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Comment on nhess-2021-114

Anonymous Referee #2

Referee comment on "Stochastic system dynamics modelling for climate change water scarcity assessment of a reservoir in the Italian Alps" by Stefano Terzi et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2021-114-RC2>, 2021

The manuscript describes the application of regression models to simulate storage volume and outflow of a reservoir using reservoir inflow time series produced by a hydrologic model. Results are presented showing the reproduction of observed reservoir volume and outflow monthly time series, as well as projected changes in volume and outflow under two climate change scenarios.

The manuscript is well structured. However, the following major issues affect the overall quality of the study:

- Key parts of the methodology are not explained in a clear manner: in particular, some formulas are only partly defined, while others are written following a software package syntax and thus not as understandable mathematical expressions (see specific comments 17, 18 and 19).
- It is not clear whether the modelling approach guarantees the closure of the reservoir water balance, due to insufficient descriptions of methodology and input data, and to the lack of discussion of relevant results (see general comment F).
- There appears to be a mismatch between the complex systems approach (System Dynamics Modelling; SDM) the authors claim to use and the actual implementation consisting of regression models (see general comments A, C, D and E).
- The stochastic implementation of SDM is presented as a novelty. However, the relevant methodological description is unclear, and the relevant results are only partly discussed (see general comments B, G and H). The authors do not provide sufficient elements to evaluate the soundness of the method and the information provided by the approach.
- The Conclusions section (p 19-20) focuses on future work/applications, while the results of this study are mentioned very briefly on p 19, l 394. The conclusions should summarise and highlight the knowledge contributed by the study.

In light of these major issues affecting the scientific quality of the study, I recommend the rejection of the manuscript.

GENERAL COMMENTS

A. The authors state that an innovative stochastic System Dynamics Modelling (SDM) approach is applied to simulate water stored and turbinated in the S. Giustina reservoir under current/past and projected climate conditions (p 1, l 18-20). SDM is described as "an approach used in the field of complex system behaviour" useful for analysing large systems and interactions between subsystems (p 3, l 69-79). In presenting the case study, emphasis is put on the importance of achieving "a better understanding of the complex interactions in the S.Giustina water management", and it is stated that "SDM can help to depict the S.Giustina reservoir dynamics and its responses to future pressures, including climate change and anthropogenic factors" (p 5, l 127-132). However, the SDM approach simulates reservoir volume and outflow using reservoir inflow produced by the GeoTransf hydrologic model as input (Fig. 3 box 3, p 8, l 168, and l 181-184). Its implementation consists of two regression models simulating reservoir volume as a function of inflow and month (Eq. 1) and outflow as a function of inflow, past volume and month (Eq. 2). This simple approach seems not to correspond to the provided SDM definitions.

B. The authors argue that the novelty of their approach lies in the application of SDM in a probabilistic/stochastic framework, in order to account for the uncertainties of future climate scenarios (p 2, l 51-57; p 4, l 94-95; and p 8, l 174-176). However, the stochastic (Monte Carlo) approach is given a very short and unclear description at the end of Sect. 3 "Material and methods" (see specific comment 21). Moreover in the results section, the outcomes of the stochastic sampling shown in Fig. 6 and 8 are either not discussed or only mentioned briefly (see general comments G and H).

C. The role of anthropogenic pressures is mentioned in several parts of the manuscript (e.g. p 2, l 61; p 4-5, l 108-116). The use of water demand/withdrawal data is mentioned briefly only on p 11, l 235 ("GeoTransf simulations considered unchanged maximum water withdrawals in the Noce catchment") and on p 18, l 374 (similar statement). However, these data are not described nor referenced in the materials and methods section. Most importantly, their role in the modelling framework is not described at all. The conclusions read that "Future model expansions include water demand from multiple human activities" (p 19, l 401-404). The authors should clarify whether non-hydropower water abstractions are simulated and, if applicable, describe how these are included in the modelling framework.

