HESS-2024-2846

Answer to comments from the editor, reviewers and revised files

We thank the editor for her evaluation and comments on the manuscript that will improve the quality of the article. We address them in this document, the original comment is written in blue, while the response is in black font. For those proposed manuscript modifications, we upload a revised version of the manuscript and a tracked-changes version. Some changes are highlighted in this answer in green.

Dear authors, After reading your manuscript, the two reviews and your rebuttals, I agree that your method, results and conclusions are in general valid. However, major revisions are necessary mostly related to the presentation of your study as indicated by both reviewers. I agree with Reviewer #1 that the knowledge gap description is not well defined (which is something else than the comment of Review #2 about well embedded in literature). The Introduction Section does not clearly state why your study is relevant: the link to evaporation is too weak. Only in line 23-25 you shortly make a general statement about evaporation, which does not explain your statement in line 41 "However, the physical mechanisms that teleconnect the Atlantic modes with the hydrological variability– especially with evapotranspiration– remain unclear". I recommend to elaborate on the link between evaporation and climate drivers: what is known and what not. The link with the moisture recycling literature (e.g., Zemp, Wang-Erlandsson, Van der Ent, Staal) might help here as well. Please make sure, that there is clear link between literature review, objectives and finally your method. Hence, please expand your current introduction.

Furthermore, I appreciate your proposed revisions in relation to the other comments in your point-by-point replies. I think these will help to make the manuscript assessable for a broader audience.

We thank the editor for taking the time to read our answers to the referees' comments and for highlighting where the manuscript needs improvement. We have considered and implemented the suggestions; we highlight some here.

We have expanded the introduction with a new paragraph about the relevance of understanding the physical processes (line 29). Moreover, we implemented the proposed modifications – stated in the answers to the reviewers – to clarify the link between the literature review, the objectives and the methods (line 54). The other modifications also include the simplification of the main Figures of maps and time series; they are now opened up and rearranged in Figures 2 to 6 (Figure 4 summarising three seasons for the effects of the AMM); analysis have also been adjusted. The justification for not using eddy-covariance towers data is in line 123. Moreover, data, methods and discussion sections were also adjusted based on the referee's comments and our previous answers to them, as well as the supplementary material.

Answer to comments from reviewer #1

1) I appreciate the wide use of resources and data to address the objectives and the nice figures used to do so. It is also commendable to explain the chain of atmospheric processes that explain evapotranspiration. Although I do not have major concerns with the scientific approach and its validity, the manuscript in its current state is convoluted, hard to read, and lacks a clear direction. First, the reasoning behind the need to understand the "chain" of mechanisms is not clear, nor is there a need to find all these correlations. Why are they needed in the first place? This even affects the potential reviewer's assessment regarding the scientific approach.

We thank the reviewer for the comment. We have added a paragraph in line 29 to bolster the relevance of the study. For example, this include:

"Net primary or biome production variability has been linked to SM-atmosphere feedbacks and climate/earth system drivers – e.g. through climate-driven droughts – since they limit evapotranspiration, growth processes and hence carbon uptake (Nemani et al., 2003; Humphrey et al., 2021; Zhao and Running, 2010). There is evidence that individual extreme weather events can coerce plant phenology, for instance on flowering, leaf senescence and plant growth (Ummenhofer and Meehl, 2017)."

Moreover, we have reorganized the introduction in the paragraph in line 54 to make the definition of the knowledge gap. This includes the rewriting of the sentence of the knowledge gap in line 66, and again a sentence also showing the relevance of the study:

"Consequently, it is still not known how the variations in regional atmospheric circulation – driven by the Atlantic modes – alter local continental atmospheric conditions and afterwards affect net surface radiation and soil moisture, the two key local controllers of ET. We refer to the latter as the physical mechanisms of the teleconnection, which consist of a chain of progressive physical processes. Ecological processes respond to the variability of hydrometeorological conditions (Eagleson, 2013); by understanding the mechanisms leading to that variability, the community can increase the potential predictability of ecosystems' activity."

2a) To explain to the authors what I mean, I opted to supply an attachment with comments where I consider that the manuscript fails to explain the purpose, aim, and methods to the reader. The introduction does not correctly identify a knowledge gap; if it does, it is not based on a robust literature review that sets the state- of-the-art. When I arrived at the three objectives at the end of the introduction, I noticed that little background was given to justify them was not much related to them.

We thank the reviewer for the comment. The paragraph in line 54 is rearranged to evidence the link between the gaps in the literature review and the objectives. This includes sentences highlighting what is not specifically known. For example, we added:

(line 55) "Previous research has established SM and Net Radiation (Rn) as the primary local controllers of evapotranspiration (Seneviratne et al., 2010; Hirschi et al., 2014); consequently, ET is classified into two regimes: water- or energy-limited. The annual cycle and the location of the regimes are not known in South America and are important for understanding ET variability."

(line 57) Some studies have statistically investigated the teleconnections of ENSO (Moura et al., 2019; Le and Bae, 2020; Miralles et al., 2014) or other climate modes with the ET around the world (Martens et al., 2018), but the physical causes for these connections are not known.

(in line 59)"Specifically, it is not known how the interannual changes in moisture transport impact net radiation"

(line 61) Moisture recycling is another factor that can impact surface radiation, but previous studies have focused on its impact on regional and distant precipitation rather than radiation (Staal et al., 2018; Wang-Erlandsson et al., 2018; Zemp et al., 2014; van der Ent and Savenije, 2011).

Other changes can be seen in the tracked changes version also upload it to this modified submission.

2b) Furthremore, the subobjectives do not relate to each other. What are the key research questions that you want to be answered? Is there a hypothesis that wants to be tested?

We thank the reviewer for the comment. We have rewrite the objectives as questions in line 75:

" 1) Where and in which season is the evapotranspiration dominated by a water- or energy-limited regime?

2) How do the Atlantic modes drive anomalous atmospheric circulation and influence the variability in local atmospheric conditions, and how do they then affect the local controllers and thus affect evapotranspiration?

3)Where do the dynamics and, thus, the impacts of the Atlantic modes overlap with those of ENSO in time and space?
”

2c) How is the manuscript advancing the current literature? It is unclear why "the teleconnection of the Atlantic modes of variability with the soil moisture and evaporation of the region remains unclear" in the literature.

We thank the reviewer for the question. Our research discovered that interannual changes in winds and moisture advection drive changes in the local controllers of ET (SM and Radiation) and, thus, in ET. Variability exerted by the Atlantic modes. Therefore, our study also helps to explain the variability of latent heat flux in the Amazon and to improve the predictability of ecological processes and ecosystem behaviour since ET is closely related to the process of photosynthesis. We discover that ENSO's convection inhibition is not the only driver of ET, but the anomalies of moisture advection exerted by the Atlantic modes also play a role and they affect different areas compared to ENSO. The changes in Atlantic regional atmospheric circulation and moisture were never linked before to the variability of radiation, soil moisture or evaporation in South America. This is shown in the new figures and stated in the conclusions.

3) The number of acronyms used and the lack of explanation of the reasoning for conducting the methods also make it hard to judge or even read the results. I recommend the results be divided into several additional sections that may independently address each objective. This is especially necessary for section 4.2., which is too repetitive and complex to understand. The figures are also complex; they have too much information and acronyms. I recommend opening these figures and analyzing each of the panels separately.
We thank the referee for the suggestions. We have modified Section 3 Methods to show how the three subsections answer each one of the three questions, and these methods sections correspond to each of the three subsections of Section 4 Results. This includes a new paragraph at the beginning of the methods sections (line 133) that states:

“Climate modes and their atmospheric circulation anomalies are expected to impact evapotranspiration through a chain of progressive physical processes. The processes start with anomalies in atmospheric circulation (coupled with SSTA), and moisture transport (VIMF). Then, the latter changes moisture flux divergence (MDiv), affecting cloud formation; which simultaneously influences precipitation and radiation availability. Precipitation then affects soil moisture, and afterwards, the two local controllers impact evapotranspiration. However, the impacts of the chain are also mediated by the climatological cycle of the ET regime (water- or energy-limited). Consequently, our research starts by determining the annual cycle of the ET regime and of the local controllers (section 3.1). Then, we use composites to show how the chain unfolds with its final impacts on ET (section 3.2). Finally, we study the joint effects of the Atlantic modes and ENSO (section 3.3)(Duque-Gardeazabal, 2025). Moisture recycling is discussed in section 5.”

We have opened up Figures 2 to 5 as suggested and the time series included in a new figure 6. New Figure 4 summarises the impacts of the AMM on ET and the chains for seasons JJA and SON have been transferred to the supplementary material.

4) Finally, I believe that most readers of HESS do not have sufficient knowledge to understand the paper. Many evident and necessary definitions of terms for the study are missing, making it difficult for the paper to understand. Conclusions should also be directly and explicitly related to your objectives of questions.

We thank the referee for indicating those terms that are not clear or are difficult to understand. However, we do not agree with the claim that the readers of HESS do not have sufficient knowledge to understand the paper; we recognize that some definitions need to be extended and propose changes in this and the answer to reviewer #2. The need to understand the chain is modified in answer to comments 1 and 2c, and the explanation of the physical mechanisms – the chain – is modified in comment 2a.

We have modified the conclusions to explicit make the answer to our questions and objectives. For example:

Line (389) "However, the chain of processes is modulated by the annual cycle of the evapotranspiration regimen which is not completely energy-limited throughout the tropical region and throughout the annual cycle."

Line (396) "The Atlantic Meridional Mode (AMM) creates cross-equatorial SLP anomalies that deflect climatological winds not just over the ocean but also over the continent. It retrieves moisture northward on the positive phase, on occasions increasing and in others reducing convergence, precipitation and radiation depending on location and season, hence causing the land-surface anomalies (SM and ET)."

Line (393) "The negative phase – in conjunction with ENSO El Niño – strengthens the trade winds and produces divergence over an extended region which significantly changes the land-surface variables."

Specific comments

Line 19 – Convoluted sentence

Thank you. We propose to rewrite the sentence from:

"Other variability sources stem from other ocean basins, Madden-Julian Oscillations or local features like topography or land-atmosphere interactions"

To:

Other sources of seasonal variability stem from other ocean basins (e.g. the Atlantic), and at other temporal scales from Madden-Julian Oscillation or local features like topography or land-atmosphere interactions.

