH) to radiative forcings?

*We rather think that the little change in MTG in the NH (meaning that the warming is the same at low compared to high latitudes) is due to the cooling in the North Atlantic present in all simulations and caused by the weakening of the AMOC. In the zonal mean, this cold anomaly offsets the polar amplification over the other parts of the northern high latitudes. The decrease in MTG is seen only in the strong forcing scenarios with CO<sub>2</sub>, where the strong polar amplification offsets the cold anomaly.*

*We added this sentence in the text:*

*“The fact that the MTG NH index is not changing much for C2x, S74 and S37C2x is due to a cold anomaly in the North Atlantic caused by the weakening of the AMOC. The average temperature change in the northern high latitudes is therefore similar to the change in surface temperature in the tropics because the strong warming caused by polar amplification is damped by the cold anomaly in the North Atlantic.”*

22) p.412 - again, does the discussion in Good et al. (2012) offer an explanation for the non-linear additivity of CO<sub>2</sub> forcings?

*Yes, we added “due to a reduced Planck response for higher LW emission levels (Good et al., 2012)” at the end of the sentence on line 12.*

23) p.412, last few lines - again, in relation to point (18) I think that the atmosphere energy budget is also integral.

*We removed “at the surface” as we agree that it is only part of the explanation.*

24) p.412 line 29 - p413 line 2 does not seem correct. I think that a weaker circulation is necessitated by a more muted precipitation response RELATIVE to the water vapor response. Also the residence time changes are surely a diagnostic of this rather than an explanation.

*This is probably a language problem because we meant to say what you write. We reformulated those two sentences:*

*“A weaker circulation is necessitated when the precipitation response is more muted relative to the water vapor response. This is illustrated by the longer residence times in CO<sub>2</sub> scenarios compared to solar scenarios.”*

## Referee #2 Antonio Speranza

Received and published: 20 April 2013

The authors consider “transient responses” of the energy budget and the hydrological cycle to CO<sub>2</sub> and solar forcings “of the same magnitude” in a global climate model and analyse the processes that determine such responses in the adopted model. They find that less energy is available at the surface for global annual mean latent heat flux and, as a consequence, for global annual mean precipitation in simulations of transient CO<sub>2</sub> concentration increase compared to what happens in simulations with an equivalent transient increase in the solar constant, while lower tropospheric water vapor increases more in simulations with CO<sub>2</sub> compared to what happens with a solar forcing increase “of the same magnitude” and, as a consequence, the response in precipitation is more relevant than the response in water vapor in CO<sub>2</sub> forcing simulations, leading to a larger increase in residence time of water vapor in the atmosphere compared to what happens in solar forcing simulations. Moreover, energy budget calculations show that poleward atmospheric energy transport increases more in solar forcing compared to equivalent CO<sub>2</sub> forcing simulations. The authors also test, with particular attention, the assumption that the responses to forcings are “linearly additive, i.e. whether the response to individual forcings can be added to estimate the response to the combined forcing” and find that the forcings do not add linearly. The authors point out in the Conclusions that “Depending on the application, the errors introduced by assuming linear additivity when it does not apply might be considered negligible or not. In any case, these results cannot be captured properly by models of lower complexity, which are often used to inform policy makers or for impact studies, and are implicit when characterizing the overall magnitude of climate change or a target for stabilization in terms of global mean temperature or total radiative forcing. The linear additivity assumption is also tested for surface temperature, large-scale and convective precipitation in the tropics, midlatitudes and high latitudes and appears to be not valid in general, regardless of the sub-region considered.”

### General comments

In my opinion the problems proposed in this paper are interesting, the analysis is conducted with care and the proposed results are relevant and adequately documented. As a consequence, I think the paper can be published in essentially the present form.

I suggest some minor text integrations: essentially clarifications addressed to helping readers to follow the reasoning and understand the proposed results without too much effort. Some specific requests in this sense are listed below.

### Specific comments

Pag. 394 line 2

“The transient responses of the energy budget and the hydrological cycle to CO<sub>2</sub> and solar forcings of the same magnitude in a global climate model are quantified in this study.”

I would suggest something like “normalised forcing procedures” rather than “forcings of the same magnitude”.