D. SDM components (stocks, flows, converters, connectors) are introduced in Sect. 1. However, the terms "stock", "converter" and "connector" do not appear in the rest of the manuscript. These terms should be used when describing the methodology (throughout Sect. 3 and in particular in Fig. 2) to clarify why SDM is relevant for this study. Moreover, the definitions and explanatory examples of "converters" and "connectors" are unclear (p 3, l 71-73). Evaporation rates, which are used as examples of "converters", may also fulfil the definition of "flows" ("variable's rate of change"). Instead, should the parameters of evaporation equations be regarded as "converters"? Otherwise, are "flows" fluxes of material between "stocks" simulated by the system (e.g. reservoirs and rivers) rather than fluxes leaving the system (e.g. evapotranspiration)? The example of "linking of temperature variations to evaporation rate" may indicate that "connectors" act in the

opposite direction of "converters", but it is not clear how these two categories are qualitatively different. These definitions should be clarified, especially because the book by Sterman et al. (2000) is not an open-access publication.

E. The description of the "causal loop diagram" (Sect. 3.1) is too brief and needs to be rewritten as the roles of several elements described in the text and shown in Fig. 2 are not understandable. In general, the authors should clarify to which extent the arrows in the diagram correspond to the flow of information between models shown in Fig. 3: if these representations overlap, then Sect. 3.1 and 3.2. could be merged into a single section. More specifically, in what ways do "soil" and "land-use" represent system vulnerability? Also, it is not clear how the "month of the year", "water turbinated" and "reservoir volume" represent exposure. Finally, the "critical states" are undefined.

F. In Table 3 (Results), average outflows are between 8% and 9% smaller than average inflows. These discrepancies, which are not discussed in the manuscript, may imply (i) that the closure of the reservoir water balance is not guaranteed, as over long time periods total inflow and outflow should be very close (except a change in storage that would be very small compared to the cumulated flows); or (ii) that the difference between inflow and outflow is lost via evaporation, percolation, non-turbinated releases, or abstracted for other uses than hydropower. However, none of the latter losses/uses are mentioned in the manuscript. The combined use of Eq. 1 and 2 (or the corresponding models #3 and #4 in Table 2) to model volume V and outflow Q_{out} may violate the reservoir water balance: once $Q_{out}(t)$ is calculated with Eq. 1, as $V(t-1)$ and inflow $Q_{in}(t)$ are also known from previous calculation (or initialisation) and input data, then $V(t)$ should be obtained by applying the reservoir mass balance: $V(t) = V(t-1) + Q_{in}(t) - Q_{out}(t)$, where $V(t)$ is the volume at the end of month t , and $Q_{in}(t)$ and $Q_{out}(t)$ are average inflow and outflow during month t . The authors should clarify how the closure of the reservoir water balance is guaranteed, as this is a prerequisite for a reservoir simulation model: to do so, they may replace the expressions in Table 2 with understandable mathematical equations, and define the "f" functions in Eq. 1 and 2 (see specific comments 17-19). If the water balance closure is not guaranteed by this approach, the reservoir simulation model should be revised.

G. The presentation of results needs to be improved. For example, Fig. 6 is not discussed in the text: it is referenced only once on p 13, l 272 while discussing the average values presented in Table 3. Figure 6 contains 12 time series plots and seems to use the grey shaded area to show stochasticity, which is presented as a major component of the methodology. Therefore, Fig. 6 should be discussed extensively, focusing in particular on the value of the information provided by the stochastic approach for the water scarcity risk assessment. Moreover, the confidence interval is only mentioned in the caption of Fig. 6 without specifying the confidence level nor the assumptions underlying its calculation, thus hindering the interpretation of the plots. Also, the apparently smoothed lines in volume and outflow plots (Fig. 6) are not defined nor discussed in any part of the manuscript. For other examples, see specific comments 22-27.

H. The emphasis on the probabilistic/stochastic framework is not followed by a thorough discussion of the uncertainty intervals derived from the Monte Carlo sampling. For example, Fig. 7 and 9 only show average variations. Moreover, although some outcomes

of the stochastic approach are shown in Fig. 6 (grey areas) and 8 (box-plots), these are either not discussed at all (Fig. 6) or mentioned only briefly (p 16, l 310-315) in the manuscript.

SPECIFIC COMMENTS

1. p 1, l 20: Based on the information provided in Sect. 3.2, the sentence "The integration of outputs from climate change simulations as well as from a hydrological model and statistical models into the SDM is a quick and effective tool to simulate past and future water availability and demand conditions." should be replaced by a more informative statement on how the components of the model tool-chain are assembled, e.g. explaining briefly the input/output flow between model components.

