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Hydrology and Earth System Sciences Discussions

Interactive comment on "Technical Note: Using wavelet analyses on water depth time series to detect glacial influence in high-mountain hydrosystems" *by* S. Cauvy-Fraunié et al.

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General comments:

I have read in detail the technical note and the review of referee 1. The review of referee 1 is a very thorough review and I largely agree with all raised points and with the main conclusion, which is that the paper is not acceptable for publication in its current form. I even doubt that major revisions might bring it to an acceptable form. The main reason for this conclusion is that, just as reviewer 1, I do not see why wavelet analysis would be useful for the proposed objective, which is the quantification of the glacier influence on

observed streamflow. Why use a "complicated" spectral method with time-resolution if the time-resolution is thrown away for the index calculation? And more importantly: why not simply quantify directly the amplitude of the daily discharge cycles (in the time domain) if this is what the index is supposed to measure? Has this been done before?

Before giving some more detailed comments, I would like to emphasize here that continuous wavelet analysis seems a priori a promising tool to analyze glacier streamflow or water level time series - but it remains relatively difficult to make a quantitative use of wavelet power spectra and to quantitatively assess the features that are visible in the spectra. This certainly motivated the use of the global wavelet spectrum in this paper even if it is not clearly stated on p. 4377.

This difficulty arises from the expansion of a 1-D signal to a 2-D representation, which includes obviously redundant "information". Any statistical test of whether the features are significant or not has to account for this and is furthermore confronted with the problem of multiple testing. See the work of Maraun et al (2007) and the hydrologic application of Schaefli et al. (2007) for a state-of-the-art of significance testing. An example of how to make a quantitative use of wavelet power spectra for model performance assessment is presented in the work of Schaefli and Zehe (2007).

If the authors want to pursue their idea of using continuous wavelet transform to assess the degree of ice melt influence on streamflow, they should first develop a consistent quantitative method that makes use of the wavelet power spectrum (i.e. not of the global spectrum where time-resolution is lost). I see indeed some potential here (details hereafter) but developing such a quantitative method might be difficult.

Furthermore, I would like to re-iterate an important critic of reviewer 1: we lack information about the climate and the main drivers of hydrological processes. I wrote the comments below having my mental image of how high alpine glacier systems work, but this might be erroneous for glacier systems in the tropical Andes. How long is the ablation season here? Do the ablation and accumulation seasons overlap? Is there

melt the whole year around as suggested on p. 4381? Is ice melt temperature or radiation-driven?

Detailed comments:

Potential of continuous wavelet transform to detect ice melt influence

The authors state that there is ice melt the whole year around. I would expect that the intensity and the shape of the daily cycles vary throughout the ablation season as a function of the build-up of the glacier drainage system. For Alpine glacier catchments, a wavelet analysis of the time-varying amplitude and shape of daily streamflow cycles (see an example in Fig. 1 hereafter or the discussion in (Baumgartner and Liebscher, 1990, on p. 289)) could potentially be a promising way to detect the influence of a glacier on streamflow (under the hypothesis that runoff processes in the absence of a time-varying glacier drainage system would not lead to such a typical pattern). Developing a quantitative wavelet-based method to quantify this effect seems a difficult task; a time-domain method might well be more suitable.

Interest of the method

The study discusses that chemical signatures might not be sufficient to detect ice melt influence on streamflow and it reports glacier-influence on locations without obvious glacier cover. Even if I do not think that the proposed method is correct, this result and the potential existence of ice melt water reemergence justifies from my point-of-view the idea of developing a streamflow / water level-based ice melt detection-method. The question whether the reemerged water has the same ecologically-relevant features as direct ice melt remains, however, open.

Methodology - water level instead of streamflow

I am not sure that the use of water level, which is much easier to measure than streamflow, is really limiting (see reviewer comment 1). The main features of the daily streamflow cycles might well be preserved in the water level observations - however only if

the stage-discharge relation is not too strongly non-linear and if the cross-sections are not changing their shape throughout the measurement period. The first aspect would need some detailed analysis for at least one study location.

