Response to editor and reviewers

Manuscript reference: hess-2013-283

Manuscript title: Storage water value as a signature of the climatological balance between resource and uses

by

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Dear Editor and reviewers,

First of all, we want to apologize for the important delay we needed to submit this second reviewed version. We will explain in detail the reasons of this delay, and the different analyses we have tried to carry out in order to improve our manuscript.

The current reviewed manuscript follows a former revised version we sent at the end of December 2013. In this latter, we tried to answer to the reviewerŝ comments as fully as possible, explaining our choices and clarifying a number of points raised by their comments. As pointed out by the Editor however, we realized that we probably misunderstood the initial assessment of the reviewerŝ and that further efforts were needed to improve the presentation of our analysis.

We carefully assessed how we could improve our manuscript based on all reviews and comments and now, we are pleased to send you this revised version. First, we would like to clarify with you different points highlighted within the second review. Then we will give specific answers to each reviewerŝ comments which are not already included within this first letter.

Note that we do not join to our resubmission the first manuscript version with modifications marks. The revised version actually is too different from the first version to allow tracking changes.

The main objective of our manuscript.

Let us first recall what our goal is, and perhaps first, what it is not:

- Our manuscript was perhaps unclear in this regard, but the aim of our study is not to show the limits or to complete the developments currently carried out on the dynamic programming technique. The numerous developments associated with dynamic programming are mainly dedicated to the important issue of the real-time and operational management of dynamic systems and a lot of outstanding works are regularly published on this topic. As we will precise it below, our work does not fit to this operational management context.

- **The aim of our study is to present and discuss two complementary metrics which allows characterizing the temporal disequilibrium between resource and demand for a given socio-climatic context.** The availability of such metric is important in a context of global change where society needs simple but informative indicators, for anticipating required adaptations. At a given time, the available resource is usually not equal to the demand. The temporal deviations between the resource and the demand can be balanced with storage and release operations to transfer the resource in excess at a given time to times with insufficient resource. The storage temporal fluctuations required to reach the best resource/demand equilibrium over the considered period directly results from and thus fully describes the natural asynchronisation between resource and demand (the optimal equilibrium sought can be simply defined in terms of water quantities, but it can be also socioeconomic, taking into account the marginal values of water for different demands/uses). These storage temporal fluctuations define what we will call in our revised manuscript **the storage requirement scheme**. The storage requirement scheme is a well-known signature of the temporal resource / demand disequilibrium, given the constraints. It is fully determined **by the temporal fluctuations of the marginal values of storage water (SWV),** which can be obtained as a by-product of the dynamic programming optimisation algorithm. The aim of our manuscript is thus to present and discuss these two metrics. We especially aim to show the strong temporal structure of the SWV fluctuations, as an auxiliary signature of the temporal signature of the temporal resource / demand disequilibrium, given the constraints, and to show how it depends on the socio-hydroclimatic context.

- Following the important modifications carried out within the new manuscript, we decided to modify the title of the manuscript. **The new title is: ñSeasonal patterns of water storage as signatures of the climatological equilibrium between resource and demand for the Upper Durance water systemò**

- In the new manuscript, **we improved the introduction for clarification. We now also discuss the storage requirement scheme** as the classical signature of the temporal resource / demand disequilibrium. The storage requirement schemes obtained for the different climate contexts considered in the manuscript are presented and commented. We added two Figures for illustration (i.e. Figure 3 and Figure 8 in the current reviewed manuscript).

- As you notice, **we only use the dynamic programming method as a tool** to produce the both signatures (the storage requirement signature and the SWV signature) used to characterize the resource/demand disequilibrium. We understood that, in the previous versions of our manuscript, we gave too much weight to the algorithm itself. We also understood that the full description of the algorithm in the heart of the article is counterproductive for understanding our main objective.

- **We therefore moved the description of the optimization algorithm to an Appendix.** We believe that this will prevent the reader from a misunderstanding of the real subject of the discussion, which is the analysis of the optimal demand/resource equilibrium, through the storage requirement scheme and the analysis of SWV signatures.

Deterministic Dynamic Programming versus Stochastic Dynamic Programming

- **We are aware that the dynamic programming technique is usually used for the real-time and day to day optimization of operational management systems**. In this context, the optimization process aims to identify the optimal storage strategy for the near future. This strategy can also be described with the marginal values of storage water. In this context, we fully agree that a key requirement for an efficient operational management of water resource system is to account for uncertain nature of the near-future inflows and demands. That is why, to our knowledge, a number of systems are optimized using stochastic optimization methods such as SSDP (sample stochastic dynamic programming) or SDDP (stochastic dual dynamic programming), even though deterministic optimization algorithms are actually used in operational management, but only for the short term management (not more than few days).

- **As explained previously, we do not want to stick to the operational context of the water resource manager.** We are interested in the resource/demand disequilibrium, independently from the uncertain nature of the near-future and from its forecastability. We therefore estimate SWV in a deterministic way from the known sequences of inflow and demand. **To reach the optimal resource/demand temporal equilibrium,** which is to our mind an important feature to be described for any global change analysis, **it is not relevant to mimic the operational management context** and the difficulty of the manager to anticipate future inflow and demand has to be voluntarily disregarded. We are convinced that the storage and SWV signatures obtained with deterministic dynamic programming are perfectly suited for our question. They only focus on the balance between the resource and the demand, independently of any forecastability issue.

- Considering this point, **we understood the term of óstorage strategyô used in the previous manuscript versions, is clumsy**, as óstrategyô automatically refers to the operational context of system management. That is why, as presented above, **we use the term óstorage requirement schemeô instead of óstorage strategy**ò. We believe this term will discard any possible confusion with operational oriented analyses.

