Dear Editor, dear Reviewers,

We would like to thank you for your time and input on our manuscript which we have now addressed in the point-by-point responses below. Please note that we did not agree with some of the suggestions, as explained in detail under the respective comments. We are still grateful for the opportunity to have this discussion, which shows that our opinion paper introducing a new perspective has the desired effect of inspiring a scientific discussion about the topic.

Our reply is printed in blue, while the comments by the reviewer are given in black.

Sincerely,

Karl Auerswald, Juergen Geist, John N. Quinton, Peter Fiener

RC1: 'Comment on egusphere-2024-1702', Anonymous Referee #1, 08 Jul 2024 reply

Auerswald et al. HESS

General comments:

The authors present a provocative piece that positions land and soil use processes at the landscape scale as important drivers of the climate change effects on floods and droughts. The manuscript reviews the relevant processes and makes a convincing argument. Only at the beginning is the piece set up in a way that suggests landscape factors to be more important than CO2-driven climate change effects. I'm not convinced by this framing, especially as the relevant evapotranspiration argument is not fully explained. I suggest positioning the two strands of impacts – via CO2 and via landscape factors – as complementary, and maybe the landscape processes have been overlooked. But the manuscript doesn't disentangle the two in my opinion. It would also be good to triangulate the arguments with timeseries data, specifically ET data from eddy-flux towers or lysimeters, soil sealing timeseries, soil compaction timeseries. I know especially the latter two are hard to come by, but maybe for the case study in Bavaria.

Unfortunately, we did not fully convince the reviewer that, at present, the CO_2-driven climate change is much weaker than the land-use-driven climate change because he had not found the long-term hydrological modeling in the supplement (see his second review submission). This hydrological modeling was set up by a working group of governmental hydrological authorities and the German Weather Service ("KLIWA"; www.kliwa.de) to detect climate change influences on hydrology. Now, we have moved the results of this modeling from the supplement, describing the natural boundary conditions in Bavaria, to the main part of the manuscript. This also avoids the frequent reference to a figure in the supplement that we had in our original submission.

Furthermore, we now include a reference to the latest analysis of return periods of heavy rain by the German Weather Service covering 1960 to 2020, which has just appeared. This analysis applied sophisticated statistical tools to detect a CO_2-driven climate change signal (Willems et al., 2023; Shehu et al., 2023). However, until 2020, the data did not clearly show such a signal, and stationarity still had to be assumed (Haberlandt et al., 2023). Thus, we conclude the section about meteorological changes by stating, “These minor changes in annual rainfall, seasonal rainfall, or event rainfall do not align with the severity of floods and droughts experienced”.

In contrast to the CO2-driven climate change hydrological signal, which is still lacking, the land-use-driven changes, such as sealing or drainage, are statistically without doubt. The only clear and pronounced CO_2-driven change, except for the rising temperatures, is the increase in rain erosivity, which was identified by some of the authors of this manuscript (Fiener et al., 2013; Auerswald et al., 2019). Rain erosivity, however, influences sediment detachment and transport. It may lead to surface puddling and crusting enhancing runoff, but this requires a bare soil surface and, therefore, relates to land use.

We do not include data on eddy covariance measurement for two reasons. (i) Long-term measurements that could detect a climate change signal do not exist. (ii) A climate change signal would say nothing about whether it is derived from CO_2 effects or land-use effects. In fact, eddy covariance measurements are strongly influenced by land-use effects due to the large fetch. We have therefore included references to eddy covariance results in the section where we describe the effects of horizontal energy advection in heterogeneous landscapes.

The request for eddy covariance data points to the central message of this manuscript. Changes in hydrological behaviour, such as those potentially identified by eddy covariance measurement, cannot prove CO_2-driven effects without meticulously considering all other effects as well. Premature assignments to CO_2 effects without considering other effects can be frequently found in hydrology. It is not enough to show that CO_2 increases over time and that some hydrological parameters also change over time. We need clear and quantitative cause-and-effect analyses in order not to overlook important drivers.

