

Interactive comment on “Mesoscale circulations over complex terrain in the Valencia coastal region, Spain, Part 2: linking CO₂ surface fluxes with observed concentrations” by G. Pérez-Landa et al.

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I have some comments on the paper.

Overall, the simulated CO₂ concentrations are shown to be far from reality, which is likely be caused by neglecting realistic anthropogenic emissions and boundary conditions for CO₂. However, the results are quite interesting, but it is more or less an idealized scenario, which I would recommend to make it clear in the title.

You use a Lagrangian model to simulate CO₂ transport. I am wondering, why this

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could not be simulated directly by RAMS. In addition, I have doubts, if you can use that approach for such a high-resolution model, where you partly resolve boundary layer convection directly.

You compute airborne H₂O and CO₂ fluxes from 3km legs. These legs are too short. Are you aware about the statistical and systematic error of the fluxes and mean quantities?

What are your boundary conditions for CO₂? I guess you do not consider any CO₂ transports apart from your Lagrangian model?

Your classification of land use types is very coarse. Your 'mosaic' type (which I would call 'mixed', since mosaic is a term used in subgrid averaging methods) is the dominating land use type, so you should separate at least forests and agricultural areas in this land use type.

Is this simple NEE parameterization really state of the art? It clearly must depend also on soil moisture. I understand that you have included this dependence in your tuning of coefficients, but there is not much to be learned for other studies from such highly tuned parameterizations.

You discuss the layered structure in CO₂ profiles. You should also see a similar structure for H₂O profiles, since water vapor is also a passive tracer in your situation (or are there clouds?).

Figures:

Fig.2: units are wrong

Fig.10: this is not CO₂ concentration, but the anomaly

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