**Comments on Reviews of Beck et al. (Seagrass depth paper)**

J. Hagy, 1/20/2017

Dear Dr. Beck:

My apologies for the slow processing of your manuscript, it has been very difficult of late to get reviewers to respond in a timely fashion. However, I finally have two qualified reviews in hand, and both recommend significant revision is necessary for your manuscript to be considered acceptable for publication in ESCO. The reviewers provide constructive comments on your manuscript, "Quantifying seagrass light requirements using an algorithm to spatially resolve depth of colonization", submitted to Estuaries and Coasts (ESCO). Please revise the manuscript to address the expressed concerns.

Please submit a list of your responses to the comments as a separate file in addition to a clean copy of your revised manuscript (no track changes or colored fonts, please). Be sure that the line numbers in your response comments match those in your revised submission.

Your revision should be submitted within the next 90 days. The due date is just a target - not a hard-and-fast date - but please contact the journal administrator at [estuariesandcoasts@erf.org](mailto:estuariesandcoasts@erf.org) if you will exceed the due date by more than one month.

Please access the following web site to submit your revised manuscript electronically:

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Your Username is: Your username is: [beck.marcus@epa.gov](mailto:beck.marcus@epa.gov) Your Password is: Your password is: available at this link <http://esco.edmgr.com/Default.aspx?pg=accountFinder.aspx&firstname=Marcus&lastname=Beck&email_address=beck.marcus@epa.gov>

Please click "Author Login" to submit your revision.

Please make sure to submit your editable source files (i. e. Word, TeX).

We look forward to receiving your revised manuscript.

Sincerely yours,

Richard C. Zimmerman

Associate Editor

Estuaries and Coasts

COMMENTS FOR THE AUTHOR:

Reviewer #1: Overview

This paper uses available seagrass presence/absence data for four Florida coastal areas and in combination with bathymetry data constructs non-linear relationships to describe the depth limits of seagrass growth for each region. Light requirements for seagrass growth in each region were calculated using either satellite retrieved diffuse attenuation coefficients, or in situ water clarity measurements depending on location. The percent of surface irradiance at the median depth of colonization was computed from attenuation or water clarity providing light availability for each region.

This is a reasonable summary of the paper. However, the objective is not to characterize light availability in each region. The existing measures of light attenuation provide that information on their own. The point is the amount of light estimated to be present at the depth where seagrass abundance is declining most quickly, which we interpret as a locally significant threshold.

Overall I do not find that this manuscript provides the community with new information.

We disagree with the reviewer on this point. Our paper was motivated by policy efforts that were challenged by the lack of any approach to provide locally relevant estimates of depth of colonization of light requirements. Generalizing results from other locations can be problematic. We are positive that our results provide information that is not currently available and that is needed. We emphasize that Reviewer #2 agreed with us, noting that this paper “is an important contribution to studying light limitations for seagrass distribution, and can be very relevant for future management and conservation efforts. “

Nonetheless, we are most concerned about this particular comment from this reviewer. We have revised the introduction to ensure that we emphasize the problem that our paper addresses, and ensured that the conclusions also point to what specifically is new. To demonstrate the value of our approach, we added an additional analysis of long term changes in depth of colonization and apparent light requirements in Tampa Bay using our method. This begins to show how our approach could be used.

There are two areas of analysis. The first is the construction of non-linear relationships between depth and seagrass presence, however, these relationships do not appear to provide new information about the presence of seagrass in these areas.

Again, we disagree with the reviewer. The seagrass coverage maps tell us where seagrasses are. The bathymetric layers tell us how deep the water is. Although it is relatively easy given a single transect to determine the depth at the edge of the delineated seagrass layer, these layers can be structurally complex and interpretation except at a single location can be labor intensive. If the scale of interest is larger than the very local scale (e.g., like a single transect of a depth gradient), then our method provides a new approach and new estimates of depth of colonization, operational metrics for defining it, and statistics regarding the confidence in the estimates. Moreover, we provide a means to distinguish between uncertainty and variability in the estimates, which is critical for describing variation within a system as well as formal hypothesis-testing of differences between systems.

