Reply to referee 2

We would like to thank you for the detailed and valuable feedback on this manuscript. We think that the suggested revisions based on the Referee's comments will certainly improve the article. Please find our responses (in blue) to the main points raised (shown in black) below.

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.

→ Thanks, we see the need to better match the description and expectations in the initial part with the actual implementation. Indeed our application tackles one portion of multiple issues related to water management and scarcity. However, we also recognize the need to provide a wider context and reference to other existing SDM applications incorporating several modular components and leading to (more) complex systems analysis (Duran-Encalada et al., 2017; Gohari et al., 2017; Sušnik et al., 2018, Malard et al., 2017; Stave, 2010; Davies and Simonovic, 2011). In the revised version, we can soften some of the initial expectations, while referring to novel SDM applications and its possible modular integration in a wider modelling context.

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).

→ Thank you for pointing that out. Similarly to the comment provided by Reviewer 1, we acknowledge the need of improving the description of the MonteCarlo approach in Section 3 as well as expand the results coming from its application. We will implement the suggestion by further describing in more detail the Monte-Carlo/stochastic procedure in the revised version.

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.

→ Thanks for your comment. We see the need of improving the reference to the conservative assumption of non-hydropower water abstractions upstream of the reservoir considered at their maximum value (according to the provincial licensed withdrawals, which data is not publicly available). We can add this information in the material and methods section referring to it within the text and adding references in the modelling framework section. In general, the assumption of constant maximum withdrawals included in the GeoTransf simulations provided a conservative estimate for the aim of looking at changes in the S.Giustina volume and water turbined due to water flow variations from climate change effects.

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.

→ Thanks for pointing that out. We will add reference to the SDM terms in figure 3, where they have been reported, and in the text describing figure 3, especially in the last part focusing on box #3 (l 174-180). The comment on the use of explanatory examples was also raised by Reviewer #1 and we proposed a different example: "converters - parameters influencing the flow rates (e.g. temperature variable acting to alter evaporation from a water body) and (iv) connectors – as arrows transferring information within the model (e.g. linking the monthly effects on reservoirs' water discharges) (Sterman et al., 2000)." We trust the new text is clearer in its meaning of the terms, offering better distinction between flows and converters.

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 turbined" and "reservoir volume" represent exposure. Finally, the "critical states" are undefined.

→ Thanks for raising this point. We see the need to provide additional information on the factors in the Fig2 description. However, we also recognise its importance as a first expert-based conceptualization feeding the following model set up as it is also carried out in other SDM applications. For this reason, we will integrate its description and relation to Fig.3.

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 Qout may violate the reservoir water balance: once Qout(t) is calculated with Eq. 1, as V(t-1) and inflow Qin(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) + Qin(t) - Qout(t), where V(t) is the volume at the end of month t, and Qin(t) and Qout(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.

→ Thanks for raising this concern. The use of the two regression models for volume stored and turbined water accounted for the water balance equation variables and dependencies on water inflow and volume from the previous time step. This provided a better fitting of both variables on past observations compared to the use of the predicted turbined water into the water balance equation to calculate the volume stored. However, we recognize the need of satisfying the water balance at each time step. We can implement it to simulate the S.Giustina volume (although having a lower performance to replicate past values) and correctly report it in the revised version of this manuscript.

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.

→ Thanks for the feedback. We acknowledge the need to better describe and present the results covering all the reported information. In agreement with the comment from Reviewer #1, we can integrate Figure 6 with better caption information and modify the figure in order to replace single projected values with aggregated time slices.

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.

→ We acknowledge the need to improve the discussion on the MonteCarlo sampling application with reference to Figure 8 as this was also pointed out by Reviewer #1. While Figure 7 and 9 reported averaged conditions on a monthly basis, but are not directly connected to the MonteCarlo sampling, Figure 8 aims to provide information on the results of the MonteCarlo sampling and we can further discuss it in the revised version.

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.

→ Thanks and we can briefly clarify the model chain statement.

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

→ Thanks for pointing that out. We agree and will add them.

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

→ Thanks and we will modify them throughout the text.

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.

→ Thanks for this comment, but this last part of the abstract does not claim to be supported by results. We will rephrase the statement to make this distinction clearer since it provides an outlook on possible future upscaling and application of SDM to include other water demanding sectors and further characterize water scarcity conditions.

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".

→ Thanks, we can rephrase the sentence to clarify that SDMs are not meant as replacements for dedicated physically-based models in a given field, but instead are useful in exploring and connecting disparate fields (e.g. Haung et al, 2011).

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).

→ Thanks, we can add information on the catchment area (1367 km^2) and the aggregated sectorial demand for the whole catchment as reported in the following table (Percentages subdivision of the licensed water withdrawals per sector (year 2017) where large hydropower plants (with average nominal capacity greater than 3 MW) water withdrawals are excluded (Data source: Provincial Agency for Water and Energy)):

Sector	**Percentage [%]**
Agriculture	16.39
Other	0.15
Domestic	3.98
Hydropower	77.81
Industrial	0.49
Snowmaking	0.28
Fish farming	0.90

The high percentage value of 77.81% of licensed withdrawals for hydropower only accounts for the small run-of-the-river plants, hence providing information on the very high use of water for hydropower purposes within the Noce catchment where the S.Giustina dam is the largest reservoir.

