**Biogeosciences
Discussions**

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Interactive comment on "Multiple observation types reduce uncertainty in Australia's terrestrial carbon and water cycles" *by* V. Haverd et al.

V. Haverd et al.

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Response to comments on "Multiple observation types reduce uncertainty in Australia's terrestrial carbon and water cycles" by V. Haverd et al. Anonymous Referee #1

Comment 1.1

The study is interesting because it uses multiple constraints on estimating ecosystem model parameters. Respective different continental-scale NPP estimates and uncertainties are presented in section 3.1. However, despite the importance of this section for the overall objective of the paper, it appears to be far to short. - I suggest comparing Australia's NPP against inventory-based NPP or against previous studies in the same fig 4. - I request comparing also streamflow data and ET at watershed level or

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continental scale.

Response 1.1

We prefer to maintain Figure 4 in its current form as its purpose is to convey the estimates of continental NPP and their uncertainties obtained using BIOS2 under constraint by different observation types. We have however already included comparisons with above-ground biomass and catchment-scale ET (derived from streamflow data) in Figure 9 (Section 4.2). Corresponding biases in model predictions are presented in Figure 16. For clarity, the Figure captions of Figures 9 and 16 have been modified to replace "ET" by "catchment-scale ET derived from streamflow". Comparison of continental NPP with previous studies already appears in Figure 17. An inventory-based estimate of Australian continental NPP is not available.

Comment 1.2

The water flux is of same interest than the carbon flux, also in the results and conclusions sections of that study. Hence, the reader wonder about the value of individual datasets for constraining the water balance. - It is unclear which parameter sets from which combinations of data for calibration has been used further in the following sections. Please clarify and also explain why to use one specific configuration. Was it the one with all three datasets included, Eddy data, streamflow+precipitation, leafNPP because you wanted to have reduced possible biases? Or are the continental results based on parameter sets constrained only by using eddy covariance data because that showed the lowest uncertainty? Would it not make sense to show both results?

Response 1.2

Following this suggestion we have included a new Table A1: Uncertainties in mean (1990-2011) Australian continental carbon and water fluxes, associated with prior parameters and 7 parameter sets obtained by using different observation sets and combinations thereof in the parameter estimation procedure. Uncertainties represent the

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relative 1s uncertainty attributable to parameter variance and covariance, calculated using Equation (3).

We also include the following text in Section 3.1:

"We also investigated the impact of the data constraints on the parameter component of uncertainty in long term predictions of four other variables: (i) the fraction of NPP attributable to recurrent (mainly grassy) vegetation (NPPg/NPP); (ii) ET; (iii) the fraction of ET attributable to soil evaporation (ESoil/ET); (iv) the fraction of precipitation converted to runoff (Runoff/Precip). Results are given in Table A1. Relative prior uncertainties decrease in order NPP> (NPPg/NPP) > (Esoil/ET) > (Runoff/Precip) > ET. Similar to the results for NPP, we find that, for all four quantities: (i) the eddy flux data alone the strongest constraint of the three data types; (ii) each data set individually leads to a reduction in uncertainty compared with the prior estimate, (iii) the estimate constrained by all three is a compromise between the results obtained using each data set individually, but with a larger uncertainty than that obtained using eddy flux data alone. Streamflow observations constrain ET and Runoff/Precip more strongly than do leaf NPP observations, while the reverse is true for the constraints on NPP, NPPg/NPP and Esoil/ET."

We have modified the first sentence of the third paragraph in Section 3.1 to clarify which parameter set we use. The remainder of the paragraph justifies the use of this parameter set:

"Although the uncertainty estimate on NPP under constraint from all three data types is not the lowest of all uncertainties in Figure 4, we maintain this as our best estimate of continental NPP, and use the corresponding parameter set throughout this work. The reason is that the error bars reflect residuals between observations and predictions via Equation (3), but not unquantified biases in the observations. Adopting the parameter set and corresponding predictions constrained by all three data types mitigates against results being biased by any single data type. Examples of sources of observation bias

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include: (i) herbivory which would reduce litterfall compared with leaf-NPP; (ii) offtakes of water from streams which are assumed unimpaired (without offtakes for human consumption), leading to over-estimation of ET from observations; (iii) underestimation of CO_2 exchange at eddy flux sites, particularly at night-time, leading to an over-estimate of observed GPPâĂš."

Comment 1.3

Model parameter estimation and initial condition: Due to the simplified representation of aut. respiration being a constant fraction of GPP, initial conditions are not important for the NPP parameter estimation. However, it remains unclear how parameters for the CASA model, e.g. turnover times can be estimated without considering the problematic (see above) initialization of carbon pools. The methods of how this parameter estimation has been performed need to be explained in detail. Was the search algorithm in parameter space performed for the full spin-up period? Starting from initial conditions which has been estimated by using prior turnover time values will have a great effect on the optimal parameter values.

