

## Anonymous Referee #2

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The manuscript presents a case study on the Volga - Caspian Sea water budget subject to the heat wave 2010. The study deserves a possible publication as being interesting for a wider hydrological community. Results are partially novel, though presentation is sometimes vague (see my comments below on section 3.2, 3.3 and 3.5) making the conclusions too uncertain. My advise is to essentially revise the manuscript, complement it with later data and more detailed analysis. After this, the study may be recommended for publication.

The overall structure and the language are clear.

Title is catchy but does not fully correspond to the contents of the manuscript. Not more than 20% of the analysis is dedicated to the "russian drought 2010". The rest deals with information from previous years loosely connected to the question of anomalous dry events.

**\*\*True but the Russian drought was the purpose of this study. We thought it necessary to investigate also other events to show the credibility of the analysis, interpretation and data**

Abstract is unnecessary long and can be shortened without loss of essential information.

- Lines 14-21: 2+7+5+7=21, not 22cm.

**\*\*In the caption of Table4 it is said that rounding might cause such differences but agreed that it might confusing and we can make the numbers fit (also page 8 line24). We now leave out these details in the Abstract**

Presenting information in such a way is too

confusing for an abstract, which should be understandable as a standalone text.

1. Introduction is too sketchy. No indepth review on state-of-the-art is presented, though the water budget of Caspian Sea is certainly not a new problem.

We have referred to the **main investigators of this matter, Rodionov and Golitsyn. We looked**

**\*\*through the papers about the Caspian Sea, which have collected during the years, and have added some more**

- Lines 19-20. The reasons and consequences of inclusion/exclusion of Kara-BogazGol in the analysis should be explained.

**\*\*We use the ECMWF reanalysis data on a 1.5° grid. With such a resolution one cannot separate the CS from the Kara-Bogaz-Gol. This explanation does not fit in the introduction and needs to be moved to the result or data section**

- Lines 20-21. The sentence contradicts to the first sentence of the abstract

**\*\*“Established” is probably not the right word in the Abstract. Does it imply that it has been done the first time? Perhaps “analysed”**

- Lines 3-5 (p7783): Avoid using future tense for description of the work already done.

**\*\*OK**

2. Data

- Lines 2-4. Sentence is unclear. Try to rephrase. ("Except" = "Excerpt"?)

**\*\*It means that ERA uses many data but no precipitation observations. We rephrased it to: “using many observational data but no precipitation gauge observations”**

3. Results - presentation is sometimes too sketchy or incomplete (Sections 3.2, 3.3, 3.5)

Section 3.1:

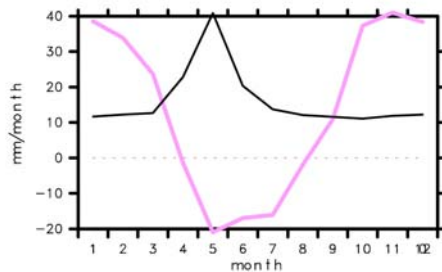
- Lines 27-29: Sentence is confusing. Where the 21% come from? Why is it "perhaps" 21%?

**\*\*The observed VRD is 71 cm CSL-equivalent this plus 20% by other river discharges (71+14=85cm) have to be compensated by evaporation over the CS. P-E over the CS is however 56 cm, so (85-56)/85=0.34. The 21 should be 34. This is only a very rough value as we used the 20% for estimating the other river discharges, therefore “perhaps”. Section 3.2:**

The section deals with estimation of the time lag between strong precipitation events over the Volga Basin and the increase of the Volga River discharge into Caspian Sea. The authors claim to present results on two different methods of the time lag estimation. The first method consisted, apparently, in a simple visual inspection of monthly averaged datasets and subjective choice of corresponding peaks in precipitation and discharge. Results are presented only verbally in Table 2 without any support for the results reliability. In the second paragraph, an attempt of applying the cross-correlation analysis to the precipitation/discharge data is described. No results of this analysis are presented. A necessity of low-pass filtering ("smoothing") of data is declared, but not explained. Two smoothing window sizes, 5 and 9 months, were used without a justification for these choices. In overall, the results presented in this subsection cannot be published in this form. Either the subsection should be rewritten by presenting a more thorough analysis, or omitted completely with subsequent changes in the analysis and conclusions.

