RESPONSE TO PEER REVIEW COMMENTS

October 2017

A new methodology entitled **Methodology for the Quantification, Monitoring, Reporting and Verification of Greenhouse Gas Emissions Reductions and Removals from Restoration of Pocosin Wetlands** was developed by The Nature Conservancy and TerraCarbon LLC for potential approval by the American Carbon Registry (ACR).

All new methodologies and methodology modifications, whether developed internally or brought to ACR by external parties, undergo a process of public consultation and scientific peer review prior to approval.

The methodology was formally submitted to ACR on August 6, 2015. ACR conducted its standard internal methodology screening and the authors submitted revised drafts on to ACR. The methodology was then posted for public comment from May 2, 2016 – June 3, 2016. Public comments and responses by the authors were finalized on August 1, 2016, and were provided to peer reviewers. ACR does not require all public comments be incorporated, but does require that a response to each public comment be documented.

Peer reviewer comments and responses by the authors are given below. Final document versions and versions as posted for public comment are also available on ACR's website under Process Documentation.

REVIEWER #1:

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
1.	As stated already in summary; Is it not possible that significant CH_4 emission takes place after rewetting pocosins?	83/84	Across a range of drainage states on pocosin sites at Pocosin Lakes National Wildlife Refuge in North Carolina, Richardson et al 2014 found that "CH_4 and N_2O emissions at PLNWR have a negligible contribution to global radiative balance since values were extremely low under all treatment conditions. CO_2 dominated gas trends at the reference, restored and drained sites, although rates were different with the reference site showing the highest annual losses of CO_2 followed by the drained and then the restored site." Richardson et al. 2014. Impacts of Peatland Ditching and Draining on Water Quality and Carbon Sequestration Benefits of Peatland Restoration. Final Report. Duke University Wetlands Center for	Generally approved, although rewetting of fertilized pocosins (former agricultural fields) should be checked for N_2O emissions.	The methodology has been revised per peer review comments to include the constraining applicability condition "The project area has been free of any land use that could be displaced outside the project area (e.g. agriculture) for five or more years prior to project start date" specifically to exclude activity shifting leakage *and* reduce potential for significantly increased nitrous oxide emissions	

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			the US Fish and Wildlife Service and The Nature Conservancy. Similar findings were obtained from research at PLNWR conducted from January to October 2016 (Eastern Carolina University (ECU), draft findings March 2017; see attached document "PLNWR Dec 2016 Interim Report"). In general, contribution of methane to total GHG flux from peat on pocosins averaged around 1-2%. Summarizing the two studies: From Richardson et al 2014, Aug 2011 – Jun 2014 data (generally dry conditions): methane contribution (in CO_2 e) to total GHG flux ranges from 0.7% (drained), 2.2% (restored) to 4.1% (mature pocosin reference site). From ECU draft report 2017, Jan – Oct 2016 data (generally wet conditions): methane contribution (in CO_2 e) to total GHG flux ranged from 0% - 0.7% across drained and restored sites.		from the project area (i.e. the applicability condition implicitly prevents any fertilizer application at least 5 years prior to project implementation). The methodology also includes the applicability condition that "N-fertilizers are not used in the with-project scenario".	

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			(see attached spreadsheet "pocosin methane contribution to GHG flux") Our aim is to constrain the methodology to application to sites with similar conditions as those on Richardson et al.'s study sites at PLNWR, by requiring (in Section A2) that "The project area is a previously-drained pocosin." This constrains application to sites in the same geography and with similar climate and hydrology and similar original vegetation (and source organic matter); pocosins are defined in Section A1 as "freshwater wetlands, often shrub-dominated, on organic soils in the Atlantic coastal plain of the southeastern United States that are seasonally saturated primarily through precipitation." We have added an applicability condition that "Infrastructure and/or management protocols are in place to manage for average annual water level at or below the surface elevation mid-point of the			

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			project area (e.g. by setting maximum height of outflow structure equal to the surface elevation mid-point of the project area)", to provide an additional safeguard to minimize methane emissions (i.e. to permit methane to be oxidized as it rises through the profile before it reaches the surface). It should also be noted that methane may be produced from two sources: methanogenesis (via several pathways) and from fires from incomplete combustion. Exclusion of the component of methane emissions from fires is unambiguously conservative as the risk of fire can be expected to be significantly less in the rewetting case.			
2.	I do not understand why this is considered to be a conservative treatment And why this is related to the occurrence of unintentional fires.	83/84	This treatment is conservative because methane and nitrous oxide emissions from fires can be expected to be greater in the baseline (drained) case than in the with-project (re-wetted) case.	Approved		

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			Where unintentional burns occur in the project, emissions from those burns are ignored in accounting, conservatively assuming the same intensity burn would have occurred in the baseline (i.e. net zero). *Intentional burns* (e.g. prescribed burns) in the project must be accounted for, if they result in significant emissions (for which threshold conditions will be set in which, ideally, light surface burns like those typical of prescribed burns, can be ignored). We spent considerable time considering various options for tracking and accounting emissions from unintentional burns (which would be reduced in a re-wetting project and thus one of the expected benefits). Please see the accompanying analysis in the document "accounting fire"			
3.	From this description the contrast is not clear between the two approaches: accretion/litterfall monitored by net	109	Text changed to "With the stock change approach, peat accretion is monitored as an undifferentiated component of net surface elevation change, while with the flux approach peat and litter	Approved		

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	elevation change in both cases?		accretion are monitored as increment above a marker horizon"			
4.	Should the use of RTKs to determine surface elevation change be restricted to shrubby or herbaceous parts of the pocosin sites? To my knowledge satellite based measurements will not be inaccurate enough to determine changes under a tree canopy.	122	Line 123 revised to state "Net surface elevation change measured using Rod Surface Elevation Tables (RSETs), Real Time Kinematic (RTKs) satellite-based approaches ***and/or*** other appropriate technologies" There is no need to specify where certain monitoring technologies are applied. Where satellite measurements are not sufficiently accurate under tree cover, the change above (from "or" to "and/or") permits using a combination of technologies, e.g. satellite-based in open strata, and RSETs e.g. in strata with tree cover.	Approved		
5.	The addition or same season is suboptimal. I suggest to state that it has to be measured at least in the same (dry) season and in addition when water tables are	134	Condition now reads: "Repeat measurements of surface elevation change are made at the same water table level (+/- 10% of level at the time of the t = 0 measurement, as recorded at the same site(s) measured at t =0) ***and in the dry season***. Water table level will be assessed from data	Approved		

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	at the same level as at t=0		from a groundwater well located at the site, or if this does not exist, from the nearest USGS groundwater well, sourced from https://waterdata.usgs.gov/nwis/gw"			
6.	Not clear what is meant by water level +/- 10% of level at t=0. Do you mean 10% of water level fluctuations? Isn't it easier to give a cm range?	134	Condition now reads: "Repeat measurements of surface elevation change are made at the same water table level (+/- 10% of level ***at the time of the t = 0 measurement***, as recorded at the same site(s) measured at t =0) and in the dry season. Water table level will be assessed from data from a groundwater well located at the site, or if this does not exist, from the nearest USGS groundwater well, sourced from https://waterdata.usgs.gov/nwis/gw" This now clearly specifies that, if the original surface elevation measurement was made with a water table of e.g. 20 cm below the surface, that all repeat measures of surface elevation must be made in the dry season and when the water table is between 18 and 22 cm below the	Approved		

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			surface; keeping in mind that the initial t=0 measurement must occur "no less than 12 months after re-wetting takes place" (per condition #6)			
7.	In my view bulk density measurements in topsoil should be stratified when a clear litter layer with different density is present. In this case litter collection (including twigs/ fruits/standing dead material) should be analyzed separately. And a 10-cm soil sample should be sampled under this litter layer.	320/	We have revised treatment to distinguish litter and soil and track their respective change in depth, mass and %C separately. Note that, with the stock change approach, litter is not monitored directly, but instead as an (eventual) input to peat accretion (monitored as a component of net surface elevation change). Rules distinguishing soil/peat surface from litter layer are applied consistently to restrict surface elevation measurements, bulk density and percent carbon samples to the top 10 cm of soil/peat underlying any litter layer (and the overlying peat is ignored). With the flux approach, accretion of litter (and peat) are directly monitored using a marker horizon. Methodology procedures now apply a separately determined	Approved		

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			litter mass and percent carbon of litter to this component. Throughout, procedures for BD and C% now specify separating out litter and peat/soil to determine mass and carbon content of each separately.			
8.	Monitoring BD in project site more often when planting of trees and shrubs is part of the project activity. Major changes in vegetation composition will lead to large changes in BD.	361	We have increased the required frequency of BD sampling to every 5 years or less.	Approved		
9.	How to deal with accretion measurements in tussock forming vegetation types?	N/A	The following text was added to the parameter tables for all ΔSE parameters: "Sample points will be located where the ground surface is measurable (necessarily outside clump centers of tussocks e.g.)." While this introduces bias (unavoidably we would counter), the bias should be conservative (as derived accretion rates will be lower excluding clump centers).	Approved		

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10.	How to deal with bioturbation in soil accretion plots? Suggestion that if there are signs of bioturbation BD needs to be resampled.	N/A	The following text was added to the parameter tables for all ΔSE parameters: "Where signs of significant soil disturbance, including bioturbation, are encountered at a sample point, the disturbed sample sites must be excluded from the analysis." Note that applicability conditions already exclude sites prone to significant soil disturbance in the baseline reference site and project area, and that the frequency of BD re-measurement has been revised from every 10 to every 5 years.	Approved		
11.	Unclear how proxy values measured in the project area can be used for the Baseline. Proxy values should be measured in the baseline site for the BGflux_bsl, t	510-512	This refers to proxy variables not affected by the project activity, e.g. temperature or rainfall. Line 512 has been clarified.	Approved		
12.	The use of the term GHG and the unit CO_2 equivalents is assuming that N_2O and CH_4 are included in this	581	Terms follow standard ACR terminology and are meant to reflect fungibility of accounted credits across project types, others of which may include	Ok clear. I did not know this.		

