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Title: Strengthening of the hydrological cycle in future scenarios: atmospheric energy and water balance perspective
Author(s): A. Alessandri et al.

Responses to Reviewers #1 and #2
[5/10/2012]

-First of all, we would like to thank the reviewers for their work. We found their comments helpful and we think they have given a contribution in increasing the quality of this paper. We have modified the manuscript according to all the reviewers' requirements and suggestions and we believe that the revised manuscript is much improved. Please see below our detailed reply to the reviewers' comments.

-Please note that First Author, Andrea Alessandri, added International Pacific Research Center (IPRC) as its second affiliation other than ENEA. So the following two affiliations will compare in the revised manuscript:
-Agenzia Nazionale Per le Nuove Tecnologie, L'Energia e lo Sviluppo Economico Sostenibile
-International Pacific Research Centre, Honolulu, HI, USA

Responses to Reviewer #1 (Dr. B.G. Liepert)
[5/10/2012]

General Comment:
In this manuscript the authors describe an investigation into the anticipated future energy and water mass constraints on the hydrological cycle in two 21st century climate model experiments. As far as I know this is the first time an Earth system model was used for such an analysis. The new results that are presented here are qualitatively consistent with a series of papers we published on the same subject. In one paper (Liepert and Previdi 2009) we used a similar Bowen ratio approach as described here (albeit with a somewhat different derivation, see "specific comments"). We focused our study on the differences of aerosol versus greenhouse gas forced changes in the hydrological cycle. Here the two scenarios of the model experiments are comprised of a 21st C mitigation strategy E1 and a medium scenario A1B. The importance of the mitigation scenario is the restriction of global warming to 2K. Hence the current study extends the previously published results by utilizing a state-of-the-art Earth system model and focusing on what will happen with the water cycle under global warming mitigation. This is an important contribution and worth publishing. The study however, has generally more potential than described in

this text. The conclusion that a global stabilization below 2K comes with a significant increase in global precipitation over the mitigation period is an important message that could be discussed more prominently (see specific comments). A more thoughtful model description that focuses on the specifics that are analyzed in this study would be advantageous. The carbon cycle feedbacks are described in the "model" section but not referred to in the analysis and interpretation of the results. Land carbon cycle feedbacks are described at length but not ocean carbon cycle feedbacks. Is there a specific reason for this? Natural aerosol feedbacks in the model are also not described in spite of their importance for the conclusions. The testing of the model capability of simulating the water and energy cycle correctly cannot be underestimated since several studies have shown that model deficiencies of the order of climate changes exist in some models (Lucarini and Ragone, 2011; Liepert and Previdi, 2012). Please add some quality control statements to the text. Some information listed in tables is redundant in figures. Overall, in my opinion, the manuscript describes an interesting study of the temporal behavior of the water cycle under extreme global warming mitigation scenarios that is worth publishing after clarifying the method issues (see below), streamlining the model section of the text, and adding some more ESM specific analysis.

Response to General Comment
We'd like to thank Dr. B.G. Liepert for the useful comments. We followed the reviewer recommendations and we think the discussion in the revised paper is much strengthened. This study gained scientific value by the important link with the Liepert and Previdi (2009) paper. It is explained that the method to thermodynamically constraining global precipitation changes is similar to the approach in Liepert and Previdi (2009) and the main difference is that it can also be applied to regional domains and not only to the global average. Furthermore, it is observed that the two methods are consistent; in fact, when considering the global average our eqs. 10 and 11 reduce to eqs. 4 and 5 in Liepert and Previdi (2009).
We discussed and reported as one of the main conclusion that a global stabilization below 2K comes with a significant increase in global precipitation over the mitigation period. To this aim we added a separate Figure 6 (Figure 5 of revised manuscript), which reports time series of both temperature and precipitation for the two scenarios zoomed in on the 2030-2100 time-frame.
Detailed information and clarification of the implementation of the radiative forcings and feedbacks in the experiment has been provided. To this aim and following request of the reviewer, figure 4 (figure 3 in the revised paper) has now been totally redesigned in order to make it easier to comprehend the discussion of aerosol effects. It is made clear now that only anthropogenic sulfate aerosol is time-varying by plotting both total sulfate (anthropogenic + natural) aerosol burden and natural (constant) sulfate aerosol burden in figure 4. From the description added to the text, it is made clear now that only the time evolving concentration of anthropogenic sulfate aerosol is provided as the boundary forcing and not carbonaceous or other anthropogenic or natural aerosols.
In the revised manuscript it is explained that (as described with details in previous paper by Johns et al., 2011 and in Vichi et al., 2011) in the ES2 experiment simulations the land and ocean feedbacks do not affect the atmospheric concentrations of GHGs, aerosols and other atmospheric pollutants. It follows that the carbon cycle feedbacks are not allowed in the ES2 simulations and the fluxes from the land-

atmosphere and ocean-atmosphere components of the ESM are used to diagnose the implied (or ''allowable'') anthropogenic carbon emissions (Johns et al., 2011). As a consequence the description of land carbon uptake is not strictly necessary and removed from the paper together with Figure 1.
As suggested by reviewer, Figure 8 and 10 have been removed from the paper as showing redundant information.
The model description has been rationalized. The description of the land Surface has now been rewritten and shortened. On the other hand, the description of the coupling between land and atmosphere is added to the text as it is relevant for energy and water conservation purposes.
Following the requests of both reviewers, a detailed discussion of the characteristics of the model in term of water and energy conservation is added to the method section.

We are grateful to Dr. B.G. Liepert whose comments considerably improved the quality of the manuscript.

Responses to Specific Comments:

1.1) **p525/25: We wrote the Feichter et al. 2004 paper as overview paper and wrote a companion paper Liepert et al. 2004 specifically on precipitation changes under GHG.**

Thanks for the comment. In the revised paper, we added Liepert et al., 2004 to reference list and put it as the first reference to discussion in p525, line 25.

