This paper addresses the question how moisture recycling ratios change under different future SSP scenarios. The authors study this question by running the UTrack moisture tracking model forced with the NorESM climate model and analyse 10-years of future climate slices under different SSP scenarios. This study on moisture recycling changes towards the future is very relevant and timely, however I have some major comments on the methodology and the reporting of the results. In short, my major concerns are 1) regarding the fact that only 10-year simulations are analysed (10-years is not a climatology), 2) that the changes in the model are not well described and not validated, 3) that present climate simulations are not validated well with literature or ERA5, 4) that relevant literature is missing in the introduction, and 5) that the results section reads as a bookkeeping exercise (of which the information can be put into a table) rather than a story highlighting the main results. I have described my major concerns in more detail below, and have also included substantial and minor comments that I encountered while reading the paper.

We are happy to read that the reviewer finds our manuscript timely and relevant, and we thank them for the constructive and thorough comments. Below we respond to these comments in more detail and explain how we aim to deal with them in a revision.

Major comments

10-years of simulations do not present a climatology

The results of this study are based on moisture recycling ratios calculated for slices of 10years of climate data (present and future). In climate terms, this is a very short period to draw conclusions from, taking into account the internal variability of our climate. By only analysing 10 years, it could happen that your results are biased to multiple dry or wet years present in the data. While the authors address the fact that they do not study interannual variability (although presenting standard deviations around moisture recycling ratios, which is an indication of interannual variability), they do not acknowledge the short 10-year timeseries as serious constrain to base their conclusions on. Further they do not compare their 10-year land precipitation and land evaporation results with a wider range of models to validate if the 10-years is a good representation of (present) climatology.

It is true that a ten-year period may be too short for a climatology, because of variability in the climate. Still, we wanted to compare equally long periods with each other and we considered the first ten years of the simulations (2015-2024) representative of “the present”. In the revision, we will explore the effects of using this ten-year benchmark. We expect either to increase the window or to present in the supplement additional results for the other ten-year windows if these results demonstrate that our ten-year period is long enough to account for interannual variability.

No validation is performed for present climate and with the new model set-up

To continue on the previous comment, one way to verify if present climate from the NorESM simulations is actually representing our current present climate, one could validate the moisture recycling ratios from NorESM baseline with moisture recycling ratios from ERA5.

The authors have published simulations with the UTrack model forced with ERA5, which is the perfect reference dataset for this study, and I am surprised to see no validation is done at all. Validation is recommended in two ways: a) to validate how well NorESM performs in representing moisture recycling ratios in current climate (2015-2024) and b) to validate the described model changes. With the latter I mean that a different model set-up is described based on the constrains of available data from NorESM and the impacts of using only limited input data (daily timestep, only eight pressure levels in the atmosphere). The impact of using daily data and limited information in the vertical (and horizontal) can be perfectly validated with the ERA5 dataset. One can run the UTrack model in the standard ERA5 set-up, and run the UTrack model with the input from ERA5 based on the constrains of NorESM, This will allow to illustrate the impacts of using limited data resources, which is currently not addressed in the paper at all. The fact that there are some grid cells that show higher precipitation recycling then precipitation itself (lines 264-269), might be related to the fact that only daily data or few vertical levels are used.

Thank you, we appreciate these suggestions. We agree that a "validation" – even though it would not be a true validation against observations – using ERA5-based global moisture recycling is valuable. Still, we want to stress that we are mainly interested in relative changes in precipitation recycling, exactly because of possible model biases. Having noted that, we can "validate" the global patterns of precipitation recycling patterns using ERA5-based results of Tuinenburg et al. 2020 (https://doi.org/10.5194/essd-12-3177-2020), which are also run for a ten-year period (2007-2018). In our revision, we will provide and discuss comparisons of the global precipitation recycling patterns from Tuinenburg et al. (2020) and the current study.