Accounting for uncertainty in future projections

- As you noticed it, we do not account - in the elaboration of the storage requirement scheme - for uncertainties in future demand and or resource projections. Uncertainty in future projections that arise from scenario uncertainty, model uncertainty and also internal variability of model can of course be very large as highlighted by a number of recent studies (Hawkins et al. 2011, Lafaysse et al. 2014). From the manager point of view, and at least **from our point of view, this makes no sense to characterize the optimal resource/demand equilibrium from all possible future projections of the future climate. Only one climate will actually realize.** We are convinced that it is therefore much more relevant to estimate the modification of the resource/demand disequilibrium and of the storage requirement scheme conditional on one future possible realization. The question to which we answer is: what would be the optimal storage requirement scheme if the future climate would present those future characteristics (in terms of temperature and precipitation). This is **the reason why we presented the modifications of the SWV conditional on different future possible climates.**

- We do not think this is necessary to clarify it in a revised version of the manuscript. Just let us know if you think it is indeed.

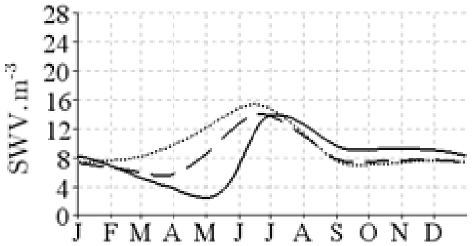
The Earth Moverôs distance

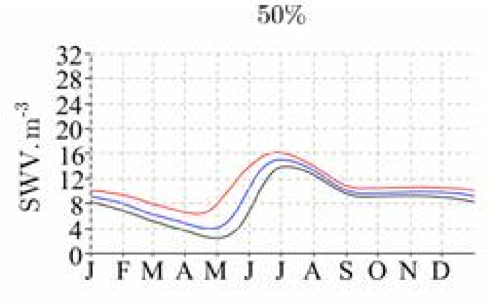
On the other hand, as suggested by the first reviewer (Ehret Uwe), we have explored the possibility to use the Earth Moverôs Distance (Moeckel and Murray, 1997) to estimate in a more quantitative way how the signatures of our study are modified between the control period and any future scenario. **As you will see in the following, we found that the Earth Moverôs Distance (EMD) is not really suited for this.** The table below shows the distance values obtained among the set of SWV signatures presented within the present version of our manuscript (storage level equal to 50 %, the unit is that of the SWV variable). The distance between two given signatures is estimated with an embedding dimension of 2 (see Moeckel and Murray, 1997, for details).

	Ctl	P10T0	P10T3	P10T5	P20T0	P20T3	P20T5	PctlT3	PctlT5
Ctl	0,0	1,7	2,5	3,8	3,8	3,9	5,8	1,7	2,6
P10T0	1,7	0,0	1,6	2,3	2,1	2,4	4,1	1,7	1,7
P10T3	2,5	1,6	0,0	1,7	1,8	1,7	3,6	1,5	0,8
P10T5	3,8	2,3	1,7	0,0	1,1	0,6	1,9	3,2	1,6
P20T0	3,8	2,1	1,8	1,1	0,0	1,2	2,0	3,1	1,7
P20T3	3,9	2,4	1,7	0,6	1,2	0,0	1,9	3,2	1,6
P20T5	5,8	4,1	3,6	1,9	2,0	1,9	0,0	5,1	3,5
PctlT3	1,7	1,7	1,5	3,2	3,1	3,2	5,1	0,0	1,6
PctlT5	2,6	1,7	0,8	1,6	1,7	1,6	3,5	1,6	0,0

Note : in the table, CTL refres to the control climate and P20T5 refers to future climate scenario with a 20% precipitation decrease and a 3°C increase (and so one for the others future scenarios).

As you can notice, **the distance between the CTL signature** (control scenario) **and the scenario P10T0** (æP=-10% and æT=+0°C) **is equal to 1.7, the same than the distance between the CTL signature and the scenario PctlT3** (æP=0% and æT=+3°C). However when you look at the shape of the curves in the following graphs (extracted respectively from figure 7 and 8 of the revised version of the manuscript), **you can see that the blue curve (P10T0) is much more similar to the CTL curve** (the dark continuous one in both graphs) **than the dashed one** (PctlT3 with the long discontinuous line segments).





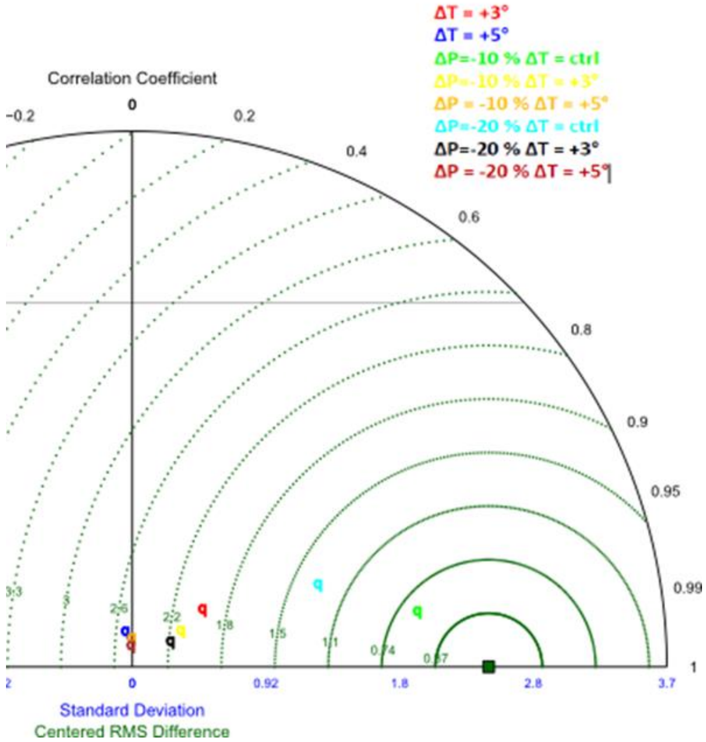
The EMD is actually built to compare the distance of two signals in terms of fluctuations and not in terms of time evolution. Moeckel and Murray. 1997 built their distance based on the following requirement: the distance between two realisations of the same stochastic process should be small as these realisations often exhibit the same probability distributions. For this reason, the EMD distance compares the distributions of the signals and not the signals themselves. As the authors say : ñ*focusing on the way the points* [of the time series] *are distributed* ***é*** *without considering where in the time series individual points occur avoids the problem of sensitive or stochastic dependence on initial conditions*.ò

