A methodology for *Voluntary Emission Reductions in Rice Management Systems* was developed by Terra Global Capital, with support from the Environmental Defense Fund, Applied Geosolutions and the California Rice Commission, and submitted to ACR for approval through the public consultation and scientific peer review process.

Following ACR review and public comments, the methodology was submitted to three anonymous peer reviewers, experts in the field of rice production systems and GHG accounting. Four rounds of peer review comments and responses on the methodology are summarized elsewhere.

This document provides three additional rounds of review, of methodology revisions including the addition of early drainage as an eligible practice, new additionality language related to early adopters, clarified definitions, and more specific guidance on verification.

Comments on ACT3, Early Drainage

	1st review	Response	2nd review	Response	3rd review	Response
1	I am personally not in favor of using this as a mitigation strategy for the following reasons: 1. There have been no studies conducted to support the fact that CH_4 emissions are actually reduced when fields are drained early. While it may make some	We conducted a careful analysis of late-season emissions (i.e., within 14 days of harvesting) vs. main season emissions. Our analysis indicated that there was absolutely no difference in DNDC's ability to simulate emissions between these two phases. We conclude that DNDC can be used effectively to predict emission	Accepted.	n/a	n/a	n/a

	1st review	Response	2nd review	Response	3rd review	Response
	sense that it would (since the fields are flooded for a week less), exactly how much reduction has not been quantified. Furthermore, based on what I have seen of the DNDC model, the model would not do a good job at predicting the reduction in emission. The DNDC does a fair job at estimating total CH_4 emissions but is rather weak at predicting the actual pattern of GHG emissions. In this case, generally what we have found (and others also) is that after panicle initiation or flowering measured CH_4 emissions tend to decrease. The DNDC model does not generally show this well. So based on the DNDC output the modeled reduction may be far greater than actual.	reductions from early drainage. A graph indicating the late-season and main season bias is included in an appendix to this document.				
2	2. Unlike the other options which are definitive and relatively clear, it will be very difficult to establish a baseline. Decisions are based on soil type, what farmers think the weather will be like in the next couple weeks, and variety.	This was discussed at length during our meetings in Arkansas. In California, most growers harvest within a fairly small window. Therefore, the observed harvesting dates of the region can be used as a proxy of the conventional harvesting date on a specific field. In the Mid-south, most growers either use a crop advisor or the DD50 software program to get a recommendation on	There should be a more specific definition to "early draining". I would argue that in CA the harvest window is not narrow and this should not be used as a proxy. While growers often would like to harvest their grain at a given moisture content which is “ideal” the practicalities of farming, climate, etc. result in farmers harvesting a very	In the new version, we require farmers to indicate how they have determined their conventional drain date as part of the baseline data collection (See parameter “Conventional Drainage Date	OK, we are pretty sure that this will be a very difficult part of this methodology to monitor. Wide variation exists among growers on this. A farmer can easily say he/she is draining	For clarification, farmers will not be asked whether they drained earlier, instead they are asked how they would determine the drain date for

	1st review	Response	2nd review	Response	3rd review	Response
		the draining date. A majority of farmers drain the field after the draining date suggested by DD50. So, it is relevant to have an explicit reference to the DD50 program.	different moisture contents. For our purposes here this is different lengths of time between field drainage and harvest. In the south they generally use the DD50 which has a huge built-in fudge factor and is not founded on any hard research. While I like the growth staging approach which we have gone to here I doubt its utility in the farm setting. In the south and CA there are significant reductions in grain yield and quality if fields are drained too early. This can be confounded with soil type and variety used. Merle Anders (of University of Arkansas) suggests that soil management practices such as ripping a field for the previous soybean crop will increase soil moisture holding capacity and thus make early drainage more appropriate. In general Merle feels we should either use a specific plant growth stage to define this or not include it in the document.	determination", which is the last parameter in section 11). We have removed any reference to the DD50 model from the definition of early drainage. In the explanation of the "Conventional Drainage Date determination" parameter, a reference to the DD50 model is included, but only as an example of how a producer may set their drainage date. Other examples that are more relevant for California are also included now. In addition, a note was added that these are just examples and not endorsements or recommendations.	earlier than usual and there is no way to verify if this is indeed the case. We seem to be going round-and-round on this. It seems that you all want this in the methodology which is fine. As reviewers we have addressed where we see potential concerns/drawbacks.	conventional drainage in the GHG project plan, which is always developed *before the start of the project*, which should take care of the concern on subjectivity that the reviewer raised. We appreciate the reviewers' concerns and will certainly evaluate the operability of this approach after we were able to test it out in the field.
3	3. Fields drain at very different rates. For a field to start draining and become aerobic varies due to	Farmers are asked to specify the time it takes from pulling the boards or stopping pumping and before mud	I do not see in the document where this is specified. I may have missed. However, you need	The time it takes from pulling the boards or stopping	OK, see earlier comment	n/a

