We very much appreciate the reviewer's critical yet constructive comments, allowing us to sharpen our focus and improve our manuscript. Please see the below for our reply.

The paper proposes mainly a method to fill gaps and partition the water fluxes in evaporation and transpiration in particular in case of wet canopies. In addition it presents a modification in a data filtering method (ustar) that is used for CO2. New methods to partition water fluxes are for sure very interesting and important. For this reason the title is misleading since gapfilling and partitioning of CO2 are not discussed (or only marginally, without real new developments). Improvements are needed in the validation, explanation and uncertainty discussion. I suggest to reorganize it and focus mainly on the more interesting and innovative part (water), adding more sites in order to demonstrate that the approach is valid.

Response: We partially agree with the reviewer's comments. More validation, explanation, and uncertainty of the proposed method (especially water flux gap-filling and partitioning part) are required to improve the manuscript. However, we do not agree to remove the CO_2 flux part. Because we argue that (1) nighttime flux data filtering is a part of gap-filling and partitioning of CO_2 flux, (2) there is still a lack of effort to link carbon and water fluxes even though they are strongly connected by stomata, and we typically measure the both simultaneously. The gap-filling (and partitioning) of flux data is a kind of interpolation (and extrapolation) of available observed fluxes using the relationship between the fluxes and its drivers. For identifying the relationship correctly, selecting data (i.e., filtering data which cannot represent the phenomena/signals we want/expect) is the most important first step. We will add a paragraph for explaining (briefly) the nature of gap-filling and partitioning of eddy covariance (EC) flux data in the introduction section. Also, we argue that the last message of this paper is also important: There is a common characteristic between the proposed methods, i.e., two existing methods are merged into a new method. Such a strategy strengthens the strength and makes up for the weakness of the original methods. It also results in better applicability. It will contribute to the standardization of eddy covariance data processing.

SPECIFIC COMMENTS:

Pages 2-3:The distinction between the ustar filtering and advection methods is not very clear and not fully correct. The ustar filtering has been proposed exactly to filter out data when there is advection so it in not true that it can not be applied when the “drainage flow is at night ”. In addition what the authors call “Advection method” is in fact a partitioning method and not a filtering method (e.g. it assumes no advection daytime)

Response: We agree with the reviewer’s comments. We think our original explanations make readers confused as reviewer’s comments. To avoid such confusion, we will add a paragraph for explaining (briefly) the general procedure of CO_2 EC flux partitioning (i.e., selecting data to identify the relationship between nighttime CO_2 flux and its driver, identifying the relationship, extrapolating the relationship to daytime (or nighttime)) in the introduction section. We will also define the related terminologies in the paragraph.

Par 2.3.2: there are a lot of parameters in the proposed model but it is not clear how they are estimated and also which is the associated uncertainty. Also it should be verified if the parameters are valid in different conditions (to test the “everywhere, all of the time” proposed by the authors P3L18). At the moment this is far to be demonstrated.

Response: We fully agree with the reviewer’s comments. We will revise the manuscript as follows.

(1) Add more detailed explanations how we can obtain the parameters from our field measurement (mainly from the flux tower) and introduce alternative ways (e.g., using MODIS product)

(2) Add a section for sensitivity analysis of the proposed method similarly to that from Shi et al. (2010), and identify the parameters which significantly affect the gap-filling and partitioning results.

