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Dear editor,

Thank you for considering my manuscript for publication upon resubmission after having addressed the reviewers’ comments. I thank Prof. Thiel and the anonymous reviewer for their constructive comments, which I have thoroughly addressed and which I believe have improved the manuscript. I would like to point out that both reviewers focussed heavily on the seagrass part of the story, which is not the focus of the manuscript. The story here is a much more fundamental one and concerns all plants.

I would like to reiterate that I believe this manuscript is perfectly suited for the Research in Context article type since it combines an experiment with a meta-analysis, the latter requiring the former. I also strongly believe that the manuscript, especially suited to the Plant Senescence special issue, will be of interest to your broad readership who are interested in fundamental botanical paradigm shifts such as I am presenting here and following on from a similar article published on your journal: Wright LS, Simpkins T, Filbee-Dexter K, Wernberg T. 2024. Temperature sensitivity of detrital photosynthesis. *Annals of Botany* 133: 17–28. https://doi.org/10.1093/aob/mcad167.

The updated data and code remain available in my open-access repository, now at [github.com/lukaseamus/plant-limbo](https://github.com/lukaseamus/plant-limbo), and I encourage you to pass these on to the reviewers again for further scrutiny. Data collated for the meta-analysis are still deposited at [github.com/lukaseamus/detrital-photosynthesis](https://github.com/lukaseamus/detrital-photosynthesis) which is linked to a Zenodo data publication.

Associate Editor  
Comments to the Author (required):  
Many thanks for submitting your manuscript to Annals of Botany, I read it with interest. The topic is indeed intriguing , and the reviewers agree that it is worthy of study. However a number of issues are raised with your current manuscript. In particular reviewers ask you to consider the definition of “detritus”,

I have now explicitly defined detritus (p. 3, ll. 5f., 14–17).

include more details on the methodology including the statistical analysis.

I have added methodological detail (p. 7, ll. 2, 4f., 11–15, 17, 22, p. 10, l. 3, p. 12, ll. 16–22, p. 13, ll. 3ff.).

The reviewers also provide further details to help improve the manuscript that you may find useful.

I have incorporated additional suggestions (see below).

Reviewer: 1  
  
Comments to the Author  
Comments to ms by Wright (see also the annotated manuscript):

I cannot find the annotated manuscript attached to the decision letter.

General evaluation: The manuscript reports on “detrital photosynthesis” of a wide range of macrophytes. The author has done experiments with two species of seagrasses and searched the literature for reports on other macrophytes and their capacity to continue photosynthesis after these macrophytes (or parts thereof) have become “detritus”. This is interesting and certainly very valuable.

Thank you.

Overall, the experimental part of the study seems to be ok, but there are a few issues of those experiments that need to be explained in more detail (see below and annotated manuscript). For the literature review, the author must provide more information about the selection criteria.

I have added further detail on selection criteria (p. 12, ll. 16–22).

One thing that is not entirely clear is the criterium that is being applied to consider a macrophyte individual (or parts thereof) as “detritus”. Reading the manuscript, it sometimes seems that it is detachment from the substratum, and other times it is when photosynthetic tissues are cut off from the main “plant”.

I have now explicitly defined detritus (p. 3, ll. 5f., 14–17). This should also be clearer from the expanded selection criteria (p. 12, ll. 16–22).

The author also emphasizes that he distinguishes between excision and abscission, but it is not entirely clear what exactly is meant by this and how to differentiate between the two terms.

Excision and abscission are well-defined and widely used terms, the former meaning cutting/detachment by an external agent and the latter meaning internally induced detachment (e.g. fruit falling when it is ripe or leaves falling in autumn). I believe these terms do not need defining for a botanical readership.

The author also talks about “the absence of physiology”, but it is not entirely clear what this means, because in the natural environment all tissue, whether dead or alive, does have some “physiology”, even though this might be by other organisms associated with these tissues.

I have made explicit that I am referring to the physiology of the plant, not the holobiont (i.e. plant and associated organisms), by simplifying this statement as also requested by the other reviewer (p. 3, ll. 19f.).

