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Interactive comment on "OzFlux Data: Network integration from collection to curation" *by* Peter Isaac et al.

Anonymous Referee #1

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The paper provides a thorough description of the OzFlux data processing pipeline and the OzFluxQC software toolkit, without being overly detailed. It also presents interesting comparisons of Net Ecosystem Exchange and derived data products such as Ecosystem Respiration, highlighting effects from using different methods from the literature to create these products. It is clear that the task of creating such comparisons is considerably simplified by the availability of uniform data formats and tools such as the ones described in the paper. Furthermore, the information in this paper provides invaluable insight for this Special Edition of Biogeosciences on OzFlux. On the negative side, some of the analyses and results drawn in the paper could be seen as not being fully supported by the evidence presented, in particular the comparison of data products. It seems likely that additional information has to be included to properly support conclusions or at least more detailed clarification on limitations and assumptions

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Discussion paper

should be added. From the presentation point of view, the paper is very well written and properly structured, requiring only minor corrections.

Besides the detailed explanation of methods used in the OzFlux data processing pipeline, one of the most interesting contributions of the paper is the comparison of the effects of selecting different methods to generate a data product. This is a very relevant problem to the eddy covariance community, especially when it comes to improving repeatability of experiments. With that in mind, a question that naturally comes to mind is: how can the authors guarantee that the differences observed are due to differences in the methods and not in the implementation/coding of these methods? Many attempts to replicate experimental results from a paper using a re-implementation of a data processing method will lead to very different results. In many cases, this happens for reasons ranging from differences in the selection of parameter that was deemed minor (like a window size for gap filling) to misinterpretation of non-algorithmic portions of methods (like thresholds or exception cases). One of the ways to assess these differences can be to compare the results from a new implementation against results for the same input data using the original code. For some of the methods, the authors indicate they had access to the original code (e.g., the SOLO ANN method), but there is no indication of cross-validation of results. What about the other methods? This is not to say the differences pointed out by the authors are not due to differences in the methods – this actually seems the most likely explanation to me – but it's not clear the possibility of implementation differences was ruled out.

In a somewhat related note, how "the redundant steps at L3" are skipped when already applied is not very clear. Is this done from metadata generated by other software packages, does OzFluxQC try to determine that from the data, or is this dependent on user input? This is particularly relevant in the comparison with EddyPro results. How can the results really be equivalent if steps such as filtering based on stationarity checks (mentioned in the paper) are not applied? At the same time, comparing results from EddyPro using processing options selected to match what is being done by OzFluxQC

(when getting datalogger data directly) does not seem like a fair comparison. All the disabled options that would ideally be applied for a site's dataset can have significant impact on the computation of fluxes. It seems to me that for this comparison to be complete (and for the conclusion that both options can indeed be used interchangeably), many more checks would have to be done. This should cover looking at effects of enabling recommended corrections in EddyPro that are not available in OzFluxQC, and quantifying differences. It would also potentially involve looking at a range of sites and site conditions and the corresponding effects. That said, to me this should not be the focus of the paper. So my suggestion is to remove this comparison (leaving the detailed version for a follow-up paper), and focus on exploring the NEE/RE/GPP comparisons in more detail as suggested above. This would entail removing the paragraphs that make up section 5.2 and Figure 8, which would not impact the paper significantly, in my opinion.

The following comments are related to other minor issues.

It is not clear in the paper how the alternative data from AWS, ACCESS-R and ERA-Interim are reconciled and used in the gap filling process. How one (or more?) of the three sources are selected and used for gap filling of driver variables?

Also on gap filling, at the end of section 3.5, the issue of gaps being close to the window size can seriously restrict the applicability of gap filling methods. A bit more detail on the "minimum points criteria" mentioned in the paper would be very helpful in clarifying how this problem is addressed.

At the end of section 3.6, there is a description of final NEE, ER, and GPP time series, from what seems to be a combination of the results or the three used for partitioning. However, the same last few lines seem to indicate that NEE and GPP obtained from the hyperbolic light response curve method are not included in this these final versions of the variable. Is this correct? If not, how is the light response curve results incorporated? If yes, it's not clear why this method would be included in OzFluxQC (and in the paper).

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The sentence below (from page 5, lines 23-24) is a bit awkward and could be rephrased for clarity: "The large range in precipitation amount and seasonality has resulted in a large range of biomes across Australia and OzFlux samples the majority"

Regarding the use of the CF Conventions, how are variables not covered in the current version of the conventions handled? This is probably worth one or two sentences for clarification.

For all gap filling comparison plots, it would be relevant to include indications of the amounts of missing data. These can be good indicators of variability in the data (or lack of variability, for some gap filling methods).

Also on the plots that use color, adding a color legend would make the plots considerably more readable compared to the text descriptions of the color associations in the figure captions.

On page 14, line 27: "drives" should be "drivers"

Section 4, which describes the data portal and data distribution, somewhat breaks the flow of the paper. Maybe moving the text in this section to section 2 would make the discussion on the data pipeline more fluid.

Finally, there is an issue that is not necessarily a discussion for this paper, but very relevant. There is a natural conflict between trying to encourage site teams to "know thy site" while providing the comfort of using standard (and vetted) software toolkit. In many cases, even experienced eddy covariance researchers will be tempted to not worry or question much about the results obtained using highly automated tools. Offering visual presentation of intermediate results, as done by OzFluxQC, is certainly a step in the right direction and is appreciated.

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