Furthermore, as a consequence, the distance between two identical signals that have just a delayed seasonality is for instance zero which is a major drawback for our analysis (this results was actually obtained in our case from a simple experiment where the 2 compared signatures are 1) the CTL one and 2) the same CTL one shifted in time by *n* days (e.g. 1 month, or 3 months, or 6 months)). This result is of course independent of the embedding dimension used to estimate the distance. We verified it empirically with different embedding dimensions (from 2 to 5 > the results are exactly the same).

Finally but not least, we could also verify from our data set that the EMD distance between two signatures is highly correlated with the difference between the mean values of the signatures (MeanSignature1-MeanSignature2) (not shown). In our case, the EMD does therefore not bring any added value to this very basic signal statistics (the mean). As a consequence, the EMD distance is not a suited distance to quantify how our signatures are different one from the other.

To provide a quantitative assessment of how are different two SWV signatures, we further tested different other numerical criteria.

Another candidate distance between signatures could be for instance the correlation coefficient computed between any future climate and the control climate, used as reference. We used it actually associated with a measure of the signal variability (standard deviation of the daily SWV values of the signature). The figure below is the Taylor diagram computed using SWV signatures at the storage level 50 % for the configuration HEP. The x-axis represents the standard deviation of each signal. The angle between x- and y- axis is the correlation coefficient between the signatures obtained for future and control climates. Taylor diagram also show the root mean square error among signatures (green circles). The reference signature is that obtained for the present climate and is illustrated with the black square on the x-axis.



This graphic shows clearly the important decrease of correlation between SWV signatures with increasing (respectively decreasing) temperatures (respectively precipitation). However, we think that this information is redundant and again does not add any value to the information already brought by Figure 9 for instance.

We also tried to use additional metric such as the Nash Efficiency criteria or Kling-Gupta-Efficiency criteria (KGE, Gupta et al., 2009). Again, we really think that such results are redundant with the descriptive analysis presented in the present version of the article.

Despite the suggestion of reviewer 1 and the requirement of the Editor based on this suggestion, we therefore decided not to include such quantitative criteria to provide a quantitative assessment of the modification of signatures. We are really convinced now, at least for our case study, that these quantitative criteria are not suited for a deeper analysis of signature modifications as they describe the signals in a much too synthetic way.

With all these elements, we really hope that you will be convinced of the value of our work and that it will be judged suitable for publication in a forthcoming issue of your journal.

Best regards

Baptiste François and co-authors

Gupta, H. V., Kling, H., Yilmaz, K. K. & Martinez, G. F. Decomposition of the mean squared error and NSE performance criteria: Implications for improving hydrological modelling. J. Hydrol. 377, 80ï 91 (2009).

Hawkins, E. & Sutton, R. The potential to narrow uncertainty in projections of regional precipitation change. Clim. Dyn. 37, 407ï 418 (2011).

Moeckel and Murray. 1997. Measuring the distance between time series. Physica D I02, pp.187-194

Lafaysse, M., Hingray, B., Gailhard, J., Mezghani, A. & Terray, L. (2014). Internal variability and model uncertainty components in a multireplicate multimodel ensemble of hydrometeorological projections. *Water Resources Research*, DOI:10.1002/2013WR014897.

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In the following, the issues raised by the reviewers are written in black and the replies in blue.

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Reviewer #1: Ehret Uwe

3. Evaluation

This is a nicely written, well structured, lean but comprehensive manuscript presenting an interesting and novel way to characterize the impact of hydroclimatic changes on multi-objective reservoir operation. The SWV signature can potentially be useful in many studies that deal with reservoir management and climate change.

However, there are a few **major points** where the paper falls behind its potential:

• I like the proposed signatures (figures of seasonal SWV variability). However, the signatures for the various scenarios can and are in the manuscript only compared in a visual-descriptive way. I think it would be useful to further condense the signatures to values that can be compared in an objective and quantitative way. Suitable values could be

  ○ the mean SWV over the period to assess the differences in mean achievable value among the scenarios.

Within the review document we sent at the end of last December, we had added one table to present the mean-interannual values of SWV on both an annual and seasonal basis. However, as it has been highlighted by the editor that the information provided by this table was substantially the same than the one observed on Figure 9. We agree, and we then removed this table.

  ○ to assess the shift in the seasonal pattern of SWV, one could e.g. use approaches like the Wasserstein Distance/Earth Movers Distance/Kontorovich Rubinstein distance , e.g. Moeckel, R. and B. Murray (1997): Measuring the distance between time series. Physica D 102 (3-4), 187-194, which is usually used to determine the distance between 2-d probability distributions solving an optimal transport problem. I think the paper and the proposed method would benefit from going a further step in this direction.

