MS No.: ESD-2015-59
Title: Observationally based analysis of land–atmosphere coupling
Author(s): F. Catalano, A. Alessandri, M. De Felice, Z. Zhu, and R. B. Myneni

Responses to Reviewers #1 and #2

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 addressing 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.

Responses to Reviewer #1

[13/11/2015]

General comment

The paper "Observationally based analysis of land–atmosphere coupling" used the CM method to investigate the coupling relationships between soil moisture and precipitation, as well as other variables. It has some merits and presents interesting results, especially those linking EOF signals to external forcings such as ESNO and volcanic events.

The scientific question proposed regarding to land-atmosphere coupling is worth investigating, but the key content discussed in the paper is the coupling of soil moisture and precipitation, which is far form enough to cover this topic.

The authors did a lot of analysis, but unfortunately the paper is not well organized. Important information on method section is incomplete and makes it difficult to understand the following analysis and results. Overall, the presentation is not satisfactory and sometimes confusing. It lacks logic and as a reader I get lost in the too much descriptive details without a clear focus. Many key concepts like "variance" haven't been clearly defined. And language and wording is another issue that has to be significantly improved for clarity and accuracy. Although a number of Tables/figures are provided, but many of them are not very useful, they feel less informative and even hard to understand. The authors need to carefully decide how to best present their results.

It seems soil moisture does not paly a strong role as the authors claimed, because the variance of precipitation explained by soil moisture is less than 20%. Therefore, the influences of volcanic AOD as well as ENSO on PRE are relatively weak signals in general. More importantly, correlation does not necessarily mean causality or feedback, the author cited a lot of reference to explain their results, but there isn't enough information to evaluate if their proposed explanations are plausible and credible. And these discussions are mixed together with results, distracting the flow of the paper. A separate and refined discussion could be much better than current layout.

Datasets are not independent and are correlated/coupled to each other. For example, both ET and LAI are based on AVHRR. Perhaps due to this reason, precipitation forced by soil moisture is nearly equivalent to other (ET and LAI). And there are many coupling left unaccounted, e.g., soil moisture is forced by ET.

Therefore, significant efforts have to be made by the authors to address those issues relating to presentation, organization and readability of the paper before publication.

Response to general comment

We'd like to thank the reviewer for the useful comments. We revised the manuscript by addressing all his/her recommendations and we think that the revised manuscript is now much improved.

The method section has been improved to include all the relevant information and definitions, as detailed in the answers to specific comments.
The finding that, globally, 19% of precipitation (PRE) variance is forced by soil moisture (SM) indicates a significant contribution of SM on PRE variability, as also pointed out by Reviewer #2. Furthermore, locally, the ratio of PRE variance forced by SM is even larger and up to more than 30% (Fig. 2 in the paper). The identification of such hotspot regions is in good agreement with Koster et al. (2000).
We agree that correlation does not necessarily mean causality. The Coupled Manifold (CM) technique has been specifically designed to analyze covariation between climate fields considering both the local and remote forcing of one field to the other and has proved to be successful for the analysis of different climate fields, like precipitation, vegetation characteristics, sea surface temperature, and temperature over land (Alessandri and Navarra, 2008; Cherchi et al., 2007; Wang et al., 2011). To improve the robustness of the analysis we applied significance tests to the computation of the forced fields (following Cherchi et al., 2007). When discussing the feedbacks between the variables we combined the CM statistical results with a physical interpretation and literature results. We have improved the discussion of our results and how they are supported by literature in Section 4.1. Please see answer to minor comment 6 for more details on the advantage of CM with respect to other methods and 14 for the significance test applied in the computation of the forced fields. See also answer to general comment of Reviewer #2.
All land-surface datasets (SM, evapotranspiration [ET], Leaf Area Index [LAI]) are satellite products independent on the PRE dataset, which is based on rain gauges. It is true that both ET and LAI products have been acquired by using the AVHRR sensor but the datasets have been produced by independent research groups which used completely different methodologies. The LAI product has been generated by applying a neural network algorithm (Zhu et al. 2013) on the NDVI satellite product. The ET dataset has been produced by using a modified Penman-Monteith approach (Zhang et al. 2010) and considering eddy covariance and meteorological data from the FLUXNET towers network. This has been reported and discussed in Section 2 of the revised manuscript.
We are aware that there are interesting couplings which were not analyzed in this paper (for example, ET forcing on SM). Nevertheless, since SM has been recognized as the most important land-surface parameter affecting seasonal to interannual variability/predictability of precipitation (Koster et al., 2000; Zhang et al., 2008) we choose to focus the paper on the coupling between SM and PRE. Future papers will further address the specific coupling contribution with other fields. This has been added to Section 5 of the revised paper.

Minor comments

1) P1940 L2-3: Does the word "variance" here mean spatial or temporal variance? Need to specify.
Thank you for the comment. It is temporal variance. Indeed, the CM technique is applied to the principal components (PC) of the variables which represent the seasonal-mean inter-annual anomalies. The sentence at P1940 L2-3 has been changed in the paper as follows:
original:
"The variance of soil moisture, vegetation and evapotranspiration over land has been recognized to be strongly connected to the variance of precipitation."
new:
"The temporal variance of soil moisture, vegetation and evapotranspiration over land has been recognized to be strongly connected to the temporal variance of precipitation."

