Atmos. Chem. Phys. Discuss., 5, S4421–S4423, 2005
www.atmos-chem-phys.org/acpd/5/S4421/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Distinct wind convergence patterns due to thermal and momentum forcing of the low level jet into the Mexico City basin" *by* B. de Foy et al.

Anonymous Referee #2

Received and published: 13 December 2005

In their paper, de Foy et al. conduct an investigation to obtain further insight on the meteorological conditions, an in particular wind patterns, that create distinct air quality conditions in the Mexico City basin. The paper is in general well written, easy to follow and has a thorough description of the authors' investigation. The issue arise by the authors is relevant and well described. I recommend the paper for final publication, after some issues are addressed.

General comments:

p.11056-11059 I found the introduction section to be comprehensive, though in parts

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

EGU

it lacks of consistency and tends to be somewhat awkward. References to past studies are presented (e.g., VTMX, MAP, Kossmann and Fiedler [2000], Kossmann el al. [2002], Regmi et al., [2003], Kimura and Kuwagata [1993]); though it is not evident how the ideas developed under those studies are related to the main discussion of the paper. Moreover, transition from the description of these past studies to the conditions in Mexico City is abrupt and awkward.

p.1060, 1062 The authors comment that the model performance was poorest for the Cold Surge episodes, and argue that a possible source of error could be the soil moisture field. Given that the simulations were 4 days long, would this provide enough time to get a degrading result solely due to soil moisture? Could there be other sources of error that merit discussion (e.g., cloud parameterization)? Did the previous simulations conducted by the same authors (i.e., de Foy et al [2005a]) gave better results? If so, why not use these previous simulations?

p.11062 I found Fig. 6 to be too complicated to follow. Given that the authors are trying to compare the main features of the trend of wind speed and direction, would it be enough to show simply that instead of all the lines that tend to confuse rather than clarify, i.e. drop the inter-quartile lines and just keep the bold lines? In contrast, the comparison between the simulations and observations discussed latter in the same paragraph could be clarified by means of a graphical representation.

p.11063-11064 The authors present estimates on the “thickness” of the low level jet. Was there a systematic way to estimate this parameter, or was it done visually. If the latter is true, caution should be taken on expressing this results and the method used to derive these numbers should be made explicit.

p.11065-11066 Comparison is made between convergence lines derived from model data and cloud transport from satellite imagery. Was the model capable of reconstructing the cloud formations observed?

p.11067 Jet strength was estimated though a surface wind velocity surrogate (an excess meridional wind speed). Given that only ground-level observations are used in this calculation, how do the authors guarantee that what they are seeing is really a meridional flow due to jet flow and not due to other phenomena? Moreover, the choice of a 1 m/s threshold appears to be quite arbitrary. Was there any reason for this choice? How sensitive are the results to this choice? The text indicates that data from “the 5 SMN stations” was used. Fig. 2 presents 7 SMN stations, which stations where used? If CATE station was used, it lies well beyond the basin invalidating the use of this station.

p.11067-11068 I am puzzled by the relevance of paragraph starting at line 25 (p. 11067). It seems to be disconnected from the rest of the discussion.

p. 11068-11069 Discussion on thermal forcing is too cursory to add much to the paper. From what is presented, even preliminary conclusions with regard to the impact on jet strength cannot be well supported. There exists lack of observational data to derive clear conclusions, and the poor or null correlations observed do not shed light on the possible causes of the results obtained. If additional data is not available for this part of the discussion, I suggest dropping this section and the corresponding statement in the abstract and the title, entirely.

p. 11069 Is it expected that only for O3-South days there is a strong correlation between wind direction at the top of the boundary layer and jet strength? Why is it that it is not the same for other days? Is there a physical explanation?

Minor comments:

Fig. 5 I had to go back a couple of times to this figure to try to decipher its content. The authors could look at a simpler way to present the same results.

p. 11064, last paragraph Figures 9 and 10 are not mentioned, only Fig. 11, even though the discussion is in regards to all of them. Same problem in p. 11065, last line.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 11055, 2005.