	1st review	Response	2nd review	Response	3rd review	Response
	the number of outlets a field has, the size of the field (area being drained), soil, and temperature. For example a large field with one outlet may take over a week to drain while a smaller field may take only a day or two. A grower with a large field and multiple outlets could easily comply with the protocol by draining a week early but only use one outlet. In the end the field would be flooded for just as long.	appears. The latter is the point that must be provided in DNDC as the drain date.	to be much more clear and quantitative. What does "before mud appears" mean? Following drainage "free" water on top of soil surface first disappears, leaving the soil saturated but still anaerobic. Over time soil water also disappears as the soil dries. Perhaps provide a photo of what you mean. A foot note on pg 60 reads "*In California, UC extension staff recommends early drainage as pulling the boards within 5 days of 50% head; conventional drainage is usually done by pulling the boards at the time of ¾ tip. The 50% head and ¾ tip are physiological growth stages that are challenging to use in a formal definition for an offset program.*" I am pretty sure this is not correct and early drain does not mean pulling boards 5 days before (or after) 50% head. This is very early and there is certainly more than 7 days between this and ¾ tip (methodology requires draining 7 days earlier). I called Cass Mutters regarding this and he says this statement is in error and would result in a large yield	pumping is specified under the parameter "Average flood-up and draining duration" in Section 11. We have rephrased the section that was quoted in this comment in a less ambiguous way as following: "average time it takes to drain a field by either pulling the boards or stopping pumping until all standing water has left the field. Note that at this stage, some water may remain in puddles, but no more water will be flowing into the ditch." The statement regarding pulling the boards 5 days after 50% tip was incorrect. This was replaced by "28 days after 50% heading" after consultation		

	1st review	Response	2nd review	Response	3rd review	Response
			reduction if boards were pulled this early.	with Cass Mutters. Please note that it is now clarified that this statement is just an example and not a recommendation (see next response). Thank you for bringing this to our attention. The reviewer is correct that the exact point at which a soil becomes aerobic again after draining is dependent on the soil type. However, the protocol focuses on emission reductions, which is the difference between the actual emissions and the emissions from a counterfactual baseline scenario. Since it is a difference between two emissions, the potential error introduced by the soil type and how fast a soil becomes aerobic		

	1st review	Response	2nd review	Response	3rd review	Response
				is cancelled out. Hence, the impact of soil type on emission reductions from early drain will be minimal.		
4	4. Early drainage can have large negative effects on grain quality. California prides itself in growing high quality grain. Early drainage can reduce milling quality and ultimately the price received for the grain. Most varieties very susceptible quality reduction due to early drainage. One variety (M206) has been shown to be able to maintain grain quality despite an earlier drain.	Other researchers and extension specialists have not seen any impacts on grain quality. Therefore, it is up to the farmer to engage in early drainage; it is definitely not an obliged project activity.	In California, where this methodology is being targeted, the only person I know that has done research in this area is Cass Mutters. His results suggest if you drain too early you do get reduced yields and quality. The trick is clearly defining as early a drain date as possible without affecting yield/quality. Again this gets back to soil type and variety.	It is now specified that the methodology does not endorse or favor a specific way to determine drainage date and that this should be left to extension staff or other experts. This statement was developed in agreement with Cass Mutters.	OK, see earlier comment	n/a

Comments on ACT1, Residue Baling and Removal

	1st review	Response	2nd review	Response
1	I had previously focused on the crop management - and apparently neglected the residue management aspects of this methodology. In fact, I really had problems to comprehend the whole concept when thinking more thoroughly about it. Table 1 gives false statements on the inclusion of	It is not apparent what the false statements are that the reviewer is referring to in the beginning of this comment. The reviewer is concerned that the level of detail used in the quantification for these secondary emissions is insufficient especially in comparison to the level of detail of the quantification of the primary emissions (i.e.	Accepted	n/a