The reviewer did not point to any references to papers or published data sets that provide the same information as we provided. If indeed we do not provide any new information, such estimates and the methods that generated them should be available in the literature. The absence of them undermines the validity of the reviewer’s comment.

We note reviewer #2’s comment that “This work is an important contribution to studying light limitations for seagrass distribution, and can be very relevant for future management and conservation efforts **“**  We agree. It is incumbent on us, the authors, to convey as best as possible what is new in our paper. Accordingly, we’ve reviewed our manuscript for opportunities to better communicate what is new and why it’s valuable. We hope that Estuaries and Coasts will value the potential value of the information for future management and conservation efforts. We know that our information has an interested audience within that community.

The authors do not show that this information can be used in a predictive manner, which leads me to ask what the benefit of the analysis is.

This paper was motivated by a very real need in the area of policy development to understand how deep seagrasses currently grow, how deep they grew in the past, and in particular if there is a relationship with water clarity. Our conceptual model is well recognized, namely, that water clarity is an important factor affecting seagrass communities. Yet, it’s also recognized that a host of factors affect the relationship. We carefully avoided narrow physiological interpretations (i.e., seagrass plants require XX% light reaching the leaf surface) of these relationships because we knew that the amount of PAR reaching the sediment surface does not fully characterize the effect of the light environment on seagrass due to factors such as spectral quality, epiphytes abundance, and species composition perhaps among others. These additional factors are acknowledged at several points in the original manuscript (e.g., Lines 441 – 444, 458 - 475).

Nonetheless, past and present distributions with respect to light availability provide useful information for establishing targets for water clarity based on the objective of protecting seagrasses. We can predict whether light is more or less likely to negatively impact seagrasses in an area given local light attenuation. We can’t predict if seagrasses will be present or should be present, absent other factors. But we can address light, which is known to be important.

Secondly seagrass light requirements were calculated for each region. My largest issue with this section is the retrieval of attenuation properties and the propagation of light from the surface to the benthos. The authors need to provide more information to convince readers that retrieved diffuse attenuation

values are reasonable for these regions.

We recognize that application of MODIS imagery to estimate Kd in water shallow enough for seagrasses is problematic. Similarly, measuring secchi depth in the conventional (vertical) way is generally problematic in a seagrass bed (the disk will often be visible on bottom), and even estimating Kd using PAR profiles is more error prone in shallow water than deeper water. We think it’s a common assumption that attenuation in deeper water adjacent to the seagrass habitat is similar to the adjacent shallow water, and we think that’s supportable on our scale and for our purposes. We do recognize that there are circumstances in which it could be different (e.g., sediment resuspension).

Whether it’s possible at all to retrieve Kd in an estuary from MODIS is a related but different question. We address that in another comment.

The use of MODIS ocean color imagery to determine the diffuse attenuation coefficient in this analysis requires more detailed description, the comparison between MODIS Kd490 and that measured in situ needs to be shown and discussed in the methods, as does the usefulness of this data in the coastal context where seagrass coverage is often not continuous within the 1 km2 footprint of MODIS, and where bottom reflectance influences remote sensing reflectance. I question whether the retrieved "water clarity" is accurate.

We agree that our paper does not provide a lot of detail regarding the remote sensing approach. This is largely because the estimates are actually from a published paper (Chen et al.) and repeating this detail in our paper would distract from the main focus of the paper. The requested comparison between Kd490 and in situ measurements is shown in Fig 2. of Chen et al. and the main focus of Chen et al. is addressing the question the reviewer suggests that we address. Most likely, would could not address this topic without directly repeating these published results.

We note as well that continuous seagrass coverage within the footprint of MODIS is not particularly relevant, since if the pixel is “seeing the seagrass” then we can’t use that pixel. As previously mentioned, the Kd estimates are derived from nearby, deeper water locations.

In our revision, we make the connection to Chen et al. clearer, such that the reader will understand that we didn’t merely use their retrieval algorithm, but rather their validated results as well.

