Cover Letter

May 31, 2024

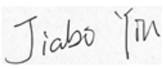
Dear Editor,

We would like to thank you, the editor and the three reviewers for constructive comments and suggestions, which have significantly improved our manuscript (**hess-2023-181**).

Climate change accelerates the water cycle, thus complicating the projection of future streamflow and hydrological droughts. Although machine learning is increasingly employed for hydrological simulations, few studies have used it to project hydrological droughts, not to mention the bivariate risks of drought duration and severity as well as their socioeconomic implications under climate change. We developed a cascade modeling chain to project future bivariate hydrological drought characteristics in 179 catchments over China, using 5 bias-corrected GCM outputs under three shared socioeconomic pathways, five hydrological models and a deep learning model. Our hybrid model also projected substantial GDP and population exposures by increasing bivariate drought risks, suggesting an urgent need to design climate mitigation strategies toward a sustainable development pathway.

In this revision, all the reviewers' concerns have been addressed. Changes made in the revised manuscript are coloured in blue. We sincerely hope you will find the revised version of the paper appropriate for publication. All authors have reviewed the paper and agree to the resubmission of the manuscript. We look forward to hearing from you.

Sincerely yours,



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Reply to Reviewers' comments

Legend

Reviewers' comments

Authors' responses

Direct quotes from the revised manuscript

Editor Lelys Bravo de Guenni:

Thank you to the authors for the revised version of this manuscript and to the reviewers for great comments and suggestions.

Two of the referees have proposed minor revisions which I consider would enhanced even more the quality of your work.

Reviewer 2 has made interesting comments, and suggested some clarifications on several calculations, as for example, the calculation on the sensitivity of several meteorological variables to daily streamflow in lines 376-380.

Reviewer 3 mainly suggests ideas for boosting the paper's presentation quality and writing-up clarity.

I think the reviewers have made a great job in reading the paper in detail and proving useful comments to further improve a manuscript that, not surprisingly, might achieve a good number of citations.

Reply: Dear Prof. Guenni, we would like to thank you for providing these helpful suggestions to improve our manuscript and proceeding the revision of our paper.

We have added explanation on the concerns from reviewer 2 in Section 4.2 as follows:

Over 30% and 38% of stations show the *SH* sensitivity rate of >10% in Western and Northeastern China respectively, indicating the dominance of *SH* in these areas.

Since a station can be attributed to catchments of different sizes, we only consider the largest catchment scales in analysis.

These negative contributions mean enhancement of these two variables will inhibit the generation of streamflow, showing the potential adverse effects of climate change on streamflow generation.

We have improved the expression according to the comments from two reviewers.

Referee #2:

I have no further comment, the manuscript can be published.

Reply: We appreciate your positive evaluation of our manuscript.

Reviewer #3 Gómez-Delgado, Federico:

1. In L.22 and L.40 it is mentioned that climate change (or a warming world) accelerates the (global) water cycle, but this statement is not very precise. The hydrological cycle is a complex phenomenon, and much more elaboration would be required to generally state that, as such, it is accelerated by climate change. It seems to me that Allan et al. (2020), cited here, refer to the expectation of acceleration of global precipitation responses (as warming increases and aerosol forcing decreases), providing quotes on how the warming influence of continued rises in CO2 concentration + declining aerosol cooling is expected to accelerate increases in global precipitation and its extremes as transient climate change progresses. They also point that nonlinear changes in streamflow over multidecadal timescales are expected in some regions as accelerated glacier melt is followed by declining glacier volume. However, these findings do not necessarily support the far-reaching argument for an accelerated global water cycle.

Reply: Dear Prof. Federico, we would like to thank you for providing these insightful suggestions to improve our manuscript. We have rephrased these sentences in the revised Abstract as follows:

Climate change influences the global water cycle and alters the spatiotemporal distribution of hydrological variables, thus complicating the projection of future streamflow and hydrological droughts.

2. L.51: I would suggest reviewing the use of the adjective "uneven" to refer to the distribution of precipitation under the effects of climate change. In principle, precipitation is already an uneven phenomenon, and perhaps this term could be replaced by "rapidly changing", or another that is more precise and refers rather to some process of change than to a static characteristic.

Reply: We have replaced the word in the revised Introduction as follows:

The rapidly changing distribution of precipitation and other meteorological elements under climate change complicates projection of future runoff and drought.

3. L.264-265: It may be worth explaining further what is meant by selecting joint design values according to “the same frequency hypothesis” that has been applied in previous studies.

Reply: We have reshaped the sentence in the revised Section 2.5 as follows:

Previous studies have only selected joint design values according to the same frequency hypothesis that considering two correlated variables follow the same cumulative probability in their distributions, but this approach lacks a statistical basis and poorly describes the physical characteristics of droughts (Yin et al., 2018).