Auerswald, K., Fischer, F. K., Winterrath, T., and Brandhuber, R.: Rain erosivity map for Germany derived from contiguous radar rain data, Hydrol. Earth Syst. Sc., 23, 1819–1832, https://doi.org/10.5194/hess-23-1819-2019, 2019.

Fiener, P., Neuhaus, P., and Botschek, J.: Long-term trends in rainfall erosivity – analysis of high resolution precipitation time series (1937–2007) from Western Germany, Agr. Forest Meteorol., 171–172, 115–123, 2013.

Haberlandt, U., Shehu, B., Thiele, L., Willems, W., Stockel, H., Deutschländer, T., Junghänel, T., and Ostermöller, J.: Methodische Untersuchungen für eine Neufassung der regionalisierten Starkregenstatistik KOSTRA-DWD [Methodological investigations for updating the regionalised extreme rainfall statistics KOSTRA-DWD], Hydrologie & Wasserbewirtschaftung 67, 138-159, 2023, https://doi.org/10.5675/HyWa_2023.3_1, 2023.

Shehu, B., Willems W., Stockel H., Thiele L.B., and Haberlandt U.; Regionalisation of rainfall depth–duration–frequency curves with different data types in Germany, Hydrol. Earth Syst. Sci., 27, 1109-1132, 2023.

Willems, W., Stockel, H., Haberlandt, U., Shehu, B., Junghänel, T., Ostermöller, J., and Deutschländer T.: Betrachtungen zur Instationarität extremer Niederschläge in Deutschland [Reflections on the instationarity of extreme rainfalls in Germany], Hydrologie & Wasserbewirtschaftung, 67, 151-159, 2023.

Specific comments:

L23: The "imbalance" suggested here needs more explanation.

We replaced imbalance with alteration.

L49-61: In this section, land use and soil use seem to be conflated. I suggest to be clear about the focus.

The paragraph, like the entire manuscript, covers both: changes in land use, such as converting cropland to urban land, and soil use, like tilling straw in or leaving it on the surface. We think that our wording is correct.

L127-129: The argument for a small effect of rising T on ET is crucial and hence needs more explanation. Maybe unpack the Penman-Monteith equation to pinpoint all the influences of T on ET and then convincingly show how these may be smaller than conventional wisdom holds.

We accept the reviewer's suggestion and include a chapter in the supplement on the Penman equation of evapotranspiration to explain why an increase in vapor pressure deficit caused by a temperature increase results in roughly less than half as strong an increase in evapotranspiration. This is, however, a very simplistic, stationary view that neglects feedback mechanisms on a global scale (e.g., decreasing wind with increasing polar temperatures). This small effect is unequivocal in the climate-change community and is the outcome of all climate projections. We had already cited three publications but added another.

Lambert, F.H., and Webb, M.J.: Dependency of global mean precipitation on surface temperature, Geophys. Res. Letters, 35, L16706, doi:10.1029/2008GL034838, 2008.

Figure 3 also misses the ET argument.

The reviewer is correct. Integrating ET into the graph is difficult because other arrows would be required. Hence, we changed the caption of the figure.

If we draw an analogous graph for ET, the effect of land use would be twofold (increasing temperature and decreasing air humidity due to unvegetated surfaces or drained, formerly wet spots). In contrast, the CO2 effect only increases the temperature.

L142-143: How to disentangle CO2-driven climate change from land use-driven climate change?

This is difficult, and it is work in this area is what we want to stimulate. As long as we are unable to disentangle both drivers, it is inappropriate to assign effects to one of both causes. This is, however, frequently done with potentially misleading results.

In some cases, disentangling is relatively easy. For example, the influence of land use on heavy rain should be minimal (but for this parameter, we still do not see significant effects; see above), while land use and CO_2-driven climate change will influence runoff and flooding. In the public discussion, flooding is presently almost exclusively associated with CO-driven climate change despite the lack of measurable effects on heavy rain. A greater scientific effort to disentangle both drivers is urgently needed.

We did not make a change here because the question raised by the reviewer requires new work and it is a question that our manuscript raises.

L150-151: What about open water evaporation from ponding on sealed surfaces?