Regarding the different set-up of the ERA5-based model: we appreciate this suggestion and agree it would be very interesting, but it lies outside the scope of this paper. This model transformation is not easy to do, the runs would still be very data-heavy and computationally expensive, and it would deserve a study on its own as we would have to do a systematic and global sensitivity analysis of all model changes. In Tuinenburg & Staal (2020, https://doi.org/10.5194/hess-24-2419-2020), we did perform some sensitivity analyses on the ERA5-based model version that we may use to interpret possible differences between the ERA5- and NorESM-based runs.

This point is also interesting in relation to the sensitivity of the results to different ensemble members of NorESM (also see our reply to Ref. 1). We believe that extra sensitivity analyses and model comparisons are very important. Indeed, currently a moisture tracking model intercomparison study is being carried out by an international community of moisture recycling modelers. This effort which will shed light on the importance of these model assumptions for moisture recycling results.

In addition, daily data from NorESM is used to force the UTrack model (Line 118: has a temporal resolution of one day). I assume that for wind fields and specific humidity instantaneous data is used and I wonder at which timestep of the day this data is taken? This is not stated in the methods and influences your results. If instantaneous wind fields are taken at midnight, features like a low level jet will enhance moisture transport, compared to instantaneous wind fields taken at noon. Opposite, sea-breeze features which

enhance ocean-to-land moisture transport are mostly present during the day and thus will also influence your results when only one timestep during the day is used. This issue can be addressed by running UTrack with hourly ERA5 forcing, and with daily ERA5 forcing (as suggested in the previous point).

We agree that the daily resolution of the data from NorESM is a limitation. Diurnal fluctuations in winds, evapotranspiration and precipitation are thus averaged out, but it does not mean that only moisture flows at e.g. midnight or noon are accounted for. We will discuss this limitation more deeply in the revision.

Last, I am a bit confused why the authors use a forced climate scenario to analyse past climate. I can follow the logic to take the scenario that is following the trajectory that the world is currently on (Fricko et al., 2017) (line 165), though citing a paper from 7 years ago feels a bit odd then. Wouldn't it be much more logic to use climate data forced with observed CO2 levels and observed SSTs?

We decided to do this for internal consistency, allowing us to isolate the relative differences in precipitation recycling during this century. We agree that citing a paper from less than 7 years ago would be more appropriate and we will look for the most up-to-date reference for this.

Introduction does not include all relevant literature and hypothesis

In the introduction the authors state that (line 50): "However, where, how, and to which extent terrestrial moisture recycling will change in the future remains unclear." Although I agree there is still research to be done on how moisture recycling is changing to the future, there is also literature available already that addresses and (partly) answers this statement, and this literature is only cited in the discussion. An introduction needs to state the relevant literature on the topic and this is currently not done. Examples of literature that addresses the changes in moisture sources or moisture recycling in a future climate (Benedict et al., 2020; Findell et al., 2019; Fernandez-Alvarez et al., 2023). Furthermore, the introduction is also the moment to state hypothesis based on current literature, for example addressing the impacts of land-use change versus climate change. Currently the introduction provides more insights on the methods, describing the SSP scenarios and different moisture tracking methods which I found more relevant for the method section, or can be reduced.

We agree. We will make an effort to include the most relevant literature in the Introduction and we appreciate you pointing us to these important papers.

Improve results and discussion section

In the first section of the results (line 187-192) the absolute and relative changes in land precipitation and land evaporation are given from NorESM. It would be very good to put these numbers in the perspective of a multi-model mean, for example given in Table 8.1 of Chapter 8 of the IPCC 2021 report, Douville et al., (2021). This indicates if the precipitation and evaporation averages from NorESM2 fall within or outside of the range of the CMIP6 multi-model mean.

This is a good suggestion. We will provide this context in the revision.

The result section reads as a bookkeeping exercise, where multiple alinea's (four alinea's from line 210 to line 245) have exactly the same structure but different numbers inserted for the different scenario's. This makes the result section dry and hard to read. This information suits well for a table instead, while in the text rather the interesting findings of the table can be reported.

Also reviewer 1 suggested to include a table to summarize the results presented in lines 211-245. We will do this.