4. L.480 and L.536: Although the word accuracy is commonly used to assess model

performance in many publications, I would suggest double-checking whether it applies here. The performance of a model can be determined in terms of its efficiency (it has good predictive skills that can be tested by measures such as the Nash-Sutcliffe model efficiency coefficient), as well as by uncertainty analysis that allows the model to be characterized in terms of precision (how well each modelled value agrees with each other, i.e., width of confidence intervals constructed around modelled values) and accuracy (how well modelled values agree with "true" values, i.e., percentage of observed test values contained within certain confidence intervals built around modelled values).

Reply: Of course, the accuracy should indicate how well modelled values agree with "true" values, which has been quantified by variations of simulations and observations in this study. The predictive skills of models should be stated as efficiency, which has been quantified by Kling-Gupta efficiency. We have revised expression in Conclusion as follows:

In this study, the hybrid LSTM-constrained hydrological models show high efficiency in studied catchments over China, demonstrating that machine learning can effectively constrain the hydrological simulation.

5. L.353-355: Very interesting indeed is the finding that the severity of droughts measured by the TWS-DSI index is twice that of the hydrological drought, and perhaps in addition to the explanation that the TWS-DSI metric incorporates all vertical water fluxes, thus offering a comprehensive view of shifts in water scarcity, a conceptual discussion could be included around the concepts and differences between meteorological, hydrological and agricultural droughts. In this case, it should be noted that TWS-DSI does not involve aquifer recharge processes, which are fundamental to explain baseflow and, therefore, the hydrological drought in its entire extension, especially for catchments with aquifer recharge and storage capacity that exceeds several times the time step of the analysis.

Reply: We have added discussion in the Section 4.1 as follows:

On the other hand, TWS-DSI can difficultly represent the aquifer recharge processes, which are fundamental physical process of baseflow and the hydrological drought in its entire extension. Therefore, catchments with aquifer recharge and storage capacity will exceed several times the time step of the analysis, enlarging the severity of droughts.

6. L.376-380: How are these percentages calculated? Are the relationships of SH, RH, radiation, etc. established considering the entire tributary catchment area to the respective streamflow gauging site? Otherwise, point-to-point comparisons on the location of gauging sites would be meaningless since streamflow is a catchment-supported process. Also, what does a sensitivity rate >10% mean? Is it significant? Are

the negative contributions of RH and shortwave radiation to streamflow significant? What do you mean by a "pronounced" negative sensitivity of shortwave radiation? If comparisons are made with catchment-support and not point-to-point, as indicated above, it does not seem to make sense that RH has an opposite effect on streamflow at 179 stations. Is this relationship statistically significant?

Reply: These percentages are calculated by dividing stations with a sensitivity rate >10% by the number of total stations. Since a station can be attributed to catchments of different sizes, we only consider the largest catchment scales. The sensitivity rate >10% is used to describe the spatial distribution of SH in Fig. 6c, which is dominant compared with other variables. The negative contributions mean enhancement of these two variables will inhibit the generation of streamflow.

We have rephrased statement and added explanation in Section 4.2 as follows:

Over 30% and 38% of stations show the *SH* sensitivity rate of >10% in Western and Northeastern China respectively, indicating the dominance of *SH* in these areas.

Since a station can be attributed to catchments of different sizes, we only considered the largest catchment scales in analysis.

These negative contributions mean enhancement of these two variables will inhibit the generation of streamflow, showing the potential adverse effects of climate change on streamflow generation.

7. Some acronyms or abbreviations in the document are not defined or appear for the first time without having been defined, such as: GCM (L.27), GDP (L.37), HMs (L.117), ML (L.128/Fig.1), POP (not defined), Tor (L.272 & L.275/Fig.3), KGE (L.391). Consider homogenizing/equating the use of terms such as streamflow/runoff, watershed/catchment, etc. I would strongly advise including a table of abbreviations in the paper.

Reply: We have added a table of abbreviations in the Supplement.

Table S3. Affiliation of acronyms and their full names in this study.

	Acronyms	Full names
Drivers	CMIP6	Coupled Model Intercomparison Project phase Six
	SSP	Shared Socioeconomic Pathways
	ISIMIP3b	Intersectoral Impact Model Intercomparison Project 3b
	GCM	Global Climate Model
	ECMWF	European Center for Medium Weather Forecasting
	ERA5	Fifth generation ECMWF Atmospheric Reanalysis of the global climate
Meteorological	*RH*	Relative Humidity

