Answer to the Anonymous Reviewer #3

The reviewer comments are in bold, and the replies in regular font.

We would like to thank the reviewer for their in-depth and detailed comments. We largely agree with the suggestions and respond to each in turn below.

This paper presents a novel model (named QUINCY) for the coupled cycling of carbon, water, N, and P in terrestrial ecosystems. This is a substantial contribution to a relatively small set of available models with a comparable scope: possible global applications [although only site-scale simulations are presented here], mechanistic representation of processes that determine the response of the terrestrial biosphere to global environmental change, applicability within a Earth System Modelling framework. The model is evaluated with respect to GPP and NEE data from FLUXNET, NPP/GPP ratios from paired FLUXNET and forest inventory data, and foliar d13C - a proxy for leaf-level water use efficiency.

The main innovation of the present model lies in the coupled representation of N, P, and C cylces; and in the model's entirely (?, please clarify) newly written code, that is designed in a modular way (p. 3, l.19) and allows for an appropriate design of the basic model structure to accommodate the new modelling capacities of simulating interactive carbon and N/P cycling as opposed to adding respective processes onto a "first-generation" C-only model. Parts of the model, however, are process parametrisations that are implemented as such in other Dynamic Global Vegetation Models (see also comments below).

The model is entirely newly written code, and the ways it is depending on earlier model developments will be now illustrated in the text (as requested by reviewer #1).

It is highly challenging for a reviewer to assess whether the present model is appropriate and accurate in simulating all key processes that determine the coupled C, N, P, and water cycling. Especially given the immensity of the number of equations and paremeters implemented in the model (see SI). Therefore, I'm trying to evaluate how far the present paper got me to being convinced that this model works.
In summary, I am convinced that this model is a highly valuable contribution and that its description should eventually be published in GMD. I am less convinced that the model works (practically and off-the-shelf) and can be used by the wider community, since the code is not made fully publicly accessible ("The source code is available online, but its access is restricted to registered users and the fair-use policy stated on https://www.bgc-jena.mpg.de/bgi/index.php/Projects/ QUINCYModel. Readers interested in running the model should contact the corresponding authors for a username and password."). Therefore, I could not apply the model myself and my assessment is merely based on the descriptions in the text. I am always disappointed to see model

code not being made fully open access along publications in GMD (an open-access journal!). In that sense, and very strictly speaking, what is the purpose of a publication in GMD? Shouldn't such a model description just remain an internal technical document then? I leave it to the editors to handle this and will evaluate the further aspects of the paper assuming that the editors support non-open access code in GMD.

We would like to point out that we have made the code available to editors and reviewers of this manuscript. The QUINCY model code will be available under a GNU license upon publication of this manuscript, but it's access will be restricted to registered users, because the software infrastructure of the QUINCY model relies on software developed by the MPI for Meteorology in Hamburg. This code is subject to acceptance of the personal MPI-ESM software licence.

Below, I'm listing a few MAJOR points that I would like to see addressed in a resubmission, followed by a number of MINOR points that I hope would improve the manuscript.

1 MAJOR

1. Evaluation. It is a practically impossible task to comprehensively evaluate a model that simulates virtually every important process that operates in a terrestrial ecosystem (and is typically represented in comparable models). I also consider that a complete and detailed description of the model itself may be the main part of a GMD paper, and that the evaluation with data may be secondary and addressed by further studies. However, as the paper is designed now, the "meat" is in the SI (all equations and parameter values), while the main text provides a rather brief description of basic model concepts and approaches in intuitively accessible language, and provides a rather brief evaluation against a small set of observational data and and overview of the model sensitivity. I think this is generally a good form of presentation.

However, the evaluation becomes a central point of the paper and the evaluation presented here is relatively slim. The key challenge is to identify what we learn from including N and P cycling and limitation in a vegetation model and to identify key phenomena that can only be explained with including nutrient cycling (What are the key phenomena that can only be explained with including nutrient cycling?).

The reviewer is correct that the main purpose of nutrient enabled biosphere models is to address the key scientific challenge noted above, and it is our intention to study exactly this challenge using QUINCY in the future. However, in the manuscript we presented the structure and provided essential background information that any model is required to meet even before the question of nutrient cycling is addressed. If we had developed QUINCY inside a modelling framework with documented carbon cycle modelling capacity, we could have reduced the effort into evaluating the baseline model behaviour and present more results from manipulation studies to evaluate the strength nutrient “limitation” in QUINCY. However, when writing the manuscript, we felt that there was already a lot of information to digest, and that we would overload readers by adding a complete set of nutrient fertilisation benchmarks, each with their own uncertainty and requirement for detailed discussion. We have therefore opted to present some key features, so as to provide a background for a number of in-depth studies evaluating the nutrient cycling effect in QUINCY at site and global scales.