Further, my suggestion would be to combine the results and discussion section to allow for direct comparison with literature. Currently, the result section is very dry as it is a sum-up of numbers and scenario's. By directly comparing moisture recycling ratios with the literature (combining results and discussion) allows for more perspective. At the moment, in the discussion the numbers from the result section are not repeated, which makes it very hard to put literature results next to the results of this study, which I think is very important to do. The same holds for the results on the major river basins, which could include more references to current literature.

We need to comply to journal requirements regarding paper structure, but we take this suggestion at heart and will reorganize and rewrite the Results section to improve its readability. Also reviewer 1 provided a lot of useful suggestions in this regard.

On the discussion on the impact of land-use change and climate change on recycling ratio. I think this is a very interesting discussion point which is now addressed only shortly, I would dedicate a whole section on this. Are their different ways forward to test this influence? Some arguments that are provided later in the text (line 411) could potentially also be used to study impact land-use change vs climate change.

This is a good suggestion. We will elaborate on this and propose ways forward to test the influence of climate change versus land-cover changes, likely in a separate section.

Substantial comments

Line 40-50: Besides including relevant literature that assessed moisture recycling under a warming climate it is also good to address the impact of circulation changes on moisture recycling, such as changes in location of the ITCZ, Hadley cells, storm tracks, as this will affect the moisture transport as well.

We will add reference to circulation changes in the Introduction.

Section 2.3 Simulations settings; I already addressed the issue of daily data in the major comments, but the limitation of only having limited pressure levels as input is not discussed at all. What is the impact of this on your results?

We believe the number of pressure layers (eight) is quite large, although it is smaller than in ERA5. See also the earlier comment. A systematic assessment of this effect is beyond the

scope of this study, but Tuinenburg & Staal 2020 (https://doi.org/10.5194/hess-24-2419-2020) did do some tests and found that severely degrading the vertical moisture profile can have substantial effects, affecting moisture transport distances in the order of hundreds of km. We will include a discussion of this in our revision.

Line 176-184: Can the significant increases and decreases be quantified? Did you use a threshold to call it a significant increase or decrease?

Yes, we used α=0.05. Thanks for pointing out this missing information.

Section 2.4: When is the model initialized? As already mentioned I am a bit surprised that for current climate a scenario is used, while we already have the observations of current climate as the boundary conditions of the model. I could imagine this is done for consistency, but it would be nice to check how well the baseline run represent the actual conditions. Further, are these atmosphere only runs? So SST is prescribed?

Indeed, we do this for consistency. As explained above, we will use the results from Tuinenburg et al. (2020) to compare the global patterns of precipitation recycling against. The NorESM runs were coupled, without prescriptions of SST (Seland et al. 2020, https://doi.org/10.5194/gmd-13-6165-2020).

Line 174: Do I understand correctly that also for the SSP3-7 and SSP1-2.6 and SSP 3-7.0 you use the same climate sensitivity as SSP5-8.5? The approach here is unclear but you would expect that the climate sensitivity is used per SSP scenario to calculate changes in moisture recycling per degree warming.

We based it on only SSP5-8.5, because it has the largest increase in temperature, allowing for greater accuracy. We will clarify this.

Further, we are currently already warmer than the 1976-2005 baseline that is mentioned for which the 3.26 degrees is determined for. In the results a 3.26 change in temperature is used to move from the SSP2-4.5 (baseline; 2015-2024) to the SSP5-8.5, but I assume there is already some warming in the SSP2-4.5 baseline run for 2015-2024, which is now not taken into account. Thus if I understand the taken approach well, using the 3.26 degrees warming is incorrect.

This is correct and it may lead to a slight error. We will improve this calculation by correcting for warming since the 1976-2005 baseline.

Results

Line 177: “wetting, land dominated” if a significant increase in precipitation coincides with a significant **decrease** in terrestrial precipitation recycling --> should this not be exactly opposite? An increase in terrestrial precipitation recycling? Line 461-462 also states that land-dominated means increase in terrestrial precipitation recycling

Thank you for spotting this typo, it should indeed be the opposite.

I would suggest to leave the min and max value of moisture recycling out of the text to make it more readable. Those min and max values could be reported in a table. By providing the std you give an idea of the interannual variability in the text.