2) L6: what does "memory" means here? I don't get it.
Thanks. The term "memory" refers here to the property of some variables characteristics of slowly

varying components of the Earth system to display persistent anomalies induced by climatic events like ENSO or volcanic eruptions (Koster et al., 2000). Since slowly varying states of the land surface can be predicted weeks to months in advance, the response of the atmosphere to these land-surface anomalies can contribute to seasonal prediction (Alessandri and Navarra, 2008). This explanation has been added to Section 1 of the revised paper. See response to comment 3.

3) P1941 L11 Please explain "soil moisture memory".
Thanks for the suggestion. Please see response to point 2. The expression "soil moisture memory" has been used in literature (Koster et al., 2004; Ferranti and Viterbo, 2006). To better clarify the expression, the following phrase has been added to Section 1 of the revised manuscript:
"The term "soil moisture memory" refers here to the property of soil moisture to display persistent anomalies induced by climatic events like ENSO or volcanic eruptions. Since slowly varying states of the land surface can be predicted weeks to months in advance, the response of the atmosphere to these land-surface anomalies can contribute to seasonal prediction."

4) L22 "improvement" of what?
Thanks. To clarify it, the phrase has been modified as following:
original:
"However, much of the improvement so far has been obtained over ocean "
new:
"However, much of the model improvements so far have been obtained over ocean..."

5) P1942L9 what does "land variability" indicate here?
The term "land variability" indicates here the seasonal-mean inter-annual variability of land-surface variables (SM, ET, LAI). The phrase has been modified in the text as following:
original:
"The comprehensive dataset is analysed to characterize the land variability and..."
new:
"The comprehensive dataset is analysed to characterize the seasonal-mean inter-annual of land-surface variables (SM, ET, LAI) and..."

6) L13-L20 what is advantage of CM method compared to other methods?
Thank you for the question. The following phrase has been added to Section 3 of the revised manuscript to explain the advantage of CM with respect to other methods:
"There are two main advantages of the CM method. The first one is that, when applied to a couple of climate fields (i.e., PRE and SM), CM is able to separate one field (i.e., PRE) into two components: the first component (forced) is the portion of PRE variability that is connected to the SM variability, whereas the second (free) is the part of PRE that is independent from SM. Therefore, the CMT enables to find robust relations between fields in the presence of strong background noise. The second advantage is that the CM technique is able to detect both local and remote effects of the forcing variable. This is not possible with other methods such as SVD (Singular Value Decomposition)."

7) P1943 L4 It is strange for me to see the claim that those datasets are "state-of-art". For example, there are many alternative precipitation, ET and LAI datasets and it is hard to say one is better than the other without rigorous comparison. The data used here are far from "state-of-art". As far as I know, GLEAM ET and GLASS LAI are also high quality products.
We agree with the reviewer that there exist other high quality datasets of ET, LAI and PRE. A rigorous comparison of recent land-surface datasets is well beyond the scope of our paper. We based the choice of the datasets for our analysis mainly on two criteria: 1) the period covered has to be as

long as possible; 2) the spatial coverage has to be global. We have discussed this in Section 2 of the revised paper. In order to avoid confusion, in the revised manuscript we changed the term “state-of-the-art” with “high quality”.

8) L20 Please briefly explain the gap filling procedure used.

Thanks. The procedure is described in Section 3 of the manuscript, as follows:

“The LAI and SM datasets contain missing values, whose number and position significantly vary with time. The application of the CM algorithms requires that the number and position of the missing values is constant with time. Hence, if a NaN is present in a given grid-point at any time, then it requires to mark as NaN that grid point, thus losing a great amount of information. In order to keep as much information as possible from the data, we decided to replace the missing values with climatological values provided that their total number, considering a particular grid-point, does not exceed a given threshold. We selected different thresholds for SM and LAI in order to obtain as similar as possible spatial coverage of the two variables. The chosen threshold is 10 % for LAI and 30 % for SM. The results are robust with respect to a ±10 % change of the threshold values.”

The following sentence which points to the explanation is further added to Section 2 of the revised manuscript:

“The gap filling procedure is described in Section 3.”

9) L22 The use of model information weakens the previous claim that these observation data are independent of models.

We agree with the reviewer that the observational datasets have been derived based on limitations and constraints. The use of model information for ET and PRE datasets is declared in Section 2. To clarify the point raised by the reviewer, we have modified the following sentence at the beginning of Section 2 of the revised manuscript:

original:

“...in order to make the analysis as much as possible independent from numerical model limitations and biases...”

new:

“...in order to make the analysis as much as possible independent from global circulation models limitations and biases...”

10) Table 1. Better to add Ref for each dataset shown in the table; and spell out ET, LAI, GPCP etc. or explain them in notes.

