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Comment

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

**Anonymous Referee #2**

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General Comments:

This research provides an interesting contribution to understanding links between complex terrain flows and transport of CO<sub>2</sub>. The combination of an empirically driven NEE model with current 3-D mesoscale atmospheric model capabilities illustrates some nice idealized (albeit fairly hypothetical) models of transport. The implications are rich for better understanding of the carbon budget. The paper is carefully organized and methods clearly and thoroughly described.

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My main concern with the study is that very little validation is presented and the visual comparison the reader is able to make shows there is clearly large uncertainty in the model performance with generally a great deal of detail being missed. The research still produces interesting results with important implications but conclusions need to be expressed in the context more directly of the large numbers of uncertainties. The results are based on simulations under idealized conditions with very limited temporal range, heavily tuned by measurements applied with a great detail of assumption about spatial variability and largely unvalidated. Both the title and general conclusions drawn should better represent this. Conclusions 1,2 and 3 are really conclusions drawn from the RAMS modeling (accompanying paper) and not explicitly the work conducted here.

NEE model:

Using nighttime respiration values for daytime will yield errors of around 100%. It would be more realistic to model based on physical variable like soil temperature which can still be obtained from (nocturnal) NEE observations.

Spatial variability of PAR is not considered in the spatial model. Even assuming no localized convective cloud, there would be significant variability as a function of topography (this could be applied via RAMS solar radiation fields).

The mosaic area (largest) is problematic. It is comprised of mixed landuse with likely large variability in CO<sub>2</sub> fluxes due both to this and complex terrain effects (e.g. on soil moisture). The simplicity of the spatial approach will yield large errors. At least the authors could use empirical evidence from a larger variety of land-use types under similar conditions and provide more detail. The NDVI map raises this question most clearly.

It is rather difficult to accept the justification that differences in location can be resolved by selecting different meteorological situations. Foehn winds have a unique control on surface energetics and biological interaction due to strong controls on VPD, turbulence characteristics and source area. That a Foehn wind blowing across coniferous forest

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at the coast is analogous to a sea breeze blowing across mixed vegetation inland is hard to swallow.

The validation of modeled CO<sub>2</sub> profiles using observations could be greatly strengthened and the reasons given for lack of comparison don't make sense to me (P2864 L23,24) since the timing is only different by a few minutes, the vertical profiles should be obtainable from anywhere in the model domain and magnitudes of signals are what you would want to compare. At the least, the end of Section 4 needs further comments on the discrepancies between modeled and observed profiles, with possible explanations related to unmodeled contributions (larger scale advection) and weaknesses in the model. The Appendix A attempt to quantify uncertainties is focused on only a few attributes. To give equal weighting to sources for error it should also include flux estimates (measurements), modeled transport (RAMS validation from other paper) and more realistic conclusions from the NDVI analysis (i.e. high variability not captured in the model).

The NEE model is often referred to as a 'simulation' model which I think misrepresents it, since it is really a spatial model based on simple land use classes and point observations. Thus you are not simulating but coarsely extrapolating.

Some comments on the validity of using CO as a proxy for CO<sub>2</sub> to derive diurnal variability would be useful -i.e. it is more acceptable if in fact dominant CO<sub>2</sub> sources also emit CO equivalently. Without knowing the relative sources for CO<sub>2</sub> in Valencia it is difficult to know how important this discrepancy would be.

Specific comments:

Title: doesn't truly reflect content. Rather little is made of linking surface fluxes with observed concentrations. Should reflect the idealized case study that the study produced.

Abstract L8: This sentence doesn't make sense, the measurements are not trans-

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P2859, L 14: need to define 'freely inspired'.

P2860 L16: Daytime respiration is included indirectly in Eq(1) since NEE is comprised of both photosynthesis and respiration.

P2863 L22: I can't make sense of point (1).

P2877 L14: I don't agree with this conclusion. I think it shows a lot of uncaptured variability and that the 'mosaic' class which makes up the largest area has the largest degree of variability.

Figures: Several were too small to read fonts. Assuming they will be expanded in the final draft, they are mostly fine. Some exceptions:

Fig 6: X-axis labels should go beneath the axis

Fig 7 and 10: need to distinguish between concentration and concentration anomaly

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