We modified the text. It now reads: "Except for the small amount of water left on sealed surfaces after a rain, sealed surfaces do not contribute to evaporation but partition their radiant energy uptake almost exclusively into sensible heat."

Some of the reference in section 3.1 are also quite old. Do recent studies confirm these findings?

We deleted Oke (1989), which we had put in to honor the pioneering work of Oke, but we keep Oke (1982), Calder (1949), Drivas and Shair (1974), and McNaughton (1976, 1978). There are no newer studies that would invalidate these old studies and justify removing them. We have clarified why we have cited these older references.

More importantly, these old studies show that we have known about the effects of energy advection in great detail for many decades, but mostly do not include them in hydrological modeling or consider them in landscape planning. We h ad also cited newer references from 1995, 1996, 2000, 2007, 2018, 2021. We have now expanded this section to include more recent publications. We have also added references to the vast literature on eddy covariance measurement, for which the lateral effects of advection are central (called fetch in this context).

Baldocchi, D.D. and Rao, K.S.: Intra-field variability of scalar flux densities across a transition between a desert and an irrigated potato field, Boundary-Layer Meteorol., 76, 109–136, 1995.

Grossiord, C., Buckley, T.N., Cernusak, L.A., Novick, K.A., Poulter, B., Siegwolf, R.T.W., Sperry, J.S. and McDowell, N.G.: Plant responses to rising vapor pressure deficit, New Phytol, 226, 1550-1566, https://doi.org/10.1111/nph.16485, 2020.

Klaassen, W., Van Breugel, P.B., Moors, E.J. and Nieveen, J.P.: Increased heat fluxes near a forest edge, Theor. Appl. Climatol, 72, 231–243, 2002.

Leclerc, M.Y. and Thurtell, G.W.: Footprint prediction of scalar fluxes using a Markovian analysis, Boundary-Layer Meteorol., 52, 247–258, 1990.

Panin, G., Tetzlaff, G. and Raabe, A.: Inhomogeneity of the land surface and problems in the parameterization of surface fluxes in natural conditions, Theor. Appl. Climatol., 60, 163–178, https://doi.org/10.1007/s007040050041, 1998.

Raatz, L., Bacchi, N., Pirhofer Walzl, K., Glemnitz M., Müller M.E.H., Joshi, J., and Scherber, C.: How much do we really lose?—Yield losses in the proximity of natural landscape elements in agricultural landscapes, Ecology Evolution, 9, 7838–7848, https://doi.org/10.1002/ece3.5370, 2019.

Savage, M.J., McInnes, K.J., and Heilman, J.L.: The 'footprints' of eddy correlation sensible heat flux density, and other micrometeorological measurements. South African J. Sci., 92, 137–142, 1996.

And how do the timelines of CO2 in the atmosphere and soil sealing compare? Does a comparison support the argument of a soil sealing-driven climate change?

This comment has three independent aspects. (i) Both, CO_2 and soil sealing continuously increase. Hence, they correlate closely as many other changes over time do. (ii) In this specific case, there is not only a correlation but also a causal relation because more sealing means more traffic, more cement production and, in turn, more CO_2 release. (iii) In this manuscript, we do not focus on the CO_2 release by sealing but on the climate effect of sealing caused by the modified allocation of radiation energy to heat and evaporation.

No change was made to the manuscript.

L244-248: What about outflow from the river to the groundwater?

Historically Bavaria had gaining rivers that received their water from the groundwater. However, due to the widespread lowering of groundwater, loosing rivers have been created. Consequently, many small rivers have disappeared while others have become perched due to colmation (clogging) of the river bed.

We added "…and caused many small rivers to disappear"

L264-266: How does soil compaction look over time?

This is an open question because no consistent monitoring over decadal timespans exists. However, in a recent German-wide survey in an 8×8 km grid analyzing 16778 soil samples, 51% of the arable land had root restrictions caused by compaction (Schneider and Don, 2019). This value is similar to the 43% of over-compacted soils found in the Netherlands (Brus and van den Akker, 2018).

We now include reference to the work of Schneider and Don (2019) and Brus and van den Akker (2018) in the revised version of the manuscript.