We agree with Reviewer 1 that such a quantitative evaluation could have added a significant value to the graphs we presented in the manuscript. As explained in our letter to the editor, we have tried to use the EMD but also alternative measures to provide such a quantitative evaluation. However, all the measures we tried revealed to be unable to add value to what is already observed from the graphs. We give a detailed discussion on this issue within the letter to the editor. We therefore decided to only discuss our results with the modifications of signatures observed from the graphs.

• In section 5, at the turn of page 9010-9011, something seems to be missing: A reference to Fig. 7 and a complete discussion of it (discussion currently starts for row 2 in the figure)

This is true, and we apologize. The manuscript conversion from word document to HESSD format should be responsible of this deletion and we did unfortunately not see it when we checked for the content of the submission. We added the missing paragraphs within the revised version of the manuscript.

Minor points:

All minor points have been taken into account. We give below additional information when required.

- 9002/9: you write that the lake volume is set equal to the mean annual inflow. Does this correspond to the true volume of Lake Serre-Poncon? If yes, please mention in the text, if no, please give a good reason for it (I would expect that as we are dealing with the real Durance basin, we should also deal with the real reservoir).

Indeed, we are not dealing with the real Durance watershed. In fact, the present case study is inspired from the real case study of Serre-Ponçon reservoir. The Serre-Ponçon reservoir is managed by Electricity de France (EDF), the biggest French private company for energy production. Due to the competitive context on hydroelectricity production in Europe and France especially, presenting the results of the study for the real system configuration was not really an option. The physical characteristics of the presented system are therefore slightly different that those of the real system (e.g. lake capacity and mean inter-annual inflow to the lake). We also give a very simplified picture of the system, disregarding some other real objectives of lake system management (water irrigation and downstream low-flow maintenance). However, we understood than it could be confusing for the reader. We thus decided to erase this information of our manuscript.

- 9006/10: I assume the temperature used here is local temperature in the Durance basin that is also used for the simulations. This implies that the power produced by Serre-Poncon is consumed only locally, and likewise the fixing of the power prices. Should this not rather be on the scale of France? And if so, would your results be strongly affected?

As the Serre-Ponçon reservoir electricity production is actually mainly consumed at the regional scale (especially in the South-Eastern part of France), the reference temperature used in the present study is the mean regional temperature.

Note however that, for a given altitude, temperatures are actually highly correlated in space over large distances. As a consequence, using another temperature series does not really influence the results. The mean temperature for the whole national territory is for instance highly correlated to the regional one and could have been used instead of the regional one. When another reference temperature series is used, the only adaptation to be made to the model is the temperature thresholds T_{heat} and T_{cool} that define the consumption for heating or cooling (those thresholds would just need to be translated according to the mean interannual lapse rate).

- 9010/18: with 'residuals', do you mean the difference between the HEP+RLM signature and an addition of the individual HEP signature and RLM signature? Please clarify.

Yes, the terms residuals means 'difference between HEP+RLM signature and the sum of both'. We have clarified the text of this section.

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Reviewer #2:
This paper applied the dynamic programming method to a simplified water resources system and investigated the impacts of climate and demand change on the seasonal variation of the marginal storage water value. The writing is good but could be improved by simply pay more attention as listed by the 1st reviewer.

My major concerns are on the methodology part.
First, why dynamic programming method instead of stochastic dynamic programming method? Uncertainty is almost unavoidable when dealing with climate and human induced changes.

We agree that accounting for uncertainties in the near future inflows is a key requirement for an efficient **operational** management of water resource. That is why, to our knowledge, several energy companies use methods such as SSDP (sample stochastic dynamic programming) for the real-time management of their water systems. The uncertain nature of the near future, in terms of inflows to the lake and also demands, leads to a storage requirement scheme that is logically different than the one obtained a posteriori from a perfectly known temporal sequence of inflow and demand. The uncertain nature of the near future leads especially to higher value of storage water. This is mentioned in the last paragraph of the conclusion section.

Note that SWV estimated with stochastic dynamic programming for an uncertain near-future context depend on the forecasting skill of the manager. A number of recent studies assumed that the manager has zero forecastability. This is likely to be not exactly the case. To improve the real-time management performance, a number of managers try to reduce uncertainties in future inflow, using some short-term or sometimes seasonal forecasts of future inflows and demands. In the operational context, the management strategy is next regularly updated (e.g. on a weekly time step), using the latest updates of inflow / demand forecasts. **For a realistic simulation of real-time operations, this updating procedure of the strategy should be reproduced. As it is highly time and calculation resource consuming, it is however to our knowledge roughly never applied for climate change impact studies** and, for the sake of simplicity, the near future is classically assumed to be fully unknown. SWV obtained for this simplified configuration are therefore also expected to be rather different than SWV that could be obtained for the operational management configuration. To conclude, even when the uncertain nature of future inflow is accounted for, the operational relevance of published works is not guaranteed.

In the present study, we do not want to stick to the operational context of the water resource manager. We actually want to highlight the best temporal equilibrium between water resource and its uses, independently from the uncertain nature of the near-future and from its forecastability. In other words we want to estimate SWV in the ideal but unrealistic case of a fully known future. This is a background and important preliminary question that cannot be answered reproducing the operational management context. It thus requires voluntarily disregarding the difficulty of the manager to anticipate future inflow and demand. The SWV obtained with deterministic dynamic programming allow answering this question. It only focuses on the balance between the resource and the demand, independently of any forecastability issue. Its application is moreover straightforward and can be applied for any future hydrometeorological scenario.

