Referee 2

We are glad for these very detailed and helpful comments of the referee. We especially thank the referee for the statement that the "topic is both relevant to the journal and of high scientific importance" and that he/she highlighted that the manuscript is generally well laid out which is on a line with the positive comments of referee 1. However, referee 2 raised a series of concerns among which linguistic problems and the misleading title where general critical points. We fully see both points and will review the manuscript by a native speaker before resubmission and we will change the title into "Regulation of CO_2 emission from temperate streams and reservoirs" (see comments to referee 1). The referee suggested also several key references and the need of "more information … on the reservoirs". These points will also be considered in the revised manuscript. The major concern was our calculation of the CO_2 flux from the reservoirs by the use of a fixed k value. We followed the suggestion of referee 2 and recalculated the k values based on actual wind data. We recalculated also all flux data for the reservoirs. The values are in the range of those measured before. Thus, even if the new calculated flux data improve the manuscript this don't change the message (see below).

In the following we answer to most of the critical points. We agree with all minor suggestions which are not listed here and will consider them in the revised manuscript.

I am unsure of the accuracy of using a mean wind speed from manual measurements to calculate K in the reservoirs (see comments below). And following on from this I think it would be more accurate to consider differences in CO2 concentrations rather than evasion rates. Differences in K between streams could be considered separately but the reservoirs cannot be included as no actual measurements have been made.

> We have to thank for this comment and the suggestions. The referee is right that our calculation of *k* for the reservoir data was probably an oversimplification. Nevertheless, a particular strength of our study is, that *k* was determined experimentally in streams. Thus, we would reluctantly reduce the discussion to a comparison of surface concentrations. Instead we tried to improve the determination of *k* for the reservoirs.
> We analyzed the wind speed data of a nearby weather station (Harzgerode, distance to pre-dams 22 km) for the investigated period. The average wind speed during the study period at the weather station was 3.7 ± 1.9 (median 3.3). This is very similar to our episodic measurements on the reservoirs ($U10 = 3.66 \pm 1.7$). Thus it is justified to use the data from the weather station to compute *k* for the reservoirs. The mean *k* was 2.9 (± 2.0) for the Hassel reservoir and 3.0 (± 2.0) for Rappbode reservoir. We will correct the *k* values for the reservoirs in Table 1 and in the whole text accordingly. We recalculated also all flux data based on the individual k values. We optained for the Rappbode reservoir following values: 14.6 (±28.4) mmol m^{-2} d^{-1} in spring, 5.7 (±7.8) mmol m^{-2} d^{-1} in summer, and 42.4 (±40.6) mmol m^{-2} d^{-1} in fall, and for the Hassel reservoir: 2.9 (±14.1) mmol m^{-2} d^{-1} in spring, -2.8 (±3.2) mmol m^{-2} d^{-1} in summer, and 58.3 (±64.0) mmol m^{-2} d^{-1} in fall. The flux data are in the range of the originally calculated values.

I do not see any benefit in stating that CO2 evasion is correlated with CO2 concentration when the concentration is itself one of only 2 parameters actually used in the evasion calculation, they are not independent so it is a circular argument and not statistically correct.

> Of course the reviewer is right that if CO_2 evasion was calculated from concentration we have to assume some correlation. Our formulation "not surprising" was meant to express that (P10032 L3). However, it was the aim of our study to analyze the relevance of CO_2 concentration for the evasion. It makes therefore sense to correlate both. Furthermore, since we recalculated the lentic k values it is now also possible to investigate the relevance of k for the CO_2 flux by the use of the Spearman correlation. Because k is also a part of the flux equation (compare Equation 1) it can be assumed that there are some correlations. Nevertheless, we found for the reservoirs no significant correlation between both factors ($r=0.35$, $p>0.05$), even if we obtained the highest flux data if k was highest ($R^2=0.78$, $p<0.001$). This highlights again the role of high wind events on CO_2 emission from lakes (see above). Under low wind conditions is k less relevant for lentic waters. However, CO_2 flux and k correlated significantly in streams ($r=0.66$, $p<0.05$). Furthermore, we found a significant correlation between CO_2 flux and velocity ($r=0.75$, $p<0.05$) as well as discharge ($r=0.72$, $p<0.05$) if summing values from Rappbode, Ochsenbach and Zillierbach and excluding the Hassel values. The Hassel had significantly higher CO_2 values in comparison to the other streams. We assume that with an increase of the CO_2 concentration the contribution of factors affecting k (velocity, discharge) decrease. We will include all new findings in a revised version of our manuscript (results, discussion, Table 2).