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.

→ Thank you for the feedback and this is a critical point we agree should only be kept if supported by references.

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.

→ Thanks, we can integrate the caption and add rainfall to the CLD as precipitation was partitioned into snowfall and rainfall in the GeoTransf application.

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.

→Thanks for pointing that out. We can add an explanation of the external and internal components division in the 3.2 section in order to clarify those components that were already developed by others (external) and those that were internally developed (internal). Also, “Field/sector” are represented by the combination of dashed line and 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.

→ Thanks and we can add reference to the weather station source, as it was provided by the Provincial Met Office (Province of Trento – Weather service https://www.meteotrentino.it/index.html#!/content?menuItemDesktop=111). We can mention the bias correction in the text and add references to the GeoTransf application, since it belongs to the external component.

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.

→Thanks for pointing that out and there is indeed an oversight. COSMO-CLM (1971-2005) data were bias corrected with observed data (1981-2005) and used in GeoTransf to replicate past stream flows during the Orientgate project. However, due to the missing temporal overlap of GeoTransf values with the S.Giustina turbined water and volume stored values, the regression models considered the 1999-2004 and 2009-2017 baseline accounting for the observed streamflow during that period. We see the need of integrating such explanations within the text, correcting Table 1 and Figure 3 accordingly.

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).

→Thanks and we can improve the description of the input data by adding reference to the GeoTransf application, since it belongs to the external component as reported in Fig.3. Moreover, we can clarify the sentence within the text of the revised version of the manuscript.

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

→ Thanks and we can add it.

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/.

→We can replace ”These blocks" with “these boxes” in order to create consistency with the used terms within the text, hence referring to box 1 (climate projection) and box 2 (hydrological model). Thanks for pointing at the new website and we can certainly use it to replace the previous one.

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?

→Yes, we can clarify that reservoir volume and outflow are outputs of the SDM. Critical conditions refer to both trends of increasing/decreasing water turbined and volume stored as well as volume conditions overcoming certain thresholds for (above the 80th percentile and below the 30th percentile from the baseline, but also 10th and 90th percentiles too as suggested in response to reviewer #1).

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).

→Thanks and we agree mentioning them earlier in the methodology section can clarify their use.

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.

→ Thank you for the comment. We intended to show the R syntax to enhance transparency and to allow the reader to easily reproduce the general workflow within the R environment. However, we agree that adding the underlying mathematical expressions of the statistical methods further enhances traceability and therefore we can add them to table 2. Associated with this table, we can add an explanation of the components to the Table caption (e.g. "s" = Function used in definition of smooth terms within the gam model formulae).

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".

→ According to the response to the general comment F, we will substitute equation #1 to correctly agree with the water mass balance equation.

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

→ Similarly to response #17, we agree to add the mathematical formula for clarity. While, the represented sum belongs to the linear mixed effect model syntax, we can briefly clarify the mean of grouping effect in the regressions in the revised version of the manuscript.

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?

→ Thanks for the feedback. Calibration and validation procedure considered the best fixed-effect and random-effect coefficients in order to best predict reservoir volume and turbined water. Differently from physically-based models, there is no a priori information on the parameter space definition since this is directly derived from the available input data parameter range. Finally, all the information coming from each iteration on model performance is used to compute an average value of RMSE. We will improve the description of this process in the revised manuscript.

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).

→Thanks and similarly to the general comment H and the comment from Reviewer #1, we will improve the description of the MonteCarlo sampling providing more information on the sampling approach (time series of continuous 14 years as for the forward time-window approach).

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.

→ Although model overestimations were very limited in number, we can highlight those cases of corrected value in the revised version of the manuscript.

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.

→Thanks for the feedback. Regarding the concern related to the time scale for the analysis, we intended to provide information on the use of a monthly time scale resulting in a good prediction performance. However, we understand the need to make the sentence clearer and we can rephrase it in the revised version of the manuscript.

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

→Horizontal dashed lines represent the two selected thresholds for the 30th and 80th percentiles. We will integrate this description into the captions.

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.

→Thanks for the comment and we can integrate the results in Table3 with more specific discussions.

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.

→Thanks for pointing this out and we will modify the incorrect values.

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

→Thanks and we can integrate this term with a brief description. We here refer to slow onset conditions as trends of increasingly reductions in water availability (e.g. slow onset usually associated with drought conditions) as projected by the climate projections and affecting turbined water and reservoir volume.

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".

→Thanks for the suggestion and we will make the sentence consistent with the previous one.

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?).

→Thanks for pointing that out and we can indeed clarify the simulation of water streamflows from the hydrological model.

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.

→The sentence in l 376-377 aims to highlight the focus of this study on trend variations at monthly scale on short- and long-term which can affect the S.Giustina water turbined and volume stored (e.g. Fig 7 and 9). We can clarify the sentence with more information on the GeoTransf model use and ability to replicate intense precipitation events providing reference to Bellin et al.,(2016) where full details of these procedures are carried out and described.

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.

→Thanks and we actually reported this under "limitations" as we wanted to point at the possible negative effects of having limited training and testing samples due to over-fitting.

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.

→ The monthly time resolution was selected since a daily resolution provided larger prediction error and monthly resolution better supported the trend analysis with information at the seasonal level.

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