Response 1.3

This is a good point. We did indeed re-initialise both the soil moisture and carbon pools at each iteration of the parameter estimation process, and have added the following text in Section 2.5 describing the parameter estimation:

"For both models, the model runs required for the parameter estimation process included the full spin-up period."

Comment 1.4

Fig 18 and continental-scale carbon and water dynamics: I suggest in addition an anomaly plot and a comparison to independent published results, e.g. there are several recent global GPP and ET products available for comparison. Once parameter values have been estimated using eddy tower NEP anomalies, I suggest comparing also your

continental net CO2 flux anomaly with the one derived from atmospheric inversion modeling.

Response 1.4

While a comparison with global products is an interesting suggestion, we choose not to present such a comparison here, since the evaluation of global products for Australia is outside the paper's objectives. We have considered atmospheric inversion modelling results for Australia in a recent assessment of the Australian C-budget (Haverd et al., 2012), where one of our key findings is that "Global atmospheric inversion studies do not meaningfully constrain the Australian terrestrial carbon budget."

Comment 1.5

Eddy-covariance based ET: Is the site-level energy balance closed? Or has the data been corrected?

Response 1.5

The data were not corrected for energy closure because energy closure is generally good across the Ozflux sites. This is clarified by the following additional text in Section 2.4:

"No correction for energy closure was applied, because energy closure is generally good across the Ozflux sites. Frequency distributions for the slope (forced through zero) of the sum of daily averaged sensible and latent heat fluxes versus available energy peak at 1.00 (full width at half maximum of 0.24) for the OzFlux dataset (Leuning et al., 2012)."

Comment 1.6

There has been no discussion on the fact that fire as one important process has been neglected. I think that a mean fire return interval is parameterized into the turnover rate. However, annual anomalies of continental-scale carbon fluxes as potentially shown in

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Fig 18 will be influenced a lot by the fire activity during specific years.

Response 1.6

We address this point in thee following text inserted in Section 6:

"The IAV of NEP (0.13 Pg C y-1 (1s) derived using AVHRR) is comparable to Australia's total greenhouse gas (GHG) emissions in 2009-10 (0.15 PgCeq y-1) (DCCEE, 2012). We do not include in this estimate of IAV the impact of disturbance (particularly fire) on the temporal variability of the rate constant for biomass decomposition. However we expect the effect to be an increase in IAV approximately equal to the IAV of gross C-emissions from biomass burning, which is relatively small (0.03 Pg C y-1 (1s)) (Haverd et al., 2012)"

Comment 1.7

Despite its importance for the overall results, the model setup has been only superficially described. Were soil moisture pools first equilibrated before calculating 1970-1989 as imput to spinup the carbon pools? Which atmospheric CO_2 have been used? A 1989 value which is not reflecting pre-industrial conditions which however determined the recent carbon pools? A 1850 value which quite different from the 1990 value used later in the transient simulation? Is it clear that 1970-1989 is a time period in which climate is representative for pre-industrial conditions in Australia?

Response 1.7a

Since we are concerned with NEP anomalies and not absolute NEP, a consideration of pre-industrial conditions is not required here (but is dealt with in our recent Australian C-budget paper (Haverd et al. 2012)). The 1970-1989 period was chosen for spin-up because it immediately precedes the simulation period. We have modified the last paragraph of Section 2.1 to read:

"CABLE was run at an hourly time-step for the 1960-2011 period, with the first ten-years being used to initialise soil moisture. Atmospheric CO_2 concentration was prescribed

using actual deseasonalised values from global in-situ observations (Keeling et al., 2001). Resulting daily aggregates of gross primary productivity (GPP), soil moisture and soil temperature were used to force CASA-CNP at daily time-steps. CASA-CNP carbon pools were initialised by spinning the model 200 times for the 1970-1989 period using CABLE output for this period. The simulation period was 1990-2011, for which monthly outputs at 0.05o (∼5km) spatial resolution were produced."

Response 1.7b

We did include uncertainty in model initialisation in the "forcing" component of the uncertainties in CASA-CNP output. This was evaluated as the absolute difference between simulations using two different spin-up periods: (1970-1989) and (1960-1979). Compared with those components attributable to uncertainties in NPP and the fraction of NPP from grassy vegetation, the model initialisation component of the uncertainty in CASA-CNP predictions is small (e.g. 2 orders of magnitude less for NEP anomaly). See also Response 2.7.

Anonymous Referee #2

Comment 2.1

Haverd et al present results from an analysis using multiple data constraints in combination with model-data fusion techniques and a process based model to estimate carbon and water cycling over Australia. I really enjoyed the manuscript – it is not only one of the few large scale attempts to use advanced techniques for merging models with data, but appears well execute and presented. I have mostly minor suggestions for improvement, but also some more serious concerns regarding the treatment and propagation of uncertainty, detailed below.