1.2) **p526/6-8: Again precipitation sensitivity is more extensively described in several other papers (see Wentz et al. 2007 for observation discrepancies with models. In Liepert and Previdi (2009) we explicitly showed that precipitation in coupled GCM is more than three times more sensitive to aerosols compared to GHGs forcing.**

Thanks for the comment. We added Wentz et al. (2007) and Liepert and Previdi. (2009) to reference list. In the revised manuscript, Liepert and Previdi (2009) is the main reference to discussion in p526/6-8 (page and lines refer to original submitted manuscript). As suggested by the reviewer the following text is added at page 526, line 8, of original submitted manuscript:
"Liepert and Previdi (2009) explicitly showed that precipitation in coupled GCM is more than three times more sensitive to aerosols compared to GHGs forcing"
Replaced
"Feichter et al. (2004) showed that the global precipitation sensitivity to a 1-K surface air temperature change is almost 3 times higher for aerosol forcing than GHG forcing."

Furthermore, the following discussion is subsequently added to the text at page 526, line 8, of original submitted manuscript:
"Furthermore, Liepert and Previdi (2009) applied a method to thermodynamically constrain global precipitation changes and showed that they are linearly related to the changes in the atmospheric radiative imbalance. The strength of this relationship is

controlled by the ratio of the change in global surface sensible heat-flux to the change in latent heat-flux (Liepert and Previdi, 2009)."

See answers to comments 1.6, 1.7 and 2.1 for further text added to the paper and linked to Liepert and Previdi (2009).

1.3) **p528: It is not clear why the land surface vegetation component of the model is described in such details here. It would be more interesting to list aerosol-climate interactions (emissions of natural aerosols) and greenhouse gas feedbacks in ocean and land.**

-Thanks for the comment. We agree with the reviewer, we put too many details on the SILVA model that were not necessary for this paper. The description of the land Surface has now been rewritten and shortened (See answer to comment 2.2). On the other hand, the description of the coupling with the atmospheric component is added to the paper, as it is relevant for energy and water conservation purposes (see answer to point 2.6).
The reason why we are putting some more details for SILVA is because it was described quite comprehensively in my PhD Thesis (Alessandri, 2006) but only partially accessible to the community through scientific peer-reviewed journals (Alessandri et al., 2007). Other components of the C-ESM were already extensively discussed in a companion paper by Vichi et al. (2011), which on the other hand refers to this paper under submission for the description of the land surface (See Vichi et al., 2011; page 1931, right column, 2nd paragraph).

-Alessandri A., 2006: Effects of Land Surface and Vegetation Processes on the Climate Simulated by an Atmospheric General Circulation Model. PhD Thesis in Geophysics, Bologna University Alma Mater Studiorum, 114 pp.

-Alessandri A., S. Gualdi, J. Polcher, e A. Navarra, 2007: Effects of Land Surface and Vegetation on the Boreal Summer Surface Climate of a GCM. J. Climate, 20 (2), 255-278.

-Concerning interaction and feedbacks between GHGs/aerosol and climate:
In the revised manuscript, we included detailed description of the implementation of the radiative forcing, and feedbacks of aerosols in the climate in the "method section" of the paper (page 529, bottom, of original submitted manuscript; see answer to comment 1.8 for this).

As explained in previous paper by Johns et al., (2011) and Vichi et al., (2011), in the ES2 experiment simulations the land and ocean feedbacks do not affect the atmospheric concentrations of GHGs, aerosols and other atmospheric pollutants. In fact, ESM are driven by GHG and air pollution forcing concentrations derived from runs of Impact Assessment Models (IAMs). This was performed following the new experimental design that has been proposed for the 5th IPCC assessment, which starts from benchmark concentration scenarios and aims to estimate the "allowable" anthropogenic emissions (Hibbard et al. 2007; Johns et al., 2011).
We thank the reviewer, the above information and explanation is very important, and it deserves to be repeated in this manuscript and not left for reference. The following text has been included at page 527 (line 7) of the original submitted manuscript:

"The ES2 experiment uses the new experimental design that has been proposed for the 5th IPCC assessment, which starts from benchmark concentration scenarios and aims to estimate the "allowable" anthropogenic emissions (Hibbard et al. 2007; Johns et al., 2011). It is important to note that in the ES2 simulations, the land and ocean feedbacks do not affect the atmospheric concentrations of GHGs, aerosols and other atmospheric pollutants. In fact, ESM are driven by GHG and air pollution concentration forcings derived from runs of Impact Assessment Models (IAMs). On the other hand ESMs produce time series of diagnostic fluxes from the land-atmosphere and ocean-atmosphere components that are consistent with the increasing concentrations and the consequent modeled climate. In this sense, the ESM record the implied (or ''allowable'') anthropogenic carbon emissions as a direct output of the experiment by subtracting to the specified atmospheric CO2 growth rate the diagnosed natural carbon fluxes from the model. The analysis and the comparison of the implied emissions from the ESM involved in the ES2 was the main aim of Johns et al. (2011) while details of the implied fluxes for the C-ESM are provided by Vichi et al. (2011)."

1.4) **p529/8-10: Why is land carbon uptake described and not the ocean carbon uptake as well?**

Thanks for the comment.
The carbon cycle feedbacks are not allowed in the ES2 simulations and the fluxes from the land-atmosphere and ocean-atmosphere components of the ESM are only intended to diagnose the implied (or ''allowable'') anthropogenic carbon emissions (Johns et al., 2011). As a consequence the description of land carbon uptake is not necessary here and removed from the paper together with Figure 1. Similarly the ocean carbon uptake is not described in the manuscript.
The text has now been modified in order to clearly inform the reader to refer to Vichi et al. (2011) for the comprehensive description of the implied (or ''allowable'') anthropogenic carbon emissions in the C-ESM simulations and the related contributions from ocean and land (See also answer to comment 1.3).

