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Geoscientific Model Development Discussions

Interactive comment on "Dynamic upscaling of decomposition kinetics for carbon cycling models" *by* Arjun Chakrawal et al.

Anonymous Referee #3

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As someone who has been thinking and working on biogeochemistry scaling for quite a while, I applaud the authors' willing to attack the problem of spatial upscaling. The authors applied a well-established theory to analyze how high-order microstructure would affect the emergent organic matter decomposition dynamics in soil under three assumed mathematical formulations. By analyzing several synthetic cases, they concluded that for non-linear formulations, the spatial mean equations generally fail to capture the true dynamics.

While the conclusions they drew are solid within their model configuration, I too share with others the concern that how this learned lesson could be translated into something universally applicable for other modelers. In particular, we in the soil biogeochemical modeling community have so far no unanimously accepted governing equation to solve

like that exist for geophysical fluid dynamics, or hydrodynamics in general, where resolving the microstructure effects can be achieved through the so called large-eddy simulation and sub-grid closure, and even field or laboratory experiments can be designed to derive parameterization schemes that are generally applicable for different situations. Personally, I am therefore wondering can the authors' approach become some tools that are easily accessible to others, e.g., like Markov chain Monte Carlo codes that are widely accessible through opensource software?

Second, I am a little bit disappointed that authors decided to ignore the interactions between different micro-grids. In physics, the successful upscaling is achieved only through the consideration of interactions. For instance, the scaling of Newton's law of momentum conservation, the derivation of center of gravity, or the scaling relationship between the Chapman-Enskog theory, lattice Boltzmann approach and the Nevier-Stokes equation, are all hinged on the interactions between their parts. Therefore, it is not surprising at all that the authors found that their mean-field-approximation deviated significantly from their so-called full model simulations. Further, from existing scaling theories in the literature, another key of success seems to maintain the essential invariants of the system when one transits from one scale to another, yet the Michaelis-Menten kinetics they use is a crude approximation and misses some important invariant that is included in its origin law of mass action (Tang and Riley, 2017), and is deemed to show the difference they found. In addition, there's no guarantee that the mean-field equation will possess the same form as the micro-scale equation. For this, a very good example can be found in geophysical fluids, where at different scales, their governing equations are different, e.g., Gill (Atmosphere-Ocean dynamics, 1982)). Another more relevant example on decomposition is in Wang and Allison (2019).

Third, I feel the authors have some misunderstanding about the mean-field theory and the meaning of well-mixed soil condition. In fact, the scaling problem we are facing here is very similar like the situation hydrologists encountered in upscaling the soil moisture and soil matric potential relationship in the 1970s-1980s. Using statistical theory, they were able to derive closed analytical relationships (e.g., Mualem, 1976) to inform important soil water retention curve formulations to be derived from empirical data (e.g., van Genuchten, 1980). Therefore, whenever moisture-pressure relationships are included in soil biogeochemical models, some microstructure is included in the so-called mean-field-theory based model (although I should admit that the authors did not consider soil moisture in this study). Or put this straightforwardly, mean field theory does not rule out the inclusion of microstructure, as was demonstrated in the recent upscaling study of substrate affinity parameter (Tang and Riley, 2019), and the study of turbulence (e.g., Takahashi, 2017). In the same vein, a well-mixed soil can also have microstructure, and be properly parameterized. In fact, the latter is what motivated the dual-porosity or the multiple-Rates Mass Transfer models, which have enjoyed many successful applications (e.g., Haggerty and Gorelick, 1995).

Other comments

I agree with Dr. Wutzler that there might be some problems with a few of their equations, e.g. Eqs (A1) and (A2), and the authors should double check their derivations.

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