Interactive comment on "Carbon dioxide and methane fluxes from different surface types in a created urban wetland" by Xuefei Li *et al.*

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The reviewer's comments are shown in blue and our responses in black.

"While there is a growing number of CO_2 and CH_4 studies from natural ecosystems, relatively few studies come from urban wetlands. Hence, this paper caught my attention as being a potentially important, new and novel contribution.
What does the term urban wetlands mean and why may greenhouse gas exchange to and from it differ from other wetlands? To my mind, I would expect urban wetlands to be recycling water from urban uses and be subject to runoff from urban landscapes, which may have elevated levels of N applications, herbicides, oil runoff from roads etc. So. these factors may affect the redox ladder and alter methane fluxes compared to those from more remote wetlands. Let's see what the authors find." I suspect the definition of an urban wetland is overly broad and more specification may be needed. In this case the authors are studying a constructed, stormwater wetland. I suspect there are many other types of urban wetlands, just look at the urban LTER in Baltimore, MD and Phoenix, AZ as a comparison. So building a database on how they may differ or be similar should be a long term goal, initated by a project like this. It would be nice to frame this urban wetland in Finland in context to those in wetter/drier and warmer worlds."

We thank the reviewer for the effort spent on our manuscript and the appreciation of the importance of our study. We agree with the reviewer that the definition of an urban wetland is very broad. We will rephrase it in the text as follows: "In this paper we present measurements carried out at a created urban wetland in Southern Finland in the boreal climate." (Line 22-23)

"A limitation of this study is the time scale..
'The measurements were commenced the fourth year after construction and lasted for one full year and two subsequent growing seasons'. This study is missing many of the important pulses after construction to truly under- stand the dynamics of this system. This aspect is one of the greatest weaknesses of this work. But given so little data on this topic, I decided it is not a fatal flaw, in this instance. But I would not view future studies of this type that miss the dynamic of the restoration pulse viable."

We are aware of the time scale of this study limited our capability to draw conclusions about the climate impact of the management (rewetting) when constructing an urban wetland. However, our study focused on the climate impact of the urban wetland after its establishment.

"The authors report:
The annual NEE of the studied wetland was 8.0 g C-CO_2 m-2 yr-1 with the 95% confidence interval between -18.9 and 34.9 g C-CO_2m-2 yr-1 and FCH_4 was 3.9 g C-CH_4 m-2 yr-1 with the 95% confidence interval between 3.75 and 4.07 g CH_4 m-2 yr-1.
I must admit I am surprised how tiny the fluxes are, given it is a wetland, even if in Finland. I would expect a stronger sink, but granted this would be conditional of what is in the flux footprint. So careful correspondence between fluxes and footprints are key to interpret these data.
As I read on I take home the key point that it is a weak sink for only 2 months and a slow C source rest of the year. Guess in hindsight it all makes sense. As I read the introduction, I am finding necessary conditional information. For example, open water is just not always open water. With N

inputs there can be other life forms. Here the authors note
'At open-water surfaces, the net production of CO_2 is a result of photosynthesis by algae, cyanobacteria as well as submerged aquatic plants, respiration of organic carbon and oxidation of CH_4 produced in the water.'
This conditions meets with some of our experiences where we see azola and other aquatic plants in the open water sections. It has changed my perspective and open to this observation. The authors will need to be careful as they evaluate their 'open' water data and inform the reader if it is or not truly open water."

In fact, we did not observe lots of metaphytic or filamentous algae during the study period. There was not large number of free floating small plants neither. There were some submerged aquatic plants which did not affect the openness of the water.

"Glad to see citation to the work of the Estonian team of Mander et al, as they are among the few teams looking at this problem. I would also double check literature by Bill Mitsch. Their wetlands in Ohio may qualify as an urban wetland as it was close to the University in Columbus OH. Recent reports of methane fluxes come from Gil Bohrer's group, Morin et al and others.
Glad to see the authors are clued in about the key role of flux footprints. As we bend the rules of eddy covariance and ask contemporary questions and problems, we will need footprint models to partition the heterogeneity of the landscape."

The two papers from Morin et al. were cited in the manuscript (Line 76).

"Materials
The wetland is over 500 ha. This is a good size field for this work. Standard and well vetted eddy covariance is used by experts in the field who know how to carefully interpret the data. Closed path CO_2, Licor, and TDL is used to measure methane fluctuations. Given the cold, wet environment I think closed path is best for this work. The authors have looked at cospectra to ensure filtering is limited or appropriated corrected for. Good micromet protocol.
Standard neural networks are used to gap fill. The methods are described in great detail and proper attention to nodes, validation data, etc are made.
Overall I am confident about these measurements as this team has a long history of well vetted studies. The paper needs an assessment, map of the heterogeneous fetch and the flux footprint. I did not see this in the material. It is in the supplement, but it may be better placed in the paper. Starting to lose track of what is a paper vs supplement."