1.5) **p531-2, (5-10): It is not clear that the total potential energy budget of the atmosphere is the best approach here. Enthalpy changes are calculated in (6) but include only LP as source term of latent heating. A sink term LE of latent heating should also be included (see Peixoto/Oort). Sensible heating of the atmospheric column as defined in (7) is separated into a source term and a horizontal advection term. Again shouldn't there be a sink term?**

Thanks for the comment; we agree with the reviewer that the usage of energy balance was not sufficiently explained. In fact, "In eq.6 we are considering the latent heat release during precipitation (LP) as an internal energy source so that we can write energy equation for the atmosphere in a different form (see Peixoto and Oort, 1992). Specifically, the budget here is applied to the enthalpy in the atmospheric column (vertical sum of total potential energy). As a consequence, atmospheric content of latent heat is not part of the budget and latent heat release during precipitation goes to the right hand side of the equation."
Above text replaces what follows at page 532(top) of original submitted manuscript:

“Note that following Peixoto and Oort (1992), latent heating (LP) in Eq. (6) is considered as an internal energy source, which results from release of latent heat during precipitation.”

Furthermore, following the reviewer’s comment we now explain that $L\bar{E}_{\uparrow}$ is the net latent heat flux (positive upward) and comes from a balance between upward and downward flux components at the surface-atmosphere interface. The following sentence is added to the text of the original submitted manuscript (p532 line 7):
“Note that $L\bar{E}_{\uparrow}$ is the net balance between upward and downward flux components at the interface between surface and atmosphere and is here defined as positive-upward for convenience.”

Similarly, for sensible heating the following sentence is added to the text of the original submitted manuscript at page 531(line 23):
“Here $SH_{\uparrow}$ is the net balance between upward and downward flux components at the interface between surface an atmosphere and is here defined as positive-upward for convenience.”

See also answers to comment 2.4.

1.6) **In Liepert and Previdi (2009) we avoided these issues and unnecessary assumptions such as neglecting kinetic energy, by using the surface energy balance equation instead of the atmospheric total potential energy equation. We derive an equation similar to (10) by combining atmospheric moisture budget and surface energy budget. The one difference is: the change in sensible heat convergence is replaced by the change in latent heat convergence. (Note, in a global mean we consider latent heat convergence as small compared to evaporation and precipitation and neglected it in our equation 3. It can easily be added here.)**

“Note that our method is similar to the one in Liepert and Previdi (2009). However, the assumption that P(ΔP) must equal E(ΔE), that is at the base of their approach, applies only when considering global-mean annual climatology and it cannot be applied to regional domains. Differently, the method we propose is not limited to global means and so can be profitably applied to the analysis we further performed over global-land and global-ocean averages. However, our method is consistent with Liepert and Previdi (2009). In fact, it is easy shown that our eq. 10 reduces to eq.4 in Liepert and Previdi (2009) when considering global mean averages.”
What above is explained in the text at page p532(bottom) of original submitted manuscript.

1.7) **p533, (11): In Liepert and Previdi 2009 we also derive a modified Bowen ratio (here Bowen ratio potential) that is defined by: atmospheric radiative flux change, the moisture convergence change but not the sensible heating convergence change as equation 11. I am not convinced the sensible heat convergence change is correct in eq. 11. We interpret the distinctly different behavior of the modified Bowen ratio for an aerosol only, a GHG only, and a combined GHG and aerosol experiment with a fully coupled GCM. The modified Bowen ratio changes signs in our experiments, which is also**

mentioned in the text as bifurcation.

See also answer to comment 1.6.
The method we propose is not limited to global means and so can be profitably applied to the analysis we further performed comparing global land average and global ocean average. However, our method is consistent with Liepert and Previdi (2009). In fact, "our eq. 11 reduces to eq.5 in Liepert and Previdi (2009) when considering global mean averages."
What above added to the original submitted manuscript at page p533 (line 8).

The fact that we got Bowen Ratio bifurcating consistently to Liepert and Previdi (2009) is discussed in the text as follows (pag. 539, line 8, of original submitted manuscript):

"The Br bifurcation appears related to the different GHG and aerosol forcing in E1 and A1B. This is consistent to Liepert and Previdi (2009), who interpret the different behavior of the PBr for aerosol-only and GHG-only experiments with a fully coupled GCM. They found that, for the GHG-only experiment, the sensible heat flux trends tend to be anti-correlated to the latent heat flux changes, whereas for the aerosol-only case sensible and latent heat flux trends display positive correlations."

Above discussion is also included in the conclusions of the paper (see answer to comment 2.1).

1.8) **p533: The discussion of aerosol effects is difficult to comprehend from Figure 4 alone. It is even unclear whether Figure 4 includes natural sea salt aerosols. More information on natural aerosol feedbacks and anthropogenic aerosol forcing is needed. For example: Does natural aerosol burden change with increasing global warming, e.g. more forest fires?**

Thanks for the comment. We agree with the reviewer, further information and clarification of the implementation of the radiative forcings and feedbacks in the experiment was needed (see also answers to comments 1.3 and 1.11).
The following description has been added to the "method section" at page 529 (bottom) of original submitted manuscript:
"The GHGs (CO2, CH4, N2O, and CFCs) and sulphate aerosols prescribed during the historical run are the observation-based concentrations available for the ENSEMBLES multi-model experiment (Johns et al., 2011; http://www.cnrm.meteo.fr/ensembles/public/model_simulation.html). As described in Johns et al (2011), for the 21st century forcing we used the GHG concentrations from the SRES A1B (Nakicenovic and Swart, 2000) and E1 scenarios. E1 was specifically developed for ENSEMBLES with the IMAGE2.4 Integrated Assessment Model (Bouwman et al., 2006; van Vuuren et al., 2007) as an aggressive mitigation scenario designed to keep anthropogenic warming below 2K. The aerosols boundary conditions consist of two components: one constant in time from the climatology developed in Tanre et al. (1984) (hereinafter Tanre climatology) plus time dependent sulfate aerosols. The Tanre climatology distinguishes spatial distributions of sea, land, urban, and desert aerosols and contains constant background aerosols of tropospheric, stratospheric and volcanic type.