**\*\*Also reviewer1 suggests some changes here. There the following comments are given:**

**For that the easiest and also most convincing would be to compare mean annual cycles of P-E over the VB and the VRD. P-E has its maximum from October to February while the VRD has a maximum in May, so there is a clear delay of several months mainly due to storage on the ground by ice and snow.**



**Table 2 concentrates on delays of anomalies during summer which is more relevant for this study and we could show all the plots used for this table.**

**The plot shown here is discussed without showing it and the plot containing the information of Table 2 is added**Section 3.3:

The results are too loosely presented. It is difficult to figure out the meaning of presented numbers and their connection to each other. The Authors try to provide the reader with a "feeling" on the uncertainty of the water budget components' estimation that is not a proper way of scientific analysis.

**\*\*Yes, we improved it by separating the discussion on the 3 periods to be followed by the error section by adding the following at the end of the paragraph, making it**

**hopefully more acceptable: This hand-waving estimate of uncertainty can be substantiated for the error of P-E over the CS itself, as the observed inflow from the Volga River and the observed CSL are available. Comparing monthly means of P-E over the CS with differences of the VRD and the CSL change after applying a 1-2-1 smoothing gives a correlation of 0.40 and a RMS error of 1.4 cm, i.e. a slightly lower value than that obtained above.**

It is interesting to note that the correlation between CSL by satellite and by gauge is lower and the RMS higher (0.35 and 1.9cm) than the comparison with P-E

- Lines 1-5 and Table 3: How the numbers +39cm and -21cm in text relate to the CSL change values of +16 and -10 from Table 3?

**\*\*Table 3 gives values per year while +39cm and -21cm are the changes for the period discussed. This was done to make the numbers of the 3 period comparable. This difference is now stressed in the text**

Section 3.4:

- Line 14: Reference to Fig. 3 appears, but Fig. 2 was not referred before. Revise figures' numbering.

**\*\*OK, we make a reference to Fig. 2 earlier**

Section 3.5:

It is hard to judge, how novel are the results presented in this subsection. It is also unclear how this subject is related to the anomalous drought of 2010. If, as authors state, it is planned "to investigate this issue... in a separate study", the subsection has to be removed from the Results section of this manuscript.

**\*\*Previous paper found the relation with ENSO for very large changes in the CSL and it is interesting that also the Russian drought occurred with the development of an La Nina event. We like to keep this information and the Fig.2 shows this correlation between CSL and ENSO very nicely although it is hard to understand the connection. This connection should be stressed and if we fail to write a paper about this connection it should be mentioned here. One would expect more a connection with the NAO but that we could not find in earlier paper and is therefore not tried further**

#### 4. Discussion

The Section presents a nice overview on regulation measures on the River Volga and their effect on the discharge and, consequently, the Caspian water level. It is stated that the effect of the Drought 2010 on the Volga discharge is significantly delayed by these measures and is expected to last, at least, throughout 2011. This is indirectly supported by data in Fig. 3, where the level decrease continues down to the latest measurement of April 2011. The apparent conclusion for the reader is: Impact of the dry event cannot be fully estimated, unless data from later periods become available.

**\*\*Yes, one needs a long history of P-E to get an estimate about the availability of water in the reservoirs as that will influence the future changes of the CSL. It is also a reason why we did not restrict the discussion to the Russian drought as criticised above.**

That is, including data at least to the end of the hydrological year 2010-2011, and performing analysis based on the extended information would make the results much more valuable.

**\*\*As the discussion of the paper started 2 months ago, we used the time already for looking into more recent data. Except that the drop of the CSL continued until August, we could not find anything exciting to change the present manuscript and one has to stop the work somewhere. It can be done.**

- Line 22: replace "was very low, i.e. 50-70% of the normal" with "was 50-70% of the normal".

**\*\*OK**

Table 1:

Abbreviation KBG should be explained in the text or in the figure subscript.

**\*\*OK**