Brus, D. J. and van den Akker J. J. H.: How serious a problem is subsoil compaction in the Netherlands? A survey based on probability sampling, Soil, 4, 37–45, https://doi.org/10.5194/soil-4-37-2018, 2018.

Schneider, F. and Don, A.: Root-restricting layers in German agricultural soils. Part I: extent and cause, Plant Soil, 442, 433-451 (2019), doi.org/10.1007/s11104-019-04185-9

L342: What is meant by "unfavourable behaviour" here?

We added "such as increasing complexity, instability and exponential growth or oscillations"

RC2: 'Reply on RC1', Anonymous Referee #1, 08 Jul 2024 reply

> I just realised that some of the timeseries I'm asking for are in the supplement. Please consider my comments in this light. And maybe move some of the analysis to the main text.

We have moved some of the analysis to the main manuscript (see response above).

Reply to **RC3**, Adriaan J. (Ryan) Teuling, 26 Aug 2024

This contribution aims to argue that factors such as land use and soil management are more important than increasing temperatures when it comes to their impact on floods and droughts. This is an interesting and perhaps controversial viewpoint that can help to stimulate the debate on the origins of hydrological extremes.

We appreciate this comment.

While I can appreciate what the authors are trying to achieve, and I second many of the points made, the contribution currently contains numerous strong claims that are insufficiently rooted in either scientific evidence or logic. The contribution also suffers from a lack of clear definitions of what a flood or drought is according to the authors, and at what scale processes are relevant. As a result, their argumentation suffers. As an example: if flooding is defined as the length of (over)saturation of soils at a given location (which is perhaps not totally unreasonable), than artificial drainage will locally lead to a reduction of flooding, and not by definition to an increase downstream. This is in fact the main reason that much of Europe has seen the introduction of intense drainage. The contribution has similar weak argumentation in other places, which in my view leads to a situation in which the main proposition (Land use, soil management, and landscape hydrology are more significant drivers than increasing temperatures) is not sufficiently backed up by evidence or strong arguments. Therefor I believe the contribution should either be toned down (Focusing on the question are Land use, soil management, and landscape hydrology are more significant drivers than increasing temperatures?), or stronger arguments should be provided to back up the claims.

Flood is not an oversaturation of soil but, according to Merriam-Webster, an inundation or overflowing of normally dry land. Please note that the first sentence of our introduction reads "*Reports of severe storms, catastrophic floods like the Simbach event (Brandhuber et al., 2017; Mayr et al., 2020), and tragic events such as the Ahrtal floods, which caused over 150 casualties (Mohr et al., 2023), are increasingly common.*" From this introduction, it should be clear to the reader that we are not equating flooding with high soil moisture.

To prevent any potential misunderstanding among different hydrological disciplines, we now clarify that 'flood' in our manuscript covers fluvial foods and flashfloods. We do not define these terms, but we expect that they will be well-known to the readership of HESS. In the introduction, we now write (insertions in bold):

"*In this paper we demonstrate and compare the CO_2-driven and land-use-driven climate change on floods and droughts.* ***Floods encompass flashfloods and fluvial floods.*** *We will…*"

The comment regarding the scales at which the processes are relevant highlights an important aspect that we had not sufficiently addressed. In response, we have added a brief section demonstrating that land use impacts often occur at small spatial scales (e.g., a wheel track), but usually encompass entire fields. Given that cropland, grassland, and forestland in developed countries are typically managed using similarly heavy machinery, these effects are likely to manifest across all spatial scales, potentially exceeding even the scale of rain cells or atmospheric pressure systems. However, current research tends to focus on small-scale effects, such as those observed in plot-level studies. Through this opinion paper, we aim to encourage research that investigates land use effects on larger spatial scales.

On the temporal scale, we demonstrate that the rate of change in land use is much faster than in atmospheric CO_2 concentration, with machinery weight, for example, increasing by a factor of 20 faster than CO_2 concentration. It is crucial that research efforts concentrate more on land use effects to avoid overlooking potentially significant impacts. Current research predominantly addresses short-term changes (e.g., through 3-year projects), while long-term, country-wide monitoring, such as that available for CO_2 concentration, is lacking for many land use-induced changes, including the evolution of soil compaction, the change of soil structure, or the expansion of drainage systems.