One could also question why we do not account - in the elaboration of the storage strategies - for uncertainties in future demand and/or resource projections. Uncertainty in future projections that arise from greenhouse gaz emission scenario uncertainty, model uncertainty and also internal variability of simulation models can of course be very large as highlighted by a number of recent studies (e.g. Hawkins and Sutton, 2011; Lafaysse et al., 2014; Hingray and Saïd., minor revision). From the manager point of view, and at least from our point of view, this makes no sense to identify a strategy for a given future prediction lead times from all possible future projections of the future climate. Only one climate will actually realize. **A more relevant analysis is therefore to estimate the modification of the storage requirement scheme conditional on one future possible realization.** The question to which we answer is: what would be the optimal storage requirement scheme if the future climate would present those future characteristics (in terms of temperature and precipitation). This is the reason why we presented the modifications of both the storage requirement scheme and the SWV conditional on different future possible climates. For all of these reasons, we therefore think that the deterministic dynamic programming framework is an interesting framework to highlight how the temporal balance between the resource and the demand could change for different possible changes in mean temperature and precipitation.

Second, the rationale to simplify the case study into a almost hypothetical system does not seem clear to me. Why ignore other water users, i.e., irrigation and drinking water supply? Do the authors at least know the relevant importance of the irrigation and drinking water demand? Do they share any common features with the hydroeletric and recreational water demand?

The Serre-Ponçon reservoir is managed by Electricity de France (EDF), the biggest French private company for energy production. Due to the competitive context on hydroelectricity production in Europe and France especially, presenting the results of the study for the real system configuration was not really an option. The physical characteristics of the presented system are therefore slightly different that those of the real system (e.g. lake capacity and mean inter-annual inflow to the lake). We give for the same reason a very simplified picture of the system, disregarding some other real objectives of lake system management (water irrigation and downstream low-flow maintenance).

If the authors indeed want to gain some deep, new understanding through this hypothetical study, then the introduction has to be rewritten to articulate the critical knowledge gap and the conclusion/discussion part should justify how generalizable the understanding we learned from this case study.

Thank you for this suggestion. We have done major adaptations of the introduction and conclusion sections. We tried to better highlight the interest of the approach within the context of other studies focusing on the climate change impact on the water resource and management.

To me, "the impacts of climate and demand change on the seasonal variation of the marginal storage water value" does not seem innovative enough.
A number of studies, where the operational and real time management of water system has been explored, have discussed the seasonal variations of SWV. In climate change impact studies focusing on water resources systems, SWV are also often used within the optimization and simulation steps of simulation models mimicking the operational management of the systems (see for instance, the so-called Water Value Method, Wolfgang et al., 2009). However, the temporal organization of SWV and the way it is modified under future climates were, to our knowledge, never discussed nor presented in these studies. To our mind, this analysis is however an auxiliary meaningful analysis as it allows for a better understanding of the temporal disequilibrium between climate and demand modifications. It potentially also allows to better relate these disequilibrium modifications to the outputs of the simulation model (in terms of the storage requirement scheme at least, potentially also in terms of water system performance), which are otherwise often impossible to relate to climate forcing changes. We actually think that an interesting part of this climate-to-disequilibrium relationship is explained by the SWV signatures and especially by their changes from one climate to another.

Above said, I have the following minor comments:
1. P8997, L22, "different served" –> "different service"?
This is corrected

2. P9001, L16-17, please rephrase the sentence.
The sentence was rephrased.

3. P9001, L7-10 seems contradictory with L23-25. Have you actually considered the other water demand, e.g., irrigation, or not?
Because of the competitive context discussed previously, we give a very simplified picture of the system, disregarding some other real objectives of lake system management (water irrigation and downstream low-flow maintenance). We did therefore not consider irrigation in this study, even if the real system accounts for this use. Note that it would be relatively easy to integrate irrigation in such an analysis as mentioned in the conclusion of the manuscript.

4. P9002, L7-8, please rephrase.
The sentence was removed.

5. P8996, L8, I don’t fully understand the meaning of "climatological balance" here and also in the title. The authors later used "temporal fit" in a similar fashion. It’d be better to provide a formal definition first before using it throughout the text.
We now used the term “climatological disequilibrium” instead. This was clarified in the text.

6. Fig.3, caption, "SVW"–>"SWV".

This was modified

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Reviewer #3:

- I like concise papers but here sometimes I found it too concise! In many parts the reader is sent to a reference without much explanation: for instance, what is actually in these studies mentioned by the authors: page 9003, lines 13-14, Paiva et al., 2010; Page 9003, line 25, Ward et al., 1996; Page 9004, line 21, Hingray et al., 2007; page 9006, lines 2-3, Paiva et al., 2010 again? A sentence or two about what is to be found in the cited reference would be much appreciated.

More details have been given in the text, in the introduction especially, to clarify the content of the cited references.

- Also related to this issue, I agree with reviewer 2 that some terminology needs to be explained. For instance, page 8995 line 4, I do not see clear what “inflows from past hydrological regimes” means. Should not a catchment have only one hydrologic regime (under stationarity at least)?

We agree this was unclear. With “inflows from past hydrological regimes”, we referred to “observed inflow series”. The text was modified for clarification.

We also clarified other terminology and replaced some with more appropriate expression. For instance, we now use the concept of “storage requirement scheme” instead of the concept of “storage strategy” which actually fits well in an operational management context but not in our *a posteriori* analysis context.

- Later on line 7, what is a “rule curve”?

This is now replaced by “guideline curve” which is the term usually found in the literature. It is also used by the operational manager community. In the present case, the guideline curve shows the level of storage required at any time to satisfy a set of objectives and constraints.