variables	*SH*	Specific Humidity
	ps	Near surface air pressure
	pr	Precipitation
	srsds	Surface Downwelling Shortwave Radiation
	srlds	Surface Downwelling Longwave Radiation
	T_{2m}	2-meter Temperature
	T_d	Dew-point Temperature
Hydrological models	GR4J	Génie Rural à 4 paramètres Journalier
	HBV	Hydrologiska Byråns Vattenbalansavdelning
	HMETS	Hydrological Model of École de Technologie Supérieure
	SIMHYD	Simple lumped conceptual daily rainfall-runoff
	XAJ	Xinanjiang
Statistical & Machine learning methods	SCE-UA	Shuffled Complex Evolution
	BIC	Bayesian Information Criterion
	MANOVA	Multivariate Analysis of Variance
	RNN	Recurrent Neural Network
	LSTM	Long Short-Term Memory neural network
	RM	Random Forest
	HTM	Hybrid Terrestrial Model
Supporting test data	GRACE	Gravity Recovery and Climate Experiment
	GRACE-FO	GRACE Follow-On
	TWS	Terrestrial Water Storage
Statistical indicators	KGE	Kling-Gupta Efficiency
	JRP	Joint Return Period
Drought indicators	SRI	Standardized Runoff Index
	TWS-DSI	TWS based Drought Severity Index

8. L.98-99: …GCMs outputs under the Coupled Model Intercomparison Project phase six (CMIP6)…

Reply: We have revised accordingly.

9. L.100: …to quantify the sensitivity of daily streamflow to different meteorological variables~~. to daily streamflow~~.

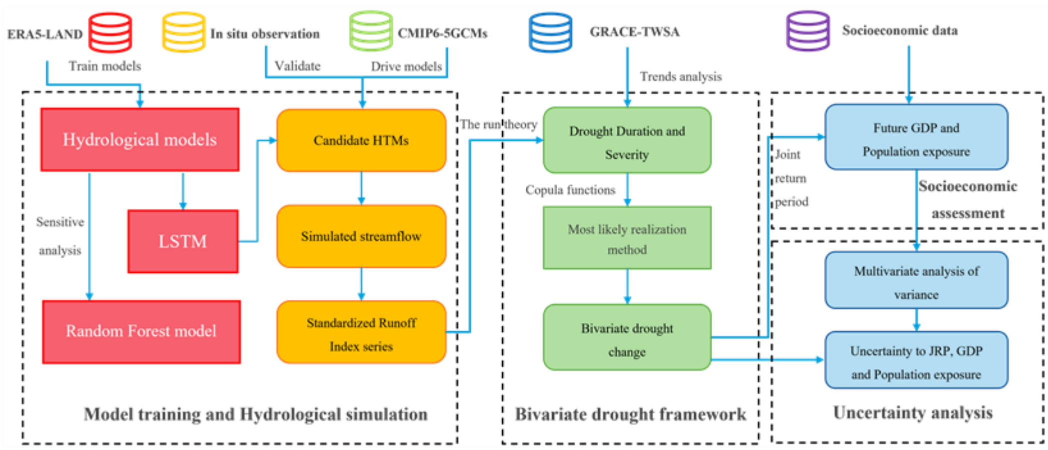
Reply: We have revised accordingly.

10. L.116-117: I would suggest including some reference for the ERA5-Land dataset.

Reply: The reference of the ERA5-Land dataset has been cited in the Section 3.3.

11. L.128/Fig.1: I would advise including the MANOVA analysis as a process here.

Reply: We have revised the Fig.1 by adding the Multivariate analysis of variance (MANOVA) as a process as follows:



12. L.131: Perhaps you could include an introductory paragraph to section 2.1, to explain why you are calculating 2-meter relative and specific humidities.

Reply: We have added explanation in Section 2.1 as follows:

As relative humidity and specific humidity are not directly available from the ERA5-land dataset, we estimate these two variables based on the physical relationship in atmosphere.

13. L.132-133: How is Eq. (1) deriving air temperature T?

Reply: We derived the temperature from ERA5-Land dataset, which is mentioned in Section 3.3. The Eq. (1) is to calculate *RH* and *SH* in Eq. (2) and Eq. (3); therefore, the air temperature here indicates the 2m air temperature and the dew-point temperature.

14. L.135: Constants T_0, e_0, L_0 and R_0 could be further explained

Reply: We have added explanation in Section 2.1 as follows:

where T_0, e_0, L_0 and R_0 are freezing temperature in Kalvin, saturated vapor pressure under freezing temperature, latent heat of vaporization and gas constant of water vapor, with a value of 273.15 K, 611 Pa, 2.5×10^6 J kg^{-1}, 461 J kg^{-1} K^{-1}, respectively;

15. L.137-138 & L.140: Does it imply (since it is not mentioned) that T_{2m}, T_d and ps are available in ERA5?

Reply: These variables were derived from ERA5-Land dataset, which is mentioned in Section 3.3.

16. L.144: … The RF model is used to calculate the sensitivity of runoff to different meteorological variables ~~for runoff~~…

Reply: We have revised accordingly.

17. L.144: I would suggest including some reference for the RF model.

Reply: We have added citations of RF model as follows:

Catani, F., Lagomarsino, D., Segoni, S., and Tofani, V.: Landslide susceptibility estimation by random forests technique: sensitivity and scaling issues, Natural Hazards and Earth System Sciences, 13, 2815–2831, https://doi.org/10.5194/nhess-13-2815-2013, 2013.