Detailed comments:

Line 24 and onward: Here, the authors attack the use of common statements by arguing they are solely based on correlations and not causality. This attack fails to convince. It is general knowledge that the evaporation process (the phase transitioning from liquid to gaseous phase) depends on temperature and vapor pressure deficit. This is not an example of circular reasoning. Atmospheric and soil (moisture) states are both part of a complex (and open) feedback system, and many statements on direction of feedbacks implicitly assume timescales associated with these processes. The argument on recycling, which I find weak, is a good example of mixing of scales by the authors. Soils can be dry because it hasn't rained locally, but indeed this drying might contribute to rainfall thousands of kms away. Not sure what the authors exactly try to prove here.

In Miralles et al. (2019), the reviewer himself wrote: "*as … land evaporation is reduced, hence the air becomes even drier, which may further decrease the likelihood of rainfall and favor the occurrence of meteorological droughts*". The only difference is that Miralles et al. (2019) focus on the meteorological origins of drought while we expand the view and point to the fact that land use can also lead to restricted land evaporation, e.g., by creating impermeable surfaces.

Line 70 and onward: Here the authors argue that because summer rainfall does not show a clear trend, rainfall cannot explain any increases in floods. This is a false comparison. A fair comparison would be to analyse event precipitation that induced flooding. It is well possible, and in line with the arguments provided on erosivity, that extreme precipitation is showing a clear increase whereas summer rainfall in itself does not. Please provide a fair comparison.

Please note that we used the sum of summer rainfall (June, July, August) to show that a change in precipitation sum in this period does not explain increasing drought. In the supplement, we compiled the results of a long-term (1951 to 2015) hydrological modeling set up by a working group of governmental hydrological authorities and the German Weather Service ("KLIWA"; www.kliwa.de) to detect climate change influences on hydrology. This modeling, based on daily meteorological data, shows no trend in direct runoff (and evapotranspiration). We have now moved the results of this modeling from the supplement, describing the natural boundary conditions in Bavaria, to the main part of the manuscript (now Fig. 1). This also avoids the frequent reference to a figure in the supplement that we had in our original submission.

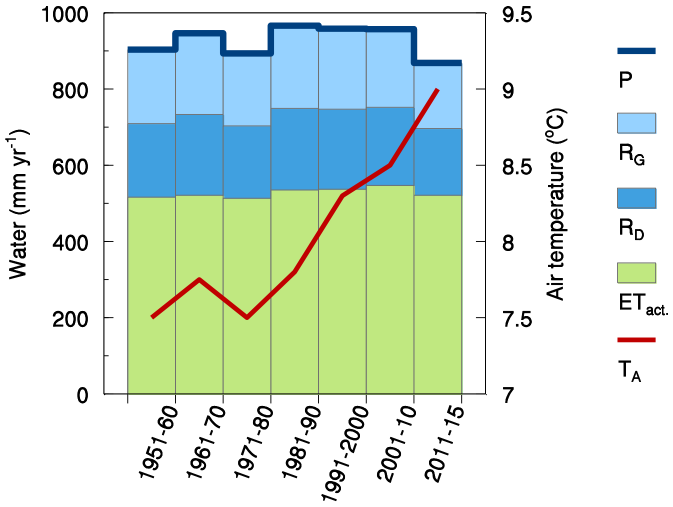

Figure 1: Change in air temperature (T_A), falling precipitation (P), actual evapotranspiration (ET_{act}), direct runoff (R_D), and groundwater recharge (R_G) during the past seven decades and averaged over entire Bavaria; the data were compiled from Baumeister et al. (2017) who had applied a hydrological model (GWN-BW) particularly designed to calculate groundwater recharge. Like almost all hydrological models, the model considers only coarse land-use classes and not accounting for the changes that occurred within the land uses. Furthermore, it does not consider lateral interactions between land uses (e.g., oasis and clothesline effects on evapotranspiration, run-on infiltration, and interflow). Hence, the model results almost entirely depict meteorological changes in space and time.

