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Interactive comment on "Carbon budget assessment of an irrigated wheat and maize rotation cropland with high groundwater table in the North China Plain" *by* Quan Zhang et al.

Anonymous Referee #2

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A. General comments : This paper present a detailed carbon budget of a rotation wheat-maize rotation cropland with high groundwater table in North China. The idea of study a two cropping rotation by calculating all carbon budget components and the effect of high groundwater table on the carbon budget and its component is interesting and worthy of enquiry. They report that the NBP for the rotation is a weak carbon sink but the authors did not took into account the uncertainties associated to carbon budget terms. Also, methane emissions are not considered and could switch the plot from carbon sink to carbon source. Moreover, because the scientific questions are not explicitly announced, the authors didn't really comments the effect of groundwater tables on carbon budget, what is expected through the title of the paper. According to

the title, the reader expect a detailed analysis of the effect of groundwater levels on carbon budget and its components. Actually, some points need to be clarified in the material and method section as the flux treatment methodology but also some results (i.e. as about positives values of daily NEE on Fig. 5 ; see my comments below for the rest). Globally, a detailed description of flux treatment is missing (filtering, percentage of gapfilled data, . . .) but also the authors should use the equation described by Reichstein et al., 2005 to partition the flux instead of the one used in this paper. I think that a footprint analysis is missing particularly because two methods are compared and associated to calculate NBP. Also, biomass sampling is too small and non-representative of the plot. That leads to impossibility to compare the two methods (eddy covariance and sampling). Finally, ER terms are not spatially representatives of the whole plot (see below for details) and the too small sampling for biomass drove to some misinterpretations in the results and the following discussion & conclusion concerning ER and its components (RAB, RAA). For all these reasons, the study is fundamentally flawed in several different ways that make it unsuitable for publication.

B. Specific comments 1. Abstract (minors issues) âđć L. 20 : modify the sentence as following ". . . from October 2010 to October 2011. During this cultural year, 2 crops were ground in the experimental period. For the winter wheat and summer-maize cycle, . . .". This is more explicit, according to me. âđć Net biome productivity is calculated for a long-term period (C budget on several years) as the NEP (minimum one year, not a season). The integration periods are too short in this paper to talk about NBP and NEP when calculated for a season. âđć L. 28 : what represents the term Âń Net ecosystem carbon release Âż ? âđć L. 28 : There is only one rotation period, not two, but two crops in one cropping year. âđć L. 29 : How was calculated the NBP term of the rotation cycle (12.8 gC m-2.yr-1) as NBP of wheat and maize were respectively 58.8 and 3.9 gC m-2.yr-1 ? âđć L 31 : phrase pas supportée par les résultats de cet article. Idem dernière phrase : conclusion n'est pas supportée par les résultats de l'étude : pas de high CUE compare à d'autres cropland et pas forcément du à des effets ferti et irrigation. You cannot do CUE forte plus nfluencée par le fait qu'il y ait 2 cultures sur

une année plus que par les ferti et irr.

2. Introduction (minors issues) : 1) L. 50 : C budget components also includes C exported at harvest and C imported at sowing and through organic fertilisation (Ceschia et al., 2010). 2) Note : Use preferentially bibliographic reference in relation with cropland instead of forest (i.e. Ekblad et al., 2005) 3) L 65 : add Ceschia et al., 2010 in bibliography list (agro-ecosystem). 4) Freibauer et al., 2004 is missing in bibliography list. 5) L. 81 add Béziat et al., 2009 6) L. 84 add Verma et al., 2015 (suyker) 7) L 90 : add Ceschia et al., 2010 in bibliography 8) L. 92/93 : "More importantly, there remains no consensus on whether agro-ecosystem is a carbon sink or source" : Depending on management practices (i.e. choice of crops in the rotation, leaving crop residues, including cover crop in the rotation, irrigation practices, ...), but also climatic year, agro-ecosystem will be C source or sink. There is no single answer for all agro-ecosystem. 9) L. 102 : is it correct to consider 5 m as a very high groundwater ?

3. Materials and methods (majors issues) : 1) L. 149 : The annual C budget have to be calculated on a full year, i.e from October 23rd 2010 to October 23rd 2011 (for 365 days). 2) L. 178 : 4 points/10 plant samples in wheat season and 3 plant samples in the maize season : globally, samples are too smalls. 3) LAI values are close to PAI values, is it really LAI? Or PAI (plant area index, meaning stem+leaf) ? or GLAI (green leaf area index) ? 4) Moreover, to compare the 2 methods to calculate NPP, it is necessary to collect the plant samples in the footprint or up from prevailing winds. Those informations are crucial and missing in this section. 5) L. 201 : The equation (1) is not the equation used by Reichstein et al., 2005 (neither Lei and Yang, 2010) as mentioned at L. 197. The equation used in your paper is not the best method, you should use the equation described by Reichstein et al., 2005. (Reichstein et al., 2005) Using this equation allow to better estimate the Rref that is temporally varying in an ecosystem. Rref has to be estimated for consecutive 4-day periods by nonlinear regression using the Lloyd & Taylor (1994) model, fixing all parameters except Rref. By this way, for each point in time, an estimate of Reco can be provided with time-dependent param-