18. L.153: But is there any modeling that has been done without all the meteorological variables?

Reply: The selected hydrological models were driven by all the meteorological variables. There is no redundant meteorological variable excluding from the random forest model.

19. L.171: I do not agree with the assessment that a model containing 21 parameters is simple and efficient.

Reply: We have reshaped sentence in Section 2.3.1 as follows:

The HMETS (hydrological model of École de technologie supérieure) model contains 21 parameters and two reservoirs (i.e., the saturated and vadose zones), which is considered to efficiently implement hydrological simulation in limited scales (Martel et al., 2017).

20. L.177-178: I would not consider infiltration as a type of runoff.

Reply: We have revised in Section 2.3.1 as follows:

There are four types of water fluxes from different sources: impervious areas, infiltration, interflow, and groundwater storage (Chiew et al., 2002).

21. L.184-185: Could you provide more references in addition to Hu et al. (2005), or further explanation, to support the statement: "To date, it is widely reported that the XAJ model usually shows the best accuracy in simulating hydrological conditions in China"?

Reply: We have reshaped sentence in Section 2.3.1 as follows:

To date, it is widely reported that the XAJ model usually shows a great performance in simulating hydrological conditions in China.

We have also added citations as follows:

Jiang, T., Chen, Y. D., Xu, C., Chen, X., Chen, X., and Singh, V. P.: Comparison of hydrological impacts of climate change simulated by six hydrological models in the Dongjiang Basin, South China, Journal of Hydrology, 336, 316–333, https://doi.org/10.1016/j.jhydrol.2007.01.010, 2007.

22. L.188: We used the SCE-UA…

Reply: We have revised accordingly.

23. L.189-190: "The most complete 20-year observation period is selected to calibrate five models in each watershed." At this point it might be convenient to specify the modelling time step. Is it a daily time step?

Reply: We have added information of time step in Section 2.3.1 as follows:

The most complete 20-year observation period is selected to calibrate five models in each watershed by a daily time step.

24. L.211/Eq.(9): Aren't the subindexes *oh* and *ox* inverted in W_{oh} and W_{ox}, in relation to the orders employed in Eqs.(5),(6)&(7)?

Reply: We have revised these inverted subindexes accordingly.

25. L.213-214: Are $W_{\cdot f}$, $W_{\cdot i}$, $W_{\cdot \hat{c}}$ and $W_{\cdot o}$ from Eqs(5), (6), (7)&(9) also weights?

Reply: All W with any subindexes are weights of corresponding gates in Eq (5), (6), (7) & (9). We have added explanation in Section 2.3.2 as follows:

$W_{\cdot}$ are the weights, where W_i, $W_{\tilde{c}}$, W_f and W_o are the weights of each gate, $W_{x\cdot}$ are the weights of each gate at time t, $W_{h\cdot}$ are the weights of each gate at the former time $t-1$;

26. L.215: In "…are the cell state of the LSTM and the hidden unit at ~~the~~ time t, respectively; ct−1 and hst−1 at the former…", could you please further explain the term "hidden unit"?

Reply: We have revised accordingly.

27. L.219-220: If in the following statement HMs stand for Hydrological Models: "…The hydrological outputs together with other climate variables are used as inputs to feed the LSTM model (i.e., the HMs are thus constrained by the LSTM)…", then I

would say that the LSTM are the ones that are being constrained by the HMs, and not the other way around.

Reply: We have revised in Section 2.3.2 as follows:

i.e., the LSTM is thus constrained by the HMs

28. L.237: What is $\overline{TWSA_y}$ and where is it used?

Reply: It should be $\overline{TWS_y}$ which is mentioned in Eq (12). We have revised it accordingly.

29. L.247: Why do you calculate two drought indexes, if Table S1 only classifies drought according to DI? If so, what is the SRI for? It is not clearly stated, but it seems like sometimes you use TWS-DSI as DI, and other times you use SRI (also not explicitly stated). Could you clarify when TWS-DSI s used and when SRI? Also, could you please explain in more detail how the maps in Figs. 4 & 5 are calculated?

Reply: The drought index (DI) includes TWSA-DSI and SRI. Therefore, two indexes are simultaneously classified by Table S1. We have reshaped caption of Table S1 in Supplement as follows:

Classification of drought and threshold values of the drought events. Two drought indexes, TWSA-DSI and SRI, both follow this classification.

We have added details to explain Figs 4 and 5 in Section 4.1 as follows:

Based on linear regression and least square method, trends in drought characteristics (i.e., frequency, duration and severity) are estimated by using the GRACE/GRACE-FO dataset and observed runoff across China.

30. L.263: "which contains infinite combinations of values of these two multivariate arrays of variables."

Reply: We have revised accordingly.