The map of study site overlapped with climatological footprints over the study period will be moved to the main body of the manuscript.

"This paper is novel with water ch4 sensors to apply the diffusion model. First time I have seen these sensors. Bravo/brava/bravum.
The authors try to partition fluxes by the veg water fraction. I realize this is a legitmate quest and one with good intentions. We have tried this approach in the past and failed. We used multiple towers to close the system of equations with water/veg fractions. But my student, Jacyln Hatala Matthes found that the fractal dimension of the patches was key. So be careful in your partitioning.
Matthes, Jaclyn Hatala, et al. "Parsing the variability in CH_4 flux at a spatially het- erogeneous wetland: Integrating multiple eddy covariance towers with high resolution flux footprint analysis." Journal of Geophysical Research: Biogeosciences,119.7 (2014): 1322-1339.
Use of the Kljun model is good. It has evolved as one of the better and most widely used.
With this the authors calculate the veg water fractions. But I must confess I don't have confidence in these numbers, especially from one tower. The reason we tried to use two towers was to get

different fractions of water and vegetation with two equations and two unknowns.
I'd like to have the authors discuss the uncertainty more and critique the pros and cons of their method.
The reporting of flux reports is straight forward and standard. I have no critique or suggestions for this part."

We will critique our method as follows: "The uncertainty of the vegetation and water fraction come from two sources. Firstly, the delineation of the distinct surface types was conducted based on a land surface map of the growing season in 2013, which neglect the change in the spatial extent of the vegetation throughout time. Secondly, although Kljun model (Kljun, Calanca, Rotach, & Schmid, 2015) is proved to be robust and general, there are uncertainties in the model prediction. To be more confident in the footprint estimation, it would be good to compare our results with large eddy simulation, however it is out of the scope of the current study. With only one EC tower we could not cross check the results as done in another study (Matthes, Sturtevant, Verfaillie, Knox, & Baldocchi, 2014).

However, we chose to follow a simple approach dividing the landscape into vegetation and open water because we did not observe significant vegetation expansion during the growing season and the area of open water is relatively constant. Furthermore, the clear effect of the footprint-weighted fraction of open water on the synchronization between EC CH_4 measurements and diffusive CH_4 flux from water (Line 471-477, Fig.S6 in the supplement material) was nicely presented in our analysis, so that we think the simple method used is sufficient to capture the major pattern in vegetation and water fraction in our study. "

"What interests me is information on controls and processes. Here the paper has an advantage with measurements of the fluxes from the water section. But we have to be careful here. If the water is open then simple models will work. But with urban systems the N inputs can green up the water and the presence of green material will cause the diffusion models to be invalid. I need to hear more about this. So first confirm if the open water is open or is it clogged."

The open water is open (see the corresponding responses above). Submerged aquatic plants should not affect the validation of the diffusion model. Furthermore, the estimated diffusive fluxes of methane (CH_4) and carbon dioxide (CO_2) were well situated in the range of the diffusive gas fluxes over open lakes from other studies (Erkkila et al., 2018; Mammarella et al., 2015), which supported our assumption that the water was not covered by floating plants.

"The controls need a bit more information on N load of the water. What is the nitrate or phosphorus levels. If there is runoff P and NO3- may affect the CH4 fluxes."

A figure showing the nutrient levels in the water will be added to the manuscript (Fig. 1) as well as the following text: "NO_3-N was measured with Scan sensors (Scan gmbh, Austria) and total phosphorus (TP) was calculated based on turbidity data which was measured at 10-min intervals (Valkama et al., 2017). The median TP concentration measured at the outflow monitoring station was 56 μg L^{-1} and the median NO_3-N concentration was 0.69 mg L^{-1}. In annual perspective TP and NO_3-N concentration consisted of several runoff peaks occurring after rain or snow melting events. This wetland serves as a nutrient removal measure as it improved water quality by retaining P and N from runoff before the release to the receiving lake, where the annual TP reduction was 13% and NO_3-N reduction was 14% in year 2014 (Valkama et al., 2017)."

"The control and process section is very simple and using correlations. I does not go into great enough detail and I am not sure if it makes a dent in our ignorance. I like methods using information theory at different time scales, I continue to worry about the roles of photosynthetic inputs to prime archaea. We learn that at different time scales temperature control may be dominant and photosynthesis may at others. Water table is important, but if it does not vary much it will not be a notable factor, yet we know mechanistically it is and if water table dropped below ground level one would see the effect."

