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Interactive comment on “RECCAP uncertainty” by I. G. Enting et al.

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Response to review comments; RECCAP Uncertainty

General response

The valuable overview comments from the two anonymous reviewers query the scope of the paper and its place within RECCAP and broader studies in biogeosciences and beyond. Within RECCAP, we see a tension between the need to have:

- 1: an initial framework for uncertainty that is adopted in all RECCAP component studies;
- 2: an overall wrap-up of uncertainty at the end of the RECCAP process.

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Given that the RECCAP plan provides for only one paper on uncertainty (and this was a relatively late addition to the plan) there is only limited scope for resolving this conflict. The main role of this paper is to document (and where appropriate extend) ideas that were circulated in an unpublished RECCAP discussion document and presented in several talks at RECCAP meetings – role (1). The RECCAP plan envisages synthesis papers that will hopefully build on this to achieve aim (2), but that is not the role of this paper, and given the late submission of many components of RECCAP, this paper cannot take on that role.

Our view is that there will always be a lot of learning in projects like this and that this is OK. It is only by actually doing a project of this kind that one comes to appreciate all the problems. In the development stages, a number of compromises may be needed. However we feel that exposing the activity, with all its limitations, to the total science community will be more productive than having a small group beavering away for a long time in isolation, releasing nothing until meeting some ideal standard. As we noted, we base this view on past experience — in point 32 below reviewer 2 asks us for details and some examples are included in our detailed response.

Any decision on whether to have a small delay in submitting our final paper, while waiting for other RECCAP papers to be published as discussions, is something on which we would be guided by the editors of the special issue. Similarly, we would be guided by the editor(s) on whether expansion of the abbreviation 'RECCAP' is needed in both the title of the special issue and in titles of individual papers. A proposed alternative title would be: Carbon Cycle Uncertainty in the REgional Carbon Cycle Assessment and Processes (RECCAP).

Detailed response

We reproduce the reviewer comments with *our responses in slant font*.

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This paper discusses the evaluation of uncertainties in the context of a specific carbon cycle effort called RECCAP. While the treatment of uncertainties is relevant to a wide range of geo-scientific questions, and this paper contains useful information and concepts about it, it seems to be written with only the RECCAP initiative in mind and consequently does likely not attract the broader readership of Biogeosciences it deserves. This starts with the title and the abstract which includes the acronym RECCAP without further explanation. But also throughout the focus is on RECCAP which is a pity. I suggest the authors to carefully go through the document and evaluate the different aspects of uncertainties with the following questions: "What part of this aspect is relevant for the whole biogeosciences community? What kind of approaches do already exist? What part is specifically relevant to carbon cycle science and RECCAP?". This does not involve a complete restructuring or rewriting, but rather adding short relevant statements in the respective paragraphs. The abstract should be broader in scope and a more general introduction sought for.

Moreover, when describing the RECCAP activities and products, it seems a more precise description would be desirable. E.g. the Ocean part is only cited as "pers comm.". On page 1852 - as a synthesis I would expect a table with the concrete properties of the RECCAP components instead of relatively general statements.

Minor points:

- Worthwhile discussing the relation between "ambiguity" and "equifinality" - the latter often used in hydrological and ecological modelling *Here we disagree*. We see 'equifinality' as having more to do with calibration than with ambiguity of definition. *Thus we propose, at this point: "We also exclude 'equifinality', i.e. ill-conditioning of calibration. This is noted in Section 3.2."* and in section 3.2, at the end of the subsection on 'parameter error': "An extreme case of parameter uncertainty is what is known as



‘equifinality’ where multiple solutions of the calibration calculation are possible because the calibration is under-determined.”