The time varying 3D concentration maps of sulfate aerosol for the historical, A1B and E1 scenarios were computed by running the chemistry-transport model (CTM; Bucher and Pham, 2002), i.e. the same model used to evaluate the SRES scenario sulfate concentrations applied to the IPCC AR4 models (Johns et al., 2011). In this regard, we report that the indirect effect of aerosol on clouds implemented in Echam5 (i.e: the atmospheric component of the C-ESM) distinguishes between maritime and continental clouds in the parameterization of precipitation formation, by considering the cloud droplet number concentration, in addition to the liquid water content (Lohmann and Roeckner, 1996). The cloud droplet number concentration is derived from the sulfate aerosol mass concentration following Lohmann and Roeckner (1996). The C-ESM simulations were performed without any variation in natural forcing (solar, volcanic and fires). This means that natural aerosol burden does not change with increasing global warming.
The ozone distribution from 1860 to 2100 is based on Kiehl et al. (1999), and includes the tropospheric ozone increase in the last decades, stratospheric ozone depletion and a simple projection for stratospheric ozone recovery applied to both both A1B and E1 scenarios."

Above text replaces what follows:
"For the historical run, the annual observation-based estimates that where made available for the ENSEMBLES project (Johns et al., 2011) have been used. For the future scenario experiments, the A1B SRES (Nakicenovic and Swart, 2000) and E1 scenarios have been used. E1 was specifically developed for ENSEMBLES with the IMAGE2.4 Integrated Assessment Model (Bouwman et al., 2006; van Vuuren et al., 2007) as an aggressive mitigation scenario designed to keep anthropogenic warming below 2K. Solar and Volcanic forcing variations were not accounted for in C-ESM simulations."

-Following suggestion of reviewer, figure 4 (figure 3 in the revised paper) has now been totally redesigned in order to make it easier to comprehend the discussion of aerosol effects. It is made clear now that only anthropogenic sulfate aerosol is time-varying (and of course not including sea-salt) by plotting both total sulfate (anthropogenic + natural) aerosol burden and natural (constant) sulfate aerosol burden in figure 4. In order to facilitate comprehension, only sulfate burden is plotted in revised version of figure 4, i.e. the forcing field that is prescribed in the model and not including anymore SO2 precursors (coming from CTM simulation; Bucher and Pham, 2002) as in previous version of the manuscript. See also answer to comment 1.11.

-List of additional references:

Boucher O, Pham M (2002) History of sulfate aerosol radiative forcings. Geophys Res Lett 29:1308. doi:10.1029/2001GL014048.

Kiehl, J. T., T. L. Schneider, R. W. Portmann, and S. Solomon, 1999: Climate forcing due to tropospheric and stratospheric ozone. J. Geophys. Res., 104, 31239–31254.

Lohmann, U. and Roeckner, E. (1996): Design and performance of a new cloud microphysics scheme developed for the ECHAM4 general circulation model . Clim. Dyn., 12, 557--572.

Tanré, D., J. Geleyn, and J. Slingo (1984), First results of the introduction of an advanced aerosol‐radiation interaction in ECMWF low resolution global model, in Aerosols and Their Climatic Effects, edited by H. Gerber and A. Deepak, pp. 133–177, A. Deepak, Hampton, Va.

1.9) **Fig.1: Not sure whether this figure is necessary.**

We agree with the reviewer. Figure 1 is not necessary and has been removed in the revised manuscript.

1.10) **Fig.2: Please see comments above. There is no sink term for latent and sensible heating in the atmospheric column.**

See answer to point 1.5
Following the reviewer's comment we now explain that $L\bar{E}_{\uparrow}$ is the net latent heat flux (positive upward) and coming as a balance between upward and downward flux components at the surface-atmosphere interface.
The following sentence is added to the text of the original submitted manuscript (p532 line 7):
"Note that $L\bar{E}_{\uparrow}$ is the net balance between upward and downward flux components at the interface between surface and atmosphere and is here defined as positive-upward for convenience."

Similarly, for sensible heating the following sentence is added to the text of the original submitted manuscript at page 531(line 23):
"Here $SH_{\uparrow}$ is the net balance between upward and downward flux components at the interface between surface an atmosphere and is here defined as positive-upward for convenience."

1.11) **Fig.4: This is only sulfur burden. How about carbonaceous aerosols and other anthropogenic aerosols? Does Fig.4 include sea salt aerosol? How about a figure for total (natural and anthropogenic) aerosol burden?**

-Thanks for the comment. We agree with the reviewer, further information and clarification of the implementation of the radiative forcing and feedbacks in the performed ES2 experiment was needed. See also answer to comments 1.3 and 1.8.
As reported for the answer of comment 1.8, a detailed description has been added to the "method section" at page 529 (bottom) of original submitted manuscript.

From the description added to the text, it is made clear now that only the time evolving concentration of anthropogenic sulfate aerosol is provided as the boundary forcing and there is no interannual change of carbonaceous or other anthropogenic or natural aerosols.

-Following the suggestions of the reviewer, figure 4 has now been totally redesigned in order to make it easier to comprehend the discussion of the aerosol effects. In the revised version of figure 4 we plotted both total sulfate (anthropogenic + natural) aerosol burden and natural sulfate burden contribution (constant magenta line in figure 4). See also answer to comment 1.8.

