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Geoscientific Model Development Discussions

Interactive comment on "Land surface parameter optimisation through data assimilation: the adJULES system" *by* Nina M. Raoult et al.

Anonymous Referee #2

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Review of paper "Land surface parameter optimisation through data assimilation: the adJULES system by Raoult et al.

Thank you for inviting me to review this paper.

Data Assimilation is often talked about in meteorological/climate modelling, and indeed is used in things like the ECMWF re-analysis products. But this is one of the first applications to land surface modelling, and so this paper is particularly important.

The reference list is comprehensive and useful, alongside good illustrative diagrams and a useful table across the FLUXNET sites.

The paper should be published in GMD, and below are a few suggestions the authors might like to consider in any final refined version. It's a long list of many points, some

almost trivial – if the paper was printed in its current form, that would probably be OK. But there may be a few things below that if the authors adopt, may just sharpen the document.

I am happy to see the paper again as a reviewer.

General things

The Abstract is clear, although just reading the Abstract, a question might be asked as to why the data is not split in to training and test data?

Maybe expand just slightly on "a third of which give similar reduction in errors as site specific optimisations". The point being made here is that this suggests parameters are similar and robust between sites. This is always good news for climate modelling, suggesting it is possible to reduce to relatively small numbers of PFTs. Maybe stress this point a bit more? (However, if this is stressed more, then need to explain Groenendijk et al 2010?).

Lines 34, page 2 – Lines 3, page 3. This feels as if it undersells the adjoint approach! I would make a key bullet point that this is a more sophisticated approach (via matrix inversion) to finding rapidly minima across multiple parameters. It would be almost impossible to replicate these findings using some sort of brute-force optimisation, with nested loops over different parameters.

Around Eqn (1), line 11. Sentence "A cost function f(z)…" looks like it has remained in by accident, and then the correct sentence is the next one. "The cost consists…." (The second sentence correctly identifies that the z-z0 differences also contribute to cost function in Eqn(1)).

Eqn(1) – Has Lambda been accidently dropped from Eqn (1) . It should multiple the second term? (I realise line 21 states it is taken as unity, but I'd still put it in Eqn(1), and state line 21 "All parameters and observations are equally weighted in this cost function – i.e. lambda=1"

Is there a good reason for selecting lambda=1 (or its implications)? Does it imply we put equal trust in the FLXUNET measurements (left term) as the local measurements that give the local parameters (right term). A couple of words on this might help the reader?

Possibly me being confused, but if B is a diagonal matrix, then this isn't about co-variances – which imply off-diagonal terms?

Section "Multisite Implementation". Could tighten slightly to say something like "and this would introduce a double summation in Eqn(1), over n locations. Hence R and B become matrices of size [n*s x n*s]". Is that correct? This would fit with, as stated, to find "values for a common set of parameters". This gives single values for each z parameter. The wording of the last sentence is slightly ambiguous? "Similarly, the first and second derivative. . ...using the sum of the derivatives at the individual sites". This reads as if the derivatives are calculated locally, and then a mean taken. Would in fact a single sweep across all n*s data points be used, and the derivatives calculated once, if common parameters are investigated. [Maybe eqn term cancellation implies they are the same, but. . ...?].

Somewhere in Section 2.3 or 2.4 – possibly remind readers that FLUXNET also comes with the meteorological data. (In other words, it's not just the fluxes and then something like NCEP or ECMWF data was used additionally to give met drivers).

Section 2.5.2. Would need to be confident that outlier points didn't do something odd in Eqn (4)? I guess the initial sweep of data ensures this is OK? (The alternative would be to normalise with SD of (mi,t)), and then get the percentage of variance explained).

Figure 3. This is a great figure. However, pictures and captions often get pulled out of papers and shown in isolation. To ensure information is safely contained, mention in caption (or across top of plot), these are broadleaf trees only? At first glance, I thought the y-axis was some sort of physical unit (for LE or NPP). However caption says this is from the 2.5.2 Section metric. The normalised values are very small, and don't ever get

near unity. Could this be the outliers mentioned above? Not a problem, but bottom of page 7 says "1 – a complete mismatch" Wouldn't we expect some of the parameters to perform quite badly, and get a bit nearer to unity? Or - does this mean that in general, even without parameter fitting, then JULES is an exceptionally good model? Fitting reduces that last small error down further?