I also think it is important that when upscaling of evasion measurements is done it is always accompanied by an error term which it is not in this manuscript. I acknowledge the error is large and difficult to calculate but some attempt is essential if these values are to be published and systems compared.

> The referee stated correctly that upscaling of evasion measurements are always accompanied by an error term and that some attempt have to be done if lentic and lotic systems should be compared. Thus, we followed his/her suggestion and estimated the rough error of our upscaling.

Major sources of errors are the area, the CO_2 concentration, and k. These parameters are very sensitive regarding environmental changes (discharge events, storm, metabolism, lateral inflow, etc.).

The mean surface area of streams and rivers in the Bode catchment was assumed to be 4 m. We calculated based on this assumption a CO_2 flux of $2.59*10^4$ kmol y^{-1} for the rivers of the catchment. We assume that the variability of the widths could be 50% and calculated the variability of this estimation for a minimal and maximal river width of 2 and 6 m, respectively. The resulting CO_2 flux ranges from $1.30*10^4$ to $3.89*10^4$ kmol y^{-1}.

We estimated the CO_2 flux from the Bode catchment by assuming a mean CO_2 concentration. The used data are originated from two campaigns were the whole Bode catchment was sampled at the beginning and at the end of the vegetation period (Kamjunke et al. 2013, Environmental Monitoring and Assessment). We used the minimal and maximal CO_2 concentration (CO_2min=8.0 µmol L^{-1}, CO_2max=274.5 µmol L^{-1}) of this data set for error estimation. The resulting CO_2 flux ranges from $5.33*10^2$ to 1.27 $*10^5$ kmol y^{-1}.

We estimated the annual CO_2 flux for the streams Hassel ($4.06*10^3$ kmol y^{-1}), Rappbode ($1.73*10^3$), Ochsenbach ($5.61*10^1$ kmol y^{-1}) and Zillierbach ($2.11*10^2$ kmol y^{-1}) based on very detailed data (widths and lengths) from different organizations and from own experimental data. We used the minimal and maximal variability of the width of each stream for the error estimation. The CO_2 flux ranges for Hassel from $1.97*10^3$ to $9*10^3$ kmol y^{-1}, for Rappbode from $3.82*10^2$ to $3.23*10^3$ kmol y^{-1}, for Ochsenbach from $2.99*10^1$ to $8.43*10^1$ kmol y^{-1}, and for Zillierbach from $1.05*10^2$ to $5.27*10^2$ kmol y^{-1}. We tested also errors based on a possible k variability of 50 %. The resulting CO_2 flux ranges for Hassel from $1.98*10^3$ to $5.95*10^3$ kmol y^{-1}, for Rappbode from $1.79*10^3$ to $5.37*10^3$ kmol y^{-1}, for Ochsenbach from $3.04*10^1$ to $9.11*10^1$ kmol y^{-1}, and for Zillierbach from $1.05*10^2$ to $3.14*10^2$ kmol y^{-1}.

The variability of k in reservoirs could also be a pronounced error source. We excluded for example stormy days and low wind during the night from our measurements. To estimate this error we computed hourly flux data by interpolating the measured CO_2 concentrations and combining them with hourly atmospheric CO_2 and wind data from a weather station. This procedure neglects short term changes of the CO_2 concentrations in the water but accounts for short term wind fluctuations. The mean annual flux computed this way was 15 (SD 55.4) mmol m^{-2} d^{-1} and 18.9 (SD 32.4 mmol m^{-2} d^{-1}) for PD Hassel (pre-dam Hassel) and PD Rappbode (pre-dam Rappbode), respectively. This is nearly identical to the mean flux of our 14 sampling days (17.1 (SD 44.1) mmol m^{-2} d^{-1} and 20.0 (SD 32.4) mmol m^{-2} d^{-1}). Obviously the opposing effects of extremely high and low wind cancelled each other out and our estimate of an annual flux from the reservoir was probably little affected.

The analytical error of CO_2 determinations in lake water was below 10%. If we use 5% higher or lower CO_2 concentrations, the calculated fluxes from the reservoirs are between 18.2 and 21.8 mmol m^{-2} d^{-1} in PD Rappbode and between 15.5 and 18.7 mmol m^{-2} d^{-1} in PD Hassel. Thus, the maximum error due to incorrect CO_2 measurements was below 10%.

