Reviewer 1:

The manuscript "Climate change impacts on regional fire weather in heterogeneous landscapes of Central Europe" by Miller et al. presents a study on climate change impacts on the fire danger index FWI in hydrological Bavaria in central Europe, using a single model initial-condition large ensemble (SMILE) data set and the RCP 8.5 emission scenario. Changes in FWI are evaluated in terms of fire danger levels over the whole region, as well as on a sub-regional scale. The study provides new and interesting knowledge of projected changes in FWI in the studied region, and the topic is suitable for the journal. Strengths of the manuscript include the multiple approaches applied to investigate the hypotheses, and the clearly communicating figures. The manuscript has potential but is not at the required level for a scientific paper in its current state. Parts of the analyses are wrong, methods are not clearly presented, the discussion is not clearly presented, and references do not always reflect work supporting the claims. I would recommend all authors to carefully revise the manuscript and correct and clarify where necessary. General and specific comments are provided below.

Thank you very much for taking the time to write this very detailed and constructive review, which we highly appreciate. We are glad that you value our work and are happy to take your comprehensive feedback into account. We think your comments shape the manuscript in a very positive way. We revised the paper carefully according to your feedback. We clarified unclear descriptions in the methods section, revised and updated references, and edited and extended the discussion. In addition, we carefully checked the manuscript for clarity.

General comments

1. The method to derive the return period is wrong. Thus, the analysis using return periods must be omitted or corrected and clarified. The applied temporal resolution is not stated, but the method is wrong regardless of the applied resolution. If all daily (or monthly) data in each year is used to extract the 99th percentile value, this value represent the 100 days (or 100 months) return period and not 100 years as stated in the manuscript. See e. g. Camuffo et al. (2020): https://doi.org/10.1007/s11600-020-00452-x. If instead the maximum value in each year was extracted, the period (30 years, i.e. 30 values) is too short to extract the 99$^{th.}$

   Thank you for highlighting the need to clarify the method used for return period calculation. We used a Single Model Initial-Condition Large Ensemble, where each of the 50 realizations represents an equally likely climate within the used climate model. Therefore, we pool over the entire 50-member ensemble to derive the percentiles from the resulting 274 500 data points (183 days per fire season x 30 years of climate period x 50 ensembles) for the present climate period from the data's empirical distribution. The future time periods (stepwise 30-year windows) were pooled over each member separately, resulting in (183 days x 30 years) 5490 data points from which the percentiles were derived from using the empirical distribution function of the ensemble member. Therefore, our sample size is not too short to extract the 99th percentile.

   We added a sentence to the description in chapter 2.5.3 to describe this more accurately: *"We pool over the entire 50-member ensemble using daily FWI values (183 days per fire season x 30 year climate period x 50 members). From this data pool we create an empirical distribution function of which we derive the percentiles representing the 10-, 20-, 50- and 100-year FWI return levels for the present climate period (1980-2009)".*

   We modified the sentence "*from 2010 to 2099, we create centered 30-year windows of our data sample and determine the FWI percentiles corresponding to the different return periods of the* *present climate period"* to "*from 2010 to 2099, we create centered 30-year windows for each member to determine the empirical distribution function and the FWI percentiles corresponding to the different return periods of the present climate period*".

We edited the remark on how we map the probabilities of the return periods:

Old: *We then compute the non-exceedance probability of the present percentiles given the future cumulative distribution. From the future non-exceedance probability, we estimate the future return periods using the function T = 1/(1 − p) (1) where T is the return period and p is the non-exceedance probability (Brunner et al., 2021; Coles, 2001)*

New: *We map the non-exceedance probability of the present percentiles given the empirically derived cumulative distribution of each member. From the non-exceedance probability, we estimate the return periods using the function* $T = \mu/(1 - p)$ *where T is the return period, μ is the inter-arrival time (1/183 days in a fire season) and p is the non-exceedance probability (Coles, 2001). We derive p from the rank r with* $p = r/n$ *where n is the total sample size by using the rv_histogram.cdf function of the Scipy package in Python (Virtanen et al., 2020).*

Lastly, we added a remark that we cropped the dataset to the time period where full 30-year windows are available (see RC-2 comment on L208).

New: *Due to the centered window approach, the first full 30-year window is 1995 and the last full 30-year window is 2084. Therefore, we crop the resulting time series to 1995 to 2084.*

2. Several references do not represent the original work reflecting your statements. Examples are line 46, 126, 157, 181 and 212. Please make sure the original references are used throughout, or in cases where this is not possible, add "e.g." before the references to avoid the reader to believe the reference is the original work.

   Thank you for pointing out the need to focus on original references. We adjusted:

   - Line 46: added "e. g. " to Bakke et al. (2023)
   - Line 126: added "i. e." to Di Giuseppe et al. (2016) and Touma et al. (2021)
   - Line 157: We modified the reference of Vitolo et al. (2019) to a direct quote:
     - Old: (Vitolo et al. 2009)
     - New: as suggested by Vitolo et al. (2009)
   - Line 181: We added Pfeifer et al. (2015) to the reference Böhnisch et al. (2021), because Böhnisch et al. (2021) adjusted the approach from Pfeifer et al. (2015).

     New: *... using the approach of Böhnisch et al. (2021) after Pfeifer et al. (2015).*

   - Line 212: We dropped Brunner et al. (2021).

   We further adjusted all sentences, which were also highlighted in your special comments and which we found during the review process. For those changes, please see the corresponding replies further below and check the new manuscript.

3. The discussion comprises multiple detailed comments on different aspects of the analysis or results, with too general subtitles. Introduced topics (e. g., uncertainties related to the chosen climate model on line 302) and summaries of results are not always followed up. Overall, the current state of the discussion chapter makes it hard for the reader to know what to expect and to follow the line of arguments of the authors. Please revise and clarify the discussion chapter,

avoid mentioning topics without commenting on them in relation to your study, and lift part of the discussion to a more general level.

Thank you for paying attention to the consistency of our discussion section. We agree that certain subtitles do not match the provided discussion points. We revised the discussion section carefully, changed subtitles, critically reflected on the chosen climate model setup and dropped paragraphs which were unrelated to the results. Further RC-3 commented, that uncertainties from the chosen climate model (structural uncertainty) and bias correction should be discussed, which we added to the discussion (see responses to RC-3). We renamed "Data Basis" to 'Uncertainties' in the discussion section. Changes in the other sections of the discussion referring to specific paragraphs of the manuscript are revised in the corresponding comments (RC-1 comments 66. to 86. and RC-3 Discussion)

Old subtitles:
4.1 Data Basis
4.2 FWI and Fire Danger Levels
4.3 Spatio-Temporal Trends and Variability
4.4 Societal and Ecologic Impacts

New subtitles:
4.1. Uncertainties
4.2 FWI and Fire Danger Levels
4.3 Spatio-Temporal Trends and Variability
4.4 Regional Shifts and Implications

4. The text is in several places informal with the use of unnecessary introductions (e. g. "another aspect which has to be discussed" on line 283 or "needs to be critically reflected upon" on line 318) or subjective words, inconsistent used of concepts, and imprecise descriptions of methods and results. Further, most of the manuscript is written in present tense. Papers are usually written in past tense when presenting analysis and results in abstract, data and methods, results and conclusions. Thus, I would recommend changing to past tense. In general, please carefully revise the whole manuscript and clean and clarify the text.

   Thank you for raising stylistic concerns regarding the writing style of the paper. We eliminated subjective words to our best knowledge and iterated over terminologies to ensure consistency. Further, we tried to avoid filling words as much as possible but kept them in places where they are necessary for a fluent reading flow. We changed to past tense in the introduction and data and methods sections. However, we kept the results section and parts of the discussion section in present because our results are here presented for the first time.

5. The manuscript refers to similar analysis in France and UK, but does not refer to results over the same region (HydBav) by others, e.g. from global or regional studies that cover the region. Please include other studies that cover your region (for example https://doi.org/10.1029/2018GL080959 and https://doi.org/10.1007/s10584-016-1661-x). In addition, please comment on potential differences between France, UK and your region, when you refer to results over these regions.

   Thank you for highlighting these global and regional studies. We incorporated comparisons from regional studies (de Rigo et al. 2017 and Carnicer et al. 2022) over Europe to better reflect on the spatial differences between regions in Central Europe and how they differ from southern Europe, e. g. the Mediterranean.

Specific comments:

1. You state in the title that your study area represents "heterogeneous landscapes". However, in line 110-111 you "assume that the water availability, climatology and landscape of different river systems reflect the fire regime of an area". A more in-depth reflection on the heterogeneity of the regions, and even subregions of your study, in relation to your spatial aggregations and findings would be beneficial to better reflect the title of your study.

   Thank you for highlighting the need to clarify that with "heterogenous landscapes" we refer to heterogeneity across subregions and not within regions. A more detailed reflection of the differences among the subregions is given by the paragraph starting in Line 129 (of the updated manuscript) and underlined by Figure 1.

   Old: *Since fire is closely related to the availability, or rather the absence of water, we assume that the water availability, climatology and landscape of different river systems reflect the fire regime of an area.*

   New: *Since fire is closely related to the availability, or rather the absence of water (in terms of precipitation or soil moisture deficit), we assume that the water availability, climatology, and landscape characteristics of the four different complex landscapes selected in our study are reflected in the subregions specific fire regimes.*

2. «Central Europe» is a concept that refers to a considerably larger region than the study area "Hydrological Bavaria". Please clarify the region to avoid exaggerating your study domain, in particular in the Abstract (e.g., line 7 changing "over Central Europe" to "in a region in central Europe" or similar), the conclusion (e.g. line 395 changing "for Central Europe" to "in central Europe" or similar), and the title ("of Central Europe" to "in central Europe"). Specifically, change to lower case 'c' ("central Europe") throughout.

   Thank you for pointing out the need to be more specific about the region in central Europe we are focusing on. We revised the manuscript according to your suggestion to "a region in central Europe". We agree to write central Europe with small c in central, because our study domain focuses primarily on the mid-latitudes of Europe, rather than the politically defined Central Europe (countries of Germany, Austria, Poland, Czech Republic, Slovakia, Switzerland, Hungary, Slovenia).

