Hydrol. Earth Syst. Sci. Discuss., 12, C1346–C1350, 2015
www.hydrol-earth-syst-sci-discuss.net/12/C1346/2015/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Identification of spatial and temporal contributions of rainfalls to flash floods using neural network modelling: case study on the Lez Basin (Southern France)" *by* T. Darras et al.

Anonymous Referee #1

Received and published: 6 May 2015

This paper focuses on the application of the KnoX modelling methodology to extract knowledge about the contribution and timing of different geographical aquifer zones to flash floods in SE France from artificial neural network models. It is a very worthwhile exercise - the karst aquifers of this region are complex and difficult to model physically. Consequently a knowledge extraction approach using data-driven modelling techniques is a sensible and novel solution. It is also excellent to see an example of the use of ANNs for geographical knowledge extraction, rather than the more commonly attempted (and rather uninteresting) lumped catchment 'curve-fitting' tasks. However, I

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

am concerned that the KnoX methodology (which is essentially a method for assessing model input contributions to the output based on the network weights) is not particularly innovative - network weights have been used for more than a decade to understand the importance of inputs (Olden and Jackson, 2002; Kingston et al., 2003; 2005)

What troubles me more, however, is the fact that the KnoX method has been applied to this aquifer before by this authorship team or members thereof (see several citations of work by Kong-A-Siou et al.). The authors make some reference to this in Section 3.5. and in Section 4 cite that the difference with this paper is the hourly data used to drive the model. This leaves me wondering what the contribution to knowledge is in this paper compared to the several other papers by the same (or similar) authorship team. If this is a repeat of previously published work that is largely the same except for a different temporal resolution of input data, then it feels like only a minor contribution to the literature. Therefore, before it can be accepted for publication, I think that the authors need to be very clear about how this paper develops the other papers by Kong-A-Siou et al., 2013 on the Lez Basin, the new findings / insights that result from this paper, and their relevance and importance for hydrologists.

The introduction / literature review is generally well structured and provides a fairly comprehensive and critical overview of the key literature and the arguments from adopting the method used. There are far more examples of the use of the multi-layer perceptron than the two articles cited - a more extensive tabulation would make the review more complete. Similarly, the application of ANN-based models in a spatially discretized structure to deal with heterogenous and complex hydrological behaviour has also been explored before (e.g. with rainfall-runoff models) and it might be worth mentioning these for completeness (e.g. Tsai et al. 2014, Hyd. Proc., 28(3), 1055). The issue of how to select the 'best' or 'correct' input data sets to the ANN is skipped over a little. The authors might like to consider mentioning the sorts of information-based methods that have emerged for selecting model inputs over the last few years (e.g. the Gamma and Evans tests) and explain their choice of inputs a little more thoroughly in light of

HESSD
12, C1346–C1350, 2015

these ideas. I also note that the authors identify ANNs as 'statistical' models. This is, of course, true. However, the term 'data-driven' is perhaps more commonly used to describe ANNs and the authors may wish to alter their terminology.

Section 2 deals with the basic concepts of ANN design and development. The MLP ANN is chosen, but there is not any real justification for this presented. Why not a RBF ANN or some other variant? I think a stronger justification for the MLP would be useful here. The terminology 'stop set' is not standard, but I do think it is clear. 'Overtraining' is more commonly referred to as 'overfitting' and this is a term that the authors might like to adjust. The authors do not explain that the issue of overfitting is exacerbated by data splits that are not fully representative of the signals in the data. There is a huge literature around methods for achieving representative data splitting to improve the generalisation of ANNs (Holger Maier at the University of Adelaide has published in this area recently) and this literature should at least be cited.

Section 2.1.2. needs a little work. I really struggled to follow what was going on in the method for identifying the stopping point and had to read the text forensically. I think a flow chart is needed to support the text in 2.1.2 and the authors need to work the text up a little more to improve readability and consistency (e.g. the term 'validation set' creeps in here but this could be confused with the other 'sets' presented earlier). The use of a median value from an ensemble of 50 ANNs to avoid the influence of the random initialisation effect is sensible - but it does risk 'damping' the model outputs. It might be worth being explicit about the impacts that using an ensemble median might have.

