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Hydrology and Earth System Sciences Discussions

Interactive comment on "A framework for upscaling short-term process-level understanding to longer time scales" *by* W. H. Lim and M. L. Roderick

Anonymous Referee #1

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The article suggests a very promising title. Upscaling and the sensitivity of the coarse scale mean to small-scale heterogeneity are long standing problems, and are of particular interest in hydrology. However, I do not think it lives up to the promise. Mostly because it ends up with a "null" result: that small scale heterogeneity creates a considerable bias which cannot be fixed. I was very disappointed that the authors didn't push their own derivations and results one step further to actually quantify the effectiveness of a potential empirical scaling method that they propose, but instead, declared that these could not be used.

General comments:

The paper spends a long time on explaining the basics of sub-grid-scale covariance. In this work the “grid” is not physical in space but has a discrete resolution in time, but space and time are exchangeable, and the introduction and derivations are the same general principals used in every eddy-covariance analysis of flux measurements, or in the sub-grid-scale parameterization of every meteorological model. As one classic example, “k” theory relates the covariance of the velocity components in scales smaller than the atmospheric boundary layer to k(dU/dz) (i.e. proportional to the vertical gradient of horizontal wind speed). I expected something very similar here, some theory (empirical or derived) that will relate the covariance terms (further derived here into the correction terms, chi). Some hints of it showed through particularly in figure 5 (that very disappointingly is wrongly referenced in the text and not discussed at all) and figure 7, but instead of using these important relationships to progress (or at least test) a generalized model, they are being quickly dismissed (or ignored, as was the case with figure 5).

As stated in Section 6.4 “if short-term data was available” (I believe it meant high-frequency data). However, if high frequency data would exist, one could directly integrate it at the high frequency to avoid the up-scaling bias. However, in most cases, such data does not exist, and that is the entire reason for this manuscript. Nonetheless, after determining which covariance terms are important and which are negligible, figure 5 and 7 show that is would be possible to parameterize the important covariance terms if only high frequency data existed for short (and hopefully representative) periods or in other (hopefully similar) sites. Using such empirical parameterization, U2 and h can be used to estimate the covariance terms (i.e. the correction factors) with at least some degree of accuracy. I was hoping that now will come the step of trying to estimate this accuracy given the most accurate possible parameterization (using all the high frequency data for the entire period, but reduced through the empirical functions presented in figures 5,7) and trying to establish the relationship between the length of

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time for which we know the high frequency data and the accuracy of the parameterized prediction over the entire period (e.g. would 10 days of high frequency be enough? How much more accurate would be predictions made on 100 days of high frequency data than 10 days?). Unfortunately, this step did not happen and instead, the rather trivial and disappointing conclusion was reached in P6214L10-14. In my opinion, this missed the point for which this paper was written (at least as far as the claims in the title and introduction go, and as far as I have wished to see).

Specific comments:

P6204 - I think the proper term is “up-scaling” and not “scaling up”.

P6206L5-10 “Bias” is a very general term. I think it’ll be better to call this “scale bias” or “up-scaling bias”.

Eq.2. fv(tao) (meaning fv as a function of tau) is a bit confusing because later on (eq. 6) you use fv(es(Ta) – ea(Ta)) but there fv is not a function of (es(Ta) – ea(Ta)) but multiplying it. I suggest putting the (tao) in superscript. Same for E(tao).

Eq.8. It would be more elegant to simplify the equation by pulling the 3 terms for es(Ts)/(es(Ts)-ea(Ta)) together, and the same for the 3 terms for -ea(Ta)/(es(Ts)-ea(Ta)).

Section 5 – Data – this section does not provide enough information to be able to understand and replicate what was done in the study. What instruments where used to measure exactly what variables? According to what formulation where derived variables such as deltaZ, es(Ts) and Dv calculated? Was there any quality control in the data processing? Gap-handling? These are critically important as they can strongly bias the covariance between observed variables.

P6210L10-17 - The coefficient of variation of 1/Ta is much larger at smaller time scales. For example, direct measurements of evaporation (latent heat flux) using the eddy-covariance method calculate the covariance between Ta and es(Ta) from measure-

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ments in 10 Hz frequency and average the covariances (fluxes) over 30-minute periods. A resolution of 30 minutes is just not suitable for this measurement (temperature-humidity covariance). It would have been great (though I suspect impossible from the data that is described here) if the analysis was expanded to the turbulence-eddy time scales (where the physical processes that are responsible for evaporation occur) and not stop at 30 minutes. I am sure that data sets of evaporation pans next to ultra-sonic anemometers and high frequency gas analyzers for water vapor exist (try looking in the ameriflx and fluxnet datasets if you do not have access to such data in your site).

Fig 5 and 7 – at what resolution where U2 and h calculated? Are these the daily averages for the daily fv(tao) (fig 5) and error (fig 7) and monthly averages for the monthly fv(tao)? Or am I missing something? Please explain.

Fig 5. Is fv(tao) in fig 5 calculated using equation 12, or another general empirical function?

Fig. 5. Typically, scaling such as the one you show in fig 5 are done based on U*, the frictional velocity, which given the roughness length can be related to U at any given height.

Appendix A is not needed, the derivation described there is well known.

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