Why did the authors use two different algorithms for Kd retrieval?

* This is a fair question I think. We’ll need to address the relationship between the two and articulate why we didn’t use the same. Can Chengfeng address the differences between approaches and rationale for application?

I am assuming that one was more suited to one region than another but this is not discussed. In such optically complex regions errors in retrieval of Kd could have large impacts on light penetration, an error analysis on the Kd retrieval and consequent impact on light availability could be useful here.

* We did an analysis of the retrieved estimates in Choctawhatchee Bay vs. *in situ* observations. In doing so, we adjusted the distributions so that the RS estimates had the same overall distribution as the in situ observations. This should eliminate the potential for a large difference between the two. The rationale for using the RS estimates is to provide a better characterization of temporal and spatial variability.

In terms of light requirements the authors should investigate seasonal patterns in attenuation instead of annual means as variability in light penetration throughout the year due to differences in light absorbing material will impact survival through the summer and winter when light intensity differs. The distribution may be linked with seasonal light penetration and not annual means.

This is a reasonable proposal. We think that it’s important to consider the resolution of the data sets involved. The Choctawhatchee Bay seagrass data are a single snapshot. There are only a few seagrass surveys as all, over several decades. We suggest that it’s probably impossible to resolve from these data a seasonal “critical period” where attenuation is a better predictor of seagrass distribution.

An additional consider is the scale of application. The relatively simple approach of comparing spatial patterns of seagrass to long term average water clarity fits better with conservation applications, where water quality regulations generally apply to annual or longer scale measures of central tendency. This is true of Florida regulations where water clarity requirements apply to annual geometric means evaluated over 5-year assessment periods.

Light requirements in coastal areas are also not often well demonstrated with percent of PAR, due to preferential absorption of blue light by phytoplankton and dissolved organic material which reduces useable light available to seagrass so that light requirements may seem artificially high. It would be far more useful to propagate a full spectral light field to the seabed so that a more rigorous evaluation of light requirements could be met.

We recognize that the spectral quality of the underwater light field can be important. However, we haven’t yet seen a clear case that accounting for the spectral quality of light dramatically improved correlations between light availability and the depth of colonization of seagrass. The fact remains that, as we noted, there are a variety of factors that affect the distribution of seagrass in addition to PAR. Spectral quality is one of those properties. However, for the purpose of environmental management, there is an interest in relating changes in light availability to changes in seagrass distributions. The data available outside a research context – meaning extensive measurements in time and space – generally don’t include spectral quality. It might be possible to relate other common water quality observations to, as the reviewer suggested, propagate a full spectral light field to the seabed, but data on the concentrations and optical properties that might be needed to do that aren’t extensive either. We think that there is still value, therefore, in gaining an improved understanding of the issue based on PAR.

Water quality measurements of phytoplankton, sediment and dissolved organic material could be used to propagate light through the water column and provide more accurate light penetration.

Le et al. (2013; Estuarine, Coastal and Shelf Science 117: 54) show that correlations between constituent concentrations and their respective components of attenuation are significant but poor. On the other hand, measures of attenuation at a variety of wavelengths are highly correlated with Kd(PAR). This suggests to me that we would not likely get more precise estimates of attenuation using models based on water quality measurements of phytoplankton, sediment and dissolved organic matter.

In conclusion I am not convinced that the percent surface irradiance calculations represent actual seagrass requirements and limits which can used in a predictive manner, rather than just simple light availability to the benthos.

I find this comment to be confusing. The concept of “actual seagrass requirements” implies that there is a fundamental light quota that seagrass plants need in every context to survive. We already know that seagrass growth depends on more than just light availability at the benthos, but that at the same time light is an important variable in every context. That’s why understanding how this quantity varies spatially could be useful for conservation efforts. The current state of water quality management with respect to seagrass habitats is to assume a fairly constant requirement. An effective method for relating distribution of seagrass in space to distribution along the bathymetry provides a way to review this relationship in space and time and adjust expectations for water quality accordingly.