Section 2.2. deals only briefly with the literature around knowledge extraction. Recent efforts based on partial derivatives have provided useful insights into the physical rationality of ANNs and should probably be mentioned at least (e.g. Mount et al., 2013. HESS, 17, 2827 / Dawson et al., 2014. Jnl Hydroinf. 16(2), 407). The KnoX method description is not particularly easy to follow - the 4 steps in the text should, perhaps, be revised to improve their readability and specificity. For example, step 3 states '...and

HESSD
12, C1346–C1350, 2015

calculate of the median of the absolute value of each parameter over the ensemble models'. What are these 'parameters'? Are they input values? Are they initialisation parameters? Are they values associated with the neurons of the network? This is all very unclear. The KnoX method is central to what follows so the authors really do need to revise this section fully and provide clarity. Again, a flow chart or schematic diagram could be helpful for all readers here.

Section 3.5 - please avoid statements such as 'fed by abundant rainfall'. What is the rainfall - please give measurements. One man's abundance is another's dearth. The legend on Figure 1 is not clear - is the conurbation the hatched area?

Section 4.1.1 presents the 'postulated model'. I find it somewhat unsatisfying and poorly argued. It relies on the author's previous papers but little evidence is offered to substantiate the spatial discretisation in the text. The inputs to the ANNs are simply the mean rainfall values in each of the four zones - as determined by Theissen polygons. This is a rather simplistic method for assigning rainfall inputs and does not account for the spatio-temporal heterogeneity of rainfall in the catchment. Is this a potential issue - I would imagine heterogeneity could be high in this catchment? RADAR-based rainfall data might be able to help answer this. What might the impact of such a simple assignment of model inputs be on the final model? Simply stating that you 'consider the rainfall information sufficient to carry out this study' (Pg 3695, Line 10) doesn't feel an adequate justification to me.

Section 4.2. The authors introduce the term 'window-width' in this section and it appears again in Table 3. I simply don't know what this is - I don't recall having seen it in the text before. Similarly, the authors appear to have experimented with developing models using various numbers of hidden neurons - but I don't recall this important process (the model complexity has a major influence on overfitting propensity) being presented in the text earlier. This leaves me rather confused and of the opinion that the methodological descriptions presented earlier in the paper have not been sufficiently clear or detailed enough. I simply can not read Figure 3 and this makes it very difficult

HESSD
12, C1346–C1350, 2015

to understand the ANN structure that has been used. Similarly, the model outputs in Figure 4 are too small to be useful - I can't see the hydrographs properly. I think that considerably more work is needed here to ensure that the methods and model structure are properly and fully described in the paper and that the model outputs that are being used to validate the model are adequately disclosed.

In section 4.3 the nature of the KnoX method becomes clearer, along with what the authors meant by 'parameters' earlier in the paper (they are the network weights). Knox is revealed as a method for determining the influence of each input, on the output, at each time step, based upon chaining of the network weights. The use of network weights to explore and quantify the contributions of different inputs is not particularly new. Work by Olden and Jackson (2002) and Kingston et al., (2003, 2006) (which has not been cited) is highly relevant because they did something rather similar. How does KnoX differ from this? Similarly, the quantification of the partial derivatives of MLPs (Mount et al., 2013 and Dawson et al., 2014 - see earlier citations in this report) are arguably more comprehensive methods for understanding the strength and pattern of influence of model inputs on the output response of an ANN. Again, why is KnoX a preferable method?

The discussion is simply a summary of the findings of the modelling. This section needs further development to contextualise the KnoX method, its value in hydrological modelling and how it contributes to the range of knowledge extraction methods that have been applied in ANN modelling (seee my earlier comments). Moreover, it would be helpful for the readership of HESS to have the contribution of this paper more clearly explained. To help with this, the authors might like to refer to Abrahart et al (2012) (Two decades of anarchy?, Progress in Phys Geog, 36(4), 480) to position their work within the framework set out therein.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 3681, 2015.