We conducted wavelet coherence analysis to reveal the processes and environmental controls of the gases at different time scales. The magnitude of the wavelet coherence and the phase differences between ecosystem CO_2 and CH_4 fluxes and environmental variables are shown in Figs. 2 and 3. Here we show the results of net ecosystem productivity (NEP; NEP= -NEE) instead of NEE for a better interpretation of the phase arrow (higher positive value in NEP means higher CO_2 uptake). We found strong positive correlations between NEP and temperature, radiation at 1-day scale due to the diel temperature and radiation cycles. On average, T_{air} and T_{water} are leading NEP by ∼3h and ∼8h, respectively, while radiation is almost in-phase with NEP. The variation of TP is leading the change in NEP at 1-day scale (more TP leads to more CO_2 uptake) where the time lag varies between 1 to 5 hours (Fig.4 (d)).
CH_4 flux has correlation with temperature at 1-day scale where T_{air} and T_{water} are leading CH_4 flux by ∼1h and ∼6h, respectively (Fig. 5). CH_4 flux has also correlation with temperatures at 16-32-day scale (Fig. 3). Radiation is in-phase with CH_4 flux at 1-day scale (Fig. 4(c)). TP has positive correlation with CH_4 flux (more TP leads to more CH_4 emission) at 1-day scale and TP is leading CH_4 flux by ∼2h. Surprisingly, water level did not show any consistent correlation with CH_4 flux at any time scale which may be due to the small variation in water level during the growing season.
GPP and CH_4 flux are correlated at multiple time scales (Fig. 6). At 1-day scale they are nearly in-phase, which indicates rapid link between photosynthesis and CH_4 emission (Fig. 7(a)). At 90-day scale, GPP is leading CH_4 flux by 17-20 days which can possibly be explained by the lags between environmental controls of GPP and that of CH_4 on a seasonal scale (Fig. 7(b)).
After all, it is worth noting that the correlations between the fluxes and environmental variables revealed by wavelet coherence analysis can be overstated, as much of the flux data has been gap-filled using these variables. Therefore, in the revised manuscript we will only add figures which show the results between fluxes (CO_2 and CH_4) and those independent environmental variables (NO_3-N and TP).

"Glad to see the authors using sustained warming potential method of Neubauer and Megonigal. I just reviewed another wetland restoration paper and they Did NOT use this method and it was a criticism of mine Methane emissions are not a single pulse, like used with the old method. It is key to use a sustained emission method."

To be consistent with other references using IPCC value as reviewer # 2 suggested, we will add also the results using the conventional global warming potential in the manuscript.

Discussion
"The authors do a nice job putting this work in context and reviewing the literature. I don't want to micromanage as there are many ways to go. I do like the discussion on O2 consumption. This is a nice angle and looks at mechanisms. I do like seeing a bit of advice on how best to design these systems. What are the pros and cons of different water/veg fractions and what can one do to minimize methane emissions or what are the effects of nutrient inputs on the greening of open water spaces."

We will add the following paragraphs in the text on the advices of designing urban wetland ecosystem.

"Firstly, in our study we found that the radiative forcing effect of the open-water area exceeded the

vegetation area in an urban wetland in Finland. Thus, if considering only the climate impact, it would be advisable to have lower water/vegetation fraction which means limiting open-water surfaces and setting a design preference for areas of emergent vegetation in the establishment of urban wetlands.
Secondly, our results showed that total phosphorus enhanced both CO_2 uptake and CH_4 emission which have contradictory climate impacts to the ecosystem. Although it is out of the scope of our study, it would be very interesting to understand the mechanisms, to quantify the magnitude and the duration of these enhancements induced by nutrient input. Previous studies have found that nutrient inputs can influence the identity of the key primary producer (submerged plants versus phytoplankton) in the water, which is crucial in shaping the CH_4 emission from shallow water (West, Creamer, & Jones, 2016; Davidson et al., 2018). Submerged plants may decrease CH_4 production in the lake by producing alleochemicals, transporting oxygen to the sediment and providing good habitat for CH_4 oxidizing bacteria (Heilman & Carlton, 2001), while phytoplankton was shown to significantly increase CH_4 ebullition by changing the quality of the dissolved organic carbon which promotes methanogenesis (West et al., 2016) or/and by altering the sediment texture and redox conditions favoring the release of bubbles. As a result, we suggest to control the nutrient input to the water of the newly established wetland to limit the abundance of phytoplankton as well as to support the existence of submerged plants."

"In closing this paper has some novel aspects and I think it will merit publication. I do think it has some lingering issues that need to be resolved. Most seriously fraction of the water and vegetation and the modeling of fluxes from the water portion if the water is not pure. The other limitation is the time scale. It misses critical dynamic of the pulse and recovery after the wetland has been developed. This is a hole that cannot be filled."

