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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Reducing structural uncertainty in conceptual hydrological modeling in the semi-arid Andes" *by* P. Hublart et al.

Anonymous Referee #1

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Overall I think the paper is very interesting and generally well presented. The topic covered is quite complex (many different components to the modelling study) and therefore it is important that the explantions are as clear as possible. In most cases, I think this is true but there is one example where I think the explabnation could be improved and that is in the last paragraph of 4.1.2 where the three models are discussed. I think it needs to be made explicit that models 6, 30 and 54 are linked to the three different snow accumulation schemes. I also found the explanation at the start of 4.2.3 to be quite confusing and could be explained a little better.

I found the implied definition of equifinality on page 12163 to be very limited. Why is equifinality limited to a single criterion? The concept was borrowed from geomorphology and relates to the same outcome from different causative processes. The definition

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used in the paper is a very limited 'mathematical/statistical' one.

It might have been useful to show some time series of flow and rain at the start of the paper to illustrate the hydrological regime (2.3.2). This could help the readers to understand the concepts of greater than 100% runoff coefficients. I assume that these are related to quite slow groundwater release processes where precip (or snowmelt) from one year only appears as runoff in the following year. Perhaps this also depends on how you define the hydrological year and this is not adequately explained in the paper. While the authors introduce some 'real hydrology' in section 4.2 these discussions are quite limited compared to the much greater detail about the statistics and mathematics of uncertainty. This aspect of the paper could be improved. I also noted that the issues of data uncertainty associated with the estimation of natural streamflow are only mentioned right at the end, while these could have a very large impact on the modelling results if the naturalisation process and the knowledge of abstractions is quite poor.

I think the paper contains too many references - it is not a review paper and many of them are somewhat superfluous. There are also several that are included in the reference list that are not used in the text (Clark et al, 2009; Fenicia et al., 2007; Fowler and Kilsby, 2007; Freer et al., 2013; Hrachowitz et al., 2013; Krueger et al., 2010; Lang and Braun, 1990; Leavelsley et al., 2002; Loukas et al., 2002; Montecinos and Patricio, 2003; Olssen and Andersson, 2007; Staudinger et al., 2011; Strauch et al, 2006 and Zhang et al., 2010). Some of these could be related to wrong dates as the following included in the text could not be found in the list: Clark et al., 2005; Fenicia et al, 2006; Freer et al, 2003; Montecinos and Aceituno, 2003). Shaefli et al, 2011 is also spelt wrong and Souvignet et al. has the wrong date?

Figures 4 to 8 could all be improved in clarity with larger font sizes and other improvements. There is space to do this.

Some other minor points:

Are the 12 and 8 (precip & temp) stations supposed to be shown on Figure 1?

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Page 12149 line 14 - where is Eq 1 referred to?

Page 12157 line 8 - Is 'emblematic' the right word here?

Page 12159 line 6 - '..internal state variable obtained..'

Page 12160 line 5 - '..absence of a sublimation..'

Page 12161 - The reference to Figure 7 at the start of 4.3 should be Figure 8 I presume.

Page 12162 line 14 - '.. filling of a moisture..'

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 12137, 2014.