Clim. Past Discuss., 8, C3342–C3345, 2013
www.clim-past-discuss.net/8/C3342/2013/

Climate of the Past Discussions

Interactive comment on "Palaeostages of the Caspian Sea as a set of regional benchmark tests for the evaluation of climate model simulations" *by* A. Kislov et al.

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Received and published: 12 February 2013

Generally I can agree with most of the statements which are made in the paper but there are a few places where one would say, please a bit more precise and more information. The title suggests more than what is delivered in the end. The paper has some basic problems which need to be addressed first before one can make a proper review. Some of the questions given below cannot easily be answered and are meant only as suggestions. Sections 4 to 6 give information from the knowledge of the authours without any clear focussing on model validation. Some information should be given about the model used here to simulate the LGM. Most of the information of the

MIROC model can easily be found elsewhere, though one line of general information would be welcomed. Important is however information concerning the CS. Is the CS specified in the model? Yes, I think so, but is it a lake model, typically the same depth everywhere and no horizontal exchange, or is a full ocean model? As the evaporation is also part of the energy budget, which is determining the evaporation, one needs to know, if the model is able to have snow over lake/sea ice. If not, the lakes/seas lose a lot of energy by radiation in winter which is then not available for evaporation in spring and then leading to an underestimate of annual mean evaporation which is very important for P-E over the CS during the LGM. The authours ignore completely that the evaporation over the CS will change by a large amount, when going from the present to the LGM. Arpe et al 2011 find a drop of the CS evaporation by a third from the present to the LGM with their most advanced model, and come as well to the conclusion that the CS was probably lower than now. With such a change of the evaporation over the CS, the author's assumption that the Volga River discharge is the only important term in the water budget for the CS, may not be valid because it drops as well by 30% in the present investigation. Why did the authors not have a look and provided the information? The statement that the CS variability stems primarily from the VRD (p5054 lin1) is only valid within the same climatological regime, i.e. Section 3. The paper suffers as well from unclear or undefined methods. On p5057, lin8 they give the equation Qin$\sim$kQ but what is Q and why is the factor 0.7? On p5058 lin5 they use Q0 but what is Q? From eq. 4 one can see then that they want to integrate the VRD anomalies to obtain the CSL which is much easier to understand than going through those equations. But why is Qout (the P-E over the sea) smaller than the mean VRD? It should be larger as the VRD is only 80% of the total river discharge into the CS. On p5059 lin22/23 they give a ratio of std between VRD and P-E over the CS by the model. It took me a very long time to figure out what they meant with that. From that one can deduct that the P-E over the CS was available to them. On P5058 lin3/4 the observed equivalent is give to be 2.0. Why not mention it here? Why is it good that this factor for MIROC is twice the observed one? If the std is so much larger why is the model

not able to reproduce the observed variability (lin25/26)? On P5060 lin1/2 it is said that CNRN is better in reproducing the phase. What are the main differences between the models? Arpe et al 2000 found a connection with ENSO and the CSL changes, has CNRN a better simulation of the ENSO variabilities Some help could be given to use Fig. 2, e.g. for the present P-E over the CS (800mm/y-p5064lin4) and VRD (243 km3/y – p5065lin4/5) the CSL is -34m. This does not look right. This example shows also how difficult it is to get some numbers out of this paper. In Fig. 2 and 3 the integrated values of VRD anomalies are shown. Such a curve by default has to end at the same value as it started. This causes however a problem when there is a trend in the VRD data. E.g. as an extreme if the input data consist only of steadily declining values the integrated anomaly curve will look like half a sinus curve with increasing numbers for the first half and decreasing in the second half. It looks as if the obs curve in Fig. 3 suffers from such a trend but my own VRD discharge data do not show such a trend, and if I do such an integration in time with my VRD data (probably the same stemming from the work from Golitsyn and Panin) I do not get such an increase of the CSL from 1890 to 1930 as shown in Fig. 3. With this in mind it cannot be understood why the CNRN curve does not return to its initial value, perhaps the long term mean is based on a different period than the one shown in the Fig.3. Showing in Fig. 2 only a part of an available time series to show that estimate and observations agree and ignoring the rest of the time series where it does not agree, I would call cheating. Though the cheating was not needed as the curves would agree also for the earlier time, if done correctly.

Some comments in detail: P5054 lin2:...Caspian Sea (CS) on decadal to centennial time-scales primarily...(see P5057 lin24) P5054 lin26: ....great success of ... (in Table 1 the high stand of +48 m could be before or after the LGM – is that a success?) P5055 lin4:....PMIP3) have focused on.... P5055 lin15 ....Such tests are more suitable for..... P5056 lin1: ... water-budget changes. ..... P5056 lin23: Rychagov 1997 a or b also 5060 lin22 and P5061 lin 3 P5056 lin25: .....further 1.2 m drop towards 1977 .... P5058 lin1/2: change mean variability? What is this sentence good for. What means

random in this context? The high evaporation over the CS in June to August 2010, is that random? P5058 lin5: Golitsyn P5059 lin1: On P5057 lin24 it is said the following applies for decadal variations and now it is suddenly also true for paleoclimate changes. P5059 lin13: ....1998; Arpe and Leroy, 2007). .... P5060 lin5: ...Arpe and Leroy P5062 lin6: ....At 21ka BP, the total precipitation over the VR catchment was substantially less ($\sim$50%) than today, see table2 P5065 lin21/22: during late Khvalynian the melting could have led to the high-stand of the CS P5066 lin10: ...CS level changes although the precipitation variations are much larger than in the present. P5066 lin12: ...studied model... only one has been studied P5066 lin19: several meter? Fig. 6 shows extremes from -1.7 to +0.3 m P5067 lin8: Acknowledgement is twice

A possibly useful reference: Arpe, K., Leroy, SAG, Mikolajewicz, U., A comparison of climate simulations for the last glacial maximum with three different versions of the ECHAM model and implications for summer-green tree refugia. Clim. Past, 7, 1–24, doi:10.5194/cp-7-1-2011.

K. Arpe, L. Bengtsson, G.S. Golitsyn, I.I. Mokhov, V.A. Semenov and P.V. Sporyshev, 2000: Connection between Caspian Sea level variability and ENSO. GRL, 27, 17, September 1, 2693-2696, 2000.

Finally: I know that the authours can do better than what has been produced here and they should try.

Interactive comment on Clim. Past Discuss., 8, 5053, 2012.