We appreciate the suggestion. In the revised manuscript we have recalled the dataset references in the table caption. We have also added the extended name of the datasets in caption.

11) P1944 The authors at least need to describe the mathematical theory of CM (Eq. 1 and 2) and explain how it works to decompose a field into forced and free components.

Thanks. The mathematical theory of CM is described in detail in the cited reference of Navarra and Tribbia (2005). In order to better explain the rationale of the CM method we added the following sentence to Section 3 of the revised manuscript:

“The linear operators A and B are found by solving the Procrustes minimization problem:

A=ZS'(SS')^-1 (3)

B=SZ'(ZZ')^-1 (4)”

Following equations have been renumbered accordingly.

12) L18 what is CCA scaling?

Thanks. CCA scaling is data scaled by the covariance matrices. This is now explained in Section 3 of the revised manuscript, by changing the following sentence:

original:
"CCA scaling is applied to the Principal Components..."
new:
"CCA scaling (data scaled by the covariance matrices) is applied to the Principal Components..."

13) Since Eq 3 and 4 are listed in method, the authors must explain their meanings. What are Zˆ and Sˆ? And I don't understand why Eq 3 and 4 are needed here?
We appreciate the suggestion. Z^ and S^ are the CCA-scaled variables. Please see also response to comment 12. Eqs. 3 and 4 are the mathematical expression of the CCA scaling. This has been specified in the revised manuscript by adding the following sentence after eq. 4 (eq. 6 of the revised paper):
"where Z^ and S^ are the CCA-scaled variables."
For further details on the CCA scaling technique we refer to Navarra and Tribbia (2005).

14) P1945 L1-3. How to understand significance level for each element in A and B?
Thanks. This has been better explained in the revised manuscript, by adding the following sentence:
"As explained in Cherchi et al. (2007), after applying the CCA scaling, the elements of A and B are correlation coefficients and can be tested (with a significance test based on the Student t distribution) to reject the null hypothesis of being equal to zero."
The following sentence is further modified in the revised manuscript:
original:
"...at the 1 % level significance level..."
new:
"...at the 1 % significance level..."

15) L6-8 Why not to follow the common practice to define four seasons as JJA, SON, DJF and MAM? The incompatible definition for season would make this study incomparable with most other studies.
Thanks for the comment. We chose this definition for seasons because most of the datasets we use start on January and with the DJF, MAM, JJA, SON stratification we would have had to discard the first incomplete winter season and because the JFM, AMJ, JAS, OND stratification has been used by Alessandri and Navarra (2008) in their CM study of vegetation and rainfall which we used to compare our results. We have discussed this by adding the following sentence in the revised manuscript:
"The JFM, AMJ, JAS, OND stratification has been used by Alessandri and Navarra (2008) in their CM study of vegetation and rainfall which we will use to compare our results.

16) L10-20. It seems this paragraph describes the gap filling method?
Thanks. We now point to this paragraph in Section 2 of the revised manuscript. Please see response to comment 8.

17) L23 EOF is an important part for the analysis but has never been explained previously in the method section.
Thank you for the suggestion. We have modified the sentence in the revised manuscript to include EOF definition:
original:
"interpretation of the EOF patterns"
new:
"...interpretation of the Empirical Orthogonal Functions (EOF) patterns (Bretherton et al., 1992)..."
The following reference has been added to the revised manuscript:

Bretherton, C. S., Smith, C., and Wallace, J. M.: An intercomparison of methods for finding coupled patterns in climate data. J. Climate, 5, 541-560, 1992.

18) P1946 L3 Again, what is "variance" here? Is it temporal or spatial variance?
Thanks for the comment. It is temporal variance. Please also see response to comment 1.

19) L1-L14. It seems all these variables are coupled with each other in multiple ways, e.g., ET and LAI, LAI and SM, and ET and SM are correlated/interconnected but not analyzed here. According to Table 2, LAI and ET have similar role on PRE compared to SM. Despite PRE, LAI and ET are also important drivers for SM and may also accounts for a fraction of variance of SM.
We agree with the reviewer that it would be interesting to analyze in detail the reciprocal forcings between ET and LAI, LAI and SM and ET and SM. Nevertheless, since SM has been recognized as the most important land-surface parameter affecting seasonal to interannual variability of precipitation (Koster et al., 2000; Zhang et al., 2008) we choose to focus the paper on the coupling between SM and PRE. We plan to write a follow-up paper taht will further address the specific coupling contribution with other fields. This has been added to Section 5 of the revised paper.

20) P1947 L1-3 how to define transitional regions, are there any quantitative criteria for that?
There are no unique quantitative criteria. Here we refer to the the transition zones between very dry and very humid environments, where ET is very sensitive to SM, as individuated by Koster et al. (2000). This has been specified in Section 4.1 of the revised manuscript.

21) Table 3 uses Rainfall but in the text that is PRE. The use of term must be consistent throughout the paper to avoid possible confusion. And NINO3 in the table should be explained.
Thanks. We changed "rainfall" to "PRE" in table caption and we recalled in the caption the definition of the NINO3 index "(average of the Sea Surface Temperature in the tropical Pacific region 5◦ S–5◦ N, 210–270◦ E)".