31. L.275-277: I suggest including the terms dT, sT, FS, FD and Tor in the legend of Fig.3 and explaining what each of them is.

Reply: We have added explanation at the caption of Fig.3 in Section 2.5 as follows:

d_T and s_T are marginal distribution quantiles for a given probability level T; F_S and F_D are cumulative probability density of duration and severity, respectively. T_{or} is a given probability level under the OR case.

32. L.279: Could you clarify what does it mean the expression "the future period" here?

Reply: We have added specific description in Section 2.5 as follows:

The future socioeconomic exposure after 2020s has directly been defined as ranging from 0 to 100% (Gu et al., 2020a), but dynamically shifting climate risks cannot be represented under this definition, without considering fluctuation in the frequency of hazards.

33. L.280: Could you please clarify why you consider this definition of socioeconomic exposure to be "static"? (At this point you had only mentioned that it varied from 0 to 100%).

Reply: We have rephrased the sentence in Section 2.5 as follows:

The future socioeconomic exposure after 2020s has directly been defined as ranging from 0 to 100% (Gu et al., 2020a), but dynamically shifting climate risks cannot be represented under this definition, without considering fluctuation in the frequency of hazards.

34. L.286: Please note that the conditions Th − Tf > 0 and Th − Tf > 0 are not mutually exclusive!

Reply: We have revised the incorrect symbol in Section 2.5 as follows:

$I(\cdot)$ denotes the controlling function, which is 1 when $T_h - T_f < 0$, or is 0 when $T_h - T_f \geq 0$ is recorded;

35. L.287: "…POP and ~~(~~GDP~~)~~ denote~~s~~ the population and the gross domestic product (in USD) (GDP) of a given catchment in the future climate, respectively…."

Reply: We have revised accordingly.

36. L.302: "…is quantified by the variance of each source ~~by~~ to the total variance…."

Reply: We have revised accordingly.

37. L.311-312: "…with at least 20 years of data ~~are~~ were selected…"

Reply: We have revised accordingly.

38. L.319-320: "…As these three mason solutions are produced at different spatial resolutions, we ~~produce~~ generated blended TWS data based on the…"

Reply: We have revised accordingly.

39. L.324: "…precipitation, temperature, ~~and~~ air pressure, etc. The spatial resolution of

the dataset is 9 km…"

Reply: We have revised accordingly.

40. L.328: I assume that here T_{dew} is the dew-point temperature, which in section 2.1 you first called T_d. Please check consistency.

Reply: It should be T_d. We have revised in whole paper.

41. L.331-332: "…The climate outputs of five GCMs ~~under~~ of the historical scenario and three SSPs (i.e., SSP1-26, SSP3-70, 331 SSP5-85) under CMIP6 are used to represent different climate scenarios…"

Reply: We have revised accordingly.

42. L.344: "4.1 Observed changes in SRI and TWS-DSI based drought"

Reply: We have revised accordingly.

43. L.346: "…employed the TWS-DSI as a supplement…."

Reply: We have revised accordingly.

44. L.348-349: In relation to Figs. 4 & 5, which describe drought trends based on TWS-DSI and SRI, I did not find a clear explanation in the paper on how these maps were calculated, and then several questions arise. For example, is Fig. 4 somehow calculated using Eq.(12) and how, considering that this equation is month-specific? Furthermore, it is indicated that the maps in Fig.4 correspond to the periods 2002-2022, but Fig.5 does not provide any reference time period. Could you include further description about this in the methodology section?

Reply: We have added explanation in Section 4.1 as follows:

Based on linear regression and least square method, Trends in drought characteristics (i.e., frequency, duration and severity) are estimated by using the GRACE/GRACE-FO dataset and observed runoff across China.

We have added time period at the caption of Fig. 5 in Section 4.1 as follows:

Trends in drought frequency, duration and severity from 2002 to 2022 over China. (c), the index of severity is based on the SRI statistic (Eq. 13).

45. L.350: Could you explain in more detail how you concluded that drought hazards have increased in recent decades? If I understood correctly, at this point in the paper

you have only provided spatial trends, not temporal trends.

Reply: We have rephrased this sentence in Section 4.1 as follows:

Overall, the two indexes show similar trends in most catchments, suggesting that drought hazards have increased during 2002-2022.

46. L.350-353: How did you estimate the percentage increase in TWS-DSI droughts? Again, maybe you produced monthly maps using Eq.(12) and analyzed temporal trends for specific locations on the map? In Figs. 4 & 5 I cannot distinguish any catchment. It looks like a normal grid-based GIS analysis, rather than a catchment-based analysis. Are you using streamflow measurements to draw conclusions about hydrological droughts?

Reply: It should be grid here. The percentage is calculated by gridded results. We have reshaped in Section 4.1 as follows:

TWS-DSI droughts have increased in 54% of grids, which are mainly located in the Qinghai-Tibet Plateau, the North China Plain and the northwestern Xinjiang Province.

