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ADDITIONAL DISCUSSION

SECOND RESPONSE TO COMMENTS ON "A SYNOPTIC CLIMATOLOGICAL ANALYSIS OF AIR QUALITY IN THE GRAND CANYON NATIONAL PARK"

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In this response, we describe the value of our synoptic classification scheme, and try to clarify aspects of this approach that are misinterpreted in the Davis and Gay (1994) response.

One value of using a surface pressure analysis, compared to more sparse upper air observations, is that the higher spatial density ground level observations available at most airports and other locations in the southwest U.S.A. can be used to assess the lower-level meteorology. Moreover, in comparison to surface winds, which are often dominated by local topography, the *surface pressure field* is representative of a larger geographic area which is why the National Weather Service routinely contours pressure, while no such contour analysis is completed for surface winds.

The reduction of pressure to a sea level height is a concern, and we agree with Davis and Gay (1994) on this point. We have introduced a new analysis procedure to remedy this shortcoming (Pielke and Cram, 1987; Cram and Pielke, 1989). Nonetheless, while our new pressure analysis approach will provide improved quantitative estimates of the pressure field, the *direction* of the geostrophic wind and its relation to the major synoptic weather features is generally not significantly changed from the standard National Weather Service pressure analyses.

The pressure field is known to force the low-level winds. We do not, however, claim that the actual surface winds correspond to the geostrophic surface wind since frictional effects and terrain, as indicated above, will alter the actual flow from a geostrophic flow. Nonetheless, larger scale transport tends toward the geostrophic flow direction, as modified by surface drag. The geostrophic wind, of course, is defined by the horizontal pressure gradient, and therefore is a measure of the large scale transport direction.

Davis and Gay misinterpret our use of the term "polar high". Perhaps if we applied the term "cold core high" the confusion would be eliminated. In any case, our definition is based on a thermodynamic definition in which the high diminishes in intensity with height because the lower tropospheric mean temperature (i.e. the thickness between pressure surfaces) is colder in the vicinity of the high than elsewhere. This does not require below-average or even average near-surface temperatures, as suggested by the Davis and Gay discussion of their Table 1. These cold-core highs can originate over the Arctic and propagate southward over the western United States; propagate off the east coast of Russia and across the north Pacific and into the United States; or as often happens in the case of the Great Basin, develop in place. During WHITEX a cold-core (as opposed to cold surface temperature) high pressure system propagated into the Great Basin from Canada on 1–3 February 1987, and remained there until 13 February 1987, all the while modifying in place. Whether propagating into a region, or developing in place, the thermodynamic influence of these systems with respect to the polar front model will be similar, even though the near-surface temperatures can be quite different. Indeed surface temperature can be above average while the lower tropospheric temperature can be cool, as a result, for example, of an anomalous lack of snow cover, or day after day modification.

With respect to air quality, we agree that polar highs are often associated with clean air in a region. Indeed, several years ago when we examined this question we also could not demonstrate that a polar high was a *sufficient* condition for poor air quality. However, when air quality was the worst, polar highs, as we have defined them in the Pielke *et al.* (1987) study, were the observed synoptic regime (see Table 1 in our first comment). From our study, the polar high conditions most likely to be associated with poor air quality occur when mid-tropospheric warm advection occurs above a lower tropospheric inversion (such that the inversion is strengthened) and the below inversion transport direction is from a lower-level pollution source toward a receptor site, or the large-scale pressure gradient is weak and the resultant stagnant air mass accumulates pollution which is subsequently advected towards a receptor.

The analysis by Davis and Gay also use an incorrect definition of inversion height in their analysis. What is the relevance of the mixing condensation level (MCL) if the winds, and resultant surface-forced turbulence are not strong enough to mix the air to that depth? This weak turbulent mixing is commonly the case in winter, as shown by Whiteman *et al.* (1991), particularly under high pressure conditions. The mean MCL of 691 mb (which is about 1.6 km above the ground (AGL) at Winslow or 3100 m MSL) for the high pollution days reported in the Davis and Gay response is an inappropriate depth scale. To contrast this approach, we have used an inversion height algorithm as applied in the ATAD model (Heffter, 1980) for the high pollution days listed in Table 1 of our first comment paper. We obtained an average§ value of 0.82 km AGL for 0500 LST and 1.4 km AGL for 1700 LST. Such a lower value is consistent with the average inversion height for the winter months reported by Hanson and McKee (1985) for Grand Junction, Colorado. At Grand Junction, for the December to February period (1959–1978), according to Hanson and McKee (1983), the largest number (532; 31%) of daytime inversions were be-