1.12) **Fig.5: In my opinion one of the key results of this study is that in the**

mitigation scenario E1 global warming can be constrained to 2K but precipitation continues to increase beyond 2070. This behavior could be shown more clearly in a separate figure.

We agree with the reviewer. Thanks for suggestion. A separate Figure 6 (Figure 5 of revised manuscript) reports zoomed 2030-2100 time series of the two scenarios for both temperature and precipitation.
The following discussion is added to the text:
"From figure 5 it is shown that E1 is effective in constraining global warning below 2K compared to the 1950-2000 historical period. More importantly, it is accomplished a stationary solution of the cumulative warming, with the E1 temperature curve achieving steadily zero derivative during the last 4 decades of the 21C. This is more clearly appreciated from figure 5 that is by zooming in on the 2030-2100 scenario time-frame for both temperature and precipitation. On the contrary, the E1 precipitation does not follow the temperature field toward a stabilization path but continue to increase beyond 2070. It follows an uncoupled behavior of temperature and precipitation in E1, which leads to divergence in the respective curves. In this respect, A1B behave very differently with the increase in temperature and precipitation that appear tightly related and exhibiting almost linear relation."

Above discussion is also reported in the conclusions of the revised manuscript (see also answers to comment 2.1):
"The mitigation scenario (E1) is effective in constraining global warming below 2K compared to the 1950-2000 historical period and with a stabilization by the end of the 21C. On the contrary, the hydrological cycle in E1 does not follow temperature towards a stabilization path and continue to increase over the mitigation period. Quite unexpectedly, the mitigation scenario strengthen the hydrological cycle even more than SRES A1B till around 2070, thus displaying the difference between E1 and A1B of the relationship between temperature and precipitation."

1.13) **Fig.8: The information here is redundant and is better presented in table 2.**

We agree with the reviewer. Figure 8 has been removed in the revised manuscript following reviewer's request. Any reference to the figure is deleted in the text or replaced with the reference to table 2.

1.14) **Fig.10: Again I think this information fits better in table 3.**

We agree with the reviewer. Figure 10 has been removed in the revised manuscript following reviewer's request. Any reference to the figure is deleted in the text or replaced with the reference to figure 11 and/or table 3.

1.15) **Fig.11: Please see comments above. I find this diagram more confusing than explanatory. Is there a better way to convene the message?**

See answers to points 1.5 and 1.10

Following the reviewer's comment we now explain that $L\bar{E}_{\uparrow}$ is the net latent heat flux (positive upward) and coming as a balance between upward and downward flux components at the surface-atmosphere interface.
The following sentence is added to the text of the original submitted manuscript (p532 line 7):
"Note that $L\bar{E}_{\uparrow}$ is the net balance between upward and downward flux components at the interface between surface and atmosphere and is here defined as positive-upward for convenience."

Similarly, for sensible heating the following sentence is added to the text of the original submitted manuscript at page 531(line 23):
"Here $SH_{\uparrow}$ is the net balance between upward and downward flux components at the interface between surface an atmosphere and is here defined as positive-upward for convenience."

Technical corrections

We modified the text according to the technical corrections suggested by the reviewer. Thanks for suggestions.

Responses to Reviewer #2
[05/10/2012]

-First of all, we would like to thank the reviewers for their work. We found their comments helpful and we think they have given a contribution in increasing the quality of this paper. We have modified the manuscript according to all the reviewers' requirements and suggestions and we believe that the revised manuscript is much improved. Please see below our detailed reply to the reviewers' comments.

-Please note that First Author, Andrea Alessandri, added International Pacific Research Center (IPRC) as its second affiliation other than ENEA. So the following two affiliations will compare in the revised manuscript:
-Agenzia Nazionale Per le Nuove Tecnologie, L'Energia e lo Sviluppo Economico Sostenibile
-International Pacific Research Centre, Honolulu, HI, USA

General Comment:
This study investigates the energy and hydrology changes associated with two future climate scenarios (i.e a low mitigation one, A1 versus an aggressive mitigation one E1), as defined in Johns et al. (2011). The authors try to understand the physical mechanisms behind the two different scenarios particularly in terms of the atmospheric energetics, the hydrological cycle and the links between them. This study gives a novel contribution by showing that for an aggressive mitigation scenario (E1) the hydrological cycle will continue to strengthen for most of the 21st century more than the low mitigation one A1. Although the description of the scientific methodology is a bit confusing, I recommend this paper for publication in ESD after some revisions, particularly of Sec. 2.3

Response to General Comment
We'd like to thank the reviewer for the useful comments. We followed all the reviewer recommendations and we think the discussion in the revised paper now much improved.
The scientific methodology to thermodynamically constraining global precipitation changes is clarified and explained following the reviewer requests.
Furthermore, it is explained that our method is similar to the approach in Liepert and Previdi (2009) and an important link with this previous work is established, further contributing in clarifying the methodology (see answers to reviewer #1).
The model description has been rationalized. The description of the land Surface has now been rewritten and shortened. Furthermore, we removed Figure 1 as well as the description of land carbon uptake since not strictly necessary for the paper and so not

included in the revised version. On the other hand, the description of the coupling between land and atmosphere is added to the manuscript, as it is relevant for energy and water conservation purposes.
We still put some more details for SILVA than the other model components because it was described quite comprehensively in my PhD Thesis (Alessandri, 2006) but only partially reported to the community through a previous scientific peer-reviewed paper (Alessandri et al., 2007). The other components of the C-ESM were already extensively discussed in a companion paper by Vichi et al. (2011), which on the other hand cross-refers to this paper for the description of the land surface.
As suggested by the reviewer, we modified the manuscript in order to make more clear the aims and new findings of this study by modifying abstract and introduction and strengthening the conclusions.
Following the requests of both reviewers, a detailed discussion of the characteristics of the model in term of water and energy conservation is added to the method section.
As recommended by the reviewer the English of the manuscript has been checked by a professional English edit service and we think it is much better now.

