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Discussions

Interactive comment on "Interactions between nocturnal turbulent flux, storage and advection at an 'ideal' eucalypt woodland site" *by* Ian D. McHugh et al.

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Thanks to the reviewer for efforts and constructive commentary. Please find responses to specific critiques below.

Reviewer comment:

Throughout the paper: you often using multiple character variables in your equations, eg NEE, Av_c, rb, ER etc. This is considered bad practise because of the potential for mis-interpretation. For example Avc may be interpreted as A * vc. I would recommend usage of subscripts and superscripts for differentiation of variable names eg F_{NEE}, Av , R_b, R_E.

Response:

All authors intending to submit to the Biogeosciences special issue agreed on a standard set of naming and sign conventions and some of the acronyms noted above (e.g. NEE and ER) are suggested as standard terms in the literature (e.g. Chapin et al., 2006). We don't think the use of F_NEE is necessarily appropriate since it implies Flux_NetEcosystemExchange. The only flux we are actually 'measuring' (i.e. through the approximating assumptions of eddy covariance) is the vertical turbulent flux at the measurement height. Therefore, when Fc and Sc are summed and corrected for low u* conditions, we get an estimate of ecosystem / atmosphere CO2 exchange that is not strictly a flux. Advection has been dealt with in the literature typically as A or Ac. Given that we cannot separate the horizontal and vertical components of advection here, we will amend the manuscript to refer only to Ac, and to discuss the horizontal and vertical contributions as appropriate in the text.

Reviewer comment (Page 3 lines 10-21):

I think the logic in these two paragraphs characterize the faults in the u* analysis used in this paper. The total flux (NEE) is a combination of turbulent flux, storage and advection. If you only measure turbulent flux then you need to model the other two – either separately or combined – to estimate NEE. In the case of this paper you have measured storage so you only need to model advection.

Response:

This may indicate that we have been somewhat unclear in defining the purpose and methodology of the paper, which we will duly amend. We have attempted to show that the advection component of the nocturnal CO2 mass balance can be coarsely approximated as the residual of the mass balance equation (equation 2 or 3 in the paper) because we can either measure or model all other components. It was not our intention to model the advection component but to show that the advection component is important even at one of the flattest forested sites in the Australian flux network;

our method allows us to do this. We also can estimate the proportional contributions of advection and storage to the shortfall in the turbulent flux relative to the source term (approximated through a simple model). This is important for reasons that we summarise below.

âĂČ Reviewer comment (Page 3 lines 10-21):

However, you have used the standard u* correction approach which assumes that advection only occurs at night and only under a limited range of u* values. This artificial restriction in the modelling of advection is what results in the double counting and some of the more elaborate explanations required later in the paper.

Response:

There are two schools of thought about whether the u* correction should be applied during the day (see Papale, 2006); I am hesitant to apply it during the day because the mechanisms that result in u* dependence are almost exclusively nocturnal. Nonetheless, another reviewer has argued that we should filter both nocturnal and daytime data using the nocturnally derived u* threshold, and we have undertaken to do so and include these results for comparison.

The u* correction of course does assume that '. . . advection only occurs under a limited range of u* values,' but surely this is precisely the point of the correction. The dependency between u* and Fc (or more appropriately the sum of Fc and Sc) is interpreted as indicating that some of the respiratory CO2 source is diverted into the remaining terms in the mass balance (i.e. advection). Implicit in this is the assumption that above the threshold where there is no such dependency, the remaining terms in the mass balance are indeed negligible. We argue that this is a reasonable inference in the absence of reliable methods to directly measure advection. There are many grounds for criticism of the u* correction (see for example Aubinet et al., 2012; Van Gorsel et al., 2007), but we make three observations: i) the site in question is relatively close to the eddy covariance ideal in terms of terrain and vegetation, and does not appear to be

subject to many of the problems that make application of the u* correction approach particularly problematic; ii) the paper is not intended to delve into the nuances of the appropriateness of the u* correction in general (it can probably be characterised as the flux community's least-worst option at present), but rather attempts to underscore the problems that arise when it is applied inappropriately, and; iii) other approaches, such as that of Van Gorsel et al. (2007) also must infer the presence of advection from the unphysical behaviour of flux and storage measurements under certain conditions, and thus the assumption that advection only occurs under a limited range of u* values is not limited to the u* correction approach. It is a necessary assumption.