*We keep the original wording because we fear that “forcing procedures” may not be clear to all readers.*

Pag. 396 line 9

“Many studies have quantified the climate responses in simulations where the forcing is increased instantaneously (e.g. Bala et al., 2010). While much can be learned from those, there is also currently a need to understand transient climate change as this is what is occurring in the real world. The aim of this study is therefore to quantify the transient response of the energy budget and the hydrological cycle to different forcing agents, globally and zonally.”

In what sense transient responses cannot be considered in a simulation in which the forcing is “increased instantaneously”?

*By definition transient response means the response to a forcing applied transiently. It is however true that when a forcing is applied instantaneously, there is a fast and a slow response since the different components of the climate system respond on different time scale. However, the latter is not usually referred to as transient response in the current literature.*

Pag. 397 line 20

“A set of idealized transient simulations is performed with the NCAR Community Climate System Model version 3.5 (CCSM3.5) (Collins et al., 2006; Gent et al., 2010). The finite volume dynamical core of this fully coupled ocean and atmosphere model has a spatial resolution of 1.9\_ in latitude and 2.5\_ in longitude, with 26 levels in the vertical.” This is the proposed description of the model. Considered that only one model is used in the proposed numerical experimentation and many conclusions concerning the internal conversion mechanisms are drawn, it would help the reader to dispose of a minimal description of how some basic processes (precipitation, in particular) are numerically dealt with in the adopted model.

*The following sentences were added:*

*“The physics of cloud and precipitation processes include a separate prognostic treatment of liquid and ice condensate, advection, detrainment, and sedimentation of cloud condensate and separate treatments of frozen and liquid precipitation (Boville et al. (2006), Collins et al. (2006)). Including the effects of deep convection in the momentum equation lead to improvements in the representation of ENSO, the Asian monsoon and the double-ITCZ problem in the eastern Pacific Ocean (Gent et al. (2010)).”*

Pag 398 line 1

“...consists of five initial condition ensemble members to robustly quantify the model internal variability.”

I would hesitate to use the adjective robust in connection with statistics of five elements.....

*We removed “robustly”.*

Pag.399 Section 3.1 Linear additivity of the responses

The authors seem to be referring to linearity in two different ways:

- “linear additivity” of a specific variable with respect to the superposition of different climate forcing modulation agents;
- linear response of a specific variable to a single forcing-modulation agent.

Do I understand correctly?

*Yes this is correct. As it is stated in the last paragraph of Section 2, low and high forcing scenarios are compared for CO2 and solar forcings separately first, and then combined. Figures 1 and 5 illustrates the three different comparisons.*

### **Anonymous Referee #3**

Received and published: 30 April 2013

The article is devoted to studying the responses in energy budget and some components of hydrologic cycle to various forcings. The main tool used in the article is NCAR's CCSM3.5. A significant amount of technical work was invested in the article and I am sure this was a very nice learning experience for the authors. Important model assessment of future changes in the components of energy budget is the main outcome of the article. Although I have to admit that the results do not sound too exciting to me – the article deals only with the question 'HOW', not 'WHY'. Analysis of the physical mechanisms behind the modeled changes and non-additivity (or non-linearity) of the response would greatly improve the article. The article should be published after addressing some questions (see below).

1. Why would responses to forcings of different magnitudes be linearly additive in a system with strongly non-linear feedbacks: snow and ice albedo, ocean circulation etc?

*We agree that the feedbacks in the climate system are non-linear and seeing some non-linear responses is expected. Still, the global annual mean responses are surprisingly linear. Although we show that it is not exactly linear for some variables (see Table 1), the assumption that the responses add linearly is not far off. Further, several techniques, detection and attribution as well as pattern scaling, are based on this assumption as we describe it in the Introduction, and we wanted to quantify the errors introduced by the assumption.*

2. Analysis of how changes in snow, ice, ocean circulation etc associated with each of the forcing impact the non-linearity should be considered (maybe not in this article). Some discussion will be useful.

*This is indeed an interesting and important question. We feel that it warrants a deeper analysis, so we are about to submit an article on the changes in sea-ice area, atmospheric circulation and oceanic circulation in CO2 forcing and solar forcing increase and decrease simulations.*

3. The title and the abstract should reflect the fact that the article deals with model output and it is only one model that is being used in the analysis. Otherwise the title sounds too general.