So, it is essential to explain the definition of “detritus” in more detail and in plain words. In this context, I would also like to mention that I am not a “hard-core” plant (or seaweed) physiologist, which is why I have urged the editors to seek the advice of a terrestrial plant physiologist and also of a seaweed ecophysiologist. That is because those folks need to evaluate the methodological approaches here.  
  
Maybe you could also consider the alternative term “detached tissue”, and within that, distinguish between different senescence stages (e.g., early senescence vs. late-stage decayed tissue), because as fronds or leaves of both algae and seagrass age or are damaged, they continuously shed tissue (and not detritus), and considering the physiological age/stage of these tissues seems to be very relevant.

I now explicitly define detritus in the broadest sense as detached tissue (p. 3, ll. 5f., 14–17). Prof. Thiel is right to point out that attached senescence would confound my continuous response variable detrital age. That is why I only accepted measures of detrital age when an exact timepoint of induced senescence upon detachment was provided. Tissue that has aged while still attached, e.g. prior to abscission, is not considered because t0 cannot be determined (p. 12, ll. 16–22).

In this context you also need to explain what you refer to with the term “chlorophyll persistence” (Page 4, Line 3), which is also unclear.

Thank you for pointing this out. I recognise that this was poor choice of words. I have changed this to “absence of chlorophyll degradation”, “chlorophyll degradation” being a well-understood process in plant physiology (p. 4, l. 12).

If chlorophyll remains in detached tissues, these tissues may continue photosynthesizing and could be considered high-quality detached tissue.

Whether or not detached seagrass leaves continue to photosynthesise in the absence of chlorophyll degradation is precisely one of the questions I am answering in this study.

This is because free chlorophyll that is not coupled to active photosynthesis can generate reactive oxygen species under light exposure and the breakdown of chlorophyll allows recycling of magnesium and nitrogen. All this underlines that you need to develop and describe a clear definition of detritus to avoid confusion.

I believe I have now clearly defined detritus in the introduction (p. 3, ll. 5f., 14–17) and my meta-analysis selection criteria (p. 12, ll. 16–22).

In essence, the question of what exactly is “detritus”, and how you account for the physiological state of the macrophyte tissues and also for that of the associated microbiome needs to be defined much better.

I have now stated explicitly that microbial photosynthesis on detritus (e.g. by periphyton on dead leaves) is generally assumed to be negligible in the studies included in my meta-analysis, but that the response variables should be considered representative of the holobiont (p. 13, ll. 3ff.). I accept that periphyton may under exceptional circumstances sustain levels of O2 production comparable to fully functional host tissue, which would give the false impression of detrital photosynthesis. A comparison will put this into perspective: maximal photosynthesis of seagrasses in this study is around 32 µmol O2 g−1 dry mass h−1, while periphyton maximal photosynthesis is around 0.026–0.15 µmol O2 cm−2 h−1, albeit in freshwater on detritus of the plant *Typha latifolia* (Neely and Wetzel, 1997). Taking leaf mass per area to be 0.0027–0.0068 g dry mass cm−2 (de los Santos *et al.*, 2012, 2016), this translates to 3.8–55 µmol O2 g−1 dry mass h−1, or 12–172% of maximal seagrass photosynthesis. That is why I had previously discussed filamentous epiphytic algae in my results (p. 16, l. 21) and discussion (p. 19, ll. 10–14). I have now extended this discussion to include an abbreviated version of the numerical comparison.

More details are required for the search and especially the selection process of the other studies. What condition did a study have to fulfill to be included in this meta-analysis and when was a study not included.

I have added further detail on selection criteria (p. 12, ll. 16–22).

In some places the way things are expressed are ambiguous or not very user-friendly – please, see comments in annotated manuscript and make an effort to express those things in a more straightforward language. Please, keep in mind that not all your future readers are experts, neither on plant physiology, nor on photosynthesis.

As mentioned above, I cannot find the annotated manuscript attached to the decision letter. I have done my best to simplify the language based on the comments available to me.