2. p 1, l 21-23: Add simulation periods to "short-term", "long-term" and "baseline".

3. p 1, l 25 and throughout the text: Replace "quantiles" with "percentiles" when using values within 1 and 100.

4. p 1, l 27-29: This statement seems not to be supported by the results, as no other water-demanding sector than hydropower is considered in this study. Moreover, the impact on hydropower production is not quantified, as model output includes reservoir storage volume and outflow but not energy production.

5. p 3, l 89: If possible a scientific publication should be referenced when asserting the "reduced accuracy in comparison with dedicated physically based models".

6. p 4, l 101-105: As detailed data regarding reservoir storage and environmental flow requirements are provided later (p 5, l 126), adding the following information about the Noce river would help understanding the context: catchment area, average discharge, and sectoral or aggregate water demand/abstraction volumes. In particular, water demand/abstraction information would be of crucial importance in light of the mentioned water scarcity events/issues (p 5, l 108-116) and because the S. Giustina reservoir provides water for irrigation and other downstream uses (p 5, l 122-124).

7. p 5, l 124-126: The statement on stakeholder concerns about "unused" water releases should be supported with some evidence. Otherwise it should be removed.

8. Figure 2: Clarify whether precipitation is partitioned into snowfall and rainfall as a function of temperature. If so, then rainfall should be added to the diagram. Consider whether the distinction between "Climate-related hazards" and "Hydrological-related hazards" is necessary. The caption seems to be truncated.

9. Figure 3, legend: The terms "external component" and "internal component" are not discussed in the text: explain with respect to which other elements they are to be considered external/internal. Also, clarify the meaning of "Field/sector" and whether its graphical identifying feature is the dashed line or the fill colour.

10. Figure 3 and p 7, l 160-161: Provide the source of weather station data. Also, describe briefly the bias correction methodology, as "Quantile mapping" is mentioned in Fig. 3 but not in the text.

11. p 7, 8 and Fig. 3: The baseline simulation period needs to be clarified. From Fig. 3 and p 7, l 160-161, it seems that weather station data (period 1971-2005) are used to bias-correct 1975-2005 COSMO-CLM precipitation and temperature (l 177), which are in turn used as GeoTransf input. However, Table 1 reports that "Geotransf_inflows" cover the period 1981-2010, while in Fig. 3 the hydrological model baseline period is 1980-2010 and the SDM baseline periods are 1999-2004 and 2009-2017. A more detailed description of the input/output flow between model components could clarify these apparent inconsistencies.

12. p 8, l 165-180: To understand the application of the GeoTransf model, further details are needed about the input data. In Fig. 3, glacier extension, land use and soil data seem to be model input, but they are either not mentioned or not explained in the text. Add the sources of these datasets. Clarify in particular whether glacier extension is an input or an output variable, as the following sentence seems to imply that glacier state is a model output: "GeoTransf provides a description of the hydrological dynamics within the Noce alpine river catchment, assessing variations in water contributions coming from climate change effects in terms of temperature, soil moisture, glaciers, snow and rainfall" (p 8, l 170-172).

13. p 7, 8: Add the temporal resolution(s) of input data and model output.

14. p 8, l 166-168: Clarify what is meant by "these blocks" (temperature and precipitation data? GeoTransf output?) and provide references in addition to the OrientGate project website. Check if the latter should be updated to <http://m.orientgateproject.org/>.

15. p 8, l 182-184: Clarify this sentence. The meaning of "covers" is unclear: are reservoir volume and outflow outputs of SDM? What are the "critical conditions" to which volume and outflow are exposed?

16. p 9, l 187-189: The following input variables are mentioned here for the first time: "hydroelectric energy market price", "water outflows from an upstream dam reservoir". They should be introduced in Sect. 3.2 ("System dynamics modelling set-up and input data") and integrated into the Fig. 3 flow chart. Now they are mentioned briefly in the Supplement without any further detail than units, temporal coverage and source (missing for the upstream dam reservoir outflow).

17. Table 2: Replace the formulas with mathematical expressions. The current expressions are not understandable. The reader should not be required to learn the R syntax in order to understand what seems to be regression models. This notation may lead to ambiguous interpretations of, for instance, the "lag" operator (how long is the lag?), the "1|month" expression, the "s" function, and so on. Moreover, model parameters are visible in these formulas. The same applies for Table S2 and S3 in the Supplement.

