Reviewer 3

Overview
The manuscript provides an assessment heterotrophic decomposition simulated by CMIP5 models to temperature and soil moisture (using precipitation as proxy) and how these sensitivities vary in space. Simulation of heterotrophic respiration remains a highly uncertain process in many models and thus any analysis which aims to diagnose the strengths, weaknesses and identifies strategies for improvement are valuable. However, in this case I find the manuscript misses many key areas of existing research in both the introduction and discussion. The writing clarity needs to be improved throughout the manuscript to make the reading as easy as possible. Unfortunately these issues leave me unclear as to what novel information is brought to the fore by this analysis. I hope the authors are able to clarify their message and highlight their novel finding.

Thanks for the careful reading and useful feedback. We agree that this manuscript needs significant revisions in many areas, but are hopeful that doing so will greatly improve its clarity, methodological rigor, and ultimately impact.

General comments
Writing style:
The authors frequently use overly complex and long sentences with many comma. This makes the manuscript more difficult to read as it frequently obscures the key point of the sentence / paragraph. Below are some examples...
P1 L30 & P8 L25: You should not begin a sentence and definitely not a paragraph with "Because".

Thank you. We agree that these need simplification and clarification, and in many other spots as well. Awkward grammar and conjunction oddities will be fixed as well in our revision.

Title:
I think that the title should be changes as it is misleading. The manuscript does not assess the causes of uncertainty in observations as far as I can see. It might be more useful to mention both the CMIP5 models and temperature / precipitation.

Thanks for the suggestion. We will look carefully at the title after fully revising the manuscript and assess its suitability, precision, and clarity.

Abstract:

The abstract needs greater clarity. It is not clear what if any recommendation are made as to which processes may be missing in the CMIP5 models. What is the pathway to improvement?

We agree, and will place more emphasis on clarity and synthesizing suggestions for future directions and steps to improve RH modeling.

The authors state that their approach can be used to diagnose causes for divergent results but it is not clear why they do not present any causes for the divergent results found here in the abstract.

This is an good point. In our revision we will clearly lay out specific hypotheses or at least expectations: how, based on our best understanding of the carbon cycle and scaling issues (Bond-Lamberty et al., 2016; Jung et al., 2017; Phillips et al., 2017), models might be expected to behave across latitudinal and global scales, and why they might diverge based on parametric or structural differences.

P1 L13: It would be good it include a quantification of RH to put into context.

We agree, and will do so.

P2 L22: "...RH dataset." This is a little misleading. As the authors point out this is a observation-driven analysis.

We believe the reviewer means line 42, not 22? Good point; we will clarify this, drawing a distinction between true observations and upscale datasets *derived* from observations.

P1 L25-27: "The relationship between observed RH and precipitation (PR) relationship is strong and positive (r > 0.5, P < 0.005), but few models consistently show this sensitivity of RH and PR." Are the models which do not show a correlation those which do not include a soil moisture response to RH? How many model fail to show the observed behaviour?

This is an interesting suggestion, and one that we will check and quantify, referencing recent work in this area (Falloon et al., 2011; Liu et al., 2016; Moyano et al., 2013).

Introduction:
The writing style needs addressing. There appears to be some large areas of the existing literature missing from the introduction which is needed to support their analysis. The authors also miss existing literature attempting to diagnose the decomposition processes in CMIP5

models. This information is needed to more clearly define the novelty of the authors work.

This is a good point. The manuscript clearly needs to do a better job of citing and discussing previous work such as Exbrayat et al. (2013a, 2013b), as well as more recent work (Guenet et al., 2018; Luo et al., 2016). In our revision we will provide more background on pattern scaling, as well as the state of CMIP5 models' carbon cycle performance more generally (Anav et al., 2013; Luo et al., 2016), and using aspects of model behavior to draw inferences about climate- and carbon-cycle response to anthropogenic forcing (e.g. Gillett et al., 2013). In addition, we think that a better discussion of how RH pattern scaling can be treated as a type of emergent constraint (Hoffman et al., 2014; Luo et al., 2015) would be useful.

The second paragraph of the introduction states most models simulate increasing RH and that existing RH process representation is simple (first order kinetics) compared to many others ecosystem processes (I assume e.g. photosynthesis?). Then moving on to compare observation driven estimate of RH with NPP estimates. I think this is too many concepts in one paragraph without adequately describing any of them. Paragraph two should deal with a description of exiting RH model structures. Highlighting known issues with first order kinetic, e.g. lack of a microbial pool and difficulties in responding to changes in litter quality versus quantity (e.g. Wieder et al., 2013, Xenakis & Williams 2014). The importance of soil moisture (Exbrayat et al., 2013ab, Exbrayat et al., 2014) or nutrient cycles (Manzoni & Porporato 2009; Exbrayat et al., 2013a).

