Atmos. Chem. Phys. Discuss., 6, S1909–S1919, 2006
www.atmos-chem-phys-discuss.net/6/S1909/2006/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Mesoscale circulations over complex terrain in the Valencia coastalregion, Spain, Part 2: linking CO_2 surface fluxes withobserved concentrations" *by* G. Pérez-Landa et al.

G. Pérez-Landa et al.

Received and published: 19 July 2006

article

We would like to thank the two anonymous referees and Dr. Heinemann for their reviews and helpful comments. The reviewers agree that the paper contributes to understanding the influence of complex terrain flows in CO2 variability. Their major concerns are about the empirically driven NEE model. In the following we address their questions.

Anonymous Referee #1:

1) I wonder how the plane has flown as only vertical profiles of concentration are used and the horizontal ones may provide additional key information about the models ability to generate the concentration fields.

CO2 data in the horizontal section exist, but was not utilized. We focused on the vertical profiles because their behavior is representative of the dynamics in the whole basin, and they are sensitive to diverse processes: drainage winds, development of thermal circulations, vertical confinement of the breeze cell and even the effects of orographic injection, as explained in the text.

2) The paper balances, between the use of CO2 as a tracer and the promise of linking surface fluxes to the regional concentration variability. I suggest to stick to the tracer issue and maybe even combine paper 1 and 2, as it their separation is rather artificial.

In the revised manuscript, the focus is clearly given to the use of CO2 as a tracer. This is because our NEE model is far too idealized to allow inferring regional fluxes from concentrations (see also remarks from Referee #2). Therefore, the model/data comparison is limited now to a qualitative analysis of the main processes.

3) The use of rectification is a little confusing. Maybe it is better to reserve this word strictly for the seasonal and diurnal effects as originally described by Denning et al. Rectification as used by the authors in this paper is almost synonymous with any type of heterogeneity.

The reviewer is correct and we followed his suggestion. The word ‘rectification’ is only applied for the particular covariance between diurnal vertical mixing and NEE (as defined by Denning et al., 1996), while for other heterogeneities in CO2 due to mesoscale processes, we use the term ‘covariance between flux and transport’.

4) I fail to understand the use of the rice respiration rate on page 2866 as the profiles show that most of the respiration flux comes from the Mosaic and Citrus. In fact the

ACPD

6, S1909–S1919, 2006

Interactive Comment

whole treatment of Mosaic is of course rather poor, but allowable because I still feel that the profiles give a right "sense" of direction.

The reviewer is correct because, at night, most of the influence is caused by Mosaic ('Mixed' hereafter, as has been changed in the revised version) land-use respiration. The back-of-the-envelope calculation was corrected accordingly to Mixed land-use respiration rates (Section 4.1).

5) I have some problems in following the switches between the various graphs in 4.1-4.3 and would recommend a slight more clearer and logical use.

The text has been simplified and shortened. Still, simultaneous reference to figures 6 and 7 in the same section is necessary (in our opinion) to describe the full CO2 variability patterns.

6) Some of the graphics captions in Fig 10 is hard to read.

We have corrected this.

Anonymous Referee #2:

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.*

The large uncertainties and the lack of dense validation data are stressed in the revised manuscript. The title has been changed accordingly. The conclusions now are focused on the use of the CO_2 model results and data.

NEE model:

1) 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.

We used an empirical NEE model (light response function) to simulate the daytime surface CO2 flux. Daytime respiration is not calculated explicitily in such an NEE model but, as pointed out by Reviewer #2: "daytime respiration is included indirectly in Eq(2) since NEE is comprised of both photosynthesis and respiration.". Therefore, the NEE daytime model does not make assumptions about the values of ecosystem respiration during daytime. It is likely that Reviewer #2 was confused by the inappropriate statement made on P2860 L16-17 of the original manuscript (corrected in the revised version).