18. Section 3.3.1: Equation (1) seems to suggest that the reservoir volume at monthly time step t is function of inflow at t and month of the year. Why is not $V(t)$ function of $V(t-1)$ as well? This may seem an incorrect representation of the reservoir water balance and should be clarified. Function "f" is not defined, which makes Equation (1) not fully understandable. Also, in the following sentence the expression "grouping effect" should be clarified: "As a random effect, the month of the year was selected (month) for its grouping effect on the recurrent water volume variations on a monthly scale".

19. Section 3.3.2: Function "f" is undefined, making Eq. (2) not understandable. Moreover, it seems that a flow (Q_{in}) and a volume (V) are summed together, thus potentially violating the dimensional consistency of the equation.

20. Section 3.4: The authors need to state explicitly which model parameters are calibrated. The description of the "forward time-window approach" needs to be improved. It seems that the whole procedure consists of a sequence of 59 successive model calibrations, each based on an increasingly longer training dataset. However, it is unclear how information from iteration $n-1$ is used in iteration n . Also, what algorithm is used to search the parameter space?

21. p 11, l 246-249: The description of the Monte Carlo approach needs to be clarified. The authors should clarify how the sampled volume time series are built, explaining in particular how the relevant characteristics of simulated volume are preserved (e.g. autocorrelation and seasonality).

22. p 11, l 255 and Fig. 4: Correcting simulated volume values to the maximum storage hides model overestimations. Moreover, the time steps of these corrections are not shown in Fig. 4, thus not allowing the reader to distinguish between simulated and corrected values. For transparency and to allow the evaluation of the modelling approach, volume

values greater than the maximum allowed for flood should be shown in Fig. 4.

23. p 12, l 263-266: The sentence "[...] low values of reservoir volume compared to the monthly inflow values" seems incorrect: the active storage volume is 152.4 Mm³ (p 5, l 121) and the average baseline monthly inflow is 71.38 Mm³/month (Table 3). Moreover, if storage were small compared to monthly inflow, then the analysis should be carried out using a sub-monthly time scale (e.g. daily), as the reservoir would not be able to significantly regulate the flow at monthly scale.

24. Figure 5: What are the horizontal dashed lines?

25. p 13, l 273-275: 2021-2050 RCP8.5 average inflow is 7.5% smaller than the baseline, while precipitation is 1.4% larger (Table 3). Is this due to increased evapotranspiration caused by the relatively large temperature increase? The authors should discuss the impact of increasing temperatures on local hydrology.

26. Table 3: the " Δ [%]" values seem wrong in several cases, e.g. the relative difference between 2021-2050 RCP4.5 and baseline temperature is 27.6% and not 0.5%. Similar errors occur for all other temperature changes. See also p 15, l 289.

27. p 17-18, l 344-346: Define "slow onset conditions of water availability" in the context of the presented results.

28. p 18, l 356-357: The sentence "At the same time, high volume events decrease [...]" seems to be redundant with the previous one, i.e. "[...] increasing number of future water scarcity conditions of high and low volumes stored".

29. p 18, l 369: The sentence "Accounting for the GeoTransf application means relying on a very accurate water evaluation within the catchment" should be supported with some evidence/references. Moreover, the expression "water evaluation" seems ambiguous: the authors should mention what specific characteristics of the water system are reproduced accurately (e.g. river discharge?).

30. p 18, l 376-377: Explain how the precipitation data used to force the GeoTransf model may miss intense precipitation episodes. This is not clear from the presentations of input data and results.

31. p 19, l 378: It seems that the authors refer to the reservoir volume time series length

(14 years) as a factor that limits the model predictive performance. Model performance was evaluated using RMSE and R2 (Sect. 4.1). However, it is not clear how time series length negatively affects the goodness-of-fit of the regression models (Table 2). In contrast, small training samples may lead to over-fitting, which then can cause poor model ability to simulate time periods over which the parameters were not calibrated.

32. p 19, l 385-387: Was the monthly time resolution chosen because of the lack of sub-monthly (e.g. daily) data or following other considerations (as it seems from the brief mention on p 9, l 193-195)? This choice should be discussed in detail in the materials and methods section as it has a significant impact on the modelling framework, e.g. the authors have not used the energy price as a regressor when modelling outflow.