

Interactive comment on “EddyUH: an advanced software package for eddy covariance flux calculation for a wide range of instrumentation and ecosystems” by I. Mammarella et al.

Anonymous Referee #2

Received and published: 14 March 2016

1) The title of this paper is inappropriate. Although the paper does provide a basic description of the EddyUH software, the primary purpose of the paper appears to be a comparison of the effect of different processing algorithms on fluxes from two example data sets.

2) The authors undoubtedly spent much effort to achieve the comparisons presented in this paper. However, whether the stated goal of “estimate(ing) the flux uncertainty due to the use of different software packages” (page 1 line16), and whether it is true that the “processing steps were consistent between the software packages” (page 1 line 21) is questionable. Instead what the authors present is a comparison of different algorithms, with an inevitable difference in the calculated fluxes. This problem appears

Printer-friendly version

Discussion paper



to stem primarily from the frequency response correction algorithms. There is no reason to believe that different algorithms for correcting frequency responses should give the same result, any more than we expect different coordinate rotation algorithms to result in the same velocity vectors. The best a software author can do is to provide access to as many algorithm versions as is practical, and to expose as many algorithms configuration options to the user – as is practical. However, if and where identical algorithms are present in the two compared softwares, then a true software comparison should be made, if only to ensure that a near identical result is obtained. This should extend beyond basic checking algorithm form as there may be other embedded assumptions in the software such as a molecular weight of methane may have a value of 16.00 in one software and 16.04 in another software, or one software may use single precision calculations while another uses double precision. Such situations may lead to differences in the results obtained from different software packages. This is perhaps what the authors refer to as “tuning” (page 2 line 22) and is actually quite important for the purpose of identifying software bugs, though perhaps not all that interesting when trying to publish a paper.

3) The authors also seem to imply that fluxes of CH₄ and N₂O inherently different from those of H₂O and CO₂, from a software perspective. They are all trace gas fluxes, and apart from sensor specific corrections, should be subject to the same processing stream. However, the poorer frequency response, or sampling path characteristics of these sensors may expose weaknesses in the algorithms applied for correcting these fluxes.

4) In section 2.1 the authors give a one page description of the software, which seems rather short and procedural considering that the software should be the main focus of the article. There is no discussion as to why it was designed in the way it was, or why its design may be an improvement over other software designs.

5) In section 2.4 a step-wise assessment is taken for comparing the effect of the algorithms in the two softwares by removing key correction steps. This is fine, but there

[Printer-friendly version](#)[Discussion paper](#)

appears to be no effort to account for the differences. One would assume that to compare the effect of an algorithm in the processing chain that all preceding a subsequent algorithms are behaving identically between the two software packages.

6) On page 7 line 6 the authors give the ‘best’ agreement between EddyUH and Eddypro, and then list five flux comparisons. Surely the best comparison will consist of a single flux comparison – unless by chance all five flux comparisons had exactly the same statistical result.

7) Page 7 line 9: did you instead mean “. . . no significant systematic differences. . .”

8) On page 7 line 12 you attribute the scatter to spurious, unrealistic spectral correction values. On what justification can you say that the corrections are unrealistic because they are more than $\sim 50\%$. While such large corrections may not be desirable, they certainly do occur and should be accounted for.

9) On page 7 line 29 you indicate a difference in the WPL corrections for the latent heat fluxes. Can the authors explain why the WPL correction should differ? If it is because the inputs to the WPL correction differ then that is another matter and attributable to a different processing step. If, however, the inputs are the same and still the WPL correction differs then you need to check your code for mistakes.

10) Similar to the last comment, the difference in humidity dependent lag times (Page 11 line 22) is quite interesting and potentially quite important but little explanation as to why there is a difference. Is it because the input data differ, or are there some presumable identical processes in the two softwares differing in some way.

11) On page 12 line 3 the authors attribute wet surface conditions as causing a large WPL water vapour term correction on the CO₂ fluxes. Wet surfaces do not cause a WPL correction, only water vapour fluxes.

12) The terms density correction, WPL correction, and dilution correction all appear in the paper. It might be best to just refer to the WPL correction as it encompasses both

[Printer-friendly version](#)[Discussion paper](#)

the dilution effect of evapotranspiration and the ideal gas law heating related volume effects. Further, I suggest the authors refer to a band-broadening correction instead of using the rather indistinct “spectroscopic correction”; which refers to any correction to an optically based measurement.

13) On page 13 line 7 the authors seem to be implying that frequency response corrections are ambiguous and not based on physical laws. I would suggest that this implication is largely untrue. Frequency response losses are very real physical processes and the algorithms used to correct for these losses are well reasoned for the conditions under which they apply. Similarly, the WPL term is a simplification of the actual process; it is also well reasoned and applies for most conditions we are likely to encounter – but it is not the true correction.

14) On page 13 line 24 the authors suggest a bias will occur as a result of an inappropriate cospectral model being used in the frequency response correction. As always, it must be the responsibility of the researcher to make sure that the applied correction is appropriate for the experimental conditions encountered. It seems that an appropriate software should allow the user the ability to choose a cospectral model appropriate for experimental conditions.

15) In the conclusions (lines 20- 22) the authors suggest “that a consistent choice of implemented methods for the post-field processing steps can minimize the systematic flux uncertainty due to the usage of difference software packages”. I find this to be a very dangerous conclusion to draw. This conclusion implies that there should be a uniformity in all flux calculations. Such a pressure to conform is likely to suppress the creativity and the willingness of researchers to explore the inherent variability of experimental field research under the presumption that all situations are identical. While I agree that algorithms should be employed as their originators intended; the concept that a prescribed set of instructions must be applied for a researchers fluxes to be considered acceptable is wholly unscientific.

[Printer-friendly version](#)[Discussion paper](#)

16) On page 15 line 19, Wilczak et al. 2001 did not present the sector-wise planar fit approach, only the planar-fit approach.

17) Appendix A, 'Calculation of turbulent fluxes': You present three methods of determining turbulent fluxes (block averaging, linear detrending, and autoregressive filtering) as if there were completely independent processes. However, for run based statistics block averaging is inherent in any of the approaches taken such that linear detrending and autoregressive filtering simply become filtering methods for removing additional low frequency energy.

18) Appendix A, 'Time lag determination and adjustment': Only a general description of lag time determination is given. No details on how lag time is selected for the many many situations in which the cross correlation curve does not show the obvious lag correlation peak.

19) Appendix A, 'Density correction': The authors indicate that this correction only applies for open path trace gas sensors. Actually, it applies for any sensor for which the sample gas is not at constant temperature, pressure or composition of other trace gas species. This should also be reflected in the processing chain shown in figure 2.

20) An important aspect that is missing from the analysis in this paper is the comparison of sensible heat flux computation. It is a vital component to many of the term and correction applied to the trace gas calculations shown in this paper and a such should warrant similar analysis.

21) There are several problems with the graphic in figure 1. A) the 'high frequency transfer function estimator' process has no outputs. B) the planar fit and footprint processes have no inputs C) the WPL and band broadening corrections have no representation D) Iteration is not presented.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2015-323, 2016.

[Printer-friendly version](#)[Discussion paper](#)