   We revised the term in:

   - Line 7 from "*over Central Europe*" to "*over a region in central Europe*"
   - Line 17ff. from "*Our results highlight central Europe's potential for severe fire events from a meteorological perspective and the need for fire management in the near future even in temperate regions*" to "*Our results highlight the potential for severe fire events in multiple regions of central Europe from a meteorological perspective and demonstrate the need for fire management in the near future even in temperate regions*."

3. Titles: please use NHESS house rules (sentence-style capitalization: https://www.natural-hazards-and-earth-system-sciences.net/submission.html#manuscriptcomposition)

We checked the titles and subtitles and adjusted according to the NHESS house rule "titles and headings follow sentence-style capitalization (i.e. first word and proper nouns only). This applies to table and figure headings as well."

We adjusted heading *Increasing Frequency of Extreme Events* to *Increasing frequency of extreme events.*

4. Line 27 and line 396-397: You state that Central Europe has not been exposed to wildfires before recent years (line 27) or to date (line 396-397). However, central Europe has been exposed to multiple wildfires at least the past three decades. Rephrase to correct statements and provide reference(s) that have fire records underlying your statements.

   We are aware that fires occurred also in the past three decades. Indeed, this sentence does not contain the message we want to communicate. Therefore, we changed the sentence:

   Old: *While the Mediterranean region and the Western US are historically fire prone areas, Central Europe showed exposure to wildfires only in the recent years, e.g. in Treuenbritzen 2022, Brandenburg, Germany (Spiegel, 2022), and Küps 2022, Bavaria, Germany (BR, 2022).*

   New: *While the Mediterranean region and the Western US are historically fire prone areas and have been well studied on a larger regional scale (i. e. Barbero et al., 2015, 2020; Abatzoglou et al., 2021; Ruffault et al., 2020), fire occurrences and risks in the temperate climates of Europe have been studied rather on a national than on a regional level (i. e. Arnell et al., 2021; Fargeon et al., 2020; Bakke et al., 2023).*

5. Line 38-40: The claim that fire indices represent a statistical correlation between fire events and meteorological conditions is wrong. Please correct.

   We changed "*represent the statistical correlation between fire events and meteorological conditions*" to "*are statistical models build on the correlation between fire events and meteorological conditions*".

6. Line 40-41: Please provide reference that "They have been proven to produce reliable ratings of fire danger in short- and long-term weather predictions on a global scale".

   We added Di Giuseppe et al. (2016) as reference.

7. Line 42: please rephrase "do not guarantee", as this is an unclear statement. Fire indices have nothing to do with ignition at all.

   Thank you for your remark, which refers to only one part of the sentence. We rephrased *"do not guarantee"* to *"do not incorporate".*

8. Line 45 and other: Various concepts are used for the fire indices; please be consistent and potentially introduce relevant relations between concepts such as "fire risk", "fire weather", "fire danger", "fire indices", "likelihood of fire" and "probability of fire".

   Thank you for pointing out these inconsistencies in terminology. We changed all occurrences of *"fire danger", "fire risk" and "probability of fire"* to "*fire danger*". "Fire weather" and "fire indices" are kept since they are not directly related to fire danger and are terms on their own. "Likelihood of fire" is not used in our manuscript. It is used as "likelihood of fire events" and cannot be replaced with "fire danger".

9. Line 49, 57 and 59: Central Europe (line 49) and temperate climate (line 58 and 59) refer only to studies of England and France here. As these concepts are used for the HydBav later, please clarify the use and links between geographical regions and climate regions. Are results for England and France directly transferrable to HydBav?

   We appreciate your comment regarding the climate comparability of our study region with other studies. HydBav, France and England all belong to the same climate zone according to the Köppen and Geiger classification' (Rubel et al. 2017 & Rubel et al. 2010). Therefore, we think that results for England, France, and HydBav can be compared and that the three regions can be jointly addressed under the umbrella of 'Central Europe and temperate climate zone.

   We changed Line 49 from "*In Central Europe, trends related to fire danger are uncertain and not clearly distinguishable from natural variability*" to "*In temperate climate regions, such as central and western Europe, trends related to fire danger are uncertain and not clearly distinguishable from natural variability*" to clarify the statement.

10. Line 49-50: Please provide reference that trends are not distinguishable from natural variability in Central Europe. Further, this sentence relates to the weak trend signal relative to natural variability independent of how models represent the natural variability, and thus the link to the next sentence is wrong or not clear.

    We refer to the weak trend signal relative to natural variability. However, in multi-model studies, uncertainty is composed of two factors: (1) natural variability and (2) model uncertainty. Our linkage to the next sentences highlights the need for using modelling approaches representing natural variability, i. e. Single Model Initial-Condition Large Ensembles, as used in our study.

    We clarified our statement:

    Old: "*In Central Europe, trends related to fire danger are uncertain and not clearly distinguishable from natural variability. Arnell et al. (2021) and Fargeon et al. (2020) have shown for England and France, respectively, that this uncertainty originates from an under-representation of natural variability in climate multimodel ensembles.*"

    New: "*In temperate climate regions of central Europe, i.e. northern France and the UK, trends related to fire danger are uncertain and not clearly distinguishable from natural variability when multi-model climate ensembles are used (i. e. Fargeon et al., 2020; Arnell et al., 2021). Arnell et al. (2021) and Fargeon et al. (2020) have shown for England and France, respectively, that this uncertainty originates from the confusion of natural variability with structural uncertainty originating from the different climate models in the ensemble (model uncertainty after Hawkins and Sutton (2009)).*"

11. Line 52-53: Unclear sentence "In France, …" Please rephrase.

    We agree this sentence is unclear. We meant to say by "the exceedance of the fire danger signal decreases from South to North", that the fire danger signal in the South of France exceeds earlier the boundaries of inter-annual variability than in the North of France. However, we think this sentence does not add any significant additional value to our introduction and removed it.

12. Line 56: clarify the meaning of "natural variability of changes".

Thank you for highlighting the need for clarification. We do not mean "natural variability of changes", we mean natural variability of changes in future climate, where changes refers to "in future climate". For a better understanding we changed the prepositions in the sentence from "*Both studies highlight the importance of quantifying the natural variability of changes in future fire weather*" to "*Both studies highlight the importance of quantifying the natural variability in changes of future fire weather*".

13. Line 59-63: The claim here is that climate model ensembles using multiple models (but fewer simulations per model compared to SMILE) underrepresent natural variability, whereas SMILE does not. Please clarify how large ensembles using a single model (SMILE) better represent natural variability as compared to large ensembles from different models, and add references that support this claim.

    We agree that the term underrepresent is wrong in this context. Multi-Model ensembles mix natural variability with model uncertainty. For this reason, we chose a SMILE framework in our study that allows for a clear isolation of climate change signals from natural variability. We changed the sentences in Lines 59ff.:

    Old: "*This limitation, i. e. the under-representation of natural variability in fire danger estimates in regions with currently temperate climate, can be overcome by evaluating climate model simulations derived from a single model initial-condition large ensemble (SMILE). SMILEs represent an ensemble of simulations derived using one single climate model started at different initialconditions. This allows SMILEs to account for the internal variability of the climate system."*

    New: *"This challenge can be addressed by evaluating climate model simulations derived from a single model initial-condition large ensemble (SMILE) which enables a clear isolation of the forced climate change signal from natural variability (Deser et al. 2020). SMILEs represent an ensemble of simulations derived using one single climate model started at different initial conditions. The ensemble spread between the different SMILE members represents the internal variability, from which the forced response of the climate change scenario (i. e. RCP8.5) can be estimated by averaging over the SMILE members (Deser et al. 2020). Therefore, SMILEs are capable to robustly sample extreme events and their probability distribution (Maher et al. 2021).*

14. Line 70: The reference period (1980-2009) is not one of the established reference periods. Please explain the choice of the period. Why not use the almost identical period 1981-2010, which is a widely used reference period?

    We chose this time period because the CRCM5-LE model runs until the year 2099. Therefore, the future time period can only be set to the maximum year of 2099, i.e. 2070-2099, which is why we set the present time period to 1980 to 2009. We argue that using 1980 – 2009 is as good as using 1981 – 2010 and does not lead to wrong assumptions in our analysis.

15. Line 71: Please change "increases" to "changes", because your analyses were also able to detect if there were any decreases.

    You are right, we adjusted *"increases"* to *"changes"*.

16. Line 74: Please clarify which TOE you refer to (TOE of what?)

    We changed "*the time of emergence (TOE) is reached latest by 2099*" to "*the time of emergence (TOE) of the FWI is reached latest by the year 2099*".

17. Line 82: Please clarify whether your mean two domains in Europe, or two domains of which one is in Europe.

    We adjusted from "*over the domains in Europe and Northeast North America*" to "*over the two domains Europe and Northeast North America*".

18. Line 88: clarify "independent" (in which regards?). Fifty members based on the same model are far from independent as such.

    Thank you for raising this concern, which is also claimed by RC-3 (general comment). We conclude that our initial data set description is not clearly pointing out the setup of our dataset. Therefore, we revised the first two paragraphs of chapter 2.1:

    Old: *To quantify changes and natural variability in fire danger trends for Central Europe, we use the Canadian Regional Climate Model version 5 Large Ensemble (CRCM5-LE) of Leduc et al. (2019). The dataset consists of 50 members at a spatial resolution of 12 km and was generated within the ClimEx project (https://www.climex-project.org/) to assess the hydrological impacts of climate change in Bavaria and Québec. It includes continuous simulations of climate variables from 1950 to 2099 under the RCP8.5 emission scenario over two domains in Europe and Northeast North America (Leduc et al., 2019).*

    The CRCM5-LE is derived from the CanESM2-LE (Fyfe et al., 2017), which was created by applying small random perturbations at two different points in time (i. e. 1850 and 1950) to a 1000-year equilibrium climate simulation under pre-industrial conditions (Leduc et al., 2019). In a first step, small random atmospheric perturbations were added to the equilibrium run to obtain five historical simulation families starting in 1850. In a second step, ten random perturbations were added to each family, resulting in a 50 member ensemble. After a 5-year spin-up phase, the modeled climate of the initialized 50 members can be regarded as independent. This global SMILE was dynamically downscaled using the CRCM5 (Martynov et al., 2013; Šeparovic et al., 2013) to obtain the regional SMILE CRCM5-LE (Leduc et al., 2019). For more details on the ensemble setup, the reader is referred to Leduc et al. (2019) (CRCM5-LE) and Fyfe et al. (2017) (CanESM2-LE)

    New:

    To quantify changes and internal variability in fire danger trends for Central Europe, we use the Canadian Regional Climate Model version 5 Large Ensemble (CRCM5-LE) of Leduc et al. (2019). The CRCM5-LE obtained by nesting the regional climate model CRCM5 (Separoví c et al., 2013; Martynov et al., 2013) into the CanESM2-LE (Fyfe et al. 2017) over two domains (Europe and Northeast America). Thereby, the CanESM2 at an original spatial resolution of 2.88° was dynamically downscaled to 0.11° over these regions. The dynamical downscaling of a regional single-model initial condition large ensemble (SMILE) was carried out within the ClimEx project (https://www.climex-project.org/) to assess the hydrological impacts of climate change in Bavaria and Québec. The dataset includes continuous simulations of climate variables from 1950 to 2099 under the RCP8.5 emission scenario (Leduc et al., 2019).