Line 21 – Convoluted sentence, you use the word "anomalies" three times in the same sentence

We thank the referee which indicates the specific difficulty with the sentence. We propose to rewrite it, changing it from:

"The modes cause their impact through atmospheric circulation anomalies; those anomalies enforce hydrological variability, which is evidenced by anomalies of precipitation, soil moisture (SM), temperature, evapotranspiration and streamflow"

To:

The modes cause their impact through atmospheric circulation anomalies and hence drive local atmospheric conditions; those interannual deviations from the climatology enforce hydrological variability, which is evidenced by variations of precipitation, soil moisture (SM), temperature, evapotranspiration and streamflow.

Line 23 – Is this the same as evapotranspiration?

Yes, transpiration is also a type of evaporation, and open water is considered part of the continents, terrestrial evaporation consists of whatever evaporative flux over the continents. Evapotranspiration consists of soil water, open-water evaporation and transpiration, so then terrestrial evaporation is also a synonym of evapotranspiration. D. G. Miralles et al. (2020) argue that using evaporation is a more accurate term and that the fact of using one concept or the other should not matter.

Line 26 – planning or considering them

Thanks. This is a typo. We also take the opportunity to rewrite it:

and their consideration in long-term planning is critical for achieving sustainable development.

Line 30 – what do you mean by this? Is this supposed to be the knowledge gap?

We thank the referee for indicating where clarifications are needed. The sentence refers to studies about precipitation variability, in which physical phenomena cause interannual changes at several temporal scales (thus, it is still not completely understood). However, as our research focuses on interannual evapotranspiration variability and at seasonal scale, we decided to suppress the second part of the sentence that talks about precipitation and cloud cover.

We added an explanation of what physical mechanisms mean in answer to comment 1. Physical processes comprise climate phenomena; they are quantifications of physical properties rather than statistical variables.

Line 38 – what do you mean with these two terms? (atmospheric bridges or extratropical pathways)

The cited literature in the sentence describes how ENSO alters the Walker cell and then generates atmospheric subsidence over East South America and the Atlantic. The latter is what they and us refer to as the atmospheric bridge; the anomalous subsidence weakens the Atlantic Hadley cell, the trade winds towards the equator and then partially forces the coupled Atlantic ocean-atmospheric modes (the AMM and the Atl3). The extratropical pathway is the excitation of the Pacific North American pattern, a series of anomalous high and low pressure systems that connect the tropical Pacific, Alaska, east North America and the Tropical North Atlantic; the pressure systems then influence the atmospheric conditions over the Atlantic, which influence the ocean, and partially force the phase of the Atlantic modes.

Line 41 – Not clear what you mean with "the physical mechanisms". This is important as it appears that this is your knowledge gap of the study.

Effectively this is the sentence and paragraph where we identify the knowledge gap. We thank the referee for pointing out that this is not clear; this has been answered in comment 1, where we have also modified the sentence. The definition of physical mechanisms is also addressed in comment 1 and it will be added to the introduction (answer to comment 2a).

Consequently, it is still not known how the variations in regional atmospheric circulation – driven by the Atlantic modes – alter local continental atmospheric conditions and afterwards affect net surface radiation and soil moisture, the two key local controllers of ET. We refer to the latter as the physical mechanisms of the teleconnection, which consist of a chain of progressive physical processes.

Line 44a – again, give an example of what a physical reason would be? It is not clear where you are going with this.

Thank you. This has been answered in comment 1, comment 2a and comment 41.

Line 44b – The studies have addressed what really? (Some research has addressed the interannual changes in moisture transport, convergence, cloudiness and associated rainfall in the region)

We see that this sentence can be complemented. The sentence supports the knowledge gap regarding the cloudiness, it will now also explicitly indicate the control climate modes exert in the variability of radiation.

We changed it from:

"Some research has addressed the interannual changes in moisture transport, convergence, cloudiness and associated rainfall in the region (Hoyos et al., 2019; Cai et al., 2020; Ruiz-Vásquez et al., 2024)"

To:

Some research has addressed the causes of interannual rainfall variability by analysing the interannual changes in moisture transport, convergence, and cloudiness exerted by Atlantic modes and ENSO in the region (Hoyos et al., 2019; Cai et al., 2020; Ruiz-Vásquez et al., 2024); those changes in moisture transport and cloudiness might affect radiation availability.

Line 48 – This again is too vague. The authors do not specify what really is poorly understood. They also do not say what are these physical mechanisms that they want to address. They also do not mention the importance of addressing these gaps.

We thank the referee for pointing out this is not complete. The cited studies concluded that there are still regions – e.g. Orinoco – where the causes of seasonal rainfall variability are not explained by the atmospheric circulation under the influence of the analysed climate modes (e.g. the TNA, which is brought up in line 47 first manuscript). As we mentioned in answer to comment 2a, the altered atmospheric circulation and moisture transport will also affect the variability of radiation – the physical process – and is now explicitly written in the modified line 42 first manuscript.

The importance of the altered atmospheric circulation in addressing the knowledge gap is to explain the variability of radiation and its effect on ET. The importance of addressing the ET variability is answered in comment 1, and also mentioned in line 23 of the original manuscript.

We propose to modify the sentence from:

"Atmospheric circulation variability produced by the Atlantic SST modes is poorly understood, especially over the north (Orinoco basin)."

To:

Atmospheric circulation variability produced by the Atlantic SST modes is poorly understood between September and November since is not known how altered moisture transport changes convergence, cloudiness and radiation, especially over the north (Orinoco basin).

Line 50 - But why only these, you had mention several modes, so why only AMM and Atl3. And also, what do you mean that their link is with evaporation? I think that here the use of English int he introduction really hampers the understanding of the manuscript an its goals.

We thank the referee for pointing out this is not clear and will improve the manuscript based on it.

The reason why we just analysed those two is what we discovered with the literature review and the principal component analysis described in section 3.2. Previous works have already analysed ENSO's influence over the ET in the Amazon (Moura et al., 2019; Le & Bae, 2020; Miralles et al., 2014), which we described in line 42 first manuscript. In the discussion section, we also mentioned other modes such as the Atlantic Multidecadal Oscillation (line 273 first manuscript), but debated that this is a mode produced by the aerosol forcing in the northern hemisphere (Hua et al., 2019; He et al., 2023), and state that the AMM and the AMO are associated (line 274). We tried to keep the introduction concise, but we recognize that is better to also include some information about it.

With the principal component analysis of the SSTs, we also discarded modes in the Indian Ocean and decided not to mention them (line 109 first manuscript "not shown"). Moreover, the ocean-atmospheric modes that are not in the neighbouring Pacific and Atlantic oceans and therefore do not change the atmospheric circulation around South America (Cai et al., 2019; Cai et al., 2020 – also in the manuscript). The cited articles are reviews of the pantropical climate interactions and the effects of ENSO over South America, and explain which modes and which do not have effects on the atmospheric circulation around South America.

We added the following sentences in line 159:

Other climate modes, such as the Indian Ocean Dipole, the AMO, etc., unfold over basins that are not close to our study area, and hence, they do not alter tropical South America's atmospheric circulation. Consequently, we discard them from our analysis.

Line 51 - Sorry but these does not make much sense. Several concepts are put together in a way that is not coherent and convoluted. Please revise these objectives so that make sense and they follow the main thread that you were trying to portray in the introduction.

We thank the referee for pointing out this is not clear, we will improve the manuscript based on it. We proposed to rearrange the sentences in this paragraph in our answers to comments 1, 2a, 2b and 2c. They modify the paragraph of the identification of the knowledge gap, the transformation of the subobjectives to questions, describe what is a physical mechanism.

Line 58 – I am missing here an overall summary of the methods and the data. Why do you need this data? Why have you chosen these products?

We thank the referee for the comment, we will modify the first paragraph which is already an introduction to the data section but can be complemented.

We added the following statements as a summary of Section 2 Data:

Line 80 – we add: This study uses – apart from ET estimates – variables that control local ET (local controllers), such as net radiation and soil moisture; but it also uses atmospheric circulation variables, such as SLP, winds, moisture transport, convergence and rainfall. We use those atmospheric variables because ocean-atmospheric modes drive the regional atmospheric circulation, which afterwards influences the local ET controllers. Sea Surface Temperature Anomalies (SSTAs) are used to identify the ocean-atmospheric modes (methods Section 3). All datasets are downloaded at monthly time scale and used between Dec-1979 and Nov-2020 (except for the satellite-based soil moisture, details in section 2.2); they are aggregated at seasonal scale and analysed for each season individually and synchronously. The aggregation method for all variables is the average of the three monthly values, except for precipitation and ET when we use the sums (Duque-Gardeazabal, Zenodo, 2025).

Line 87 – beginning of a new paragraph: Satellite and reanalysis data sources have strengths and limitations. Satellite data can provide some of the needed data mainly over land and moisture transport is not available from this source. Reanalysis data are considered physically-based interpolations of observations and provide atmospheric variables that satellite data does not directly acquire. "Satellite-based datasets have strengths ..."

Line 59 – But what variables are you focusing on?

We thank the referee. In the previous answer – line 58 – we propose to separate the paragraph in two and add sentences describing which variables we will use, even though they are mentioned in subsections 2.1 and 2.2. We will also add a table (see answer to comment 1-9 from reviewer #1).

Line 63 – of reanalysis or satellite-based methods. (?)

Yes. The subject of the previous sentence is "both data sources", in the paragraph which topic is satellite-based and a reanalysis datasets. The pronoun "their" refers to "both data sources".

Line 69 – Why do you choose thee variables? For what purpose?

The mentioned variables are those related to atmospheric circulation and then influence the local atmospheric conditions (i.e. the ET local controllers, already said in line 51). The purpose is to analyse the chain of events that link the atmospheric circulation anomalies – under the Atlantic modes –with local atmospheric conditions and the land-surface controllers (which is the second subobjective as we said in line 55).

We propose to clarify it by adding the paragraph described in answer to comment about line 58. The comment will be written in line 59.

"... it also uses atmospheric circulation variables such as SLP, winds, moisture transport, convergence and rainfall because they drive the local ET controllers."

Line 93 – This title does not agree with the content of the paragraph. I also think that a brief summary of your methods is needed here. I still do not know where are you heading with this manuscript.

As we mentioned in our subobjectives, we first intend to find the location and the time of the water- or energy-limited evaporation (line 54). Afterwards, in the second subobjective we find the influence of climate modes driving the physical processes. The title and the paragraph indicate we want to find the seasonal annual cycle of local land-surface controllers and not yet the climate variability drivers.