Figure 3 mentions training versus validation. This appears different to the impression of the Abstract that all data is used to train?

Figure 3. Usual practice is to put the legend inside the plot – there is space for the 5 symbols, top left hand maybe?

Figure 4 is great. But on my print out, the vertical lines cannot be seen in many instances. Thicken them maybe? As always, a matter of style, so just a suggestion. To make Figure 4 less crowded, would it make sense to not put the value of original & optimised as text annotations as this repeats information in the plots. Then the plots can be made bigger and bolder? Maybe put units in left column?

Section 3.3 and Figure 5. What is so remarkable about Figure 5 is that the strong correlations between parameters are not consistent across the PFTs. Maybe not for this paper, but some sort of physical interpretation of that would be really interesting. Returning to the governing equations and their scaled amounts might help. Is "correlation" the best word – "collinearity" might be more appropriate?

Figure 6 is like Figure 3, but a lot less cluttered. The data on Figure 6, BTs is same as that on Fig 3, except the "multi-site". Looking back at Fig 3, need to understand better the "five sites" algorithm (again, page 10, line 4 – some text accidently deleted?).

Figure 6 – I'd make the lower y-axis bound 0.0 (rather than what looks like 0.001)? Gives a better feel then of the improvement in absolute terms.

Conclusions – To my eye, Figure 6 says it all, and I would stress far more the real headline findings that:

(!) There is a general reduction in error of around 50%

(2) Possibly of more importance, using cross-PFT parameters, often get very similar improvements than local fits. This implies robust parameterisations independent of geography – which GCM modellers always like to see.

Small things

Maybe get the words “Data Assimilation” used a few times on the paper on page 1 / Abstract, so it gets picked up for anyone using that expression in an Internet search. (It’s an older terminology used for this sort of approach, but is still valid).

Abstract: Line 2, maybe mention that JULES is also used comprehensively as an impacts tool, sometimes forced with known climatologies and/or alternative GCMs in to the future. So it is not used just coupled to UK Met Office models.

Abstract - could “automatically differentiated” be expanded slightly to “automatically differentiated with respect to JULES parameters. . .. . .”

Maybe line 25, page 1. To make topical post COP21, could add something like: “Any future decreased ability of the land surface to draw-down atmospheric CO2 could imply fewer “permissible emissions” in order to stay below key warming thresholds such as two degrees”

Top page 2. Is there a process other than nitrogen cycling that can be mentioned – preferably one that has been introduced in to the JULES model version used here?

Sentence “Given the small spatial footprint. . ..”. Maybe clarify why this gives over-tuning? Presumably because it might be see some anomalous plants in the small footprint, and that are not representative of PFTs over a broader area?

Bottom of page 3. Line 30. Could mention that “available observations” are about independent large-scale measurements (such as FLUXNET)? These are different to the specific process measurements used to calibrate the individual components that

are mentioned in line 27.

Page 4, line 5. Possibly: “As used widely in weather forecasting, along with other disciplines”.

Page 4, line 14. Maybe: “ . . ..outputs at time 1 <= t <= s” (so defines s).

Page 4, line 22. To anyone new to data assimilation, could say: “optimal vector. . ..minimizes the cost function (Eq. 1) via JULES model itself though m=m(z) (left terms) and directly via z in the right terms”

Page 4, line 25. I can understand box constraints = upper or lower bounds. But what does “limited memory” refer to?

On the diagram, Figure 1, right-hand side, maybe word as “Hessian to give uncertainty bounds”

Page 5, line 14. Table A1 has a lot fewer than 500 entries, so quite a lot of data is rejected?

Page 7, line15. “One or two” refers to whether a plot is a standard plot, or a contour plot in two parameters?

Page 8, line 5. The analysis here is more testing the concept of common parameters between sites, rather than testing the methodology?

Figure 2 is really nice. Just a few small things. Is there are reason black is also dashed? Style thing, but I’ve have maybe put as the individual panel titles “Broadleaf LE, Broadleaf GPP”, etc. So across the top of the panels. And then the y-axes, put the units – so W/m2 left panels etc. Then possibly not bothered with the labels (a)-(e)? Maybe make the lines with slightly larger line width? Inside each box, give the site ID as annotated text, as these are timeseries for just single sites.

Page 10, line 4. Some text missing from sentence?

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