	1st review	Response	2nd review	Response
	CO_2 emissions from straw burning and alternative uses of straw. Or am I missing here something? I have principle concerns in comparing the rather small emission savings from baling rice straw against a big source such as ruminants. This described procedure is based on just one factor (1%) to account for switching to low-digestible food. Only small changes in this factor will have huge impacts on the net savings computed. Computing emissions from straw used as heifer/cattle fodder will probably require more explanations than just a simple footnote. There is an enormous level of detail embedded in this methodology to compute emissions from rice. Even though I am not an expert on ruminant-borne CH_4 emissions, I can hardly believe that this can all be captured in just one factor. There must be a high level of uncertainty implied in the category "Unused and accumulated in piles near the farm" in Table 6. That seems to be a very broad term and the in turn, the value of 250kg CO_2eq. seems rather arbitrary I think that the methodology should be limited to the genuine ALTERNATIVE uses of straw, i.e. type of uses with AEROBIC decomposition. In these cases, we could fairly assume that there will be no CH_4 involved. Everything else will require a 'whole farm' approach which I think will stretch this methodology too far. Sorry for bringing this up rather late in the day, but I did not realize the problems involved in this	methane from rice). The factors used in this table are derived from lifecycle-analysis studies. The procedures to quantify were deliberately kept simple at the expense of being more conservative. The justification for this is that we believe that the level of effort in the quantification of emission reductions should be proportional to the magnitude of the emission reductions; the magnitude of the secondary emissions is very small in comparison to the primary emissions. Most of the emission factors used here are coming directly from recognized sources such as the IPCC and have been used in a wide range of applications. If no factors were available from the IPCC for a specific quantity, we used peer-reviewed literature. While the magnitude of the emission factors may seem small, keep in mind that they do not represent all of the emissions related to ruminants, but only the increase in emissions from replacing conventional ruminant feed with rice straw. The end uses of straw mentioned in this document were identified by a wide group of stakeholders. It is not clear to us why the reviewer is of the opinion that some of these uses of straw are not genuine alternatives.		

	1st review	Response	2nd review	Response
	aspect beforehand.			
2	In regards to residue management, it is not clear in the methodology how straw removal may affect soil carbon stocks and C sequestration. Is the assumption that soil C will remain the same regardless of straw management? I would guess that initially soil C would be reduced.	As far as we know, the only research on the impact of removing straw on SOC was done by Dr. Chris Van Kessel. The results of this study are not published in peer-reviewed literature. However, in personal communication, Dr. van Kessel indicated that the impact of straw removal on SOC were undetectable in his study, even after multiple seasons of straw removal. Therefore, we do not believe that the impacts will be significant. In addition, note that the SOC components of the DNDC model must be included in the quantification. The DNDC model predicts a short decline in SOC due to lowered inputs, even if this was not observed in the field. Therefore, we believe that our approach remains conservative.	OK, but realize this is based on scant and unpublished data. Two other items related to straw management. 1. Another issue that is related to this and I think needs clearing up in the methodology is what growers are supposed to do in terms of winter flooding if they choose to remove straw. The methodology says “removal of rice straw from the field after harvest and before winter flooding”. So rice fields will be flooded even if straw is removed. However, most growers flood the field to speed up decomposition of rice straw. If straw is removed then growers may choose not to winter flood. In an earlier version of this methodology reducing flooded winter acreage by 10% was one of the possible mitigation options. This was removed due to concerns related to amount of wildlife habitat. By removing rice	1. Baling has such a low adoption rate that we do not believe that any interactions of baling and winter flooding will be significant, especially given the year-to-year variations in winter flooding rates. However, we will monitor the adoption of baling and take corrective action if winter flooding rates are significantly negatively affected. 2. Because of the impact of straw management on GHG emissions during the subsequent growing season, the accounting period is always from harvest to harvest. So, the growing season following the fallow straw management is always included in the accounting.

	1st review	Response	2nd review	Response
			straw, the end result may be to decrease winter flood area. 2. Fallow straw management including removal and winter flooding during the winter have effects on GHG emissions during the winter but also have significant effects in the following growing season. This is also evident in the IPCC protocol where scaling factors are introduced depending on how soil was managed prior to the growing season. This methodology needs to be clear on how it will handle those effects.	