We thank the reviewer for his constructive suggestions. We are aware of the limitation in our study and they will be more clearly acknowledged in the revised manuscript. Future studies are ought to be planned in a manner which can "fill the hole".

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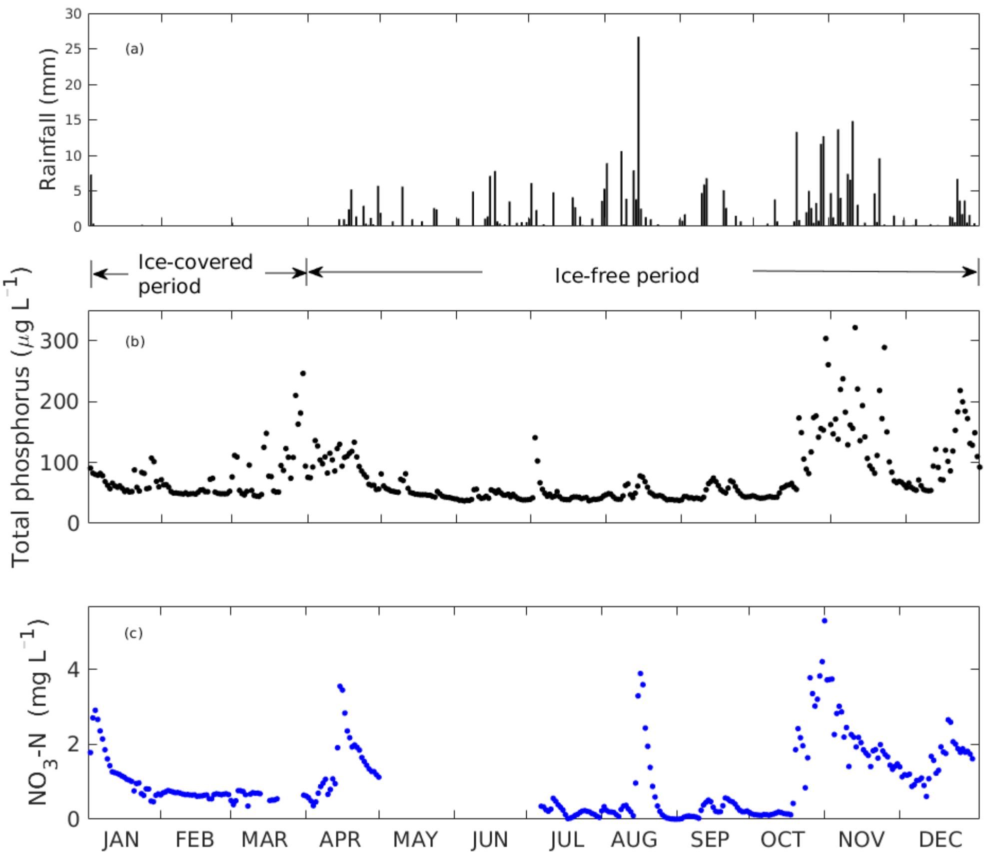

Figure 1: The daily average of (a) rainfall, (b) total phosphorus concentration and (c) NO_3-N concentration measured at the outlet monitoring station in year 2013. The lake was covered by ice from January to March and it was free of ice after the end of March.

