

Interactive comment on “Eddy-covariance data with low signal-to-noise ratio: time-lag determination, uncertainties and limit of detection” by B. Langford et al.

Anonymous Referee #2

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In my opinion, this manuscript from Langford et al. would make a welcome contribution to the eddy covariance flux literature. By performing simulations using sensible heat flux data, and using real-world flux data with low signal to noise, the authors carefully address the effect of instrument noise on flux errors. As the authors state themselves, the effect of instrument noise on flux measurements has been studied before, but this work contains many novel additions (in addition to demonstrating some established issues in very useful ways). In particular, I agree with the authors that this paper may be the first to systematically address the effects of using various automated post-processing methods to determine the time lag. The implications of their conclusions

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need to be considered by the community, and the paper closes with a nice list of worthy recommendations. I commend the authors for their work here.

In general, I have no major issues with the approach taken by the authors and my comments for improvement are minor. Overall the paper is written clearly enough and is very carefully-developed. However, the manuscript is very technical in nature, and I agree especially with Anonymous Referee #3 (comment posted on 26 March 2015) that it was difficult to follow in some places (which I try to outline specifically below). I believe this results generally from somewhat imprecise definitions of things like “signal to noise (SNR)” (when are the authors referring to instrumental mixing ratio “signal”, vs. measured flux “signal”); and “limit of detection (LoD)” (when are the authors referring to a limit based on the total random error, vs. just the error from instrumental white noise). It is actually interesting that, in the flux community, something as seemingly straightforward as “signal-to-noise ratio” should result in confusion. This is one reason why the present manuscript could make a significant contribution – but as it stands, some slight confusion remains.

Specific comments:

Section 1.2: Equation 1 and 2, the symbol “RE” is not explicitly defined (although it is implicit in the text that this represents “random error”).

Section 2.1: The authors state their focus is on unstructured white noise only, and here offer a definition of SNR (Equation 4). However, throughout the paper there is also discussion of “total” random error which includes structured noise. I would find it useful to see short comment on how the authors expect structured noise to affect the calculation/definition of SNR, and what the authors expect to see in the cross-covariance results and/or in the cospectra when structured noise is present. Furthermore, when is signal-to-noise in the manuscript determined from total random error, and not just instrumental white noise, if ever?

p.2922, lines 5-7: The authors state that it is necessary to extrapolate the autocovari-

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ance function back to the zero point, “typically using the first few points”. According to whom? Is this specific to flux applications, or is this used generally by others to establish any instrumental noise?

p.2924, lines 1: The authors state that the departure from the $-5/3$ slope at high frequencies may not show up in the auto-covariance approach. Do they mean in the frequency domain approach (Fig. 1b)? Or is the auto-covariance approach (Fig 1a) the same as the frequency domain approach (Fig 1b)? If so, I am confused at the distinction made in this paragraph (since the frequency domain approach is presented as an alternative to the auto-covariance approach).

Section 2.2: I believe this is where the authors first define a limit of detection “LoD” (multiplying the SD of $f_{sub_w’c’}(\tau)$ by 3). However, if I understand correctly, the authors settle on using a LoD based on the RMS approach for the rest of their manuscript, which is slightly different from the SD of $f_{w’c’}(\tau)$ approach. The definition and calculation of this “new” or “alternative” LoD (used throughout the rest of the manuscript) is not really explicitly defined nor formulated for the readers (including, if necessary, an equation for the RMS deviation of $f_{w’c’}$ from zero). Later, the “total random error” is defined as $3XRMS$ – but I didn’t find RMS to be adequately formulated itself.

p.2927, line 18: Is the -1 superscript in “(SNR-1)” a typo?

Section 3.1: Here the authors explicitly define “total random error” ($3XRMS$) and random instrument error ($REnoise$). When the authors later on refer to flux data points potentially being rejected for being “below conventional limits of detection”, is this always referring to the $3XRMS$? Are they ever referring to exceeding $3XREnoise$ (as determined by the Gaussian simulated data) alone? The later discussions of Figure 9 and 10 (e.g. Section 3.3.1) made me think they might only be considering the F_{sub_GN} (Gaussian simulated flux data) when determining whether a data point might be rejected.

Section 3.2.1: The “signal-to-noise ratio of the analyzer” Does this refer back to Equa-

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tion 4? I wondered if “signal to noise” here refers to the measured FLUX signal over the determined flux noise – but to me this is different than instrumental signal (i.e. mixing ratio measurement) and instrumental noise. Could the authors clarify?

p. 2930, line 14: I believe the “AVE” should be “AVG”

p. 2934, line 7: after “the red time traces” please insert “in Figure 9”. Moreover, here is where the authors refer to some data points exceeding or not exceeding the Gaussian white noise flux (or, “random sensor noise flux”) – but shouldn’t the determination of a true limit of detection be based on the “total random error” not just the Gaussian white noise flux?

Section 3.3.2: The shaded areas in Figure 11 are said to represent the average LoD. Here again, I am wondering whether this is based on the “total random error” calculation ($3 \times RMS?$) or simply the white noise/Gaussian flux/random sensor noise flux ($3XREnoise?$).

Figures:

In some figures, the “prescribed” approach to lag determination is abbreviated as “PRES” and in others it is abbreviated as “FIXED”. Please be consistent.

In the annotations that report a best-fit line, I am not convinced that 5 significant digits are necessary.

In general, I encourage the authors and editors to make sure that all the annotations in Figure 5, 9, and 11 are legible in the final version of the manuscript – They are currently very small, but it is not clear how legible they might be once finally implemented.

Figure 3: I agree that Figure 3 and its discussion in the text should be moved to the Supplemental Material, and shortened respectively.

Figure 8: Personally I thought this figure was unnecessary, too. The discussion is pretty clear without it.

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