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	methodology, which is (as far as I can see) not the case.		accounting of N2O and CH4 in CO2 equivalents.			
13.	What will happen with the coarse organic material that does not pass through the 2-mm sieve?	835	Coarse organic root matter that does not pass through the 2-mm sieve is included in the belowground biomass pool (coarse roots of live trees and shrubs). As such, it is tracked and accounted for separately as either ΔABGB or ΔAGB.	OK		
14.	Peat depth measurements are not very accurate with the prescribed method based on resistance. Preferably this should be checked in some profiles with %C measurements in depth, or slightly refined with profile descriptions including horizons, color, texture)	837	We agree that resistance can be challenging for peat depth measurements, but do not anticipate encountering problems for the relatively shallow peatlands (1.5-3m depth) of the pocosin landscape (within which the methodology is applicable). We have used this approach to measure peatlands to a depth of 4-6 m elsewhere. Peat depth is not directly used for net GHG accounting, it is used to impose a cap on long-term emissions, and thus in fact it would be conservative if the mean peat depth were underestimated. Note also that, conservatively, only the BD and %C of the top 10 cm is	Agree that it is conservative.		

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			referenced to establish total peat stocks. This simplifies the application of the methodology and precludes the need for detailed stratification/delineation of soil horizons of the peat/soil profile as suggested.			
15.	Why is tree height not included but only change in dbh? Often height needed for allometric equations.	ABGB Page 44-45 And AGB 46-47	Independent variables (e.g. dbh, diameter at root collar, height) will be dependent on the allometric equation used and are not specified in the methodology. Text in relevant parameter tables in Section E have been revised to clarify.	Approved		
16.	Biomass plots sizes should be according to the vegetation type including the variability. For project sites with tree planting programs plots should be large enough to incorporate the future Variability.	ABGB Page 44-45 And AGB 46-47	The size of sample plots is not specified to allow for flexibility when designing the vegetation inventory. Note the requirements in Section E regarding determination and treatment of uncertainty. Where plot sizes are increased, and inter-plot variability is thus reduced, precision is improved (where sample size is constant) and uncertainty deductions will be less. Thus, there is already an incentive to do this, and it is not necessary, nor advisable, to be prescriptive here, again, to permit	Approved		

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			project proponents to select the sampling intensity and plot sizes, based on their resources and consideration of the trade-off with net credits potentially generated.			
17.	We often dry organic soils at 70 degrees instead of 105 degrees	BD Page 48-49	We recognize the variety of appropriate temperatures to dry soils but are more concerned with ongoing decomposition of the sample rather than loss of organic matter due to higher drying temperatures. In our experience, drying peat samples at less than 90C can take over a week.	OK if you experienced that drying takes significantly more time at lower temperatures. Then I agree that higher temperatures are preferred over longer drying time. Our experience is that 48 hours is enough and respiration ceases quickly.		
18.	Doc shows a mixed use of Arial and Calibri fonts.	General	Entire document changed to Calibri font.	Approved		
19.	Since the scope has a strict regional limitation, can a map be provided?	General	We have included a map delineating the coastal plain of southeast Virginia, North Carolina, South Carolina and Georgia.	Approved		

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20.	Definition provides little procedural guidance	43	We have made some revisions to further specify the definition of pocosins: "Pocosins are here defined as freshwater wetlands, <u>often dominated</u> by broad-leaved evergreen shrubs or low trees, on organic soils in the coastal plain of southeast Virginia, North Carolina, South Carolina or Georgia, that are seasonally saturated primarily through precipitation." The definition is adapted from and in general concordance with the definition from Sharitz and Gibbons 1982 (Sharitz, R.R. and Gibbons, J.W., 1982. *Ecology of southeastern shrub bogs (pocosins) and Carolina bays: a community profile* (No. FWS/OBS-82/04). Savannah River Ecology Lab., Aiken, SC). The revised definition is operable in an audit sense, and of comparable specificity to other methodologies.	Approved But consider rephrasing the underlined as it is not fully operable.	"Often dominated" replaced with "with some component of"	Approved
21.	Apparently, this is an appl. cond. to preclude	54	Have added the following applicability condition: "The	It seems there are no procedures in	We counter that the applicability	Approved.

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	leakage. What about ecological leakage (hydrological connectivity)? May it occur or should it be avoided by project design?		project activity does not result in increased GHG emissions outside the project area via hydrological connectivity (i.e. would not result in drainage of adjacent areas)"	place that work to guarantee avoidance of such leakage. You may consider including procedures for a hydrological buffer zone or otherwise account for potential negative effects in adjacent areas.	condition is sufficient and verifiable to address risk of ecological leakage, and consistent with precedent set in the ACR methodology Restoration of Degraded Deltaic Wetlands of the Mississippi Delta v2.0, which addresses ecological leakage with a similar applicability condition "WR activities may include wetland management activities to increase net wetland sequestration as long as activities do not cause deleterious	The appendix named "Ecological leakage appendix 16Aug2017" gives sufficient assurance that ecological leakage is unlikely to occur. The applicability condition now not only refer s to drainage of adjacent lands but also to raising water tables in adjacent lands, which may also increase off-site emissions.

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
					impacts or diminish the GHG sequestration function of habitat outside the project area"	
22.	- Add 'in the baseline' and remove 'previously' - Why scope limited to pocosin? Organic soil and rewetting - as defined - allow for a wider scope - Table 4 provides an exact regional delineation of eligible areas, which may be provided here too	56	Have added the text "Continuation of the drained state is the most likely without-project scenario." The text "The project area is a previously-drained pocosin." Is retained unaltered, as this serves to constrain application to lands with organic soils of pocosin origin and similar climate. The reasoning for limiting the methodology to pocosins is that: (1) The approach, particularly the stock change approach, is best suited to the organic peat soils and generally closed hydrology (no sedimentation or erosion) of pocosins (though admittedly, not exclusive to pocosins). (2) Research findings specific to pocosins support the assumption that methane emissions are	Minor point: Wondering why "without-project" is used instead of "baseline". Re 2: The fact that in the study of Richardson et al. the undrained (and rewetted) sites emit more CO2 than the drained sites, suggest that measurements do not show net heterotrophic emissions, but rather total soil emissions that include root respiration. Methane emissions are	"Without-project" changed to "baseline" Second comment addressed in accompanying document "response to comments re methane emissions.doc" *See Appendix to this document. Section "*Authors' Response to Reviewer #2 Comments #5 And #10"	Approved

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
			generally maintained below *de minimis* levels (3% of total GHG flux, per ACR) and provide justification for excluding this source in accounting in the context of pocosins (response to Reviewer 1 item #1). The same assumption may not be valid in boreal peatlands (see e.g. Wang et al 2015; Wang, H., Richardson, C.J. and Ho, M., 2015. Dual controls on carbon loss during drought in peatlands. Nature Climate Change, 5(6), pp.584-587). Furthermore, long-term research underway at Pocosin Lakes NWR by USGS and others (see e.g. ECU 2017, referenced in response to Reviewer 1 item #1) is expected to make the flux approach operational in the near term, providing a proxy relationship applicable to pocosin restoration projects in the region. Having available data and research, as well as available (drained) baseline reference sites at PLNWR, to support project accounting would substantially reduce monitoring burdens and make	easily below 5% if you include a source of CO2 that should not be accounted. Has this been considered?		

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			application of the methodology more accessible to project proponents. Have added specified geographic constraints from Table 4 in the operative pocosin definition in Section A1.			
23.	'expected to occur' is a rather weak condition. This should be 'does not occur' and the project needs to exert some form of control over this.	65	Changed from "is expected to" to "will."	Approved		
24.	- A ref area for baseline monitoring may be quite a challenge. I'm not sure what is gained over doing the classic ex-ante bsl assessment. Both have significant uncertainties but the ex-ante has no monitoring burden. - Even in the current approach, one needs procedures for defining	69	Although more involved than simply modeling and fixing the baseline ex ante, we deliberately chose the dynamic baseline approach (i.e. baseline determined ex post) as the most credible for accounting GHG benefits, particularly with changing climate that may not be well-predicted from recent historic conditions. Imagine modeling and fixing a baseline hydrologic model of the project area based on the last 10 years of	The approach does have merit and as it is argued is not inferior to ex-ante baseline assessment. The methodology should however provide good guidance for selecting a reference area for baseline monitoring, that	The methodology already contains detailed, and I would add *exhaustive*, criteria that must be verified to establish the validity, and continued validity, of a baseline reference site (Table 4).	Approved, after another read.

