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Interactive comment on "Interannual variability in Australia's terrestrial carbon cycle constrained by multiple observation types" *by* Cathy M. Trudinger et al.

Anonymous Referee #2

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Trudinger et al. present a modeling study investigating the range in inter-annual anomalies (or inter-annual variability – IAV) in net ecosystem production (NEP) in Australia due to parameter uncertainty and some further adaptations to the existing model set-up, namely a new function for the impact of soil moisture on soil respiration.

The work is extensive, interesting and of relevance to the community, especially given the recent finding that semi-arid regions are likely the dominant driver of the IAV and trend in the global C sink, as the authors detail nicely in the introduction. One of the main conclusions that the authors highlight, that there may be a range of values for the NEP anomalies that are dependent on parameter uncertainty, and therefore that we cannot just use one optimised parameter set, is an important point to make.

However, I have several concerns about the description of the overall objectives, and how the experiments link to these objectives, that I have laid out below. In my comments I have tried made some suggestions that I feel would improve the manuscript in these respects.

The objectives could be more clearly laid out in the introduction, especially given the number of different tests (forcing, model structure and optimisation set-up with different observations) that are then detailed, which I feel could lead to the reader feeling confused as to what the overall aims are. For example, stating (in the introduction), "...we use BIOS-2 to explore inter-annual variability in NEP..." lacks some detail. "We are also interested in the effect of uncertainty in model parameters on modelled IAV" is more clear, but it might be better to formulate some key objectives as bullet points or questions and then directly link the results to these objectives.

I like the description of the issues related to parameters that can and cannot be well constrained by the optimisation in Section 2.4 (p6 lines ∼10-20). And I think the aim of producing a range of estimates of NEP anomalies based on parameter uncertainty/equifinality is a good one. However I think it is important to detail what you mean by "parameters that are not informed by the calibration dataset". Equifinality can occur for two reasons (broadly speaking): i) parameters to which the model variables used in the optimisation are not sensitive (therefore will not be constrained by the optimisation), and ii) parameters to which the model variables are sensitive, but which are correlated with other parameters. In this latter case it is possible that the parameters will be well constrained, but to the "wrong" values (i.e. if you did a test with psuedo observations where you knew the right value for the parameters, the posterior value would be well-constrained but not correct, possibly due to the particular noise realisations in the given set of observations. In my understanding, parameter equifinality mostly Are you trying to account for both types in your ensembles? From p6 lines 28-29 (point i) it seems that perhaps just the first type? Also, it isn't clear to me that you are also accounting for uncertainty that still remains in parameters that have been well-constrained (but not perfectly) by the optimisation – i.e. calculated from the posterior parameter covariance matrix. Given these questions, I think the manuscript would benefit considerably from a more detailed description of exactly what uncertainty is represented in the parameter ensembles that are generated using the null point Monte Carlo method (p6 lines ~21-34).

Given your focus on the inter-annual anomalies, I think it would be good to have a plot showing the inter-annual variability of the observations, prior and posterior, perhaps as a bar graph of mean year to year anomalies, in order to asses how (if) the optimisation improves the modeling of the inter-annual anomalies. It is not very easy to evaluate this from the annual time series plots, and it is not shown on Figure 9. It is not clear that the simulations of the IAV are improved by all this assimilation work, which was a question I had when I started reading the paper, given previous studies have shown that parameter optimisation can improve the fit to seasonal fluxes but not necessarily to the IAV (Kuppel et al. 2012). Indeed on page 11 Line24 you state that the NEP anomalies (the key topic under investigation in this study) are not significantly improved by the optimisation, yet the majority of the paper is about the different optimisation tests, rather than an investigation of the IAV simulated by the model after the optimisation. Therefore at times I felt like I was reading a study about different optimisation configurations, and the link to the IAV was missing (until the discussion). There is a lot of information in Figures 9, S14 and 15 that has not been discussed and could contribute greatly to the discussion on IAV. The authors mention that it appears that NPP is the main driver but most of the uncertainty is in the heterotrophic respiration. This is a useful conclusion but I feel a greater discussion of this result would add value to this study (I have made further comments on this in the minor comments below).

Given that the optimisation does not dramatically improve the IAV I was left feeling the paper was somewhat disjointed in its objectives. Also, the fact that the optimisation does not improve the simulation of anomalies is a key finding and could be discussed more. What could the modeling community do to better optimise the model/simulate

the NEP anomalies? Some of the tests, for example the focus on the soil moisture affect on soil respiration, the contribution of different data streams, are not well linked to the initial objectives that are laid out. For example it was unclear to me why there is a focus on the soil respiration function despite the study of Exbrayat et al. (2013), particularly because in the introduction the authors discuss that previous studies have shown that NPP is the key driver. While obviously it would appear this function is important to investigate in terms of the development of the model in general, it was not clear that this was crucial for this study investigating the NEP anomalies. I would detail more clearly up front (in the introduction) why you have included different aspects (including the soil respiration function) that haven't been included in previous modeling studies with BIOS-2, i.e. explain (as you do later in the discussion p11 lines 30-35), that the purpose is to have best model set-up, otherwise I'm searching for a reason as to why these tests are particularly important for simulating the NEP anomalies.