- I think the introduction is too concise and fails in telling the reader what the major achievements in the field are. More information on the literature is needed in the paper: what other recent studies applied deterministic dynamic programming? What studies show operational applications of dynamic programming for reservoir control? What are their main achievements/conclusions?
  What has been done of innovative in dynamic programming and what are the current challenges in the field (isn’t there something more recent than the review of Yakowitz, 1982?) How the study presented in the paper searches to fill the existing gaps and/or answer remaining challenges? This would highlight the contribution of the authors to the topic, which, although the study is an interesting one, is not clear in the paper. In my opinion, the topic of uncertainties (nor pertaining only to future inflows in real time operation of reservoirs, but also to observed flows and expected climate changes), which is very quickly treated in the conclusion, should be

presented already in the introduction. This would help in justifying, for instance, the choice of the authors for a determinist dynamic programming, instead of a stochastic approach.

In our manuscript, we do not use dynamic programming to mimic the operational management of the reservoir but to assess a posteriori the temporal disequilibrium between the resource and the demand via the so-called Storage Water Values (SWV). As a consequence, it is out of the scope of the manuscript to present studies that show operational applications of dynamic programming for reservoir control. We think also that it is out of the scope of the manuscript to present the current challenges in the field of dynamic programing, because dynamic programming classically sticks to the operational issue of efficient and optimal real-time water resource management.

We fully acknowledge that if we would have aimed to give an accurate representation of the real water management system, deterministic dynamic programming would have not been a relevant choice. We especially agree that accounting for uncertainties in the near future inflows is a key requirement for an efficient and optimal operational management of water resource. That is why, to our knowledge, several energy companies use methods such as SSDP (sample stochastic dynamic programming) or SDDP (stochastic dual dynamic programming) for the real-time management of their water systems. The uncertain nature of the near future, in terms of inflows to the lake but also in term of demands, leads to a storage strategy that is logically different than the storage requirement scheme that can be a posteriori obtained with deterministic dynamic programming from a fully known temporal sequence of inflows and demands. The uncertain nature of the near future leads especially to higher value of storage water. This is mentioned for information in the manuscript but this issue is out of the scope of the paper.

Note also that SWV estimated with stochastic dynamic programming for an uncertain near-future context depend on the forecasting skill of the manager. A number of recent climate change impact studies assumed that the manager has zero forecastability. This is likely to be not exactly the case. To improve the real-time management performance, a number of managers try to reduce uncertainties in future inflow, using some short-term or sometimes seasonal forecasts of future inflows and demands. In the operational context, the management strategy is next regularly updated (e.g. on a weekly time step), using the latest updates of inflow / demand forecasts. **As a consequence, for a realistic simulation of real-time operations, this updating procedure of the strategy should be also reproduced. As this procedure is highly time and calculation resource consuming, it is however to our knowledge roughly never applied for climate change impact studies** and, for the sake of simplicity, the near future is classically assumed to be fully unknown. SWV obtained for this simplified configuration are therefore also expected to be rather different than SWV that could be obtained for the operational management configuration. To conclude, even when the uncertain nature of future inflow is accounted for, the operational relevance of published works is not guaranteed.

In the present study, we do not want to stick to the operational context of the water resource manager. We actually want to highlight the best temporal equilibrium between water resource and its uses, independently from the uncertain nature of the near-future and from its forecastability. In other words we want to estimate SWV in the ideal but unrealistic case of a fully known future. This is a background and important preliminary question that cannot be answered reproducing the operational management context. It thus requires voluntarily disregarding the difficulty of the manager to anticipate future inflow and demand. The SWV obtained with deterministic dynamic programming allow answering this question. It only focuses on the temporal equilibrium between the resource and the demand, independently of any forecastability issue. Its application is moreover straightforward and can be applied for any future hydrometeorological scenario.

One could also question why we do not account - in the elaboration of the storage requirement scheme - for uncertainties in future demand and / or resource projections. Uncertainty in future projections that arise from greenhouse gaz emission scenario uncertainty, model uncertainty and also internal variability of simulation models can of course be very large as highlighted by a number of recent studies (e.g. Hawkins and Sutton, 2011; Lafaysse et al., 2014; Hingray and Saïd., minor revision). From the manager point of view, and at least from our point of view, this makes no sense to identify a strategy for a given future prediction lead times from all possible future projections of the future climate. Only one climate will actually realize. **A more relevant analysis is therefore to estimate the modification of the storage requirement scheme conditional on one future possible realization**. The question to which we answer is: what would be the optimal storage requirement scheme if the future climate would present those future characteristics (in terms of temperature and precipitation). This is the reason why we presented the modifications of both the storage requirement scheme and the SWV conditional on different future possible climates. For all of these reasons, we therefore think that the deterministic dynamic programming framework is an interesting framework to highlight how the temporal balance between the resource and the demand could change for different possible changes in mean temperature and precipitation.

We have improved the introduction and conclusion to clarify some of the above discussed points.

- In the conclusions, I think the authors could discuss more about how their modeling approach could be used in real-time forecasting, mainly considering the use of probabilistic or ensemble-based predictions of future inflows: can it be directly used? How? What adaptations would be needed?

As discussed above, we do not want to stick to the operational context of the system management. We do especially not consider any real-time forecasting objective which defines a very different analysis context. We nevertheless modified our conclusion to better highlight the interest of our work.

- Also, page 9013, lines 27-29, for instance, the sentence "The SWV is known to increase in this case when compared to the SWV obtained with perfect foresight, as a result of inflow variability and forecastability." Is not clear to me: it is known by whom? What studies/results are you referring to?

The reference Draper et al. (2003) has been added.

- Also, Page 9014, line 3: when you talk about "changes in the variability of future inflows", I was wondering if these would not already have been captured (under the hypotheses of stationarity) in the control period. What changes otherwise are you talking about?

In our study, future climate scenarios are obtained using a classical perturbation method where relative (precipitation) and absolute (temperature) anomalies are applied to the meteorological forcing of the control climate (see Maraun et al. 2011).