In general, the description of methods requires more important details and better justification: For example, the seagrass acclimation was done (briefly - how long?) under 11 ± 1.6 µmol photons m⁻² s⁻¹, which is an extremely low light level. It is not explained why these low irradiances were chosen, nor what the ambient light conditions were at the collection site, or at what depth the material was collected.

I have added the requested information (p. 7, ll. 2, 4f., 11–17). The brief acclimation period was 2 hours. 11 µmol photons m−2 s−1 translates to 475.2 mmol photons m−2 d−1 on a 12-12-h light-dark cycle and is not extremely low for coastal temperate Western Australia. I did not measure PAR at the collection sites (<1-m depth) but based on the literature I believe 475.2 mmol photons m−2 d−1 is quite representative of this high-energy, turbid coast and even a bit of an overestimate for the kelp forest or seagrass meadow understorey where detritus tends to accumulate. Among the papers I cite, see for example Masini & Manning (1997) who kept seagrasses at 5 µmol photons m−2 s−1 and Wright et al. (2024) who conducted a decomposition experiment under less than 8 µmol photons m−2 s−1 and measured more detrital photosynthesis than in the dark treatment, published in *Aquatic Botany* and *Annals of Botany* respectively.

Later (Page 4, Line 13), a saturation irradiance of 420 ± 19 µmol photons m⁻-2 s⁻-1 was used to measure O2 production. This is very inconsistent with the previous low-light acclimation. Since the plants were acclimated to such low light, they would become light-saturated at much lower levels and may experience photoinhibition at 420 µmol photons m⁻-2 s-1, likely resulting in low O2 production. I am not confident that valid O2 response data were obtained under these conditions.

The saturating irradiance was chosen based on the literature I have cited. I am not aware of any evidence of photoinhibition of seagrass O2 production at irradiances of this magnitude. Again, please see Masini & Manning (1997) who kept seagrasses at 5 µmol photons m−2 s−1 and then exposed them to over 1000 µmol photons m−2 s−1 with no evidence of photoinhibition. Wright et al. (2024) also used saturating PAR for kelp during O2 incubations and were able to measure practically unchanged maximal photosynthesis in fragments that had been kept in complete darkness or at 8 µmol photons m−2 s−1 for months. Again, no evidence of photoinhibition. With the exception of circalittoral plants, photoinhibition is practically never observed in marine plant O2 production data. Much confusion originated from pseudo-photoinhibition seen in rapid light curves based on chlorophyll fluorescence, now widely recognised to be a methodological artefact. Besides, the aim of this study is not to most accurately measure seagrass photosynthesis at saturating PAR, it is to measure the relative decay of seagrass photosynthesis with advancing senescence for standardised comparison with plants at large. All samples were treated the same, so the trends remain unchanged regardless.

For Amphibolis antarctica, no information is provided on how the samples were treated after collection.

I added this information (p. 7, l. 4).

In Line 22 (possibly referring to Equation 1), the equation used for Pmax determination is not clearly presented or referenced, or I missed something.

p. 8, l. 22 shows Equation 1 in the original manuscript (now p. 9, l. 13). I am not sure why Prof. Thiel considers this equation incomplete. Betas refer to the slopes of O2 over time (µM min−1) as mentioned in the text before the equation and subscripts denote whether this is the slope of a sample or blank as described in the text following the equation.

It is also not fully clear whether the experiments were conducted on individual leaves, total biomass, or both. The author mentions something about mesh bags but it is not really clear (see also comments in the annotated manuscript).

I am unsure what information to add. I state that at each timepoint 6 samples were taken (p. 7, l. 16) and that each *A. antarctica* sample consisted of one leaf cluster and each *H. ovalis* samples consisted of 10 leaves (p. 7, ll. 22f.). Mesh bags are described to keep all detached leaves weighed down to the sediment during the decomposition experiment (p. 7, ll. 10f.).

