

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

Anonymous Referee #1

Received and published: 30 April 2013

Review of hess-2013-129 by Cauvy-Fraunié et al.

This short note proposes a time series analysis-based approach to characterizing and quantifying the degree of glacial influence in a watershed. The technique is based on wavelet analysis of river stage measurements. Wavelet power in the 24 hr band is used as the basis for a measure (which the authors dub the “Wavelet Glacier Signal,” WGS) of glacial melt effects. The technique is interesting, and potentially valuable given the downstream water resource implications of mountain glaciers, as well as the rapid change being experienced by most mountain glaciers worldwide. The topic

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



is certainly suitable for HESS. However, although the method seems promising, as it currently stands the paper is flawed and incomplete. Nevertheless, I believe there may be sufficient promise to the work that I will recommend that the manuscript be accepted pending major revisions. However, the changes required for publication are substantial and possibly fundamental.

I will focus my more detailed comments below on a few key issues. I hope the following points will help the authors improve their manuscript. Note that the references cited in the comments are listed at the end of the review.

(1) Although it is to be acknowledged that the paper is submitted as a note and conciseness is therefore important, the background provided in the “Introduction” and “Study sites” sections is nevertheless inadequate, being both simplistic and incomplete. There are two main problems. (a) More and better context and literature citation is required, so that a broad hydrologic audience can understand why the technical question under study is important and where they can go for some further information. For example, after the sentence ending with “...end of the glacial influence on outflow (Huss et al., 2008)” in the first paragraph of the introductory section, it would be useful to readers of a broad-based hydrology journal like HESS to add something like the following: “Statistical studies of long-term data from glacial and non-glacial catchments has demonstrated that streamflow responses to warming depend on whether glacial ice is present in the basin, and further, that glacial rivers have shown both increasing and decreasing trends, depending on the particular region and where it stands along the aforementioned deglaciation trajectory (Fleming and Clarke, 2003; Stahl and Moore, 2006; Casassa et al., 2009; Moore et al., 2009; Li et al., 2010; Fleming and Weber, 2012; Dahlke et al., 2012).” Also, after the last sentence of the first paragraph, I suggest adding something like, “An increasing number of studies have quantitatively explored the potential future impacts of various climate change and glacial recession scenarios upon water resources, using modern glaciological and hydrological modelling techniques (e.g., Stahl et al., 2008; Jost et al., 2012; Clarke et al., in press).

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Interactive
Comment

These studies and others have demonstrated that glacier change effects are likely to be hydrologically substantial, even in relatively lightly glaciated basins.” (b) Even more importantly, additional baseline hydroclimatic information is absolutely required about the study area, for readers to properly assess the scientific content and merit of the paper. We need to see basic background information like a graphical presentation of the typical annual cycles in river flow, air temperature, and precipitation within the study region; snow accumulation and melt information, in terms of both amount and timing, to the extent this information is available for the area; some basic weather and climatic influences, e.g., the general origins and types of weather patterns affecting the area (frontal vs. convective storms, for instance); and some sense of how the major sources of runoff evolve over the course of a typical year for these rivers (e.g., rainfall, melting of seasonal snowpack, melting of perennial snowfields and glaciers). Providing all of the requested additional background information would only require a few extra sentences and perhaps another figure or two, yet I believe it is key to improving the paper.

(2) Given the operations actually performed to calculate the so-called Wavelet Glacier Signal, it is unclear from the manuscript as written why wavelet analysis is used instead of simpler, standard, Fourier transform-based spectral analysis. On lines 14-16 of page 4377, the authors write, “To compare the spectral power of different stream sites, it was necessary to determine the global wavelet spectrum, which is the average of the local wavelet spectrum at every scale over the whole time series.” This quantity is then used to generate the WGS values lying at the heart of the study. Okay, but if that’s all that is needed, then why bother using wavelet analysis – the main advantage of which is to localize spectral content in time, information which it appears is never actually used for anything in the study? Why not just use much simpler Fourier power spectra instead, right from the start? Occam’s razor seems relevant here. A convincing justification has to be presented as to why to use the more complicated wavelet method (for this particular application).

(3) There appear to be some technical issues with the way the background spectrum

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

and statistical significance estimates are generated and reported. On lines 18-20 of p. 4378, it states, “Here, we chose the white-noise spectrum (at 95% confidence level) as we were particularly interested in measuring the significance of the wavelet power spectrum at one specific scale, namely 24 h.” There seem to be two technically substantial problems here. First, this statement (as written) is illogical: it seems to imply that the particular period one is investigating determines which background spectrum is to be assumed in the generation of significance levels. That is not at all correct. Rather, that assumption should be guided by the nature of the background noise, on the basis of either empirical or theoretical considerations. Second, the assumption of a white-noise background spectrum is almost certainly the wrong choice. It is well-known that river stage and discharge measurements, particularly those taken at a relatively high sampling rate (hourly or daily observations), such as is the case here, are strongly serially correlated in most rivers (even small, flashy catchments). Consequently, a red-noise background spectrum would seem to be a more justifiable choice for the particular type of application presented in this paper.

(4) Some of the nomenclature and definitions around the so-called Wavelet Glacier Signal (WGS) are problematic. The first issue may be in part a matter of personal preference, but I will flag it anyway: the world doesn't really need yet another three-letter acronym, so please don't call this quantity “WGS.” More broadly, every part of the term “WGS” seems slightly dodgy and cumbersome. As noted in point (2) above, the spectral power at the 24 hr band could equally well be determined using Fourier or other techniques, so it's not clear that the “wavelet” part quite captures the basic concept. Also, the Wavelet Glacier Signal doesn't necessarily have anything to do with glaciers at all – see point (5) immediately below – so “glacier” seems a bit off as well. And finally, “signal” doesn't quite describe the mathematical quantity in question here (the ratio of the spectral power in the one-day band to the value it would take on if it was statistically significant at a confidence level of 95%, if I understand the description correctly). I would suggest sticking to terminology which is a little more mathematically descriptive and narrowly correct. Perhaps something like “diurnal variation factor” or

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



“excess diurnal power” might work.