This proposed structure ties in nicely with the previous comment, and would make things clearer for the reader, we agree.

P1 L44-45 "While both temperature and precipitation have a positive effect on the global terrestrial carbon flux" Are you still talking about respiration? Net ecosystem exchange, Net biome exchange?

This will be clarified.

Methods:
Linking back to the decomposition review from the introduction details of which temperature and soil moisture response functions used in the CMIP5 models seems appropriate to me.

This is a good idea, and will help the reader understand the links between these processes, their implementation in models, and how this links to the current study.

P3 L1-6: Does the observation-driven estimates come with an uncertainty analysis?

Unfortunately not really, no. We will discuss this issue and note frequent sources of error in these and other upscaled observational products (Hashimoto et al., 2015; Jung et al., 2011; Xiao et al., 2011).

P3 L21: "...we only used the first realisation..." Would it not be more appropriate to use the mean across ensembles?

This was also raised by Reviewer 4. Preliminary analyses suggested that there was little ensemble-to-ensemble variation for these variables in CMIP5, and we can include this as supplementary information; if the reviewers feel strongly about this point, however, we are happy to re-do the analysis using the mean of all ensemble members.

Results:
P5 L14-20: It is not immediately clear whether you are talking about correlations in space or time. Also please be clear throughout the manuscript that precipitation is a proxy for soil moisture availability. Therefore you should not be talking about both soil moisture and rainfall being limiting. Soil moisture / plant available water is limited.

Good point–thank you. We will clearly separate temporal and spatial correlations, as well as note the precipitation/soil moisture point.

P5 L28-30: "This is likely due to less land (and this higher variability is model averages)..." Is it not equally or more likely that greater divergence between models occurs because the models are trained and developed using observations which are bias to the temperate northern hemisphere?

Yes, and this ties in with a point we (somewhat obliquely) made on page 5 (see response to Reviewer 1). This revised manuscript will more clearly explain the potential links between inconsistent model performance in the 'global South' and lack of observations in those regions. We will also note that this problem has been addressed in other contexts, e.g. upscaling of FLUXNET data (Jung et al., 2011).

P6 L13-21: Is there no observation equivalent for this analysis?

We can do so, for example by discussing differing regional sensitivities to climate change (Liu et al., 2016; Schimel et al., 2015).

P7 L1-14: I think both of these paragraphs need to be clearer. It is not always obvious to me whether you are talking about simulated vs observed RH. Would be improved if you make better use of the available figures in this section.

We agree; currently the text too frequently is unclear in which specific result is being focused on

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Discussion:
P8 L19-20: This is the first time pools have been mentioned. This should be first mentioned in the introduction.
Moreover, I feel a more thorough discussion of the associated literature is needed.

Yes, we agree, the introduction should–and will–do a better job of linking fluxes and stocks, and discussing their relationship. As noted above, the manuscript clearly needs to do a better job of citing and discussing previous work.

P8 L25-46: I think both these paragraphs need rephrasing to improve clarity. There is no use of any figures or tables from you manuscript here.

See previous comment; we agree, these paragraphs to be clearer about their links to specific results.

P9 L15-17: Your text appears to be referring to the global average but what about spatial patterns? You present a large number of figures with spatial variation, can you make greater use of these?

This is a good suggestion–thank you. We will look at this point and try to break the global means down, taking advantage of the spatial information presented.

P9 L22-30: I think this would be a good area to discuss some of the possible parameterisation / model processes missing within the existing models within the context of the material I suggested should be added to the introduction.

As noted above, we agree that this would significantly strengthen the context and interpretation surrounding our results.

P9 L39: "...temperature response (Q10)..." please provide range for context.

An interesting suggestion! We will do so.

P1 L22 "Compared to observations, ESMs consistency..." -> "Compared to observations ESMs consistently..."
P1 L36 "carbon cycle" -> "carbon (C) cycle"
P2 L31 "...an observation-based data product." -> "...observation-driven analysis."
P2 L46 "(Hashimoto et al., 2015):(Hashimoto et al., 2015)"
P4 L40: "...majority (65 %)..." please state number of models.
P5 L1: "...smallest projected trend..." trend in what? not clear from text. P6 L24: "...weaker correlated models..."

These points will be clarified and/or fixed.

P7 L21: "...ESMs examined here (Figure X)" ?
P7 L23: delete "On one hand, "
P7 L25: delete "On the other,"
P7 L30: "...soil moisture response functions."
P7 L35: "robustly" I'm not convinced you can say this. "Consistently" would be a more appropriate word
P8 L1: delete "Interestingly,"
P8 L9: "...across empirical datasets." Such as?
P8 L25: "Because..." you should not begin a sentence with "because", let alone a paragraph or subsection. Please rephrase.

We will fix these issues.

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