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Interactive comment on "Impacts of rice varieties and management on yield-scaled greenhouse gas emissions from rice fields in China: a meta-analysis" *by* H. Zheng et al.

Anonymous Referee #1

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The manuscript used meta-analysis to investigate the effects of rice variety and management on yield-scaled greenhouse gas emissions from rice fields. It is an interesting topic and within the scope of BG. The manuscript contributes to our understanding of how management practices influence GHG emissions from rice fields and the development of strategies to mitigate climate change. However, the manuscript is not written with fluent language and it is not well organized, such that the presentation is not clear and sometimes confusing. A substantial revision of this manuscript will greatly improve the quality of this manuscript.

In the method section (2.2.5), the authors stated that 95% CI was used to indicate statistical difference among treatment effects, but at the same time, in the result section

(e.g., Fig. 1) p values were also provided without any indication of calculations of these values and significant levels. Furthermore, the authors frequently over-stated or miss-used "statistical difference" of the treatment effects. For instance, in Fig. 3, suggesting by the overlaps of the 95% CIs, there were no N effects on CH4, N2O, area-scaled GWP, and yield-scaled GWP, and a marginal effect on yield at high application rates. However, the authors considered N effects significant overall. The discussion was plain and most of time simply repeated the results or results from other studies. For example, as stated by the authors, the study was to "achieve a trade-off between increasing rice yield and reducing GHG emissions. . ." (Lines 401-404), but yet there is no in-deep discussion on how the results of the meta analysis would contribute to this goal. Instead, the authors focused on what management would reduce yield-scale emissions, largely ignored the balance between GHG emission and rice yields and no rice yield was provided and discussed.

The authors stated in lines 105-107 that the objective of this study was to "provide references for appropriate cultivar selection based on yield-scaled GHG emissions — to achieve higher yields with lower GHG emissions". The objective was miss-leading and not the focus of this study at least for the following observations, 1) as mentioned above, the authors largely ignored the rice yields in the section of results and discussions; 2) it is not clear whether the studies on rice varieties used in meta-analysis were actually side-by-side (at least not indicated in method section). The varieties effects on yield and GHG emission can be easily confounded by soil properties, temperature, and other environmental factors, if the studies were not conducted side-by-side; 3) management can affect the grain yield, GHG emission, and yield-scale GHG emission. Recommendations on cultivar selection should consider the interactive effects of management, rice yield, and GHG emissions.

Wetland rice fields are generally considered as sinks for CO2. The rates of carbon sequestration can be affected by rice varieties and managements, which further influence net GWP. However, the authors did not provide or discuss any of the information

with this respect and simply provided recommendation on yield-scaled CH4 and N2O production, which is not convincing and could be wrong.

Other comments,

Line 33-35, need to revise, it is kind of COPY AND PASTE from Linquist (2012); Lines 55-58, 70-74, 77-87, 270-272, it is not easy to understand the points or logics that the authors are trying to state;

Lines 62-64, it is overstated; it is not even supported by your own results that there is no significant difference in total GHG emission between Indica and Japonica;

Lines 105-107, the meta analysis on just two varieties in not enough to provide "reference and recommendations ...", especially the experiments of rice varieties were not side-by-side;

Line 112, why CO2 emissions are not included? Need justification, even though rice field is generally considered as a sink of CO2.

Lines 124-127, the statement is confusing.

Lines 164-169, need more descriptions, e.g. whether the GDAT is sub-section of the growing season? If so, how the rice productions use to calculate the yield-scaled emissions of GDAT? And what is the relationship between rice varieties with GDAT? Do different varieties have different GDAT?

Lines 173-176, what is the number of the side-by-side studies? Is the number statistically big enough for your calculation and conclusion?

Line 184, "exp", typo?

Lines 200-201, you have set the significance level as 95% CIs, why later on (result section) use p-values? It is confusing. Need more descriptions, otherwise the result and subsequently discussion is confusing and not easy to understand;

Lines 251-257, repeat of the results;

Lines 322-327, repeat of the results;

Lines 351-355, this temperature effect is not related to your discussion of variety and management;

Lines258-362, you state that seedling stage is important, but your analysis was based on studies without seedling stage, does this mean your conclusion and analysis is less important or invalid?

Line 363, based on your determination of significance as 95% CIs, Fig 3 indicates that the difference among N treatments were not always significant.

Line 380, does your suggestion of replacing high yield scale-GWP varieties with low ones also involve in changing cropping systems (described on lines 382-384)? How about rice yields?

Interactive comment on Biogeosciences Discuss., 10, 19045, 2013.