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	what the baseline is and what needs to be done if bsl deviates from what is assessed ex ante; current proposed monitoring seems very weak.		rainfall data, then suddenly during the project accounting period there is a dramatic increase in rainfall – the already fixed baseline assumes drier conditions than the actual, and would result in non-conservative net accounting. The only assumption fixed ex ante is that the baseline remains under a drained state; we have added the applicability condition: "Continuation of the drained state is the most likely without-project scenario." Once this is established ex ante, the monitoring requirements are sufficiently detailed to measure and account net emissions from the drained state baseline; certainly, superior to the absence of monitoring entirely in previous fixed baseline methodologies.	guarantees a true baseline during the entire monitoring period.		
25.	- AG biomass: It is not included in the BSL; do projects need to demonstrate that this is conservative? - Flux approach: first-time occurrence - refer	79 Table 1	AG biomass is always accounted for, though not explicitly as a baseline pool - change in aboveground biomass carbon stocks in the baseline is accounted in parameter ΔAGB_{wp} which	Nothing relevant in line 75.	Flux approach introduced in (current) line 101.	Approved

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	to relevant section where this is explained		represents the net of baseline and with project changes in aboveground biomass carbon stocks. Flux approach now introduced in line 75.			
26.	Is CH4 not increased due to rewetting? Very surprising for such systems. I have no access to the report referred to.	83	See response to Reviewer 1 item #1 A copy of the research findings is provided with our responses.	The drained sites of Richardson et al. are abandoned sites with dense fern vegetation. The rewetted sites of Richardson et al. do not include sites that were used for agriculture up until rewetting and that could provide copious easily degradable material until the more recalcitrant vegetation re-establishes. I understand that the pocosins of the PLNWR have	The methodology includes the applicability condition "The project area has been free of any land use that could be displaced outside the project area (e.g. agriculture) for five or more years prior to project start date"	Approved

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
				all been abandoned, but the meth should indicate that sites that were recently still used for agriculture are excluded via its applicability conditions.		
27.	- Is 'belowground' used instead of 'soil' because litter and roots are also included? I'd prefer to sue 'soil emissions' or better 'heterotrophic soil emissions' and explain that this is not limited to soil material strictly. When reading through the methodology this avoid confusion with BG biomass. A sentence like 794/795 becomes awkward. - One needs to exclude autotrophic root respiration of all vegetation not covered	88	"Belowground" is a broad, but essential, term that permits discussion across the accounting approaches, referring to both parameters BG_{stock} and BG_{flux}. The scope of these parameters is slightly different due to the different limitations of the approaches; the stock change approach focuses on net surface elevation change (which necessarily covers change in the soil and roots, and *includes root growth*), while the flux method focuses (now) on heterotrophic emissions from the soil (and dead root biomass, *but not root growth*) pool. See also response to Reviewer 3 item #6.	Re flux method: Not so in case of Richardson et al. 2014, who used dark chambers on soil from which vegetation has been removed. The CO2 fluxes related to remaining roots of clipped vegetation and of trees and shrubs nearby are not assessed. It is telling that CO2 fluxes are highest in the undrained (reference) site where large trees are present,	Comment addressed in accompanying document "response to comments re methane emissions" *See appendix to this document. Section "*Authors' Response to Reviewer #2 Comments #5 And #10"	Approved

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	in flux chamber, like trees and shrubs, and focus on heterotrophic respiration only – or use eddy covariance, which is difficult to link to proxies, however.			indicating that total soil respiration was measured (incl. autotrophic respiration) and not heterotrophic respiration only. The nearby eddy covariance measurements may show a net sink for the reference site, but how much of this is related to increase in standing biomass is unclear, although Noormets et al. conclude that about 1 t C per ha and year was lost from the soil. Compared with that number, the CH4 fluxes are not de minimis.		
28.	Surface elevation change occurs twice in	109	This has been corrected – see response to Reviewer 1 item #3	Approved		

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	this sentence and is both associated with stock change approach and flux approach.					
29.	Why are intentional fires in the bsl excluded? A rewetting project stopping intentional fires may gain significant carbon credits. See also figures 1, 2 and table 3.	111	See discussion re challenges to modeling any fires in the baseline in accompanying "accounting fire document. The current treatment excluding intentional fires in the baseline is conservative. Further, discussions with land managers in the region do not reveal e.g. stopping prescribed burning necessarily as a restoration goal; consider that some pocosin communities are fire dependent.	Approved Noting that a conservative standardized or default value approach (if feasible) might have benefitted projects.		
30.	Specify which GHGs to be accounted for. N2O and CH4 were excluded, which is not realistic if fire is applied in the wp case	113	GHGs accounted now specified in text. Agree. We have added N2O and CH4 emissions from intentional burns.	Approved		
31.	These are procedures, not applicability conditions	122 124 126 134 136	Added text "and measurement procedures adhered to"	Approved		
32.	How is 'appropriate' defined?	123	We have stricken the word "appropriate" as it is unnecessary. The parameter tables set sufficient precision/accuracy	Approved		

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
			requirements (i.e. it is implicit that any technology meeting those requirements is "appropriate"). We want to leave this open so that developing (particularly satellite-based) technologies may be used in the future (i.e. not restrict surface elevation change measurement to RSETS and RTKS). Total stations could also potentially be used. "Appropriate" need not be defined here, beyond defining the parameters produced and general requirements as already detailed in Section E under Data and Parameters Monitored: "Acknowledging the wide range of valid monitoring approaches, and that relative efficiency and robustness are circumstance-specific, sampling, measurement and estimation procedures for measuring are not specified in the methodology and may be selected by project proponents based on capacity and appropriateness. Stratification may be employed to			

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			improve precision, but is not required. Estimates generated must: 1. Be demonstrated to be un-biased and derived from representative sampling 2. Sampling error quantified with 90% confidence 3. Accuracy of measurements and procedures is ensured through employment of quality assurance/quality control (QA/QC) procedures (to be determined by the project proponent and outlined in the monitoring plan)"			
33.	Emissions occur from the entire non-saturated layer (and a little also from the saturated part with methanogenesis). The lower part of the aerated layer has lower BD than the top 10 cm, so using the top 10 cm is not necessarily conservative. Subsidence is in part due to compaction,	127	We disagree. Net benefits will be driven by baseline emissions. Use of the lowest BD in the profile will result in an estimate of emissions in the baseline biased downward, and thus conservative. Note that compaction must be avoided per the last applicability condition.	Approved But note the term compaction is used with a specific meaning in studies on peat subsidence. To avoid confusion, I suggest rephrasing to 'compaction by machinery or treading'	We have further specified in the text that compaction is "by machinery or treading"	OK

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	both of the aerated and saturated layer.					
34.	What is 'root expansion'? if not growth, do they swell? However, in section E (for $\Delta SE_{wp,t}$) it reads: 'No root expansion and related swelling'. Confusing	130	"root expansion" refers to root growth (i.e. expansion of root networks), which can result in surface swelling (and be registered as part of net surface elevation change using the stock change approach). The baseline reference site per applicability condition #4 avoids swelling of surface elevation due to root expansion that typically follows going from a wet to a drained state (that would not occur in the project area, which must have been drained for a similar length of time per baseline reference site criteria Table 4) and permits focusing the approach on emissions from peat oxidation (though not exclusively). Following text (line 150 and Section E) removed (unnecessary, and incorrect as root expansion and surface swelling may occur in the project case where revegetation occurs) "No root expansion and related swelling is	Approved		

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
			expected in the with-project re-wetted case, and subsidence due to root die back is treated as an emission (assuming emissions from belowground biomass mortality occur at the time of measurable subsidence)."			
35.	- This assumes that the soil profile is the same all over the project area, which will not likely be the case. Moreover, it seems to assume that the area is completely flat and that water table depth (relative to the surface) is linearly correlated with surface height (relative to datum). Both assumptions are likely to be false. How many repeat-measurements? How to avoid measuring pseudo changes? - Need to define if rewetting has already occurred at t=0. Does the crediting period by definition start at t=0?	135	The applicability condition is here to exclude the influence of moisture-related shrink and swell from surface elevation measurements (so that they can be related to CO2 flux), and avoid measuring "pseudo changes." For the applicability condition to be effective, it need not be assumed that water table is constant across the project area, only that water table *relative* to water table at t=0 is constant across the project area. We have added the text ", as recorded at the same site(s) measured at t =0" to reflect that water table may be measured at one or more sites across (or near) the area, and must be re-measured at the same sites.	Approved Minor comment re "constant across the project area": Can you rule out hysteresis effects (different volume whether the soil is drying or getting wetter)? Just a point of interest, error is most likely within your margin of error.	Comment noted. We agree that swelling at a given water table could be slightly different if wetting vs drying.	OK