    The driving CanESM2-LE (Fyfe et al., 2017) consists of 50 simulations, which were started by adding random perturbations to the initial atmospheric state of January 1st in 1950. These random perturbations were introduced by parameterizing a single aspect of model cloud properties using a different pre-set seed for each of the 50 simulations. This ensures that the climate change realizations are different from each other without changing the model dynamics, physics, or structure (Fyfe et al., 2017). After a 5-year spin-up phase, the modelled climate of the *initialized 50 members in the CRCM5-LE can be regarded as independent (Leduc et al., 2019), because the chaotic climate properties cause diverging climate trajectories soley based on the macro- and micro-initialization of the CanESM2 (Wood, 2023). Therefore, the differences among the 50 CRCM5-LE members can be interpreted as natural variability. For more details on the ensemble setup, the reader is referred to Leduc et al. (2019) (CRCM5-LE) and Fyfe et al. (2017) (CanESM2-LE).*

19. Line 91: please clarify "at this time"

    We dropped the sentence during editing our manuscript.

20. Line 93: please clarify the link between your study choice and the provided references.

    We changed the sentence from "*In this study, we interpret internal variability as natural variability (Böhnisch et al., 2021; Von Trentini et al., 2019; Kay et al., 2015)*" to "*Therefore, the differences among the 50 CRCM5-LE members can be interpreted as natural variability (Böhnisch et al., 2021; Wood, 2023; Mittermeier et al., 2019; Leduc et al., 2019)".*

21. Line 93: Please comment on this assumption (internal variability = natural variability) in the discussion or here. Potential limitations or lack thereof? Comment why this assumption is correct.

    We agree that internal variability does not equate to natural variability in any case. We decided to stick to the term internal variability throughout the manuscript, because we used the term natural variability to describe model internal variability. We clarified this section and changed all terms of "natural variability" to "internal variability".

    Old: *In this study, we interpret internal variability as natural variability, similar to Böhnisch et al. (2021); Von Trentini et al. (2019) and Kay et al. (2015)"*

    New: *Therefore, the differences among the 50 CRCM5-LE members can be interpreted as natural variability (Böhnisch et al., 2021; Wood, 2023; Mittermeier et al., 2019; Leduc et al., 2019), but will be referred to as internal variability throughout this paper (Hawkins and Sutton 2009)*.

22. Line 95: How does smaller (temperature) and equal (precipitation) member spread in your SMILE compared to EURO-CORDEX relate to the previous claim that SMILE overcome the limitation of multi-model ensembles related to under-representation of climate variability. The results of Von Trentini (2019) imply that multi-model ensembles represent a larger variability as compared to CRCM5-LE.

    Thank you for this comment. Ensemble spread in a SMILE is originating solely from internal variability, while in multi-model ensembles ensemble spread also includes structural variability (model uncertainty (s. Hawkins and Sutton 2009)). In cases where structural uncertainty is larger than internal variability, a multi-model ensemble such as EURO-CORDEX that also includes structural uncertainty would show larger variability between members than a SMILE that does represent internal variability only. However, the spread of the multi-model ensemble in such a case would come from structural uncertainty rather than from climate variability. If we want to achieve a good representation of climate variability rather than structural uncertainty, SMILEs are needed.

Old: *Their results have shown that the CRCM5-LE shows smaller member spread for temperature and equal member spread for precipitation than EURO-CORDEX (Von Trentini et al., 2019). In cases where structural uncertainty is larger than internal variability, a multi-model ensemble such as EURO-CORDEX that also includes structural uncertainty would show larger variability between members than a SMILE that does represent internal variability only.*

New: *Their results have shown that the CRCM5-LE shows a smaller member spread for temperature and equal member spread for precipitation than EURO-CORDEX (Von Trentini et al., 2019). In cases where model uncertainty is larger than internal variability, a multi-model ensemble such as EURO-CORDEX that also includes structural uncertainty would show larger variability between members than a SMILE that does represent internal variability only.*

23. Line 98: state which observational data were used for the bias correction.

Thank you for pointing out this very important missing information. We added a more specific description of the observation data used for bias correction:

Old: *The CRCM5-LE was bias corrected over the study area for the FWI input variables at a three-hourly resolution using the quantile mapping approach of Mpelasoka and Chiew (2009) (Poschlod et al., 2020).*

New: *The CRCM5-LE was bias corrected using the univariate quantile mapping approach of Mpelasoka and Chiew (2009) (Poschlod et al., 2020) over the study area for the different FWI input variables. […] For the bias correction, the ClimEx project's own meteorological Sub-Daily Climatological REFerence dataset (SDCLIREF) served as an observation reference. It combines hourly and disaggregated daily station data and is described in detail in Brunner et al. (2021). For each quantile bin of each month and sub-daily time step, correction factors were determined by pooling data over all members. The correction factors were applied to each member of the CRCM5-LE separately (Brunner et al., 2021).*

24. Line 100: please rephrase and clarify. Better represented when evaluating against what (wouldn't that be against climate data, which you state is what should be bias-adjusted in the first place)? "climate data" is very general, do you mean data from climate models?

We changed from "*Bias corrected data are commonly used for projections of fire weather indicators like the FWI, because frequencies of FWI extremes are significantly better represented than in non-bias-corrected climate data*" to "*Bias corrected data are commonly used for projections of fire weather indicators like the FWI (e. g. Yang et al., 2015; Cannon, 2018; Kirchmeier-Young et al., 2017; Ruffault et al., 2017; Fargeon et al., 2020) because they have been shown to be more accurate in reflecting fire danger than raw climate data in comparison to observation data (Yang et al., 2015)*".

25. Line 100: Has there been any studies evaluating the data you use against meteorological variables from observations (independent of the bias adjustment) or reanalysis over the region?

Thank you for this valuable remark. Yes, a comparison between observation data and bias corrected and reanalysis data is provided in the supplementary material of Poschlod et al. (2020). Their results show that the biases for temperature are positive and highly variable for the Alps, while they are negative for the other parts of the study area in the summer months June, July and August. For precipitation, the bias correction affects the pre-alpine regions, where the non-bias corrected data show differences > 200 mm from the bias corrected data.

26. Line 103: Please insert the stated rivers in Figure 1 in order to inform a reader, who is not familiar in the region, how the named rivers relate to the study area.

    We updated the figure according to your suggestions by adding the rivers.

27. Line 108: Does 's' refer to 'see'? Please write out. Also, use capital F in figure names.

    We adjusted to capital F in figure names and changed "s." to "see".

28. Line 110: 'water' and 'water availability' are imprecise. Clarify what water you mean and add a supporting reference. If you mean soil moisture or precipitation, these (and thus also the fire regimes) are likely highly heterogeneous within each subregion (in particular in mountainous areas).

    Thank you for pointing out the need for clarification. We changed the sentence to:

    Old: *This subdivision into complex landscapes is adopted from the ClimEx-Project and the study of Willkofer et al. (2020). Since fire is closely related to the availability, or rather the absence of water, we assume that the water availability, climatology and landscape of different river systems is reflected the fire regime of the selected subregions.*

    New: *This subdivision into complex landscapes is adopted from the study of Willkofer et al. (2020) and derived from the Bavarian State Office for the Environment (Landesamt für Umwelt n. d.). Since fire is closely related to the availability, or rather the absence of water (in terms of precipitation deficits), we assume that the water availability, climatology and landscape characteristics of different complex landscapes are reflected in the fire regime of the selected subregions.*

29. Line 109-111: Please clarify and justify your assumption. The subregions are not defined according to the river systems, i.e. river catchments (as seen from Fig. 1). As you state earlier (line 102 and title) and later (line 120-123), hydrology, climatology and landscape are highly variable in the study area, and is likely highly variable in particular in the mountainous areas, within a subregion, with consequences for fire characteristics. What is mean by "an area"?

    We adjusted the unclear terminology regarding river systems (to landscapes) and "an area" (to selected subregions) as described in the previous comment (28.). The subdivision of the study region into four subregions aims to address the trade-off between the number of regions and the amount of inter-subregion variability. While a further subdivision into even more subregions would further increase within subregion homogeneity, it would also make it more difficult to summarize findings. The four regions chosen for the analysis are sufficiently similar in terms of their climate in order to allow for a succinct spatial summary.

30. Line 112-120: Which period and data underlie the numbers presented here? Could a figure (temperature and precipitation spatial patterns) be added (e.g. in appendix) to make the information more intuitive to the reader?

    The numbers presented in the sentence you are referring to are supported by plots of mean temperature and precipitation in Willkofer et al. (2020). As we think that reproducing the content of these figures is of little added value, we instead added a clearer reference to the paper to the text: *Figures illustrating the climatology are provided by Willkofer et al. (2020).*

31. Line 129: As written, 'noon' refers only to wind. Rephrase so it refers to all variables (even 24h precipitation is measured at noon).

    We changed: *(temperature, relative humidity, wind speed at noon and 24-h accumulated precipitation)* to *(temperature, relative humidity, and wind speed - all at noon - and 24-h accumulated precipitation).*

32. Line 132: what is meant by 'bookkeeping'? This concept is linked to financial transactions. Can it be replaced by a more commonly used concept within natural sciences to be more intuitive to the reader?

    We adjusted: *The first three sub-indices represent the fuel moisture codes and can be understood as bookkeeping systems, which increase moisture after rain and reduce moisture for each day of drying.* to *the first three sub-indices represent the fuel moisture codes that contain information about antecedent conditions, e. g. increasing moisture after rain and decreasing moisture for each day of drying*.