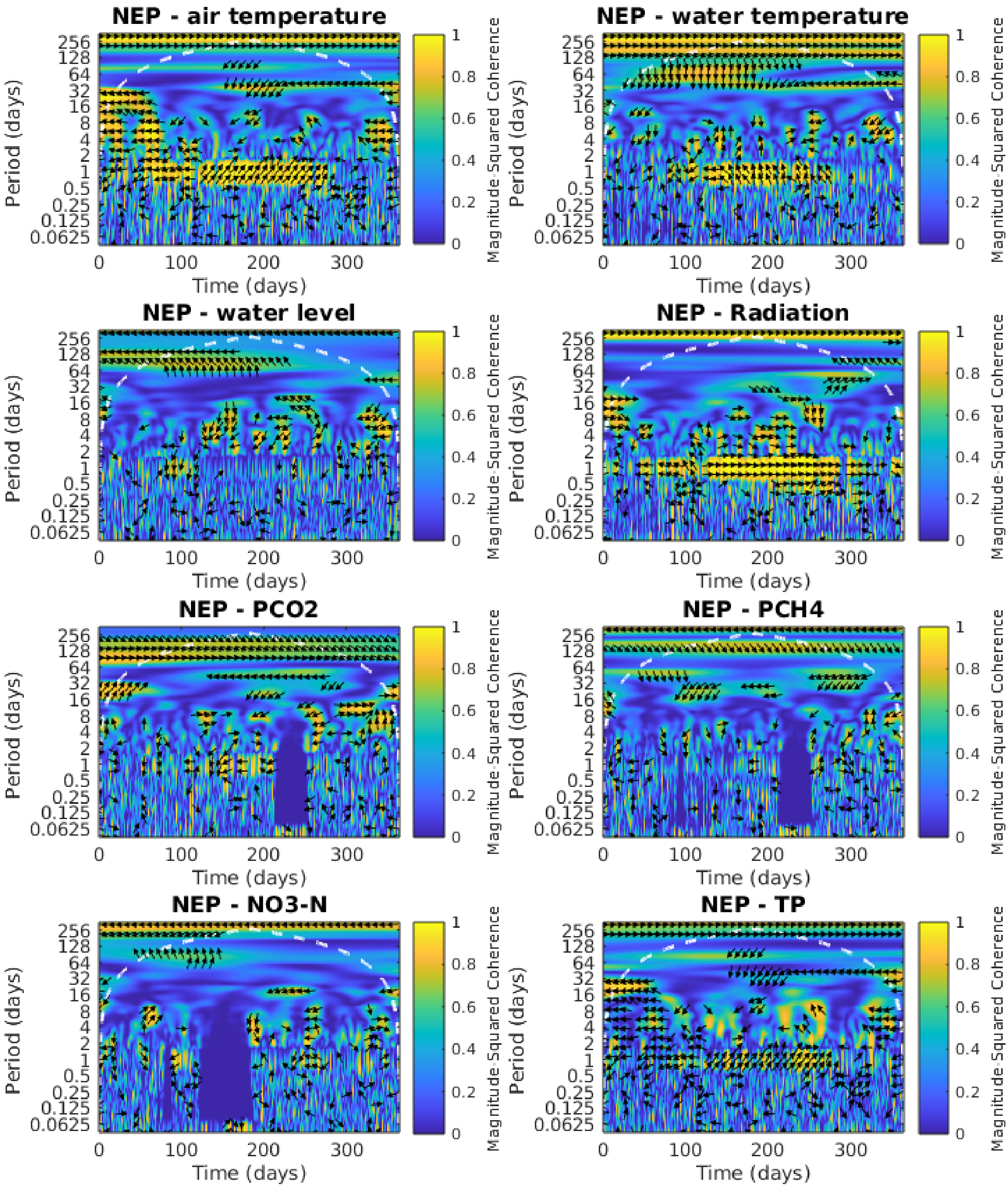

Figure 2: Wavelet coherence analysis and the phase difference between net ecosystem production (NEP; NEP=-NEE) and environmental controls from January to December 2013. The color represents the power of the coherence from 0 to 1. The phase difference is indicated by black arrows which only show up where the coherence is greater than or equal to 0.5. → indicates in-phase (two time series in synchrony) and arrows in other direction indicate out of phase (representing lags between time series), i.e. ← indicates anti-phase, ↓ indicates the 1^{st} series (NEP) leads by quarter-cycle and ↑ indicates 2^{nd} series (environmental controls) leads by quarter-cycle. White dash contour lines indicate the cone of influence. PCO2, PCH4, NO3-N and TP indicate the concentrations of CO_2, CH_4, NO_3-N and total phosphorus in the water.