Detailed comments

See attached annotated PDF

Reviewer #2: This study intends to develop a new algorithm to estimate light requirements and the optimal depth of colonization for seagrasses in Florida, at different spatial scales. The study takes good advantage of existing databases to produce maps for seagrass distribution, and associated environmental parameters such as PAR profiles and archived satellite products to generate maps of surface irradiance, water clarity and light attenuation.

We agree. These outstanding data sets are widely available, but have not been put together in this way.

The manuscript is well written and mostly easy to follow. However, some issues need to be addressed and corrected. More specific comments are listed below. A few figures need some more work in terms of clarification and labeling. All figures in this version for review are in really low quality, and one would expect that they will be provided in higher resolution if the paper is accepted.

There is no reason that the quality of the figures cannot be outstanding. Graphical quality is reduced for figures in the compiled submission. High quality originals are available to the editorial staff.

**This work is an important contribution to studying light limitations for seagrass distribution, and can be very relevant for future management and conservation efforts.**

We appreciate the reviewer’s recognition of the value for management and conservation.

My main concern with this work is the use of different methods to derive light availability/attenuation among different study sites. For example, satellite imagery is processed and used one way for Tampa Bay and in a different way for Choctawhatchee Bay, even though the same information could be derived for both sites using the same method (from data processing to data validation).

We agree that this is an issue. One of the strengths of the analysis is the common approach for computing depth of colonization. This is undermined by having uncommon approaches for quantifying water clarity. In our revision, secchi depth will be a common method. We propose that remote sensing could potentially be developed to improve temporal and spatial resolution, but the basic conclusions will be sound without remote sensing.

Also concerning is the lack of information about satellite imagery processing and results including number of pixels used, masks, errors, mean vs median values for outliers, etc. This combined with my previous point, gives the impression of a lack of familiarity or knowledge with remote sensing processing. A great addition to this work could be to include modeled Kd values from radiative transfer simulations, expanding future predictions of light availability (from in situ or remote sensing measurements) and seagrass distribution.

We agree that there is not extensive methodological detail regarding remote sensing. In our paper we were seeking to demonstrate an application of existing remote sensing methods. In this regard, our processing was standard, in keeping with previous work. Co-author Le is very familiar with remote sensing processing and has previously published papers specifically addressing many of the detailed topics. In trying to keep the focus of the paper on seagrass, rather than remote sensing methodology, we may have gone too far. In our revision we will add more detail, while referring to published material as much as possible to avoid excessive information in our paper.

We are not clear what the reviewer is suggesting about “predicting light availability.” Predicting light availability is not our objective. Rather we want to know, how changes in water clarity might relate to the distribution of seagrasses.

Comments:

Ln 152-154: Authors state that the depth of colonization was estimated based on data availability and water clarity for all four areas, but Ln 155-158 stated that Big Bend area was not used for depth limits and light requirement analyses. Please rephrase and expand on how you determine when there was "insufficient water quality data".

The initial statement on lines 152-154 does not apply completely to Big Bend due to insufficient attenuation data to estimate light requirements. These locations were primarily chosen to provide broad geographic coverage throughout the state and secondarily based on seagrass coverage data. We have rephrased the text to better describe why we chose the locations.

‘…chosen based on geographic coverage of Florida coastal areas and availability of seagrass data.’

Ln 181-184: Satellite images from the Tampa and Choctawhatchee Bays may and will also be affected by bottom reflectance. How did you deal with this for these two areas, since it is stated that only for Indian River Lagoon was an issue?

To estimate attenuation without excessive effect from bottom reflectance, it’s necessary to have deeper areas within the study area. Indian River Lagoon does not have a deeper central basin where bottom reflectance is not a major issue. It is also very narrow, so that image resolution is a problem. Most pixels are contaminated by shorelines.

Ln 185-192: Did you use an empirical or a semi-analytical approach, or both? This makes a difference when deriving Kd490 from satellite imagery. So far, based on Table 1 and extracts from the document, I was under the impression that secchi disk data were also available for the OTB. If so, why are field measurements used only to validate satellite values from the Choctawhatchee Bay and the Indian River Lagoon, and not for Tampa Bay (Ln 192-207)? Can this change your results for the OTB?