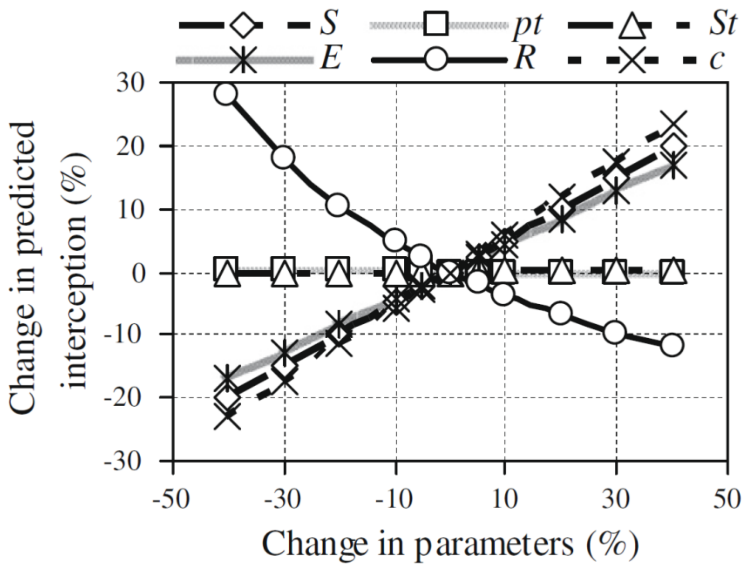

Fig. 1. Sensitivity analysis of the revised analytical model: influence of parameters *S* (canopy storage capacity), *c* (canopy cover), p_t (proportion of rain diverted to stem flow) and S_t (trunk storage capacity), and of climate variables *E* (mean evaporation rate during rainfall) and *R* (mean rainfall rate) (copied from Shi et al., 2010).

(3) Add a section for the (sensitive) parameters optimization. We should maximize the validity of (a small number of) the observed H_2O flux data under wet canopy condition. In the original manuscript, we used all available data under wet canopy condition to validate the method. In the revised manuscript, we will divide the available dataset into the datasets for parameter optimization and validation (i.e., validation after optimization). The ratio of the optimization-validation datasets may be 7:3. Such strategy can improve the applicability of the method (i.e., generalization for '*everywhere, all of the time*').

Shi, Z., Wang, Y., Xu, L., Xiong, W., Yu, P., Gao, J. & Zhang, L. (2010) Fraction of incident rainfall within the canopy of a pure stand of Pinus armandii with revised Gash model in the Liupan Mountains of China. *Journal of Hydrology*, *385*, 44-50.

I find the section 2.3.3 not clear in the second part where the application of the model is reported. More information and a clear description of the procedure are needed.

Response: We agree with the reviewer's comments. In the original manuscript, the detailed explanations of the model were omitted to avoid self-plagiarism (authors' previous study, Kang et al., 2012). We will revise the section 2.3.3 (add the detailed information).

Kang, M., Kwon, H., Cheon, J. H., & Kim, J. (2012). On estimating wet canopy evaporation from deciduous and coniferous forests in the Asian monsoon climate. *Journal of Hydrometeorology*, *13*(3), 950-965.

Section 2.4.1 reports the NEE processing but the level of details is not sufficient to understand what is done (how are the parameter estimated? How is the ustar threshold calculated?). In addition three methods are presented as three independent approached but it is not explained if, when the light response curve method is applied, the data below the ustar threshold are removed (in this case they are not independent, if not removed that it is an error because it is known that the data are not correct). That said, I also find this part not relevant for the paper that is more focussed on H2O.

Response: We mostly agree with the reviewer's comments (except the last argument). We will rewrite the section 2.4.1 including the following contents.

We adapted 0.3 m s^{-1} of *u** threshold from Kang et al. (2014 and 2017). We had checked the dependency of nighttime CO_2 flux on friction velocity during the growing and dormant seasons (Fig. 3). 0.3 m s^{-1} of *u** threshold can be determined during the growing season, while it is hard to clearly decide a threshold during the dormant season for the both sites. But, the threshold during the dormant season would be smaller than 0.3 m s^{-1}. Therefore, we applied the constant threshold of 0.3 m s^{-1} for the sites.