33. Figure 2 caption: suggest replacing 'vegetation' with 'organic matter', 'fuel layers' or similar (as used in the main text) for clarity.

    We replaced *"vegetation"* with *"organic matter"*.

34. Line 143: please rephrase or clarify "without memory of past conditions". Because the fuel moisture codes have memory of past conditions, BUI and ISI have too.

    We changed *they are stateless and without memory of past conditions* to *they are stateless and only indirectly linked to past conditions.*

35. Line 156: High-altitude parts of the region will likely have snow in the beginning of the defined fire season. Please state if/how you have accounted for snow in the evaluation here. E.g. see last section in https://www.nwcg.gov/publications/pms437/cffdrs/fire-weather-index-system. If you are neglecting the effect of snow on fire danger, it is worth reflecting on it in the discussion.

    Thank you for your comment. Indeed, we do not take snow cover into account. As you suggest, we added this to the discussion section (s. RC-1 comment 81. and RC-3 comment on Line 359).

36. Line 157: Vitolo et al (2019) does not apply any fire season and should be replaced by reference(s) using or arguing for using April-September.

    We derive the definition of our fire season from Vitolo et al. (2019) who state: "By convention, the dry season in the northern hemisphere is assumed to start on 1st April and ends on 30 September, while in the southern hemisphere it starts on 1st October and ends on 31st March." Therefore, we believe that the reference is appropriate to justify the choice of the fire season definition.

    Old: *The generated dataset is later cropped to the fire season (April 1st to September 30th) of the northern hemisphere (Vitolo et al., 2019).*

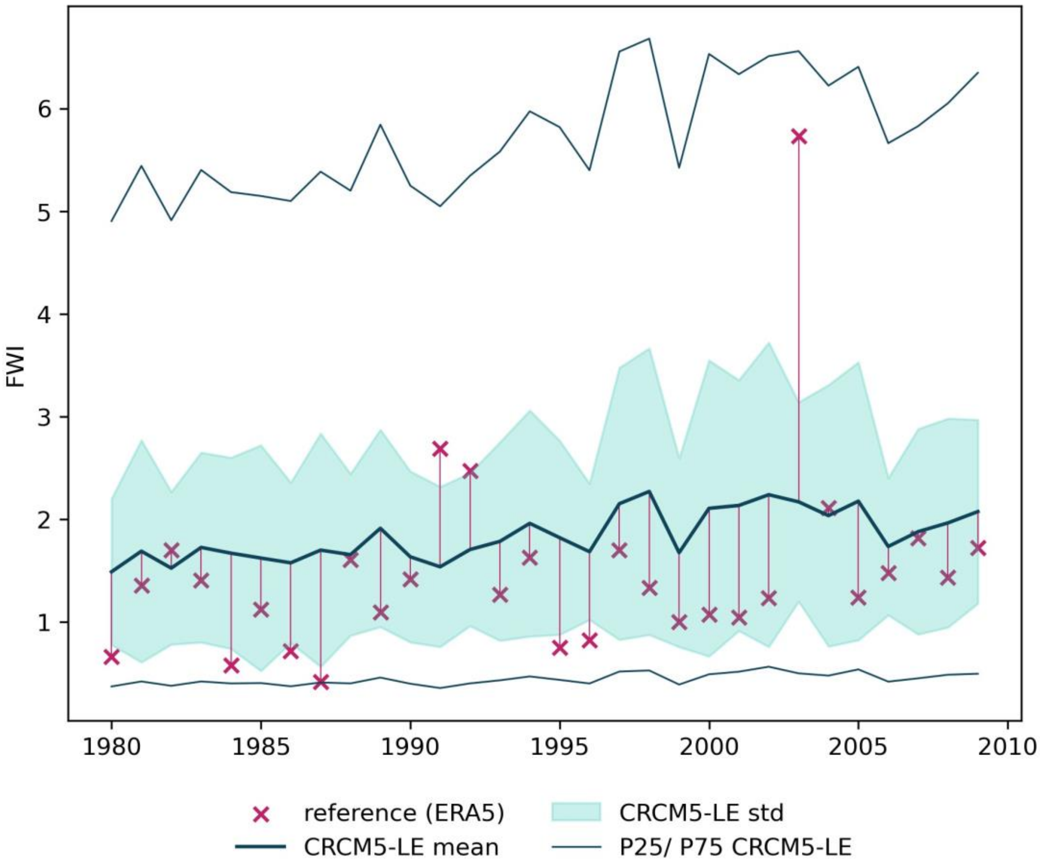
    New: *The generated dataset is later cropped to the dry season (April 1st to September 30th) of the northern hemisphere, which is used as the fire season in our study as suggested by Vitolo et al. (2019).*

37. Line 158: "annually calculated FWI values" is unclear (may refer to annual values, which I assume is not the case). Suggest to delete "of annually calculated FWI values" for clarity.

We implemented your suggestion and deleted "of annually calculated FWI values". Further, we changed "*We calculated daily FWIs for each year (January to December) and climate model ensemble member between 1980 and 2099 using the CFFDRS R package (Wang et al., 2017)*" to *"We calculated the FWI on a daily basis for each full year (January to December) between 1980 and 2099 and for each climate model ensemble member using the CFFDRS R package." for clarification.*

38. Figure 3: Please add a legend (ref. NHESS figure composition: https://www.natural-hazards-and-earth-system-sciences.net/submission.html#manuscriptcomposition).

We added a legend to Figure 3.



Median FWI for the CRCM5-LE mean (thick blue line) and standard deviation (light blue shading) in comparison to the reference dataset of Vitolo et al. (2020) marked pink (X for values, lines for deviation from the CRCM5-LE mean). Top and bottom blue lines mark the 25th and 75th percentile of the CRCM5-LE.

39. Line 170: As you state yourself, the results of the evaluation you have performed does not affect the climate change impact assessment of your study. Why do you not evaluate your data using measures that can actually reflect your data's ability to assess climate change impacts? For example its ability to represent historical changes.

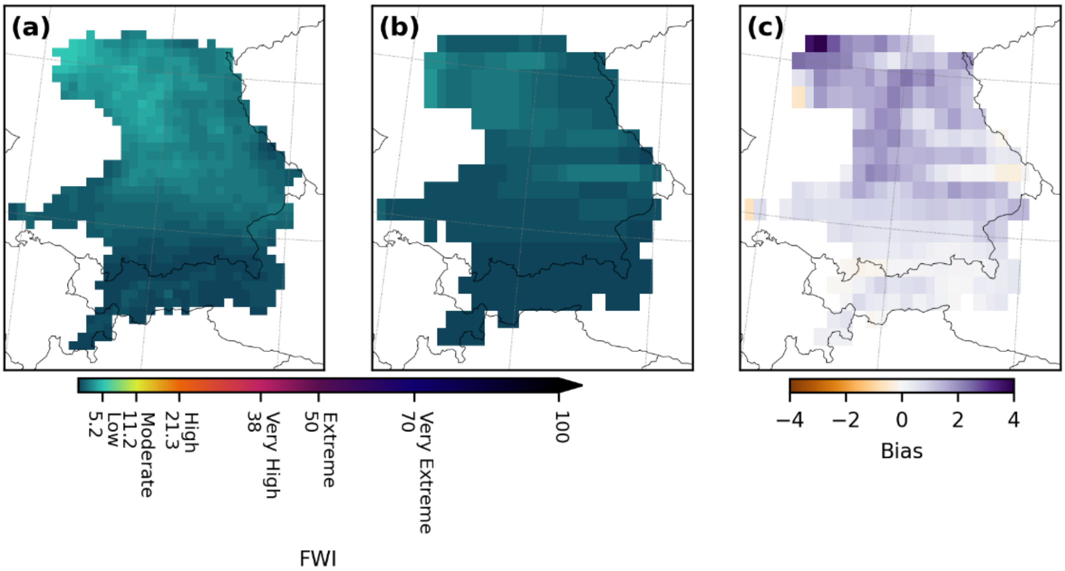
Thank you for this remark. We agree that this sentence is misleading in its message. On a temporal scale (see Figure 3), we show that the CRCM5-LE captures observation values within a tolerated range between the 25th and 75th percentile well.

We clarified this sentence also in correspondence with RC-3 (comment on Line 170) and added a remark to this paragraph, that the ensemble overestimates interannual variability:

New: *The CRCM5-LE overestimates the internal variability in comparison to the reference dataset (s. Figure 3 following the framework of Suarez-Guiterrez et al. (2021) to evaluate internal variability in SMILEs.*

40. Figure 4: The applied FWI colour scale is almost identical to the FWI colour scale provided in Table 1, but they reflect different FWI intervals. Please change for clarity.

We adjusted the color scale to the colors in Table 1 and updated the figure.



Median FWI of (a) the CRCM5-LE, (b) reference dataset of Vitolo et al. (2020) and (c) difference (CRCM5-LE – reference dataset) for the present time period (1980–2009). The dataset difference is calculated from resampling (a) to the spatial resolution of (b) using a nearest neighbour approach.

41. Section 2.5.1: The description is unclear in terms of when the different aggregations were applied (both in space and time), and when the continuous analysis vs the data split are applied. Consider reorganise the section to better fit each part of the analysis. Please also clarify how the 'extreme condition' (90th percentile) is computed (is it over the analysed time period, region or models, and in which order is it calculated). Clarifications in this section are necessary for reproducibility.

Thank you for pointing out the need for clarification. We did not perform a trend analysis but compared two time periods and therefore changed the subsection title from "*Trends*" to "*Changes in Fire Danger*". Further, we clarified the description of the pooling procedure, e. g. how we derived the median and extreme percentiles:

Old: *We evaluate the fire danger trends derived from the CRCM5-LE over the time period 1980 to 2099 in the study area with statistical metrics: Median conditions are examined using the 50th percentile (median) of the FWI. Extreme conditions are evaluated via the 90th percentile* *(extreme). The percentiles are calculated for different aggregation levels, either temporally, summarizing FWI values of a fire season on daily, monthly or annual basis, or spatially for the previously defined subregions. Increasing fire danger is either analyzed continuously from 1980 to 2099 or compared between two climate periods. For the climate period comparison, the dataset is split into two 30-year periods: 1980–2009 as present and 2070–2099 as future.*

New: *We evaluate changes of fire danger derived from the CRCM5-LE over the time period 1980 to 2099 in the study area with statistical metrics: Median conditions are examined using the 50th percentile (median) of the FWI. Extreme conditions are evaluated via the 90^th^ percentile (extreme). The percentiles are calculated for different aggregation levels, either temporally on a monthly scale or spatially for the previously defined subregions. We derive the median (50^th^ percentile) and the extreme (90^th^ percentile) for each ensemble member separately. Changes of fire danger are either compared between two climate periods or analyzed continuously from 1980 to 2099. For the climate period comparison, the dataset is split into two 30-year periods:1980–2009 as present and 2070–2099 as future. For these periods we derive the median and extreme percentiles for each fire season month for each of the 50 members of the CRCM5-LE*.