We disagree that '. . . This artificial restriction in the modelling of advection is what results in the double counting.' In the absence of storage measurements, carbon is stored within the control volume nocturnally, thereby understating the efflux of CO2 from the ecosystem. In the morning, the stored carbon is either consumed by photosynthesis or 'flushed' upwards through the measurement height at the onset of turbulence; in either case, this results in an underestimation of morning carbon influx into the ecosystem equivalent to the magnitude of underestimation of the prior nocturnal efflux. This must be (approximately) so to satisfy conservation of mass. The application of the nocturnal u* correction in the absence of storage measurements corrects for the underestimation of Fc due to the effects of BOTH storage and advection. Thus the efflux of the stored carbon is double-counted; once nocturnally via the u* correction, and again in the morning when the effects of the stored carbon alters the magnitude of the turbulent flux. We are open to being convinced that our interpretation is not correct here, but the idea that double-counting occurs in the absence of profile measurements is not new, and has been made by Aubinet et al. (2002), Papale et al. (2006) and Aubinet et al. (2012), among others. As noted above, we also ascertain the proportional contributions of storage and advection to the nocturnal mass balance, because this is critical to the question of whether the u* correction should be applied in the absence of storage measurements. If the advection term is larger than the storage term, then the u* correction should be applied. But if the storage term is larger (as was the case for our site), then it should not, because the double-counting problem noted above actually increases the bias in estimates of annual NEE relative to applying no correction.

Reviewer comment (Page 3 lines 10-21):

A better approach would be to model the advection term for the entire diel period (see appendix "A" in Clement, Jarvis, Moncrieff, Agricultural and Forest Meteorology, Volume 153, 2012, Page 106). Taking such an approach would provide a simpler interpretation your already excellent data set.

Response:

This appears to be a relatively complex analysis and would constitute a substantial further undertaking. While a comparison of such an approach with the more conventional u*-filtering approach would be valuable, the current paper is intended to provide a cautionary note about the use of the u* filtering technique by demonstrating the biases that arise when the technique is applied in the absence of storage measurements. It is not clear to us that this aim would be enhanced by introducing a different method of assessing the contributions of other terms in the control volume mass balance. Moreover, we note that the site in the above-cited paper is on a slope of approximately 7o, substantially in excess of that for the current site (<1o).

Reviewer comment (Page 5 line 13):

Are you using the newer low heat emission LI7500 – or are you accounting for sensor associated heat flux enhancement in the WPL correction?

Response:

We are using the original LI7500. George Burba at Licor has previously suggested that we don't need to apply the heating correction unless temperatures are below 5oC. This occurs very rarely at this site. We can apply the corrections using EddyPro and check for any substantial changes in the results.

âĂČ Reviewer comment (Page 7 line 22):

Was soil moisture not included in the respiration model because it was not measured or because there was no relationship? It seems that a 15 day window smoothing is a poor way of incorporating any soil moisture effects resulting from episodic precipitation

Response:

The use of an additional soil moisture response function increases the number of parameters to be fitted to the data when the signal:noise ratio in the data is already low, so we sought to avoid estimating too many parameters. There is little seasonal variation over the years in the free parameter in the Lloyd and Taylor temperature response function we used, but it adding a soil moisture function may improve the response to episodic soil wetting. We propose to run a more complex optimisation using a soil moisture response function and using AIC or BIC to select the most parsimonious model.

Reviewer comment (Page 7 line 23):

Using temperature measurements from 36 m as the driver for respiration would require better explanation, particularly at night when there may be decoupling between the surface/canopy microclimate and the air well above the canopy. It may be that temperature profiles were homogeneous throughout the night (show average nocturnal T profile) or it may be that heterogenic respiration is negligible - which would likely further complicate the interpretations in this paper.

Response:

We reported that the lowest RMSE for our respiration model was obtained when using the uppermost temperature sensor – this suggests that it is the most representative of all of the measurement heights, but the difference in RMSE between heights was VERY small. We believe that it is likely that the majority of the respiration is most likely from the vegetation given the very small quantity of carbon in the soil, but we have included nocturnal profiles of temperature and expanded discussion relevant to this point.

Reviewer comment (Page 8 line 22):

The error character used in the equation differs from that used in the text.

Response:

Amended.

âĂČ Reviewer comment (Page 9 line 1-6):

It is likely that the environmental data corresponding to the missing flux data are not representative of the environmental data corresponding to the available flux data, and it is likely that these environmental data may be under more extreme conditions. Your approach of removing random observational data – likely from periods when the model fits the data well – seems as though it will underestimate the model error.

Response:

This is a good point, although it doesn't necessarily follow that because the conditions would be more extreme that the model would fit the data more poorly. More variance in both stimulus and response may improve the signal:noise. In any case, since the data are missing, this is a moot point. We now emphasise that our approach has limitations and may underestimate the error to some extent.

Reviewer comment (Page 9 line 14-17):

Why do you assume there is a threshold u*? It is possible, or likely, that advection is occurring under all u* conditions- albeit more severely at small u*. Determining a u*th confidence interval is simply giving you a false sense of security. The only way to truly test for the presence of advection at night is to measure the soil and canopy respiration components and scale them up to verify that EC flux at high u* matches scaled up chamber measurements – an approach that has its own limitations.