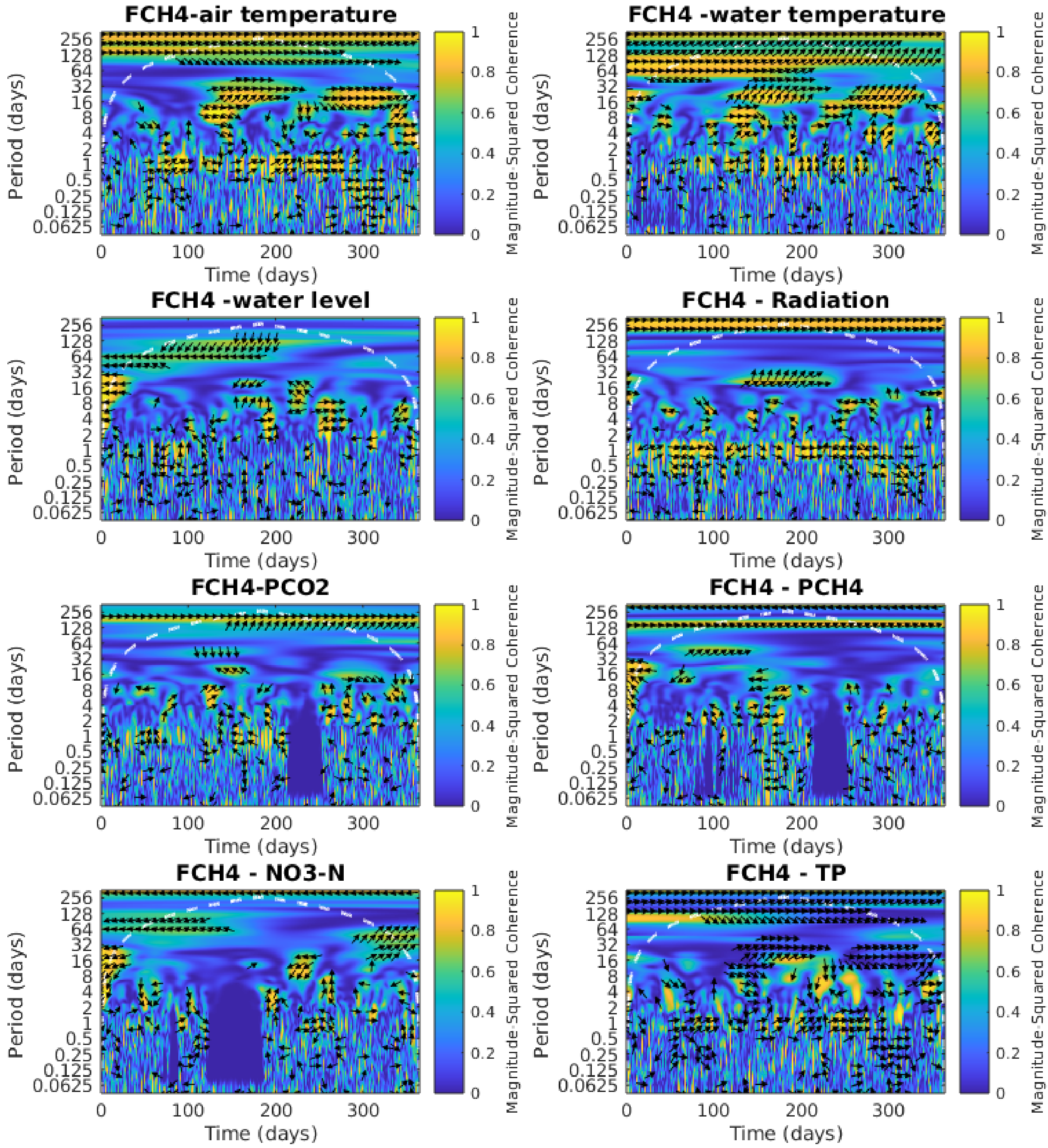

Figure 3: Wavelet coherence analysis and the phase difference between ecosystem CH_4 flux (FCH4) and environmental controls from January to December 2013. The color represents the power of the coherence from 0 to 1. The phase difference is indicated by black arrows which only show up where the coherence is greater than or equal to 0.5. $\rightarrow$ indicates in-phase (two time series in synchrony) and arrows in other direction indicate out of phase (representing lags between time series), i.e. $\leftarrow$ indicates anti-phase, $\downarrow$ indicates the 1^{st} series (FCH4) leads by quarter-cycle and $\uparrow$ indicates 2^{nd} series (environmental controls) leads by quarter-cycle. White dash contour lines indicate the cone of influence. PCO2, PCH4, NO3-N and TP indicate the concentrations of CO_2, CH_4, NO_3-N and total phosphorus in the water.