Taken together the errors for the emissions from the catchment and the streams were very high. The error could be over a magnitude of one. The error for the annual emission from the reservoirs was lower than one order of magnitude. We will include this rough error estimation in the revised manuscript. Nevertheless, it was the aim of this study to investigate the different regulation mechanisms on CO_2 emission from lotic and lentic systems. We don't have enough values for an exact budget for the whole catchment (including Monte Carlo simulation for error propagation). The data are a rough estimate as it was already stated in the manuscript. Future work has to address this point in more detail.

Much of the discussion concerning the pre-dam surface concentrations (and subsequently fluxes) regards stratification. Presumably depth is important in this (though admittedly my expertise is more focussed on stream systems), in which case the bottom profile of the reservoir is likely to be important, especially if it is sloping from very shallow at one end to deep adjacent to the dam. This needs to be discussed and the profiles described where possible given that sampling was only carried out at the deepest points in the reservoirs.

The reviewers is right that lateral transects are typical features of reservoirs. Lateral heterogeneity in the pre-dams was investigated in a master-thesis (Perz 2013, http://uppsok.libris.kb.se/sru/uppsok?query=anywhere+all+%22perz%22+and+publisher+all+%22SLU%22&startRecord=1&recordSchema=dc&operation=searchRetrieve&stylesheet=databases%2Fuppsok%2Fhitlist.xsl&maximumRecords=25). Major results of that thesis were that while e.g. plankton composition changed along the reservoirs, the surface CO_2 concentrations were quite similar along the reservoirs. For a precise carbon budget of the reservoir, transect measurements are probably necessary, but for the purpose of this manuscript, the sampling point near the dam can be considered representative for the reservoirs.

10025 Ln 15: It is not clear what a 'typical central European setting' is, more detail is needed or the sentence should be omitted

We rewrite the whole sentence. The sentence was thereby split into two sentences:

"We measured the CO_2 flux from 4 streams and 2 reservoirs in the same catchment located in central Europe. We wanted to find out, whether streams or lakes emit more CO_2 per area and what the underlying reasons are for that."

10026 Ln 1: It is unclear what is meant by 'rural' and how it differences from 'pristine'...I think you mean 'agricultural'? Furthermore I don't know that true 'pristine' catchments exist in central Europe given anthropogenic air pollution and enhanced N deposition, it's a technicality but I think the wording is important. Maybe 'unmanaged' or something similar would be a better term.

We will change the expression „rural" into "land-use" as it was previously used in Halbedel et al. (2013, Biogeosciences). Nevertheless, the expression "pristine" had the meaning unsullied, or unmodified from a natural state. The streams Ochsenbach and Zillierbach are next to the Nationalpark Harz and their immediate environment is strongly protected. We think that "pristine" is the right expression in this case. But we see also the point mentioned by the referee and will change "pristine" into "rather pristine".

10026 Study sites: I recognise the information can be found elsewhere and references have been given but I think things like landuse are important enough to be described in this paper as well. I think a bit more information should be included here.

We included the following sentences into the mentioned chapter:
"The streams have the following land-use types in their catchment: Rappbode has 6 % agriculture (cattle), 1% urban areas, and 93 % semi-natural areas and forest, and Hassel has 14 % agriculture areas (cattle), 3 % urban areas, and 83 % semi-natural areas and forest."

Also include some meteorological information, i.e. you mention later that winter emissions are not included due to ice cover so information of the ice cover season should be included, min and max annual temperatures and precipitation also.

These information are well described (including soil, land-use forms, geological underground, climate, precipitation, etc.) in the recently published and already cited article of Halbedel et al. (2013, Biogeosciences). With regards to referee 1 our manuscript is too long. Thus, we would rather refrain to repeat this information here again.

10026 13: biweekly or monthly from what date until what date... its unclear over what period this study is carried out and how many samples the analysis is based on.

The reservoirs were sampled 14 times between 9.3. and 19.12.2011. This information will be included in the methods section

10026 15: What depths were samples collected and what depth was the reservoir

Samples were taken from 0, 2, 5, 8, 10, 12 m and above ground. The maximum depth is 14 m and 17 m for the Hassel reservoir and for the Rappbode reservoir respectively. We will include these points in the method chapter.

10026: Why was the same method for headspace collection not used for the reservoir as the lakes, an important difference seems to be equilibration at water temperature in the streams for 1 minute compared to equilibration in a lab for 30 minutes for the reservoir samples... has any test been done to calculate the difference due to method.