This is maybe a style issue, but to deal with the issues raised in the previous comments, I think it may be better to merge some of the discussion into the results. I found the "story" of the manuscript came together more in the discussion, but the description of results seemed incomplete at times or in need of interpretation. As already mentioned I was left wondering how each section (that may have contained a very nice test in itself) fitted in with the overall goals of better quantifying the NEP anomalies, instead of just being an interesting parameter optimisation test bed.

Minor comments

Why did you use monthly observations when I presume daily (or half hourly) fluxes are available? I guess this is because you are focusing on seasonal to annual (inter-annual) timescales but this needs a justification. Out of interest have you done a test with daily observations to see if the optimisation of IAV is improved? Similarly, why were long-term means used for streamflow data?

How were the observation error correlations taken into account? This will be a particular issue for GPP observations that were derived from the NEP eddy covariance data via a flux partitioning method.

Figure 2: it would be informative to see prior and posterior RMSE or R, some metric to show the improvement.

Does Figure 4 only show the posterior results?

It might be good to put which observations are used to optimise the CABLE and CASA-CNP parameters in Table 1 and 2.

Section 2.3: do you mean evaporation or evapotranspiration measurements were used? In the figures you say "ET", which is often used to denote the latter.

P5 Line 30: define the PEST acronym (it might also be better to put the website here instead of later).

Section 2.4: it might be good to describe briefly what the "down-gradient" method is for the non-specialist.

P6 Line 21: How did you define the larger number of parameters from the initial SA used in the Haverd et al. (2013b) study?

P6 Line 35 to next page: apologies if I've missed this, but why 21 parameter sets for CABLE and 19 for CASA-CNP?

P7 Line 35: What does this tell us? It shouldn't be the case that an optimisation with one data stream degrades the fit to another if the models are consistent with each other and with the observations, and the prior error covariance matrix are properly characterised.

P8 Line 5: "likely attributable" → can you do a test to quantify this further, given the focus on this new function?

Figure 6b: Is this for each observation type when it is used on its own, or when it is left

out (as I understand for Figure 6a)?

As mentioned above, Section 3.2 is one example of a section that lacks some more detailed description, rather than just a summary and explanation of the figures and how to interpret them (which is also useful). Aside from NPP, which observations are useful for which processes (even qualititatively, or using a summary metric), particularly in reference to constraining the IAV? There is also no description of Figure S11.

Figure 7: is this the total model-data mismatch using all observations?

P9 Line 1: parameter identifiaility → I like this analysis, but one could also simply look at the parameter posterior covariance matrix which may be easier for some readers to follow as an initial example of parameter correlation.

P9 Line 10: Figures 12 and 13, not 11 and 12.

P10 Line 1: We don't know that there is only a little difference in the misfit function (when looking at fig. 11a) if we don't know what the prior misfit is.

P10 Lines 13-15 (Section 3.5): would be good to have a reference to a figure or table here.

P11 Line 16: Is it really clear or fair to say that the parameters related to heterotrophic respiration are particularly important when you have introduced a new function for the impact of soil moisture on respiration which has quite a number of parameters? Is it surprising that you have more equifinality for heterotrophic respiration given this new function? It would be good to describe this more in the results because at this point I was wondering if this was a clear conclusion of the analysis (i.e. that the heterotrophic respiration-related parameters are particularly important). A link to a figure at this point in the discussion may also help.

P11 Lines 21-22: Similar point to above. It is not immediately obvious to me, looking at Figure 9, S14 and S15 that the NEP anomaly is dominated by equifinality in the heterotrophic respiration. It depends which ecosystem contributes to the anomaly, which was only briefly discussed. Looking at Australia overall for 2011, it looks like the uncertainty due to parameter equifinality is the roughly the same for NPP and heterotrophic respiration. I think the same can be said for the ecosystems the authors say below contribute most to the NEP anomalies (savanna and sparse vegetation).

In general all the routines used from PEST should be described in more detail somewhere (supplementary?).

Kuppel, S., Peylin, P., Chevallier, F., Bacour, C., Maignan, F., and Richardson, A. D.: Constraining a global ecosystem model with multi-site eddy-covariance data, Biogeosciences, 9, 3757-3776, doi: 10.5194/bg-9-3757-2012.

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