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	If yes, the remark in line 258 should also make clear that this is t=0.		Soil profiles are not, nor need not be, assumed similar across the project area. Repeat measurements (of surface elevation, and also necessarily of water table) are made as often as monitoring takes place (at least every 5 years). Line 258 has been changed to read "The start of the crediting period is marked by the start of the project activity, i.e. following the onset of rewetting. Note that using the stock change approach the start of the crediting period must be no less than 12 months following the onset of rewetting."			
36.	The parameter table for $\Delta SE_{bsl,t}$ mentions 6-12 months. This is not the same as 'no less than 12 months'.	136	Changed to no less than 12 months. Now consistent.	Approved		
37.	- After 'swell' add 'of organic top soil' - How does one assess whether this swell is concluded? Once fresh vegetation starts	137	The only operative constraint is the no less than 12 months following re-wetting. The text in parenthesis "after initial swell has occurred" simply explains the rationale behind this, to exclude	Approved Minor remark re last sentence: Will the new material behave exactly as the old with	Soil moisture-related shrink/swell behavior of the new material will likely be different	OK

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	growing one will have to deal with an entirely new situation in which swelling of the upper layer may be prominent		surface elevation change due to soil moisture. Once fresh vegetation (and root biomass) starts growing, it will be registered as a component of net surface elevation change and corresponding CO2 flux.	respect to swelling/shrinking when more or less moist?	with increased root networks and pore space; however, we suspect that this effect on surface elevation will probably be overwhelmed by swell due to new root volume (= surface elevation change that is accounted). Recall also that change in carbon stock accounted = surface elevation change (corresponding volume) * BD, and that BD must be re-measured every 5 years with the stock change approach (and we expect that BD will change due to root expansion	

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
					and organic matter inputs where vegetation is growing).	
38.	No wind erosion, no animal disturbance: any criteria and procedures to establish this?	139	Wind erosion not a significant factor in the applicable region and organic soils involved. Animal disturbance likewise not expected to be a significant factor impacting surface elevation levels on a large scale.	Approved Noting that this is difficult to verify	Comment noted.	
39.	Is this realistic for the bsl? What are criteria and procedures to establish this?	140	Procedures to avoid compaction due to surface elevation measurements need not be specified. They could include e.g. constructing board walkways around RSETs on which to stand while taking measurements. Other options are possible and we leave this open intentionally to allow for innovation and for project proponents to select approaches that are appropriate to their circumstances/site conditions. Otherwise, what significant source of compaction might take place in the baseline reference site or project area? Passage of heavy	Approved But note remark in #16.	We have further specified in the text that compaction is "by machinery or treading"	OK

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
			machinery? Sites could easily be selected where this possibility is precluded.			
40.	Flux Approach: some justification is needed as to how appropriate this is for forested systems.	149	The flux approach has been revised to focus solely on heterotrophic respiration. Autotrophic respiration of tree and shrub roots is excluded per revised requirement: "The methodology now specifies for the flux approach that "Independent variable is restricted to heterotrophic emissions (due to microbial respiration) from the soil organic carbon and dead belowground biomass pools" See also response to Reviewer 3 item #6.	Approved		
41.	Also, here not all are applicability conditions. If part of procedures, they should be dealt with there and not here.	151 - 165	We've stricken the term "applicability." Certainly, these are all clearly "conditions." These are not necessarily procedures for the project proponent, though they could be, because some pre-existing, and applicable, research could be used.	Approved		
42.	- Check if this is covered: Where to	155	The proxy relationship is established from measurements	Approved		

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	measure proxy? How to deal with spatial heterogeneity? How many repeats? Yearly coverage (frequency)? Refer to section E - Does one need a measure for goodness of fit, e.g. RMSE or r2. How do you develop proxies? How many measurements? Would be good to add more guidance.		at a "… study site … on pocosins or former pocosins (as defined in Section A1)" (applicability condition for flux approach #5). For monitoring, proxy variables are measured at locations per condition for flux approach #6 a, b and c (and repeated in the parameter tables in Section E). How to deal with spatial heterogeneity? How many repeats? Yearly coverage (frequency)? Each of these questions is addressed in the relevant parameter tables in Section E. Note that stratification may be employed, but not necessarily. We have added the text "See also Section E for further guidance." RMSE must be calculated as the basis for determining uncertainty in the relationship. Number of base measurements is not specified, nor need be. The existing requirements (significant, un-biased) should be sufficient to ensure that only robust relationships are employed. Where errors around *predicted* –			

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
			observed are still substantial, these will be reflected and accounted for through treatment of uncertainty.			
43.	- Sites for proxy development must be similar to the sites in the bsl and wp. Define similarity or refer to section E - 'Former pocosins' are not defined	157	The proxy relationship is established from measurements at a "… study site … on pocosins or former pocosins (as defined in Section A1)" (applicability condition for flux approach #5). "Former" pocosin has been revised to "drained" pocosin.	Approved		
44.	So many unknowns/assumptions in such an approach. Calculations based on site characteristics are a standard approach in deriving annual fluxes from intermittent measurements, but generally applicable, robust derivation of GHG fluxes from driver parameters alone are yet to be developed? Apparently, there are	163	"Driver" changed to "proxy" to avoid interpretation of the variable as necessarily causal; only correlation is needed. It is also expected that fluxes will be estimated from multiple variables. See also response to Reviewer 2 item #49.	Approved		

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	always additional factors that drive fluxes, e.g. soil characteristics, porosity, WFPS, microbiome… Instead of driver variables one can us correlative proxies like water table depth or other.					
45.	This text warrants a remark that the project should avoid the models to produce unrealistic outcomes if extrapolated outside the measured range.	167	Not necessary. Already constrained in Section E "If the value of any proxy variable is outside the range of values for which the relationship with emissions was determined, the emission value is set equal to the corresponding lowest or highest estimated emission value for that range"	Approved		
46.	- Although high level, it is confusing that it seems as if biomass increase in the bsl is not accounted for (not accounting for biomass increase in the bsl is not conservative). Only later (line 292) it turns out that the term in	186 Figure 1 198 Figure 2	We have added the following note in the caption for each figure to explain and clarify. "Note that change in aboveground [or above- and belowground] biomass carbon stocks in the baseline is accounted in $Net\Delta AGB_{wp}$ [or $Net\Delta ABGB_{wp}$] which represents the net of baseline and with project changes in this pool, hence "*net* change""	Approved		

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	the figure is a net term comparing bsl and wp. Consider if a note would improve readability. - No intentional fires in the bsl? See also Table 3.		No intentional fires in the baseline, as already explained in response to Reviewer 2 item #12.			
47.	Not clear why bsl proxies are monitored in the project area	207 Table 3	See response to Reviewer 1 item #11	Approved		
48.	Minimum size of reference area not defined. How is cherry picking of favorable reference areas avoided?	210	The suite of similarity criteria should be sufficient to prevent cherry picking, or selecting an unrepresentative site. We deliberately did not set a minimum size, for 2 reasons: (1) the area and total emissions of the reference site are unnecessary for accounting (it is only a frame within which to locate samples), and (2) it allows for reducing the management burden, as well as the challenge of identifying an area that meets all of the criteria, by allowing a small, but representative area to be used. The last point is critical to making a control site approach work, which we agree with the	Approved Noting that this explanation may somehow be added to the procedure.	The explanation is not critical to the functionality of the methodology, and we choose to leave it out to minimize the volume of the narrative.	OK

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
			reviewers, is a challenging, but we believe necessary, aspect of this methodology.			
49.	'Existing baseline reference site' seems a term for insiders	213	We have removed this term, as well as reference to the PLNWR site in Table 4 (which, in the future, could still be used as a baseline reference site by projects, provided that all of the criteria in Table 4 are met and documented in a project GHG Plan).	Approved		
50.	- 2nd row: What about former land use? Similarity of vegetation (incl. criteria)? - 3rd row: Criteria? Procedures to establish this? Soil compaction as active process or as in compacted peat? - 4th row: Vague: about half of observed water table depths may be above the average annual water table - 5th row: 0.2 - How valid is this? One may expect BD to vary a lot also across the site,	214 Table 4	2nd row – have deleted "formerly with pocosin vegetation" as it is not easily verifiable, and the current constraint around geography, freshwater and organic soil would seem sufficient to establish similar origin of peat. We would counter that former land use is unnecessary – it would be redundant as critical factors impacted by former land use are already covered (e.g. bulk density, percent carbon, peat depth, length of time drained) 3rd row – clarified to refer to *ongoing* soil compaction. Compacted peat would be	Approved		

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	particularly considering these are woody peats - 6th row: % of dry weight - 9th row: rational of 'less than' is unclear - There should be a measure of quality of the comparison e.g. based on minimum sample size for some of these parameters. The table lacks criteria. - In line with comment on 210, it seems that allowing +/- 20% difference in bulk density can give the project a significant free ride. Perhaps this is unlikely to be the case but some justification would be welcome. - 'Similar' vegetation is hard to apply operationally. Any suggestions for more detail?		covered under the bulk density similarity criterion. To make the criteria more auditable, they have been changed to "Flat terrain (slopes not exceeding 10%), not located within any immediate river floodplain, and unlikely to be subject to significant ongoing soil compaction and/or mechanical disturbance (e.g. tilled farmland subject to repeated traffic by heavy machinery)." The above serve to ensure that the reference site is not subject to significant erosion, sedimentation or soil compaction. 4th row – removed text "significant sustained flooding above average annual water table or" (not necessary, the requirement for "drained" in the first criterion is sufficient). 5th row – we recognize that bulk density, and many of the other parameters covered here, can vary considerably across the landscape. This should be an average value drawn from representative			