The CO_2 concentrations were determined with different methods, because the samples are from different campaigns: The reservoir samples were taken during routine water quality monitoring while the stream data were obtained during a metabolism study (cp. Halbedel et al. 2013, Biogeosciences). The monthly to biweekly monitoring campaign is normally done within one day. It is from our point of view in this case more accurate to bring the samples as fast as possible in the lab and analyze them there immediately. For the metabolism studies the whole team was in the field for 4 days. It is in this case recommended to perform the equilibration directly in the field and to store the gas samples until analysis. Both techniques are standard techniques. We used in streams a technique that is for example well described by Hope et al. (2004, Hydrological Processes) and Roland et al. (2010, Aquatic Sciences). The method that we used in lakes was e.g. used by Striegl and Michmerhuizen (1998, Limnology and Oceanography) or Anderson et al. (1999, Limnology and Oceanography). We showed with test experiments in the laboratory that both techniques lead to a complete equilibration. Furthermore, own comparisons with CO_2 probe measurements both in streams and reservoirs showed good conformity. We are therefore certain that both techniques produce accurate results.

Also was pressure at the different water depths accounted for when calculating reservoir concentrations in the profile?

Pressure is only relevant for the calculation of the CO_2 partial pressure. For the GC analysis of the absolute concentration in $\mu mol\ l^{-1}$ the actual pressure is not needed.

streams. Were they just sampled twice (noon and before sunset on same day) for each season in 2011? It should be made much easier to find information of what was sampled, from when to when the sampling occurred, and at what sampling frequency.

We agree with the referee, and will include more details on the sampling campaign in the method chapter. Details will include the sampling dates, that we measured for two days at noon and before sunset, and that we measured in regular intervals along stream reaches of 50 two 135 m length as well as the number of samples that were analyzed. More details are given in our recently published article (Halbedel et al. 2013, Biogeosciences).

10027 Ln 14: GF/F 45 m pore size... is this correct? Usually GF/F refers to 0.7 m pore size

We apologize for this mistake. Because GF/F filter have an unspecific pore size, we will delete the stated pore size and write only GF/F.

10028: More detail needed, e.g. detection limits, name of DOC/TIC analyser.

We are not agreeing with this comment. These are standard techniques that were often used and well described by members of our institute. We already described the main technique in the manuscript and cite the references were these methods were described in detail. With regards to referee 1 - who stated that the manuscript is too long - we would leave the mentioned chapter as it is.

10028 Equation 1: In text the authors refer to the CO2 concentration in the surface water as C2water yet in the equation it is CO2water and the air equilibrated water is C0 in both text and equation, I cannot make sense of this notation... revise.

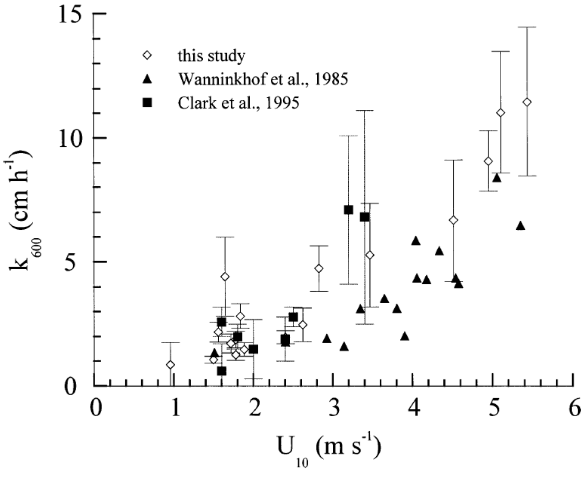
We are sorry for this inaccuracy and corrected this expression. It is now CO_{2air} in both text and equation.

Also no account of solubility is included in the flux calculation whereas most studies calculate flux based on water-air gradient times solubility times gas transfer coefficient?

CO_2 concentrations are often expressed as partial pressure (µatm). In that case indeed the solubility coefficient must be included in equation 1 to convert µatm to mmol L^{-1}. However, we expressed our concentrations as molar concentrations and the solubility coefficient is included in the calculation of the concentrations. This is stated in 2.3 line 6 were we write “the concentration of CO_2 in the water (mmol L^{-1}) was calculated by applying Henry´s law (Kling et al. 1991)”.

10029: The wind speed equation assumes a certain fetch to allow wave/turbulence development. Given that reservoirs are usually long and thin in shape is there a sufficient fetch across all wind directions to achieve the k predicted by the equations?

