REPLY to reviews of the draft manuscript:
"Is MM5 good enough for air-quality models?"


As this contested paper will not be revised or resubmitted, and as readers who have gotten this far have already formed independent judgments about it, of one flavor or another, a detailed rebuttal of the critical reviews would seem tendentious, at best. Some general comments may still be useful, however.

Clearly, the tone of the manuscript offended both reviewers. The title was rhetorical, as is always a risk.

Before and since submitting this paper I have circulated it among several colleagues in both air-quality and meteorological communities. Without exception the former group replied in variations of "60° rms directional errors are certainly not acceptable", and "Yes, we suspected as much." Without exception the latter replied with variations of the comments by the two anonymous reviewers cited in: <http://www.atmos.washington.edu/~harrison/reports/reviews.pdf>

Both sets of reviewers, however, failed to discuss what I conceive to be the more interesting of the yet-to-be well answered questions:

    Why do the MM5 winds differ so greatly from both the rawin- and surface observations, when the winds are light, at all altitudes, and which are more credible?

As conjectures, only, the large discrepancies revealed in this report may plausibly result primarily from timing errors in the MM5 that in turn may be sensitive to problems with initializing. The objection that both surface- and rawin- measurements pick up short-period variances that the MM5 cannot resolve is both correct and not relevant, for the reasons I discuss in the text.

Your comments are welcome.

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