We agree that the minima and maxima can also be included in Table 1.

Line 213-215; How relevant is it to give this information if it only concerns such a small percentage of land grid cells (1.1% and 1.3%)? Instead, it would be nice if some words are dedicated on where those 8.7% of land grid cells are on the globe that show a significant change in precipitation. And do you mean with precipitation absolute precipitation or precipitation recycling? In line 170 it is stated that statistical significance is tested for precipitation recycling, but from the result section it reads as if it is checked for absolute precipitation changes, which is confusing. I read in the caption of Figure 2 and 3 it is about significant differences in precipitation recycling, it would be nice to have those regions that show a significant change are hatched in figure 2 and 3.

We believe that even if it represents a quite small area of the global land, these numbers are still relevant to report. We agree, though, that some more detail about where the areas with significant changes are located will be a worthwhile addition. We will also try to clarify to solve mentioned confusion.

Line 279-277: I am not sure about the purpose of this alinea. Are these findings of this study? Or are these numbers given here to indicate the impact of land-use change on moisture recycling? If so, I would discuss them in combination with the discussion section 4.1.

These are numbers we calculated, but in essence are not novel results. Indeed we provide these numbers so we can better interpret the land-cover versus climate change effects on moisture recycling. In our restructuring of the Results section we will take this point into account.

Minor comments / typos / small unclarities:

Define precipitation recycling in abstract

OK.

Line 20: moisture recycling ratio--> do you mean with moisture recycling precipitation recycling? Terms are used throughout it each other but it is unclear what is what

Thank you, we will be more clear.

Line 108: For equations --> equations of what? Of the moisture tracking model?

Yes. We will specify this.

Line 134: ‘There is some overestimation of global mean temperature’ --> this is very vaguely stated, can you quantify?

We will be more specific based on the referenced paper.

Line 148: ‘We used these forcing data directly without interpolation’ --> How can you have daily data and run the model on 4-hourly timesteps, without interpolation?

Individual moisture parcels may cross multiple grid cells during one time step if the time step is too large. This may cause errors in the parcel trajectories, which is solved by taking a sufficiently small time step, even if the data themselves are not interpolated.

Line 159: In Line 153 it says 1000 parcels per mm, and here 100 parcels per mm

Correct. We used different settings for the global runs and the basin runs, based on the fact that the areas of the two differ by orders of magnitude. Effectively, the 100 parcels per mm in the basin runs mean that the same parcel already represents a larger volume of water in the basin runs compared to the 1000 parcels in the global run.

3.1 Global land --> can you make the headers more self-explanatory? The result section will also benefit from more section and section headings to illustrate the red-threat

We agree it would be good to make the headings more self-explanatory.

Line 492: less clouds but more rain? Maybe I misunderstand

We will delete “likely linked to underestimated cloud cover”, as the point simply is that precipitation over oceans is overestimated in NorESM2.

Line 499: ‘Around one-fifth of global precipitation is attributed to vegetation’ --> to me it is not clear what is meant with this sentence

We will rephrase. What is meant is that vegetation, through enhanced evapotranspiration, is estimated to be responsible for one-fifth of global precipitation, that is, without vegetation global precipitation would decrease by that amount.

Conclusions

Line 538-539: ‘widespread drying accompanied by disproportional reductions of moisture supply over land’ à this sentence counteracts the argument of global greening and increased evaporation (stated in the discussion)

We will be more clear that here regional-scale drying is meant, such as in the Amazon and eastern Europe, which contrasts with the global average.

Here the word disproportional is often used, what is meant with that?

We mean that the relative change in moisture supply exceeds the relative change in precipitation. We will make sure this is clear to the reader.

Line 541-542: can you back-up this last sentence by findings from the study?

This is a high-level take-away based on the discussion of our results in the context of the literature. We will make this more explicit.

Figures

Figure 2 to 6: These figures can be improved and made more readable by only displaying one legend (colorbar) per figure, and not for all subfigures. In this way the figures can be enlarged.

Good suggestion, we will do that.