Response:

We do not assume that there is a u* threshold. We use change point detection to determine whether there is a change point in NEE when expressed as a function of u*, and the algorithm determines that there is. We thus deduce by objective methodology that there is a u* threshold. Of course it may be the case that advection occurs under all u* conditions, but we are making an assumption (that is reasonably theoretically justified) that above the u* threshold, the advection term makes a small contribution to the nocturnal mass balance. Again, such assumptions are not confined to the u*-filtering approach. Since we cannot measure advection directly, we are inferring its presence or absence from the behaviour (in response to meteorological conditions) of the mass balance terms we CAN measure. It seems therefore that this is a criticism that applies to the field rather than our paper alone. We also note that the profile system gives us a secondary and completely independent estimate of the u* threshold. It seems reasonable to assume that if the storage term goes to zero under well-mixed conditions, advection (at least associated with drainage flows) is likely to be minimal. We thus argue that allowing for the possibility that the u* threshold may be anywhere within a large uncertainty range (and propagating this to annual NEE estimates) is perhaps unduly conservative, since the profile system yields the same best estimate for u* threshold. Nonetheless, we are happy to further emphasise that the range of uncertainties we have been able to quantify are only a subset of the true uncertainty in the NEE estimates. We also agree that chamber-based scaling up of nocturnal (and ultimately daytime) respiration would be a highly desirable addition to this (and pretty much every other eddy covariance) study, but it was beyond the resources we had available for this project. It is something we hope to do in future, and we will emphasise this in the conclusions of the paper.

Reviewer comment (Page 10 line 29-31):

This statement is inconsistent with your use of the 36 m temperature as the primary determinant of nocturnal respiration.

Response:

Agreed, and will be amended.

Reviewer comment (Page 11 line 1-6:

Is it not equally plausible that cooling initiated at the surface and progressed upwards, resulting in suppressed respiration as the depth of surface cooling increased. This could be verified by seeing the strength of the temperature profile changes. If surface cooling is strong they it is likely that your simple one-temperature respiration model will be incorrect and you will need a multilayer model. If you indeed do have advection of low CO2 air into the bottom of the canopy then you likely have a situation of non-homogenous land cover- which may be (possibly) observed as directionally dependent effects on the nocturnal CO2 profiles.

Response:

We prepared figures that show increased cooling as a function of u* in the lower layers relative to the upper layers, but in the end excluded them because we determined that this should not logically result in declines in Sc. This is because lower temperatures imply two antagonistic effects: i) lower temperatures will result in reduced respiration, but; ii) lower temperatures at lower levels are also generally indicative of less turbulent conditions, since radiant heat loss from the surface cools the lower layers, which will will only remain so if there is limited turbulence to replenish heat from above. The limited turbulence will therefore limit the magnitude of Fc whilst increasing Sc, unless there is advection. Nonetheless, we will clarify this point in the text and include a plot showing the temperature profiles as a function of u*. With respect to the point about non-homogeneous land cover, we expect that the flux footprint becomes sufficiently large at low u* (stable conditions) that the flux footprint most likely extends beyond the boundaries of the ecosystem. The flow is predominantly westerly, and we consider it likely that under calm conditions, there are gravity flows from the northwest. Under these conditions, the flux footprint may extend several kilometres to the ridge, in which case there is likely entrainment of CO2-depleted air from the overlying atmosphere that

results in horizontal variations in the CO2 field and corresponding advective losses. However, this is largely speculative; in the literature, most terrain-induced flows are <0.4m s-1. To measure such weak winds requires 2D sonic anemometers (generally precision of up to 0.01 m s-1). Our cup anemometers have a start-up speed of 0.5 m s-1 so we cannot measure these effects. Wind directions for the mechanical vanes are not valid once the velocity is <0.8 m s-1 (and most certainly not below 0.5m s-1, when the measured wind speed will be zero), so that a directional analysis is unlikely to be useful. Again, we can emphasise this point in the text.

Reviewer comment (Page 11 line 11):

It appears as if the 8 to 36 m layer has an exponential increase with decreasing u* down to the level of u* 0.1 at which it also appears to decrease. Implementing such an exponential increase is unlikely to result in as good a fit with your results. Perhaps I don't understand why you simply do not use the temperature response model of respiration using data from u* greater than 0.5 to parameterize what was missing. Does it really matter if the advection is reducing turbulent flux or storage, it is still 'missing' flux.