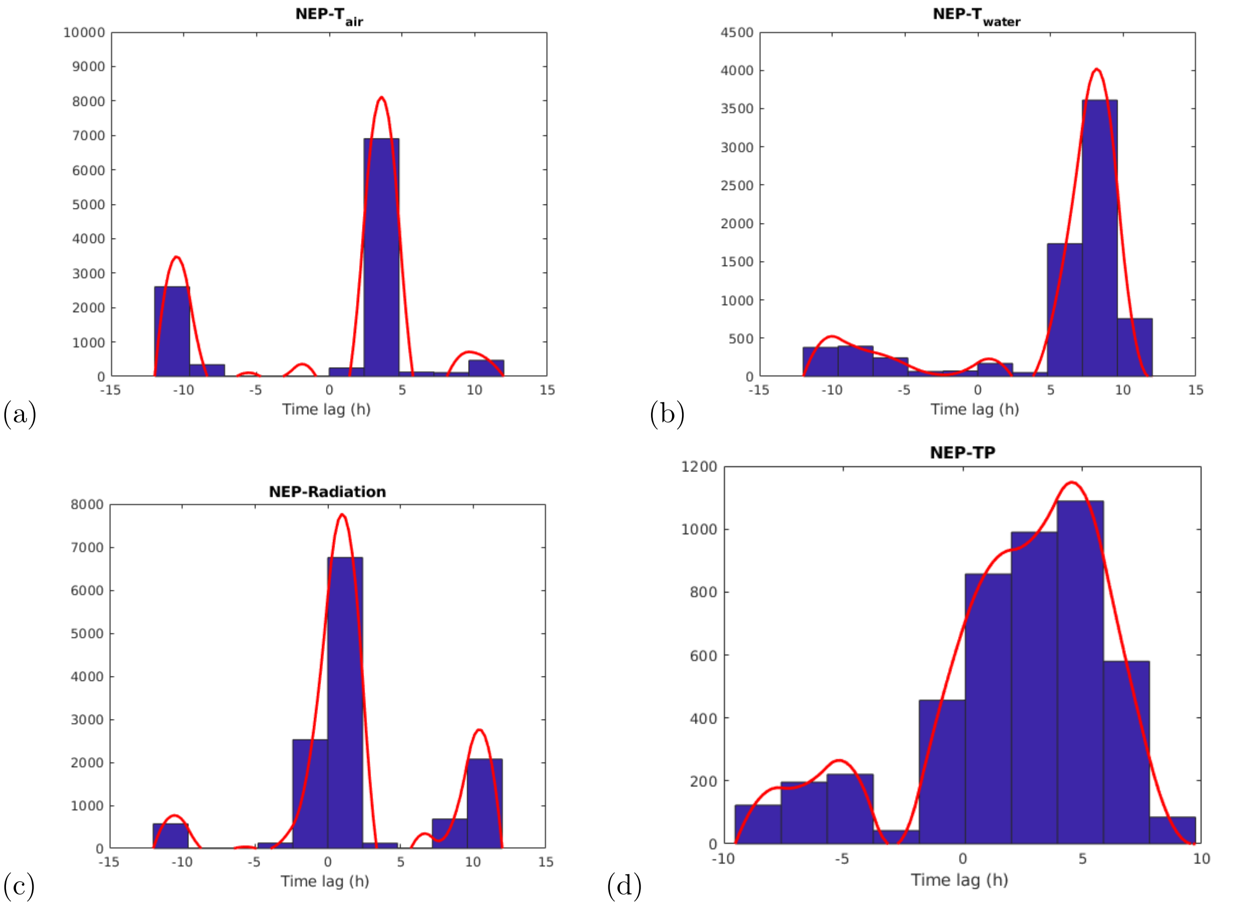

Figure 4: Time lag between NEP and (a) air temperature(T_{air}), (b) water temperature (T_{water}), (c) radiation and (d) total phosphorus (TP) at 1-day time scale. Positive time lags indicate the environmental variables are leading NEP and vice versa.