42. Line 174: Please provide which trend method was applied.

    We did not perform a trend analysis, instead, we compared FWIs across two time periods (1980-2009 and 2070 - 2099). Therefore, we rephrased "trend" to "changes".

43. Line 177: You state that you are "summarizing FWI values over a fire season on daily, monthly or annual basis". Summarizing would provide very different ranges of FWI on the different temporal scales, and it does not look like they are summarized in e. g. Fig. 5 (looks like average or median over each month). Please correct or clarify.

    Thank you for indicating this misleading description. We clarified the sentence by changing from: "*The percentiles are calculated for different aggregation levels, either temporally, summarizing FWI values of a fire season on daily, monthly or annual basis, or spatially for the previously defined subregions*." To: "*The percentiles are calculated for different aggregation levels, either temporally on a monthly scale or spatially for the previously defined subregions*".

44. Line 178 and line 182: Suggest replacing 'increasing' with 'changes in' for clarity. The analyses allow for changes in both directions.

    We changed "*increasing*" to "*changing*" in the suggested lines.

45. Line 216: Your results are scenario specific. Please specify the scenario, e. g. "according to RCP 8.5".

    The sentence starts now with "Based on the RCP8.5 emission scenario…".

46. Line 219-222: Please rephrase to clarify your reasoning.

    We agree that the original distinction between weak (one-level) and strong (two-level) fire danger level increases is imprecise and subjective. We rephrased "*weak*" to "*one level*" and "*strong*" to "*two level*". Further, we added a sentence, which explains why we also look at two-level increases. This is mainly because one level increases are found throughout the entire study area and we want to provide additional information by distinguishing between one level and two level rises in fire danger.

Old: *We distinguish between weaker (one 220 level, thin dots) and stronger (two levels, thick dots) fire danger level rises, because in July and August almost the entire study area shows a robust fire danger level rise of one level for both median and extreme conditions. This helps us to identify regional hotspots*.

New: *We distinguish between one-level (thin dots) and two-level (thick dots) fire danger level rises because the entire study area shows fire danger level rises of one level for both median and extreme conditions in July and August. Highlighting grid cells, which experience a rise of at least two levels, helps us to identify regional hotspots of future increases in fire danger*.

47. Line 223: Why is not the Southgerman Escarpment mentioned here (regions of strongest rises in extreme FWI in July to Sep)?

    We clarified this sentence, which refers to changes of at least two fire danger levels.

    Old: *The Southgerman Escarpment in the northwest of our study region is most affected by changes in the median FWI, while the Alps and the Eastern Mountain Ranges experience the strongest fire danger level rises in the extreme FWI in the months July to September (s. figure 5)*

    New: *We find increases in fire danger of at least two levels for the Southgerman Escarpment in July and August for the median FWI. The other subregions (Alps, Alpine Foreland, Eastern Mountain ranges) are affected by a two level rise only in July, whereas the western parts of the Alpine Foreland and parts of the Eastern Mountain ranges are affected by a two-level rise in September (see Figure 5).*

48. Line 226, 228 and 229 and potentially other places: 'median case' and 'extreme case' are unclear concepts. Do you refer to 'median FWI' and 'extreme FWI'? Please be consistent with concepts or clarify newly introduced ones.

    Thank you for highlighting this. We refer to the median FWI (50th percentile) and extreme FWI (90th percentile) of our results. We adjusted the sections accordingly.

    Old: *The median case points out that high fire danger becomes the average condition in the Alpine Foreland by 2080, in the Southgerman Escarpment by 2060 and in the Eastern Mountain Ranges by 2070 (s. figure 6 [1]). The Alps are exposed to high fire danger only in the extreme case (s. figure 6 [2]) from 2070 onwards. The other subregions are much more strongly affected in the extreme case:*

    New: *The median FWI points out that high fire danger becomes the average condition in the Alpine Foreland by 2080, in the Southgerman Escarpment by 2060 and in the Eastern Mountain Ranges by 2070 (see Figure 6 [1]). The Alps are exposed to high fire danger only in the extreme FWI (see Figure 6 [2]) from 2070 onwards. The other subregions are much more strongly affected in the extreme FWI:*

49. Line 233: Why do you state 'mean conditions (median)' and not 'median conditions' (what is the difference)? Please clarify in the text. Similarly, clarify similar statement in line 349: 'On average (median)'

Thank you for pointing this out. We are looking at median conditions throughout the study and clarified this by replacing "mean" or "average" conditions by "median" conditions. Further, we edited Section 2.5.1. to clarify that we derive the median and extreme percentile on a member basis and use the average / mean to derive the ensemble mean for the median and extreme percentiles, when we speak about ensemble mean.

Old: *The ensemble mean shows hardly any fire danger changes over the 21st century in the median and extreme case for April (s. figure 6). High fire danger becomes the mean condition (median) in the summer months for large parts of the study region (figures 5 and 6).*

New: *The ensemble mean shows hardly any fire danger changes over the 21st century in the median and extreme FWI for April (see Figure 6). High fire danger becomes the median condition in the summer months for large parts of the study region (Figures 5 and 6).*

50. Line 236: Please consider using the phrasing 'mid 21st century' instead of 'middle of the 21st century'.

    Thank you for pointing this out. We replaced "*middle of the 21st century*" by "*mid 21st century*".

51. Line 240-241: Please clarify how your findings indicate that the distribution of the FWI extremes resembles the distribution of the FWI median? Figure 7 clearly shows that the distributions are different both in terms of mean and standard deviation.

    We removed this statement from the text.

52. Line 242: consider replacing 'changes' to 'increases' for clarification.

    We clarified the sentence by following your suggestion.

    Old: *FWI changes in the Alps are weaker than in the other subregions.*

    New: *FWI increases in the Alps are weaker than in the other subregions.*

53. Line 244: please specify what you mean by 'strongly'.

    We exchanged *"strongly"* with *"continuously"*.

54. Line 244-245: Please clarify what you mean and which parts of the results you refer to. Your statement here seem opposite compare to the preceding sentence (median FWI increase strongly vs median FWI shows hardly any changes).

    We clarified that this section soley refers to the results for the Alps.

    Old: *Throughout the 21st century, the median and extreme FWI increase strongly. While the extreme FWI is projected to shift from low to moderate fire danger, the median FWI shows hardly any changes and remains in the no danger level (below five) according to EFFIS (2021).*

    New: *Throughout the 21st century, the median and extreme FWI increase continuously in the Alps. While the extreme FWI is projected to shift from low to moderate fire danger in this subregion, the median FWI shows hardly any changes and remains in the no danger level (below five) (see Table 1).*

55. Line 245: Why state the EFFIS reference here, when every classification of fire danger level in the (also previously mentioned) results is based on it?

    You are correct, this is duplicated information. We changed the reference to Table 1 (see applied changes in comment 54.)

56. Line 247: clarify which average you are referring to.

    We clarified the sentence from "*In the extreme case, the average fire danger is moderate*" to "*In the extreme case, the ensemble mean fire danger is moderate".*

57. Line 247-248: the interval signs in parenthesis are wrong.

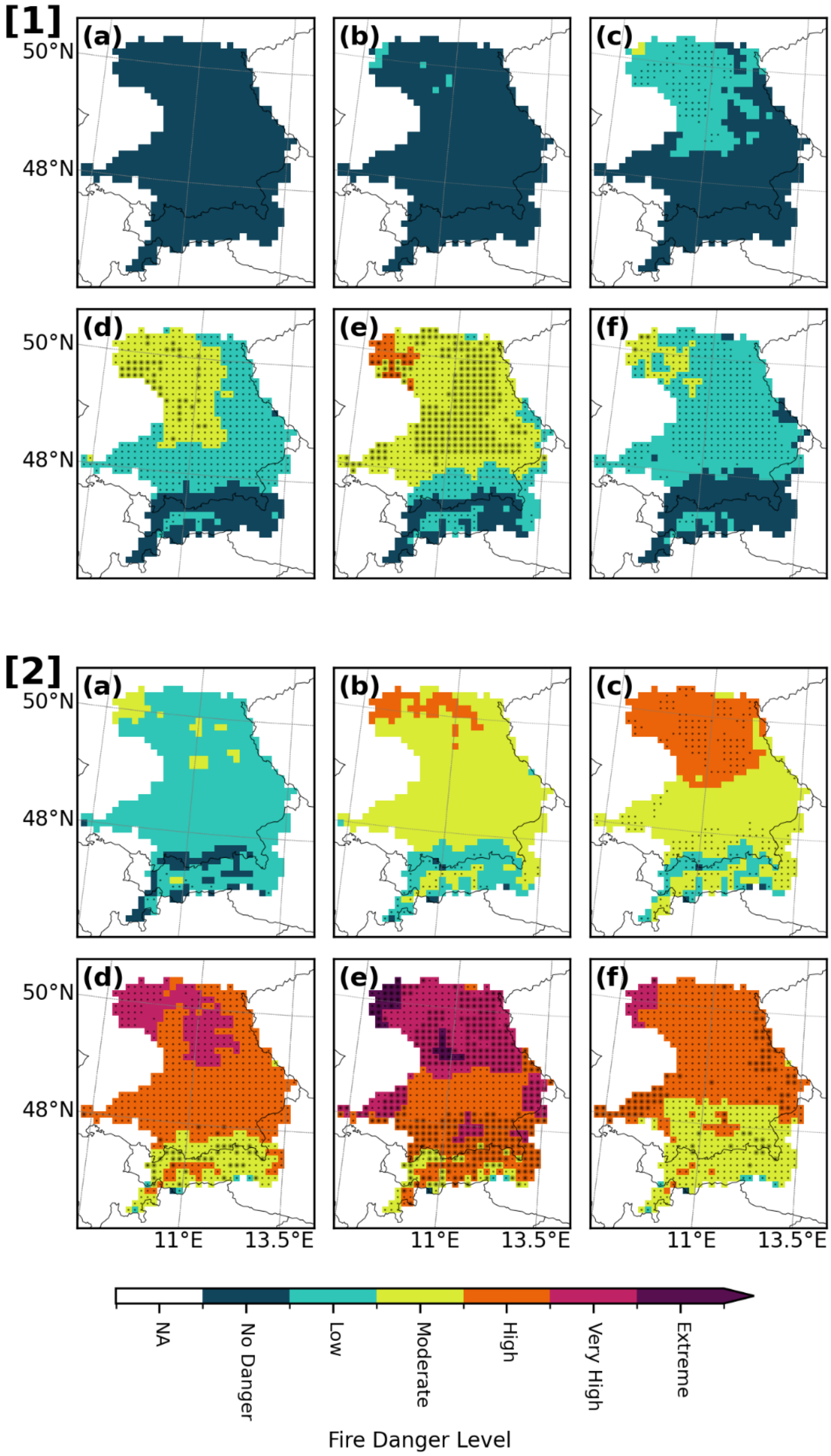
    Thank you for finding this tiny but very relevant mistake – we corrected it.