47. L.364/Fig.4: It might be convenient to explain the unit of measurement "/10 years" for drought trends, since although it is a little more intuitive in the case of frequency (count of events in a 10-year period) and duration (number of months over a 10-year period), in the case of the TWSA-DSI index this might be less obvious.

Reply: The TWSA-DSI represents the severity of drought. Although the severity has less obvious trends, we also analyzed it in the unit of "/10 years" consisting with other drought characteristics for showing changes of drought condition.

48. L.381: When you say "This" do you mean increasing or decreasing RH-streamflow relationships?

Reply: Yes, **"**This" refers to the finding about RH-streamflow relationships mentioned above.

49. L.386: Could you refer to the different maps of Fig.6 here?

Reply: We have added this information in Section 4.2 as follows:

(Fig. 6i and 6f)

50. L.389/Fig. 6: Why do you use a thin plate smoothing spline method to interpolate your data, rather than more data-driven techniques such as directional kriging? Also, when you say point-based station data, are you referring to your hydrological stations?

Reply: The directional kriging method sounds a great alternative. We will consider this in further studies.

The point-based station data refers to observation dataset mentioned in Section 3.1. It includes hydrological variables.

51. L.391/Fig. 7: This is a very interesting figure, but it needs better explanation. For example, the legend of Fig. 7a should indicate that the regions colored according to the best performing models are the study catchments. Fig. 7b should perhaps include labels for the different categories/models, since the use of only colors is a little ambiguous.

Reply: We have added explanation in the caption of Fig.7 as follows:

(a), The best-performing model with the highest KGE value. The catchments are colored according to the best performing models.

52. L.418: Higher carbon emissions and other climate forcing factors should be listed and explained (at least briefly) in the methodology section.

Reply: We have added explanation in Section 3.4 as follows:

Generally, the SSP5-85 configured the highest carbon emission and human interference with the natural environment. The SSP3-70 and the SSP1-26 have progressively conservative changes to represent climate change resulting from different levels of human activity.

53. L.434-436: A better description could be provided for terms such as IPSL_CM6A_LR. Perhaps you could also better explain in the methodological section the geo-statistics behind terms like median relative change of severity.

Reply: We have reshaped sentence in Section 4.3 as follows:

The median relative change of severity based on the IPSL-CM6A-LR under SSP3-70 are 30%, and 22% of catchments have a relative change over 200%, representing the most severe case of drought evolution.

54. L.438: Regarding the finding of substantial spatial heterogeneity of drought across China: Are the study catchments distributed homogeneously and equally throughout the country? Considering geospatial sampling techniques, a homogeneous density of catchments may be necessary to reach such a conclusion, in a strict sense.

Reply: The studied catchments cover all the nine major basins within China and basically satisfy a homogeneous spatial distribution. The density of studied catchments is also consistent with the spatial density distribution of river networks in China.

We have added explanation in Section 5.2 as follows:

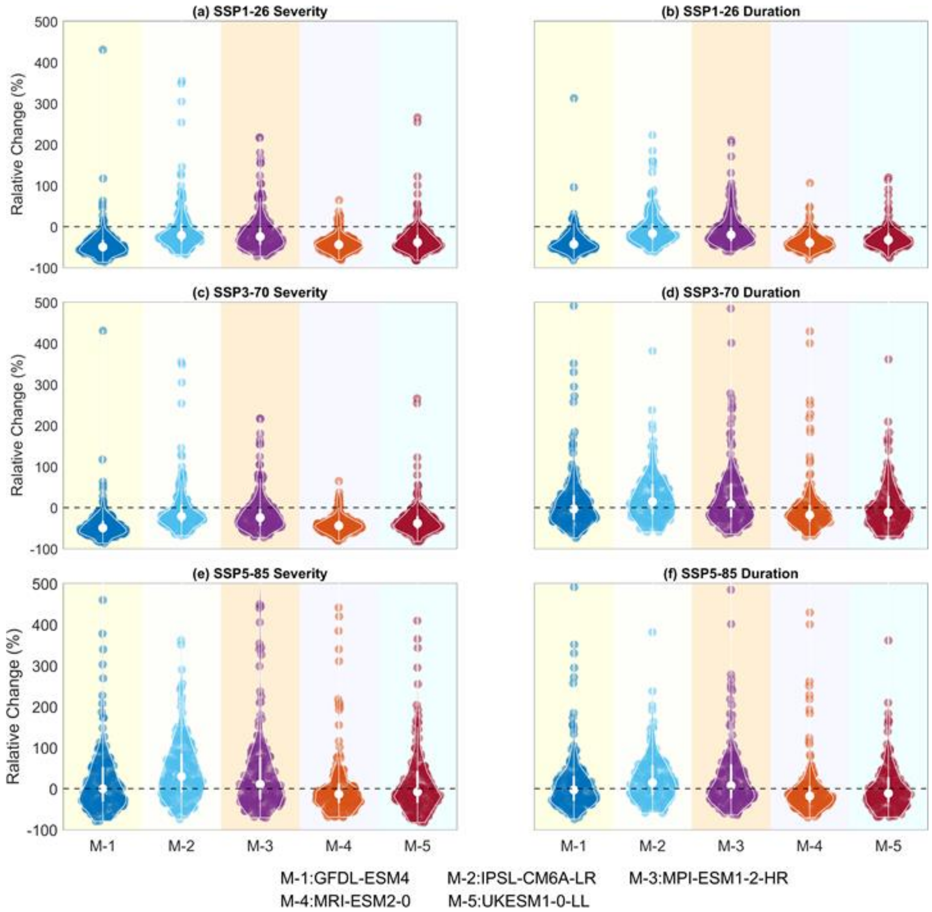
Although the catchments gathered in this study cover nine major watersheds in China, there is still a requirement for streamflow data with a more uniform spatial density. Considering geospatial sampling techniques, a homogeneous density of catchments is significant to reveal the spatial distribution of drought.