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
			sampling of the site (and where monitoring sites would similarly be located in an un-biased manner per requirements in Section E). Stratification could be employed, but is not necessary. 6th row – have added text "(as % of dry weight)" to clarify 9th row – the criterion is constructed in this was "Equal to or less than mean peat depth in project area" to provide flexibility → the baseline reference site may have a similar depth of peat, or it may have less depth of peat, which is conservative (i.e. subsidence and corresponding emissions would stop sooner than on the project). In response to this comment, we have added the following text to ensure that all quantitative criteria are accurate and drawn from un-biased sampling. For BD, %C and peat depth: "Note that for all quantitative criteria, estimates must be derived from un-biased, representative sampling of the			

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
			reference site, with a minimum sample size of 20, and accuracy ensured through adherence to the same measurement procedures for corresponding parameters measured and monitored in the project area (Section E)." For water table depth: "Average annual water table (for the year preceding the project start date) must be estimated from data from a groundwater well located at the site, or if this does not exist, from the nearest USGS groundwater well, sourced from https://waterdata.usgs.gov/nwis/gw ." *[note also that we have now standardized parameter requirements across the methodology to require minimum sample size of n=20 for all parameters without treatment of uncertainty and all parameters subject to significance testing, and for all other parameters where uncertainty is quantified and deducted, minimum sample size is not specified]*			

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
			The +/-20% similarity threshold provides flexibility in identifying reference sites (from an anticipated limited pool), and is in keeping with the magnitude of threshold allowances set in other methodologies (e.g. ACR REDD methodology modules). Have revised the vegetation similarity criterion to improve auditability. Criterion now specified as "age class within 10 years, percent cover trees and shrubs, and basal area of pines > 10 cm dbh within +/-20%"			
51.	- Unintentional burns are included in baseline accounting. Why would a bsl burn disqualify the reference area? It may also happen in the baseline of the project area itself. - Otherwise the flexibility provided here is excellent.	221	Unintentional burns are not included in baseline accounting. A burn in the baseline reference site would invalidate the samples, which must be applied to un-burned areas in the project area (hence the term $(A - A_{burn\_unint,wp,t})$ in eqs 2, 4, 7, 11 and 14)	Approved		
52.	'reconfigured to comply with the	223	Text has been rephrased to: "reconfigured, while continuing to	Approved		

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	similarity criteria' - unclear what this means. What if this re-adjustment results in net emissions? Should one not account for those?		ensure compliance with the similarity criteria" Reconfigured means, e.g., excluding a burned area (and any sample points within it) from the reference site, and this explanation has been added to the methodology text to clarify. Any change to the baseline reference area (and the population of samples within that area) may affect baseline rates of emissions and net accounting going forward, and is unavoidable. The old and new reference sites, which both must meet the same similarity criteria, are considered "correct" for the accounting periods they are applied to. No "correction" in accounting is needed at this transition. Impact of a transition are also mitigated by the fact that the area and total emissions of the reference site are used in accounting (the reference site is only a frame within which to locate samples which drive			

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
			baseline rates per unit area, not total baseline emissions).			
53.	If this is the annual change, where are the annual changes up to time t summed? See also 344, 410 and various other equations.	228	Annual changes are not summed for the monitoring interval. They are annualized across the monitoring interval, with the term 1/x (x = # years in the monitoring interval) in equations where needed, and then applied as annual values to each year *t* within the corresponding interval. The subscript *t* refers to the year since project start, using the same convention as all methodologies.	Approved. It would however be helpful if the name of the variables would already suggest that these concern annual averages.	We have further specified that annual values refer to *mean* annual values in the descriptions of the following parameters: $\Delta BG_{stock\_wp,t}$, $\Delta BG_{stock\_bsl,t}$, $Acc_{bsl,t}$ and $Acc_{wp,t}$	OK
54.	% dry weight	310	"(percent dry weight)" added to text for further specificity	Approved		
55.	This description does not seem to be in line with 'in monitoring interval ending in year t' in line 304. See also elsewhere in the document.	315	See response to Reviewer 2 item #36 above. The description is correct. The subscript *t* does not refer to the monitoring interval ending in year *t*.	Approved		
56.	'valid' and 'control site' need to be explained. Use terms 'reference area' and 'monitoring plots' consistency.	318	Good spot. Changed to "the baseline reference site" (which by definition must comply with all similarity criteria in Table 4, and therefor "valid").	Approved		

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
57.	If this is the annual change, where are the annual changes up to time t summed? See also 410 and various other equations.	344	See responses to Reviewer 2 items #36 and 38.	Approved		
58.	How is 'significant' determined? Refer to what is provided in step 2.	369	Inserted here same text from Step 2 "(significantly different using an unpaired t test at P <0.05)"	Approved		
59.	Replace 'than' with 'from'	427	"than" changed to "from"	Approved		
60.	Unclear what 'predate their implementation' means. 'Plans for intentional burns (e.g. prescribed burns) in the project area, that predate their implementation…' could be replaced with 'Intentional burns (e.g. prescribed burns) in the project area, that predate the implementation of burn plans…'. See also 758.	438	Text is correct, and explains that the absence/presence of burn plans establishes the distinction between unintentional and intentional burns (which are treated differently in accounting). Obviously "plans" should logically predate implementation, and for clarity we have stricken this text as redundant. Note that burn plans are also essential for accounting emissions from intentional burns using the flux approach, as the planned burn areas require monitoring prior to implementation (see Section D.2.3).	Approved		

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
61.	Belowground: See comment on line 88	486	See response above to Reviewer 2 item #10	Approved		
62.	- Is accretion of peat not included in the flux measurement? - How is litterfall measured?? No need to assess litter from herbs and grasses separately if they are included in the chamber	487	Accretion of peat (via litterfall) is not included in flux measurements. Flux approach now restricts BG emissions to heterotrophic respiration from the soil (i.e. autotrophic respiration and heterotrophic respiration of litter (above the soil/peat surface) must be excluded). Peat accretion/litterfall is monitored independently by monitoring accretion above a reference marker in the baseline reference site and project area.	Approved		
63.	For sake of perfection, description of t is lacking. Also 605 and perhaps elsewhere.	492	"t 1, 2, 3, … t years elapsed since the project start date" added here and throughout	Approved		
64.	Parameter lacks a t and the description is not the same as the one for eq 8. See comment for line 344 - unclear how annual emissions are summed over the 'interval ending in year t'.	492	See responses to Reviewer 2 items #36, 38, 40 and #46. Eq. 8 and 9 descriptions for delta BG reconciled.	Approved		

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
65.	Helpful to refer here to the relevant section D.2.2	503	Not clear how Section D.2.2 is relevant here. We looked again closely and don't see a need for an additional reference.	Reference may be incorrect but the idea was to refer again to the procedures for flux method	Section D.2.2 referenced in narrative below equation D1.	Approved
66.	Precipitation: strange proxy. Unlikely to work well as much depends on structure of vegetation, soil, root system, air temperature etc. Is there any good experience?	515	Proxy variables need not be defined in the methodology. Precipitation could be used as a predictor of water table level or soil moisture (e.g. to drive a hydrologic model), which in turn are proxies for emissions; Richardson et al found the following to be the strongest correlates with emissions on pocosin study sites: air temperature, water level and volumetric soil moisture. The methodology does not preclude two modeled relationships from being used together.	Approved Note: Richardson et al looked at instantaneous emissions (expressed as gram per hour); consider looking at proxies for annual emissions. Meta-studies have e.g. shown that water table is a weak proxy for instantaneous emissions but a very good one for annual emissions.	Comment noted. Recall that the flux approach requires that the "[proxy] relationship must be based on emissions assessed over at least one entire year, with frequent (at least bi-monthly) measurements" and that the output variable must be in t CO_2e ha^{-1} yr^{-1}	OK
67.	See comment on line 344. Where is the summation executed?	519	Not the same issue as item #40. Summation is immediately below equation 10 (and below equation 13).	Approved		
68.	Please justify why mean peat depth is	527	We don't see the potential source of inaccuracy in this approach for	You need to make sure that	We understand the issue raised,	Approved

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	going to work and why one does not need a spatially explicit approach and stratify the area according to peat depth. Fluxes are related to peatland area and the decline of the area will not be linear with time (the smaller the area becomes, the smaller the losses as these occur along the ever-shorter shallow edge). Moreover, the project area will not be homogenous in terms of peat loss in the bsl or restoration success in the wp.		deriving an overall stock estimate at the project area scale. We expect peat depth (and other variables) to vary considerably across the area. The area can be stratified, but not necessarily. All of the measured components here, bulk density, C% and peat depth must be derived from representative sampling and each with a minimum sample size of 20, requirements that should be sufficient to produce un-biased (and reasonably precise) estimates of total stock. Referencing a fixed area (and an average depth) over time yields a constant overall rate of emissions. If the area were instead stratified as recommended, rate of emissions over time would decline (as expected with a declining surface area where shallower areas are progressively depleted), but emissions would continue for a longer time than if using an average depth and fixed area. With both approaches, the total stock emitted *is the same*.	everywhere peat is thick enough so as to avoid being lost entirely before the project ends. Otherwise one would claim reduction of emissions that would never occur because the peat would be depleted. This criterion requires stratification of the project area into areas with peat thicker than would be lost over the project duration and areas with shallower peat. It also requires a conservative (high) estimate of subsidence rates in the drained baseline. Re "With both approaches, the	and counter, again, that stratified or un-stratified, total estimated volume/mass of peat will be the same, and so the constraint on total potential emissions is the same. Reviewer 1 agrees that the approach is conservative (see Reviewer 1 comment #14). While stratification could improve the accuracy (or better, realism, as this discussion relates to a counterfactual scenario) of annual emission estimates, its inclusion as a requirement would not	