Comments on 2. Definitions and Acronyms

	1st review	Response	2nd review	Response
1	Most of the inconsistencies in the text are directly or indirectly related to unclear terminology. Especially the terms ‘activity’, ‘practice’ and ‘input parameter’ lack clear distinctions. I don’t want to be prescriptive, but I suggest the following definitions [see table below], that have to be applied consistently in the text and have to be added or replaced in the table with definitions on page 11.	The suggested definitions are much appreciated. After circulating the methodology among a wider group of stakeholders, we received similar comments to this one. Therefore, the definitions sections was greatly expanded and improved. Some of the definitions suggested by the peer reviewer were adopted. Defined terms are now capitalized throughout the methodology to make it clear when a term is being used in the specific sense defined in section 2.1.	Accepted	n/a

	1st review	Response	2nd review	Response
	However, I have to admit that it was not always clear what was meant by these terms, so please take these definitions as suggestions that may be changed in cases I got it wrong. In any case, however, the authors have to strive for a much higher level of precision in their definitions as in the previous version.			

Management parameter	A given measure within a crop management practice, e.g. different types of fertilizers, irrigation patterns etc.
Critical management parameters	A management parameter that is impacted by the project activities, either directly or indirectly.
Non-critical management parameters	A management parameter that is related to crop management but not impacted by project activities.
Project activity	Change in a management parameter that leads to a reduction in GHG emissions.
Project management practice	The entirety of management parameters (critical or non-critical) implemented as project scenario for calculating emission savings
Baseline management practice	The entirety of management parameters (critical or non-critical) implemented as baseline scenario for calculating emission savings
Model input parameter	A data item that is supplied as input to a process-based model, e.g. describing critical or non-critical management parameters.

Comments on 3. Summary Description of the Methodology/Revision

	1st review	Response	2nd review	Response
1	Page 8: The section on ‘partially fixed’ baselines is not	The reviewer is right that the project management is not fixed as non-critical parameters may change. We have	Accepted	n/a

	1st review	Response	2nd review	Response
	really thought-out which may also be due to unclear terminology in the first place. If the baseline management has to be adjusted – why not the project management? After reading this section numerous times, I sense – though I am not really sure – that the authors mean an adjustment of non-critical parameters (alongside with weather data). In contrast, critical parameters should not be altered. However, even if project activities remain unchanged, the project management practice has to be adjusted because it also incorporates non-critical parameters. Thus, some statements in the previous text are misleading.	tried to clarify the misleading statements with respect to not adjusting the project management.		
2	Page 9: This section would greatly benefit from illustrations in form of flow charts similar to those shown on page 43. In its present form the text is very difficult to comprehend because of long-winded sentences and sometimes convoluted logical flow. Once the illustrations are in place the entire section can be streamlined and be purged of redundant sentences (one glaring example: “Any practice for which the adoption is smaller than 50% cannot be considered common practice because less than half of the producers are implementing the practice”).	This comment is greatly appreciated. We drafted some flowcharts, but did not find them very instructive. Instead, we tried to add a lot more structure to the text by using graphical devices such as bullets, bold formatting, and enumeration lists. We have circulated this revised section among a wide group of people and received feedback that it was much more logical and digestible.	Accepted	n/a

Comments on 6. Procedure for Determining the Baseline Scenario and Demonstrating Additionality