    Old: *(11.2 < moderate > 21.3) & (21.3 < high > 38)*

    New*: (11.2 > moderate < 21.3) & (21.3 > high < 38)*

58. Figure 5: The levels referred to in the result section would be more easily recognisable if a colour scale using discrete colours was used. Discrete colours would provide a more clear message to the reader, in particular when these levels are the main message of these results, and not the small varieties in between. Please consider changing to discrete colours.

    We agree that a discrete color scale is more appropriate here than a continuous color scale. Therefore, we, adjusted the plot accordingly.



Fire rings show the ensemble mean of the monthly median ([1], 50th percentile) and extreme ([2], 90th percentile) FWI of each subregion (Alps, Alpine Foreland, Southgerman Escarpment and Eastern Mountain Ranges (a-d)) during the fire season (April - September) between 1980 and 2099.

59. Figure 5 caption: do you mean "at least" two levels (indicated by thick black dot), or are there never more than two levels?

    We clarified this to at "at least one/two levels".

    Old: *Ensemble mean of the median ([1], 50th percentile) and extreme FWI ([2], 90th percentile) by fire season month (April (a) - September (f)) for the future time period 2070–2099. Dots indicate that 90% of the CRCM5-LE members agree on a fire danger level increase of one (thin black dots) or two (thick black dots) levels compared to the present period (1980–2009).*

    New: *Ensemble mean of the median ([1], 50th percentile) and extreme FWI ([2], 90th percentile) by fire season month (April (a) - September (f)) for the future time period 2070–2099. Dots indicate that 90% of the CRCM5-LE members agree on a fire danger level increase of at least one (thin black dots) or at least two (thick black dots) levels compared to the present period (1980–2009).*

60. Line 259-260: please provide numbers or proportions in parenthesis.

    We provided the percentage of days per fire season in parenthesis.

    Old *While the higher danger levels already occur in the present, very high danger levels additionally occur in the future.*

    New: *High fire danger already occurs in the present (1 % in the Alpine Foreland, 3 % in the Eastern Mountain Ranges) and shifts towards fractions of 10% in the Alpine Foreland and almost 20% in the Eastern Mountain Ranges in the future, where very high danger levels additionally occur (1 % in the Alpine Foreland, 3 % in the Eastern Mountain Ranges)*

61. Line 262: Please provide in what ways they are similar. EMR is not described in other terms than relative to Alpine Foreland.

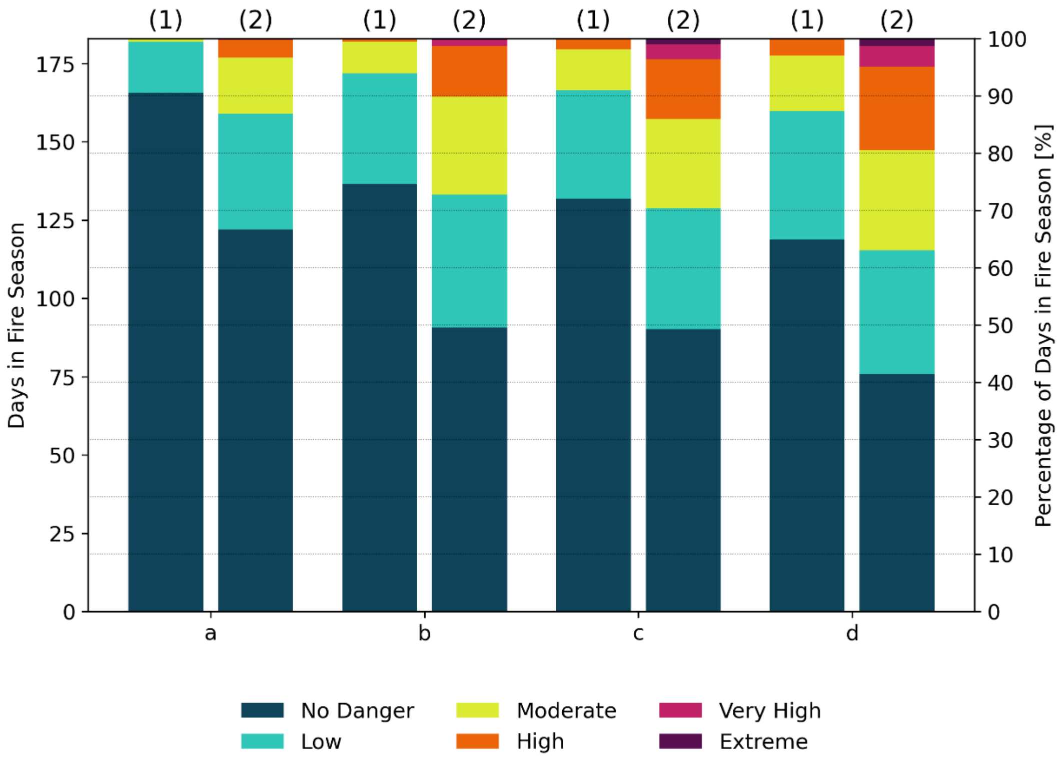
    We clarified the section.

    Old: *In the Eastern Mountain Ranges, similar results are observed:*

    New: *The Eastern Mountain Ranges show similar results as the Alpine Foreland in terms of the number of days with a certain fire danger level. However, the Eastern Mountain Ranges differ slightly from the Alpine Foreland: Higher fire danger levels already occur in the present, very high danger levels additionally occur in the future. In comparison to the Alpine Foreland, moderate fire danger days are less frequent and high fire danger days are more frequent in the Eastern Mountain Ranges.*

62. Figure 8: Please consider adding proportions on the right y-axis, as proportions are used in the text.

    We added proportions to the figure.



Number of days of fire danger levels in the fire season (April – September, 183 days) for the present (1; 1980–2009) and future (2; 2070–2099) climate period. FWI danger classes are derived for the subregions (a) Alps, (b) Alpine Foreland, (c) Southgerman Escarpment and (d) Eastern Mountain Ranges

63. Figure 8 caption: Please clarify by specifying what is meant by frequency (e.g. "number of days within a fire season")

    We exchanged *"frequency"* by the *"number of days"*. For the implementation see the Figure caption in the previous comment (RC-1 comment 62)

64. Line 278: As in line 215, clarify the scenario dependence of your results (in line with your statement in line 306-307). The way it is phrased now imply more certainty about the future than we can state.

    We added "*when a RCP8.5 scenario is assumed*" to the sentence to emphasize that our results soley refer to the RCP8.5 scenario.

    Old: *Our results demonstrate that fire danger increases dramatically over the next few decades in Central Europe.*

    New: *Our results demonstrate that fire danger increases dramatically until the end of the 21st century in central Europe, when a RCP8.5 scenario is assumed.*

65. Line 279: Why move away from the defined classes? How is hazardous defined?

    Thank you for recognizing this stylistic inconsistency. We rephrased the sentence:

    Old: "*The trend towards hazardous fire danger conditions in the future emerges for all presented metrics in this study, i.e. different temporal, spatial and ensemble aggregation levels.*"

New: "*The increase of days with conditions favoring fire danger levels of high and higher in the future emerges for all presented metrics in this study, i. e. different temporal, spatial and ensemble aggregation levels*".

66. Line 285-286: please state the variable (FWI) that is compared.

    We added FWI to the sentence.

    Old: *Before starting our analysis, we compared the results from the CRCM5-LE to the dataset of Vitolo et al. (2020) for the present climate period (1980–2009).*

    New*: Before starting our analysis, we compared the FWI results from the CRCM5-LE to the FWI dataset of Vitolo et al. (2020) for the present climate period (1980–2009).*

67. Line 289-291: Please state in what relevant ways the formulas have been adjusted (i.e. relevant implications). Is this a more likely reason for the differences as compared to the fundamental differences in how the underlying meteorological data are produced?

    Thank you for this valuable remark. This statement originates from Vitolo et al (2019). Vitolo et al. (2019) state in their algorithm validation section "Although the outputs are rather close, they do not match exactly. The reason is that the ECMWF model follows the formulation defined in the reference FWI implementation outlined in Van Wagner (1987) without modifications. Wang et al. (2017) instead, have modified some of the original equations (i.e. EQs 12 and 15) leading to the calculation of DMC and DC. As a consequence, FWI and DSR also slightly differ."

    However, Wang et al. (2017) do not explain where and how they adjusted the original formulas. We therefore keep this sentence as it is.

68. Line 293: please rephrase sentence to be more to the point. It is unclear how the tiling patterns referred to in the text 'has to be discussed' and not the ones seen e. g. in September (Fig. 5 [2]f) or at smaller scale in the Alps in July-Sep (Fig 5 [1]def and [2]def).

    Thank you for your comment. We agree that this sentence is not clearly brought to the point. We rephrased the whole section to clarify our intentions with mentioning the tiling pattern, though it is not as strongly visible in Figure 5 after adjusting to a discrete color scale.