I was intrigued by the evaluation of carbon use efficiency, CUE (Fig. 6c) but would have liked to understand more about why the model captures the overall magnitude of observed values, but does not explain the substantial variability in observations within vegetation types (e.g. NE forests).

The QUINCY model is driven by meteorology, soil texture and atmospheric deposition of nutrients, but does not consider other factors that might contribute to within-PFT variability of CUE, such as site fertility not related to N and P availability, soil pH, site history, and species-level effects on CUE. Such discrepancies have been previously recorded for other similar models. We will extend the discussion of the data comparison to include this issue.

I would also have like to see how foliar stoichiometry, C allocation, the root:shoot ratio, soil respiration, or N fixation change across climatic and N (and P)-deposition gradients and how it (broadly) compares to observations. These processes have been identified previously as key mechanisms that determine the coupled C and nutrient cycling (Medlyn et al., 2015). I was less convinced that the diurnal and seasonal GPP evaluation (Fig. 3) provides much insight in that respect. I suspect that the model can easily be tuned to match the magnitude of observed fluxes for each model setup (C, CN, CNP), and it is stated in the text that nutrient limitations do not affect diurnal and seasonal C dynamics (p. 11, l. 16).

While our model has not been tuned to any specific site, we agree with the reviewer that the evaluation based only on GPP does not provide the necessary mechanistic insights. To address this, we will include a series of explanatory figures (taking into account these suggestions by the reviewer) which can help to understand these dynamics in our model. Additionally, we include a series of more conceptual figures, as suggested by reviewer #2.

However, we would like to clarify that long-term N and P availability does affect the daily and seasonal maximum GPP, but short-term variation in nutrient availabilities do not affect the shape of the diurnal or seasonal cycles, because of the lagged effect response of plant growth to nutrient uptake.

An explicit representation of chlorophyll (Chl) was included in the model in order to provide a useful diagnostic (with readily available Chl data), but no evaluation was shown.

While some chlorophyll data are available, largely from remote sensing products but also from site-level observations, these data are not straightforward to use in model comparison given the scaling problems involved in comparing model and observations. We will therefore make this a subject of a separate study which will further explore the assumptions and implications of the canopy representation within QUINCY.

2. Sensitivity analysis. I am most interested to learn about which parameters the modelled variables X are most sensitive to, and not primarily about how much X varies when several variables are varied at the same time (which is shown now in Fig. 8 if I understand this correctly). Could the results of the sensitivity analysis be shown differently? Also, in my interpretation, the sensitivity analysis primarily reflects the choice of the range over which the model parameters are chosen to vary. Therefore, the conclusion on p. 10, l. 18 that "the model output (Fig. 8) is well constrained and centred around the results of the standard parameterisation" is mainly an implication of this choice. If the range over which the parameter values were sampled was larger, then the range of simulated variables would be larger ("less well constrained"). However, I agree with the authors that non-linear, interactive effects could lead to asymmetric simulated distribution. Anyway, I think this sensitivity analysis as presented now does not provide very useful information. Providing information about the sensitivity of modelled variables w.r.t. A selection of the most important parameters, and to clearly show which variables are most important in a figure, would be more useful.

We thank the reviewer for pointing out that the model sensitivity analysis has not been explored in a satisfactory manner, a point which has also been raised by reviewer #2. Fig. 8 is meant to illustrate the stability of model state variables, while Table 3 shows parameters ranked in order of importance. We agree that, as it stands, this table is hard to interpret and we will change it to clearly show which parameter is linked to each process (e.g. photosynthesis, growth) and the implications of the ranking will be discussed in the text.

We would like to point out that we did not intend to quantify the overall model uncertainty, but to test the overall stability of the model and its sensitivity to its parameters, given the large number of sometimes badly constrained parameters and non-linear equations. Defining realistic bounds for many of these parameters is a challenge, and 10% is large for some, but certainly too narrow for others. The choice of the variation range for each parameter was meant to be a conservative estimate for the variation these values. It is correct that a larger variation in parameter values would lead to a larger variation in model variables. However, we do believe that an assessment of the 10% deviation already gives a clear indication of whether a parameter is highly important or less, and that by using latin-hypercube sampling rather than an OAT approach, we can robustly assess model stability.