Response 2.1: We thank the reviewer for this positive comment and address the concerns about uncertainty treatment below.

Comment 2.2

Page 12183 Line 5: Is it useful to cite this website? Clarify exactly where the numbers 0.9-3.1 PgC come from.

Response 2.2

We have modified this sentence to read:

"More recent estimates of Australian NPP (1990-2010) from global ecosystem models participating in the carbon cycle model intercomparison project (TRENDY) (Sitch et al., 2012, submitted) are also highly variable (2.2 PgCy-1 (range) and 0.8 PgCy-1 (1ïĄş))."

Comment 2.3

Page 12185 Line 1:5. Explain in more detail what these 'instabilities' were. Also explain the Thornley problem used to justify holding the ratio GPP to NPP constant. What is the implication of these simplifications on the results? I'm guessing the main problem stems from there not being any information in the data sets you are using that can constrain these processes, so you end up with high equifinality and thus 'instability' when you extrapolate to larger regions. Whatever the problem is, the paper would be a lot stronger if you identified it and discussed implications, rather than just saying there was one.

Response 2.3

We thank the reviewer for raising this point. The problem was not in fact model instability, but simply that the model performance against observations was improved when we switched to static allocation coefficients and a fixed ratio of GPP to NPP. We have modified text in the third paragraph of Section 2.1 to read:

"Additional CASA-CNP modifications, made to improve model performance against observations in this application, included using static allocation coefficients (rather than allocation coefficients dependent upon phenology, temperature, and soil moisture), and holding the ratio of NPP to GPP constant in time, instead of using the default growth respiration/maintenance respiration paradigm which is known to be problem-

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atic. Thornley (2011) reviews the use this paradigm and summarises that if fails largely because "There is no straightforward way of dealing with growth and maintenance separately because the pools and anabolic processes are the same for both growth and maintenance".".

Comment 2.4

Page 12220: Line 1:2. Remove 'enable' Line 3: 'Assumed fixed. with' Line 14: 'Soil C pools are

Response 2.4

The text has been corrected accordingly.

Comment 2.5

Page 12186: Line 12: Setting k equal to 0.5 is equivalent to assuming a spherical leaf angle distribution? Should this change by biome?

Response 2.5

Yes, there is an assumption of spherical leaf angle distribution (and also no clumping) inherent in the conversion of fPAR to LAI. We choose to maintain this simplification as it is consistent with the corresponding parameter choice for the canopy radiation extinction coefficient in the CABLE land surface model, and we do not have information to make this parameter spatially variable.

Comment 2.6

Page 12187: Line 2: MODIS spans the observation period of the flux data, but so too does AVHRR. So why use MODIS?

Response 2.6

First, our AVHRR fPAR record for Australia only extends to the end of 2006 (due to delays in detailed correction processes). Therefore we use MODIS to cover more re-

cent years. Second, as noted in Section 6, there are significant discrepancies between the two products during the period of overlap (2000-2006), so it is worth performing simulations with both remotely-sensed fPAR products, and using the differences (after repeating the parameter estimation process) to quantify the corresponding component of the forcing error (as noted in Section 2.5).

Comment 2.7

Page 12191: Line 14: It's not clear why this sequential optimization was necessary, (i.e. how it reduced the computational burden, and to what extent) and whether it affects the results. The problem with doing it in this way is that uncertainty in CABLE-SLI is not being propagated through to CASA-CNP. It appears that CASA-CNP is being driven with just one parameter set form CABLE-SLI (output from the optimum set), which ignores the variability generated from CABLE-SLI driven by all the other equally plausible parameter combinations (and thus equally plausible but potentially very different CABLE-SLI output). The question is, are you quantifying the true joint uncertainty of the two models?

Response 2.7

We have indeed propagated uncertainty in CABLE-SLI parameters through CASA-CNP, albeit in a simple manner. As noted in Section 2.5, forcing uncertainties in CASA-CNP outputs were assessed as the absolute change in predictions associated with perturbations to forcing inputs (namely NPP, fraction NPP attributable to grassy vegetation, soil moisture and soil temperature). We have made it clear that these "forcing uncertainties" actually represent the propagation of uncertainty from CABLE-SLI to CASA-CNP by modifying the last paragraph in Section 2.5 as follows:

