Dear Editor,

Thank you for giving us the opportunity to revise our manuscript; we hope that you find our revisions and responses to reviewers acceptable.

We hope that our opinion goes some way to addressing a deficit within the hydrological community, namely that that the land-use community and the $CO_2$ community act separately and largely ignore each other. Both communities deliver a biased explanation for the increased occurrence and severity of floods and droughts, as long as land use and $CO_2$ are not considered together. To highlight this deficit, we found it more appropriate to express our title as a question that requires to be answered in the future. This also complies with the suggestion of referee #2 that the title should better reflect the opinion character of our manuscript. The title reads now:

HESS Opinion: Floods and droughts - Are land use, soil management, and landscape hydrology more significant drivers than increasing CO2?

We hope this manuscript emphasises the need to tackle all drivers of floods and drought together and that this is a message that HESS would like to see published as an opinion..

Thank you for your consideration.

Kind regards

Karl Auerswald, Juergen Geist, John N. Quinton, and Peter Fiener
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Dear Editor, dear Reviewers:

Below, the reviewer's comments are given in black, while our response is printed in blue. We inserted the respective parts of our previous manuscript as comments to allow an easy comparison without the need to switch between documents.

**Reviewer 1**

The authors revised their manuscript in response to the comments and suggestions of two reviewers, one of them me. The authors clarified sentences, moved figures from a modelling study and corresponding explanations from the appendix and added a new section on scale effects. They didn't modify or tone down their arguments.

We thank the reviewer for acknowledging that the adjustments improved the clarity of our paper. However, we disagree with the reviewer that modifying or toning down our arguments would improve the paper. For instance, it is well-known how land use influences hydrology. Hence, we do not believe that toning down and writing something like "sealing may increase runoff; compaction may increase runoff…" would improve an opinion paper. The disagreement only results from interpreting a complex system response like flooding or drought, when suddenly only one explanation ($CO_2$) seems to be allowed. We argue that these land-use-related mechanisms must be fully considered when explaining extremes like the Valencia flood. Should we write "land use may be considered as well"? This would clearly be wrong. According to the journal guidelines, especially opinion papers are intended to deliver clear messages and inseminate discussion. This impact would be lost if we further tried to tone down our argumentation.

I'm sympathetic to publication since this is an opinion piece that will hopefully attract scholarly debate. I would still suggest the following revisions to improve the authors' arguments. They mainly relate to their arguments against CO2-driven causes of floods and droughts, which, as reviewer 2 noted, are rather unbalanced and suffer from mixing and matching various kinds of information (data, theory, models) from various spatial and temporal scales. The authors' arguments for a greater consideration of landscape scale drivers of floods and droughts, on the other hand, are generally balanced.

Thanks for being sympathetic to the publication despite our different opinions on some of the suggested aspects – we also hope that this paper will initiate further debate on this topic. Concerning the additional suggestion for revision, we think this is too unspecific to be answered. We took great care using official data from a well-defined region. We consider it a strength of the paper that we use multiple data sources from the published literature and that we do not exclusively rely on either data, or theory, or modelling. Furthermore, mixing measured data and models is not our responsibility because we rely on the suite of published studies on this topic. Furthermore, this situation is generally unavoidable in hydrology because several parameters like groundwater recharge, evapotranspiration or runoff are usually derived from modelling, particularly when a large region and a long period is considered.

A lot of the authors' arguments hinge on the plausibility of the modelling studies they put forward to support their arguments. These models are not scrutinized at all with respect to their model structures, parameter values and input data. And none of them include the complexity the authors argue is needed! If the models only include climate drivers, which the authors use to isolate climate effects (and ultimately suggest they are minor), how can the models ever shown to be plausible?

We fully agree with the reviewer that models, which are indispensable in hydrology, are a critical point. Models can – at best – only find things that are included in the model. We then conclude that those parameters are important, which is circular reasoning. We added a sentence on this to the conclusions.

Next, I pinpoint specific places where the argument is weak in that sense in addition to other comments.

Specific comments:

Title: I wonder whether "temperature" should be replaced with "CO2" because the authors talk in these terms in the text (CO2 is also the section 2 heading). Especially as they explain how landscape scale factors can also increase temperature. Making this change I think will make the framing of the paper clearer.

Thanks for spotting this. We agree and have changed this accordingly. Following a suggestion of Ref. #2 we also modified the entire title and present it as a question to avoid the impression of a definite answer.