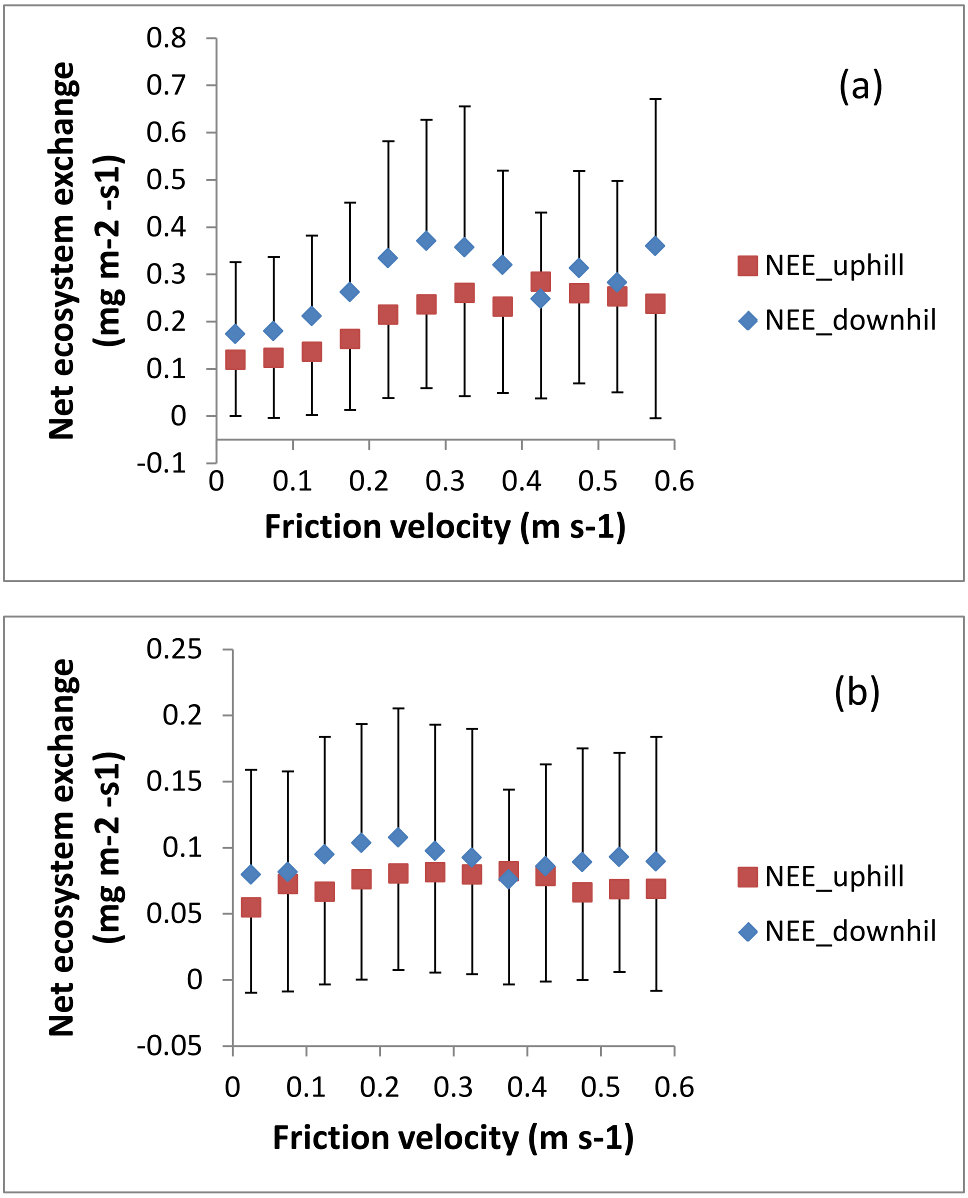

Fig. 3. The dependency of respiration of ecosystem (i.e., nighttime net ecosystem exchange) on friction velocity for the uphill (GDK) and downhill (GCK) sites during the growing (a, DOY 121-300 for the uphill and DOY 91-300 for the downhill) and dormant (b, non-growing season) seasons. (adapted from Kang et al., 2014 and 2017).

Each method is independent each other. In cases of the light response curve method and the advection-based method, the nighttime CO_2 fluxes were filtered out when the observed nighttime CO_2 fluxes were underestimated out of the 95% confidence interval of the ecosystem respiration model (i.e., Lloyd and Taylor equation).

P7L28-30: authors should explain why the MPT method is "inappropriate for hilly terrain sites" while the ustar filtering (and so also the MPT) has been developed specifically to filter out the advection.

Response: We annexed a proviso: inappropriate for hilly terrain sites "that are usually affected by drainage flow." The advection-based method was developed for such sites (van Gorsel et al., 2007, 2008, and 2009). Figure 4 (copied from van Gorsel et al., 2007) (and Figure 5 which is adapted from Kang et al., 2017, our previous study for the same study sites) shows why it is hard to apply the u^* filtering method for such sites. During the nighttime except near sunset, the CO_2 fluxes were close to 0 and much smaller compared with the data from the other independent observation (i.e., chamber), due to drainage flows. It suggests that we should consider not only u^* but also time (when the drainage flow is fully developed) for nighttime CO_2 flux filtering. We will add such explanation in the introduction and method sections.