We propose to modify the title from:

"Local and seasonal changes of ET drivers"

To:

Determining the location and annual cycle of local ET controllers

Line 94 – I thought that the modes were the drivers of evaporation. So there are other drivers besides the modes?

We thank the referee for pointing out the misunderstanding we are generating in the manuscript. Local land-surface conditions are the direct drivers of ET and the ocean-atmospheric modes are the drivers of the local conditions; we now see that calling both of them "drivers" generates confusion (even though both are drivers or

controllers of the beginning and end of the chain). Therefore, we change now to use the concept of climate modes *drivers* for the atmospheric circulation variability impacting local conditions, and *local controllers* of ET to refer to net radiation and soil moisture. As mentioned in answer to the general comment.

Each methods' subsection corresponds to one subobjective. First subobjective is to determine the location where and specific local controller dominates, and second, is to determine the ocean-atmospheric phenomena and its influence on the physical processes driving the local controllers.

Line 97 – What do you mean here? (evaporation regime)

The sentence in lines 96 and 97 clearly states the relation between evaporation and water and radiation, the controllers. As we also state in line 53, supporting it with citations (Seneviratne et al., 2010; Hirschi et al., 2014), evaporation is classified into two regimes: water- and energy-limited.

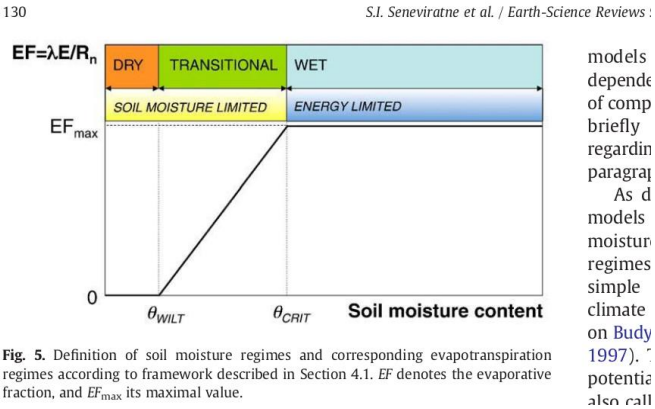

Fig. 5. Definition of soil moisture regimes and corresponding evapotranspiration regimes according to framework described in Section 4.1. *EF* denotes the evaporative fraction, and EF_{max} its maximal value.

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Line 99a – What do you mean with composite analysis?

Composite analysis is a statistical technique widely used in climate sciences to identify and characterize typical conditions or patterns associated with specific events or phenomena (Wilks, 2011). It involves combining multiple instances of a particular event (specific time steps) to create an average or representative picture of the conditions during those events. In line 112, we described how we defined those particular events.

Line 99b – Can you define the active phase for the normal reader?

As described in line 112, "The positive and negative phases are identified when their indices are above or below ±1 standard deviation, respectively, and otherwise are defined as neutral phase (threshold for mode's activation)". "their indices" refers to the climate modes indices based on Sea Surface Temperature Anomalies (SSTAs). This definition comes from the definition of ENSO phases, El Niño and La Niña, when El Niño characterises by high positive SSTAs and La Niña by low negative SSTAs.

Line 108 – "?" (Not shown)

The sentence describes the intermediate step of an exploratory correlation analysis that indicated us which ocean-atmospheric modes have an impact on hydrological variables (the whole paragraph talks about identification of the modes). Those intermediate results, with also the literature review, allowed us to focus on the Atlantic modes and discard ENSO and other modes for the analysis (IOD, AMO, PDO, etc). As those results are not significant for explaining the physical processes of the phenomena, we decided not to show them (see answer to comment about line 50).

Line 132 – Where do I see this in Figure 1?

Thanks for the question. The ITCZ is a narrow band of tropical ascent of moist air, clouds and heavy rainfall (Schneider et al., 2014). Figure 1 e-h show how the net radiation – same energy-limited regime – migrates with the annual cycle from south to north and comes back, as also specified in lines 132 and 133. The fact that energy-limited evaporation unfolds when there is enough availability of soil moisture is evidence of saturated soils (Seneviratne et al., 2010), which happens at high precipitation rates (feature of the ITCZ), but also energy-limited ET unfolds with reductions of net radiation or high cloud cover (also a feature of the ITCZ). The latter are typical conditions of tropical equatorial areas and the migration of the ITCZ. The maps in Figure 1 also display the latitude to clarify that the analysed region is on the equator.

We still propose to add the next sentence to line 133:

, since the heavy rainfall of the ITCZ saturates the soils and influences the locations of energy-limited regime.

Line 134 – what p-value did you use for the test? What test specifically? More details on the correlation analysis are needed.

Figure 1 is not a correlation analysis; it is a multi-linear regression, as indicated in the caption's first line when it says "regression coefficient" and in section Methods 3.1 where the multi-linear regression is described (line 95). The dependent variable is the ET, whereas the independent variables are soil moisture and radiation. The main metric of multiple regression is not the p-value, it is the adjusted coefficient of determination (R^2), which is shown in Fig1 a-d and written in its caption. The R^2 gives information on the quality of the regression. p-values in regression analysis are associated with the individual independent variables and this is shown in Fig1 e-h with yellow colour when a p-value >0.05 (as indicated in the legend). The latter indicates where neither of the independent variables is significant for explaining the dependent variable.

We propose to change the figure's top panels (see answer to comment 3 from reviewer #2), and specify the p-values of the regression coefficients. Therefore, we propose to change the third line of the caption from:

"and (e-h) variable with the highest significant coefficient"

To:

"(a-d) multiple linear regression slope coefficient for Soil Moisture, (e-h) slope coefficient for the Net Radiation and (i-l) variable with the highest significant linear slope coefficient (p<= 0.05)"

Line 135 – The explanations that come now are very insightful; however, I do not understand where are you getting all this information just by looking at the figures.

We thank the reviewer for the compliment. We are describing the annual cycle of the phenomena of the ITCZ and at the end of the sentence in line 136 we refer to the "energy-limited evaporation", which is one of the main results in Figure 1. The description goes season by season as Figures 1 e to h are displayed. To make easy to spot, we will add the label of each subplot panel to the text, that is:

Line 135 – In MAM (Fig 1e); line 137 - As the ITCZ moves northward in JJA (Fig 1f); line 140 – In SON (Fig 1g); line 142 – In DJF (Fig 1h).

Line 134 – The correlations (R2) are between what variables? Between a mode and a variable? It is not clear.

As specified in the caption, the analysis is a multi-linear regression to find the slope coefficients, not a correlation analysis; this is also explained in section Methods 3.1 multi-linear regression (line 95). The R^2 is the coefficient of determination of the multiple regression (Fig 1 a-d); in panels a to d there are some zones with red and yellow colours but almost all areas are blue and green, meaning what we state in line 133, the majority of ET variance can be explained by just considering SM and radiation.

Caption Figure 1 Line 2 – targeting those of ET? (the multiple regression of SM and Rn standardised anomalies targeting those of ET)

Thank you for the question. Targeting is used – in a linear or non-linear regression analysis context – to also refer to the dependent variable (Martens et al., 2018). First the independent variables are mentioned and afterwards the dependent. The equation for our multi-linear regression is line

Caption Figure 1 Line 4 – Also, if the correlations are low (e.g.., red and yellow areas) meaning that there is now correlation, how can you say that the driver is driving SM and Rn anomalies.

Because this is a multiple regression analysis and we are displaying the coefficient of determination (R^2), not a correlation analysis. We are able to determine the driver with the slope of the regression, which is in panels (e) to (h). The colours of the R^2 bar relate to panels a-d. Regarding the red and yellow areas, as we said in line 134: "with some exceptions where wind speed or vapour pressure deficit might be important." Almost all the maps are colour green or blue, meaning R^2 is higher than 0.6, a good threshold for regression performance.

Line 146 - What information in Fig 1 or what interpretation makes you arrive to these statements? Sorry, It is not clear.

We thank the referee for indicating this is not clear. This was meant as an introductory paragraph to section 4.2, where we analyse the moisture convergence, clouds and radiation, rainfall and soil moisture. As the reader will see in the other figures, there are cases when above-average evapotranspiration unfolds during negative precipitation anomalies (Fig 2a and 2g, e.g. over northeast Brazil).

To avoid the confusion with Figure 1, we propose to move the paragraph to line 151, after the title of section 4.2.

Line 150 - Do you mean relationship? (Chain)

We thank the referee for the question. As answered to comment 1, with the chain we refer to the progressive atmospheric processes that generate interactions in the components of the earth system.

To clarify, we propose to change the section title from:

"Chain between the Atlantic modes and the evapotranspiration"

To:

"chain of progressive physical processes linking the Atlantic modes and continental evapotranspiration"

Line 151 - You mean soil moisture and radiation? (key climatic drivers of evapotranspiration)

We thank the referee for the question that highlights the confusion between the drivers and the local controllers. Yes, in this case we refer to the local controllers because at the beginning of the sentence we say that "The interannual variability of atmospheric circulation affects the key local climatic drivers of evapotranspiration". To

avoid the confusion, and to be coherent with what we proposed to comment 1, we propose to replace the word "drivers" for *controllers*.

So line 151 will change from:

"The interannual variability of atmospheric circulation affects the key local climatic drivers of evapotranspiration"

To:

The interannual variability of atmospheric circulation affects the key local climatic controllers of evapotranspiration (Soil Moisture and radiation)

Line 152 - I think what you mention in this sentence should have been explained in the introduction

We thank the referee for the suggestion that will clarify an important concept we used in our manuscript. We addressed this comment in answer to major comment 2a, where we proposed to complement line 66 with the sentence:

"Consequently, it is still not known how the variations in regional atmospheric circulation – driven by the Atlantic modes – alter local continental atmospheric conditions and afterwards affect net surface radiation and soil moisture, the two key local controllers of ET. We refer to the latter as the physical mechanisms of the teleconnection, which consist of a chain of progressive physical processes

Answer to comments from reviewer #2

1-0) Data – spatiotemporal scales: One of the attractive aspects of this paper is that it includes many data products. This fortifies the conclusions drawn as they are not hung up on a single methodology. In Section 3 the spatial extent of the AO regions are specified as well as the total timeframe (Dec1979-Nov2020) and the aggregated time resolution of the data (3 months). The information on spatial and temporal resolution and the method of aggregation of the underlying original data products is unclear or scattered over different sections and figure captions. As this is a data driven study, this requires attention. The most logical place for this is the Data section (Section 2).