L39: What is a "common relation"?

The decreasing birthrate with the decreasing number of storks. This is a special case of spurious correlations, sometimes also called confounder correlation. Such relations can often be found when comparing time series. We replaced the word by the better known but wider term "spurious relation"

Equations 1 and 2: Following a concern of reviewer 2, I suggest that the authors include a note on scale for both equations. At which time and space scales are they expected to hold?

We use the equations only for illustration purposes and not for our own calculations. The two equations are intended to illustrate the following:

1) Both equations are coupled. This applies for all systems and scales where evapotranspiration happens
2) If one parameter changes, at least a second parameter must change to close the equation. This applies for all balances.

Allen et al. recommend these equations for time scales of days to years and fields to landscapes. The parameters and how they have to be derived will differ depending on scale. We added that, in principle, the equations apply to all scales but the relative importance of the different terms changes with temporal and spatial scale.

L50: The discussion here seems unbalanced: In addition to R_nl, CO2-driven climate change also impacts all other factors via cascading effects, whereby CO2-effects interact with landscape scale factors.

We disagree on this point. The discussion is perfectly balanced because we name only those parameters that are directly influenced, while we neglect all cascading parameters. Hence, we treat $CO_2$ and land use equally. However, we explicitly state now, that cascading effects are ignored.

L55: From the response to reviewer 2 I understand that the authors wanted to add "on average" here, which I find a good idea!

We added “on average” after country-wide

L67-69: Here, the authors jump to quickly from a “typical Mid-European setting” (if one accepts that Bavaria is typical for Mid-Europe) to “will occur globally”, even if they note regional differences afterwards. There is no evidence provided to make such a statement. Regional differences can easily overwhelm any similarity.

We mention that there are large regional differences. However, the general trend that agricultural machinery weight increases, road density increases, and soil sealing increases can be found in many regions. We have added ‘most regions’ to globally as there might be settings where this in not the case.

L79: Rather than “constant energy”, with reference to equation 2 the authors have noted an increasing energy input with climate change. The argument needs to be adjusted in that light.

We are not aware of any mechanism for how $CO_2$ can influence solar radiation.

L98-101: There is another contradiction here between the statement that any trend was minor compared to spatial variation (L98) and the statement that the changes were spatially relatively uniform (L101).

We do not think the was a contradiction because these are two different statements. The spatial variation of the parameters is large but the spatial variation of change is small. Both sentences are correct as they stand.

Figure 1: The model results here are given on a decadal scale and extend until 2015 only. First of all, the model needs greater scrutiny before the reader can place any trust in it. Second, important variations might be hidden in the decadal figures, i.e. floods and droughts. It would also be relevant to see the results for the most recent 9 years.

Please note that these are not our results. Hence we cannot change the resolution or the period that was considered. How likely is it that nine years in a 65-yr period would make a large change, in particular as these recent years did not differ much regarding precipitation as shown in the following figure. Temperature has risen further but this increase can already be nicely seen until 2015.

L113: It is not demonstrated in the paper that these years show no CO2-related pattern. The authors should either present this analysis or quote it if this was done somewhere else or delete the statement.

We can assume that every reader roughly knows that $CO_2$ increases over time with highest concentrations in recent years. We show in this sentence, that there is no trend to drier years or wetter years. The same can be seen in Figure 1 and Figure 2. Providing $CO_2$ concentrations would not change our sentence.

From the driest to the wettest of the five driest years, [$CO_2$] was: 300, 378, 311, 376, 309 ppm
From the wettest to the driest of the five wettest years [$CO_2$] was: 305, 304, 319, 312, 310 ppm

There is no descending or ascending order of [$CO_2$] when approaching the average. Furthermore, between 1900 and today, [$CO_2$] ranges from 300 to 425 ppm. The high concentrations do not fall together with the five driest or five wettest years.

L113-114: How is the winter precipitation trend related to CO2? The authors should analyse all these aspects symmetrically, at the same level of detail.

If desired, we can delete the part after "while". It has a marginal influence on our manuscript. We would prefer this over expanding. Expanding land use effects would be much more valuable because the changes that happened there are orders of magnitude larger and apparently widely unknown.

L121-122: I suspect the authors of that study did a Null hypothesis test of the trend and could not reject the Null. But this is not the same as no trend! And what exactly where these "sophisticated statistical tools"? This is another example of an unbalanced (here uncritical) discussion. For the reader to understand the argument, these studies need to be scrutinised at the same level of detail as other studies that the authors are more critical of.