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
			Our approach is an intentionally simplifying procedure, to avoid intensive peat mapping. While we acknowledge that it could produce over-estimates of short-term rates where there are large areas within the project area with substantial differences in peat depth, again, it should not produce long-term over-estimates of total emissions. We should also note that there is a grossly conservative assumption employed here, referencing bulk density from the top 10 cm and applying it throughout the profile to produce the total initial peat stock estimate. This results in an under-estimate of total stock, which means that peat stocks are depleted, and emissions stop, in the baseline sooner than they should.	total stock emitted *is the same*": Yes, if one assumes that ALL peat will be gone in the baseline. Is that a reasonable assumption or will a drainage limit be reached at one point, beyond which drainage simply becomes too costly? You may explain how your approach meets ACR requirements.	improve the overall conservatism of the methodology. Stratification is not made a requirement in the methodology specifically to improve its simplicity and operability, avoiding the need to define "homogeneous", or define to what resolution a parameter must be stratified. To address the principle concern raised by the reviewer, we have revised the peat depth parameter to represent the first quartile of the range of peat depth	

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
					measurements taken, rather than the mean, ensuring application of a conservative value to constrain total potential emissions. The comment regarding drainage limits is a good one, and we have also revised the peat depth parameter as … "peat depth above low water level" and established procedures to define the low water level (or observed drainage limit). Note that the stock change approach addresses this issue by	

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
					monitoring subsidence rates at a baseline reference site with mean peat depth at project start date equal to or less than mean peat depth in project area (Table 4).	
69.	(and elsewhere) tons should be tonnes as in 878	533	"metric tons" (Mg) is retained and follows convention of other ACR methodologies. "Tonnes" changed to "tons" in line 878.	Approved		
70.	Why? In absence of shrubs or trees, all fluxes are included in the chamber measurements (unless chambers are very small)	553	As before, BG emissions in the flux approach now restricted to heterotrophic respiration. Autotrophic respiration must now be excluded per new procedural requirements. See also response to Reviewer 3 item #6.	Approved		
71.	Technically this might be in order, but what kind of project are you referring to here? It seems that rewetting and restoration have	625	We don't expect peat stocks to be depleted in a re-wetted project area, but this safeguard needs to be here for completeness and to account a failure, should it occur, however unlikely. The	Approved Technically indeed in order, just a question out of curiosity.		

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	totally failed. Are there other checks and balances to make sure that rewetting is executed properly and the situation of the left-hand term in line 630 even moving in the direction of $BG_{wp,t=0}$ is avoided?		methodology is only a monitoring and accounting instrument, it need not set bounds to drive outcomes. Avoiding a reversal is left to the capacity and resources of the project proponent and land manager (and accounted in part, separately, through the risk assessment).			
72.	Replace dot with comma	769	We could not find the referenced dot/period.	Sorry, 796, right after parameter $\Delta ABGB_{wp,t}$	Change made (now line 877)	Approved
73.	f) RMSE versus 95% interval? - 'the emission value is set equal to the corresponding lowest or highest estimated emission value for that range': need to justify that this is conservative, as this seems not necessarily the case	837	The requirement has been revised as "If the value of any proxy variable is outside the range of values for which the relationship with emissions was determined, emissions are set equal to the predicted value corresponding to the end of the proxy variable range (closest to the actual proxy variable value)" to ensure that the regression model is not used to project beyond the range of independent variables from which it was derived.	Approved		
74.	$BD_{wp,t}$ 'sample cores of known volume': need to define a minimum size if including living	Section E	The following text was added to the BD QA/QC procedures as found in the parameter table:	Approved In the parameter table, you	Parameter tables for BD now specify "using ***cores collected***	OK

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	and dead biomass as well as litter and peat		", it is essential that compaction is avoided in the process of obtaining and working with field samples. The following precautions should be adhered to: 1) When obtaining the sample, particularly when trimming the end of the core to a sampling ring, avoid compressing, compacting or disturbing the sample. 2) The core should be oven-dried prior to sieving. 3) Large cores (≥ 8 cm diameter) should be used preferentially; compaction tends to occur where the edge of the sampling ring meets the soil surface, and larger cores have a smaller surface to area ratio in cross section." Note also that there are now 2 different BD parameters, one for the stock change approach (includes coarse root biomass) and the other for the flux approach (excludes coarse root biomass, i.e. "traditional" BD). Litter is now excluded from all of these, and litter mass and carbon content measured separately.	mention that monitoring can be done on permanent sampling plots, which is awkward as BD determination is destructive. Consider rephrasing.	***from*** temporary or permanent sample plots"	

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
75.	$\Delta SE_{Acc,bsl,t}$: 'soil horizon' is not defined (criteria?) and markers within the organic soil can change their absolute height in response to subsidence.	Section E	The text has been changed to revise "soil horizon" to "marker horizon", a commonly used term (see e.g. https://www.pwrc.usgs.gov/set/theory.html). The new text reads "Procedures to monitor surface elevation change due to peat accretion/litterfall shall use a marker horizon, such as a feldspar marker." For accretion (only) measurements, the absolute height of the marker relative to a reference datum need not remain fixed. Only depth of the accreted layer above the marker horizon is needed.	Approved In the parameter table, you write 'Where signs of significant soil disturbance, including bioturbation, are encountered at a sample point, the disturbed sample sites must be excluded from the analysis.' This raises the question of whether such disturbances are frequent and to what they amount. If large animals (bears, deer, hogs?) disturb the soil to a significant extent, how do you deal with that?	We expect these disturbances to be infrequent and small scale, and thus to rarely occur on sample points. Again, per the parameter requirement, where they do coincide with sample sites, those sites must be excluded from analysis.	OK
76.	$\Delta SE_{wp,t}$ - 'four decimal points (1/10 mm)':I s this	Section E	Each project must explain how they define the surface, and maintain that definition constant	There is no problem with approaching	We had originally included tracking accretion to	Approved

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	realistic? How is the surface defined? - 'No root expansion' Is this realistic considering growth of herbs and shrubs? - 'swelling': How realistic is this? Swelling will also depend on the upper layer of vegetation/litter that will swell independent of the peat. Should new peat accumulate, this peat will swell and sink depending on the available water. Deeper peat layer may sink/swell as well.		(see relevant parameter tables in Section E). Precision of measurements of net surface elevation change is set to 1/10 mm; relevant parameter tables specify measurements of surface elevation (in meters) to "four decimal points (1/10 mm), where possible." Webb et al. 2013 give a comparison of vertical accuracy using different techniques for measuring wetland surface elevation. Webb, Edward L., et al. "A global standard for monitoring coastal wetland vulnerability to accelerated sea-level rise." Nature Climate Change 3.5 (2013): 458-465. Highest precision achieved in surface level measurements ranges from 0.5 mm to 2 cm across total stations, RSETs and RTKs. Admittedly, measurements to a 1/10 mm may be ambitious, but we add the qualifier "if possible", and have developed the methodology anticipating	subsidence (in the cm range) in this way. Yet, the question remains whether it is sensible to measure with such accuracy and then claim changes to be due to peat accretion (which is likely less than a mm per year). The accuracy would far outweigh precision. More so, as you allow each project to define its own surface. The question where litter stops and peat begins hardly has an objective answer… One option would be to not account for peat accretion, but only for	account for potential reductions in the litter pool in the with-project scenario (due to reduced inputs), however, 1. Reviewing Richardson et al 2014's findings of *higher* litter accumulation rates on reference and restored sites compared to drained sites, and 2. acknowledging the reviewer's	

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
			eventual improvements in resolution of these technologies, particularly some of the remote sensing technologies (that currently provide too coarse measurements to detect the level of changes in surface elevation expected). We anticipate that subsidence rates in the drained baseline reference sites will be ~ 0.8 – 1.2 cm/yr. (U.S. Department of Energy and NC Energy Institute. 1982. CGIA: Peat Deposits of the Pamlimarle Peninsula. and Dolman, J.D. and S.W. Buol. 1967. A Study of Organic Soils (Histosols) in the Tidewater Region of North Carolina. North Carolina Agricultural Research Service Technical Bulletin 181, 52 p.) And expect that current resolution of these approaches should be sufficient for monitoring every 5 years (as monitoring frequency currently specified in the methodology).	avoided peat degradation. Not much would be lost in terms of credits (accretion in the mm range, avoided subsidence an order of magnitude higher) and it is an additional conservative approach that raises credibility. Other methodologies and projects follow this approach as well.	comment, and agreeing that peat accretion is unlikely to be resolved with any precision on typical project monitoring timescales, we elect to follow the suggestion to exclude litterfall/peat accretion from the methodology, and have revised the methodology accordingly. Note that accretion will continue to	