§ The average was computed for 12 of 14 times for 0500 LST and all 14 times for 1700 LST of the dates presented in Table 1 of our first comment paper. The two remaining time periods were not available in the data base.

tween 1000 and 1500 m above ground level (AGL), with 467 days (27%) between 500 and 1000 m AGL. Whiteman *et al.* (1991) from NGSVS data show a typical winter inversion height at 2000–2700 m MSL (which would be 400–1100 m AGL at Winslow), whereas the NGS plume travels at 1800–2400 m MSL (which would be 200–800 m AGL at Winslow). The level most representative of the NGS plume level used in the Davis and Gay scheme is 800 mb, but the altitude of their other data levels are above the typical winter inversion. That the NGS plume transport is often uncorrelated to upper level flow direction (> 800 mb) when the Great Basin High is in place will not be accounted for systematically by the Davis and Gay system.

It should also be emphasized that the synoptic classification scheme used by Davis and Gay, even in their own words, is more appropriate for categorizing free tropospheric flow. The title of the December 1992 *Journal of Climate* paper by Davis and Walker, which uses the same synoptic classification as in the Davis and Gay paper, is "An upper-air synoptic climatology of the western United States". Apparently, even they recognize their approach does not properly include near surface flow.

Table 2 of the Davis and Gay response shows a comparison of measured and analyzed transport winds for the Winslow rawinsonde site. This table is inappropriate for the following reasons. First, their measure of shear within the transport layer is inadequate. Davis and Gay calculate transport level shear using the winds at 800 and 500 mb. While the 800 mb level is often within the transport layer, the 500 mb is not. The second deficiency involves the question of spatial inhomogeneity. Davis and Gay do show the average wind as determined by four distinct observation sites, but these sites span a distance of over 300 km of highly varying local terrain characteristics. That these sites, when averaged, will produce a representative average transport wind for the entire region has not been shown.

To address these two deficiencies, we have obtained the sounding data for a site nearer to Grand Canyon National Park (GCNP), Page, AZ (36.93°N, 111.49°W), than is the Winslow site, for the WHITEX days used in the Davis and Gay response. (Grand Canyon National Park [Hopi Point] is 110 km south southwest of Page and 194 km northwest of Winslow.) Figures 1–5 show the vertical profile of the winds for each of the case study days at Winslow and Page. The first thing to notice in these plots is that the Winslow sounding rarely has an observation below 750 mb and if it does, this observation is at 850 mb. This means that the 800 mb observations shown in the Davis and Gay response represent either the winds at a level at 750 mb, at 850 mb or some combination. Our examination of the data suggests that a