These are published studies that the German Weather Service released. They provide the base for all planning in Germany that requires return periods. We were extremely critical in this case because of the relevance. The implications by the reviewer are not justified. Shehu et al (2023), appeared in HESS, it has nine (!) pages of statistical methods. Explaining them would completely distract from our topic with no additional insight. Nevertheless, we especially recommend reading Shehu et al. to all interested in rain data because they were able to identify shortcomings of rain-gauge measurements that could falsely lead to a trend in heavy rainfall. Willems et al. go even more into detail in this respect with illustrative figures but this is unfortunately in German.

Figure 2: Please specify what the black line is. The running median? And haven't the authors complemented the graph with years 2000 to 2024 (not 2020 to 2024)? And why complement at all? This makes the graph confusing. Can the calculations not be repeated with the original and extended dataset? This would make it easier to understand for the reader and help the argument.

The black line was a running 30-yr mean that we included because it was in the original publication. It has no relevance and we will delete it. Adding the years 2020 to 2024, which were later than the study, has the purpose to have a wider overlap of measured data with climate projections to show the good agreement between both. The original data and the complemented data are homogeneous because all data were taken from the same source (published country-means by the German Weather Service). We will additionally cite this source.

Please note that your critique of the previous figure was the opposite to your critique here. In Figure 1 we were not able to add years after the study appeared because of the complex modelling that had been used in this study.

L152: Here is another example that calls for a more symmetrical analysis: How was "the only parameter" determined? How was the CO2-driven climate change signal (or lack thereof) determined? With a model? With data?

The titles of the references clearly show that these are measured data. It is important to note that both publications used independent data and approaches. The influence of the $CO_2$-driven signal was (also) analyzed in several follow-up publications that we do not cite to avoid inflating self-citations. The mechanisms relating to $CO_2$ are rather complex and irrelevant for this manuscript. One mechanism is that winter precipitation changes increasingly from snow to rain, which causes a pronounced change in the seasonality of erosivity. To make it clearer for the reader we added "The only measured parameter …"

Figure 3: For symmetry, this figure, too, would benefit from a trend analysis. From the appendix I understand that runoff was calculated with the SCS curve number method and erosivity with a transfer function from precipitation and temperature. This should be briefly mentioned in the caption and/or text, even if the appendix is referred to for details. For a balanced analysis, I would also expect a discussion of the limitations of those methods (some of this is already in the appendix for SCS). The erosivity model must be sensitive to the exact values of the exponents of the P and T effects (since small changes amplify through the power law), which is downplayed by the logscale of Figure A3.

The figure superimposes measured (historic) values (in cases of erosivity) or data from official, up-to-date modeling (in the case of direct runoff) based on measurements with modelling using data from climate projections. The behavior that we describe is fully visible in the historic data. If desired, we can delete the data derived from climate projections. This would have no effect on the entire manuscript, which focusses on the explanation of droughts and floods in the past.

The trend analyses exist. For the historic data they are published. They cannot show anything else than the very clear "raw" data.

L170-171: This statement the authors should qualify by adding "based on model simulations".

The reviewer is not correct. There is no modeling involved. The change of rain erosivity is based on measurements and the still undetectable change in the return periods of heavy rain is also exclusively based on measurements.

L187: This contradicts with statements in the previous paragraph where in the chain of events triggered by increasing erosivity runoff was argued to increase!

Yes, there is a contradiction but still, our sentence is correct. In hydrological modelling with the SCS curve number but also with other common approaches, rain erosivity is not considered. Ironically, the reviewer criticizes our conclusion on line 480 (see below) that runoff modeling should take soil crusting and infiltration-excess runoff into account. Furthermore, an increase in rain erosivity does not cause more runoff as long as the soil is protected from crusting. An increase in runoff due to increasing erosivity has to be assigned to a land use that does not keep the soil covered.

L190-191: Arguably the CO2-driven ET effect is subject to the same self-intensifying effects described in the previous paragraph. Even if the CO2-driven ET effect is modest to begin with.

In an arid environment, this may be the case, but in a humid area with functional soils, it will be extremely rare that a 5% increase in ET will deplete the soil. As long as this is not the case, the self-intensification and self-propagation is not initiated. We clarified:

"Therefore, a 2 K temperature rise would increase evapotranspiration by only 5 %, **which should be buffered by functioning soils in humid areas**. Consequently…"

L191-193: In L191 it says 2-3%, in L193 5%. Is this a typo? Or what is the difference?