In the discussion, turnover of photosynthetic tissues should be considered. While this probably has not been evaluated quantitatively, there is qualitative information, especially about the main groups. I would suspect that tissue turnover is much higher in seaweeds than in seagrasses or terrestrial plants, and that this could partly explain the findings. So, when photosynthesis is still recorded months after a seaweed has turned into “detritus” (whatever the definition of detritus might be), then it is very probable that the tissue doing photosynthesis is actually new tissue that has grown in the months after detachment.

Macroalgae certainty have a higher turnover rate than other plants on average. There is quantitative information that shows that turnover in plants varies across almost five orders of magnitude (Cebrián J, Duarte CM. 1995. Plant growth-rate dependence of detrital carbon storage in ecosystems. Science 268: 1606–1608.). I am afraid I do not follow how this is pertinent to my question of the presence of absence of plant physiology in the detrital phase. Photosynthesis implies that the plant can continue to grow. For my question it does not matter what tissue is photosynthesising. It would be highly unlikely to assume that only newly grown tissue photosynthesises since data never show an initial increase in detrital photosynthesis with detrital age; hence my choice of the logistic model.

This is probably what the author refers to when he suggests (citing a previous reviewer) that “…since macroalgae are much less differentiated than higher plants and, thus, there is no reason why the single cell shouldn’t be able to continue its activity for a while…”. The question is which specific tissue is doing the photoysynthesis – how long do individual cells do photosynthesis?!? I am sure that this information is available and I would strongly recommend to look into the physiological literature.

On the contrary, I am quoting this reviewer to show that assumptions about macroalgae are being made where no published information is available. I have extensively studied the physiological literature, most of the 127 papers included in my meta-analysis are published in plant physiology journals, and this is only a fraction of the papers I have sifted through. I have come across papers that have gone even one step further than isolating cells, reporting on photosynthesis in isolated chloroplasts (e.g. Choe HT, Thimann KV. 1975. The metabolism of oat leaves during senescence: III. The senescence of isolated chloroplasts. Plant Physiology 55: 828–834.) but they did not make my meta-analysis selection criteria because I am interested in detrital photosynthesis in the ecological context of carbon cycling and an isolated chloroplast or cell would be insignificant in this context.

The overall topic is very interesting, but I consider that certain aspects must be defined more explicitly (¿when is the macrophyte tissue being considered as “detritus”?),

I have now explicitly defined detritus (p. 3, ll. 5f., 14–17).

and also that the author should explore the underlying physiological basis of photosynthetic cells and tissues, their longevity and their renewal in these main functional groups he is distinguishing.

I have dedicated one paragraph of my introduction to the mechanisms which led to my prediction that seagrass detrital photosynthesis would be of intermediate longevity between macroalgae and terrestrial plants (starting at p. 5, l. 5). I also mention seagrass leaf life span in the discussion (p. 19, ll. 17f.) even though attached senescence is only tangential to my question, as mentioned above. With all due respect, I do not see how known differences in attached plant growth/turnover/renewal (fast for seaweeds, slow for seagrasses) add to this story of detached photosynthesis.

It might also be useful to explore special cases in more detail, e.g. I am sure that there is information about photosynthesis and tissue growth in holopelagic floating plants like Eichhornia or exclusively floating seaweeds like Sargassum fluitans/natans.

I see that there has been some misunderstanding. I do not consider holopelagic plants, i.e. those that are detached during their entire life cycle, as detritus. *Sargassum fluitans*/*natans* and *Eichhornia* detritus would only be classed as detritus once it sinks, because this plant is detached to begin with, so detachment is no longer a suitable definition of the beginning of the detrital phase. To avoid future misunderstandings, I explicitly exclude holopelagic plants (p. 3, l. 15, p. 12, ll. 19ff.).

Similarly, it seems important to explore and discuss these macrophytes as holobionts, which are host for an entire suite of organisms, including other macroalgae (as mentioned by the author for some of his seagrass results) but also extremely diverse microbial communities.

As Prof. Thiel mentions I already discuss this in the results and I have added further mention of microorganisms and periphyton as part of the holobiont elsewhere (p. 13, ll. 3ff., p. 16, l. 21, p. 19, ll. 12ff.).