3. Model description - several points here:

- **In the main text should provide an intuitively understandable description of the model, a characterisation of its behaviour, and a clear identification of the most important**

assumptions and choices made for model structure. This is done on p. 3 l. 1-18, however I would have liked to see this description more comprehensive and better referenced to the existing literature. In particular, I encourage the authors to make some of the central assumptions underlying
the model more explicit, e.g. the following - if I'm correct:
– A "sink limitation term" (function of temperature, soil moisture, and nutrient availability) is included on Vcmax and Jmax, Eqs. 7d.
– Using air temperature for photosynthetic rates
– Canopy N determines photosynthetic rates. This implies that photosynthetic capacity (A for saturating light conditions) is strongly controlled by N availability, and not by climate.
– Biochemical (acting on Vcmax and Jmax) and stomatal limitations by low soil moisture considered
– Acclimating basal respiration following Atkin et al., 2014
– Resource uptake respiration depending on the form of N uptake (NO3 or NH4)
– Root respiration scales with temperature but not with N or P uptake capacity.
– Strict space constraint in forest stands by prescribing a maximum foliar projective cover. Constrains the number and size of individuals.
– SOM turnover is N limited.
– Labile pool dynamics determined by sources and sinks, sink limitation on growth by temperature and soil moisture
– P just limits (imposing a "cap") growth (unlike N which also regulates the photosynthetic capacity)

These are indeed some of the main new assumptions in QUINCY, and we once again thank the reviewer for the in-depth analysis of the paper. We will follow their suggestions and extend the model description to include these points as well as further references.

• Model structure (and complexity): The model contains a very large number of parameters and it remains unclear how the parameters can be constrained from observations, or whether they are relatively well known from independent measurements. E.g., the fraction of C allocated to fruit production (Eq. 29) seems enormously complex. Is the complexity chosen here always necessary?

As noted above, we conducted a sensitivity study sampling hierarchically all model parameters with the aim to assess whether this complex model still produces reliable results. We will improve on the presentation and discussion of the sensitivity analysis, as mentioned above.

Our model development was guided by the principle of a balanced model complexity. Our decision to avoid latent model complexity by for instance not allowing for hidden biogeochemical pools to buffer short-term flux variability, as commonly done in other models, we were forced to implement scaling equations to accommodate these short-term flux variations. The structure of any single equation can be debated, and alternative models might be applied with similar outcome. Given the large degree of internal feedbacks, however, the precise shape of an equation may be less relevant than the fact that a certain effect/process is represented at all. Some of the processes and parameters introduced can potentially be constrained by additional data (e.g. the labile and reserve dynamics can be constrained in principle by measurements of non-structural hydrocarbons, and carbon isotope tracer studies), and we plan to use the QUINCY model to use these additional data sources in future studies. As the reviewer has pointed out, many of these evaluations require a scientific paper on their own. We agree with the reviewers concern that not all parameters of this model (or in any other terrestrial biosphere model) are constrained by observations, which introduces uncertainty in the model outcome. This fact calls for the need to perform parameter ensemble simulations wherever possible to ascertain the robustness of the model finding.

In the QUINCY model, the fruit allocation is important because it affects the seed pool available for re-establishment of PFTs. It was therefore important for the model development to ensure a minimum of fruit production, while at the same time not slowing down foliar development at the beginning of the growing season. Therefore a simple, year-long fixed fraction allocation as employed in many other models was insufficient. Whether the exact form chosen here is required, or simpler forms would also yield reliable result is a question that we might explore in future studies.

Equations are presented mostly without reference to justify the choice of the model structure. It is unclear whether the structures of equations used to describe the many processes are adopted from other references, are grounded in fundamental laws that are sort of standard representations, or whether they are designed here for the first time. If so, it may require some additional words on the motivation. For example, the photosynthesis scheme in SI Sec. 2: Is it adopted from Kull Kruijt (1998) or what parts of what's implemented here are new? Reference for N retranslocation upon heartwood formation (Sec. 3.5)? Many of the parameters are "shape parameters" of the functions used, and the systems dynamics may not be very sensitive to these.