55. L.440: "…intensification as a result of global warming…"

Reply: We have revised accordingly.

56. L.441/Fig. 10: Color legends seem to be missing for the five GCMs.

Reply: We have revised Fig.10 as follows:



57. L.445: "…drought severity and duration, we used a Copula…"

Reply: We have revised accordingly.

58. L.448: "…The medians of the projected future JRP are 38.78 years, 14.52 years and 19.24 years under…"

Reply: We have revised accordingly.

59. L.449: "…under SSP3-70 and SSP-5-85…"

Reply: We have revised accordingly.

60. L.455: The use of bivariate drought analysis can "synthesize", or rather "amplify" the individual effects of two drought characteristics?

Reply: We have revised accordingly.

61. L.457: Are the figures contained in Figure 12 absolute or marginal? If they are absolute, you should also present the relative change with respect to the period of reference.

Reply: The Fig.12 shows the exposure of GDP and population in the 2071-2100 time period, which is defined by changes in drought frequency and demographic and economic fundamental of catchments stated in Section 2.5. So, the exposure has considered the relative change with respect to the period of reference.

62. L.479: It is very surprising to me that HTM is the main source of uncertainty, since this analysis also includes SSPs, which, being products of socioeconomic studies and models, I would think involve much higher levels of uncertainty. Does this analysis quantify uncertainties, or rather the variance explained by each of the factors?

Reply: The uncertainty analysis in this study, as explained in Section 2, based on the MNOVA and quantified the contribution of data from different sources to the variance of the results. In other words, it can be considered as the uncertainty of each component in the cascade model chain.

63. L.505: Is "interference" the right term? Perhaps "intervention" would be more appropriate. In addition to that, how about other high impacting factors such as political and economic crises, changes in culture and expectations of the populations, and wars and other conflicts?

Reply: The "intervention" is more appropriate, we have revised accordingly. The listed social impacting factors absolutely have a high potential influence, but beyond the attention of this study. We would like to consider these aspects in future studies.

64. L.545: In line with previous comments, it really seems that your uncertainty analysis is returning rather explained variance, and not induced uncertainty.

Reply: The uncertainty analysis in this study, as explained in Section 2, based on the

MNOVA and quantified the contribution of data from different sources to the variance of the results. In other words, it can be considered as the uncertainty of each component in the cascade model chain.

Reviewer #4:

1. Starting at the highest level, as most diagrams concern China, it feels like this country domain should be mentioned in the title. Then, in the Abstract, please make it much clearer what some of the terminology refers to. For instance, the extensive use of "bivariate" – is this referring to the two variables (i.e. "bi") of drought duration and severity? Or maybe water storage and runoff? Although points like this are made clearer in the paper, the Abstract should be as complete as possible for people to understand the analysis. I am sure this can be achieved without making the Abstract excessively long.

Reply: We have rephrased the title of this study as follows:

Machine learning-constrained projection of bivariate hydrological drought magnitudes and socioeconomic risks over China

We have rephrased sentence in Abstract as follows:

Although machine learning is increasingly employed for hydrological simulations, few studies have used it to project hydrological droughts, not to mention the bivariate drought risks, referring to drought duration and severity, as well as their socioeconomic effects under climate change.

2. The paper has a particularly large number of acronyms. A reader trying to understand the manuscript will be quickly attracted to the schematic of Figure 1, which sets out the methodological components. However, it is then necessary to work back through the manuscript to discover all of the acronyms. Would the authors like to consider a table? In the Table there would be different sets of rows explaining all acronyms of (1) drivers (ECMWF, CMIP6, SSP), (2) hydrological models (SIMHYD etc), (3) key meteorological variables (RH, srlds, srsds), (4) statistical / AI methods used (LTSM….), (5) Supporting test data (GRACE…), (6) statistics of performance (e.g. KGE). I reckon there are at least 30 acronyms in this paper, and a single point where all are listed would be extremely helpful to the reader.