No, this is correct. The reviewer probably missed that the unit of "2-3" is %/K while the unit of the second number is % because it was derived from the first number by multiplication with the change in temperature.

However, we adapted the sentence a bit to make the relation between the 2-3 % evapotranspiration per K and the increase in evapotranspiration in case of a temperature increase of 2 K clearer.

" … evapotranspiration rises only modestly by 2 to 3 % K-1 temperature increase (Lambert and Webb, 2008; Roderick et al., 2014; Bürger et al., 2014; Skliris et al., 2016). Therefore, a 2 K temperature rise would increase evapotranspiration by only **4 to 6** %...."

Figure 4: The authors modified the figure in response to my comment, but I still don't understand why the crucial ET argument is missing. At the top of page 8, the argument is put forward quite succinctly. Could this be transferred to the figure?

We had added a chapter to the supplement dealing with the ET argument, which shows, in accordance with the cited publications, why the $CO_2$-driven effect is small (2 to 3% per K). The reviewer seems to expect a much larger effect.

What we describe on top of page 8 has nothing to do with the $CO_2$-driven effect but it describes the self-propagation and self-intensification of drought in landscapes that are poorly buffered. At least in central Europe, this is caused by dysfunctional soils (due to drainage, compaction, sealing, poor cover). The increase in runoff causes the drought. This is included in the figure.

L218: The modelled figure of 528mm ETa per year must already account for some of the sealing historically, even if implicitly by adjusting other processes to fit the historical observations. How can we otherwise trust what the model predicts? The analysis seems to be too simplistic here, placing too much trust in a particular model to support an argument, without scrutinising the model.

ET modelling is usually based entirely on meteorological parameters without accounting for the reasons that shape these parameters. If dry surfaces cause temperature to increase and humidity to decrease, this will increase modelled ET. Hence the sealed surfaces are included without explicitly accounting for them. In consequence, the effects of sealed surfaces are usually overlooked and it will be tricky to disentangle the influences. We did not model ET but we clearly stated that we just made simple balance considerations to show that the effect is not negligible and deserves disentangling.

The surprising thing is that our simple balance consideration reproduces the gap groundwater recharge. The Bavarian Environmental Agency, responsible for the observation wells, find a similar decrease in groundwater recharge although rainfall has not changed.

L258-259: I believe it should be 6% in both instances. As above with ETa, the recharge figure of 206 mm per year must already include the sealing to some extent, even if implicitly through effective parameters.

Yes, you are correct – thanks for spotting this. We had replaced an older publication stating 5% sealing by a newer publication stating 6%, and we had forgotten to adjust all numbers.

Remarkably, both publications were from the same group applying similar methods. They are only 10 years apart but still, soil sealing has relatively increased by 20%. This also illustrates the incredibly fast changes in land use that requires attention.

L261: I don't understand from the text how the 44mm come about.

We have modified the text to improve clarity:

Furthermore, sealed areas impede groundwater recharge. **Six** percent sealing reduces the overall mean groundwater recharge (206 m yr-1, Baumeister et al., 2017) by 12 mm $yr^{-1}$. Neighboring areas, if they compensate for the loss **of 32 mm $yr^{-1}$** in evapotranspiration (Blumröder et al., 2021; Herbst et al., 2007), will, in consequence, recharge **32 mm $yr^{-1}$** less groundwater. Ultimately, this may lead to a calculated 44 mm $yr^{-1}$ decrease in groundwater recharge if vegetated surfaces compensate for the entire loss of evaporation caused by sealed surfaces.

L407-409: This statement misses a reference.

The statement was explained in the following sentence. To address the comment, we now reversed the sentences and now write:

Rain cells or pressure systems move over a location within hours or days, while subsoil compaction can persist for years or even centuries, tile drainage can remain functional for many decades or longer, and soil sealing is rarely reversed. Hence, on a temporal scale, these influences of land use last considerably longer than weather phenomena.

L455: The authors didn't "illustrate" any relative importance in this paper, so the statement "at least equally important" should be clearly labelled as speculation.