We do agree that there are parts of the model description, for which justification and referencing can be improved. We will rectify this in the revised manuscript, for all sections as well as the specific points raised by the reviewer.

It is indeed correct that the overall system dynamics in general may not be very sensitive to many of the shape parameters, however, this is not universally the case, and specific threshold values for e.g. the onset of sink limitation on photosynthesis, or the downregulation of nutrient uptake given the labile nutrient constraint, can have important effects on seasonal fluxes, even if they do not have a major contribution to the overall uncertainty in stoichiometry or productivity. The parameter sensitivity study demonstrates that the model's predictions are not strongly affected by such threshold values. However, it would be wrong to conclude that the shape of the response functions does not matter for the model predictions. A full analysis of the effect of these response functions is beyond the scope of this paper, but could be the subject of future studies focussing on the interactions of specific processes.

It would be useful to identify the most important feedbacks and discuss how these may shape the system dynamics in response to manipulations of temperature, CO2, N-input, etc.

We agree with the reviewer that the mechanistic behaviour of the model has not been fully explained, and in accordance with the comments of reviewer #2, we have added a series of explanatory plots. We will add a discussion about how this may alter projected responses to changes in model forcing.

• Motivation and description of advantages of this new model:
– Merit of model is described as “decoupling of photosynthesis and growth” (p. 11, l. 6). This is unclear.

We will extend the presentation of this decoupling by more clearly motivating that it is well known that photosynthesis and growth are independently controlled (Körner, 2006; Fatichi et al., 2013). The present model structure allows testing whether this de-coupling has important implications for the simulation of long-term biogeochemical cycling, which we will do in a future study. What this approach already now allows to simulate is the ability of the model to temporally decouple carbon from nutrient uptake, and therefore allows for a more realistic simulation of seasonal cycles without having to rely on the heuristic representation of reserve generation under nutrient stress as commonly employed in other biogeochemical models.

– The model is described as “modular” (p. 3, l. 19), but then, the model description refers to specific model representations, not alternative ones within the same model. It remains unclear, what “modular" means in this sense.

We did not present the modular structure of QUINCY in the first version of the manuscript, as it was meant to demonstrate the performance of the standard version of QUINCY, against which future studies relying on the modularity can be compared. The QUINCY model provides modularity on two levels, which we will describe more clearly in the SI of the revised manuscript: The first provides modularity regarding the scope of the model, allowing it to run as a canopy flux scheme, a stand-alone vegetation model without biogeochemical and biogeophysical soil feedbacks, a stand-alone soil model without biogeochemical and biogeophysical vegetation feedbacks, and the fully coupled canopy, vegetation and soil model as applied here. This approach allows for testing the implications of particular processes at reduced model complexity. The second level of modularity relates to the fact that the subroutine structure of the model facilitates the testing of alternative process hypotheses. In the revised manuscript we will take advantage of this structure to showcase for instance the effect of photosynthetic and respiration acclimation and the vertical soil representation.

We thank the reviewer for the in-depth analysis of our paper and for reading the entire model description in such detail. We address all the minor comments below.

2 MINOR

2.1 Main text

• p. 2, l. 3: "induce" instead of "provide"

Thank you, this has been changed.

• p. 3, l. 17 "nutrient uptake" instead of "root uptake"

Thank you, this has been changed.

• p. 4, l. 16: From what sources were these inputs prescribed? In particular: What is the source for rooting depth?

The sources of these inputs are described in Section 2.3.1 (we will add a reference to that section here). The source for the rooting depth was unfortunately missing from these descriptions, we thank the reviewer for noting this. We obtain the rooting depth from Jackson et al. (1996).

• p. 4, l. 31: Description of plant nutrient uptake

We will broaden this section 2.1.1 on vegetation processes and will include there also description of plant nutrient uptake.

• p. 4, l. 22: turnover at two time scales: What is the motivation and the effect of this fast nutrient turnover and resorption/remobilisation to/from the labile pool?

The fast nutrient turnover is based on the observation that the Rubisco and other photosynthetic molecules break-down at a faster rate than the lifetime of a leaf, leading to seasonal variations in foliar nutrient concentrations, as the reflux of nutrients into the leaf are based on the current availability of nutrients for leaf growth (Zaehle & Friend 2010).