The perturbation method leads actually, mainly as a result of temperature warming, to a modification of the seasonality of inflows (and of demand) as illustrated in the manuscript. However, the time sequence of future precipitation scenarios is exactly that of the observed precipitation in the control period (future time series are obtained from the observation with a multiplicative scale factor). Thus, the observed time sequence of wet / dry periods is kept for future scenarios. The succession of wet / dry years in the future climate is the same as the succession of wet/dry years in the control. The perturbation method does not allow for changes in these sequences. This is true for precipitation but also, as a consequence, to inflow to the reservoir.

Of course, the interannual variability of precipitation (and thus also of discharges) is likely to change in the future. Such changes are likely to seriously impact the storage requirement scheme. We could not analyze this in our work but we expect to do it in our further works using future meteorological scenarios derived with perfect prog statistical downscaling models (Lafaysse et al., 2014).

We clarified this point in the conclusion.

- Finally, Page 9014, line 5-7, last sentence: "Analysing these signature changes would probably improve our understanding of modifications of system performance classically reported on the basis of a variety of performance criteria in climate change impact analyses.", what reports are you talking about? What performance measures?

We have added some more material on these performance measures within the introduction section.

- About the data and the use of a hydrological model: Why do you need a model to apply the dynamic programming and discuss on the variability of SWV as you did in the study? Couldn't you have used the long time series of observations (instead of "control simulations")?

We agree. We could have used observed discharge instead of simulated discharge for the control period. Note however that the "simulated discharge for the control period" are obtained via CEQUEAU simulation with the *observed* meteorological variables corresponding to the control period. Next, for the simulation in a modified climate context, we required simulating the inflow discharges from future meteorological scenarios. We therefore required a hydrological simulation model. The model we applied is of course not perfect. The main reason why we also used simulated discharges instead of observed ones for the control period is that we wanted to avoid any biased interpretation of the results that could have been introduced for the future by some deficiency in the hydrological model. Using simulated discharges series for the control from observed meteorological variables make sure that differences in control and future SWV signatures are only due to changes in the climate context and that they are not due to some possible drawback of the hydrological model.

Besides, are the observed data stationary? If not, couldn't any changes in observed data be used to test the effects of inflow changes on SWV?
This is also an interesting point. Over the 1959-2005 period, there is no significant discharge trend but inflow data can obviously not be considered as stationary (Lafaysse et al., 2014). The seasonal pattern of SWV is rather dependent on the year, as a result of both interannual variability of demand and inflow.

This is highlighted in figure 4. Future conditions for some future years could have therefore been observed already for some recent years. SWV signatures of some recent years would possibly allow having an idea of possible future SWV patterns for specific future year. However, for most cases, future conditions are much too different from those of already experienced years, mainly because of warming. This is for instance the case for inflow conditions (where a large change in seasonality is obtained for the future, which was not really observed for any of the recent years) and next for SWV signature (see figure 5, 6, 7). Simulated inflows are thus likely to give a more exhaustive view of possible future configurations than already observed ones.

- Since you used a hydrological model, can you add a sentence or two on the quality of the simulations of the model? How good/bad is the model in reproducing discharges?

The Nash efficiency criterion is now given in the text.

- Also, why did you use meteorological reanalyses (Page 9004, line 13-14) and not observed data for the control period, i.e., why not use the same data used for the calibration of the model? What is the quality of the reanalyses in this catchment?

Meteorological precipitation reanalyses used in our work are not the precipitation outputs of the well-known atmospheric réanalyses (e.g. NCEP/NCAR, ERA40 or some other reanalysis). Precipitations from those reanalyses are actually known to be of poor quality. The precipitation reanalyses we used in our work are those of Gottardi et al. 2012. They were obtained on a 1km grid basis from all relevant observational data (precipitation observations as well as snow pit observations) available within the region. They are known to provide the best precipitation estimate for the French alpine domain. In our work, those data were used for all the analyses we presented. They were also used for model calibration when needed.

- Page 9004 "climate scenarios": when using the scenarios of changes in precipitation and temperature together, did you take into account correlations between the variables?

In our study, we do not take into account any correlation between temperature and precipitation variables. Indeed, we only do a sensitivity analysis on SWV modifications for different hypothetic precipitation and temperature scenarios. This follows for instance similar sensitivity analysis by Horton et al., (2006).

Minor points:
All of minor points have been taken into account. Precisions are given below when required.

Page 8997, lines 10 and 14: is t0 and ti+1 the same thing? Shouldn't it be equal in these lines?
Here, we agree that the notation was misleading. We modified the paragraph accordingly.

Page 8997, lines 21-22: something seems to be missing in the sentence
We agree. Actually the word "served" has to be replaced by "services"

Page 8999, line 10: "Different methods were. . .", what are these methods?
We have modified the paragraph for clarification.

Page 8999, lines 12-16: can you rephrase/explain this sentence? Not clear to me.
We have modified the paragraph for clarification.

Page 9000, lines 21-22: can you rephrase/explain this sentence? Not clear to me.
We have rephrased this sentence for clarification

Page 9003, line 7: ". . .due to the important depth/width ratio of the reservoir.": can you say what this ratio is?
We agree that this sentence was not appropriate. Direct precipitation (roughly 1300mm over 1994-2004) to the lake and evaporation from the lake (1100mm+/-100mm over 1994-2004, Vachala, 2008) are actually of same order on a yearly basis. The net balance between both terms (200mm for a surface area of 30km2, which corresponds to) is next much less than 1% of the mean river discharge to the lake (80m3/s). Both Direct precipitation and evaporation from the lake can therefore be neglected in the lake water balance.