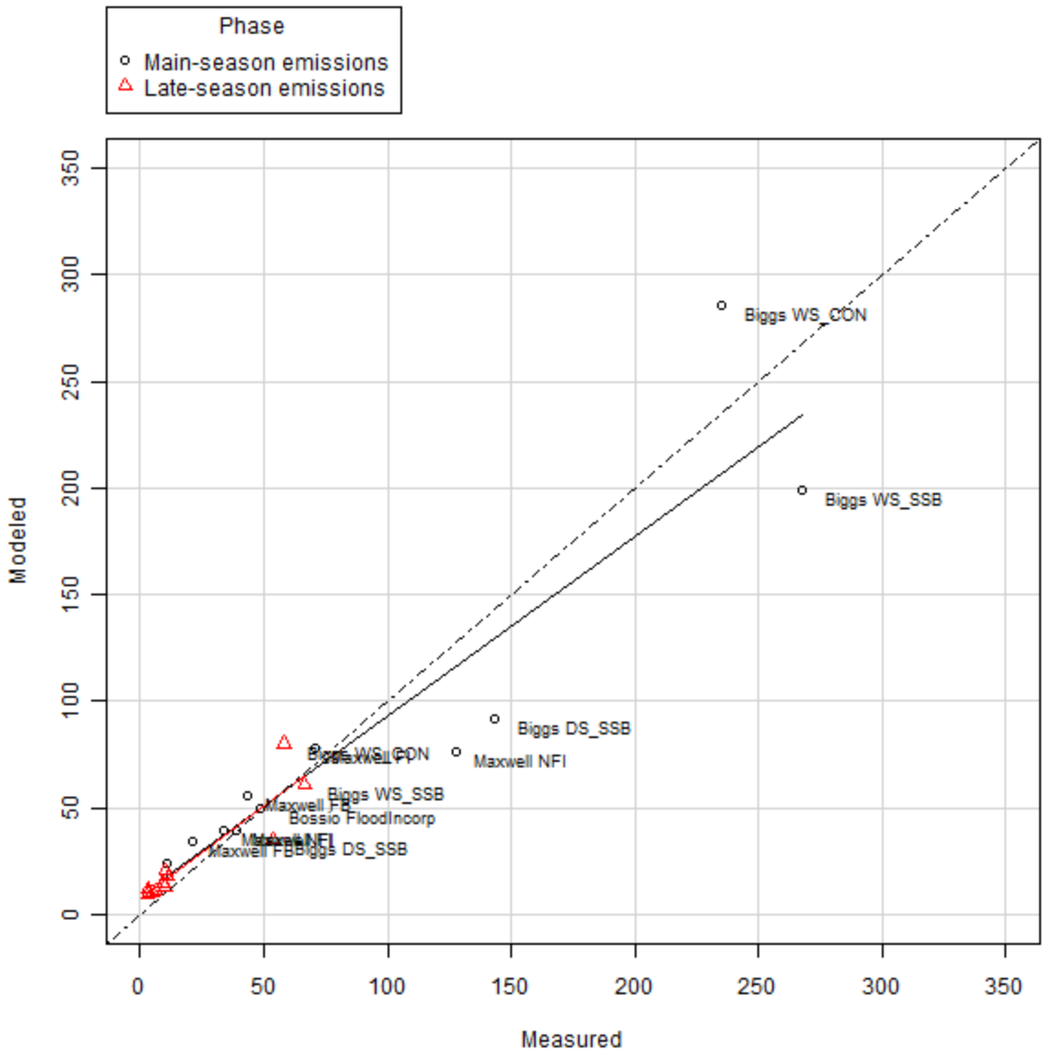
	1st review	Response	2nd review	Response
1	Page 16: I take the liberty of suggesting two new terms and respective definitions that should help to ease the rather unclear sections on applying regional and field-specific baseline scenarios: *Marginal management practice*: Project management practice applied on less than 5% in the reference region prior to project activities *Established management practice*: Project management practice applied on more than 5% of the reference region prior to project activities	We really appreciate the constructive thinking of the peer reviewer. After consulting with our stakeholders, we decided not to adopt this language as "marginal" has a pejorative connotation, and practices that are not widely adopted in the Rice Growing Region may still be seen as "established" on a given farm (if that farmer has executed these practices over many growing seasons). We tried to look for similar terms, but were not able to and decided to stick with the previous terminology.	Accepted	n/a
2	Page 17: I find the stipulations for expert opinions a bit odd. How do you define 'expert'? And what is the point of requiring 3 of them as a minimum? I wonder if there is really no other approach to that. Maybe you can specify an agency that should be consulted in the first step. Then, the survey may only become necessary in the second step if the agency cannot provide a clear answer. But I don't know the local circumstances well enough to come to a definite solution on this procedure.	It is really difficult and subjective to define an expert more explicitly. Even if one indicates "an expert needs x years of experience", one would have to define what the experience entails. Therefore, we feel that defining an expert more explicitly does not add value to the notion of using expert opinion. The onus of demonstrating that someone is an expert lies with the Project Proponent. We believe that in most circumstances, it will be clear and uncontested whether someone is an expert or not. The point of requiring three experts is to receive independent consensus in cases where no survey data is available, or where it is clear that a practice is not adopted widely. For example, three experts must agree the adoption rate of a particular practice is below 2% in order for a survey not to be required.	Accepted	n/a