eters and variables indicated by the symbol t in parentheses contrarily to the equation used in your paper (Reichstein et al., 2005). 6) It is essential that an uncertainty analysis is performed to quantify the uncertainty on the annual NEE (and maybe GPP and ER) values as it was done in Richardson & Hollinger (2007) for instance. Moreover, I would include a footprint analysis and an uncertainty analysis associated to footprint. An analysis using other cropping years would allow a direct comparison of the measurements and modelled data could give more credit to the analysis (see below). L. 206 "... the eddy covariance system failed from October 23rd 2010 to April 1st 2011": a comparison could be realised to estimate the uncertainty of the model compared to measured fluxes by eddy covariance and validate the use of the SVR model during this period (6 months) in your site. During the same period of the cropping year (before wheat sowing), the exercise aims at modelling fluxes with SVR model from October to April during bare soil period (for a cropping years with measured data) to compare them to measured fluxes. For example, the exercise is possible by modelling fluxes from October to April 2005-2006 by using 2009-2010 for model training. Then, those results would be compared with measured fluxes from October to April 2005-2006. By this way, you will be able to compare measured fluxes with modelled fluxes and estimate the uncertainties of the model to predict fluxes at this site and during this period. L. 515, "we have a total of 466 GPP data samples and 483 ER samples" : is that a total of half-hourly or daily data ? With the term "data samples" we understand half-hourly data that is not sufficient. 7) L. 216 : "Rs" term is not introduced – done in L. 242. 8) L. 219 : 3 replicated pairs of comparative treatments are not enough to reduce the uncertainty associated with spatial variability. 9) L. 244 : where were realised portable soil respiration system measurements ? Those measurements are they contained in the footprint? This is necessary to be comparable to eddy covariance measurements. If no footprint analysis were realised, specify the direction of prevailing wind. Moreover, provide information concerning the threshold for considering that the fluxes are representative of the plot and what is the percentage of the data filtered by the footprint analysis. 10) L. 258 : "NEP (also the inverse of NEE with eddy covariance)" :

this definition is false. Net Ecosystem Production (NEP) is annual NEE cumulated. In other word, NEE data are integrated over one year (365 days) to obtain annual NEP (Ceschia et al., 2010). 11) L. 266 : Replace “grain carbon storage” by “the amount of C exported at harvest”. Globally, a detailed description of flux treatment is missing (filtering, percentage of gapfilled data, . . .) but also the authors should use the equation described by Reichstein et al., 2005 to partition the flux. I think that a footprint analyses is missing particularly because two methods are compared and associated to calculate NBP. Also, biomass sampling is too small and non-representative of the plot. That leads to impossibility to compare the two methods (eddy covariance and sampling).

4. Results/Discussion/Conclusion (majors issues) : 1) L. 184 : Âń The crop organs are separated Âż : How the authors seperated the roots ? L. 294/295 : 3.0 % and 2.3 % root for wheat and maize, respectively. Those values seem underestimated. Did the authors compared thoses results ? 2) L. 272 : Replace “a yearly total of 7, 072.18 mol m-2” by a mean value. 3) L. 276 : Replace “The groundwater table fluctuation well follows irrigation event during winter and spring seasons, while follows precipitation during summer and autumn seasons” by “The groundwater table fluctuations follows well irrigation events during winter and spring seasons, while they follows precipitations during summer and autumn seasons”. 4) Figure 5 : Positives values of daily NEE are not caused by low temperatures as lower temperatures during those periods were never below 10°c. It might be a filtering problem. Many tests allow to control flux quality data as described in by Foken et al., 2004; Foken & Wichura, 1996; Göckede et al., 2004. Explain or detail the reason of positive daily value of NEE during the full crop development when temperatures are never under 10°C. The whole methodology of flux treatment has to be revised. âđć L. 302 : “In this section, GPP is presented in negative values. . . from the atmosphere” but in L. 328, the GPP is positive : “the seasonal total NEE, GPP and ER are -437.9, 1078.2 and 640.4 gC m-2 for wheat, and -238.8, 779.7 and 540.8 gC m-2 for maize”. GPP + ER = NEE âĞŠ -1078.2 + 640.4 = -437.9 âđć L. 308 : replace “cropping rotation” by “fallow rotation” and simplify the sentence (L. 308-311) “The fallow periods are the main carbon source periods, especially as well as