    Old*: Another aspect, which has to be discussed, is the strong tiling pattern visible in figure 5 [2] in the months June and August. This tiling pattern is already visible in the extreme values of the input variables. We provide a sensitivity analysis of the FWI in the Appendix (s. figure C1), where the tiling occurs for temperature and relative humidity in the 95th 295 percentiles as well. The pattern correlates with invariate fields from the geophysical baseline parameterization of the CanESM2, e.g. bedrock depth. Over the areas where the strong tiling occurs, bedrock depth is about 5m. The water storage potential of the ground is especially high in this area compared to its surrounding areas with an average bedrock depth between 1 or 2 meters. Such high storage potential can affect evaporation and leads to a higher cooling in areas with high bedrock depths which results in lower 300 temperatures and higher relative humidity.*

    New: *Though the CRCM5 reproduces the response structures much finer than CanESM2 and adds robust high-resolution features (Böhnisch et al., 2020), we find in the northern parts of the study area tiling patterns corresponding to the geophysical baseline parameterization of the CanESM2 (see Figure A3). The tiling occurs in the sensitivity analysis provided in Figure A.3 for temperature* *and relative humidity in the 95th percentile, when the FWI is calculated with a factor of two for temperature and relative humidity. The pattern correlates with the bedrock depth of the CanESM2, which might affect the water storage potential of the ground. Over the areas where the tiling occurs, bedrock depth is about 5m, which is relatively high in comparison to the surrounding areas with an average bedrock depth between one or two meters. Such high storage potential can affect evaporation and leads to a higher cooling in areas with high bedrock depths which results in lower temperatures and higher relative humidity. The tiling occurs only under very extreme FWI conditions (95th percentile) and might lead to an overestimation of our results in the extreme FWI (90th percentile) for the Southgerman Escarpment.*

69. Line 296: please change 'correlates' with a more appropriate word or provide correlation results.

    We exchanged *'correlate'* with *'correspond'*.

70. Line 302: you mention the uncertainty related to the chosen climate model. Please elaborate on this point in relation to the specific model you applied.

    Thank you for highlighting the missing discussion of the performance of the CRCM5-LE in comparison with other climate models. We edited this section carefully by adding a sentence that explains the difference between the CRCM5-LE and other CORDEX models in terms of precipitation and temperature.

    New: "*In comparison to the CORDEX ensemble, the CRCM5-LE shows drier and warmer climate change signals for temperature and precipitation (Von Trentini et al. 2019). These characteristics of the CRCM5-LE are in line with the results from the validation (see Figure 3) and indicate an overestimation of our results*."

71. Line 316: please remove "potential of". FWI describes the fire weather, not the potential of fire weather.

    We removed "*potential of*".

72. Line 318-327: Please consider deleting this paragraph, and alternatively reduce the main message to a single sentence in the methods chapter arguing for your use of danger levels.

    We deleted the paragraph and explained the reason for using fire danger levels in the methods section.

    New sentence in chapter 2.4 (Methods): *To facilitate the interpretation of the FWI, we use the seven fire danger classes proposed by the European Forest Fire Information System (EFFIS) (EFFIS, 2021) and assign the FWI to particular fire danger levels. These FWI danger levels and their corresponding color scheme are shown in Table 1.*

73. Line 328-331: Please elaborate briefly on the flammability of the surface in your study region.

    We added a land use map to the appendix and described the flammability in the study region and subregions briefly in the second paragraph of chapter 4.2. We also discussed your previous comment on snow cover here.

    New: *While the FWI addresses fire danger in a meteorological context, it does not account for the flammability of the surface. Land-Use in our study area is complex, but contiguous forests are* *present in all four subregions, especially the Eastern Mountain Ranges and the Alps. Persistent snow cover from snowfall in the winter season prevents fire occurrences in spring in the Alps (Conedera et al. 2018) and other regions of high elevation, though fire weather conditions might be met. Large parts of the South German Escarpment and Alpine Foreland are used for agricultural purposes, where fires can spread fast under dry conditions (see Figure A1). However, these regions are more densely populated than the other two regions (Eastern Mountain Ranges and the Alps), which allows a faster mitigation of fire incidents. For large-scale FWI analyses, non-burnable areas such as deserts and bare soil are masked out (Vitolo et al. 2020, Touma et al. 2021). In the context of the study area HydBav and the 11-km resolution of the CRCM5-LE, land use is highly variable on a sub-pixel scale and non-burnable areas (e. g. lakes, snow- and ice-covered areas and urban areas) are therefore not masked out (see Figure A1).*

74. Line 333-339: Please reflect/explain results rather than summarise them.

We shortened this section and emphasized the differences between mountainous and non-mountainous terrain.

Old: *We find that the region affected most strongly by FWI increases is the northwest, i. e. the Southgerman Escarpment (s. figures 5, 6 and 8). Noteworthy is, that average changes (median) are smaller in the Alps but increases in the extreme FWI are strongest in the Alps. The trends of the median are similar for the Alpine Foreland and the Eastern Mountain Ranges, but FWI extremes in the Eastern Mountain Ranges increase more strongly than in the Alpine Foreland. We summarize that increases in fire danger extremes are more pronounced than increases in median conditions and therefore variability increases in regions with heterogeneous terrain (Alps and Eastern Mountain Ranges). For less complex terrain (Alpine Foreland and Southgerman Escarpment), the increases in fire danger extremes are less variable. These findings corroborate findings by Wastl et al. (2012), who explained the higher fire danger variability in mountain regions by the higher terrain variability, i.e. rain-shadow effects and katabatic dry winds (foehn).*

New: *Our results (see Figures 5, 6 and 8) show that increases in fire danger extremes are more pronounced than increases in median conditions. In the Alps, this is demonstrated by smaller changes in the median FWI than in the extreme FWI. For less complex terrain (Alpine Foreland and Southgerman Escarpment), the increases in fire danger extremes are less variable. For example, the increases of the median FWI are similar for the Alpine Foreland and the Eastern Mountain Ranges, but extreme FWI (90th percentile) in the Eastern Mountain Ranges increase more strongly than in the Alpine Foreland. This finding indicates that variability of the FWI increases more strongly in mountain regions than in non-mountain regions and corroborates findings by Wastl et al. (2012), who explained the higher fire danger variability in mountain regions by the higher terrain variability, i.e., rain-shadow effects and katabatic dry winds (foehn), by evaluating weather station data.*

75. Line 340: increases in variability (line 337) is not the same as high variability in general (line 340). Please elaborate what you mean by your findings (increasing variability over time in mountainous regions) corroborate the findings by Wastl et al (2012; higher variability in mountainous regions than other regions).

We clarified this sentence in line with specific comment 74.

New: *For example, the increases of the median FWI are similar for the Alpine Foreland and the Eastern Mountain Ranges, but extreme FWI (90th percentile) in the Eastern Mountain Ranges increase more strongly than in the Alpine Foreland. This finding indicates that the variability of* *the FWI increases more strongly in mountain regions than in non-mountain regions and corroborates findings by Wastl et al. (2012), who explained the higher fire danger variability on in mountain regions by the higher terrain variability, i.e. rain-shadow effects and katabatic dry winds (foehn), by evaluating weather station data.*

76. Line 345: Unclear whether 'extreme FWI conditions' represent the 90th percentile or the classes (FWI>50). In case of the former, do you mean elevated conditions compared to former months or compared future to present. In case of the latter, is that not seen directly from the figure and not 'implied' from your findings? Please clarify the meaning of this sentence.

    We changed this sentence from: *This finding indicates that the seasonal variability is higher for the last three months of the fire season and implies that the probability for extreme FWI conditions is elevated during these late summer months*.

    To: *This finding indicates that the variability of the FWI is higher in the last three months (July, August, September) in comparison to the first three months of the fire season (April, May, June). This implies that extreme FWI events are more likely to occur in the second half of the fire season (July, August, and September) than in the first half of the fire season (April, May and June).*

77. Line 348: 'tremendous' is subjective, please clarify. See also 'dramatic' in line 362 and 'strikingly' in line 364-365.

    We changed "*tremendous*" to "*substantial*", "*dramatic*" to "*as high fire danger levels as*" and "*strikingly*" to "*remarkable*".

78. Line 349: by 'seasonal', do you mean 'monthly'? In which ways are they hotspots, in terms of general conditions/increases/other?

    We rephrased this sentence:

    Old: *Especially the months August and July can be identified as seasonal hotspots throughout the study area. On average (median), the fire danger will be high in the Alpine 350 Foreland, Southgerman Escarpment and Eastern Mountain Ranges and moderate in the Alps by the end of the century.*

    New: *Especially the months August and July can be identified as months with the highest fire danger of the season throughout the study area*.

79. Line 358: The use of vegetation in Figure 2 caption implies also litter and organic matter on the ground. In this context, vegetation is necessary for fire development because it comprise the fuel. Is it the same use of vegetation here? I assume vegetation is highly present during winter also, although parts are covered by snow, and deciduous trees lack their green leaves. Please clarify the text.

    We clarified this sentence. We do not refer to vegetation itself, but to the vegetation period. In the winter season, the vegetation is not actively growing, which leads to decreased fuel moisture. We rephrased this section (see Wastl et al. 2012 and Conedera et al. 2018).

    Old: *For the Southern Alps, Wastl et al. (2012) identified the main fire season between December and April because of low precipitation and missing vegetation cover in the winter half year. Therefore, future studies assessing changes in fire danger in the Alps should focus on the whole year instead of the summer season only.*

New: *For the Southern Alps, Wastl et al. (2012) identified the main fire season between December and April because of low precipitation and decreased fuel moisture outside the vegetation period in the months December to April (Conedera et al., 2018). With respect to the increasing altitude of vegetation, increasing length of the vegetation period and decreasing snow cover (Rumpf et al., 2022), future studies assessing changes in fire danger and fire events in the Alps and other temperate climate regions should consider analyzing the whole year instead of the summer months only.*

80. Line 359: 'half year' typically refers to six months. Consider changing to 'period' or similar, as you refer to December-April.

    We exchanged *"winter half year"* to *"months from December to April"*.

81. Line 358-360: would FWI be suitable for the winter season? The reasoning provided here include lack of vegetation, whereas this is not accounted for in FWI. And what about snowfall and snow cover? Further, would you assume the temperature thresholds included in FWI calculation be exceeded in the Alps in winter? Please reflect on the considerations needed for such assessments.