(5) The authors have not, in fact, made a convincing case that the Wavelet Glacier Signal – a measure of the strength of diurnal variability – is indeed a robust and specific measure of glacial influence. Snowmelt-dominated rivers will also show such a diurnal signal, at least up until the prior winter’s snowpack is gone. For basins containing mountain glaciers, it is virtually guaranteed that this will be a powerful source of ambiguity. Similarly, rivers in regions which experience regular convective storm activity over at least part of the year, e.g., summer afternoon thunderstorms and associated runoff (perhaps these Ecuadorian basins are an example of such a region, but we can’t tell because such basic context is not provided in the paper – see point (1) above) could also have a daily cycle. As a result, the so-called WGS does not appear to be a unique index of glacial influence. If one could rule out those other potential effects on different grounds – using river stage data only from the post-snowmelt season, or finding no evidence for a 24-hr spectral band in rainfall data – only then can WGS be reliably and confidently employed as a glacial influence indicator. Put another way, WGS is really just an index of whether river flows have a strong daily cycle, and it’s up to the user to attribute that signal. The authors found a statistically significant association between the amplitude of the WGS and % of the basin covered by glacial ice, but that is hardly surprising given the glacial region they picked and the fact that glacial melt does indeed impart a very strong diurnal signal during the melt season. This outcome is not, therefore, by itself a proof that WGS is a robust, unique, and precise indicator of degree of glacial influence. This issue seems to be a major problem with the work as presented here. However, I could perhaps see how the concepts used in this paper might be modified and evolved into something more useful and robust. A starting point might be to use the full wavelet analysis results (rather than just the time-averaged spectrum) to track seasonal changes in the WGS, and relate these to the major sources of runoff expected at different times of year for the different rivers (see again point (1) above). Another avenue might be to explore how the WGS relates to other data measured for these rivers, e.g., the water quality data listed in Table 1. Perhaps out of these anal-

Interactive
Comment

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



yses one might make some more reliable and defensible inferences about when and how WGS can be used to monitor glacial influence.

(6) Nowhere is it clarified why the Wavelet Glacier Signal is a better indicator of the degree of glacial influence on river flows than % of the basin area covered by glacial ice. Percent glacial cover is by far the most common, and almost certainly the easiest to generate and use, measure of glacial influence on watershed hydrology (and that's in all fields of study, including both geosciences and life sciences – note that the second paragraph in the introductory section seems mis-phrased in this respect). There is also a related problem with the second paragraph of the concluding section, which draws all sorts of comparisons except the most important one. Again, this is a key issue with the work presented here – it isn't made clear if or why this approach works better than the most common and probably easiest descriptor. Put another way, it seems the advantages, disadvantages, and potential role of the proposed index have to be thought through a bit more carefully.

(7) Some additional thought also seems useful around the choice to use river stage rather than river discharge as the basis for calculating WGS, and the implications of that choice. On the one hand, in practice, discharge is usually inferred from stage measurements using a stage-discharge (rating) curve. This inference is subject to error in the rating curve, so that in this sense there is some advantage to using stage data instead. Similarly, stage data are easier to come by than discharge data, being easily obtained using staff gauges or pressure transducers, whereas discharge data additionally require detailed velocity measurements under a range of conditions to generate a rating curve. On the other hand, stage at a certain location is generally determined by both the discharge delivered to that point from upstream, and the local hydraulic characteristics. Thus, it would seem that the local value of the WGS index as calculated in this paper should also reflect local channel geometry, bed roughness, etc. – that is, hydraulic controls entirely unrelated to the degree of glacial influence. Put another way, WGS determined from flow measurements may be more meaningfully

Interactive
Comment

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



comparable between different rivers or different locations on a given river, than WGS derived from stage measurements. Some additional reflection on this potential source of uncertainty seems necessary.

(8) The last paragraph of the concluding section seems poor. It over-reaches rather severely, I think. The passage also seems to imply that the notion of glacial rivers having diurnal discharge variations, known to scientists for a very long time and to others perhaps much longer, is some kind of relatively new and powerful discovery which must now be capitalized upon. I suggest deleting the passage. More broadly, the concluding section, especially the last paragraph, seems to highlight the general issues with the paper as discussed above. The idea of using empirical time series analysis to generate reliable, objective, and quantitative indices of environmental state is a great one, but the execution here appears somewhat flawed and incomplete – further thought and refinement is required around how the index is calculated, what it means, and how it can be correctly used.

References cited (this is a minimum list - the authors may wish to conduct some additional literature searches as well):

Casassa, G, and others (2009), Hydrological Processes, 23, 31–41. Clarke, GKC, and others (in press), Journal of Climate. Dahlke, HE, and others (2012), Hydrology and Earth System Science, 16, 2123-2141. Fleming, SW, and Clarke, GKC (2003), Canadian Water Resources Journal, 28, 69–86. Fleming, SW, and Weber, FA (2012), Journal of Hydrology, 470/471, 36–54. Jost, G, and others (2012), Hydrology and Earth System Science, 16, 849–860, 2012. Moore, RD, and others (2009), Hydrological Processes, 23, 42-61. Stahl, K, and Moore, RD (2006), Water Resources Research, 42, doi:10.1029/2006WR005022. Stahl, K, and others (2008), Water Resources Research, 44, doi:10.1029/2007WR005956.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 4369, 2013.

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)

