

Interactive comment on “Mexico City basin wind circulation during the MCMA-2003 field campaign” by B. de Foy et al.

Anonymous Referee #2

Received and published: 2 May 2005

Review of “Mexico City basin wind circulation during the MCMA-2003 field campaign” by B. de Foy et al.

As the title suggests, the paper describes wind circulations derived from the available operational surface measurements and rawinsonde profiler in Mexico during April 2003 when an air chemistry field campaign took place in Mexico City. In addition to finding circulations that have been reported previously, the authors perform a systematic analysis of the data and obtained three general categories of wind circulations that affect where the highest ozone is expected to occur in the valley. One purpose of the manuscript is to provide background meteorological information that can be used by future analyses of the air chemistry data collected during the field campaign.

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General Comments:

The paper builds upon previous studies on the circulations in the Mexico City basin by employing a longer period of measurements rather than episodes. While the paper's classification of the basin circulations represents a new and useful work, the paper reads more like a report than a scientific paper. Many of the details will be useful for only a small audience of scientists involved in the MCMA 2003 field campaign; however, that is one of the purposes of the manuscript. The length of the paper could be shortened by eliminating material that is repetitive and reorganizing the text. For example, section 6 seems to repeat a lot of material that has been covered before. In section 4, the authors first present data at the CENICA site to define the 3 categories along with the basin peak ozone locations. Then in section 5, surface observations over all of Mexico are described. Section 6 moves back to the local scale with the basin winds that are followed by upper-air winds in section 7. Finally, in section 8 the association of the local diurnal wind and ozone evolution are presented. The authors should consider reorganizing the text to local to larger scale (section 4, 8, 6, 5 and 7).

Introduction, paragraph 3: The authors state that the complexity of the meteorology in the MCMA "cannot be simplified to one individual factor". This is certainly true, but seems to contradict the main method of the paper in dividing the circulation/ozone patterns into only 3 categories. I am not sure it is that simple. The paper is useful in that it provides the meteorological analyses for subsequent studies. Some investigators, however, may wish to focus on short episodes that may not fit nicely into the classifications schemes presented in this paper. Section 10, it would be useful to comment on ozone patterns or winds that do not seem to fit the 3 categories. There is probably more of a continuum of effects than suggested by the authors.

Specific Comments:

Section 1, paragraph 1: The authors describe the topography in the vicinity of Mexico City; however, they do not state why the topography is important (as alluded to in line

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11). It would be useful for a broader range of readers to indicate how the topography affects the near-surface winds and boundary layer that will also affect local air quality.

Section 1, paragraph 3: Change “the effect of the urban heat island which increases the drainage flows \check{E} ” to “the effect of the urban heat island which may increase the drainage flows \check{E} ”. Unless the authors can refer to a particular study, the impact of the urban heat island on the local circulations is just speculation at this point.

Section 1, paragraph 6: I do not recall that the Whiteman et al (2000) paper found the absence of nocturnal inversions was due to urban effects. The authors should check this.

Section 2, paragraph 1: The authors list the 3 main meteorological questions for the field campaign. Question three on the evaluation of the mesoscale model performance is an end to itself. The importance and relevance of this statement needs to be elaborated for the average reader.

Section 3, paragraph 1: The paper employs data from operational surface sites and no meteorological data collected from the field campaign. It is unfortunate the surface data could not be coupled to the special upper-air data at this time. This will have to wait for a future study and should be pointed out again in the conclusion section. The present study only gets one step closer to the “three main meteorology questions” of MCMA-2003.

Section 4, paragraph 6: The second sentence is contradictory. How can the winds have a preferred direction from the northeast and south, but show no clear pattern?

Section 4, end: While the surface meteorology seems to be divided into three categories, Figure 18 seems to indicate a lot of variability in ozone patterns within a category. Please comment.

Figure 18: This plot is based on an interpolation of surface ozone stations. The contours erroneously lead the reader to believe that there is a smoothly varying ozone

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pattern in the valley, when in fact, variations in emissions likely lead to large horizontal gradients in ozone concentrations. I strongly suggest changing the figure to plot discrete ozone peak values as colored circles, using a color scale for the ozone concentrations. Do peak ozone values occur at significantly different hours of the day among the stations?

Section 5, paragraphs 3 and 4: It would be useful to refer to Bossert (1997) here since these paragraphs describe the flow patterns in his paper.

Figures 10 and 11: It would be useful to overlay these two figures, and omit the surface ocean winds far from the central Mexico (so that winds of Fig. 11 are still readable).

Section 5, last sentence: The authors frequently refer to many local names that are hard to keep track of. In this instance (and perhaps other in the paper), the authors should refer to a figure that contains these place names. If these place names are not on a map, the text should be modified.

Section 7, line 7: Change “robust feature to “common feature”?

Figure 11 caption: Change “Ěshowing its displacement Ě” to “Ěshowing its estimated displacement Ě”

Section 7, paragraph 2: The operational soundings usually report that data using a coarse vertical resolution that introduces relatively large uncertainties in the estimates of mixed layer depth (up to a few hundred meters). Please comment in this paragraph.

Section 7, paragraph 4: Be more specific than “in the middle of the height range” in the third line. I found this paragraph confusing. Please explain what is meant by the jet and the altitude of the jet? What processes form the jet?

Section 9, line 4: The authors state that on O3-south days there is weak forcing and thermal flows. I assume they mean synoptic forcing. But are the thermal flows responsible for transporting ozone to the south? The sentence seems contradictory.

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Section 9, line 11. What are the authors referring to as “slope winds in the basin”? Slope flows would be expected to occur along the steeper slopes of the valley sidewalls (where there are no observations). Most of the surface stations would be “valley” or “basin” winds with modifications that might indicate the presence of slope flows along the valley sidewalls. The terms for the types of winds are being used quite loosely here.

Section 9, line 15: Ozone formation depends on photochemistry, so there must be some sunshine to account for the ozone production? In other words, there are clouds but not too many clouds? Where there clouds in some parts of the valley and not in others?

Section 9, paragraph 2, line 10: Suggest changing the sentence to “O3-North days have weaker near-surface upslope flows during the day due to the stronger westerly components of the wind aloft”. Please be more specific on the elevation of the winds that are being referred to.

Section 9, paragraph 4: The authors state that compared to the IMADA campaign, the flow patterns during MCMA-2003 show gap flows that were clearly separated into 2 categories. Has anyone done a similar analysis for 1997 to indicate that the gap winds fell into different categories? If so, include a reference. If not, please rephrase the sentence. Later in the paragraph the authors indicate the gap wind showed an association the ambient wind direction; however, they have no temperature gradient inside and outside the valley to compute the thermal forcing mechanism described in Doran and Zhong (2000). This comment also affects a similar statement in the conclusion section. Note that “association” is probably a better word than “predictor”

Section 11: Some comment is needed on how applicable the 3 categories for April 2003 for other years and other times of the year.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 2503, 2005.

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