We are grateful to the reviewer for noticing our intention to look for consistency in the dynamics shown by different datasets and for the comments to clarify the details of the datasets. We acknowledge that line 100 – about the timeframe and time resolution – is more suitable for the data section and move it to line 80; although it is a procedure/method that is also recorded in the computational codes we attached as a research asset (which we propose to change from GitHub to Zenodo to completely comply with FAIR principle, here cited as (Duque-Gardeazabal, Zenodo, 2025).

We propose to modify line 80 – to also tackle a comment from the referee's #1 to line 58. We will divide that paragraph and add:

This study uses – apart from ET estimates – variables that control local ET (local controllers), such as net radiation and soil moisture; but it also uses atmospheric circulation variables, such as SLP, winds, moisture transport, convergence and rainfall. We use those atmospheric variables because ocean-atmospheric modes drive the regional atmospheric circulation, which afterwards influences the local ET controllers. Sea Surface Temperature Anomalies (SSTAs) are used to identify the ocean-atmospheric modes (methods Section 3). All datasets are downloaded at monthly time scale and used between Dec-1979 and Nov-2020 (except for the satellite-based soil moisture, details in section 2.2); they are aggregated at seasonal scale and analysed for each season individually

and synchronously. The aggregation method for all variables is the average of the three monthly values, except for precipitation and ET when we use the sums (Duque-Gardeazabal, Zenodo, 2025).

Spatial aspects:

1-1) Mention the spatial resolution of all the data products.

We propose then to add the following sentences.

Line 70 – division of paragraph for ERA5 and ERA5-Land. We propose to add:

"All atmospheric variables from ERA5 have a 0.25° resolution."

Line 74 – we propose to add:

"All variables from ERA5-Land have a 0.1° resolution."

Line 78 – we propose to add:

"(MSWEP)(Beck et al., 2019) and the Climate Hazard group InfraRed Precipitation with Station (CHIRPS)(Funk et al., 2015)", with a spatial resolution of 0.1° and 0.05°, respectively.

Line 81 – we propose to add:

"Three satellite-based datasets complement the three ERA5-Land variables (all of them at 0.25° resolution):"

Line 88 – we propose to add:

ERSST is at 2°, and HadSST is at 5° resolution.

1-2) How did you deal with gaps in the spatial data (e.g. in the satellite products)

Thanks for the question. The only product which has gaps is the satellite-based soil moisture (ESA CCI SM), which helped to corroborate the dynamics found in ERA5-Land. It was also the only product downloaded at daily resolution (not directly available at monthly scale); to transform/aggregate the ESA CCI SM to monthly values, we impose that each month had at least 4 values, one per week, and assume a slow dynamics of Soil Moisture within the week (Zanin et al., 2024). This unifies the temporal features of the ESA CCI SM with the other datasets and was applied grid-wise. Supplementary material Figure S2 displays the composite maps with the gaps that were not able to be filled due to the lack of data under the dense forest canopy. For the time series in panel (a,c,e,g) in proposed Figure 7, we averaged the values inside the rectangles, excluding the missing data.

We propose to add the next sentence to line 84:

ESA-CCI-SM was downloaded at daily resolution and transformed to monthly values by averaging the days within each month as long as the month had at least four values; the remaining spatial gaps were not filled and were excluded when needed. "GLEAM uses a three-layer ..."

1-3) What is the common grid resolution that all products are projected on and how did you regrid data the data going to either coarser or finer resolutions

Thanks for the question. We did not regrid or combine satellite and reanalysis data products because we were looking for consistency in the dynamics shown by them separately (as mentioned in line 65). The main products displayed in the manuscript are ERA5 and ERA5-Land due to their homogeneity in the whole time frame; dynamics

in satellite products are displayed in the time series and in the supplementary material. Figure S2 shows the ESA-CCI-SM anomalies, Figure S3 shows GLEAM anomalies, and Figure S4 shows CLARA cloud cover correlations.

Line 95 – we propose to add:

"Therefore, we look for consistency in the dynamics of both sources of information; we do not regrid and do not merge any datasets because we do not perform operations between them, we just display the datasets conjointly when necessary and analyse that the dynamics unfold in both sources (Table 1)."

Temporal aspects:

1-4) Mention the temporal resolution of all the data products.

Thanks for the suggestion. In our answer to comment 1-0, we have included a sentence at the beginning of section 2 Datasets that describes the temporal resolution of every dataset as monthly, except for the ESA-CCI-SM, which is given in answer to comment 1-2. The time resolution was mentioned in line 68 of the original submitted manuscript, but it should be clearer now with the change.

1-5) What was the method of aggregation to the 3-month products that you use in the analysis? For some variables such as ET and precipitation it can be sums or averages.

Thanks for the question. In our answer to comment 1-0, we have included a sentence that describes it.

Line 59 -

... they are aggregated at seasonal scale and analysed for each season individually and synchronously. The aggregation method for all variables is the average of the three monthly values, except for precipitation and ET when we use the sums (Duque-Gardeazabal, Zenodo, 2025).

1-6) What was the method of aggregation from the 40-years of 3-month aggregated products filtered by O-phase. In the caption of Fig2 I see that precipitation is averaged but it is not clear if it this refers to a 40-year average of 3-month precipitation sums or averages. For ET no aggregation method mentioned.

Thanks for the question. The anomalies of the variables are calculated form the seasonal (3-month) time series for each season individually. The aggregation method for precipitation and ET was the sums as answered in comments 1-0 and 1-5.

1-7) The derived data products (e.g. MDiv, …) are based on non-linear relations. In which order did you do determine the 3-month aggregates? Did you determine MDiv based on 3-month aggregate fields of moisture or did you determine MDiv based on hourly data and aggregate those?

We very much appreciate the question. The ECMWF-Copernicus-CDS web portal directly provides monthly datasets that come from the accumulation of the hourly values, with every variable aggregated after its individual calculation. Vertically integrated Moisture Divergence (MDiv) is also directly provided by the web portal in the single-levels category; hence, the variable is simulated at hourly resolution by the ECMWF, aggregated to monthly scale by the ECMWF, downloaded and afterwards aggregated to seasonal scale by us. This can be consulted at the Copernicus Climate Data Store web portal https://cds.climate.copernicus.eu.

In the case of the net surface radiation, we derived it from the energy balance equation using the surface net solar radiation and surface net thermal radiation at a monthly time scale (both directly downloaded from the ECMWF-Copernicus-CDS). This should not be a problem since this calculation is a simple subtraction (linear). Afterwards,

we aggregated the net surface radiation to the seasonal intervals. For some variables, it was necessary to multiply for the number of days in the month. The latter is specified in our codes uploaded to GitHub and that we propose to transfer to Zenodo (Duque-Gardeazabal, Zenodo, 2025).

1-8) What about night-time data? For the relation between radiation and ET it matters whether you include night-time data or not.

Thanks for the question. Even though at night-time there is positive ET, its value is far less than during the day. If one excludes the night-time from calculations – i.e. just considers daytime – one will still retain the biggest share of ET. We include both day and night ET in the only dataset which could separate them, ERA5-Land (which is aggregated to monthly resolution by the ECMWF). All other ET datasets at daily resolution consider the night-time because they use daily mean input values to calculate the ET, and their formulations are targeted for daily totals (Martens et al., 2017).

1-9) I think a Table with an overview of the data products and their original spatial- and temporal resolutions would be a good addition followed by a description on how you bring these data together.

We thank the reviewer for the suggestion. We will include the table and the required description is included in answer to comment 1-3.

Line 65 -

... we do not regrid and do not merge any datasets because we do not perform operations between them, we just display the datasets conjointly when necessary and analyse that the dynamics unfold in both sources (Table 1).

Variable	Reanalysis (ERA5 and ERA5-Land)		Satellite		
	Spatial	Temporal	Dataset	Spatial	Temporal
Sea Level Pressure	0.25°	Monthly	-	-	-
Winds at 850 hPa	0.25°	Monthly	-	-	-
Vertically Integrated Water Vapor Flux (VIMF)	0.25°	Monthly	-	-	-
Vertically Integrated Moisture Divergence (MDiv)	0.25°	Monthly	-	-	-
Precipitation	-	-	MSWEP v2.8 (Beck et al., 2019)	0.1°	Monthly
	-	-	CHIRPS v2.0 (Funk et al., 2015)	0.05°	Monthly
Net surface thermal radiation	0.1°	Monthly	EUMETSAT CLARA-A3 cloud area fraction	0.25°	Monthly
Net surface solar radiation	0.1°	Monthly			
Soil Moisture (volumetric water content 1st soil layer)	0.1°	Monthly	ESA-CCI-SM v08.1	0.25°	Daily (aggregated to Monthly using the mean)

Total evaporation	0.1°	Monthly	GLEAM v3.8a	0.25°	Monthly
	Sea Surface Temperature Anomalies				
Sea Surface Temperature Anomalies	ERSST v5			2°	Monthly
	HadSST v4.0.1			5°	Monthly

2 - 0) Atlantic mode to ET chain: In the introduction you state that links between the Atlantic mode and the hydrometeorology in the Amazon has been mainly studied from a statistical perspective and that one of the innovations of this paper is that you will explore the underlying physical mechanisms. In the methodology section you don't explain how you will do this, but at the start of section 4.2 you outline an Atlantic mode to ET chain that I interpret as a chain, your chain, of underlying physical mechanisms. Is that correct?

We thank the reviewer for the question. Yes, we use the concept of a chain to refer to the progressive physical processes/mechanisms that link the ocean-atmospheric dynamics of the Atlantic modes with the ET over the continent (i.e. atmospheric circulation anomalies affecting local ET controllers).

Following referee #2 suggestion, we will include the detailed description of the chain at the beginning of the Section 3 methods (line 92), by moving and complementing the sentences in line 151.

Climate modes and their atmospheric circulation anomalies are expected to impact evapotranspiration through a chain of progressive physical processes. The processes start with: atmospheric moisture transport anomalies (VIMF); which changes moisture flux divergence (MDiv), affecting cloud formation; which simultaneously influences precipitation and radiation availability; precipitation then affects Soil Moisture; and afterwards, the two local controllers impact evapotranspiration. However, the impacts of the chain are also mediated by the climatological cycle of the ET regime (water- or energy-limited). Consequently, our research starts by determining the ET regime and the local controllers' annual cycle (section 3.1), then shows – with composites – how the chain unfolds with its final impacts on ET (section 3.2), and finally studies if there are conjoint effects of the Atlantic modes and ENSO (section 3.3). Moisture recycling is discussed in section 5.