*The title was changed to reflect that the study is about model results. However, it is already mentioned in the first sentence of the abstract that it is "in a global climate model", and "simulations" or "scenarios" are written in almost all sentences.*

4. Some English proofreading is probably needed.

*We carefully proofread the text.*

## Referee #4 Andrea Alessandri

Received and published: 15 Mai 2013

This paper studies the transient response of the energy budget and the hydrological cycle to CO<sub>2</sub> and solar forcing, both globally and zonally. To this aim, a set of idealized transient simulations are designed and performed using the NCAR Community Climate System Model version 3.5. The paper analyzes the transient response and compares the sensitivity to CO<sub>2</sub> and solar forcings. The main aim is to test the assumption that the response to the forcings is linearly additive, i.e. whether the response to individual forcings can be added to estimate the response to the combined forcing.

### Main comment:

This paper designed a novel set of transient simulations to specifically test the assumption that the response to the forcings is linearly additive. As far as I know this is the first time a state-of-the-art CGCM was used for such an analysis and by using specifically conceived ad-hoc transient simulations. The authors show that, for the global climate model considered, the responses of most variables (regardless of the sub-region considered) of the energy budget and hydrological cycle, including surface temperature, do not add linearly. This has important implications for policy makers, who often use lower complexity models, which behave most likely following the linear additivity assumption.

Overall, the manuscript describes an interesting study and contains original results that are worth of publication. However, there is room for further improvements by (i) clarifying the text, (ii) better explain the method in relation to the equations of the energetics in the atmosphere (see comments) and (iii) possibly by adding some further analysis. I recommend publishing this paper after return for some modifications to address the specific points reported in the following:

-In this work it is used only one CGCM. Please, state that the results are likely to be model dependent.

*We stated in the last paragraph of the conclusions that this is a limitation of the paper:*

*"It is important to stress that the presented results are based on one global climate model and cannot claim universal validity. Still, it would be useful to compare them with the same scenarios performed with different GCMs to assess whether the described processes are robust."*

*Further, the whole last paragraph encourages the comparison of modeled results.*

-The title appears too general and should better reflect what is the new contribution of the paper.

*We changed the title according to the comment of the third reviewer.*

-In the introduction (page 3, lines 10-12): "Precipitation, and its energy equivalent, latent heat, are variables that belong to both the energy budget and hydrological cycle (e.g. Bosilovich et al., 2008), hence the need to analyze them jointly."

In this respect, Alessandri et al. (2012) developed a method to analyze the precipitation change that is based on both water and energy conservation principles in the atmosphere. The method generalizes the approach in Liepert and Previdi (2009) as it can also be applied to regional domains and not only to the global average. It is suggested to use the method developed in Alessandri et al.(2012) in this work to possibly strengthen the outcomes and the conclusions of the paper.

Alessandri, A., Fogli, P. G., Vichi, M., and Zeng, N.: Strengthening of the hydrological cycle in future scenarios: atmospheric energy and water balance perspective, *Earth Syst. Dynam. Discuss.*, 3, 523-560, doi:10.5194/esdd-3-523-2012, 2012.

Liepert, B. G., and Previdi, M.: Do models and observations disagree on the rainfall response to global warming?, *J. Climate*, 22, 3156-3166, 2009

*Both references were added.*

-Introduction (page 3, lines 15-18): "It is widely accepted that global mean precipitation change per unit temperature change is more sensitive to changes in solar radiation than to changes in CO<sub>2</sub> concentrations (Allen and Ingram, 2002; Gillett et al., 2004; Andrews et al., 2009; Bala et al., 2010)." It should be also mentioned here the relation between aerosols and solar forcing in the context of the

already performed climate scenario studies (comparing the sensitivity to GHGs and Aerosols). The importance of the anthropogenic sources and the possible mitigation appears of particular relevance. For instance, Liepert and Previdi (2009) explicitly showed that the precipitation in coupled GCM can be more than three times more sensitive to aerosols compared to GHGs forcing. Alessandri et al (2012) warns that mitigation policies that promote aerosol abatement, may lead to an unexpected stronger intensification of the hydrological cycle and associated changes that may last for decades after global warming is effectively mitigated.

*We modified the sentence to "...to changes in aerosols or solar radiation..." and added the reference to the Liepert and Previdi paper. We do not want to put too much focus on aerosols as we do not perform simulations with changes in aerosols. We however do not see an added value in mentioning the question of geoengineering, which is a whole topic in itself, in the Introduction, since this paper does not further discuss this issue.*

-Introduction (Page 4, lines 16-29 and page 5, lines 1-13): I'd suggest to put this discussion before stating the aim of the paper (page 4, lines 10-14).