22) L6 In table 2, SM only accounts for 17% variance of PRE, why EOF shows 48% of total variance in table 3?
In table 2, SM accounts for 19% variance of PRE. In table 3, we list the variance explained by each mode of the PRE forced field. 48% is the variance explained by the first 3 modes of PRE forced by SM.

23) L12 All data used including AOD should be descripted in method section.
Thanks for the comment. We added a brief description of the AOD and SST datasets at the end of Section 2 of the revised manuscript, as suggested also by Reviewer #2.
"In order to evaluate the effect of major volcanic eruptions on land-atmosphere coupling, we used the stratospheric Aerosol Optical Depth (AOD) at 550 nm, available from the NASA GISS dataset (Sato et al., 1993). To evaluate the effect of ENSO, we compute the NINO3 index based on the HadISST 1.1 – Global sea-Ice coverage and Sea Surface Temperature (1870–present; Rayner et al., 2003) dataset."
The following reference has been added to the revised manuscript:
Sato, M., Hansen, J. E., McCormick, M. P., and Pollack, J. B.: Stratospheric aerosol optical depth, 1850-1990. J. Geophys. Res. 98, 22987-22994, 1993.

24) I am confused by Table 3 and Table 4 that show different variance explained by PCs.
Table 3 shows the variance explained by each EOF mode of the PRE component which is forced by SM. On the other hand, Table 4 reports the variance explained by each EOF mode of the whole original PRE field (that is, forced+free components). The sentence has been added to the revised

manuscript.

25) P1950 L18-24 and Table 5: How to understand "PRE forced by SM and ET(LAI)". Do they mean PRE forced by SM is further decomposed into two parts - forced by ET and free? The corresponding text is not clear enough to correctly get the meaning. My understanding is that in this case, 17% x 20% = 3.4%, does it suggest only 3.4% PRE variance is explained by ET? ET and LAI are closely related especially in vegetated areas as the calculation of ET may have used LAI. This can be seen in the similar distribution of identified hotspots.

Thanks for the comment. We modified the following sentence in the revised manuscript to clarify the procedure:

original:

"...we applied the CM technique between PRE forced by SM and ET (LAI)..."

new:

"...we further applied the CM technique between the components of PRE forced by SM and ET (LAI)..."

We found that 19% of PRE variability is forced by SM (Table 2). On the other hand, ET explains 20% of the variability of PRE forced by SM. We further added the following sentence to the revised paper:

"It is important to note here that 19%x20%=3.8% represents only the ET forcing on PRE mediated by SM and not the whole ET forcing on PRE which is actually 18% (Table 2)."

26) P1951 The analysis in the second paragraph is very difficult to follow! And I don't quite understand the explanations that linking ET to AOD.

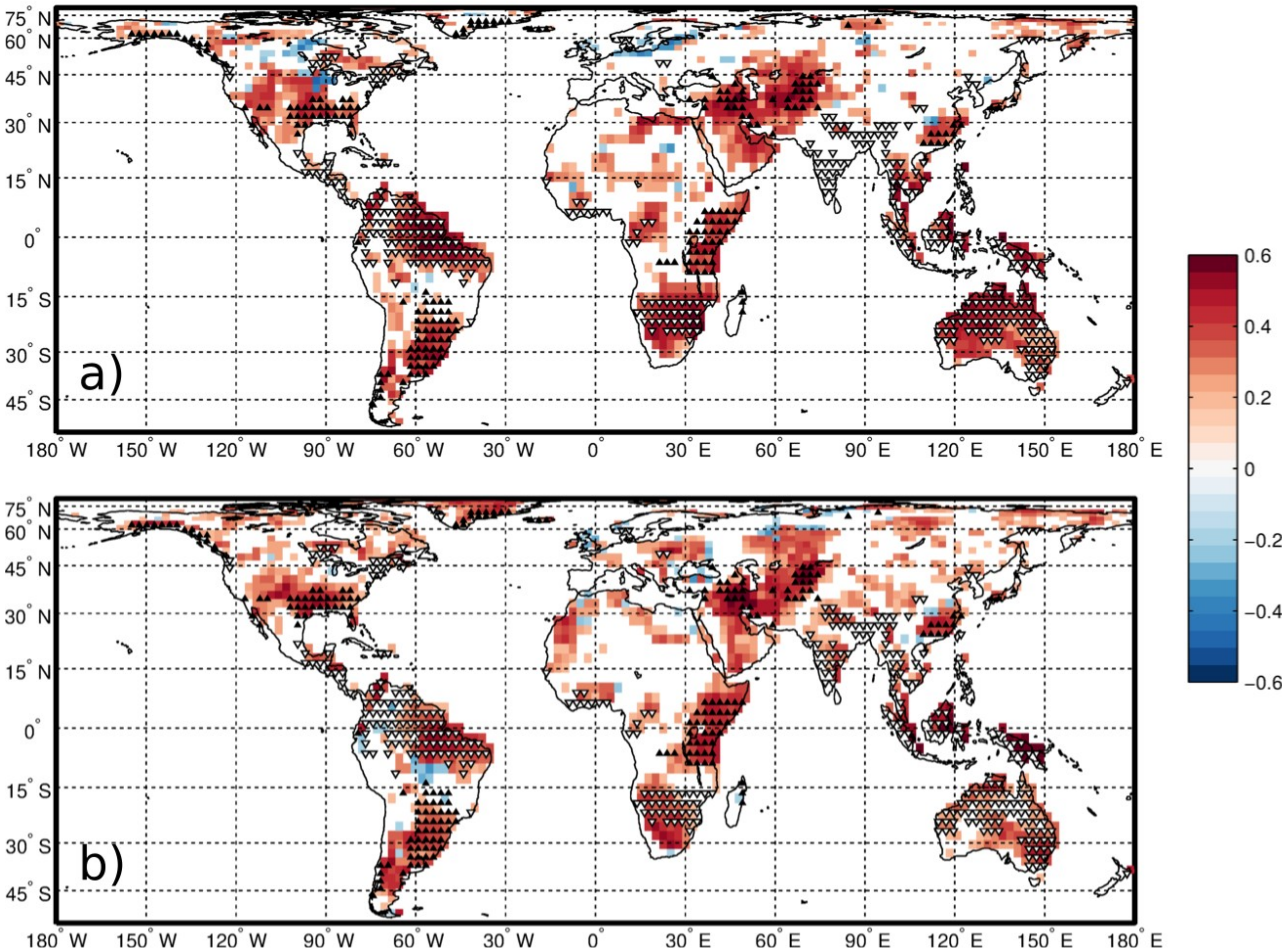
We have added the following sentence in the third paragraph of Section 4.2, after "between each of the physical fields corresponding to the first three modes of variability of PRE forced by the SM and ET (LAI) ":