Furthermore, we now include a reference to the latest analysis of return periods of heavy rain by the German Weather Service covering 1965 to 2020, which has just appeared. This analysis applied sophisticated statistical tools to detect a CO_2-driven climate change signal (Willems, et al., 2023; Shehu, et al., 2023). They analyzed rainfall depth–duration–frequency curves for 12 rain durations comprising 5, 10, 15, and 30 minutes, 1, 2, 6, and 12 hours, 1, 1.5, 3, and 7 days for the years 1965 to 2020. No trend was found for all time durations and stationarity had to be assumed (Haberlandt, et al., 2023). Thus, we conclude the section about meteorological changes by stating, "These minor changes in annual rainfall, seasonal rainfall, or event rainfall do not align with the severity of floods and droughts experienced".

In contrast to the CO_2-driven climate change signal on precipitation, which is still not significant, the land-use-driven changes like sealing or drainage on hydrology are statistically without doubt. The only clear and pronounced CO_2-driven change, except for the rising temperatures, is the increase in rain erosivity, which was identified by some of the authors of this manuscript (Fiener et al., 2013; Auerswald et al., 2019). Rain erosivity, however, influences sediment detachment and transport. It may lead to surface puddling and crusting enhancing runoff, but this requires a bare soil surface and, therefore, relates to land use.

Auerswald, K., Fischer, F. K., Winterrath, T., and Brandhuber, R.: Rain erosivity map for Germany derived from contiguous radar rain data, Hydrol. Earth Syst. Sc., 23, 1819–1832, https://doi.org/10.5194/hess-23-1819-2019, 2019.

Fiener, P., Neuhaus, P., and Botschek, J.: Long-term trends in rainfall erosivity – analysis of high resolution precipitation time series (1937–2007) from Western Germany, Agr. Forest Meteorol., 171–172, 115–123, 2013.

Haberlandt, U., Shehu, B., Thiele, L., Willems, W., Stockel, H., Deutschländer, T., Junghänel, T., and Ostermöller, J.: Methodische Untersuchungen für eine Neufassung der regionalisierten Starkregenstatistik KOSTRA-DWD [Methodological investigations for updating the regionalised extreme rainfall statistics KOSTRA-DWD], Hydrologie & Wasserbewirtschaftung 67, 138-159, 2023, https://doi.org/10.5675/HyWa_2023.3_1, 2023.

Shehu, B., Willems W., Stockel H., Thiele L.B., and Haberlandt U.: Regionalisation of rainfall depth–duration–frequency curves with different data types in Germany, Hydrol. Earth Syst. Sci., 27, 1109-1132, https://doi.org/10.5194/hess-27-1109-2023, 2023.

Willems, W., Stockel, H., Haberlandt, U., Shehu, B., Junghänel, T., Ostermöller, J., and Deutschländer T.: Betrachtungen zur Instationarität extremer Niederschläge in Deutschland [Reflections on the instationarity of extreme rainfalls in Germany], Hydrologie & Wasserbewirtschaftung, 67, 151-159, 2023.

The authors use the study of Davin et al. (2014) to argue that land management would have contributed to a shorter, less intense drought. There are several problems with this paragraph. First, the authors claim that the change of albedo would have lowed temperature country-wide by 2 K. This claim is not supported by the work of Davin et al. Yes, during some individual days during the 2003 heatwave, their simulations showed a reduction in temperature maxima of close to 2K, but only averaged over pixels with more than 60% cropland (their Fig 4). These are likely not the pixels where the high death toll mentioned by the authors took place, and the effect of cropland management on these would likely have been very small. The authors also argue that this 2K difference during the warmest days would have lead to a reduction in soil drought – seemingly in contradiction with a later statement that 2K warming does not significantly affect evaporation. The authors also seem to have done some selective shopping for arguments: in Davin et al. it is also shown that for much of France (NE, see their Fig 3) the warming impact of NOTILL for lower temperature quartiles is nearly the same as the cooling impact for higher quartiles. Please study this reference (and other references) again to make sure statements are in line with evidence provided in these works.