Methodology - background spectrum

I agree with reviewer 1 that the appropriate spectrum to test against should be a red-noise spectrum. I would, however, give a slightly different motivation. The objective of the significance test of the wavelet power spectrum is to decide whether the visible features could also simply arise from a purely random process. From this point of view, we might want to test against a random process that has similar features as the analyzed process at hand (see Schaefli et al., 2007). This reasoning obviously falls short for a process with strong cycles. In time series analysis, this cycles would typically be removed from the data before estimating the wavelet spectra - since we do not want to detect things that we see already in the time-domain (Schaefli et al., 2007). Reviewer 1 advances the fact that the errors of discharge observations are typically red-noise processes. While I am sure that discharge observations do not have Gaussian errors (given how they are estimated from stage-discharge observations), I am not quite sure whether they are really autocorrelated.

Methodology - Morlet wavelet

In its general form, the Morlet wavelet has a parameter that determines its time/scale resolution (e.g Maraun and Kurths, 2004). This parameter also determines the relation between the scale and the Fourier period (p. 4378).

Terminology

- I agree with the critical comment of reviewer 1 on the index terminology.

- p. 4387: "power spectrum of a time series" is misleading, it is the estimated power spectrum of the natural process.

- I would also suggest to pay attention to clearly distinguish between ice melt and

glacier runoff (including all sources of runoff from a glacier). As far as I can see, for the link between climate change and ecological processes (introduction), it is important to separate between water stemming from ice melt, from seasonal snow, perennial snow etc. The proposed method does not allow to separate them. Accordingly the method does not allow to assess the effect of climate warming as suggested in the abstract.

Method - SAGA

Does SAGA provide an algorithm to identify the stream network and the catchment? And if yes, how well does it perform? Any hydrological study published about this? Or should it be compared to a more well-known method such as Taudem (hydrology.usu.edu/taudem)?

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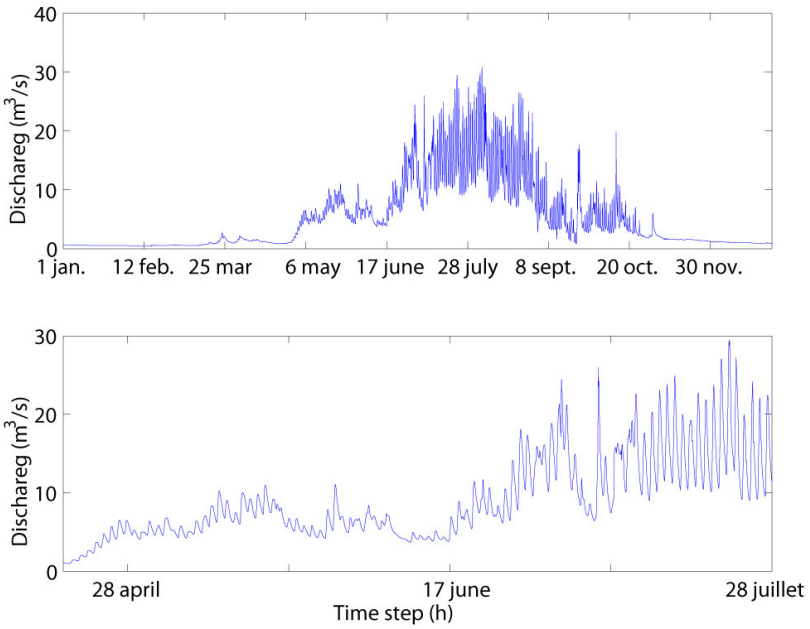

Fig. 1. Observed discharge of the Swiss Lonza river; top: a complete year, bottom: zoom on spring and early summer; source Hingray et al., 2009, data from the Swiss Federal Office for the Environment, http://