"Here we take the physical fields corresponding to the first three modes of variability of PRE forced by SM and further decompose them to extract the parts of each mode that is forced by ET and LAI, respectively. This analysis allows to figure out how ET and LAI contribute to each component of PRE forced by SM which has been identified to be linked to external climate forcing (volcanic eruptions, ENSO and a trend)."

Eventually, we apply the CM to find the components of PRE forced by ET and LAI, respectively (Table 7) and we show that we can find the same link with external climate forcings in the EOFs. This confirms the robustness of the signals we found in the EOFs of PRE forced by SM and the lagged correlation with AOD shown in Table 7 indicates that ET and LAI contribute to extend the land-surface "memory" of the volcanic eruptions. In the case of ET, we found that this variable explains 21% of the variance of the first mode of PRE forced by SM (linked to AOD). Volcanic signal is found also in the third PC of PRE forced by ET, indicating a mediation role of ET in the response of PRE forced by SM to volcanic eruptions.

27) The direction of triangles in Figure 7 is almost indistinguishable without zooming.

Thanks for the comment. We have improved Figs. 7, 8, 9a,b by using larger markers.



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−0.2
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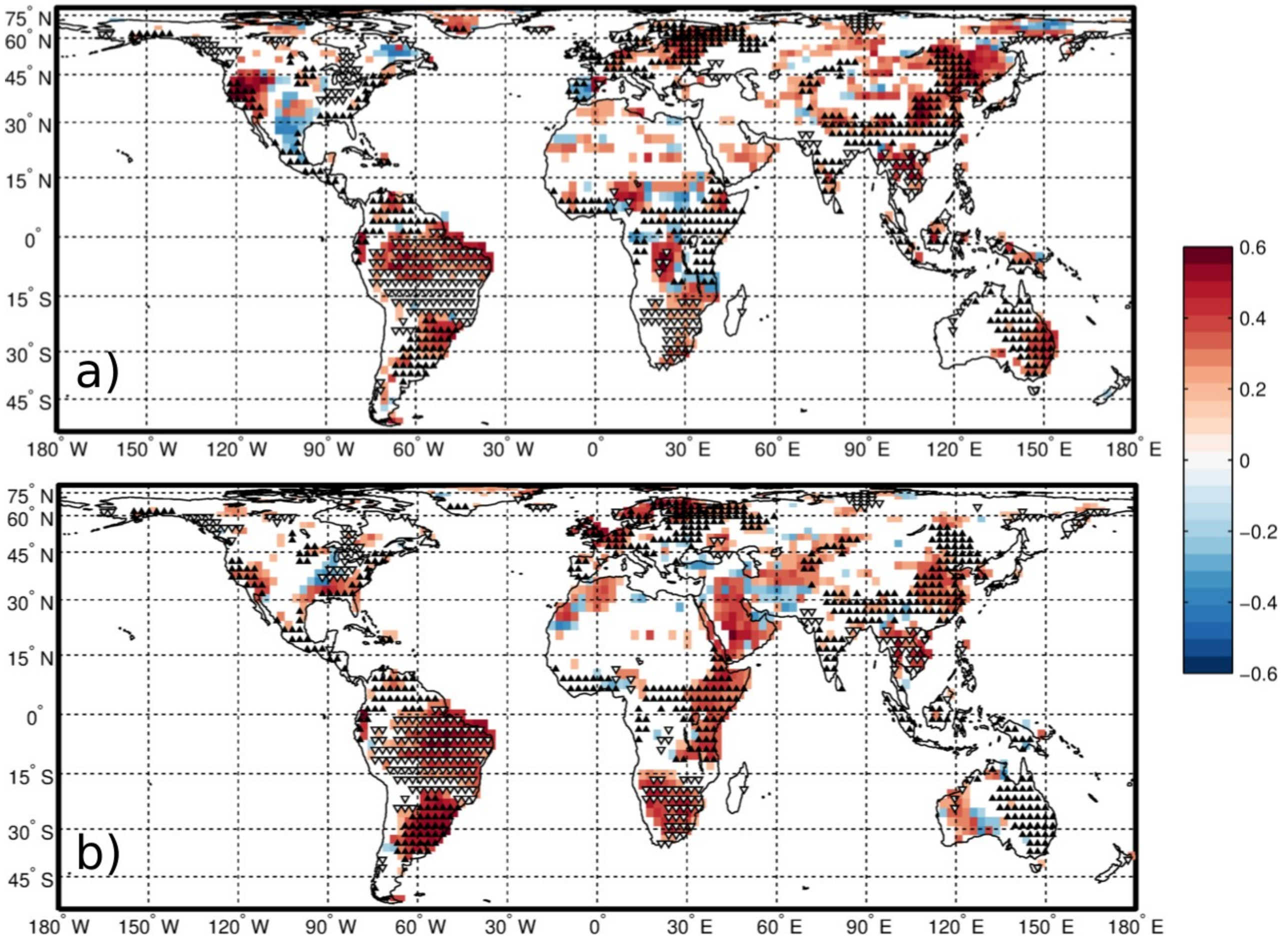
new Fig. 7.