• p. 6, l. 9: 'Microbes' or 'microbial' is mentioned at several instances, yet a microbial biomass pool is not explicitly modelled. Please specify how this is to be understood.

The microbial biomass is indeed not explicitly modelled, but we have been using this term quite loosely when referring to the fast soil pool. We will clarify this issue in the new version of the manuscript.

• p. 9, l. 3: Table 2 does not provide information about model performance. Can it be replaced by something that gives insight into performance?

Taking into consideration this comment (also one below) and comments for Table 2 from Reviewer #2, we have decided to redo this table, by showing observed and simulated values of GPP, TER and leaf C:N -values for these four sites.

• p. 9, l. 5: Should mention modelled value next to observed value in the text.

In this version of the manuscript only the modelled values were in the table. In the new version we will have both modelled and observed value in the Table, and won't be writing their values in the text.

• p. 9, l. 18: Is there no data available to support this statement?

We are here referring to values of normalized standard deviation, that was not unfortunately clear in this context. The point we here try to make is that the modelled standard deviation was changing mostly because of model-data differences in the seasonal maximum values of the fluxes. We don't really see how we could use data to support this, since this is just a characteristic of the model behavior.

• p. 10, l. 22-23: How does this statement relate to the results shown in Fig. 8?

The reviewer is referencing to the statement "The model shows, as expected, clear dependencies between the rates of net N and P mineralisation, GPP, and carbon stock in vegetation and soil." We agree that this sentence can be confusing in this context and plan to write a longer explanation about this figure. What was meant here is that the sites having larger GPP also have larger carbon stocks and N & P mineralization rates.

• p. 11, l. 5-6: What does "decoupling of photosynthesis and growth" refer to?

This has been now answered above.

• p. 11, l. 12-13: This is not shown, is it?

The Taylor plots (Figure 4) and the evaluation of the GFDB forest sites (Figure 6) show that there is no systematic model bias for any single PFT. The Taylor plots are made for the half-hourly data, so strictly speaking it is true that this is not shown also for seasonal and annual carbon fluxes. We will rephrase this sentence, so that it points out to exactly what we have shown in the results section.

• p. 11, l. 16: This does not seem to be what the figures suggest (substantial effect by CN and CNP vs. C)

This sentence is maybe a bit unclear. Indeed there is a substantial long-term productivity effect of CN and CNP compared to the C only version. It is the short term dynamics, which are similar between these different model versions. This point was also raised by reviewer #2, and we will add clarification to this issue.

• p. 12, l. 12-13: give modelled values here too

In the next version of the manuscript we will have either both the observed and simulated values in a table or report the observed and simulated values in the text.

• Table 2: Just showing modelled values, without observational data is not very informative.

We mentioned now above how we will change the table.

2.2 SI:

• p. 2, l. 8: Worth noting that layer 1 is the top layer.

We have added this to the text.

• p. 2., l. 9: Worth noting that this is the total canopy N content (if that's correct?).

We have added this to the text.

• Eq. 7: Why is the CO2 compensation point not subtracted from ci in the numerator?

This is not done in Eq 7, but Eq. 16a, as already noted in Kull and Kruijt (1998) eqs. 2 and 14.

• p. 4, l.19: Introduce the term Av again.

We've done this now.

• Eq. 15: It's described on p. 3, l. 26 that A is the minimum of two rates (Ac and Aj). It appears confusing that Ah is introduced here as another

limiting rate. Isn't it just determining the Aj rate (actually, it may also appear confusing that Aj is independent of light, as of eq. 7).

On p. 3 l. 26 it is said that A is the minimum of the two rates (Ac and Aj) in the *light saturated* conditions. The Ah here is the photosynthesis taking place in the *light-limited* conditions. The formulation of the Farquhar model by Kull and Kruijt (1998) differs from some other formulations, as here it is assumed that each leaf layer has potentially both light-saturated and light-limited region, which is dependent on the leaf N concentration and the incident light environment of that layer (eq 16). We will add few general sentences about this photosynthesis formulation in the beginning of this section 2.2 to make the text clearer for the readers.

• Eq. 16: should spell out 'for' or use appropriate mathematical symbol

We have modified the equation accordingly.

• Eq. 17: Is aerodynamic conductance a fixed parameter?

No, it is not a fixed parameter, it is calculated from the aerodynamic resistance introduced in eq. 110. We will add this to the text.

• p. 8, l. 4: Why "co-limitation" and not (just) limitation?