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- p. 1839 l. 8-11: Not clear how the authors infer an underestimation of uncertainties on GPP by evaluating NEE. GPP and NEE will simply have differing uncertainties *This was intended to note some aspects of the Jung et al results rather than be an inference from them. Clearly we need to express this better (and fix the grammar and avoid duplication). We think it best to drop the quoted modelling efficiency values (rather than add a detailed explanation). We propose dropping line 10 and finishing line 9 after (about 6%).*
- p. 1841 l. 18: "misinterpretation of results" is very unspecific. What different from "bias" do you mean here? *Essentially a bias that can be corrected. Thus we would propose to say: The last of these problems represents a bias that can be corrected by simple post-processing of the results,*
- p. 1844 l 20ff.: this whole paragraph needs better explanation and references (e.g. "weak constraint formalism") *We propose to expand the reference to weak constraint, particularly describing the natural correspondence with the Kalman filter where parameters and state are optimized together and the state need not follow exactly the model dynamics. References to Trudinger et al (OptIC paper as already cited) and possibly additional reference to Wang et al. on carbon data assimilation and model-data fusion: Agricultural and Forest Meteorology (2009) 149, 1829–1842.*
- p. 1845 : there is very little specific guidance on treating spatial decomposition correctly, the paragraphs are very general (cf. temporal decomposition) *Indeed. In our view this reflects the state of the science. This is an area where additional research is needed. We propose, following the first sentence of the section: Compared to the time-domain, there is much less in the way of well-established statistical techniques. Furthermore, and assumption of statistical stationarity must be restricted to the smaller scales. For the carbon cycle, spatial differences occur in many different contexts.*

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- p. 1845 l. 25 : right panel instead of lower panel (or better put letter (A) and (B) into figure. *Specific location should be less of a problem for a final paper (unlike online discussion where browser vs print-friendly versions seem to differ in layout).* Nevertheless we agree that (a), (b) is preferable, being less error-prone.
- p. 1850 l. 12: but note that this uncertainty based on parameters is very likely an underestimation (due to model structural error). Yes, but we think that it is better to make this explicit (rather than implicit as at present) at the beginning of section 3.2.

Reviewer 2

1 General comment

This overview paper is one of the chapters contributing to the Regional Carbon Cycle Assessment and Processes synthesis coordinated by the Global Carbon Project. It discusses and classifies the sources of uncertainties for carbon budget estimations. This paper written on commission should be an ambitious and fascinating document. It may actually be too esoteric in its present form not to disappoint the reader. Significant work remains to be done to vulgarize and broaden the topic. *Response: Indeed. Hopefully what we have done can provide a useful starting point.*

1. p.1830, l.1: at the scale of regions (ie the RECCAP scale), the world can be considered as deterministic: it may not be obvious to the reader why characterizing regional carbon budget is inherently a statistical task. *It is, of course, the estimates of budgets that are subject to statistical uncertainty. While this is implicit in our use of the word ‘characterisation’, explicitly saying ‘estimates’ is far better.*
2. p.1830, l.9: The comparative could be omitted (“hard” rather than “harder”) . For inversions for instance, uncertainties are not harder to compute than the mean because both come together. We agree.

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3. p.1830, l.15: The expression “designed to explicitly incorporate the spatial dimension” is not clear. ‘incorporate’ is changed to ‘consider’.
4. p.1830, l.20: two analyses confirming each other do not necessarily reduce uncertainties because they may provide redundant information. We propose changing ‘different analyses’ to ‘analyses of different data streams’ since the latter captures what RECCAP is aiming to do, and implies independence of at least some of the errors.
5. p.1830, l.26 and p.1850, l.11: Referencing a paper in preparation is not appropriate. We agree. We propose (for p1830): “In this overview we propose that :” – (followed by the list) and for p(1850) the appropriate reference should be changed to Poulter (personal communication). However, we would prefer to remove this sentence as giving too little detail (as the relevant study is still unpublished) and expand on other aspects of spatial statistics – see point 30 below.

There was also one other mention (without citation) to our (ongoing) pilot study. This is to be removed.

6. p.1831, l.21 and p.1851, l.13: The correlations are among the flux errors and not among the fluxes. We do not say that the correlations are among the fluxes — we say that the correlations are among the flux estimates. Since the estimates are notionally flux + error, correlated estimates is mathematically equivalent to correlated errors, but since ‘estimates’ rather than ‘errors’ is what we actually know, we think our expression is more to the point.
7. p.1831, l.25: the top-down ellipse is not contained in the bottom-up ellipse, which indicates that the top-down ellipse has missed key bottom-up information. Indeed This is a counter example of how top-down should be used. The point of the figure is to illustrate that purely top-down approaches do not contain small-scale information. The referee’s ‘how top-down should be used’ implies that a pure top-down is unusable in practice and that one needs to combine the information given in the solid and dashed