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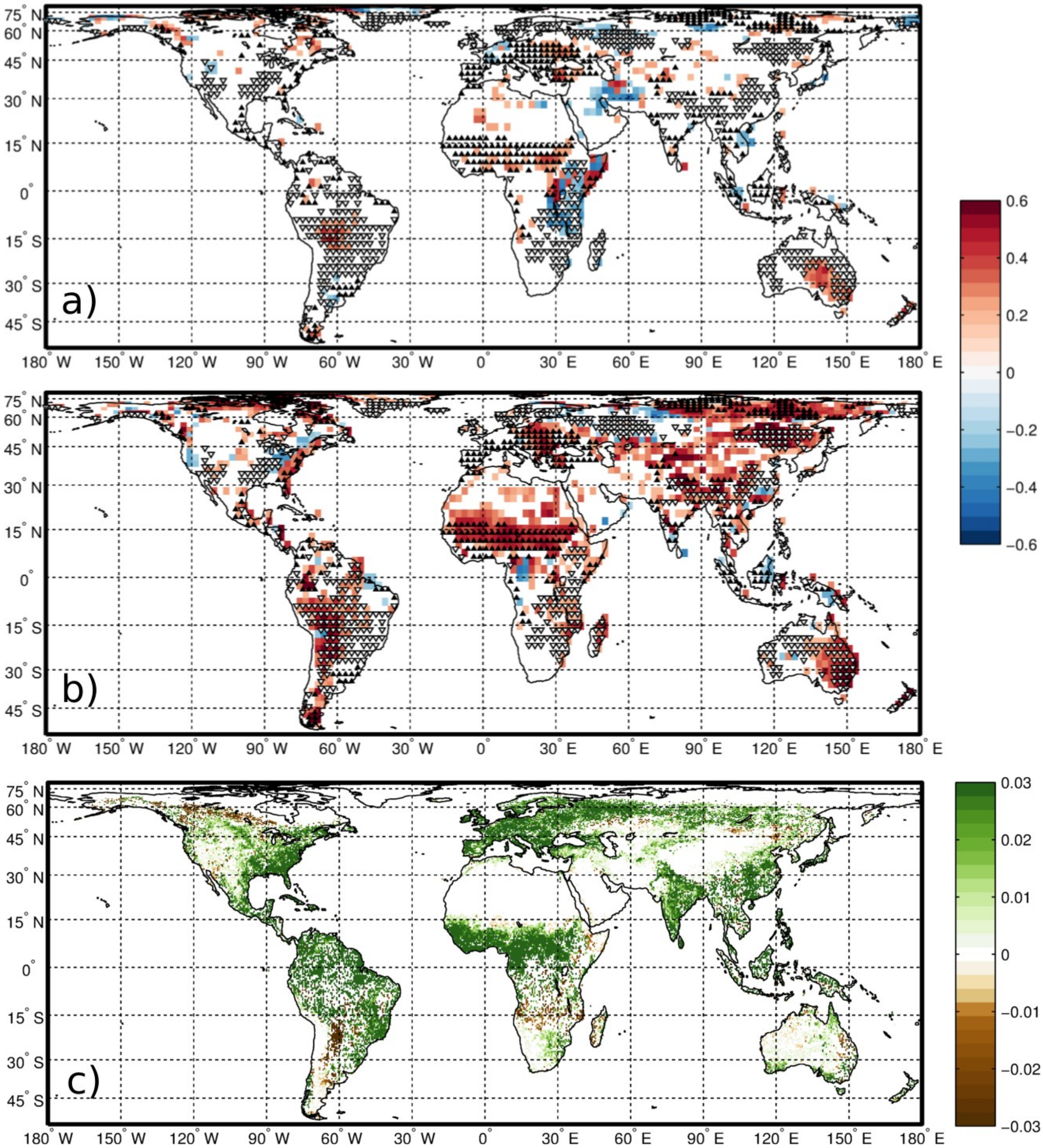
new Fig. 8.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0.4
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−0.2
−0.4
−0.6
0.03
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−0.01
−0.02
−0.03
new Fig. 9.