Referee #1 made a comment (to line 94) where we realized about a misunderstanding with what we call drivers. We previously referred to both – climate modes, and local net radiation and soil moisture conditions – as "drivers", and this might confuse the reader. Even though both are drivers, climate modes are at the beginning, and the others are at the end of the chain. Therefore, we propose to change and use the concepts of *drivers* to refer to the climate modes that drive atmospheric circulation variability which impact local conditions, and *local controllers* of ET to refer to net radiation and soil moisture (which control water- or energy-limited regime).

We implement this change in several parts of the text, but we want to highlight one answer to referee #1 –comment 2a– where we propose to add to line 45:

Nevertheless, it is still not known how the Atlantic modes drive regional atmospheric circulation, which then alter local continental atmospheric conditions and afterwards affect net surface radiation and soil moisture, the two key local controllers of ET. We refer to the latter as the physical mechanisms of the teleconnection, which consist of a chain of progressive physical processes.

The previous paragraph will tell the reader about the chain in the introduction section of the manuscript, as requested by Referee #1.

2-1) You outline the chain as:

atmospheric moisture transport anomalies → (moisture flux divergence, clouds and radiation + precipitation and Soil Moisture) → evapotranspiration.

Arrows indicate that variables change/impact the following variable(s). Variables between brackets are changing simultaneously.

If exposing the physical mechanisms is indeed the main goal, this chain should be better defined in the methods section and followed through in the presentation of the results.

To start, I see the chain not so much in a straight line but more of a branched chain:

- Atlantic-mode chain (external moisture)
- atmospheric moisture transport anomalies → moisture flux divergence → clouds → (radiation + precipitation).
- Precipitation → Soil Moisture.
- Soil Moisture + Radiation → evapotranspiration
- Moisture recycling chain (local moisture)
- evapotranspiration → clouds → transport → rain
- evapotranspiration → Soil Moisture (negative feedback)

We are sincerely grateful to the reviewer for outlining our words with a sketch, which we will add to our summary figure (answer referee #1 comment 2a). We agree with the reviewer that there is a branched chain that includes moisture recycling; this will be addressed in the next comment 2-2. We propose making small changes on how we display the chain – considering the referee comments – and we show them in the answer to comment 2-3.

We outlined the chain previously in line 151. We now decide to briefly mention it in the introduction (line 42) and describe it more extensively at the beginning of the methods section (line 92). See previous answer to comment 2-0.

2-2) Moisture recycling is a well-known mechanism in the Amazon region. You make an effort to separate the Atlantic-mode effect from ENSO. Can you do the same for a direct Atlantic-mode effect versus the following moisture recycling that can be seen as a secondary effect? This deserves at least a discussion. Right now you mention the term moisture-recycling briefly around line 250 without giving an idea of its impact.

We greatly appreciate the referee's comment. It is interesting and challenging to know: How much of the ET from one basin becomes moisture that influences solar radiation and precipitation in other regions?. Previous studies have focused on precipitation, indicating that between 25 to 35% of rainfall in the Amazon comes from the same basin. However, using computational tracers, Dominguez et al. (2022) discovered that this is a short lifetime phenomenon very linked to the diurnal cycle of advected moisture and convection. Apparently, local ET precipitates quicker than moisture from remote locations, especially in the east part of the Amazon (Dominguez et al., 2022). This is also supported by Staal et al. (2018), who measured the distance of transpired water before precipitating again over land, finding that for the particles transpired in the Amazon, the distance is below or around 500 km (which is short for the size of the Amazon basin). Some have determined the influence of transpiration on moisture convergence (Makarieva et al., 2023), yet the potential influence on radiation is still to be determined. Dominguez et al. (2022) also imply that "ET of Amazonian origin is less likely to contribute to downwind precipitation than originally thought".

Both the diurnal cycle and the short distance might imply that the ET influence on moisture convergence and radiation is not as big as could be thought. A quantitative estimation of the influence would require a Lagrangian

analysis and specialized cloud parametrizations to detect the transpiration impact on cloud cover and radiation; that kind of analysis is out of the scope of our research.

We will still expand its discussion by dividing the paragraph in line 250. We will add to line 252 the following text:

Although moisture recycling inside the Amazon comprises between 25% and 35% of rainfall, Dominguez et al. (2022) discovered that it is a short-lifetime phenomenon very linked to the diurnal cycle of advected moisture and convection; recycled moisture precipitates quickly. Staal et al. (2018) measured the distance of transpired water before precipitating again over land, finding that for the particles transpired in the Amazon, the distance is below or around 500 km (which is short for the size of the Amazon basin). Makarieva et al. (2023) determined the influence of ET on moisture convergence, which potentially might influence radiation. It remains to be clarified to what extent moisture-recycling influences radiation availability and soil moisture at other locations in South America; the latter is out of the scope of our research.

2-3) Figs. 2 to 5 are beautifully composed and very rich in the use of data products. They try to both show the chain of processes (panels on the left-hand side) and show ppt anomaly + temporal variability of ppt and ET over the Atlantic modes and when they coincide with ENSO (panels on the left-hand side). This too much and at the same time too little, as the chain is very difficult to follow because of lack of detail. For instance, the ET anomaly is at the end of the chain and its most direct drivers are soil moisture and radiation. The soil moisture anomaly is plotted clearly in a separate panel but the radiation anomaly is given as hard to distinguish contour lines in the ET panel. This suggests that the soil moisture anomaly is more important but Fig 1 shows that most of the region is energy limited.

We are grateful to the referee for noticing our effort to convey several dynamics. Following her/his suggestion, we include now panels on the anomalies of radiation that also show their consistency with the anomalies of satellite-based cloud cover. Moreover, we include the precipitation anomalies as contours over the SM to show the step in the physical processes. Over the last panels that show the ET anomalies (and the last step in the chain), we now draw the local controllers – radiation and SM – in contours. The modified Figure should convey how the modes drive the variability of atmospheric circulation, moisture and its convergence (panels a and b); then affect cloud cover and radiation (panels c and d); which then affect precipitation and SM (panels a, b, e and f); which afterwards locally control the ET (panels g and h), as shown in the next image.

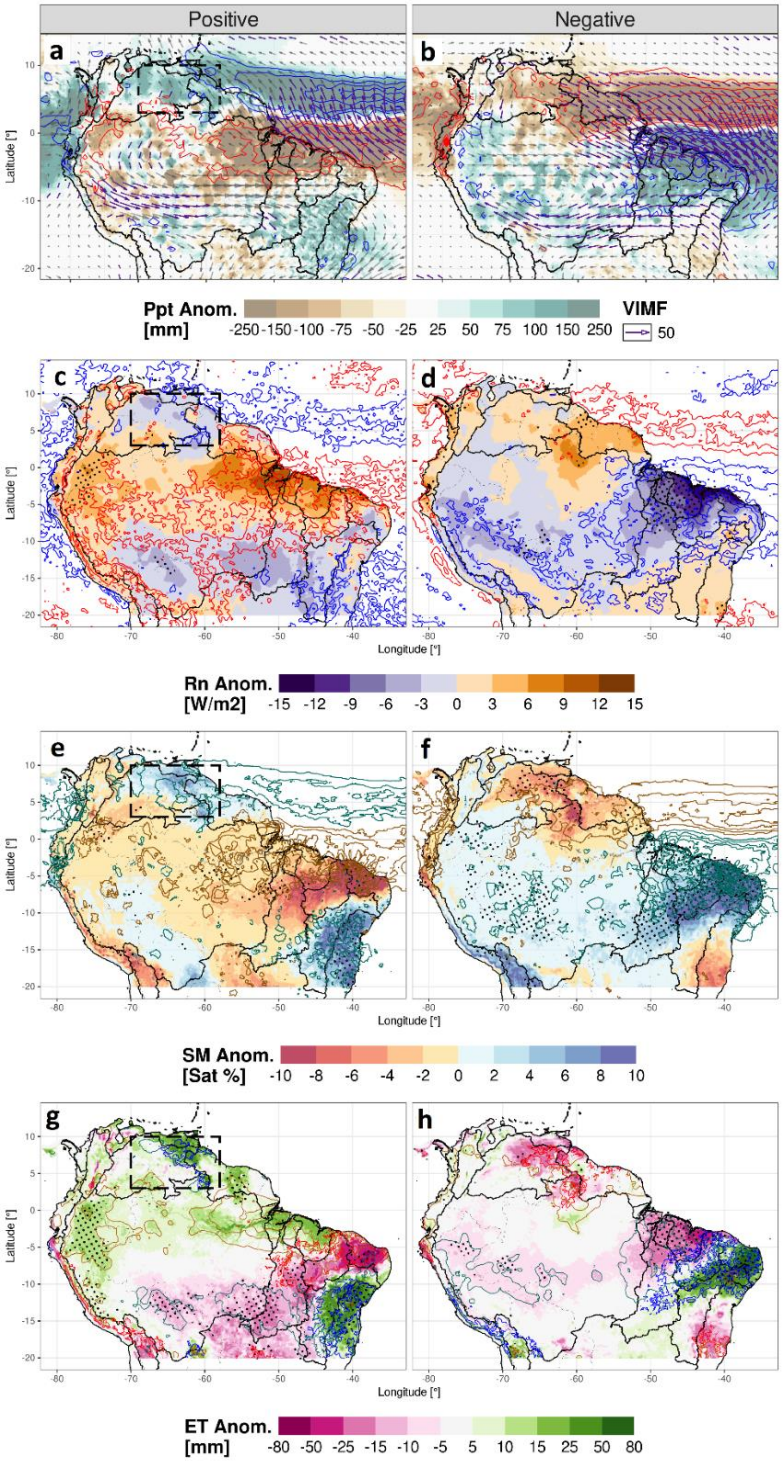


Figure 2. For Austral Autumn - MAM, composites of AMM for (a) VIMF (arrows), MDiv (contours) and MSWEP precipitation (shading) anomalies in the positive; positive MDiv anomalies are drawn in red and negative in blue repeated every 3 kg/m^2, precipitation is in mm and VIMF in kg/m/s. VIMF is depicted in purple arrows when the difference with the neutral phase is statistically significant at a 90% confidence level and in grey otherwise. (b) The same as (a) but for the negative phase. (c) Composites of ERA5-Land surface net radiation anomalies in the AMM positive phase (shadings), and satellite CLARA cloud cover anomalies (contours); positive cloud cover

anomalies are in blue and negative in red repeated every 4%. (d) same as (c) but for the negative phase. (e) Composites of ERA5-Land SM saturation percentage anomalies in the AMM positive phase (shadings), and MSWEP precipitation anomalies (contours); positive precipitation anomalies are drawn in aquamarine and negative in gold repeated every 100 mm. (f) same as (e) but for the negative phase. (g) Composites of ERA5-Land evapotranspiration (shadings), net surface radiation anomalies (contours, gold for positive and aquamarine for negative), and Soil Moisture anomalies (contours, blue for positive and red for negative), in the AMM positive phase; radiation anomalies are repeated every 3 W/m^2 and SM anomalies are repeated every 5%. (h) same as (f) but for the negative phase. In every panel, black stipple dots depict regions where the difference with the neutral phase is statistically significant at a 95% confidence level for radiation in panels (c and d), SM in panels (e and f) and ET in panels (g and h).