In summary, very good idea and interesting approach, but the underlying mechanisms should be explored more in-depth, which probably can be achieved by a more extensive literature study, especially of the physiological characteristics of these macrophytes.

Thank you. I am confident that I have addressed Prof. Thiel’s concerns.

In case of any questions about my comments, I invite the author to contact me directly.  
  
Sincerely, Martin Thiel    
Martin Thiel, Facultad de Ciencias del Mar, Universidad Católica del Norte, Larrondo 1281, Coquimbo, Chile; email: [thiel@ucn.cl](mailto:thiel@ucn.cl)

Reviewer: 2  
  
Comments to the Author  
Genereal comments  
This laboratory and meta-analysis study focused on the ability of marine macrophythes to performed photosynthesis even after being detached from the parent individual, therefore technically entering the detritus web. This is a very interesting study that highlights our poor knowledge of carbon dynamics in marine environments, especially costal ones, despite they key role in the global carbon budget.

Thank you.

Despite the conceptual idea is very appealing, the manuscript needs much improvement. The methods are rather obscured at the moment and it is not possible to fully understand them. The statistical section needs particular attention into clarifying what has been done, particularly explaining the choices made for the analyses. The manuscript would benefit from an English native speaker editing.

I have expanded the methods (p. 7, ll. 2, 4f., 11–15, 17, 22, p. 10, l. 3, p. 12, ll. 16–22, p. 13, ll. 3ff.). I am a native English speaker.

At the moment, it seems that the goal of the study does not match with the experiment performed and the analyses done: the goal was to understand how persistent physiological activity in living fragments of seagrasses delay the decomposition of these fragments. However, as it is written now, my understanding is that this study cannot investigate this aspect but rather it unravel how long seagrass fragments can survive (i.e, doing photosynthesis).  

The goal of the study is to quantify detrital photosynthesis across plant lineages to get a better understanding of how different we expect the influence of physiology on detrital dynamics to be in seaweeds compared to other plants, which in turn helps us re-evaluate existing seaweed decomposition experiments. I never mention that I aim to quantify the delay in decomposition on a mass basis. I have clarified the aims to avoid future misunderstandings (p. 6, ll. 4ff.).

Specific comments  
Abstract - I think the transition between the background and the methods needs improvement. From the background I expected that the study would have focused on seaweeds, yet, in the methods I discovered that actually focused on seagrasses (which is very interesting).

This study focusses on neither seaweeds nor seagrasses but considers plants *sensu lato*. I necessarily introduce the topic of detrital photosynthesis with seaweeds because that’s where the story starts but then transition to seagrasses because that’s where the knowledge gap lies. I have added the explicit aims to the introduction of the abstract to clarify this (p. 1, ll. 18ff.).

Page 3, Line 8 - “This stands in stark contrast to the traditional ecological definition of detritus as “dead organic matter””. This point may actually weaken this study conceptual background, at least partially. The definition of detritus is indeed “dead organic matter”. The fact that excesses leaves of seagrassess or macroalgae can still photosynthesis highlights that these living fragments are not part of the detritus yet. So I would not say that the cpacity for photosynthesis in macrophyte fragments question the definition of detritus. The interesting part is that it retard their entering the detritus trophic level.

This is a purely semantic issue. In my manuscript I quote the traditional definition of detritus because I question it. I do not consider it wise to exclude a reference because it “may actually weaken” my argument. Seaweed detritus has been termed detritus for decades. And recently it was found to be partly alive. There is no existing word for detached plant material that is alive. Detritus is the closest suitable word and was used to describe detached plants all along. Coining an entirely new term would add to the confusion, so it becomes necessary to redefine detritus. Detritus *sensu lato* then is tissue that is detached and detritus *sensu stricto* is detached *and* dead tissue. I have defined detritus (p. 3, ll. 5f.) and added a note on semantics (p. 3, ll. 14–17).