"Uncertainties in model predictions associated with forcing uncertainties were estimated as the absolute change in prediction associated with perturbations to forcing inputs. These were summed in quadrature to give the total forcing uncertainty. For CABLE-SLI, we perturbed meteorological, vegetation cover and soil input data. Me-

teorological inputs were perturbed by the following estimated 1s uncertainties: precipitation (10%); incoming solar radiation (10%); air temperature (1oC); vapour pressure (10%); wind speed (50%).Vegetation cover was perturbed by switching from LAI derived from AVHRR fPAR to that derived from MODIS fPAR. Soil input data were perturbed by randomly permuting the locations of the soil principle profiles, while maintaining their frequency distribution. Forcing uncertainties in CASA-CNP predictions were estimated by perturbing the inputs (derived from CABLE-SLI). They represent an upper limit to the uncertainties propagated from CABLE-SLI to CASA-CNP, because CASA-CNP parameters were not re-optimised for each perturbation of CABLE-SLI input. NPP and the fraction of NPP attributable to grassy vegetation were perturbed by the derived 1s uncertainties in these variables (Section 5.1, Fig. 11). Volumetric soil moisture content was perturbed by 10% and soil temperature by 2oC. In addition, the model initialisation was perturbed by substituting the spin-up period of 1970-1989 with the 1950-1969 period."

In section 5.2, where the carbon pools and biospheric residence times are discussed, we have a added a sentence on forcing uncertainties:

"Forcing uncertainties are dominated by contributions from uncertainties in NPP and the fraction of NPP attributable to grassy vegetation, with minor contributions from uncertainties in soil moisture and temperature and model initialisation."

Sequential optimisation of CABLE-SLI and CASA-CNP was necessary because spin-up is included in each (of $\sim$ 100) model runs in each parameter estimation process. As noted in Section 2.1, the spin-up time for CASA-CNP is 200x 20 years. Even using parallel computing, each parameter estimation process for CABLE-SLI required $\sim$ 1 day. Therefore simultaneous optimisation of parameters in CABLE-SLI and CASA-CNP would require hundreds of days and would not be feasible.

Comment 2.8

Page 12192: Line 14. Uncertainty in model predictions also stems from model struc-

tural error, not just parameter and forcing data uncertainties. This is not a problem for your approach, as the model structural uncertainties are mapped to parameter uncertainties, but should be acknowledged. All the data estimates used as constraints have associated uncertainties. The net effect of having uncertainty in data, and propagating that uncertainty through to the model parameters, as opposed to ignoring it, is an increase in uncertainty in model projections (to match the uncertainty in the observations). This can be particularly important when extrapolating in space/time. To quote Raupach 2005: ". . ., providing data and allowing another researcher to provide the uncertainty is indistinguishable from allowing the second researcher to make up the data in the first place." I.e., data is relatively useless without information on how accurate it is. Are the authors greatly underestimating the uncertainty in their projections by not taking into account measurement uncertainty?

Response 2.8

Measurement uncertainty is accounted for in the parameter covariance matrix, which is a mapping of residuals onto parameter covariance using the model Jacobian. The residuals comprise model-observation residuals and prior-posterior parameter residuals. Each model-obs residual consists of components from observation error, parameter error and model structural error. While we cannot distinguish between these three types of errors, their combined effect is accounted for in the parameter covariance matrix, which is in turn mapped onto uncertainties in model predictions, as described in Section 2.5. For clarity, we have added the following text to Section 2.5:

"While parameter errors, observation errors and model structural errors are not accounted for separately, their combined effect is realised in the residuals between model predictions and observations, and hence in the cost function. Thus "parameter uncertainties" encapsulate all three of these types of errors."

Comment 2.9

Page 12193: Line 7: CACA-CNP Page 12194: Line 6: "faithfuly" -> accurately? Page

12203: Line 14: 'shows plots' Page 12205: Line 29. Cite the article which reports this drop in sea level (and attributes it to increased water storage in Australia, among others). http://www.agu.org/pubs/crossref/2012/2012GL053055.shtml

Response 2.9

The text has been modified according to these suggestions. The Boening et al. paper reporting the drop in sea level has been cited.

Comment 2.10

Page 12206: Line 4: Although the anomaly of 0.5Pg is indeed very large compared to the mean global sink, did it lead to large respiratory losses in subsequent years? It is likely a very fast cycle compared to the slower cycle of mean annual NEP.

Response 2.10

Yes, as already noted in Section 6, NEP starts to decline in 2011 because of of increasing heterotrophic respiration:

"NEP also shows high values associated with the high rainfall years of 2000 and 2010 (Fig 18(viii)), but unlike NPP, does not continue to increase in 2011, owing to the delayed increase in heterotrophic respiration."

"

Comment 2.11

Figures 12,13,14 It would be great to see these figures of mean values accompanied by maps of the associated uncertainties. Does model uncertainty very spatially, and what can that tell us about the design of future data collection networks?

Response 2.11

These are interesting questions. Regional variation in uncertainties is evident in Fig.11. For example, uncertainty in NPP is highest in the Tropics and lowest in the Desert.

Such information could be mapped and used in the design of data collection network, but these are activities are outside the scope of the current paper, which is already quite extensive.

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