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
			The referenced text re root expansion and swelling has been deleted (see response to Reviewer 2 item #17).		be tracked in the stock change approach as a component (but probably undetectable one) of net surface elevation change.	
77.	The baseline scenario assumes continuation of the pre-existing drained state, and ongoing emissions from the soil organic carbon (peat) pool associated with drainage. The use of the term baseline threw me until I got well into the document. I for some reason was thinking it was a reference and not disturbed drained site so maybe a clarification is	45	"Baseline" and "baseline scenario" are standard terms used in all ACR methodologies and should be readily interpretable in a project context. We use the term "baseline reference site" consistently throughout the methodology to refer to the site outside the project area that represents conditions in the baseline scenario, and within which permanent sample sites are monitored for surface elevation change, proxy variable values and/or live tree and shrub biomass stock change. Because our approach is not a strict	OK, now except on line 122 both baseline and reference are used again. This could be simply solved by defining what is meant by baseline site (done in document already) and then saying this is being defined as a "baseline reference "against which to compare. Otherwise folks	To avoid confusion, we have changed the term "baseline reference site" to "baseline site"	

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	needed on this since some reviewers may think baseline is a reference undisturbed site. On line 132 you even call it a baseline reference site and this really confuses the comparison Two years may not be long enough since N fertilizer legacy may last longer. A more reasonable number may be 5 years for agriculture lands that are not in use, other could be shorter (2 yrs.) if say in forestry or pasture	54	experimental design we avoid the term "control" or “control site.” We have changed the applicability condition from 2 to 5 years without productive land-use, to avoid both activity shifting leakage and N2O emissions.	not familiar with this will think they may not be the same. A reference site is not referred to as a baseline site in most environmental work. Ok, now as changed to 5 yrs.		
78.	While water table and temperature have often been used as proxy variables it may be important to note that water	91	We have intentionally not specified which proxy variables may be used (and have seen the improved correlations with soil moisture of Richardson et al 2014, and Wang et al., 2015,	Ok, changed to Soil Moisture		

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	depths have not been as accurate as soil moisture in predicting GHG flux in pocosins (See Richardson et al 2014, and Wang et al., 2015) Richardson et al. 2014. Impacts of Peatland Ditching and Draining on Water Quality and Carbon Sequestration Benefits of Peatland Restoration. Final Report. Duke University Wetlands Center for the US Fish and Wildlife Service and The Nature Conservancy. Raleigh NC. USA. 19p.		which a prospective project proponent in the region should consider).			

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	Wang, H., C.J. Richardson, and M. Ho. 2015. Dual-controls on carbon loss during drought in peatlands. *Nature Climate Change* 5:584-587.					
79.	RSETS can be used but the number of replicates and season (wet vs dry) measurements must be carefully considered. More replicates are needed since the surface of the sites are uneven and shrink and swell conditions in peatlands are large. Feldspar markers at the surface may give a reading over the short-term.	122	Season of measurements/re-measurements has been specified in the applicability criteria/procedures for the stock change approach. Sample sizes have been intentionally left open, however note that a minimum sample size of n=20 is required for all monitored parameters, and the relative precision achieved directly impacts the uncertainty deduction.	OK		
80.	The top 15 cm may give a more realistic	126	Methodology procedures for BD now specify separating out	Line 160, should clarify that the	Have added "(below any	

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	picture of site characteristics and emissions etc. Meaning the litter should probably be separate and the 10 cm of soil below be used. This should be addressed		litter (existing litter depth, variable) and peat/soil (to 10 cm below soil surface) and determining mass of each separately. See also response to Reviewer 1 item #7.	BD is taken from the soil -10 cm with the litter removed.	overlying litter layer)" to referenced text.	
81.	Why is the term reference added here? Very confusing to most scientists and in the field of wetlands for sure.	132	See response to Reviewer 3 item #1 above.	Line 166 still refers to baseline reference site. clarify, See above statement 1	Term "baseline reference site" changed to "baseline site"	
82.	The difference between heterotrophic soil respiration (R_h) and autotrophic soil respiration ($R_{a,}$) must be considered in flux estimates between sites, otherwise, one cannot accurately calculate C storage differences as R_a in	144	The methodology now specifies for the flux approach that "Independent variable is restricted to heterotrophic emissions (due to microbial respiration) from the soil organic carbon and dead belowground biomass pools (i.e. autotrophic respiration and heterotrophic respiration of litter must be excluded)"	Ok, clarified on line 207, but this needs to be tested in the field before one can truly verify	The requirements for the flux approach (starting line 164) will ensure that any approach is tested in the field and verified (both through the	

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	restored sites is as much as higher (3 times) than that in drained sites due to higher production at the restored sites than at the drained sites. Moreover, drained site R_h respiration accounts for up to 90% of soil respiration but only ≈ 50% in the restored sites. (unpublished data).		This also corrects a previous embedded error in the methodology whereby autotrophic respiration was being double counted (also tracked as a component of net stock change in belowground live biomass). Note that we have not specified how the research methods must exclude autotrophic respiration, to permit flexibility (across a range of circumstances and available resources) and allow for innovation. Other wetland methodologies have similarly avoided being prescriptive in this regard (e.g. VCS VMD0042 and VCS VMD0046 of VM0007, and VM0033). Some possible approaches to exclude (or minimize) autotrophic respiration would be to incubate peat bags in situ (e.g. Blodau, C., Roulet, N.T.,		requirement for peer review, and subsequently via the ACR project validation and verification process). Also, on line 168 "Independent" corrected to "Dependent" variable.	

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
			Heitmann, T., Stewart, H., Beer, J., Lafleur, P. and Moore, T.R., 2007. Belowground carbon turnover in a temperate ombrotrophic bog. Global Biogeochemical Cycles, 21(1)), or to site flux chambers to reduce the influence of tree and shrub roots; while the latter introduces some bias, the same bias would be necessarily applied in both the baseline and with-project cases (because "The same relationship must be used in both the project and baseline cases"), and accounting is driven not by the magnitude of the estimated flux, but instead by the *relative difference* between the baseline and with-project scenario flux estimates.			
83.	Not sure what is meant here by peer review? Is this just validation of the flux chamber studies? Again, note the	149-152	Peer review is meant to demonstrate validation of the studies deriving the proxy relationship(s).	Ok		

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	problem of not separating out R_h from R_a. as total soil respiration will not give an accurate number of true C storage differences.		See response to Reviewer 3 item #6 above.			
84.	I am not sure significance at P < 0.05 is needed as 0.1 would probably be more realistic given the variation in the measurements found in the field. Good goal but may not be realistic unless more replication is used.	155	Significance threshold for the proxy relationship revised from P < 0.05 to P < 0.10. (note though, unrelated to the proxy relationship, that for demonstrating significant differences in live biomass growth, a higher bar is needed to demonstrate a "real" difference from the counterfactual, because we are inferring a change in growth rate referencing a baseline reference site that cannot be expected to be a perfect match, even despite the similarity criteria in Table 4 being met.)	Ok		
85.	Clearly define all these terms (GHGwp etc.) in a table	186	All base parameters are clearly defined in tables in Section E. Calculated parameters are	Ok		

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	please. Otherwise it will lead to confusion and a problem in transferability		clearly defined in the equations. The extent of exposition of parameters is in keeping with other ACR methodologies.			
86.	Fire line: Monitored in project area via aerial imagery (Does this mean LIDAR imagery)? Accuracy a problem? Prior data needed for the area?	Table 2	The methodology intentionally does not specify what imagery need be used. It could be done using LIDAR, or it could be done visually interpreting burn scares from orthophotos or from Google Earth imagery. Most recent burned areas should be readily identifiable using aerial photography, and no prior data should be needed to date the burn. The same source is often used to stratify forest carbon inventories to an acceptable level of accuracy in terms of area.	Ok		
87.	This is the first time one mentions other states and it should be mentioned up	Table 4	Geographic constraints now set as part of the pocosin definition in Section A1. Note the geographic range of	OK		

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	front that these (histosol) peatland type areas exist throughout the SE. SC 115,000 (acres) NC 1,281,000 137,000 VA (Richardson, unpublished)		applicability now extends from VA to GA, justified by (and in fact more limited than) the reported range of pocosin habitat (Sharitz and Gibbons 1982 (Sharitz, R.R. and Gibbons, J.W., 1982. *Ecology of southeastern shrub bogs (pocosins) and Carolina bays: a community profile* (No. FWS/OBS-82/04). Savannah River Ecology Lab., Aiken, SC)).			
88.	Again 10 cm vs 15 cm needs to be verified and bulk density needs to be carefully measured in an undisturbed coring system	Table 4	Methodology procedures for BD now specify separating out litter (existing litter depth, variable) and peat/soil (to 10 cm below soil surface) and determining mass of each separately. See also response to Reviewer 2 item #57. Sampling to 10 cm, rather than 15 cm, is more conservative in accounting (see response to Reviewer 2 item #16).	Ok		
89.	" Note that both the project area and baseline reference	Table 4	See response to Reviewer 3 item #1.	See Comment 1	Term "baseline reference site"	

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	site must have been subject to drainage/hydrological alteration for at least 10 years per applicability condition for the stock change approach." Again, not sure why baseline reference is added here? We need at the very least to separate reference site, baseline site and project site, otherwise very confusing at least to me		To summarize, the methodology covers accounting on only one site: the project area. Two scenarios are accounted on the project area: actual and counterfactual (baseline scenario). The baseline scenario is estimated see comment 1***referencing*** measurements sampled from a baseline reference site demonstrated to be reasonably representative of the project area under a baseline (continued drained state) scenario.		changed to "baseline site"	
90.	Baseline reference site similarity criteria Again, not sure why reference is added here? I would remove this term from next to baseline??	219	See response to Reviewer 3 item #1 and others above.	See comment 1	Term "baseline reference site" changed to "baseline site"	