2) 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 reviewer is correct. Despite the fact that there were no clouds during the episode, the presence of slopes adds spatial variability in the PAR. The RAMS short-wave radiation over a mountainous region every ten minutes shows a variability that can reach up to 30% between 05:00-07:10, less than 10% between 07:20-17:50 UTC and 30% in a few regions between 18:00-19:20 UTC. This variability, not considered in our model,

ACPD

6, S1909–S1919, 2006

could increase the NEE variability of the Mixed land-use class around dawn and sunset. The effect of PAR variability on the simulated CO2 vertical profiles is minor, given the small contribution of Mixed land use during the daytime (Fig. 7).

3) The mosaic area (largest) is problematic. It is comprised of mixed landuse with likely large variability in CO2 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.

The heterogeneous nature of the fluxes in Mixed land-use regions has been discussed as a contributing source of uncertainty in the model/data comparison.

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

Lacking flux tower measurements over the Mixed types (impossible to carry out in such complex terrain, anyway), we justify using El Saler because this forest is ecologically close to the vegetation assemblages found in mountain ranges of the region (Pine canopy and Mediterranean shrubland). Thus, to fit the NEE model parameters of the Mixed category, we used the El Saler eddy covariance NEE data from the 12 days presenting Foehn episodes between mid-June and mid-August 2001. For these 12 days, the daily maximum air temperature was 32.4 ^{o}C ($\pm$ 2.0 ^{o}C) and the daily minimum air relative humidity was 30.3 % ($\pm$ 3.9 %). Such conditions are quite similar to the ones encountered over the Mixed area on 1st and 2nd July 2001 (Figure 3a of Part 1 paper).

ACPD

6, S1909–S1919, 2006

The text has been modified in the corrected version to clarify this point.

5) The validation of modeled CO2 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.

In the validation discussion, the model/data discrepancies are more clearly stated. Effects of NEE uncertainties are mentioned and estimated. We meant in the criticized sentence (p2864) that although the model may present realistic processes, it does not necessarily present the same "timing" as the observations (e.g., the layer that is generated afterwards is actually observed). We did not mean that the model's timing mismatch creates a large CO2 mismatch. The end of section 4 has been rewritten.

6) 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).

Source of error for NEE idealized model are now listed in the text (see end of section 3.2). The sensitivity test (Appendix A2) account for both error associated to eddy covariance measurements and potential bias due to mismatch between flux measurements footprint and sources regions. The effect of high spatial variability within source regions are now accounted for in section 4 and stressed in the appendix A3. Additionally, a short section on the modeled transport error was added to the appendix (A4).

ACPD

6, S1909–S1919, 2006

Interactive Comment

7) 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.

This is true; the NEE 'model' is now called 'idealized model' and the coarse nature of the extrapolation highlighted within the text.

8) Some comments on the validity of using CO as a proxy for CO2 to derive diurnal variability would be useful -i.e. it is more acceptable if in fact dominant CO2 sources also emit CO equivalently. Without knowing the relative sources for CO2 in Valencia it is difficult to know how important this discrepancy would be.

We have information from local authorities about the main emission factors in the region (this dataset will be published next October in http://www.cma.gva.es). According to this data, car traffic is the dominant CO2 source in the region, which justifies the use of CO. This has been clarified in the text.

Specific comments:

1) 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.

The title has been changed to "Mesoscale circulations over complex terrain in the Valencia coastal region, Spain. Part 2. Modeling the CO2 transport using idealized surface fluxes".

2) Abstract L8: This sentence doesn't make sense, the measurements are not transported

The sentence has been corrected.

ACPD

6, S1909–S1919, 2006

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

This term was suppressed.

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

We fully agree with the reviewer. P2860 was an uncorrected statement and has been removed.

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

This has been clarified in the text. Because of computing limitations, a particle packet could not be emitted in each grid cell at each time step in a source region. So, at each time step a particle packet is emitted randomly within each source region.

6) 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.

This is true, but our discussion was focused on the different mean NDVI values between Mixed regions, rather than the variability within regions. A sentence highlighting the large uncaptured variability was added (second paragraph of section 4).