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8. p.1833, I.3: The covariance is not the most minimal description of the uncertainty, but rather bounds (actually used p.1848, I.1). Also nothing is said about systematic errors. This is a typical example of a paragraph that should be made broader. *The use of single number for uncertainty can be regarded as ‘minimal’, whether it is the variance, mean absolute deviation or a (symmetric) range. Thus we see no need to change this paragraph. Rather we would seek to frame it in the broader context by preceding it with an introductory sentence.*
9. p.1833, I.5: Using “low-resolution”/“high-resolution” as an example may make the matrix singular. *This appears to apply to p1834, not 1833. Of course it will be X (and/or submatrices) that is singular – in this case C will not exist. We don’t see this as affecting our discussion of the implications of truncation, although it is important in actual syntheses.*
10. p.1833, I.5: Also, C was defined as a grid-point matrix in the previous section. The example may therefore not be appropriate. *As with the first half of this comment, this appears to apply to p1834, not 1833 We propose: .. illustrated by considering X transformed so as to be partitioned into sets 1 and 2 ...*
11. p.1835, ii: this point is not clear. *The best response here seems to be to interchange points (ii) and (iii) so that the new (ii) is Backus-Glibert truncation, and the new (iii) which the reviewer finds unclear can be described as a tapered truncation of the solution space, cf the ‘sharp’ truncation in (new) case (ii). .*
12. p.1835, I.11: “disaggregation by process” is not clear. *We propose: “ When a total*

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carbon flux is decomposed into contributions from different processes, ambiguity can arise from imprecision in the definitions, with a given flux component attributed more than one process, or to none.”

13. Section 3: this section piles up references but lacks numbers. The authors should say what they have learned from all these studies. For instance, Section 3.2 distinguishes four classes of model errors, but no indication is given about their relative importance. *section 3.2 is more about general principles – we believe that the point at which to add specific detail (to the extent that this can be done in the absence of the relevant RECCAP papers) is in section 5.2.4, as noted in point 30 below.*
14. p.1837, l.1: this section only deals with a subset of models used in RECCAP (physical models of the ocean and of the terrestrial biosphere) and its title should reflect this fact. *This section deals with characteristics of models. The references are chosen (mainly but not always) from topics related to RECCAP in order to illustrate/document specific points, and are not presented as a survey of all relevant RECCAP-related practice. At this point, in part in response to the last comment from referee 1, we propose: These types of error are likely to be cumulative, although not necessarily strictly additive.*
15. p.1837, l.2: “can” should be removed. We agree.
16. p.1837, l.20-21: the sentence lacks substance. It basically says that the uncertainty of parameters arise when calibration is used and when calibration is not used. *We propose: Uncertainties from model parameters arise from either the range of uncertainties from calibration or, as an unknown bias, from the choice not to tune a particular parameter.*
17. p.1837, l.24: OptIC is not defined. We agree and propose, as a short description and well as expanding the acronym: “*in particular the Optimisation InterComparison (OptIC) for comparison of parameter estimation methods in terrestrial biogeochemical models*”

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18. p.1837, l.26, 29: models of what? *Propose: ORCHIDEE terrestrial carbon model and: Jung et al (2007) compared three terrestrial biosphere models ...*

19. p.1838, l.7: what is the metric of uncertainty here? *rephrase as : real uncertainty in projections of biochemical fields ...*

20. p.1838, l.12: “harmonized” would be a better word. . *We agree, but think that maybe ‘standardised’ is better still.*

21. p.1840, l.13: the reason for referencing Ciais et al. (2010) here is obscure. *We were looking for an up-to-date reference of CO₂ inversion, to supplement citation of a book which, while comprehensive for its time, is now 10 years old. On reflection, Chevalier 2010 may be more suitable for this purpose.*

22. p.1840, l.25: there has been other attempts like Lavaux et al. (BG, 2009), who explicitly computed the error covariances. *Their results can be easily exploited here to put errors in an order of importance. We would feel that it is ‘their approach’ rather than specific results that can be exploited (although not necessarily easily!). However we think that the place to refer to Lavaux et al, is on the following page – see point 24.*