    We added a sentence explaining that the FWI is not suitable for the winter season and suggest to consider using other approaches in cases where the winter season is explicitly considered, e.g., the one proposed by Pezzatti et al. (2020). However, our study only focuses on the months April to September, when snow cover in a 11 km grid scale plays a minor role for forest fire danger, because it occurs only in unvegetated high alpine terrain, which is sampled only by a small fraction of the 11 km grid.

    Old: *For the Southern Alps, Wastl et al. (2012) identified the main fire season between December and April because of low precipitation and missing vegetation cover in the winter half year. Therefore, future studies assessing changes in fire danger in the Alps should focus on the whole year instead of the summer season only.*

    New: *For the Southern Alps, Wastl et al. (2012) […]. However, the FWI can not capture these land cover and vegetation specific changes and therefore other methods should be considered to quantify fire danger outside of the summer period, i. e. Pezzatti et al. (2020).*

82. Line 366: states 'exists currently no fire danger', however you have fire danger everywhere (as fire danger is defined as the estimates from the index, regardless of values). Please clarify.

    We rephrased to *"the fire danger (FWI) is almost zero"*.

83. Line 372-373: you mention overestimation of natural variability. How does this relate to line 59? What about potential underestimation when using SMILE? If a model has a limitation (e.g. in representing natural variability), all realisations from that model suffer from the same limitation. If you or other have validated the ability of SMILE to represent natural variability, please state this in the text and refer to relevant evidence. Applies also for line 375.

    Line 59 refers to multi-model ensembles, which do not overestimate natural variability but do not allow to distinguish between natural variability and model variability. We revised this section also in accordance with specific comment 12:

Old: *Reasons for this delay could be due to the later and shorter reference period (1995–2015), the overestimation of natural variability in the multi-model ensemble (Fargeon et al., 2020) or the slight overestimation of the CRCM5-LE (s. chapter 2.4).*

New: *Reasons for this delay could be the later and shorter reference period (1995–2015), the larger uncertainty range originating from natural variability, model uncertainty in the multi-model ensemble (Fargeon et al., 2020), or the warmer and drier climate change signal of the CRCM5-LE (Von Trentini et al., 2019).*

Further, we edited line 59 in RC-1 comment 13 and elaborated on this in the new discussion chapter uncertainties as suggested by RC3 comment 1.

84. Line 373: 'slight overestimation of the CRCM5-LE'. Clarify, what does it overestimate?

    Revised in specific comment 83.

85. Section 4.4: the title and content of the section does not match (impacts [title] vs conditions influencing flammability, emergency in other regions. Further, the content is not coherent. Please revise and clarify the message.

    Thank you for your comment. We agree that this section is not consistent in terms of its message. We revised this section in the following way:

    1. Adjusted the Title to "Regional Shifts and Implications"
    2. Focused on spatial differences between the fire regime in the Mediterranean and Central Europe in the first paragraph:

       Old: *However, for wetter, more productive regions and seasons, i.e. our study area in Central Europe, aridity does not limit fuel availability, which implies higher sensitivity to flammable conditions (e.g., after hot and dry seasons) and points out the importance of considering vegetation and fuel structure changes in further studies (Pausas and Paula, 2012; Turco et al., 2018). Further, Bowman et al. (2020) suggest that declining snow cover in spring and drier fuels in summer will increase burned area in mountain forests, as present in the Alps and Eastern Mountain Ranges in our study area.*

       New*: For wetter, more productive regions, i.e. our study area, aridity does not limit fuel availability. Bowman et al. (2020) suggest that declining snow cover in spring and drier fuels in summer will increase burned area in mountain forests, as present in the Alps and Eastern Mountain Ranges in our study area. This implies higher sensitivity to flammable conditions (e.g., after hot and dry seasons) and an extension of fire events to more northern latitudes and higher elevations.*

    3. We generalized the Bavarian specific section to a broader call for mitigation measures in Central Europe

       Old: *For the Mediterranean, Turco et al. (2018) expect changes in meteorological fire weather of such a magnitude, that current fire suppression measures are not sufficient anymore. The guidelines for forest fire defence in the federal state of Bavaria currently only ask the public for cautious behaviour when fire danger is elevated. In case of high or very high fire danger, 390 surveillance flights are carried out in the respective areas*

(StMLF, 2013). Studies in other regions, i.e. the UK (Arnell et al., 2021) and France (Fargeon et al., 2020), suggest that increases in fire danger should be considered in emergency, land use and management planning to mitigate future fire risk. Taking the results of our study into account, these suggestions apply for Hydrological Bavaria as well.

New: *Expected changes in fire weather in the Mediterranean are of such a magnitude, that current fire suppression measures are not sufficient anymore (Turco et al., 2018). Studies in other regions, i.e. the UK (Arnell et al., 2021) and France (Fargeon et al., 2020), suggest that increases in fire danger should be considered in emergency, land use and management planning to mitigate future fire danger. Despite the differing climatic conditions and land cover in comparison to France and England, our research findings indicate that forest fire mitigation measures must be proposed for central Europe as well.*

86. France and UK: Several places in the manuscript, results of France and UK is used for guiding and comparing the results of the present study, and to make final recommendations for fire emergency. However, you do not reflect on potential relevant differences between the regions (e.g. hydroclimotology and vegetation). Please consider commenting on such aspects.

    We agree that our manuscript was not taking differences between the study areas of Fargeon et al. (2020) (France) and Arnell et al. (2021) (UK) into account sufficiently. We stressed the regional differences between France, UK and Germany in the last sentence of our Discussion chapter.

    Old: *Taking the results of our study into account, these suggestions apply for Hydrological Bavaria as well.*

    New: *Despite the differing climatic conditions and land cover in comparison to France and England, our research findings indicate that forest fire mitigation measures must be proposedfor Central Europe as well.*

    For a broader context of this paragraph the reader is referred to the response of the previous comment (RC-1, comment 85, 3rd answer)

87. Line 397 (and line 406): You state that the study area is not affected by high fire danger to date, but high fire danger is present in relatively large areas in current climate (Fig. 5[2]def, where the dots indicate a change from a currently high level).

    We agree with your comment and rephrase the sentence:

    Old: *The study area is not yet affected by wildfires and high fire danger to date, but will be affected in the future when assuming an RCP8.5 emission scenario and accounting for natural variability.*

    New: *To date, the study area is irregularly affected by wildfires and high fire danger occurs only under extreme conditions (90th FWI percentile). However, high fire danger will become more frequent in the future when assuming an RCP8.5 emission scenario.*

88. Line 397-398: Please clarify ‘by accounting for natural variability’.

    We dropped that phrase (see specific comment 87).

89. Line 398: Please clarify the difference between “strongest increase” and “most hazardous developments”

    We rephrased most *“strongest increases”* to *“strongest changes”* and *“hazardous developments”* to *“highest fire danger levels within the study area”* for clarification.

90. Line 400: please clarify in what terms, and in which results the statement “less strongly affected” applies. For example, in fig 5[2], Alps is the only region with dots in April and May, and the two regions you mention increase multiple fir danger levels as seen e.g. in Fig. 6[2] august. As mentioned earlier in the manuscript (line 324-326), increases in classes may provide a better approach to assess increases due to non-linearity, and thus and linear comparison (e.g. Fig 7) may not the best way to conclude the strongest trends.

    We revised this section and dropped the imprecise statement about regions which are “less strongly affected”.

    Old: "*The strongest increases and most hazardous developments are observed North of the river Danube in the summer months July and August for the subregions South German Escarpment and Eastern Mountain Ranges. Regions south of the Danube (Alps and Alpine Foreland), are less strongly affected by changes in the FWI but increases are still significant.”*

    New: *We find the strongest changes and highest fire danger levels north of the river Danube in the summer months July and August for the subregions South German Escarpment and Eastern Mountain Ranges.*

91. Line 401: the statement that FWI has a stronger variability for Alps and Eastern Mountain Ranges contradicts the findings in Fig. 7, where the standard deviation is smaller for these regions compared to the other subregions. Please clarify.

    We appreciate your comment to set this in context with the findings of Figure 7. Figure 7 is derived from highly aggregated data (30-year daily fire season running means) and therefore has a different aggregation level than Figures 5 and 6. However, this does not clarify the findings and we decided to drop this section.

92. Line 404: please consider repeating the hypothesis, and structure the conclusions by these.

    We restructured the conclusions to follow the different research questions/hypotheses.

    New Paragraph: *Our results provide clear answers to our initially proposed research questions. They demonstrate that fire danger increases significantly throughout the study area. We find the strongest changes and highest fire danger levels north of the river Danube in the summer months July and August for the subregions South German Escarpment and Eastern Mountain Ranges.. Our results also show that the time of emergence (TOE) is reached in all subregions before 2050. Further, we showed that not only the mean but also the lower boundary of the running mean, represented by the CRCM5-LEs standard deviation, exceeds the upper boundaries of the present climate (1980 - 2009) standard deviation before 2099 in all subregions for the 90th FWI percentile. Last, our findings highlight that the return period of present 100-year events shifts towards 10-year events by 2090 and the return periods for 100-, 50- and 20-year events shift to 50-, 20- and 10-year events, respectively, before 2050 throughout the analyzed subregions.*

93. Line 407: please clarify what ‘also’ refer to.

We dropped “also”.

94. Line 410: What about the data of the subregions and land cover (Fig. 1 and A1)?

We added the sources for the subregions and landcover in the data availability section.

Old: *The datasets used in this study can be found in the following repositories: CRCM5-LE: https://www.climex-project.org/de/datenzugang and ERA-5 based FWI: DOI: 10.24381/cds.0e89c522(31.01.2023).*

New: *The datasets used in this study can be found in the following repositories: CRCM5-LE: https://www.climex-project.org/de/datenzugang, ERA-5 based FWI: DOI: 10.24381/cds.0e89c522 (31.01.2023), sub-regional division: https://www.lfu.bayern.de/natur/naturraeume/index.htm, landcover data from Copernicus Land Monitoring Service: https://land.copernicus.eu/pan-european/corine-land-cover/clc2018.*

95. Figure C1: Why do you use 95th percentile and not 90th percentile as done in the remaining analysis?

Thank you for this remark. We aimed to show more extreme results in the sensitivity analysis and therefore decided to use the 95th percentile.

96. Why number the Figures A1, B1 and C1 instead of A1, A2 and A3 as is normally done?

Thank you for pointing this out. We fixed this overleaf template issue and numbered the Figures in the appendix according to your suggestion.