As stated above we agree completely that the parameterization of k from wind speed includes a lot of uncertainty. This becomes immediately clear if looking at the graph from Crusius and Wanninkhoff (2003, Limnology and Oceanography):



The applied calculation of k from wind speed, however, is widely used in the literature and at the moment probably represents the best compromise between practicability and high temporal resolution. From personal observation we know that the local wind field on the reservoirs is somewhat heterogeneous. The major wind direction is along the reservoir. At the sampling site the reservoir is rather wide and the good agreement between our episodic direction independent measurements and the data from the weather station gives us the confidence that the used wind data are acceptable for our purpose.

Also more information is needed describing where the wind speed was measured, was it measured at all reservoirs or just one? And why has the average been used, would it not be more accurate to use individual wind speed measurements with the associated concentration gradient and take the average emission over time rather than disassociating individual k and concentration measurements? I assume the lack of continuous wind speed introduces a significant uncertainty into the evasion calculations and presumably a significant underestimation given the shape of the wind speed relationship. Are no meteorological stations available nearby that manual measurements could be correlated to improve this accuracy? Perhaps even having wind speed data from nearby would allow an analysis of the wind speed frequency distribution, knowing the shape of the data distribution would allow a basic mathematical calculation of the error introduced when the wind speed equation is used?

As suggested by the reviewer and explained above we now include wind data from a weather station 22 km away. For future studies we are actually installing local weather stations.

10030 Ln 14: P-value for r = -0.79... is this significant?

This correlation was significant. We will add all p values.

10030: Throughout this paragraph results are repeatedly described as higher or lower but no statistics are presented for the reader to know if these are significant results or just observations.

We will include all statistical details (p-values).

10030 Ln 22: How were surface areas calculated? This is a major part of the upscaling and subject to significant error so should be acknowledged. Also it is unclear how many measurements these calculations are based on. I understand the need to upscale but it is vital that any calculation such as this include an error term which I suspect for these values is extremely large. A large error term does not diminish the results in any way, it simply highlights the difficulty in measuring this flux and the need for more work to be done in the area and I don't think the numbers should be published without it.

Surface area was calculated based on very detailed official data (width and length) that we obtained from different organizations (Ute Enders: Unterhaltungsverband Holtemme, Detlef Cöster: Talsperrenbetrieb Sachsen-Anhalt and Otfried Wüster: Nationalparkverwaltung Harz). We will describe this in more detail in the method chapter. As stated above, we estimated the error of our upscaling and will include also these details in a revised manuscript.

10031 Ln 10: With reference to Hassel CO2 concentrations it appears as though concentrations are actually similar to other sites in both spring and summer and it is only the fall concentrations that makes it significantly different from the others?

The concentration of Hassel was in all seasons significantly higher than in the other streams. We will include corresponding indices into figure 4 and we will explain them in the figure legend.

10031 Ln 12: the Rappbode did not have the highest evasion in spring, this was again the Hassel?

It seems to be a misunderstanding, based on linguistic problems. Thus we rewrite the mentioned sentence (P10031 L11-13). They are now: "In comparison, the Rappbode had with 132.9 (± 15.6) mmol m^{-2} d^{-1} its highest CO_2 evasion rate in spring."

10032 Ln 6 (and elsewhere): when describing correlations or regressions include both r/r2 values and P values.

We will follow this recommendation.

Table 2: CO2 concentration is used in the evasion calculation, the parameters are therefore not independent and do not meet the assumption of regression or correlation statistics.

It was the aim of our study to investigate the regulation of CO_2 emission from lotic and lentic waters. Regulating factors could be the CO_2 concentration as well as k, and factors affecting both parameters (metabolism, groundwater, velocity, discharge, temperature, etc.). The referee stated correctly that the CO_2 concentration is a part of the equation that was used for the flux calculation. Thus, it appears to be logic that CO_2 concentration

correlates with the flux. But the question was how good this correlation is. Since also k is one of the main factors in this equation, it can also be assumed that k correlates with the flux. As stated in our manuscript, it was not surprising that the CO_2 concentration was found as a major factor regulating the flux from streams and lakes (P10032 L3-4). We found strong significant correlations. But we found only for streams a moderate correlation between k and CO_2 flux and no significant correlation for reservoirs. This indicates that k is only relevant for streams and not for our lentic waters; even if we now use variable k values instead of constant values. Thus, the correlation analysis seems to be an appropriate tool for the investigation of regulation mechanisms. Furthermore, we used temperature for the correction of k as well as discharge for the calculation of $k_{propane}$. Thus, if we would follow this suggestion it would not be allowed to correlate discharge and temperature with the CO_2 flux.
The referee stated also that it is not correct to use regression analysis for the description of the relation between CO_2 flux and concentration. As already shown is the CO_2 concentration a strong predictor of CO_2 evasion, especially if the variability of k is low. Thus, it is also correct to use regression that "describes one variable, the dependent … variable, as a function of one or more independent … variables." (Gordon et al. 2008 Stream Hydrology: An introduction for Ecologists (second edition)). In our case is CO_2 flux the dependent variable (Y) and CO_2 concentration is the independent variable. We used this tool in our manuscript (P 10032 L4-8). The regression equations are the basis for the estimation of the CO_2 emission from the catchment. We will describe this point in the method chapter of the revised manuscript.