The temporal variability panels are moved to a proposed Figure 6 (see answer to comment 2-4). We still want to display the VIMF, the MDiv and the satellite-based precipitation in the same panel since this shows the consistency between different sources of data (reanalysis and satellite); precipitation is also – as suggested by the referee – displayed over the SM (comment 2-5). We will make a more proportional layout with high-resolution images for the revised manuscript. Figures 3 to 5 will be similar to the proposed new Figure 2 but for the other seasons and climate modes.

To simplify the article, we also propose to transfer Figures 3 and 5 to the supplementary material. Consequently, we will keep just two plots of the chain in the main manuscript – Fig. 2 and 4 one for each Atlantic mode – and consolidate the difference between seasons with the following plot. The changes in the location of the AMM impacts will be summarised in the next plot in the main manuscript (new Figure 4), and sections 4.2.2 and 4.2.3 will be rewritten.

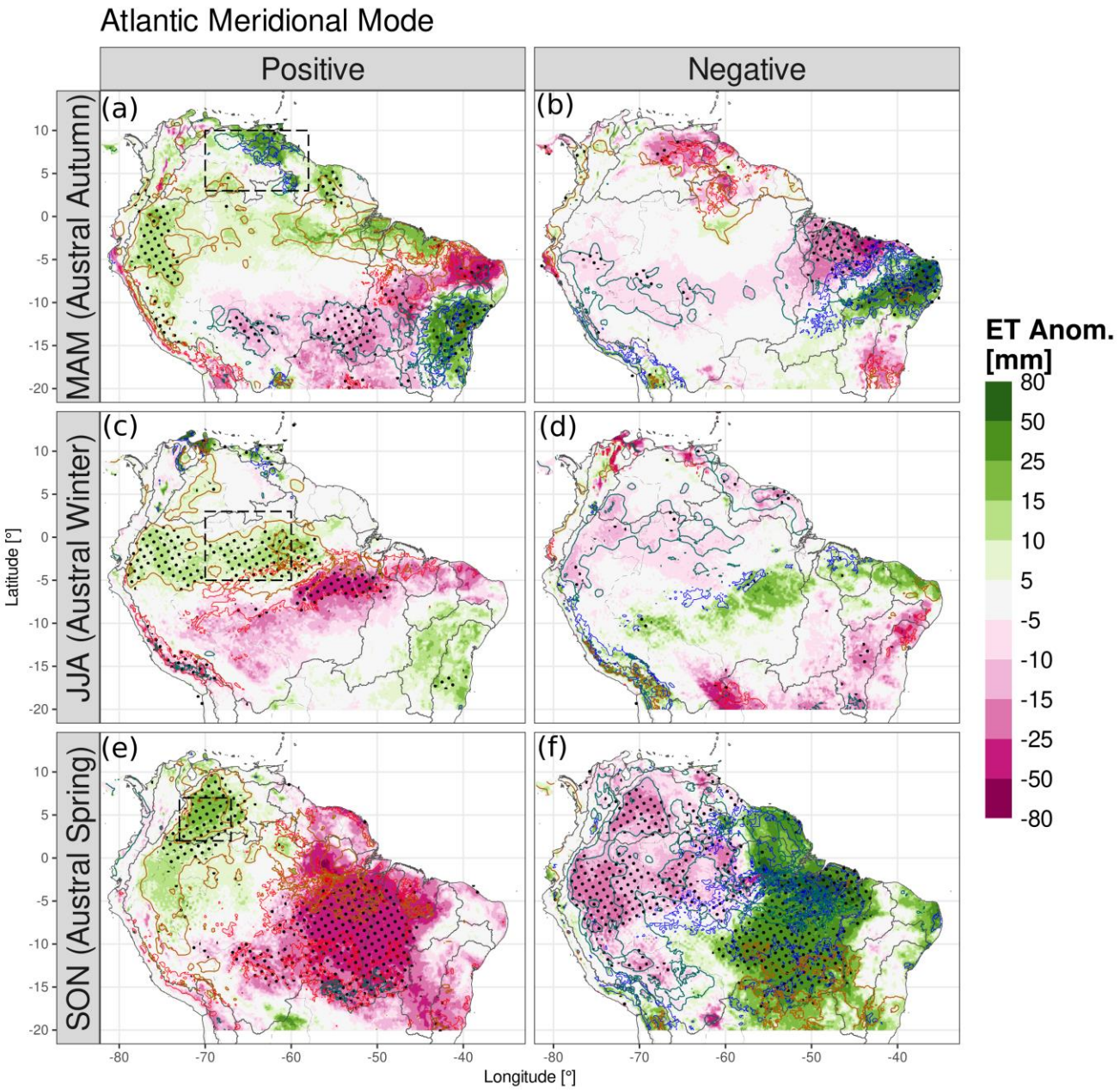


Figure 4. Anomalies of evaporation in the positive and negative AMM phases, for seasons (a,d) March-May, (b,e) June-August, (c,f) September-November. Positive phase in panels (a,b,c) and negative (d,e,f). Blue stipple dots depict regions where the difference with the neutral phase is statistically significant at a 95% confidence level.

Some suggestions to improve this:

We sincerely thank the reviewer for thinking about options to improve the manuscript.

2-4) Move all the right hand panels that are hardly discussed to a separate section or supplementary materials. Maybe in the now very short Section 4.3 as these panels show the interplay with ENSO which is discussed in 4.3?

We are sincerely grateful for the referee's advice, and hence, we have separated some of the righthand time series, simplify and joined them in a proposed Figure 6. The precipitation composite bars in the Figures have been deleted and some precipitation and SM time series have been sent to the supplementary material. This will allow us to describe and discuss specific time series of ET regarding how the modes overlap with ENSO, in section 4.3 of the results.

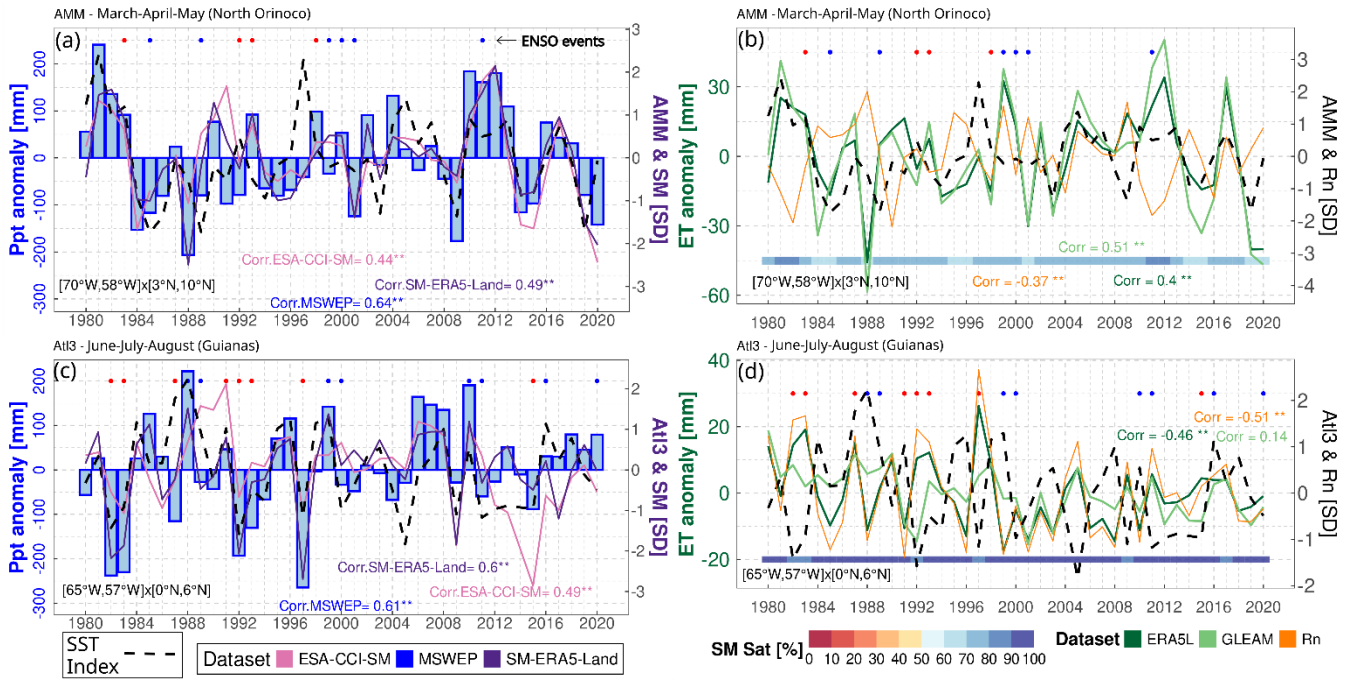


Figure 7. (a) Area-average precipitation (bars) and SM standardised anomalies time series (lines) for the same boxes in Figure 2; the Atlantic index time series is in black dashed lines in standard deviation (right axis), and top points show ENSO active periods (positive phase in red and negative in blue). (b) Area-average evapotranspiration time series (greens), ERA5-Land net surface radiation (orange), standardise Atlantic index (black dashed) and ERA5-Land absolute SM in saturation percentage at the bottom of the panel with coloured rectangles. For all panels, Pearson correlations are calculated between the variable – either precipitation, SM, Rn or ET – with the respective Atlantic index, 95% confidence level is indicated with **. Boxed region: North Orinoco. (c and d) same as (a and b) but for the boxes in Figure 4; Boxed region: Northeast Amazon and Guianas.