Here we wanted to point out that both nitrogen and phosphorus can be limiting the growth, therefore we wrote co-limitation.

• p. 8, l. 4/5: Should mention here that this refers to the turnover rate of the labile pool and that the labile pool turnover defines this part of the growth limitation.

We will add this clarification to the text.

• Eq. 28: Should mention the exponent 2 also in the text below.

We will add this to the text.

• Eq. 30: Better write functions as f(N, P, H2O) instead of arguments as subscripts. In general, Eq. 30 needs an explanation/motivation.

We will write it in a function form as requested and will add explanation and motivation to this equation.

• Eq. 37: What are lambda and k?

Lambda and k are shape parameters, the values are shown in Table 3. We have added explanation and reference to the table in the text.

• Eq. 39b: k reserve not k store ?

Yes, we have corrected this.

• Eq. 45b, ‘dt’: clarify that this refers to daily.

We have corrected this in the revised version.

• p. 15, l. 5: Is the seed-bed pool and fruit production related?

Yes, the seed-bed pool is related to the fruit pool such that turnover from the fruit pool enters the seed bed pool, where it is either used for re-establishment of new seedlings, or turns over to form litter. We will mention this in the new version of the manuscript.

• p. 16, l. 3/4: But later, C pools of newly established individuals are averaged with C pools of existing ones, leading to a reduction in the average-individual C pool, right?

The mass and number of individuals from the newly established individuals are added to the mass and number of individuals from the existing average-individual population (i.e. this is an addition, not averaging at the grid-scale level), leading to a reduction of the mass per individual, just as in the LPJ model (Sitch et al. 2003). We will clarify this in the revised manuscript.

• p. 16, l. 17: ‘met, str, ...’ Introduce these abbreviations at first mention.

We have added this.

• Eq. 65b: What is Ed,decomp?

This is the de-activation energy for decomposition, shown in Table 4. We have added an explanation now to the text.

• p. 19, l. 5: ‘increased’ At first I though this should be ‘reduced’? I thought that the fast and slow SOM pools have a lower C:N ratio than the structural pool and mass transfer from the structural to fast/slow SOM leads to net immobilisation. If not, please state upfront which step of mass transfer leads to immobilisation and relate it to respective pool stoichiometries.

The turnover times are of the metabolic and structural litter pools are increased, so that the turnover rates of the litter pools are reduced, leading to a decrease of the immobilization demand (immobilisation rate from litter to fast SOM pool).

• Eq. 73a: Point out in the main text that uptake is linear w.r.t. fine root biomass.

We will do this.

• Eq. 94: Start with stating what the reflection coefficient determines. Maybe better to start with something "high-level", like the surface energy budget? Or just start with equation 97.

We will re-organize the section as suggested.

• Sec. 6.3: Start stating what sort of scheme is applied for soil hydrology, how many layers, . . .

We will add this kind of explanatory paragraph to the beginning of this section.

• p. 29, l. 10: Need to introduce the meaning of "skin" here.

We will add this.

• p. 29, l. 19: I'm confused: field capacity is not part of Eqs. 114.

We apologise for the confusion. Eq. 114a uses the volumetric water content at field capacity (W_fc,sl=1). We agree that the equation 114 is hard to follow and we will clarify this in the revised version. If the soil water per layer would exceed its field capacity, the amount of water exceeding the field capacity is moved into surface runoff or drained to the layer below. We will add this explanation to the text.

• Eq. 114: Throughfall is not defined. Is sl=1 the topmost layer? In general, I don't understand Eq. 114.

Throughfall (Fthrough) is what is left from the precipitation after interception. Yes, sl=1 is the topmost layer. We will also add more explanation to eq. 114 to better explain the water partitioning at the surface to interception, surface runoff and deep drainage.

• Eq. 116: What is Ei? Evaporation of intercepted water? What is ra? aero-dynamic resistance? Repeat here to clarify. It would be helpful to start with the high-level water budget.

Yes, Ei is the evaporation of the intercepted water and ra is the aerodynamic resistance. We will add these to the text. We will re-organize this section as suggested.

• p. 30, l. 5: How is surface temperature calculated? Please add reference to equation.

The surface temperature is calculated by eq. 107. We will add here a reference to that here.

• p. 31, l. 1: This is better put upwards (start with high level description of first principle (water/energy conservation).

Yes, as already mentioned above, we will do this.