We are grateful to anonymous reviewer whose comments considerably improved the quality of the manuscript.

Responses to Specific Comments:

2.1) **Abstract/Introduction/Conclusion: I suggest the authors to make more clear what are the aims of this study in the abstract/Introduction and to strengthen the conclusions by stressing what are the new findings of this study. Of course this is somehow a subjective issue, but I found that there were a bit "hidden" in the text and this doesn't do the paper justice;**

Thanks for the comment. We think the reviewer is right and we modified the manuscript in order to make more clear the aims and new findings of this paper.
The abstract has been modified and the following text has been added at page 524, line 14, of the original submitted manuscript:
"Our results show that mitigation scenario effectively constrain the global warming with a stabilization below 2K with respect to the 1950-2000 historical period. On the other hand, the E1 precipitation does not follow the temperature field toward a stabilization path but continue to increase over the mitigation period. Quite unexpectedly, the mitigation scenario is shown to strengthen the hydrological cycle even more than SRES A1B till around 2070."
And replaces:
"Quite unexpectedly, mitigation scenario is shown to strengthen bydrological cycle more than SRES A1B till around 2070."

Introduction has been modified and the following text has been added at page 526, line 8, of the original submitted manuscript:
"Furthermore, Liepert and Previdi (2009) applied a method to thermodynamically constrain global precipitation changes and showed that they are linearly related to the changes in the atmospheric radiative imbalance. The strength of this relationship is controlled by the ratio of the change in global surface sensible heat-flux to the change in latent heat-flux (Liepert and Previdi, 2009)."

The following text at page 526, line 18, is added to summarize the aims of the paper:
"In this study, we evaluate the effect of the E1 mitigation scenario on the strengthening of the hydrological cycle by comparison to the SRES A1B scenario. The strength of the hydrological cycle is simply measured by taking the spatial-average of the precipitation rate in one of the Earth System Models (ESMs) participating to the ENSEMBLES centennial climate projection exercise (Johns et al., 2011). The reasons for the different precipitation changes during the E1 mitigation scenario compared to A1B are investigated by applying a method that is based on both water and energy conservation principles in the atmosphere. It is the first time that such kind of analysis is applied to a state-of-the-art ESM. The method is similar to the approach in Liepert and Previdi (2009) and the main difference is that it can also be applied to regional domains and not only to the global average."
Above text replaces:
"In this study, the strengthening of hydrological cycle is simply measured by taking the spatial-average of precipitation rate. By exploiting both water and energy conservation principles in the atmosphere, the strengthening of global-scale hydrological cycle is analyzed in one of the Earth System Models (ESMs) participating to the ENSEMBLES centennial climate projection exercise (Johns et al., 2011). The reasons for the different precipitation changes during E1 mitigation scenario compared to A1B are investigated in detail."

Conclusions has been modified and the following text has been added at page 539, line 14, of the original submitted paper:
The following text is added at page 539, line 14, of original submitted manuscript:
"The mitigation scenario (E1) is effective in constraining global warming below 2K compared to the 1950-2000 historical period and with a stabilization by the end of the 21C. On the contrary, the hydrological cycle in E1 does not follow temperature towards a stabilization path and continue to increase over the mitigation period. Quite unexpectedly, the mitigation scenario strengthen the hydrological cycle even more than SRES A1B till around 2070, thus displaying the difference between E1 and A1B of the relationship between temperature and precipitation."
Replaces:
"Compared to baseline (SRES A1B), mitigation scenario (E1) effectively reduces global warming and hydrology acceleration in the C-ESM by the end of 21st Century (21C). On the other hand E1 warms relative to A1B in the early 21C (until about 2050) and precipitation increases more in E1 than in A1B till nearly 2070, thus displaying the weakness of the relationship between global precipitation and temperature. "

The following text is added at page 541, line 14, of original submitted manuscript:
"Consistently to Liepert and Previdi (2009), bifurcation of the bowen ratio appears related to the different GHG and aerosol forcing in E1 and A1B. They showed that the forcing by GHG tends to produce changes in sensible heat flux that are anticorrelated to the changes in latent heat flux. On the other hand, for the aerosol forcing case the sensible and latent heat flux trends show positive correlation.
"

The following text (page 541, line 14, of original submitted manuscript):

“We show that mitigation policies by abating both anthropogenic GHGs and sulfate aerosols may obtain opposite effects on global temperature and precipitation, depending on the relative GHG and aerosol reductions. While they can obtain stabilization of global warming by the end of the 21C, we warn that the abatement of sulfate aerosols may lead to”
replaces:
“Importantly, we warn that mitigation policies, by abating aerosols, may lead to“

2.2) **Is there any reason to describe in such details the SILVA models with respect to the other components of the C-ESM model??**

Thanks for the comment. We agree with the reviewer, we put too many details on the SILVA model that were not necessary for this paper. The description of the land Surface has now been rewritten and shortened. Of particular relevance for energy and water conservation purposes (see answer to point 2.6), a description of the coupling with the atmospheric component has now been included in the description of the model.
The reason why we are putting some more details for SILVA is because it is a relatively new component of the C-ESM which was described quite comprehensively in my PhD Thesis (Alessandri, 2006) but only partially reported to the community through a previous scientific peer-reviewed paper (Alessandri et al., 2007). The other components of the C-ESM were already extensively discussed in a companion paper by Vichi et al. (2011), which on the other hand cross-refers to this paper (Alessandri et al., 2012; in preparation at that time) for the description of the land surface (See Vichi et al., 2011; page 1931, right column, 2nd paragraph).