We propose to modify the paragraph in line 228, in section 4.3, from:

"Both ENSO and the Atlantic modes are connected through tropical and extra-tropical mechanisms, but each of them has effects on South America's hydroclimate. The partial correlation shows a conjoint effect of ENSO and AMM over the evaporation of northeast Brazil in austral autumn and in JJA (Fig. 6a,b,e,f), yet ENSO also impacts the eastern Amazon and the AMM the Orinoco (see Sect. 5 Discussion). Whereas in SON, the AMM and ENSO tend to impact different regions (ENSO being strong over the Guianas and the AMM over the west and southeast). The Atl3 does not show strong correlations and the ENSO pattern for JJA is very similar to the Atl3 negative phase composites (Fig. 6b and h), indicating some overlapping dynamics with ENSO probably related to the increased divergence and radiation. The impact of ENSO in DJF causes a reduction of convergence and rainfall and increases radiation (Fig. 6d)."

To:

Both ENSO and the Atlantic modes are connected through tropical and extra-tropical mechanisms, but each of them have effects on South American hydroclimate. Figures 5 and 6, separate the effects of each mode in the spatial and temporal dimensions, respectively.

ENSO and AMM have impacts on ET at similar but also over different locations depending on the analysed season. Figures 5a,b,e,f show the influence of both modes on ET in northeast Brazil in MAM and in JJA, yet ENSO mainly impacts the eastern Amazon and the AMM the Orinoco (see Sect. 5 Discussion). ENSO usually also induces droughts in the Amazon during El Niño events – mostly during its peak season DJF – and causes heavy rainfall and floods in La Niña events. Figure 5(c,d) shows the spatial impact of the increased evapotranspiration during ENSO-driven droughts and Figure 6(c,d) displays the impacts on rainfall and ET of specific events (e.g. 1983, 1992, 1997 and 2015). However, Figures 5(a,e) and 6(a,b) show that in the northern Orinoco the ENSO forcing might be superseded by the meridional moisture advection induced by the AMM (e.g. 1983 – El Niño year but higher rainfall and ET; 1985 and 1989 – La Niña year but drought). The correlation of the AMM with rainfall is up to 0.64, and with SM and ET are up to 0.5, all significant. Another period when the AMM superseded ENSO impacts was in La Niña 2010 when the central Amazon experienced a prolonged drought (Fig. 5f and S6); the cause was the positive AMM event (see Sect. 5 Discussion). Note also the reduction or increment of ET when SM changes (water-limited regime). For season SON (Fig. 5 c and g), the AMM and ENSO tend to impact different regions, ENSO being strong over the Guianas and the AMM over the west and southeast.

The Atl3 does not seem to strongly correlate with ET over the Guianas, and the ENSO pattern for JJA is very similar to the Atl3 negative phase composites (Fig. 5b,h and 3h). This indicates some overlapping dynamics between the two modes, which are probably more associated with the atmospheric dynamics of El Niño phase that has simultaneously unfolded with the Atl3 negative phase (Fig. 6c and d and Fig. S8). We discuss the latter in Section 5. Figure 6c and d show the droughts over the northeast Amazon and Guianas at Atl3 negative events with the corresponding increase in ET, also expected effects of El Niño phase. The correlation of the Atl3 with the area-average ERA5-Land ET is -0.46 but 0.14 with GLEAM; the index correlates well with SM and also with rainfall. However, only some Atl3 positive events significantly reduced radiation and ET in the region (e.g. 1988, 1999 and 2008); other events kept ET close to the average (1987, 1998 and 2016).

2-5) Expand the left hand panels: VIMF and MDiv can be combined, but ppt, SM, Radiation and ET should have their own panels. Contours of driving variables could be added on top of a plotted variables, e.g. contours of rain on SM or contours of SM and Radiation on ET.

We thank the reviewer for the suggestion. Even though contours can represent the driving variable in each step of the chain (i.e. Cloud Cover with net surface radiation or precipitation with SM), having more than one type of contour in the panels of ET makes the plot difficult to interpret. The request for the case of ET – where SM and Rn are the two local controllers – implies that we need to draw the contours very thin so that the ET in shadings can be seen properly. The accompanying plots are in answer to comment 2-3 with their respective caption.

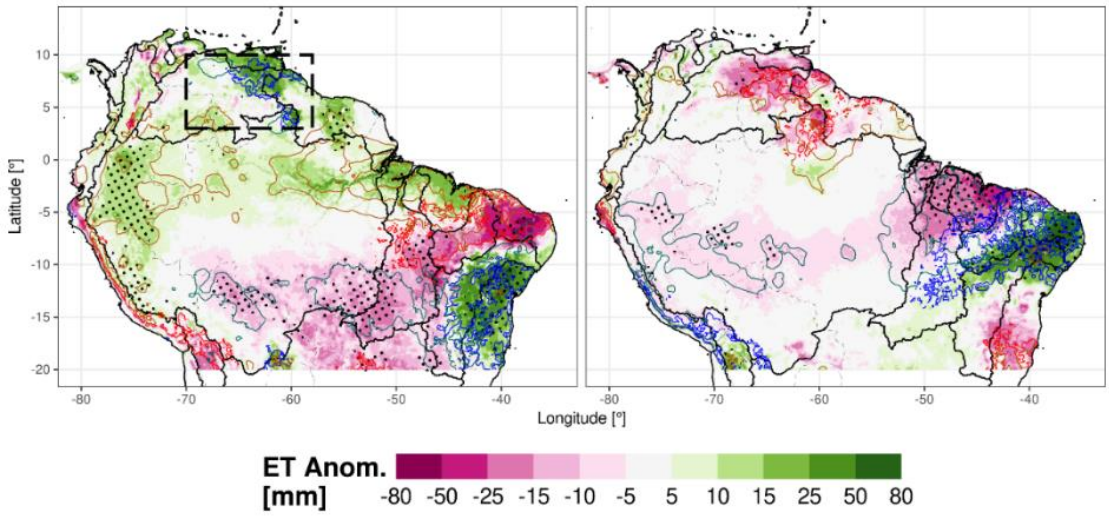


Figure 2. (g) Composites of ERA5-Land evapotranspiration (shadings), net surface radiation anomalies (contours, gold for positive and aquamarine for negative), and Soil Moisture anomalies (contours, blue for positive and red for negative), in the AMM positive phase; radiation anomalies are repeated every 3 W/m^2 and SM anomalies are repeated every 5%.

We proposed the new panels for the radiation – with the cloud cover in contour as a driver – and for the soil moisture (with precipitation in contour as a driver) in the answer to comments 2-3.

2-6) In addition: Black dots are explained for SM but not ET. I assume it means the same thing? Do you need it? It is hardly discussed.

We thank the reviewer for the comment. Even though statistical significance is not discussed much it indicates that the dynamics found in the analysis are more probable than random chance since it has repeated on several occasions. The description of their calculation is explained in line 116.

We have modified the caption of the figure to indicate in each panel the meaning of the black dots (see answer to comment 2-3). The specific text added is:

In every panel, black stipple dots depict regions where the difference with the neutral phase is statistically significant at a 95% confidence level for radiation in panels (c and d), SM in panels (e and f) and ET in panels (g and h).

2-7) Net radiation, now plotted with contours, has relatively small (3 W/m2) anomalies. Is that because the night is included that contributes very little to the anomaly?

We thank the referee for the question. Yes, we consider the nighttime when the net surface radiation is smaller than during the day. However, we want to highlight that the contours are repeated every 3 W/m^2, and in some plots, there are even four levels of contours, meaning that the radiation anomaly reaches even 12 W/m^2 (which should be more visible in the new Figure 2).

We propose to add the word "repeated" to the caption. So it would change from:

"(g) Composites of ERA5-Land evapotranspiration (shadings) and net surface radiation anomalies (contours) in the AMM positive phase; positive radiation anomalies are in red and negative in blue every 3 W/m^2."

To:

(g) Composites of ERA5-Land evapotranspiration (shadings), net surface radiation anomalies (contours, gold for positive and aquamarine for negative), and Soil Moisture anomalies (contours, blue for positive and red for negative), in the AMM positive phase; radiation anomalies are repeated every 3 W/m^2 and SM anomalies are repeated every 5%.

2-8) Does it make sense to add a correlation between the ET anomaly and the radiation and soil moisture anomalies in line with Fig 1 to see which one is dominant in explaining the differences for each Atlantic mode? This could replace Fig1. Alternatively consider adding the ET regimes (Radiation or SM driven) marked in Fig1 to the ET anomaly panels of Figs2 to 5

We thank the reviewer for the question and the suggestion, the other question about Figure 1 and its answer is in comment 3-0. Multiple correlation analysis and multi-linear regression have very similar applications, yet in the regression the assumption of causality is stronger as it specifies independent variables and a dependent variable (in our case, Soil Moisture and radiation impact over ET). The latter was the reason for not using correlations for subobjective #1.

We see the reviewer's point of view that it is uncomfortable to look first at Figure 1 and then at the individual Figures from 2 to 5 to understand the dynamics of the end of the chain. However, we intended to first contextualize the reader with a description of Figure 1 and then describe the results of the chain. That is also why we decided to start Section 4.1 Results with the ET regimes in Figure 1 and show the chain in Section 4.2 from Figures 2 to 5; the classification of the ET regime is the 1st sub-objective. For instance, in the text in line 165, in section 4.2, we stated:

"The Orinoco behaves as water-limited (Fig. 1e) as this is the transition from dry to wet season, then the increase in rainfall and SM causes above-average evaporation (Fig. 2g)"

which links the results of both figures. This is repeated in the description of the results in section 4.2 for Figures 3, 4 and 5 (we propose to transfer Fig 3 and 5 to Supp. Mat., see answer to comment 2-3).

We consider the suggested alternative of displaying the ET regime over the ET anomalies, yet we believe it has the disadvantage of making the Figure even more dense. Plotting the regime with a hatch would not allow to draw the anomalies of the two local controllers – SM and radiation – in the same panels with ET (as shown in answer to comment 2-5). SM and radiation anomalies are located over the anomalies of ET and allow the reader to visualize the last step in the chain. We propose to leave Figure 1 separate from the others with a change (see answer to comment 3-0).

2-9) Variable names are missing in the Colorbar titles of panels a+b (precipitation) and g+h (ET). It is given for panels d+e (SM)

We thank the reviewer for noticing this. We have complemented all the labels in all the colour bar legends; see answer to comment 2-3.