Response:

We can amend the algorithm to include a non-linear relationship in the 16-36m layer, but we are not convinced this will make a large difference, since the correction is based on using the average of the 8-16m and 16-36m layers. Our rationale for including this element of the analysis was that the response of the storage term to u* implies the presence of a specific advective mechanism (drainage flows, which are expected to mostly affect the lower layers, because they are typically shallow features. If our simple linear correction of the lower layer storage terms removes the decline in NEE as a function of u* (or as reported in the paper, causes the corrected Sc to be approximately equal to the modelled source term minus the observed turbulent flux), this suggests that loss of carbon from the lower layers of the control volume is primarily responsible for the decline in Fc + Sc as a function of u*. In turn, this supports the hypothesis that drainage flows may be primarily responsible for advective losses. We acknowledge that this element is somewhat speculative (and in the paper we emphasise that instrumentation to directly measure the presence of these flows – e.g. a profile of windsonics – would be a valuable addition). Our primary motivation for its inclusion was to show that for sites with more difficult measurement conditions (e.g. hillier terrain) where a larger proportion of turbulent flux data may be compromised, it may be possible to develop a correction – or at least gain insight into the prevailing nocturnal dynamics – based on the behaviour of the storage term. We didn't emphasise this as our motivation, and will amend the text to do so (as well as noting that the additional instrumentation noted above would make such an endeavour less speculative).

Reviewer comment (Page 12 line 10):

(" . . . and applying the uR*R correction,") Which u* correction - the one you developed using the complete profile or a new u* correction based on only the point calculated storage?

Response:

This was not made at all clear. The analysis used u* estimates derived from the sum of the point storage estimate and the turbulent flux. This will be made clear in the amended text.

Reviewer comment (Page 12 line 12-14):

("We expect . . . a decline of corresponding magnitude in storage") It seems as though this effect should not have an effect on storage. If within canopy, stored, CO_2 is ventilated in the morning then surely the above canopy CO_2 must see an increase - which would be represented as increased storage in the morning.

Response:

We are not sure how to interpret this criticism. We still expect a bias towards efflux

because the point-based estimate of Sc is smaller in the morning than the 'true' (i.e. profile-based) estimate of Sc. That being the case, it doesn't properly compensate for the effect of the nocturnally respired CO_2 that is either released or consumed by photosynthesis in the morning. So we may not have double counting, but perhaps 1.5 counting! In any case, the point is that we don't consistently see this because the point-based estimates are so erroneous that they seriously affect the parameter optimisation and subsequent model estimates in unpredictable ways.

Reviewer comment (Page 15 line 14- 19):

This is a very useful point to make. This point alone justifies the need for storage measurements.

Response:

We also thought this was important, and to some extent novel. However, another reviewer suggested removing the figure upon which this part of the analysis ultimately rests (Figure 13)!

Reviewer comment (Page 16 line 14- 15):

(" . . . the uncertainty resulted in an increase in the potential uptake of carbon") How can uncertainty result in an increase in uptake? Or do you mean the lower estimate of u*th resulted in increased uptake?

Response:

The latter interpretation is correct. The phrasing in this section was poor, and will be amended to improve clarity.

âĂČ Reviewer comment (Page 16 line 15-19):

From this section I assume that you are implying that true NEE will fall within the uncertainty of Fc + Sc while true NEE will not fall within the uncertainty of Fc alone (because Fc does not fall within the uncertainty of Fc + Sc). What is your justification for believing that the true NEE value will fall within the uncertainty estimate for Fc + Sc?

Response:

Some of the phrasing in this section is poor as noted above. We intended to argue that calculating and quoting uncertainties in the absence of storage measurements is a meaningless exercise, because the best estimate for NEE calculated from turbulent AND storage fluxes is not within this range, and there is not even any overlap in the uncertainties. We also tried to convey that our uncertainties were not definitive (i.e. we cannot know that the true NEE value lies within our uncertainty range), with the following: 'It should be emphasised that there are numerous sources of uncertainty that have not been quantified here. Perhaps most important of these is systematic errors in the measurements themselves, which may be an extremely important source of true uncertainty (Lasslop et al., 2008). Thus the uncertainties reported here for FRcR + SRcR also should not be formally interpreted as total uncertainty in the true source / sink term, but as the uncertainty contributed by a subset of quantifiable errors.' If this is inadequate we can expand the qualifications if the reviewer has further suggestions.

Reviewer comment (Page 16 line 31):

Can you explain why using u* to remove observational data will reduce random error?

Response:

Since every observational datum contains random error, the more observational data is removed by filtering, the more random error is reduced.

Reviewer comment (Page 16 line 31 and page 17 line 3):

On the first line you indicate that model error should be larger at night and on the second line you indicate that is larger during the day – which is it?

Response:

Poor wording again. The first statement is meant to explain that the nocturnal model

error is large relative to the nocturnal random error because much of the observational data is filtered out by the u* filter at night. The second statement is meant to say that the daytime model error is comparable to the nocturnal model error because although there is less missing data during the day, random nocturnal error affects the parameter estimation for ER, which then propagates to the day because these parameter estimates are used in the NEE calculation. This will be clarified in plain English. Also, we have realised that a table that should contain the random and model uncertainty estimates was left out of the manuscript. This will be reinserted.

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