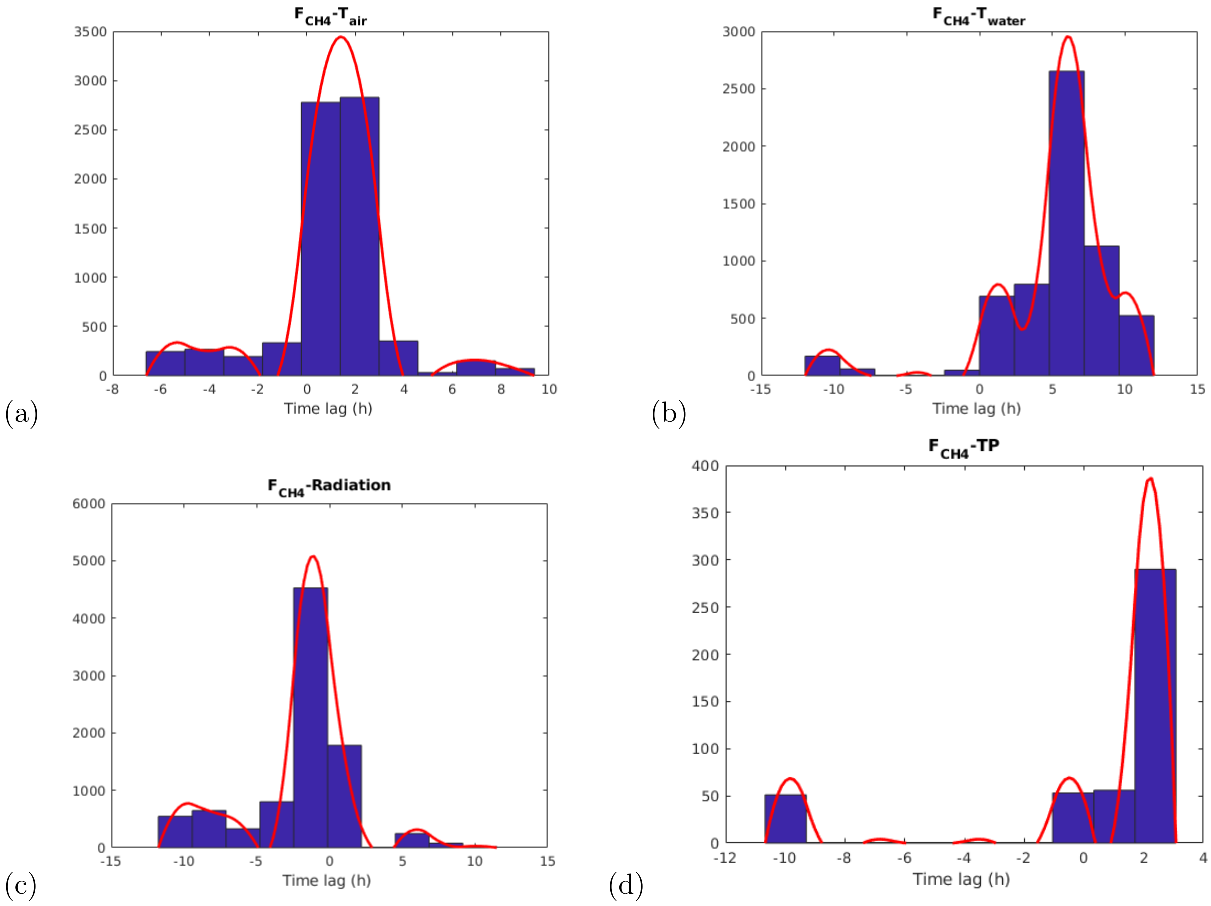

Figure 5: Time lag between CH_4 flux (F_{CH4}) and (a) air temperature (T_{air}), (b) water temperature (T_{water}) and (c) radiation and (d) total phosphorus (TP) at 1-day time scale. Positive time lag indicate the environmental variables are leading (F_{CH4}) and vice versa.

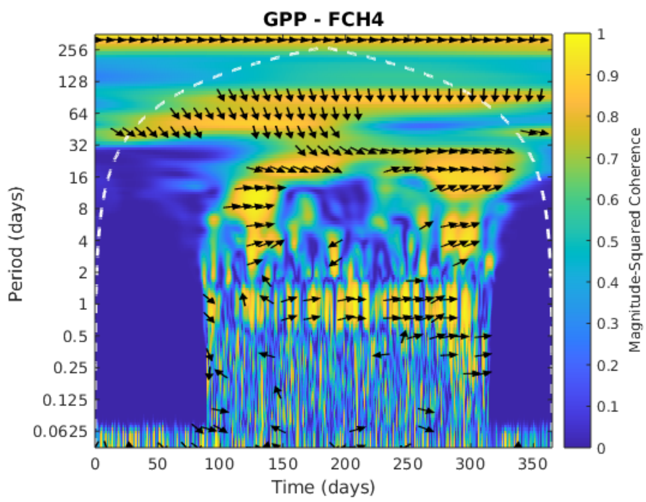

Figure 6: Wavelet coherence analysis for the time series between GPP and FCH4 (caption same as Fig. 2).

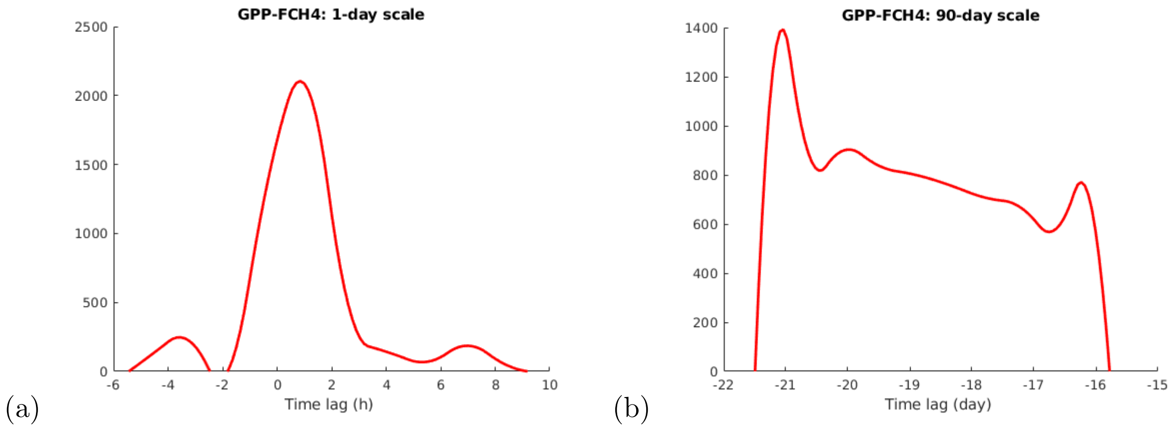

Figure 7: Time lag between gross primary productivity (GPP) and CH_4 flux (FCH4) (a) at 1-day and (b) 90-day time scale. Negative time lag means that GPP is leading FCH4.