Furthermore, as k is held (almost) constant across reservoirs would it not make more sense to consider what drives differences in CO2 concentration directly rather than evasion. Differences between stream k values could be considered separately. A simple sensitivity analysis could then be carried out to see how changes in concentrations vs changes in k values contribute to the final evasion flux and the drivers of evasion considered this way.

The reviewer is right here. As stated above, we would reluctantly reduce the discussion to a comparison of surface concentrations. Instead we tried to improve the determination of *k* for the reservoirs as explained above.

10036 Ln 12: I don't think the data in this study allows conclusions to be made on the drivers of reservoir fluxes, only the drivers of reservoir surface CO2 concentrations. This comment should be considered throughout the discussion.

We agree with the referee that it would make more sense to compare CO_2 concentrations of the surface water if constant k values are used. But we followed his/her former suggestion and use now variable k values for the calculation of the flux (see above). This allows the discussion of the role of wind for reservoir fluxes.

10037 Ln 19: Here the authors consider what controls the temporal variability in fluxes however the only analysis is based on spatial variability. It is not clear what this is based on as spatial and temporal drivers are likely to be very different.

We will delete this sentence.

10038 Ln 2: no correlation between Q and FCO2... again there is confusion over whether the authors are considering spatial or temporal variability which are likely to be driven by different things.

We investigated all parameters at different seasons and at different sites. Thus, it is generally correct to interpret our result regarding their spatial and temporal variability. Nevertheless, the referee suggested correctly that the effect of hydrodynamic factors could be site specific. Since one of the investigated streams is clearly different in comparison to the other streams (Hassel) we tested several correlations again by excluding this stream. We found a positive correlation between discharge and CO_2 flux ($r=0.72$, $p<0.05$) if summing the values from Rappbode, Zillierbach and Ochsenbach. We will include this new finding in the revised manuscript.

10038 L15: Why use this linear relationship rather than the actual concentrations and a summary of measured k values. Mean/median k could be used with known concentrations and the min and max used to produce a range of potential upscaled values?

Because k is known to be highly variable in space and time, we clearly contradict the suggestion of referee 2 to use a summary of measured k values for an upscaling of the CO_2 flux on the catchment scale. We actually don't have enough k values for such estimation and simple estimations of k are widely known to be very uncertain. Furthermore, CO_2 concentration is the main predictor of CO_2 flux. That's why we found a strong correlation between both for streams and lakes. We think it is in our case more accurate to use the linear regression which was highly significant in lotic and lentic waters (P10032 L4-8).

Figure 7: As it is presented I do not think this figure provides any additional clarity or information more than simply writing 3 sentences. I assume the size of the dots and arrow relate to CO2 evasion but no axis or legend is available to show this.

We are disagreeing with this comment. As stated by referee 1, this figure summarizes the main outcome of our study. Thus, we keep it in the manuscript, even if we have to extend the scheme for the factor reservoirs at high wind conditions. In addition, this figure is a model or scheme describing the effect of different regulation mechanism. Models or schemes not necessary need a scale.

Boxplots: I suggest adding letters to boxplots to show streams/reservoirs that are statistically similar or different e.g. use of statistic such as Tukey's family test.

We will follow this comment.

The referee suggested also additional literature that might be useable for our argumentation.
Billet and Harvey (2012, Biogeochemistry)

We will use this reference as an example for measurements of reaeration in streams.

Lundin et al. (2013, Journal of Geophysical Research)

We will cite this article as an example from a subarctic catchment. They showed that the CO_2 flux from streams is also their relevant on a catchment scale.

Dinsmore et al. (2004, Global Change Biology)

We will cite this article as an example that the annual variability can be correlated with the catchment gross primary production.

Dawson et al. (2004, Biogeochemistry)

This article is interesting, but not relevant for our research question.