We had written:

"*For France, it was estimated that, during the centennial European heat wave in August 2003, if the farmers had left the straw from grain harvest on the soil rather than tilling it in, the change of albedo would have lowered temperature country-wide by 2 K (Davin et al. 2014).*"

Davin et al. (2014) had written:

"*During the peak of the heat wave in August, the daily maximum temperature was 9.9 °C above the 1986–2009 climatology according to a gridded observational dataset for temperature (20) (taking an average over 10 d between the fifth and 14th of August). This figure is well reproduced by the model, with an anomaly of 10.2 °C in the CTL simulation. In simulation NOTILL, the anomaly is only of 8.4 °C owing to the effect of no-till management, which represents a 2 °C reduction of the heatwave anomaly over this 10-d period. Fig. 4 also* *shows that the albedo increase is the dominant factor, the evaporation effect having a relatively minor role during this specific event."*

The intention of Fig. 4 in Davin et al. (2014) is not to support this 2 °C statement, but to differentiate between the effects of albedo and soil evaporation exerted by a straw cover. Davin et al. (2014) had indeed restricted their data set for this particular task because the evaporation effect is very small, but not as much as the reviewer suggests. The caption of Fig. 4 says," *... averaged over France (44 - 50° N; -5 - 5°E), considering only grid cells with more than 60% of cropland*." The figure shows, that the albedo effect is larger (!) than the total effect because of a small antagonistic effect of soil evaporation.

Also, the argument that there are regional differences is trivial. Yes, there are regions with a lower temperature decrease (especially along the north coast) and others that have a higher effect (especially in the northwest) to yield an average of 2 °C.

We do not see a significant discrepancy between our sentence and the detailed statement in Davin et al. (2014), but we add "on average" to our sentence to remind the reader that France is not homogeneous.

Neither the reviewer nor we know where the death toll occurred, and we did not write about this. It is evident, however, that settlements were also within the study area of Davin et al. (2014).

Line 117 and onwards: Here the authors argue that "As the droughted area heats up more than neighboring well-watered areas, the higher temperature spreads to these nearby areas and causes them to transpire more until they also run short of water. Thus, the area with reduced evapotranspiration grows and may finally spread over an entire continent, described as "event self-propagation" by Miralles et al. (2019)." Several things are wrong here. First of all, this self-propagation is never the only process operating. Please also elaborate on the role of atmospheric dynamics. And this statement seems to conflict with the arguments on recycling provided earlier. The more evaporation and drying of soils in one place, means more precipitation in other places. Again please be clear in the argumentation about spatial of temporal scales at which processes occur.

We describe what is shown in Fig. 1 by Miralles et al. (2019). It does not include atmospheric dynamics but describes, as stated by the caption, "Land feedbacks as local intensifiers of hydro-meteorological extremes," which is relevant to our topic. Of course, we agree that persistent large-scale circulation anomalies are critical for the initiation of drought and heatwaves (Miralles et al., 2019). However, how long "persistent" must be to initiate such events will depend on the moisture storage on the ground and whether it is sufficient to bridge the anomaly or not.

For clarification, we added to the text: "*Even though persistent large-scale circulation anomalies are critical for the initiation of drought and heatwaves (Miralles et al., 2019), the length of such anomalies required to initiate droughts or heatwaves will depend on the moisture storage on the ground and whether it is sufficient to bridge the anomaly or not.*"

Line 130 and onwards: Here, the authors claim based on the work by Roderick et al. (2014) that a 2 K temperature rise would increase evapotranspiration by only 5 %. This is misleading. In my understanding of the Roderick study, this is based on long-term global average values including feedbacks with precipitation. For a smaller region like Bavaria and the focus on individual droughts, the sensitivity is likely much higher. I did a quick analysis based on data for De Bilt, where potential ET (by the Makkink method) has been measured since 1957. Comparing August values for 1957-1966 and 2014-2023 results in 82.9 mm and 106.6 mm, respectively, or an increase of over 28%. The same periods show a warming of 2.67 K, resulting in a sensitivity of nearly 11 %/K, much much higher than the 2.5 %/K mentioned by the authors. This analysis took me only 5 min. I expect similar results will be found for Bavaria. Please investigate a bit further than simply picking a global number not relevant to regional drought from a 10-year old study.