We have modified the text accordingly and have added the following reference which is available online:

Vachala, S. 2008. Évaporation sur les retenues EDF du Sud de la France. Master Report, UPMC, ENGREF, Paris. http://www.sisyphe.upmc.fr/~m2hh/arch/memoires2008/Vachala.pdf

Page 9003, line 16: what is "an empirical rule"? How is it defined?

In our context, an empirical rule is obtained from historical experience. It is not necessary optimal. In the corrected document, we replaced this term by "empirical guideline curve". In operational management, this guideline curve is obtained considering the historical series of inflow and downstream water.

Page 9003, line 20: what do you mean by ". . .will be balanced on the midterm."?

Here, we use the term "balanced on the midterm" to explain that RLM objective, which is now a priority, could be, in a future climate, in competition with HEP at the seasonal / yearly time scale.

Page 9006, line 1: as expressed here, I think that you should be careful to use HEPI as "Hydroelectric production interest" Index all over the paper. Sometimes it is confusing.

We agree this sentence was confusing. We have modified the text for this sentence and elsewhere when needed for clarification.

Page 9006, lines 15-16: can you explain why they were set to unit and justify the arbitrary higher value?

We actually defined in the text $HEPI_h$ and $HEPI_c$ as respectively the additional HEPI for each additional heating and cooling degree day.

Note first that the choice of unity for $HEPI_h$ has no implication. Simple mathematical developments actually show that results obtained using a given objective function g are the same than those obtained with the objective function $g \times B$ (with B a constant parameter) (Kirk, 2004).

We next chose for $HEPI_c$ a value arbitrary higher that that chosen for $HEPI_h$ for the following reasons:

On the one hand, energy consumptions observed in several Mediterranean countries show that the sensitivity to temperature is higher in summer than in winter. In other words increasing temperature of 1°C in summer implies a higher augmentation of the electricity consumption than a decreasing temperature of 1°C in winter. This is for instance the case in Italy (François and Borga, 2013).

In France, almost 80 % on the energy generation is next brought by nuclear power plants. Many nuclear power plants are located for cooling purposes in low-elevation areas close to rivers. The cooling capacity of rivers is obviously much smaller during low flow periods which mainly occur, for low-elevation areas, during summer, a season when temperatures of river stream flow can be also very high. The summer season is therefore potentially critical for cooling nuclear plants. It can lead to temporal sequences where electricity generation from those plants has to be significantly decreased. In the case of such critical configurations, the interest for hydroelectricity production becomes conversely very high.

For these reasons, we thus assumed that the additional HEPI for each additional cooling degree was higher than for each additional heating degree. We therefore defined a higher value for $HEPI_c$ than that for $HEPI_{h.}$ More data would have been of course required to provide a better estimate of the ratio between both parameters. This is an issue we want to precise for further works.

Reference

François, B., Borga, M., 2013. Modeling energy consumption in Italy. Report, 18p. Available upon request.

Page 9007, lines 12-13: Why the chosen 4-year period? Were the other years the same?

For the sake of legibility, we could not present the whole simulated period. We think however important to illustrated the time variations of the different variables under consideration. We have chosen this 4-year period because it contains several inflow conditions: i) high spring flood flows (year 1977), ii) very low spring flood flows (year 1980), iii) fall floods related to storm precipitation (autumn 1979). In this way, this 4-year period is the better sample that we can find, from the whole period used, to comment how SWV variations are related to variations in inflows and HEPI.
The temporal pattern obtained for the other year is similar, for inflow, HEPI and SWV variations.

Page 9010, lines 18-19: "The most significant residuals are observed for the winter season at low reservoir levels.": can you give an explanation for that?
No, we have not a precise explanation and more analyses would have to be done to bring a comprehensive reply. An explanation would be perhaps that the stress increase on the resource is the highest for this configuration. We know that a significant part of the water used during the winter in HEP configuration period must be kept within the reservoir to fulfill recreational requirements in RLM+HEP configuration.

Page 9011: suggestion: maybe separate into sub-sections for better reading: 5.1 Effects of warmer temperatures; 5.2.Effects of precipitation decrease; 5.3 Conjugated effects of warmer temperature and precipitation decrease.
Thanks for the suggestion. We did however not separate the text in sub-sections because they would have had very ill-balanced sizes.

Page 9011: Fig. 7 needs to be mentioned together with its results.

This is true and we apologize. A part of the manuscript, including the reference to figure 7, was missing in the submitted version. The manuscript conversion from word document to HESSD format should be responsible of this deletion and we did unfortunately not see it when we checked for the content of the submission. We added the missing paragraphs within the reviewed manuscript.

Page 9011, lines 9-10: in the sentence: For example, for the 50% storage level, the large SWV decrease observed in 10 the control climate during the six first months of the year tends to disappear…", which year are you talking about?

As we are talking about SWV signature, we do not discuss a specific year but the SWV mean inter-annual cycle over the calendar y. Misunderstanding should be avoided with the inclusion of the missing paragraph mentioned above.

Page 9011: some more sentences helping the reader to read and understand fully the figures would be nice here.

Difficulty pointed out here should be avoided with the inclusion of the missing paragraph.

Page 9020, Fig. 3: should it be HPEI instead of EP in the legend?

You are right. This was actually HEPI in the legend. We have modified the legend and the caption of the figure.

Figures 4 to 7: Could you use also different colours to facilitate reading the different information?

We have use colored figures for figures 4 to 6. We kept the black and white version for figure 7 in order to keep the consistency of the different lines symbols with those of figure 8.

Reference

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Horton, P., Schaefli, B., Mezghani, A., Hingray, B., Musy, A., 2006. Assessment of climate-change impacts on alpine discharge regimes with climate model uncertainty. Hydrological Processes 20, 2091–2109.

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