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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 #1**

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In general I found this paper to be interesting, informative, and useful. Building on earlier work by the same principal author as well as on previous modeling and analysis studies by others following the IMADA campaign in the late 1990s, it does a nice job of relating synoptic conditions and local convergence patterns to ozone distribution in the Mexico City basin. I have a number of minor points that I believe need some attention and one more significant comment that should be addressed. These are described below.

Major comment:

The discussion on "jet strength" needs to be modified. Jets are usually understood as relatively narrow regions of stronger winds with weaker winds above and below. The jet strength then says something about the maximum velocity and perhaps the velocity gradients above or below the peak. For winds through a mountain pass one might extend that definition to include lateral jets, i.e., a core of higher winds near the pass with decreasing winds off to the sides. This latter type seems to be the kind of jet that the authors are considering but they should make that clearer. They refer, instead, to a "low level jet", but, as discussed below, that may not be what they are actually looking at.

Although I understand that no profiler data were available from the Chalco area for the 2003 experiment, which thus required some alternate measure of "jet strength", I am uncomfortable with the choice made in this paper. The authors are using an "excess meridional wind speed" averaged over five stations as an indicator of jet strength. This seems problematic for several reasons. First, the authors have not shown a correlation between the strength of these excess winds and the strength of the winds flowing through the gap near Chalco. Presumably there is some relationship but it would be nice to know how strong it is. Even without observations of the winds near the gap, the model should be able to give some idea of the connection. Second, the excess southerly component of the wind should depend not only on the strength of the wind coming through the gap at Chalco but also on the strength of any flows that may be coming in from the north. The resulting southerly flow is another reflection of the convergence patterns that the authors discuss but it does not seem to be a direct measure of the "jet strength". Third, it is reasonable to assume that under some conditions the synoptic winds can enhance or weaken this "excess" component without channeling everything through the gap but it is unclear how the authors would separate out the various contributions to the overall flow. Perhaps they could integrate the flow through the gap and try to estimate how much of the excess meridional flow actually arises from that.

The authors point out that their results differ from those of Doran and Zhong, who found that stronger thermal contrasts between the basin and the valley to the south tended to result in stronger jets. The comparison is misleading because not only are the measures of the jets different but even their definitions are different. The current paper uses a surrogate measure of jet strength whereas the Doran-Zhong used measurements from a wind profiler in the Chalco area to characterize the strength of the jet. The D-Z paper looked at jets characterized by a peak in the vertical profile of the wind speeds; the current paper is a more general analysis of winds through a mountain gap and those winds may or may not have an elevated maximum in the vertical. The D-Z paper made some effort to eliminate cases when strong southerly flows masked the jet-like nature of the wind profile or northerly winds interfered with its formation, but no such distinction is made in the current paper. In fact, flows through a gap do not necessarily mean that "thermal imbalance" is responsible. For example, for strong southerly synoptic flows, winds would be channeled through the Chalco gap, which would then result in a lateral jet regardless of the thermal balance between the basin and the valley to the south. The D-Z analysis tried to exclude such cases in an effort to isolate the importance of the thermal driving mechanism. Perhaps it is the inclusion of such cases in the present paper that lead the authors to say that thermal imbalance between the basin and the valley to the south is the main mechanism driving the jets but then to say it's "a poor predictor of jet strength." Those two statements seem somewhat inconsistent.

I believe it is important to clarify these differences. I also suggest that the authors consider using some terminology other than "jet strength" to describe the quantity they are examining because they are looking at a more generic feature of the flow patterns and not just "jets". This would make their discussion clearer and would help differentiate the flow features considered in this paper from those treated in the D-Z paper. The other analyses and conclusions in this paper should not be affected.

Minor comments:

p.11057 The Salt Lake City campaign took place in October 2000, not 2002. p. 11058 Although one can piece this together from the descriptions of the Whiteman et al. paper and the Doran and Zhong papers, for those not familiar with the Mexico City region it would be useful to note explicitly, early in the paper, that the "jet" the authors will discuss is one that enters the basin from the south in the region around Chalco. p. 11059 Define AGRMET and AMSR-E. p. 1102 The description of the way in which the data are plotted in Figure 5 is not terribly clear and could be improved. Even though there is a reference to a previous paper, some more explanation here would be helpful. p. 11063 - In the last sentence before Section 5, the antecedent of "this" in the last clause of the sentence is unclear. p. 11064. I assume the comment about the jet being 2000 m "thick" refers to the total depth of the jet. It seems odd to refer to this as a 'low level jet". It also seems quite deep compared to any of the profiler observations in the paper by Doran and Zhong. p. 11064 "1300 m above ground level" is described as being "either above or towards the top of the surface layer." What do the authors mean by the "surface layer"? It is not the mixed layer depth and it is much too deep to be a constant flux layer. p. 11064 Here and on the following page are references to Figure 11. The information is actually contained in three figures, numbers 9-11. p. 11065 There are several comments made about cloud formation as evidence for convergence zones. Did such clouds form in the simulations? It would be useful to show them to provide further assurance that the model is behaving reliably. If such clouds did not form, why not? p. 11069 I found the correlation of "jet strength" with wind direction somewhat peculiar. The statement that "the linear fit explains 84% of the variation" for the O3-South days is also odd. Surely there must be some physical mechanism that accounts for this relationship. Is it simply the channeling of winds aloft along the valley, either from the south or from the north? Wouldn't one then expect a correlation with the component of the wind velocity along this direction? That seems to be more along the lines of an explanation than simply showing a correlation with wind direction. "Explaining" flows near the surface solely in terms of wind directions aloft, rather than wind velocities, is hard to understand. p. 11082 The legend in Figure 6 has entries for South, North, and Cloud, which appears to be a holdover from a previous paper in which the cold surges were associated with cloudy, wet weather. I believe the "Cloud" should be replace by "Cold" or something similar.

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5, S4367–S4371, 2005

Interactive Comment