Please note that we did not only cite Roderick et al. (2014) but Bürger et al. (2014) and Skliris et al. (2016) as well. This value reflects a consensus, and it does not include any precipitation feedback on evaporation. It may be that the reviewer has the impression that there would be a feedback with precipitation, because on a global scale, precipitation must equal evaporation.

The smaller than CC rate is a result of the fact that evaporation requires energy; and net irradiance does not change due to the greenhouse effect (at least as long as the effect of clouds are not well considered in climate projections). We have added a short description to the supplement, why an increase of about 2.5 %/K results from the Penman model. The FAO Penman-Monteith method is long established and is viewed as the gold standard in evaporation modelling.

The reviewer wrote "*Please investigate a bit further than simply picking a global number not relevant to regional drought from a 10-year old study*." Roderick et al. was cited 199 times since 2014 and is much younger than the Makkink (1957) method. Furthermore, Roderick et al. (2014) do not focus on the global scale but they identify the drivers of local-scale changes. Remarkably, they conclude "*Much public and scientific perception about changes in the water cycle has been based on the notion that temperature enhances evaporation. That notion is partly true but has proved an unfortunate starting point because it has led to misleading conclusions about the impacts of climate change on the water cycle*."

We tried to find a publication of De Bilt on evaporation or evapotranspiration, but Scopus returned no result when using these keywords. Please note that in the supplement, we also provide the results from an official hydrological modeling study for the years 1951 to 2015 (about the same time range as De Bilt) covering entire Bavaria. It does not show any increase in evaporation despite a 1.5 °C increase in temperature because an increase in evapotranspiration of about 4% is too small to become visible. We have moved this figure from the supplement to the main part (Fig. 1 now; see comment above) because pointing to the supplement seems to attract insufficient attention.

Line 228: "In consequence, the remaining precipitation …" -> What is remaining precipitation? Relative to what? In natural systems there will also be a considerable amount of drainage, perhaps even equal to that of drained lands (where storage on average is lower, but this does not necessarily have a strong impact on average partitioning).

We do not give a relative number.

The full sentence reads: "*According to Tetzlaff et al. (2010), 23% of the agricultural land in Germany has been artificially drained, and drainage runoff in the southern part of Bavaria can be up to 500 mm yr^{-1} (Wolters et al., 2023). In consequence, the remaining precipitation on the drained land in the landscapes with the highest rainfall is as low as the precipitation in the landscapes with the lowest rainfall*."

What we are saying is that in a landscape with 1100 mm yr^{-1} precipitation, where 500 mm yr^{-1} is lost via artificial drainage, the difference is 600 mm yr^{-1}. This is the remaining amount of water available for all other hydrological processes, and this amount is equal to the total precipitation in the driest parts of Bavaria.

We rephrased the sentence to make it clearer. It now reads:

"In consequence, after subtracting the water lost by artificial drainage the remaining precipitation in the south, which has the highest rainfall in Bavaria, is as low as the total precipitation in northern Bavaria where the rainfall is lowest."

Section 3.4: This is a good example of selective argumentation. When discussing the study of Davin, the authors argued that a small reduction in warming would help to counteract drought. Using the same reasoning, hedgerows, which as the authors correctly point out have a warming impact on their environment (besides many other advantages!) should lead to an increase in drought because of the higher temperatures. I personally don't think this effect is very strong (and if so it is likely beneficial), but for this contribution it is important that arguments are consistent.

No; the reviewer is not correct. Based on Davin et al. (2014) we argued that a straw cover can mitigate a heatwave.

The increase in temperature due to hedgerows is an effect of reduced evapotranspiration due to less wind (allocating more energy to sensible heat at the expense of latent heat). This is simply a result of eqn 2. It cannot be argued that the increase in temperature will increase evaporation because a hedgerow does not provide any additional energy that could increase sensible heat and latent heat at the same time.