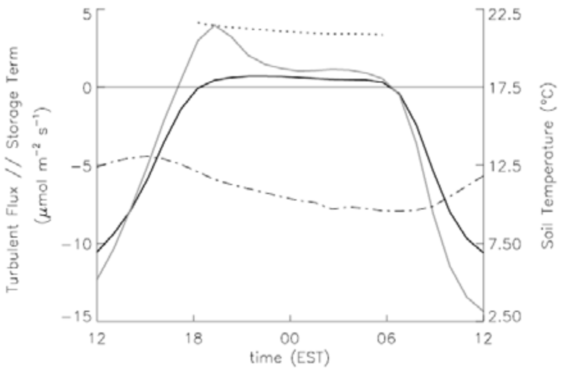

Fig. 4. Mean daily course [2001–2005] of the turbulent flux of CO_2 (black line), the sum of eddy flux and change in storage term (gray line) and the soil temperature at 0.02 m (dash dotted line). The dotted line represents total nighttime respiration derived from chamber measurements (copied from van Gorsel et al., 2007).

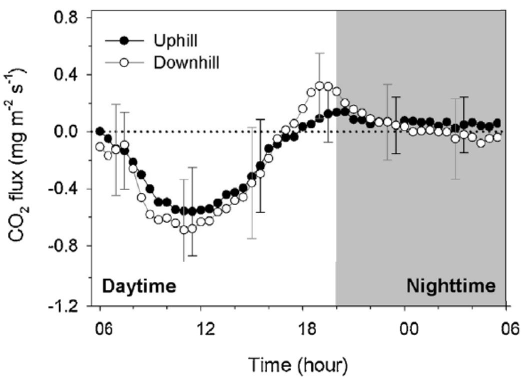

Fig. 5. Mean diurnal variations of the observed CO_2 fluxes and storage terms for the uphill (GDK)

and downhill (GCK) sites during the growing season (i.e., June–September). The error bars indicate the standard deviation for each half hour. The shaded areas represent nighttime, and the unshaded areas represent daytime.

van Gorsel, E., Leuning, R., Cleugh, H. A., Keith, H., & Suni, T. (2007). Nocturnal carbon efflux: Reconciliation of eddy covariance and chamber measurements using an alternative to the u*-threshold filtering technique. *Tellus B*, *59*(3), 397-403.

van Gorsel, E., Leuning, R., Cleugh, H. A., Keith, H., Kirschbaum, M. U., & Suni, T. (2008). Application of an alternative method to derive reliable estimates of nighttime respiration from eddy covariance measurements in moderately complex topography. *Agricultural and Forest Meteorology*, *148*(6), 1174-1180.

van Gorsel, E., Delpierre, N., Leuning, R., Black, A., Munger, J. W., Wofsy, S., ... & Chen, B. (2009). Estimating nocturnal ecosystem respiration from the vertical turbulent flux and change in storage of CO_2. *Agricultural and Forest Meteorology*, *149*(11), 1919-1930.

Kang, M., Ruddell, B. L., Cho, C., Chun, J., & Kim, J. (2017). Identifying CO_2 advection on a hill slope using information flow. *Agricultural and Forest Meteorology*, *232*, 265-278.

P7L31: the fact that near sunset the drainage has not yet completely developed must be proved.

Response: It had been proven using bulk Richardson number, CO_2 concentration profile, CO_2 flux measured by chamber method (van Gorsel et al., 2007, 2008) and information flow between the uphill and downhill in the previous studies (Kang et al., 2017). We will add the details in the manuscript.

van Gorsel, E., Leuning, R., Cleugh, H. A., Keith, H., & Suni, T. (2007). Nocturnal carbon efflux: Reconciliation of eddy covariance and chamber measurements using an alternative to the u*-threshold filtering technique. *Tellus B*, *59*(3), 397-403.

van Gorsel, E., Leuning, R., Cleugh, H. A., Keith, H., Kirschbaum, M. U., & Suni, T. (2008). Application of an alternative method to derive reliable estimates of nighttime respiration from eddy covariance measurements in moderately complex topography. *Agricultural and Forest*

Meteorology, *148*(6), 1174-1180.

Kang, M., Ruddell, B. L., Cho, C., Chun, J., & Kim, J. (2017). Identifying CO_2 advection on a hill slope using information flow. *Agricultural and Forest Meteorology*, *232*, 265-278.