Reply: We have added a table of acronyms in Supplement as follows:

Table S1. Affiliation of acronyms and their full names in this study.

	Acronyms	Full names
Drivers	CMIP6	Coupled Model Intercomparison Project phase Six

	SSP	Shared Socioeconomic Pathways
	ISIMIP3b	Intersectoral Impact Model Intercomparison Project 3b
	GCM	Global Climate Model
	ECMWF	European Center for Medium Weather Forecasting
	ERA5	Fifth generation ECMWF Atmospheric Reanalysis of the global climate
Meteorological variables	*RH*	Relative Humidity
	SH	Specific Humidity
	ps	Near surface air pressure
	pr	Precipitation
	srsds	Surface Downwelling Shortwave Radiation
	srlds	Surface Downwelling Longwave Radiation
	T_{2m}	2-meter Temperature
	T_d	Dew-point Temperature
Hydrological models	GR4J	Génie Rural à 4 paramètres Journalier
	HBV	Hydrologiska Byråns Vattenbalansavdelning
	HMETS	Hydrological Model of École de Technologie Supérieure
	SIMHYD	Simple lumped conceptual daily rainfall-runoff
	XAJ	Xinanjiang
Statistical & Machine learning methods	SCE-UA	Shuffled Complex Evolution
	BIC	Bayesian Information Criterion
	MANOVA	Multivariate Analysis of Variance
	RNN	Recurrent Neural Network
	LSTM	Long Short-Term Memory neural network
	RM	Random Forest
	HTM	Hybrid Terrestrial Model
Supporting test data	GRACE	Gravity Recovery and Climate Experiment
	GRACE-FO	GRACE Follow-On
	TWS	Terrestrial Water Storage
Statistical indicators	KGE	Kling-Gupta Efficiency
	JRP	Joint Return Period
Drought indicators	SRI	Standardized Runoff Index
	TWS-DSI	TWS based Drought Severity Index

3. Please make the captions complete, especially as these days, people often extract single diagrams from paper to give talks. For instance, under Figure 5, at the minimum, please state the period over which the data applies. Maybe cite back to Eqn (13) for the SRI statistic. Remove any ambiguity, e.g. that SRI statistic only applies to panel (c). (for instance, write:. "..and severity over China. The index of severity (panel (c)) is based on the SRI statistic (Eqn 13)".

Reply: We have revised the caption of Fig.5 as follows:

Figure 5. Trends in drought frequency, duration and severity from 2002 to 2022 over China. (c), the index of severity is based on the SRI statistic (Eq. 13).

4. Check in Figure 1 that the mention of CMIP6 is in the correct place. As it stands, being to the left of the diagram (which is more about the contemporary period), it gives the impression CMIP6 drivers may be somehow bias corrected for their projections of the historical period? i.e. part of the data assimilation. Is this the intended meaning?

Reply: The GCM outputs in CMIP6 has a time span from 1850 to 2100, including both the historical period (1985-2014) and the future period (2030), which is mentioned in Section 3.4. The biased corrected historical data is used to drive the models, which can be treated as a part of the data assimilation.

5. Please provide a little more background detail on the Copulus method, even if only a more definitive sentence at the beginning of Section 2.5. Please make clearer how this statistic is used in future projections. Is it to interpret combined fitted hydrological models with climate drivers – i.e. interpret future droughts only? Or is the statistic used more deeply, linking drivers with severity and duration – and then only using the ESM-based drivers. (In other words, it is an additional predictor to using conceptual hydrological models).

Reply: We have added explanation of Copula functions in Section 2.5 as follows:

To integrate the assessment of drought change arising from the duration and severity under climate change, we employed a Copula framework by constructing joint probability distribution of two variables.

6. Please make sure "headline" findings jump out of the paper. The most important feature of the manuscript is the Abstract sentence "By the late 21st Century, bivariate drought risk is projected to double over 60% of catchments". This summary needs to really jump out in the manuscript, and the reader taken to the key plot that illustrates this. For example, thinking of a policymaker who might not be interested in all of the details, but recognises the importance of raised drought risk. And again, even here in the Abstract, please try and help the reader. Would it be better to write "…bivariate

drought risk (which is a merged statistic capturing both drought duration and intensity)….” Also, to avoid any ambiguity, state what SSP scenario this refers to. Please check throughout the paper there are no points where misunderstanding could easily occur, and that can be resolved with a little more clarity and detail.

Reply: We have reshaped findings in Abstract as follows:

By the late 21st century, bivariate drought risk is projected to double over 60% of catchments mainly located in Southwest China under SSP5-85, which shows the increase of drought duration and severity.