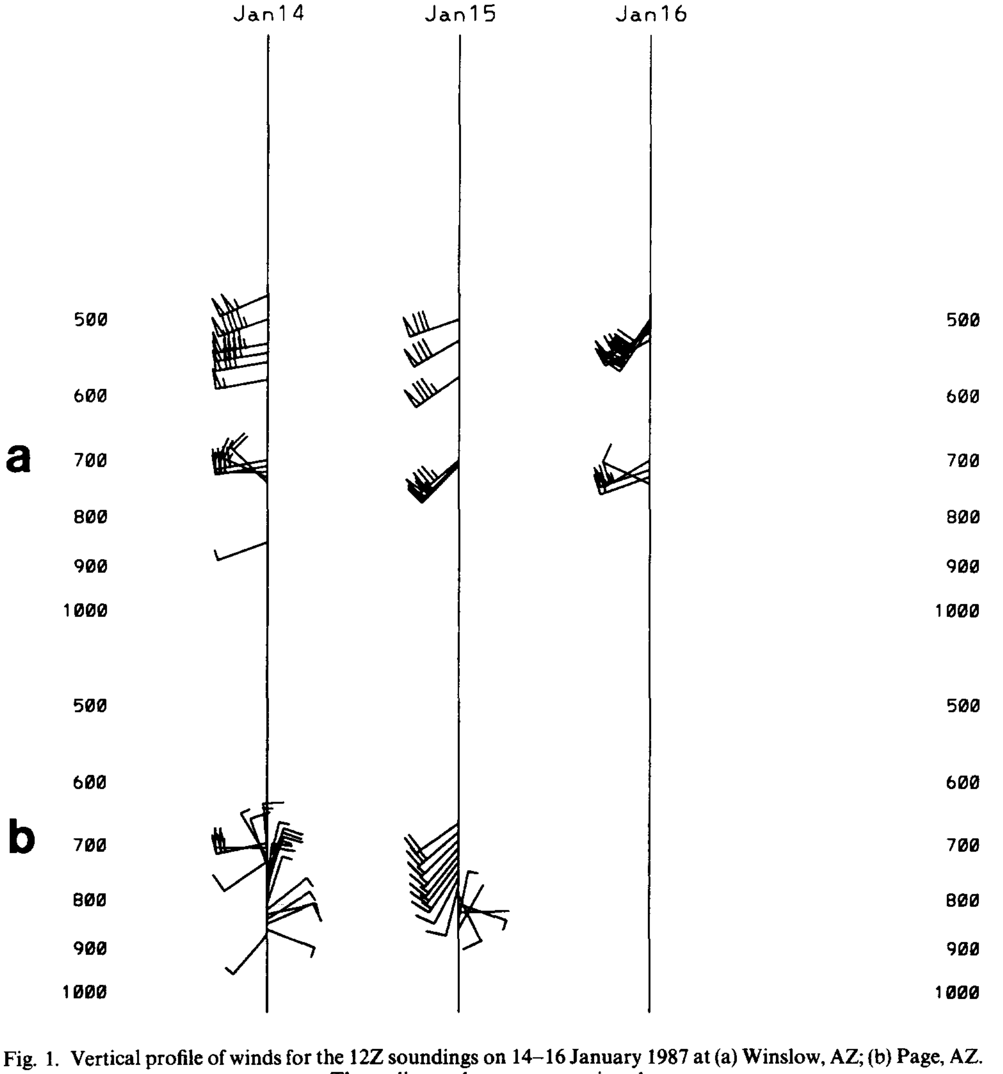

Fig. 1. Vertical profile of winds for the 12Z soundings on 14–16 January 1987 at (a) Winslow, AZ; (b) Page, AZ. The ordinate shows pressure in mb.