Responses to Reviewer #2

[05/01/2016]

General comment

The manuscript "Observationally based analysis of land–atmosphere coupling" by Catalano et al. has analysed the covariation between satellite derived observationally based monthly precipitation, soil moisture, evapotranspiration and leaf area index using the coupled manifold technique, which considers both the local and remote forcing of one field to the other. This generalized linear method is used to assess the reciprocal forcing of seasonal mean land surface variables and precipitation anomalies over land.

This is an interesting study providing new insights on the understanding of the land surface atmosphere feedbacks by quantifying the linear coupling between the land surface variables and the climate. The finding that 19% of the inter-annual variability of the precipitation over continental areas is forced by the SM variation is useful new information. The analysis also reveals that the dominant components of the SM forced precipitation variability are the volcanic eruptions and ENSO.

However the finding using the stratospheric AOD estimates that the aerosol emitted during the volcanic eruptions has the effect of reducing the intensity of precipitation over areas of wet climate is not well supported by the cited references, for example, the statement on page 1948, line 6 referring to Alessandri et al., (2012) and the discussion in page 1948, line 6 "the negative signal over India may indicate a suppression of the monsoon linked to the effects of the aerosol released during major eruptions according to Iles et al. (2013)" contradicts IIes et al. finding that HadCM3 precipitation response to volcanic eruptions exhibit drying in monsoon regions except India. The finding that the second dominant component of the precipitation variability forced by SM indicates positive precipitation anomalies over South India related to the positive phase of ENSO also need to be clarified as most of the previous research has found reduced precipitation over India during ENSO years.

The data gaps in the satellite derived SM and LAI are replaced at many grid points with climatological values for applying the CM technique. Figure 1 shows that the seasonal cycle of the percentage of number of grid points replaced globally for SM ranges from 28 to 48%. It is suggested that a figure can be added with the grid point locations using climatological SM values marked so that how much the missing SM data has influenced the major findings of this study can be discussed and highlighted in the abstract.

Overall the paper is well written, structured and referenced. The abstract reflect the content of the paper and provide a clear and complete summary. I recommend its publication after the minor issues mentioned above are addressed.

Response to general comment

We would like to thank the reviewer for the useful comments. We followed all the reviewer recommendations and we think the discussion in the revised paper is now much improved.

We have changed the discussion of the effect of the aerosol on precipitation on page 1948 to make it consistent with the literature cited.

Original:

"In particular, the negative signal over India may indicate a suppression of the monsoon linked to the effects of the aerosol released during major eruptions according to Iles et al. (2013)."

new:

"In particular, according to Joseph and Zeng (2011) and Iles et al. (2013), the negative signal over the monsoon regions may indicate a suppression of the monsoon linked to the effects of the aerosol released during major eruptions. Furthermore, differently from our results and other observational

(Trenberth and Dai, 2007) and modelling (Joseph and Zeng, 2011) studies, the HadCM3 results of Iles et al. (2013) showed a wetting signal over India during the summer season (although not significant in the observational dataset they used)."

With respect to the relation between the second mode of variability and ENSO, we are grateful to the Reviewer for having evidenced this interesting negative feedback. We have added the following sentence to the discussion on page 1949, after "Central and East Asia, South India and the East Coast of Australia.":

"Most previous research showed reduced precipitation over India during ENSO years (Ropelewski and Halpert, 1989; Trenberth et al., 1998). The positive anomalies of PRE forced by SM over South India related to the positive phase of ENSO evidence an interesting negative feedback of the land-surface on the effect of ENSO on the rainfall over India."