*Changed as requested. In addition, following the suggestion of the first reviewer, the paragraph was re-written.*

-Section 2: A table summarizing the main characteristics of each transient simulation performed would be very helpful.

*According to the comment of the first reviewer, we changed the labels of the five scenarios, which are now clearer. In addition, we added the following sentence in Section 2:*

*"Figure 1a shows the time series of the global annual mean surface temperature anomaly for the five scenarios."*

*This hopefully helps the reader to have a better representation of the five scenarios.*

-Section 3.1: Fig.2 introduced in the text before Fig.1;

*Thanks! Fig.1a is now mentioned first, in Section 2 (see previous response).*

-Section 3.1 (page 9, line 11): "The values shown represent non-linearities arising from long-term feedbacks."

Please, try to clarify and discuss further this sentence.

*We added a short explanation at the beginning of the sentence:*

*"Since the responses are scaled by the adjusted radiative forcing, the values shown represent non-linearities arising from long-term feedbacks."*

*As described in Section 2, "The adjusted forcing is different from the radiative forcing in that it includes the rapid adjustments occurring within a few days in the troposphere and land-surface", which is why we do not quantify non-linearities arising from short-term feedbacks, but those arising from long-term feedbacks.*

-Section 3.2 (Page 10, lines 15-16): "First, scaling the responses would make the assumption that each variable at each grid point scales linearly with the adjusted forcing. While scaling the responses of diagnostic variables might be justified, other quantities such as the zonal mean profile of specific humidity or residence time of water vapor in the atmosphere cannot necessarily be scaled with the adjusted forcing."

Not clear, I cannot understand. Please consider substantial revision.

*We reformulated the second sentence, which was not clear:*

*"While scaling the responses of global or continental temperature might be justified (Meehl et al., 2004), other quantities such as the zonal mean profile of specific humidity or residence time of water vapor in the atmosphere cannot necessarily be scaled with the adjusted forcing."*

-Section 3.2 (page 10, line 26): Please consider revision. E.g: replace "due to the fact that" with "indicating that".

*Done.*

-Section 3.2 (page 14, lines 9-11): “In addition, changes in surface temperature are larger in CO<sub>2</sub> scenarios, which in itself causes a larger LW back radiation, and consequently larger increases in water vapor.”

Causes and effects are mixed here. The direction of causality is not clear. Please revise or remove text.

*We revised this sentence according to comment 17) from the first reviewer.*

-Page 14 (line 13): “Changes in global annual mean precipitation can be understood either from an atmospheric (Mitchell et al., 1987; Allen and Ingram, 2002)” Please, cite Liepert and Previdi (2009) and Alessandri et al. (2012).

*Done.*

-Page 14 (bottom): “and some of this excess energy will be taken up by the ocean  $\Delta\text{NET}_{\text{surf}}$ .” Please replace with “and the excess energy will be taken up by the ocean or land surface ( $\Delta\text{NET}_{\text{surf}}$ ).”

*Changed as requested.*

-Section 3.3.1 (page 14, lines 27-28 and page 15, lines 1-10):

The changes in partitioning at the surface between latent and sensible fluxes are strongly coupled through the surface energy balance. Specifically, LH and SH compete for the available energy and this is not considered adequately in the text when analyzing SH decrease. For instance, over oceans and wet lands (and increasingly towards equator) the increase in temperature is expected to affect more LH (since potential evaporation is proportional to saturation specific humidity at the surface; i.e. exponential function of Temperature) with respect to SH (linearly increasing with temperature). Therefore, the larger SH decrease over ocean appears more consistent with the fact that LH is at its potential value there [I suppose  $\Delta\text{LH}$  largely dominates and  $\Delta\text{SH}$  acts by partially compensating the LH increase].

Surface warming is present in all transient simulations (see Fig.1), while SH is seen to mostly decrease. Therefore, we can hardly infer causality as in following sentence: “over land, SH slightly increases (around  $1\text{Wm}^{-2}$ ) due to surface warming”. Conversely, much colleagues would probably believe causality is going in the opposite direction (i.e: surface temperature increases more where LH do not dominate, e.g: over land with large SH/LH; See Sutton et al., 2007).

I’d like to encourage the authors to consider using the method in Alessandri et al (2012) when analyzing the change in partitioning at the surface between SH and LH (Bowen ratio potential; equation 11 in Alessandri et al., 2012), both globally and zonally.