Page 3, Line 12 to 16 - I am not sure I understand this part. Does “absence of physiology”  means death organic tissue? And, does the overall sentence means that tissue that is still living retard the decomposition because it still maintain chemical defences and etc.?

This was also raised by the Prof. Thiel, so I have simplified to “when it is dead” (p. 3, l. 20). Growth and chemical defences are explicitly mentioned in the following sentence.

Page 4, Line 7 to 13 - I think I understand the point that the author wants to rise, but I think the introduction of a scientific paper should be grounded on published literature and not on discussions with reviewers.

Unfortunately, there is no published literature on this point, just preconceptions, which is what I wanted to highlight by quoting an anonymous reviewer with a different opinion to mine. I leave it up to the discretion of the editor and journal policy whether such quotes are accepted.

Page 5, line 11 to 15 - Based on the provided information, this argument is definitely logical. Yet, here it implied that the “living detritus” (for a lack of better word) remains in the water. But seagrass leaves, and many macroalgae, large get washed ashore, with likely a very different decomposition dynamics. I think this is worth to mention here.

“For lack of a better word” is exactly my point. I propose that we simply stick with detritus. Of course, a fraction of exported detritus ends up as wrack (beach-cast detritus), which has its own definition for that reason. And wrack can logically not be physiologically viable because it has left the hydrosphere. I now explicitly exclude wrack to avoid any possible misunderstanding (p. 3, l. 5).

Page 6, line 10 - Many more details on the methods are needed. For instance, from where the sediment placed in the tank has been collected? Is the same sediment for both seagrass species? Has A. antartica been transplanted in the same tanks as the other species?

This information is already present. The coordinates for both seagrasses and the sediment are provided (three locations) (p. 6, ll. 21f., p. 7, ll. 3f.). It is also explicitly mentioned that the very same tank and sediment are used for the decomposition experiment (p. 7, ll. 11f.). The last question is a misunderstanding about the biology of *A. antarctica*. This seagrass does not grow extensive roots through sediment, it merely hooks to the surface of the sediment. I clarified that *A. antarctica* was not acclimatised (e.g. by planting) and immediately used in the experiment (p. 7, l. 4).

Page 6, Line 23 - So, the litter bags were left into the water for the entire time of the experiment right?

Yes. “Destructive samples“ here means leaves were removed from the mesh bags underwater so the mesh bags were kept in place. I have clarified this (p. 7, l. 17).

Page 7, Line 1 - More details on the sampling procedure is need. At which distance sampling time-points where taken(e.g., 1 week?)? Wath does it mean that “destructive samples were taken from the mash bags”? Usually the mash bag represent the sample units, so all the material in the bag should be collected.

I believe this information is already present. The sampling frequency is given as t0 plus 5–6 times over 34 days (p. 7, ll. 15f.); sampling interval is variable so cannot succinctly be put into words but can clearly be seen from the figures. The definition of sample is provided in the same paragraph (p. 7, ll. 9f.) and in the next paragraph (one leaf cluster for *A. antarctica* and 10 leaves for *H. ovalis*) (p. 7, ll. 22f.). I never mention that I consider mesh bags the unit of replication.

Page 7, line 4 to the end - I am confused here. For what I understood the author standardised leaves photosynthesis by the volume of the jar chamber, because the two species has diffent leaf morphology and therefore occupy a different volume of the jar. This is only partially correct. The volume correction is necessary to quantify the entire O2 produce in the jar, but it does not a real standardised comparison in net photosynthesis capacity between the two species. The correct comparison should have involved, first, measuring the rate of oxygen production in the jar and the dividing by the weight of the leaves in the jar. This would standardised the net oxygen production by weight of leaf tissue, I.e., the net photosynthesis per gram of leave tissue.

I did standardise by volume and mass or leaf number to get at mass- or leaf-based estimates. Please see Equation 1 with terms *m* (mass) and *V* (volume), which the reader is referred to in this paragraph (p. 8, l. 16).

Page 8, line 9 - Why here is reported that posterior distribution of the betas? Have baysian models been employed? Why not using simple linear regressions and just extracting the beta coefficients?