Our estimates from Tampa Bay are obtained directly from Chen et al., which extensively calibrated their RS based estimates against secchi depth. Their estimates are expressed as “water clarity” because they are, effectively, a RS-based estimate of secchi depth. We felt that there was no need to repeat this analysis ourselves. We have ensured that this is clear in the paper.

For Choctawhatchee Bay, there is no equivalent published work, so we needed to validate the estimates ourselves. In our revision, we’ve relied more heavily on secchi observations themselves, although we’d like to suggest that consistent temporal and spatial record provided by remote sensing could be valuable for this management application.

Ln 221-222: What do you mean by "infrequent observation" ? Please explain.

This more appropriately describes isolated patches of vegetation, rather than a definable edge as the lower limit of growth based on light availability. It is not uncommon to observe vegetation beyond the maximum depth of colonization, but these do not represent a measurable limit of growth, both empirically and physiologically. This was changed to ‘…excluding isolated patches (or outliers) at deeper depths.’

Ln 232-238: What was the used radius at the end?

Those chosen radius is problem specific. We have added some text for clarity:

‘The chosen radius depends on the information desired about a location. In general, a sufficient radius will result in a plot illustrating a decreasing proportion of points with seagrass with increasing depth (Fig. 3). The radius should also change depending on whether an estimate at a single location is preferred or if a sampling grid is used. In the former, the radius depends on an expected area of influence given a priori knowledge of a location. As an example, the radius to characterize depth of colonization at the outflow of the Steinhatchee River in Fig. 1 should be large enough to describe variation in growth as affected by local conditions, while small enough to not over-sample beyond the expected influence of river outflow. In the latter, an appropriate radius will provide complete coverage of the grid while minimizing redundancy of information through overlap of the sampled area by each point.’

Ln 276-287: Not clear why the depth of colonization maps (Fig. 4) don't better correspond with the geographic coverage of seagrasses (Fig. 2), especially if a "Proportion of points in seagrass" was used.

An estimate at each grid location is only possible if seagrass were present and a sufficient depth gradient is characterized by the sample area. This was previously explained on lines 362 – 365. Additional text was added for clarity:

‘Conversely, individual points absent of seagrass can be assigned a depth estimate if the radius of the sample area satisfies the above criteria. This can produce gridded maps that may not resemble the original coverage maps because they do not describe the same information.’

Ln 304-305: plenty of more recent bibliography discussing the relation between Kd and Zsecchi.

We agree there is certainly more recent and detailed research on the relationship between the two. However, we use the conversion of Kd\*Zsecchi = 1.7 because it is widely applied and the link between the two is not a focus of our analysis.

Ln 326-327: No need to repeat one by one results from Table 1.

Omitted.

Ln 377-379: Why not use satellite-derived Kd values for Tampa Bay? Comparing Fig. 5 & 6 can be confusing, since one shows units of light attenuation (m-1), while the other represents ~the opposite, water clarity (m).

Agreed, we have changed figure 6 to show Kd and have changed all instances of ‘clarity’ in the text to ‘attenuation’.

Ln 408: Semicolon after Steward et al. 2005

Added.

Figure 1: It can use a distance scale, to better picture the size of the area, including the sampling area around the test point. Also, please add the depths of the contours, since just knowing that they are every 2m is not enough to know depths.

Added to figure 1a and 1c.

Figure 2: Segment boundary line hard to see. Please change to a yellow or other more visible color.

Changed to yellow, lat/lon borders also added.

Table 1: Only one out of four URL links work. Please update working links at the time of publication.

These have been updated and verified.

Table 3: Superscripts for mean Zc and %light are confusing. A proper table showing statistical results (used analysis, significance levels, p-values, errors etc when appropriate) can be better.

We have modified the table and caption to simplify the information but we have not added additional tables because this method of presenting comparisons is not uncommon.