23. p.1840, l.25: There were similar studies before the one by Michalak et al. (2005), e.g., Dee and da Silva (Mon Wea Rev, 1999). *DRAFT: We were mainly drawing on carbon cycle examples so that the context is more familiar to our target readers. However, the Dee and de Silva paper is particularly interesting (and we thank the authors for bringing it to our attention). It is a good reference for the following point (which concerns statistical modelling of uncertainties). We would propose to revise these sentences in a more integrated way,*

24. p.1840, l.13: the reason for referencing Chevallier et al. (2010) here is obscure. *Assume this means line 3 of p 1841.. Our point was about statistical modelling which*

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is used in that study. Chevalier has played a leading role in such considerations – we will check to see if one of his other papers is even more directly targetted, and, as noted in the previous point, integrate that discussion with messages from the Dee and da Silva paper. As noted above, this seems to be the point at which we should refer to the study by Lavaux et al.

25. p.1842, l.5: “for the purpose of RECCAP” should be moved before the “:”. Indeed this caveat is specific to RECCAP. OK
26. p.1842, l.5: there is also a positive side. This statement means that the inverse modelers have already estimated the “exabyte covariance matrix” (Section 2.1) for some of the bottom-up information. It would be very relevant and interesting here to discuss what they have learned. *This is to be covered by a specific paper in the RECCAP special issue. This is, unfortunately, one of the papers that has missed the target submission date.*
27. p.1847, l.9: “significant”. Indeed.
28. p.1847, l.15: some information from Rayner et al. (JGR, 2010) would be interesting here. *DRAFT: We propose to add a description of how Rayner et al 2010 calculated and expressed uncertainties and the consequences for their spatial structure. A proposed draft is: Rayner et al 2010 generate uncertainty in the parameters of their fossil fuel model using a Montecarlo technique. Their model calculates emissions at a point as a product of national emissions and a spatial proxy blending nightlights and population. Both the spatial field and national multiplier carry an uncertainty so the pointwise emissions have a large but uncorrelated component of uncertainty arising from uncertainty in the spatial proxy plus a smaller component arising from uncertainty in national emissions. This second component of uncertainty is correlated at national scales. When emission estimates are aggregated to these national scales the first component becomes insignificant and uncertainty is equal to the specified uncertainty in national emissions.*

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29. p.1849, l.9-12: the authors should say which uncertainty metric they use. *The draft RECCAP ‘ocean’ paper (i.e. the personal communication from Rik Wanninkhof) indicates ± 1 s.d. ranges.*

30. p.1850, l.10-11: how does the 0.64 PgC/yr figure (actually not defined) compare with those from the other uncertainties listed in Section 3.2? *We propose to drop this sentence (including the reference to unpublished work on which the number is based) and expand the previous sentence — this is a response to point 13 above.*

31. p.1851, l.4: “North” (rather than Nth). *OK.*

32. p.1852, l.25: examples of other contexts should be given. *We propose: Examples from our own experience include:*

- *carbon cycle calculations as input to the 1994 IPCC Radiative Forcing Report (Enting Wigley and Heimann) — this was already a second attempt, following an unsuccessful attempt organised by IPCC TSU, and even then problems were encountered with the specifications.¹*
- *The Transcom intercomparison where initial compromises (eg the neglect of reduced carbon, fossil emissions either fixed or with a single global uncertainty) have persisted;*
- *The OptIC exercise (Trudinger et al 2008), where a limited focus on the sampling distribution of the estimates has meant a failure to fully address the original aims.*

33. p.1853, l.13: the added value of repeating those elements here is not obvious. *On consideration, we still that quoting these words in both the introduction and conclusion is important as an indicator of the state of the science.*

34. p.1854, l.5: the sentence is too short to be of real interest to the reader *We propose to use this point to emphasis the point made by referee 1: the poorly characterised nature*

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¹Reference is CSIRO Atmospheric Research Tech paper 31, 1994.

of spatial structure cf. temporal structure, and simply cite examples of studies that (in our view) provide guidance about future Directions: Huntzinger et al, but also Scipal et al (GRL, 2008) and possibly others.

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