	1st review	Response	2nd review	Response
3	In the section dealing with early adopters there is a large emphasis placed on the percentage of farmers or acreage using or adopting a certain practice. The numbers mentioned suggest a high degree of sensitivity. For example, there are large implications for numbers of 4, 5 or 6% adoption after 10 years (See pg 8-9 and example 1 in early adopter language"). These numbers are determined either from surveys or expert opinion. Surveys usually do not have this accuracy and neither does expert opinion. I would be considered an expert on rice in California but I cannot tell you accurately the amount of straw that is removed from rice fields, what percent is drill seeded, etc. I can make a guess but may be 5% off.	A survey must be compliant with ACR's guidelines on significance: a confidence interval of 10% with 90% significance. As for the use of expert opinion, please note the methodology states that only when expert opinion from 3 independent experts indicates that the adoption rate is smaller than 2%, can the adoption rate be assumed to be smaller than 5%. In other words, there is a safety buffer built in for the use of expert opinion; when 3 experts cannot agree the adoption rate is less than 2%, a survey must be conducted.	Accepted	n/a
4	Another example of this is sort of problem is that often the number of 5% of rice is drill seeded in California is thrown around as an educated guess. I am sure this is in the ballpark, but for the purposes of this methodology it seems it would be important that it is not practiced on 6% of the acreage. It would be very difficult to determine the exact number as data are not kept on this and the number changes from year to year. The same goes for all of the mitigation practices. These are not practices that are statistically monitored.	We agree that there is some level of uncertainty with the estimates. However, one needs to operationalize the idea of a "small adoption rate" with a set threshold. Therefore, whatever the threshold is, one will always have the case in which the adoption rate just does not make the threshold level. In our opinion, the 5% threshold is small enough so that in case the true adoption rate is 7% or 8%, adoption can still be considered marginal and far from common practices. Please note that the methodology contains procedures to account for changes in adoption rates from year to year.	Accepted	n/a
5	Section 6-page 16. Very difficult to follow this. Perhaps include some sort of diagram or flow chart.	We agree that this section was not straightforward to follow. This section was completely rewritten and subsequently reviewed by multiple stakeholders.	Accepted	n/a

Appendix: graph of late-season vs. main-season modeled vs. measured emissions

This appendix is presented in support of the authors response to comment #1 under Comments on ACT3, Early Drainage. We provide a graph of modeled vs. measured fluxes for both main-season emissions (black points and regression line) and late-season emissions (red points and regression line). Late season is defined as three weeks before harvest. Values are in kg C ha^{-1} yr^{-1}. Points above 25 kg C ha^{-1} yr^{-1} are labeled with the Site and Treatment Code. A description of each Treatment Code is summarized below.

Observation nr.	Site	Treatment Description	Year	Treatment Code
1	Biggs	Drill seeded in a stale seedbed	1	DS_SSB
2	Biggs	Water seeded in a conventional seedbed	1	WS_CON
3	Biggs	Water seeded in a stale seedbed	1	WS_SSB
4	Bossio	Burned residue and winter flooded	1	FloodBurned
5	Bossio	Incorporated residue and winter flooded	1	FloodIncorp
6	Maxwell	Burned residue and winter flooded	1	FB
7	Maxwell	Burned residue and winter flooded	2	FB
8	Maxwell	Residue incorporated and winter flooded	1	FI
9	Maxwell	Residue incorporated and winter flooded	2	FI
10	Maxwell	Residue incorporated and not winter flooded	1	NFI
11	Maxwell	Residue incorporated and not winter flooded	2	NFI



One can observe that there is no difference in the regression line of modeled vs. measured values for either main-season or late-season emissions. Obviously, the late-season emissions, which are accumulated over only 3 weeks, are much smaller in magnitude than the main season emissions, which are accumulated over several months. Therefore, it is concluded that there is no evidence that the DNDC model is biased later in the growing season when fluxes are summed over 2-3 weeks. Note that this does not necessarily mean that daily fluxes are unbiased.