

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.

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1. General

First I must say that I'm never been convinced by the interest of publishing software inter-comparisons. Indeed, when differences between software appear, they are either due to errors in the software or to a difference in the use or the implementation of the computation procedures. In the first case, the solution is to correct the error in the concerned software and this does not deserve publication; in the second case, it is clearer and of much more interest for the readers to focus the discussion on the impact of these procedures themselves. When we performed the first software inter-comparison, in the frame of Euroflux, in 1996, we didn't see any relevance to publish these results but the

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exercise led us conclude to the necessity of a procedure clarification, which led finally to the publication of the Euroflux methodology paper.

This said, the present paper brings interesting material in that it points out some sources of uncertainty in the computation procedure. In this way, Figure 2 potentially presents a nice synthesis of these impacts. It would however be more useful to scientists if it was structured differently and rather focus on the impact of each computation step on the flux, in relation to the site and to the analyzer. It is also important to specify that the procedures that were identified as critical are those that were treated differently by the software. This list is thus strongly related to the software used and could be not exhaustive. It is also specific to the sites investigated.

I found of particular interest the discussion relative to the spectral correction. This point is important as I think that standardized procedures have not been proposed yet and their impact on the fluxes is large and do not concern LE only (see below). I would like to see this part expanded. Concerning the reference cospectrum, in a recent research, we pointed out that, at sites where local cospectra deviated from theoretical ones, the choice of the Kaimal cospectrum as reference could lead to an important overcorrection (in our case, it made the site switch from a carbon sink to a source!). A paper by Mamadou et al. is presently submitted to AFM on this point. I can provide it if you are interested.

Concerning the theoretical approach, I'm not in favor of its use, at least for closed path. Indeed this approach does not take all high cut filtering processes into account. As an example, it was found recently that the presence of a rain cap at the tube inlet could affect greatly the system cut off frequency. An enclosed system with a 25 mL rain cap would never provide cut off frequencies larger than 1 Hz at 15 L/min! This was established recently by two teams who published in AMTD (Aubinet et al, Atmos. Meas. Tech. Discuss., 8, 10735–10754, 2015; Metzger et al, Atmos. Meas. Tech. Discuss., 8, 10983–11028, 2015). At present, this effect was not taken into account by theoretical functions which therefore should underestimate the spectral correction factor. An

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experimental approach appears always preferable to me as it provide validated transfer functions.

2. Clarifications needed I think that some concepts presented in the paper need to be simplified or clarified. I suppose that “spectroscopic correction” refers to the additional cross-sensitivity of some analyzers to water vapor. This is really an instrumental effect, due to collisional broadening of absorption lines, and it affects all analyzers. The importance of this effect depends on the measurement technique and on the absorption line of concern. It potentially affects both closed and open analyzers and is physically independent of dilution (density?) corrections. I thus think that it could be misleading to associate systematically density and spectroscopic corrections (even if the correction procedure often mixes the two).

In addition, the figure 2 suggests that spectroscopic correction would not be necessary for the closed path systems, which is wrong to my opinion (I suppose that it is included in the conversion to molar fraction). The necessity to apply or not this correction depends mainly of the analyzer: some manufacturers include the correction in the analyzer software (Aerodyne, for example); some do not and suggest a way to implement it like did McDermitt or Rella.

I think that the authors present WPL (or density or dilution) corrections in a too complicated way. First they use different terms (WPL, dilution or density correction) to characterize what appears to be the same correction to me. Maybe do some expressions refer to only one of the WPL term – density to the H term and dilution to the LE term? If it is the case, this should be clearly stated. Secondly, the effect of WPL correction is exactly the same as the conversion to dry mole fraction. The fact that one or the other procedure is used does not depend on the fact that the analyzer is an open or closed path but mainly on the availability of high frequency measurements of water vapor and temperature (or, in closed paths, the fact that high frequency temperature fluctuations are supposed negligible).

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Finally, the case of the LI-7200 is specific as it is not really a closed path, so that “H” term of WPL (or conversion to molar fraction considering air density fluctuations) is necessary but it must be computed on the basis of chamber temperature and not on air temperature. Uncertainties linked to this specific case deserve a discussion. Figure 2 should also be adapted to take this into account.

Some miscellaneous clarification required: P6L9: Spectroscopic correction is sometimes called line broadening correction. Please harmonize. P18L32 P18L23 ISO 8000-9 recommends using the term “Density” only to characterize single components. In the present case, the right term is “molar concentration”. The sign convention could lead to confusion: equations 1-4 suggest that micrometeorological sign convention is followed, i.e. downward fluxes are considered as negative and upward fluxes as positive. However, this is contradicted in Figure 3 where FCO₂ are considered as positive. As a result, an expression like “a 7% higher FCO₂ flux” (P11L27) appears ambiguous: does it finally mean that the sink is over or underestimated?

3. Some additional comments on the chapters 3.1 Material and methods The material and methods part is imbalanced as it only presents EddyUH and not Eddy Pro. As said before, the main differences between the results will come from differences in computation procedure and it is therefore important to understand what these differences are. In addition this part rather looks as an instruction leaflet to the UH program and is of few utility for the present analysis. I thus suggest that this part presents the computation procedures implemented by the two software and focuses on their differences. If the authors find necessary to give an organigram, as in Figure 1, it should be given for both software.

I agree with the experimental protocol that consists in performing different runs, introducing progressively the computation steps. This is indeed a good way to estimate the impact of each individual step.

3.2 Results The result presentation is a little bit intricate and difficult to follow in that it

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mixes two sites, four systems, two software and seven to eight correction/computation procedures. I suggest to better organize the presentation and to focus on the impact on flux of each correction procedure according to site characteristics and analyzer type.

3.3 Discussion and conclusions Here again, this part would benefit from a reorganization focusing on the impact of computation procedures on the fluxes rather than on a software comparison. In particular, in the conclusion (P14 L23), if I agree that further methodical researches are still necessary, to my opinion, they don't have to focus on software inter-comparison but rather on these impacts and contemplate a larger spectrum of sites (different ecosystem types, different climates).

4. Miscellaneous P6 L27-28: Units are probably not correct (not the same for each flux). Please clarify. P7 L8-9 : I suggest to rephrase : “was obtained for LE and FCO₂, measured by LI-7000, LE and FCH₄ by G1301-f, at Siikaneva (Figs. 3d,e and 3c,a) and FCO₂ by LI-7200 at Erottaja (Fig. 3i).” P12L15 : not really the ecosystem type : more specifically the LE importance. P22 : reference to Rella is incomplete. Fig 8 : CO₂ and H₂O curves are difficult to differentiate.

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