In the following the revised description of the SILVA model, which replaces text at pages from 527(bottom) to 529(top) of original submitted manuscript:

“In the following we briefly summarize the characteristics of the SILVA model, which was only partially documented to the community through a previous scientific peer-reviewed paper (Alessandri et al., 2007). The other components of the C-ESM were already extensively discussed in a companion paper by Vichi et al. (2011), which on the other hand cross-refers to this paper for the description of the land surface. The SILVA model parameterize the flux exchanges at the interface between land-surface and atmosphere as described in Alessandri et al., (2007) and following the SECHIBA (“Schematisation des Echanges Hydriques a l’Interface entre la Biosphere et l’Atmosphere”, Ducoudre et al., 1993) scheme approach, while the Vegetation and Carbon dynamics is developed using the core parameterizations from VEgetation-Global-Atmosphere- Soil (VEGAS, Zeng et al., 2005). SILVA can fully integrate the vegetation-carbon dynamics mechanistically with the characteristics of four Plant Functional Types (PFTs): broadleaf tree, needleleaf tree, cold grass and warm grass; with the different photosynthetic pathways distinguished for C3 (the first three PFTs above) and C4 (warm grass) plants. Competition between C3 and C4 grass is a function of temperature and CO2 following Collatz et al. (1998). Phenology is simulated dynamically as the balance between growth and respiration/turnover. Competition is determined by climatic constraints and resource allocation strategies such as temperature tolerance and height dependent shading of each PFT. The terrestrial carbon cycle starts with photosynthetic carbon assimilation in the leaves and the allocation of this carbon into three vegetation carbon pools: leaf, root and wood. After accounting for respiration, the biomass turnover from these three vegetation carbon pools cascades into a fast soil carbon pool, intermediate pool and a slow pool. Temperature and moisture dependent decomposition of these pools returns carbon back into atmosphere, thus closing the terrestrial carbon cycle. A natural fire module includes the effects of moisture availability, fuel loading, and PFT dependent resistance to combustion. As already discussed, the ES2 experiments do not allow the land and ocean feedbacks to the carbon cycle (Johns et al., 2011) so that, in the simulations analyzed in this study, the land carbon fluxes do not affect the atmospheric the atmospheric concentrations of GHGs and other atmospheric pollutants."

-Alessandri A., 2006: Effects of Land Surface and Vegetation Processes on the Climate Simulated by an Atmospheric General Circulation Model. PhD Thesis in Geophysics, Bologna University Alma Mater Studiorum, 114 pp.

-Alessandri A., S. Gualdi, J. Polcher, e A. Navarra, 2007: Effects of Land Surface and Vegetation on the Boreal Summer Surface Climate of a GCM. J. Climate, 20 (2), 255-278.

2.3) **Fig.3 and 4 introduced in the text before Fig.2;**

Thanks for the comment. Figure 2 is moved to #4 so that Figs 2, 3 and 4 now are introduced in the text in the right order. Note that Fig.1 of the original submitted manuscript has been deleted in the revised version, so that Figs.2,3 and 4 become Figs.1,2 and 3 in the revised manuscript.

2.4) **Section 2.3. This section, describing the water/energy equations should be dealt with more carefully. In Eq. (2) Q is a vector as V, isn't it? In eq. (5) what you call "E_int" is, as far as I understand, the total potential energy ? Why can you neglect changes in kinetic and potential energy? Please be a bit more accurate on these points and check the equations in the literature, e.g. Peixoto/Oort. Also I'd avoid the nabla symbol to represent something which is not what it really is: a differential operator;**

Thanks for the comment. We agree with the reviewer and we modified section 2.3 according to reviewer requests. In the following the list of changes following the reviewer requests:

-Description of eq (2) has been modified making clear that both Q and V are vectors (page 530, line 8, of original submitted manuscript):
"Q is the vertically integrated (from the Earth's surface to the top of the atmosphere) horizontal transport vector of water vapor"
replaces:
"Q is the vertically integrated (from the Earth's surface to the top of the atmosphere) horizontal transport of water vapor"

At page 530, line 11, of original submitted manuscript:
"where V is the horizontal wind vector"

replaces
"where V is the wind"

-Actually E_int is the internal energy and not total potential energy. This has been clarified in the text also using the same notation as in Pexioto and Oort (1992, Chapter 13, pag.308) as suggested by reviewer.
Description of eq. (5) is modified as follows (page 531, line 8, of original submitted manuscript):
"Further constraint to the hydrological cycle comes from the principle of conservation of total potential energy which is defined as the sum of Internal energy and gravitational-potential energy (Peixoto and Oort, 1992)"
replaces
"Further constraint to the hydrological cycle comes from the principle of conservation of total potential energy"

Internal energy is reported in eq.5 with "I" instead of "E_int" using the same notation as in Peixoto and Oort (1992; see pag 308).

At page 531, line 13, of original submitted manuscript:
"Where I is the internal energy and ϕ is the gravitational-potential energy;"
replaces
"where **E**int and ϕ are internal and gravitational-potential energy"

-At page 531, line 9, of original submitted manuscript it is explained that:
"We are not considering kinetic energy here, since the related changes vanish when considering annual-mean climatologies over a long period of time (Peixoto and Oort, 1992)" (from Peixoto and Oort, 1992 page 319, top)
above text replaces
"(we neglect here changes in kinetic energy)"

-Concerning the changes in total potential energy, at page 532, line 11, of the original submitted manuscript:
"For annual-mean climatologies considered over a long period of time, we can assume the tendency term to vanish (e.g. Peixoto and Oort, 1992; Trenberth et al., 2001)" (from Peixoto and Oort, 1992 page 319, top)
Replaces:
"By analogy to the water balance equation it is generally reasonable to assume that the tendency term is small when considering climatological annual-mean basis (e.g. Peixoto and Oort, 1992; Trenberth et al., 2001)."