P8L1-3: how can the authors be sure that the average nighttime flux in the two windows is not different for biological reasons? Or because of the uncertainty in the storage term? Or because a different wind direction distribution (i.e. footprint) is present in the two time windows?

Response: Wind directions (mountain wind) were same. And, we did not find any driver which makes a difference between the averages nighttime flux in the two time windows except ecosystem temperature. So, we compared the averages after normalizing the flux measurements using the temperature response function (i.e., Lloyd and Taylor equation) as same as the original MPT method proposed by Gu et al. (2005). We will add this argument in the manuscript.

Gu, L., Falge, E. M., Boden, T., Baldocchi, D. D., Black, T. A., Saleska, S. R., ... & Xu, L. (2005). Objective threshold determination for nighttime eddy flux filtering. *Agricultural and Forest Meteorology*, *128*(3), 179-197.

P8L4-5: it is not clear what is proposed. If the two windows are different then all the data in the second window are removed? And ustar filtering is applied to what?

Response: Yes, all the data in the second time window are removed. The u^* filtering is applied to the first time window. We will revise the sentence.

P8L18-21: is the model validation made using a leave-one-out method or using independent dataset?

Response: Originally, the model was fully independent. Following the reviewer's comment and reminding the nature of gap-filling and partitioning of EC flux data, we should maximize the validity of (a small number of) the observed H_2O flux data under wet canopy condition. In the original manuscript, we used all available data under wet canopy condition to validate the method.

In the revised manuscript, we will divide the available dataset into the datasets for parameter optimization and validation (i.e., validation after optimization). The ratio of the optimization-validation datasets may be 7:3.

Section 3.1.2: the comparison of the methods is no a validation and no strong conclusions can be derived from this. In addition, in the Figure 4 uncertainty is not considered to understand how much (if) the two approaches are significantly different. The only way to prove this is to add other sites, for example where close path IRGAs with heated tube are used (so that ET measurements are ok also with rain) and then apply the methods to artificial gaps.

Response: The difference between the two methods and new information provided by the newly proposed method motivate readers to apply the new method. If another actual measurement (like the reviewer mentioned, i.e., close path IRGAs with heated tube are used) can be obtained easily, such gap-filling and partitioning would not be a scientific issue. Even though the number of observed data under wet canopy condition is small (in this study), we can validate and optimize the model. For quantifying the uncertainty of the model, we apply Monte Carlo simulations for sampling the optimization and validation dataset similarly to Richardson and Hollinger, 2007. We will add this discussion in the manuscript.

Richardson, A. D., & Hollinger, D. Y. (2007). A method to estimate the additional uncertainty in gap-filled NEE resulting from long gaps in the CO 2 flux record. *Agricultural and Forest Meteorology*, *147*(3), 199-208.

Section 3.2.1: in Figure 5 the comparison of measurements before and after sunset is used to show the presence of drainage. However it is not clear if the data shown in the plot are filtered by ustar (they should otherwise the plot is biased by a known issue).

Response: The figure shows the results after applying the u^* filtering. It also showed that determining the u^* threshold is difficult when drainage is fully developed due to ~0 of CO_2 fluxes. We will consider adding the data before applying the u^* filtering in the figure.

The Table to add is the 2 and not the 1.

Response: We will correct as suggested.

Figure 6 doesn't show the original method and how much it is different respect to the modified. In addition it is not a validation because however compared with other models/methods.

Response: We will consider adding the results from the original MPT method in the figure. The other method was developed in our previous research, Kang et al. (2017). Currently (without other independent measurements such as chamber method), it was the best results which have been published in the peer-reviewed journal.

Kang, M., Ruddell, B. L., Cho, C., Chun, J., & Kim, J. (2017). Identifying CO_2 advection on a hill slope using information flow. *Agricultural and Forest Meteorology*, *232*, 265-278.

Figure 7: how was heterotrophic respiration measured?

Response: Heterotrophic respiration was not considered. We will mention the limitation of this comparison, or estimate (and consider) the heterotrophic respiration based on the literature review.

Section 4: I find this section, although with some nice and interesting aspects, out of scope respect to the rest of the paper.

Response: This section showed readers why we should do ET partitioning.

Appendix A, lines 22-23: the main reason of the minor effect of the storage method in ET is that the fluxes are always very low at night, when the storage component is important.

Response: We will revise as suggested.