Sutton, R. T., Dong, B., and Gregory, J. M.: Land/sea warming ratio in response to climate change: IPCC AR4 model results and comparison with observations, Geophys. Res. Lett., 34, L02701, doi:10.1029/2006GL028164, 2007.

*We rewrote this paragraph according to your suggestions and those of the first reviewer. We do not want to further investigate the partitioning of the turbulent fluxes, as it is not the focus of the paper. Our goal was to briefly describe those changes in an effort to present changes in all variables of the energy cycle but not to investigate them in depth.*

-Section 3.3.2 (Page 16, line 13): please replace “heat balance” with “energy balance”.

*Done.*

-Section 3.3.2 (Page 16, line 18): please replace “decreases” with “is expected to decrease”.

*Done.*

-Section 3.3.2 (Page 17, lines 14-19): “Focusing again on the differences between CO<sub>2</sub> and solar scenarios, the changes in convective precipitation seem to follow the changes in surface temperature shown in Fig. 1b–d. Convective precipitation increases more in the tropics and mid-latitudes in solar

scenarios due to the stronger warming while in the high latitudes, the convective precipitation response is larger in CO2 scenarios due to the stronger polar amplification in these scenarios (see Figs. 5c and 1d).”

Consider revision, e.g. as follows:

“The different changes in convective precipitation, between CO2 and solar scenarios, seem to follow the changes in surface temperature shown in Fig. 1b–d. Convective precipitation increases more in the tropics and mid-latitudes in solar scenarios due to the stronger warming, while in the high latitudes the convective precipitation response is larger in CO2 scenarios (see Figs. 5c and 1d).”

*Changed as requested.*

-Section 3.3.2 (page 18): Please, define and explain briefly meridional temperature gradient (MTG). *This is not something that is strictly defined, we refer to the paper of Gielman et al. (1997) and we write how we defined it:*

*“Using the sub-regions defined above, we calculate a MTG index in each hemisphere as the difference between the respective high latitudes and the tropics.”*

*In addition, “meridional temperature gradient” is quite self-explanatory.*

-Section 3.3.2 (page 18, 15-29 and page 19):

(i) Please, replace “poleward energy transport” with “energy convergence at the mid-high latitudes” *We prefer to keep “poleward energy transport” as it is found in the literature.*

(ii) I recommend you to give a rigorous mathematical derivation of the energetics here, when discussing the atmospheric energy convergence at the mid-high latitudes. Please, show equations and motivate the simplifications and neglected terms in your computations. [See for instance equation 10 in Alessandri et al (2012). You can obtain atmospheric energy convergence easily by substituting equation 4 in equation 10.]

*We prefer to keep the method used in Rugenstein et al. (2012), as it is quite straightforward and sufficient for our purpose. Since it is simply the difference between surface and TOA net energy flux, we do not think that it is necessary to provide an equation.*

(iii) This treatment of the energy budget is better suited for Section 3.3.1. I suggest moving text. Instead the atmospheric water budget and the combination of both water and energy principles can better be used in 3.3.2 (see iv).

*We believe it makes more sense to have those results here.*

(iv) It is suggested to use the analysis as in Alessandri et al. (2012) to strengthen the outcomes and the conclusions of the paper. The method in Alessandri et al. (2012) is based on both water (its equation 4) and energy conservation principles (its equation 10) in the atmosphere. It can be applied to regional domains as well as to the global average. Applied to the mid to high latitude zonal band, the method allows to answer why “large-scale precipitation increases more in solar scenarios compared to CO2 scenarios at higher latitudes”. Note that, by substituting equation 4 in equation 10 of Alessandri et al (2012) you can obtain the following equation for the computation of the moisture convergence.

$$\Delta\{-\nabla_h \cdot Q\} = 1/L (-\Delta\{-\nabla_h \cdot SH\} - \Delta\{-S_{net}\} - \Delta\{-T_{net}\} - \Delta\{SH_{\uparrow}\} - \Delta\{LE_{\uparrow}\})$$

Please, also note that  $-\Delta\{-\nabla_h \cdot SH\}$  can be obtained as the residual using equation 10.

*While the analysis in Alessandri et al. (2012) is interesting and more thorough in describing changes in the water cycle and energy budget, the simplified method we use essentially follows the same principles, and to us appears appropriate in the context of our analysis. We have referenced Alessandri et al. (2012) in two places for interested readers. In addition, we do not want to duplicate the work of the reviewer.*