3 - 0) Figure 1: If I understand well to the top panels give the maximum correlation of either SM or net radiation (Rnet). In the panels at the bottom the variable of maximum correlation is given. The "neither significant" seems to indicate pixels where the correlation with SM and Rnet are not sufficiently different to declare a winner. At the same time there are winners that have very low correlations with r2-classes going down to almost 0. Doesn't

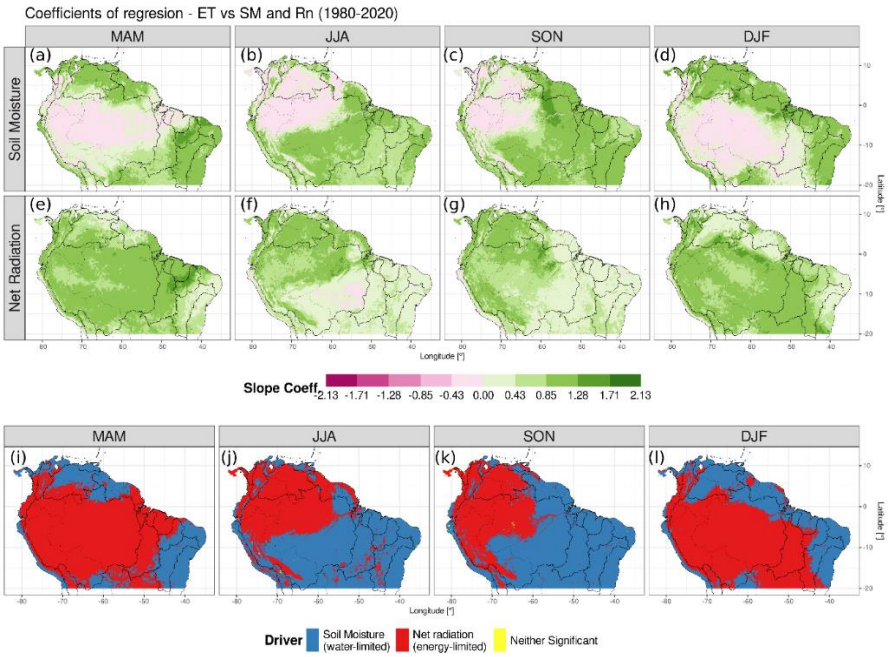
it make more sense to define “neither significant” as pixels where r2 is below a certain threshold (maybe 0.5 or 0.6)?

We thank the referee for the comment. There is a misunderstanding in the referee’s interpretation; Figure 1 top panels (panels a to d) are not correlations, they are coefficients of determination of a multi-linear regression. The latter is written in the Figure caption “(a-d) Adjusted coefficients of determination for the multiple regression ...” and the method is described in section 3.1 Methods line 95 (which corresponds to section 4.1 results where Figure 1 is located). The coefficient of determination is a metric of the performance of the multiple regression and does not refer to the individual independent variables. We included the metric in the Figure to show that it is enough to just consider SM and radiation for explaining the local variability of ET (climate modes are the earth system drivers of ET).

The bottom panels (panels e to h) show the variable with the maximum coefficient of the multi-linear regression, as indicated in the caption “(e-h) variable with the highest significant coefficient.”. This coefficient is the same slope described in the Methods section 3.1 line 95. The significance comes from the t-student test p-value in the regression for each independent variable; we used a 95% confidence interval. Hence, when the p-value for one of the independent variables/predictors is below 0.05 ($p<=0.05$), it means that the variable is a meaningful addition to the multi-linear regression, and it does not depend on the coefficient of determination (R^2). This is another advantage of regressions over correlations; we do not need to specify a threshold. We see that we forgot to specify the p-value we used for identifying if the coefficients of the dependent variables were significant and hence we will add it.

Consequently, the significance of the predictors cannot be defined with a threshold of the R^2. However, a very low R^2 would tend to produce high p-values over all predictors (not significant variable) because already indicates that the chosen independent variables cannot explain the dependent variable.

Since the coefficient of determination can create a misunderstanding, we propose to change it for the plots with the values of the multi-linear slope coefficients; the R^2 will be sent to the supplementary material. The new Figure 1 will be:



Therefore, we propose to change the caption from:

"(a-d) Adjusted coefficients of determination for the multiple regression of SM and Rn standardised anomalies targeting those of ET and (e-h) variable with the highest significant coefficient"

To:

"(a-d) multiple linear regression slope coefficient for Soil Moisture, (e-h) slope coefficient for the Net Radiation and (i-l) variable with the highest significant multiple linear slope coefficient (p<= 0.05)"

We also propose to add the equation of the multi-linear regression to the methods Section 3.1 (line 147).

4 - 0) Tower data: You use model data and satellite remote sensing products that are models themselves (e.g GLEAM) or rely heavily on empirical calibrations procedures (e.g. Soil Moisture). Did you consider to do this analysis for in-situ tower data? There are now many, long-term tower sites in the Amazon, which can be filtered based on the same Atlantic mode index and evaluated for the rain-radiation-soil moisture-ET chain. It would make the message stronger.

We appreciate the comment and question. Yes, we looked for the data but found that the time span was short (most of them after 2000s), for having enough repetitions of the inter-annual climate variability modes. For example, the Atl3 had just two positive events and three negative during JJA after 2000s (look at Figure S6 in our supplementary material). Moreover, there were few ENSO events, making it difficult to separate the effects of this mode from the Atlantic modes. Consequently, even a correlation analysis would probably indicate not statistically significant correlations due to the few degrees of freedom. Baker et al. (HESS, 2021) managed to use one tower with 19 years of data (1999-2017) and highlighted that the records in the other towers were only available for a few years (mainly between 1999 and 2006). Moreover, Jung et al. (2019) produced the FLUXCOM product which is available after 2001, imposing the same problem.

The performance of ERA5-Land and GLEAM ET have been evaluated by several research (Muñoz-Sabater et al., 2021; Xie et al., 2024). Xie et al. (2024) specifically evaluated several products against eddy-covariance towers –

with some of them in the east part of the Amazon – and found a correlation with ERA5-Land of around 0.7 and with GLEAM of 0.6 for the Evergreen Broadleaf Forest (which is the land cover category assigned for the Amazon towers). Furthermore, some have reported problems with the energy balance closure across tower sites (Jung et al., 2019; Mauder et al., 2020).

Therefore, we excluded the data from our analysis and mentioned in the conclusions – line 330 of the original manuscript – that a longer time series could confirm the dynamics exposed in our research. Moreover, in Section 4 discussion in line 258, we cite some studies that evaluated the performance of ERA5-Land and GLEAM against eddy covariance towers.

We still propose to add this description to section 2 Data, in line 66 in a new paragraph:

Some eddy-covariance towers are located in the Amazon and other places in South America and their measurements are –in general– after 2000. Baker et al. (2021) managed to use records from one tower with 19 years (1999-2017) but highlighted that in the other towers the data was only available for a few years (mainly between 1999 and 2006). Other global products based on FLUXNET towers, such as FLUXCOM (Jung et al., 2019), also have data after 2001, which constrains the possibility of registering several events to analyse the effect of the climate modes (few degrees of freedom). The performance of GLEAM and ERA5-Land ET have been evaluated against eddy-covariance towers and have found correlations of around 0.6 and 0.7 for the Evergreen Broadleaf Forest, respectively (Muñoz-Sabater et al., 2021; Xie et al., 2024). Therefore, we choose not to analyse this source of data.

Minor issues – comments specific lines:

Line 26: remove “planning” and replace “achiving” with “achieving

Thanks. Changed.

Line 30: replace “…physical mechanism is...” with “…underlying physical mechanisms are …”.

Thanks. Changed.

Line 37: replace “besides” with “as well as”

Thanks. Changed.

Line 50: previously you also mentioned TNA as relevant Atlantic mode. Motivate why you will not include that one in this study

We appreciate the referee’s comment. We do include the TNA dynamics within the Atlantic Meridional Mode (AMM). Many define the TNA as the spatial averaged SSTA in the region [70°W-15°W]x[5°N-110 25°N]. Many authors have just studied the TNA without considering the southern part of the tropical Atlantic (Arias et al., 2020). However, we discover in our exploratory analysis that the influence of the TSA is needed for finding statistically significant correlations of the climate modes with rainfall, ET and many other variables used in our study. The AMM could be defined as:

$$AMM = TNA - TSA$$

as specified in our manuscript in line 109 - “The AMM monthly index is defined as the subtraction of spatially averaged tropical southern Atlantic SSTA [40°W-0°W]x[25°S-5°S] from the northern domain [70°W-15°W]x[5°N-110 25°N];”

We propose to add to line 111 in the Section 3.2 Composites the next text:

The spatial definition of the AMM comprises also the TNA.

Line 68: “related”??? Do you mean “relates”?

Thanks. Yes, it was a typo. Corrected.

Line 73: You take the first soil layer, is that because the root-zone is defined in this layer?

We appreciate the referee’s question. The dynamics of the lower layers are linked to the changes in the first layer. Moreover, the root-zone in ERA5-Land depends on the type of vegetation as a root percentage is defined for each soil layer (table 8.4 of the technical documentation of the IFS – Cy48r1 (ECMWF, 2023)). There are some vegetation types that do not have roots in the bottom layers, and in many types, the percentage of roots in the 1st layer is the highest. Hence,

Trees with deep roots can still access water from the deep layers when stressed, but this is a survival mechanism, and hence, they access the 1st layer most of the time. We discuss this in line 261 of the original manuscript as requested in a previous submission.

Line 79-80: Incomplete sentence “MSWEP also uses ERA5 rainfall estimates but strongly in the extra-tropics whereas …”

We do not understand this comment; in the manuscript the sentence continues with: “satellite data is given stronger weights in the tropics”.

To clarify if we propose to add:

“where as the ingested satellite data in MSWEP is given stronger weights in the tropics”

Line 95: Rephrase “SM and net radiation are classified with a multi-linear regression slope,..“ to “SM and net radiation are classified with the slope of their multi-linear regression against evaporation, ..”

We are grateful for the proposed change that can clarify the misunderstanding.

Title section 4.2: replace “… and the evaporation” to “..and evaporation”

To also answer referee #1 comment about line 150, we have modified the title of the section from:

“Chain between the Atlantic modes and the evapotranspiration”

To:

“chain of physical processes linking the Atlantic modes and continental evapotranspiration”