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
91.	ERTs (spell out what this means) the first time	29,30,256	"Emission Reduction Tons" (ACR term) now spelled out in full at first usage.	ok		
92.	Baseline (missing an e)	296	Couldn't find this misspelling.	ok		
93.	The 20-year approach may be difficult to determine for carbon credit. First, the stock change approach is calculated from net change of surface elevation, and preliminary C^{14} and SETs data suggest that without multiple sites and replicates measurements and it may not be easy to detect the difference in C stocks after 20 years.	296	The 20-year crediting period may be renewed per ACR procedures to extend monitoring and reporting. See response to Reviewer 2 item #59. For this approach, project proponents will need to consider closely the resolution of surface level measurement technologies, precision achieved (i.e. sampling intensity needed) and likely rates of subsidence, in developing their monitoring strategy.	ok		
94.	Below ground emissions and proxy variables need some	485	See response to Reviewer 3 item #6.	Ok, has been addressed as best one can,	Comment noted and continuing	

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	clarification in terms of what is being measured (R_h vs R_a versus total SR) and the proxies being used. Richardson et al. 2014, Wang et al (2015) show that water depth does not work well and soil moisture may be a better proxy for GHG emissions. Not sure what peer-reviewed proxy variables means?	511	As before, proxies are left unspecified intentionally, see response to Reviewer 3 item #2. Where proxy variables are mentioned, they are explicitly illustrative. Proxy variables are not peer-reviewed (only flux models, or models to predict the value of a proxy variable, e.g. a hydrologic model to predict soil moisture or water table).	measurements still difficult and some relationship need to be tested. Ok, soil moisture has been added and clarified	research underway at PLNWR.	
95.	What type of soil horizon marker (feldspar)? Why is it optional?	556	The soil horizon marker could really be any inert compound. Feldspar has been specifically mentioned as it is commonly used and readily available.	Ok		

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
			Litterfall/accretion is now a required pool/source.			
96.	What type of soil horizon marker (feldspar)? Why is it optional?	649	The soil horizon marker could really be any inert compound. Feldspar has been specifically mentioned as it is commonly used and readily available.	Ok		
97.	What type of aerial imagery (LIDAR)? Need before and after measurements?	802	See response to Reviewer 3 item #10	Ok		
98.	"The 10-cm depth must not contain the litter, thus soil must be sampled below the litter. It also might be better to sample 15 cm or at least test which depth is more representative before a full study is undertaken. Determination of the soil organic carbon fraction (or percent soil organic carbon) should follow	836	Methodology procedures for BD now specify separating out litter (existing litter depth, variable) and peat/soil (to 10 cm below soil surface) and determining mass of each separately. See also response to Reviewer 1 item #7. Sampling to 10 cm, rather than 15 cm, is more conservative in accounting (see response to Reviewer 2 item #16). Further we have added the citation below on laboratory	ok		

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	established laboratory procedures, such as those found in: Nelson, D.W., and L.E. Sommers. 1982. Total carbon, organic carbon, and organic matter. p. 539–580. In A.L. Page et al. (ed.) Methods of soil Analysis. Part 2. 2nd ed. Agron. Monogr. 9. ASA and SSSA, Madison, WI. Schumacher, B. A. Methods for the determination of total organic carbon (TOC) in soils and sediments. U.S. Environmental Protection Agency, Washington, DC, EPA/600/R-02/069 (NTIS PB2003-100822), 2002."		procedures for soil organic carbon fraction: "or DeLaune, R.D., K.R. Reddy, C.J. Richardson, and J.P. Megonigal, eds. 2013. Methods in Biogeochemistry of Wetlands. Soil Science Society of America Book Series No. 10. Madison, WI: Soil Science Society of America. 10004p"			

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	These methods book procedures are ok, but the more up-to-date methods and approaches strictly focusing on wetland soils (especially how to analyze peat soil) is DeLaune, R.D., K.R. Reddy, C.J. Richardson, and J.P. Megonigal, eds. 2013. Methods in Biogeochemistry of Wetlands. Soil Science Society of America Book Series No. 10. Madison, WI: Soil Science Society of America. 10004p					
99.	The section on proxy develop is weak and needs more clarifications on how to establish. Not sure how water table modeled from	837	The methodology intentionally does not specify how proxy relationships are developed. Rather, the methodology lays out the minimum requirements for proxy relationships to be	ok		

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	precipitation can be developed without a lot of work as water table response is quite variable after a rainfall event depending on antecedent soil water moisture conditions. Also soil moisture in some determined average depth may be a better long-term proxy.		acceptable to use for carbon accounting. As before, proxies are left unspecified intentionally, see response to Reviewer 3 item #2. Where proxy variables are mentioned, they are explicitly illustrative.			
100.	The number of peat depth samples needed should be determined by the size of the test area. I would think percentage of the area covered randomly is better way of determining this number is needed rather than just saying 20 sites.	838	If a certain level of precision were required, sampling intensity would be dependent on variability of peat depth, not area (directly). We recognize that peat depth is variable, however, because peat depth does not drive rates of emissions (only sets a long-term cap on emissions, and only relevant using the flux approach), we felt that the monitoring and measurement	Peat depth could drive GHG emissions if top layers are gone and only recalcitrant material left. Lower older peat does not respond the same.	Good point. For the flux approach, we now further specify that "The study site(s) from which proxy relationship developed *must include drained pocosins* (as defined in	ok

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	Or is this 20 sites per ha?		burden on this parameter could justifiably be relaxed by ignoring precision (but not bias), while still requiring a minimum sample size of 20 (at which point variability tends to stabilize). Bias is avoided by the requirement for representative sampling. See also response to Reviewer 2 item #51.		Section A1) *that have been subject to drainage/hydrological alteration for no less than 50% of the length of time that the project area has been subject to drainage/ hydrological alteration* prior to project start" to ensure inclusion of study sites with comparable or older (more conservative) base material. Note that for this requirement, and for the	

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
					corresponding requirement for the stock change approach (in Table 4), we have used a 50% threshold, rather than 20%, acknowledging that land management histories will be largely anecdotal and less precise in assessing.	
101.	What is the size of the area to be measured for AGB? Doing 1 ha block is often not feasible as a block so are multiple sites being measured at say 10x10 m^2? Often dbh and tree height are	842	The project area and baseline reference site are the areas sampled for AGB. The size of the sample plots is not specified to allow for flexibility when designing the inventory. Sampling requirements are detailed in Section E and require a minimum sample size of 20. The measurement	ok		

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	used together for a better biomass estimate. Also, carbon % can be easily measured for the dominant tree and shrubs and has already been done in most cases for Pinus serotina and shrubs.		parameters/independent variables, such as dbh or height, will be dependent on the allometric equation used and have intentionally not been specified. We employ a general value for %C of vegetation (0.47, from IPCC 2006GL) rather than using species specific values, consistent with other ACR methodologies. See also response to Reviewer 1 items #15 and #16.			
102.	"Allometric equations shall be peer reviewed, published in a scientific journal or government publication, relevant for the geographic area where the project occurs, and appropriate for the species/vegetation type found in the project area"		As in our response to Reviewer 3 item #7, peer review is meant to demonstrate validation of the studies, and follows precedent of other ACR methodologies (with no further specification). Peer review should unambiguously imply to an auditor that the study has undergone some process of independent review.	ok		

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	Not sure what peer reviewed means or adds in terms of validation?					
103.	"Because coarse (>2mm) rocky fragments occupy space in the soil profile in which carbon is not stored, the volume in the bulk density equation is the volume of the core. Discounting this volume, as in traditional bulk density calculations, would overestimate soil carbon stocks when applied to a volume that does not distinguish between coarse and fine fractions." There are very very few rocks in pocosin		We understand that coarse rocky fragments are rare in the landscape of interest, however retain these standard procedures as a precaution. We recognize that sampling peat bulk density is challenging and may require special equipment. We leave the specific equipment used up to the project proponent to best accommodate their site conditions and available resources. See also precautions added for bulk density measurements in response to Reviewer 2 item #57. We have also added a reference to the mentioned text to orient users to potential approaches/tools.	Ok ok		

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	peat samples so not sure what this means in these ecosystems? Just a precaution? Also measuring bulk density in peat soils is a major problem so special techniques and corers must be used to accurately determine BD in peat samples. Soil pits with careful extraction of peat cores by depth may work best. Also, nearly as good are 3 –sided box peat corers, which reduce compaction. Also, Russian peat corers are better than traditional soil augers in peat soils to reduce soil compaction.					

#	1st Round Reviewer Comment	Line or Section #	Author Response	2nd Round Reviewer Response	Author Response	3rd Round Reviewer Response
	(See: DeLaune, R.D., K.R. Reddy, C.J. Richardson, and J.P. Megonigal, eds. 2013. Methods in Biogeochemistry of Wetlands. Soil Science Society of America Book Series No. 10. Madison, WI: Soil Science Society of America. 10004p)					