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

We have changed Figs. 6, 7 and 10.

ACPD

6, S1909–S1919, 2006

G. Heinemann:

1) Overall, the simulated CO_2 concentrations are shown to be far from reality, which is likely be caused by neglecting realistic anthropogenic emissions and boundary conditions for CO_2. 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.

The title has been changed to "Mesoscale circulations over complex terrain in the Valencia coastal region, Spain. Part 2. Modeling the CO_2 transport using idealized surface fluxes"

2) You use a Lagrangian model to simulate CO_2 transport. I am wondering, why this 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.

CO_2 transport could have been simulated directly by RAMS using an Eulerian approach. However, we selected a Lagrangian approach given the complexity of the dynamical processes involved. This has allowed us to determine the time-since-release of the different contributions to the simulated vertical profiles shown in Fig. 7, identifying the differences in the regional transport of CO_2.

Previous studies in the region have shown that the use of grid lengths close to 1 km in mesoscale models coupled to Lagrangian models is a valid modeling approach for simulating dispersion in the region (Palau et al. 2005a, Palau et al. 2005b). These studies, besides validating the meteorological fields, also compared the modeling of power plant plumes dispersion with the measurements taken aloft during several field campaigns. Other studies in complex terrain regions have used similar approaches to reproduce effects like diurnal cycles, wind convergence, recirculations or orographic injections (Zhong and Fast 2003; de Wekker et al 2005; de Foy et al. 2006). It is clear that a coarser grid could be in better agreement with the assumptions that typical

PBL parameterizations consider. However, under the conditions showed in our study, solving the adequate scales is essential for a proper simulation of the dispersion of any tracer (Palau et al. 2005b).

3) 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?

Calculations of the fluxes performed during the campaign are described in Gioli et al. (2004). In this paper is explained how the proper averaging length depends on the flight altitude, surface roughness and atmospheric stability, and how it can be calculated by means of the cumulative integral of the cospectrum, measured following Desjardins et al. (1989). In any case, we do not use aircraft flux data in the simulation.

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

Our study is limited to the influence of the local signal on the observed CO2 variability. We have stated this limitation more clearly in the revised text.

5) 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.

Limitations in the NEE idealized model are more clearly stated in the revised version. We agree with the referee that "Mosaic" can be a confusing term and that "mixed" is more adequate to represent miscellaneous types of land use. We have changed this in the text. The heterogeneity of this land use class over the mountainous regions, with the presence of both forest and agricultural areas in few kilometers, strongly difficults the separation of both land uses.

ACPD

6, S1909–S1919, 2006

ACPD

6, S1909–S1919, 2006

Interactive Comment

6) 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.

The main objective of this work is to study the influence of the coupling between atmospheric transport and surface CO_2 fluxes in the observed variability of CO_2. Obviously, our NEE model is very coarse; however, it has been useful to identify processes that can help to explain the observed variability.

We also think that some aspects of our work could be relevant for other studies. For example, we have shown that the diurnal wind cycle, including the recirculations, orographic injections or vertical confinement of the breeze cell, is essential for explaining the vertical CO_2 pattern. Modeling approaches applied in complex terrain regions to study CO_2 have often not shown high enough resolution to address these processes.

7) You discuss the layered structure in CO_2 profiles. You should also see a similar structure for H_2O profiles, since water vapor is also a passive tracer in your situation (or are there clouds?).

As the referee suggests, water vapor is a passive tracer in our situation due to the absence of clouds. It is true that if we have important variations in CO_2, we should also see variations in water vapor. In fact, vertical profiles of water vapor (Fig. 6 d) show an important layering, as a consequence of the stable conditions and the different histories of each air parcel. Anticorrelation between CO_2 and H_2O should be expected if both have the same sources. However, the presence of moisture is not only due to the vegetation activity, as the sea also acts as a source of moisture for the profiles.