the period at the start of maize season when the crop is tiny and the high temperature greatly favors respiration Âż. âđć L. 312 & 319/321 : “ inclement weather suppressed crop metabolism rate” There is no visible and drastic climatic variation in Fig. 3. It could be a filtering problem. Realise a thorough assessment of the data. âđć L. 322/324 : “When water logging occurred in August and September, both the soil heterotrophic respiration and below-ground autotrophic respiration were suppressed to zero”. In this case, ER = 100% meaning that RAB is huge : approx. 5 gC m2/day in at the end of August and beginning of September while before RAB represented only 1 gC m2/day. This example reflects the lack of spatial representativity of the measures between the two methods : âđć Either there is a measurement problem for RH and RS when water logging occurred âđć Or RH and RS are highly overestimated most of the time (by this way, those results would be in line with the results reported by Wang et al., 2015 and Moureaux et al., 2008 but also more consistent with aboveground/belowground biomass measurements cited before. This example also show that the footprint analysis is missing in this study because the measurements of ER and all its component has to be done in the footprint zone to be comparable to eddy covariance measurements. âđć If you consider that negative values indicate carbon removal from the atmosphere, the NPP should also be negative. In L. 330, NPP values are positives. âđć L. 333 : the C exported at harvest should be a positive term to indicate C back to the atmosphere as in L. 302 they said that the “GPP is a negative term to indicate C removal from the atmosphere”. âđć Whatever the definition of the sign convention chosen, ensure that this convention is respected in the whole document. âđć Moreover, describe explicitly the sink/source effect of each component of the C budget as did Whang et al. (2015); cited in the paper (i.e. L. 65). âđć Also add a table with chosen sign convention for each term as did in the Table 2 in Whang et al. (2015). âđć L. 340 : How is calculated the “net carbon loss” ? âđć L. 340-342 : There is only one rotation period, not two, but two crops in one cropping year. âđć L. 350 : detail “management intensities” : straws removal or not, use of organic manure or not,. . . âđć L 355 to 357 : Further develop âĞŠ other differences : âđć Straw management, organic manure? âđć Strong differ-

ences for NEP terms between those several years associated to contrasted climatic years ? âđć Differences in exportations (at harvest) terms ? âđć L. 359 to 361 : I recommend further analysis of theses differences. âđć L. 361 to 365 : “The water logging event is occasionally reported in upland croplands, Terazawa et al. (1992) and Iwasaki et al. (2010) found water logging cause damage to plants, potentially explaining GPP decline in Dold et al. (2017) and also our study. While our study further implies that water logging diminishes ecosystem respiration even more, therefore reduces overall cropland carbon loss. ” âĞŠ so it affects certainly the yield and the carbon budget ! Not discussed. âđć L. 368 to 372 : “Comparing with another study at Luancheng site reporting North China Plain as a carbon source (Wang et al., 2015), we found their estimates of GPP (1051 gC m-2) and ER (692 gC m-2) in wheat season are pretty close to our results (GPP of 1078.2 gC m-2, and ER of 640.4 gC m 2), such resemblance probably attributes to irrigations that prohibit both wheats from experiencing water stress.” âĞŠ potentially many other causes : way too much focus on the effect of irrigation. âđć L. 372 to 373 : “However, maize of two studies exhibit considerable different carbon fluxes. ” âĞŠ compare the components of the C budget with other studies for cropland (i.e. Moureaux et al., 2008). âđć L. 387 : “. . .featuring the major difference between these two sites ” âĞŠ what are the others terms of management? âđć L. 405 : replace “corn” by “grain” âđć L. 412 : “NPP/GPP” not reliable in this study, root biomass estimates are probably very underestimated + too few samples. âđć L. 432 : add “be” before “subject to environnemental. . .” âđć L. 437 : “ecosystem respiration is dominated by below-ground and above autotrophic respirations” âĞŠ this difference with the literature reveals a methodological problem in this study (see before for details). âđć L. 449 : Âń 10% Âż for the root ratio compared to only 2.3 and 3% in this study. âđć L. 460 : “because of the sufficient samples” âĞŠ too low according to me. âđć L. 469 : not enough samples for estimating NPP ! âđć L. 473 : Calculate uncertainties for each component of NBP. Also NBP = 12.8 gC m-2 yr-1 is not significant because of uncertainties associated to the term. âđć L. 475 : replace “rotation period” by “fallow period”. âđć L. 479 to 484 : “NEP are 405.5 and 269.1 gC m-2 for wheat and

maize. This cropland has high carbon use efficiency (i.e., the NPP/GPP is 73 % and 72 % for wheat and maize, respectively), which indicates this wheat-maize rotation cropland maintains a relatively higher proportion of assimilated carbon via photosynthesis. The high RH/ER (i.e., 59 % and 54 % for wheat and maize, respectively) implies that soil heterotrophic respiration dominates ecosystem respiration in this cropland. ” âĞŠI have some doubts concerning those conclusions given the small number of soil respiration measurements. âđć In that state, it is difficult to evaluate the results of this study as signs conventions are not explicitly described. Moreover, the sign convention described in L.302 is opposite to the rest of the document. Whatever the sign convention adopted, please respected it in the whole document. Indeed, it is hard to understand which terms are C sources or C sinks and conclude about the results.

C. Technical corrections (non exhaustive) âĂć Replace the “;” by “.” in lines : 294, 378 âĂć Add dots in units, i.e. replace “m m-3” by “m.m-3” (L. 274, L. 286) . . . âĂć Fig. 3c : irrigation events are difficult to distinguish on the figure. âĂć Figure 4 : add standard deviations. âĂć Figure 6 : explain abbreviations HW. And PM.

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