The following new reference has been added to the revised manuscript:

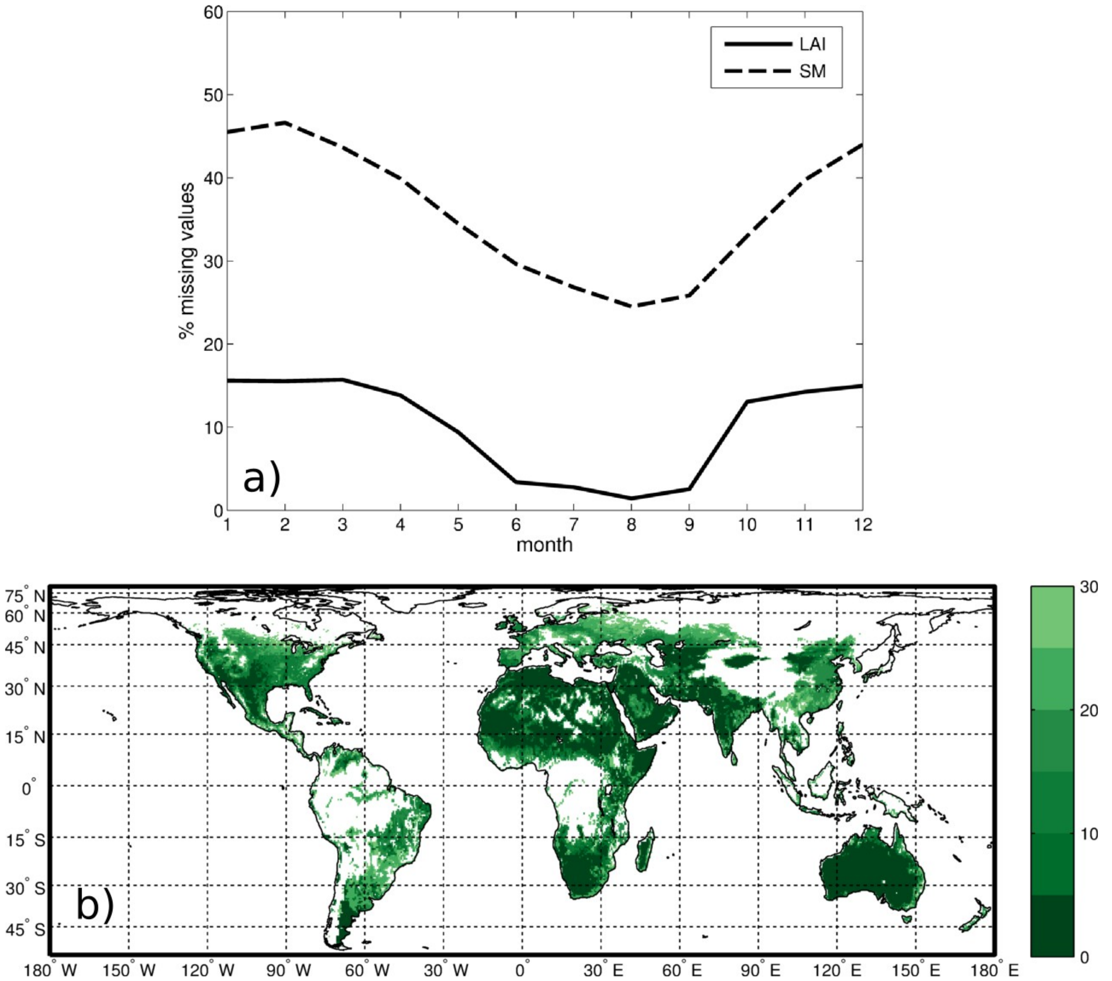
Joseph, R., and Zeng, N.: Seasonally modulated tropical drought induced by volcanic aerosol, J. Clim., 24(8), 2,045–2,060, doi:10.1175/2009JCLI3170.1, 2011.

As suggested, in order to better evaluate and discuss how the missing SM data has influenced the major findings of the study we have added a panel in Fig. 1 with a map of the percentage of SM missing data for each grid point. We recall here that all grid points with a percentage of missing number larger than 30% have been discarded (white areas in Fig. 1b). The following sentence has been added to Section 2:

"Fig. 1B shows the percentage of SM missing data for each grid point. All grid points with a percentage of missing number larger than 30% (white areas in Fig. 1b) have not been considered in the analysis."

The following sentence has been further added to Section 3, after "The results are robust with respect to a ±10 % change of the threshold values.":

"As shown in Fig. 1b, the areas more affected by the replacement of SM missing values (30% of values replaced by climatology) are North-East Europe, East coast of Central-South America, East China and Korea. Since the replacement of missing values with climatology reduces time variability, the coupling in these regions may be underestimated as a consequence. We note that these gap-filled regions do not correspond to transition zones between wet and dry climates (Koster et al. 2000). Therefore, they are not expected to display a strong coupling between SM and PRE and to significantly affect the main results of present study."



new Fig. 1.

Minor comments

1) P1947;L13: Please provide details of the stratospheric AOD dataset with relevant references in the Dataset section.
Thanks for the comment. We added a brief description of the AOD and SST datasets in Section 2 of the revised manuscript. See also answer to minor comment 23 of Reviewer #1.

2) P1948;L1 and P1949;L12: Replace "horizontal" with "spatial".
Thanks. Changed as suggested in the revised manuscript.

3) P1948;L26: The description of the HadISST dataset may be moved to the Dataset section.
Done. Thanks for the suggestion. Please see also response to comment 1.