-Following the suggestion of the reviewer the "Nabla" symbol is not used anymore to indicate Top minus Surface Radiative fluxes:
"Snet"
replaces
"$\nabla_z \cdot S$"
"Tnet"
Replaces
"$\nabla_z \cdot S$"
At page 531, line 19, of the original submitted manuscript:
"here Snet and Tnet corresponds to top of troposphere minus surface downward

radiative-fluxes for S and T, respectively;”
replaces
“here $-\nabla_z$ corresponds to top of troposphere minus surface downward radiative-fluxes;”

2.5) **In eq. (6) you consider as the same the meridional transport (of dry static energy?) and the vertical turbulent fluxes at the surface. Since they are associated with very different processes (large scale horizontal transport vs. small scale surface layer turbulence), it would be helpful to comment on this;**

We agree with the reviewer, we clarified that the two components contributing to sensible heating come from different processes, which involve very different spatial scales. The following text is added at page 531, line 20, of the original submitted manuscript:

“SH stands for atmospheric column sensible heating and as reported in eq. (7) it can be further decomposed into two components: one is the turbulent flux of sensible heat at the surface ($SH_{\uparrow}$) that take place at very small spatial scales. On the other hand, the convergence term from atmospheric horizontal transport ($-\nabla_h \cdot SH$) is characterized by the large-scale dynamics.”
Above text replaces:
“SH stands for atmospheric column sensible heating and can be further decomposed into sensible flux component from the surface and atmospheric convergence component:”

Furthermore, the usage of energy balance (eq.6) is further explained in the text (see also answer to comment 1.5).

2.6) **The authors assume that the model conserves energy and water when they say that the time mean is practically zero; GCMs can unfortunatelly have biases in the energy and water budget. Have they checked how well the C-ESM model conserves these quantities?**

Thanks for the comment. The discussion of the characteristics of the model in term of water and energy conservation is added to the model section (page 527, line 23, of the original submitted manuscript). We think this further improves the quality of the paper. Added text is reported in the following:

“When considering atmospheric energy and water budgets it is of particular importance that the CGCM satisfy the basic conservation principles. As discussed in details in Fogli et al., 2009, the C-ESM model has been carefully checked for energy and water conservation and to this aim it implements on-line procedures aimed at ensuring conservation of the exchanged fluxes at the interface between surface and atmosphere. Recent studies have shown that some models have deficiencies in conserving energy and water at the interface between surface and atmosphere (Lucarini et al., 2007; Lucarini and Ragone, 2011; Liepert and Previdi, 2012). In particular, Lucarini et al. (2007) showed that imperfect closure of the energy cycle may lead to severe inconsistencies in some land models. As explained in Polcher et

al., (1998) and in Alessandri et al.,(2007), these land models use semi-implicit or explicit coupling numerical scheme at the interface between surface and atmosphere which does not ensure conservation of fluxes. This is due to the fact that within these schemes the solution of the surface energy balance equation by the land is done after the vertical diffusion parameterization is performed through the boundary layer. To obtain flux conservation at the interface, in the C-ESM model the coupling between SILVA and ECHAM5 is achieved by means of a fully implicit coupling numerical scheme for the energy and water fluxes. This scheme has been implemented with a flux conserving "Neumann closure" of the atmospheric vertical diffusion at the surface (Polcher et al. 1998; Alessandri et al. 2007), which allows for the simultaneous solution of both the surface balance equations and the closure of the turbulent fluxes in the boundary layer (Polcher et al., 1998; Alessandri 2006; Alessandri et al., 2007).
As discussed in detail in Fogli et al., 2009, the conservation of energy exchanged between the atmosphere and the ocean is ensured by virtue of a two steps procedure: first the global integrals of the fluxes over the open water domain of the atmospheric model are computed. Thereafter, the global integrals seen by the atmospheric model are used to eventually correct the fluxes received by OPA8.2. A similar procedure is applied in order to obtain water conservation over Ocean. However, it is noted here that in the present version of the model, the water cycle between land and ocean is not closed, because the river-routing scheme is not implemented in current version of the atmospheric model. Therefore, the conservation of the water mass is imposed after including a climatological river runoff."

J.Polcher, B. McAvaney,P. Viterbo,M.-A. Gaertner,A. Hahmann, J.-F. Mahfouf, J. Noilhan,T. Phillips, A. Pitman,C.A. Schlosser,J.-P. Schulz,B. Timbal,D. Verseghy and Y. Xue (1998) A proposal for a general interface between land-surface schemes and general circulation models, Global and Planetary Change, 19 (1998) 261-276.

Lucarini, V., R. Danihlik, I. Kriegerova, and A. Speranza, 2008: Hydrological cycle in the Danube basin in present-day and XXII century simulations by IPCCAR4 global climate models, J. Geophys. Res., 113, D09107.

V. Lucarini, F. Ragone, Energetics of Climate Models: Net Energy Balance and Meridional Enthalpy Transports, Rev. Geophys. 49, RG101 doi:10.1029/2009RG000323 (2011)

Liepert, B. G., and M. Previdi, 2012: Inter-model variability and biases of the global water cycle in CMIP3 coupled climate models. Environ. Res. Lett., 7, 014006.

2.7) **Finally I have to point out the sloppiness of the English and the presence of too many minor English mistakes (e.g. "changes in whole atmospheric.." / changes in the whole; "last decade of 21st century"/ last decade of the 21st century and so on), which makes the manuscript painful to read and sometimes even fairly irritating. Therefore I suggest the authors to ask a native english speaker to check the English before resubmitting**.

We agree with the reviewer. The English of the manuscript has now been checked by the professional English edit service here at the School of Ocean and Earth Science and Technology (SOEST, University of Hawaii) and we feel it is much improved now. Thanks for the suggestion.

(Attached you can find the manuscript with all the English-edit track changes, as returned by the English edit service)