In the previous paragraph I clearly state that I use Hamiltonian Monte Carlo models (which are Bayesian) throughout (p. 8, ll. 20ff.). Whether or not to use Bayesian inference is both a philosophical and practical question. I and many colleagues believe that it is more intuitive and powerful and prefer to consistently use it rather than switching between Bayesian and frequentist. In this particular case distributions of beta contain information about uncertainty which central tendencies such as coefficients do not. I mention that I propagate this uncertainty through my models (p. 12, ll. 4–9), so distributions are necessary.

Page 8, line 20 - So, here it is said that oxyen rate has been standardised by weight of leaves. This information should be given before (see comment above).

I already gave this information before (p. 8, ll. 14ff.).

Page 9, line 1 to 3 - There are several incongrouence with information provided previously in the manuscript.  
First, previously has been reported the incubation period for recording oxygen production lasted 35 minutes, while here the calculation has been made considering a 60 minute run time.

Incubation time is variable and given as at least 35 min, which is correct. The reviewer has misread the meaning of Δ*t*, which is given in units min h−1, so is the conversion from the rate denominator per minute to per hour because there are 60 minutes in every hour.

Second, previously has been reported that the volume of the jar has been adjusted considering the volume occupied by leaves of each species. But here the calculation has been done using the fixed volume of the jar for both species.

I adjusted p. 7, l. 22 to clarify that I did not mean exactly the same volume. The volume displacement by these seagrass leaves is negligible, so jar volume can be estimated gravimetrically using empty jars and deionised water, as I have described (p. 8, l. 15) and as has been done before (Wright et al. 2022, 2024). The key point is that I wanted to have similar leaf mass per volume for both species, which is not strictly necessary but improves comparability of photosynthesis rates of differently sized leaves measured over the same interval.

Page 9, line 12 to 17 - This religious explanation is completely inappropriate for a scientific study.  

I believe the reviewer has misinterpreted as gospel what is in fact the rationale for the naming of my model parameters. I am explaining that my Greek parameter names (the status quo in mathematics) are based on Hebrew letters from the well-known folklore of the golem, which has no religious connection. Just like Greek, Hebrew is primarily a language which like any other language can lend letters to mathematics. Its ties to the Judaism are secondary.

Page 9, line 10 to 23 - The statistical approach is not clear and over complicated. First, the study measure oxygen rates in jars with seagrass leaves only. Community respiration rate was not measured here, or at the very least, it is not written in the manuscript. Furthermore, it is not even clear to which community the author refers to.

I have removed “community” as it evidently caused confusion (p. 10, l. 1). Tau simply parameterises the net oxygen consumption on dead detritus. I did not measure dark respiration but I still get net respiration in the light when there is no net photosynthesis to counteract it.

Second, it is not clear to what the mid-point photosynthetic death refers to and how it has been measured.

Mu is a parameter, so it has not been directly measured; it is inferred from the measured quantities Pmax and *A*. Mu is clearly parameterised in the logistic function that is Equation 1. It represents *x* when *y* is at the halfway point between *y*max and *y*min. In this case it represents the number of days at which photosynthesis has reached the halfway point. I have added “half-life of photosynthesis” to clarify this (p. 10, l. 3). Mu, often called *x*0, is a standard parameter of the logistic function, which can be read up on everywhere (e.g. [wikipedia.org/wiki/Logistic\_function](https://en.wikipedia.org/wiki/Logistic_function)).

Third, It is not clear what is the purpourse of this analysis. This model measure how respiration decays over time (i.e, oxygen rate ~ collection time points). but this is not a measure of decomposition; rather, this is a measure of mortality rate of the leaves, I.e., how long they can survive before dying.

To reiterate, I never claim to be estimating mass decomposition here, although I do present these data in the seagrass results. I am estimating detrital photosynthesis, which is defined as the persistence of photosynthesis across detrital age. I have added a definition (p. 4, ll. 4f.).

Sincerely,

Luka Seamus Wright