We changed the sentence and also revisited the new title. It reads now (changes in bold):

However, exclusively focusing on this goal would ignore other important mitigation measures that urgently need to be realized. As illustrated here, restoring hydrologically functional landscapes and soils **should be considered** ~~at least~~ equally important to mitigate climate change, especially concerning extremes such as floods, droughts, and heatwaves **and to preserve the foundation of food and life**. **The question of whether land use or $CO_2$ are more significant drivers of floods and drought deserves more attention even though a simple answer will never be possible.**

L480: The caution the authors advise here when it comes to models should be exercised by the authors themselves when they use models to support their argument.

We did not use models ourselves but, indeed, we cited papers that used models. Most of them have appeared in respected journals and we can hardly pinpoint all deficits that may be in the models. Our argument was clearly different: The effects caused by $CO_2$-driven climate change call for a reappraisal of modeling approaches. We see nothing wrong in this statement.

**Reviewer 2**

The authors have made considerable changes with respect to the original version. While I appreciate their efforts, I do not in all cases agree with their response, given that on several issues the authors try to argue their way out rather than improving the argumentation. However for a HESS Opinions contribution, I think it should be acceptable that a certain level of disagreement will remain between authors and reviewers. In my view this is fine, and since I believe the underlying message in the manuscript is important, I am willing to overlook some of the more minor issues.

We thank the reviewer for taking the time and effort spent with our paper. We also appreciate the openness towards allowing us to present our "opinion" supported by an evidence-based chain of arguments.

In spite of having spent considerable time reading and thinking about this work, it only appeared to me last week what in the overall pattern in, and reason for, my comments on the previous version in fact is. A HESS Opinions contribution is, in my view, a discussion on a topic with the aim to raise awareness of an issue that is typically understudied/overlooked where a case is built by the authors on arguments. This is, by its very nature, not an objective process. My main problem is already reflected in the title of the contribution: "… are more significant drivers …". This wording does not reflect an opinion, but something that would normally be concluded based on a model study or controlled field experiment. These are the only possible ways to formally test whether "land use, soil management, and landscape hydrology are more significant drivers than increasing temperatures". This can never be "proven" based on arguments. So the title, and some of the main conclusions (for instance "is is at least equally important"), are problematic since they do not reflect that opiniated nature of the contribution. Luckily, this is an issue that is rather easy to solve. Overall, I believe the authors need to change the phrasing from essentially suggesting that we know everything there is to know about land use and management and its importance relative to that of climate change, to presenting the referenced studies as rare examples that suggest that land use and land management (and lateral interaction) are being overlooked and might in fact be much more important drivers of hydrological change as currently believed. From this, the authors' main opinion can be that those aspects should not be overlooked (which I would fully support), and that more research is urgently needed to provide a formal answer to the questions of what is their relative importance (a call for agenda setting). Possible new titles that would fit with this approach could be "HESS Opinion: Land use, soil management, and landscape hydrology should not be overlooked as main drivers of changes in floods and droughts" or "HESS Opinion: The need to consider land use, soil management, and landscape hydrology as main drivers of changes in floods and droughts". This opinion would be defendable based on the evidence discussed, and it would solve some of the issues that might be seen as conflicting or where I disagree with the interpretation of the authors (such as the discussion on hedges).

We changed the title by asking a question to accentuate that we do not give an answer, but encourage a discussion. Furthermore, "temperature" was replaced by $CO_2$ following the suggestion of Ref. #1.

HESS Opinion: Are changes in land use, soil management, and landscape hydrology more significant drivers of floods and drought than increasing $CO_2$?

In terms of textual changes needed to accommodate this suggestion, these could likely be minor. I leave it up to the authors to double-check the formulations in the main text. In the conclusions, it is important that phrases such as “is at least equally important” are removed or changed since these are not directly and formally supported by the evidence. Ideally, the conclusions would end with a clear recommendation or call for agenda setting.

We changed the sentence and also revisited the new title. It reads now (changes in bold):

However, exclusively focusing on this goal would ignore other important mitigation measures that urgently need to be realized. As illustrated here, restoring hydrologically functional landscapes and soils **should be considered** ~~at least~~ equally important to mitigate climate change, especially concerning extremes such as floods, droughts, and heatwaves **and to preserve the foundation of food and life**. **The question of whether land use or $CO_2$ are more significant drivers of floods and drought deserves more attention even though a simple answer will never be possible.**

We have also carefully checked the entire text again to avoid any parts that were misleading or not fully supported by our data. This also included explaining some steps in more detail (see response to referee #1).

I hope the authors see the value of these comments, and can make the changes that would, in my view, make this contribution to a strong and important HESS paper.