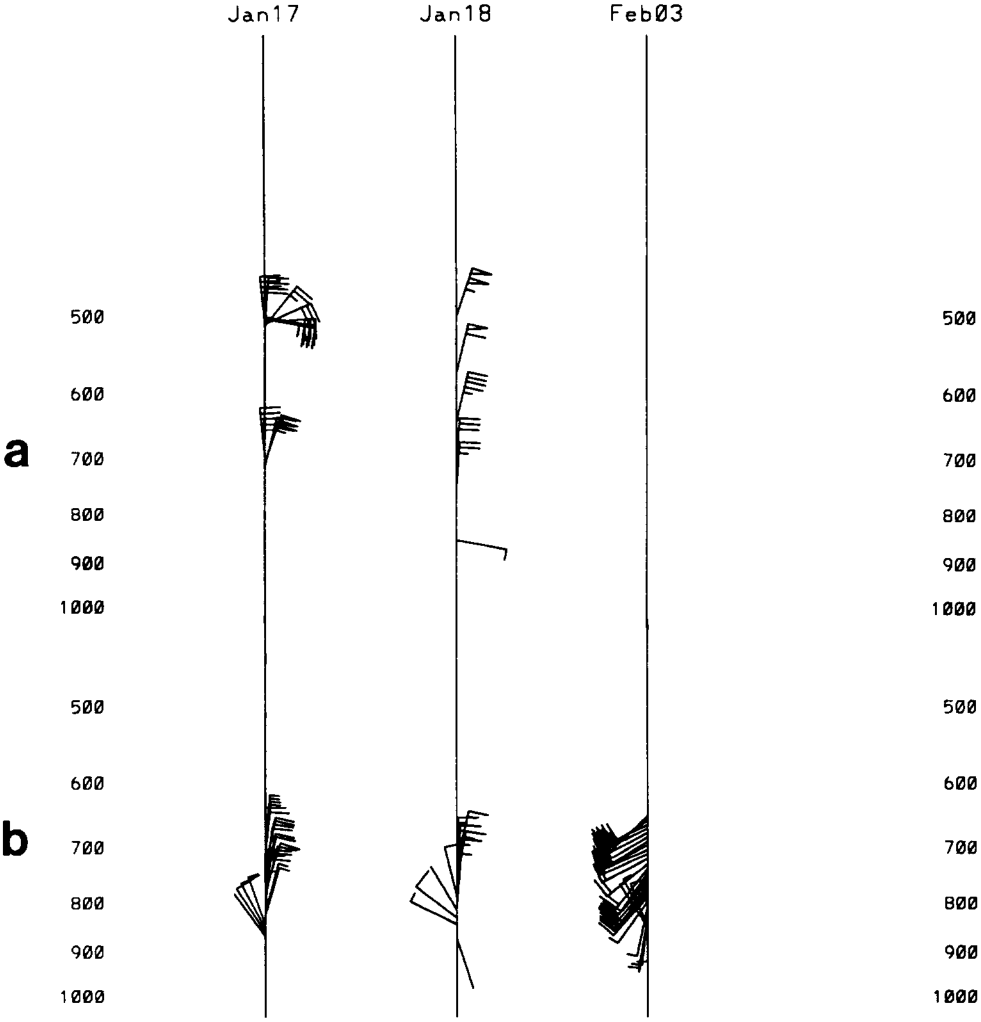

Fig. 2. Vertical profile of winds for the 12Z soundings on 17–18 January and 3 February 1987 at (a) Winslow, AZ; (b) Page, AZ. The ordinate shows pressure in mb.

level at or higher than 750 mb was used in their Table 2 and called the 800 mb observation. In any case, they need to define how they obtained their 800 mb winds used in their analysis. Secondly, there is very little directional shear in the Winslow sounding. This is most likely due to the low PBL depth in the morning at this site and the lack of low level winds available in the measured profile. In contrast, the wind profiles at Page clearly show more directional shear of the winds with height at levels generally unreported for Winslow. This directional shear of the winds with height clearly poses a problem in developing an average wind direction, even for a single site. For example, the northeasterly winds near the surface in the Page sounding for the 11–12 February, 1987 sounding (see Fig. 4), have been shown (Malm *et al.*, 1989) to be dominant transport wind for that pollution episode. Using the Davis and Gay averaging procedure, this wind flow would be erroneously averaged with the southwesterly winds aloft.

To address the question of spatial inhomogeneities we have used the same test that Davis and Gay used in creating their Table 2 by comparing average winds at Page and Winslow. The wind speeds at Page and Winslow are compared (as either measured, or if unavailable at that level, interpolated from other levels) at the 800 mb level in Table 1. On nine of the 11 (82%) days for which observations were available, the 800 mb winds at these two sites were within 45° of each other (column 7). However, when the Davis and Gay averaging procedure is run using the Page data in place of the Winslow data, greater differences occur for the comparison with the area averaged surface wind (column 8, 58% agreement). For the same time period but using the Winslow soundings in the average, the agreement is 77% (not shown). This 19% difference in agreement using soundings at two different locations emphasizes that the spatial inhomogeneity of winds in this region is significant. Using the subjective and objective classification methodology described in our first Comment, wind direction for these days is also shown in Table 1 (columns 5 and 6, comparisons in columns 9 and 10). Compared to Page, the subjective results (column 9) agree on 78% of the WHITEX days whereas the subjective vs Winslow agree only 55% of the time (not shown). A similar comparison for the objective scheme at both sites shows Page vs objective agreeing 58% of the time, and the Winslow comparison agreeing 31% of the days. This suggests that the Davis and Gay averaging procedure is very sensitive to the sounding locations used.

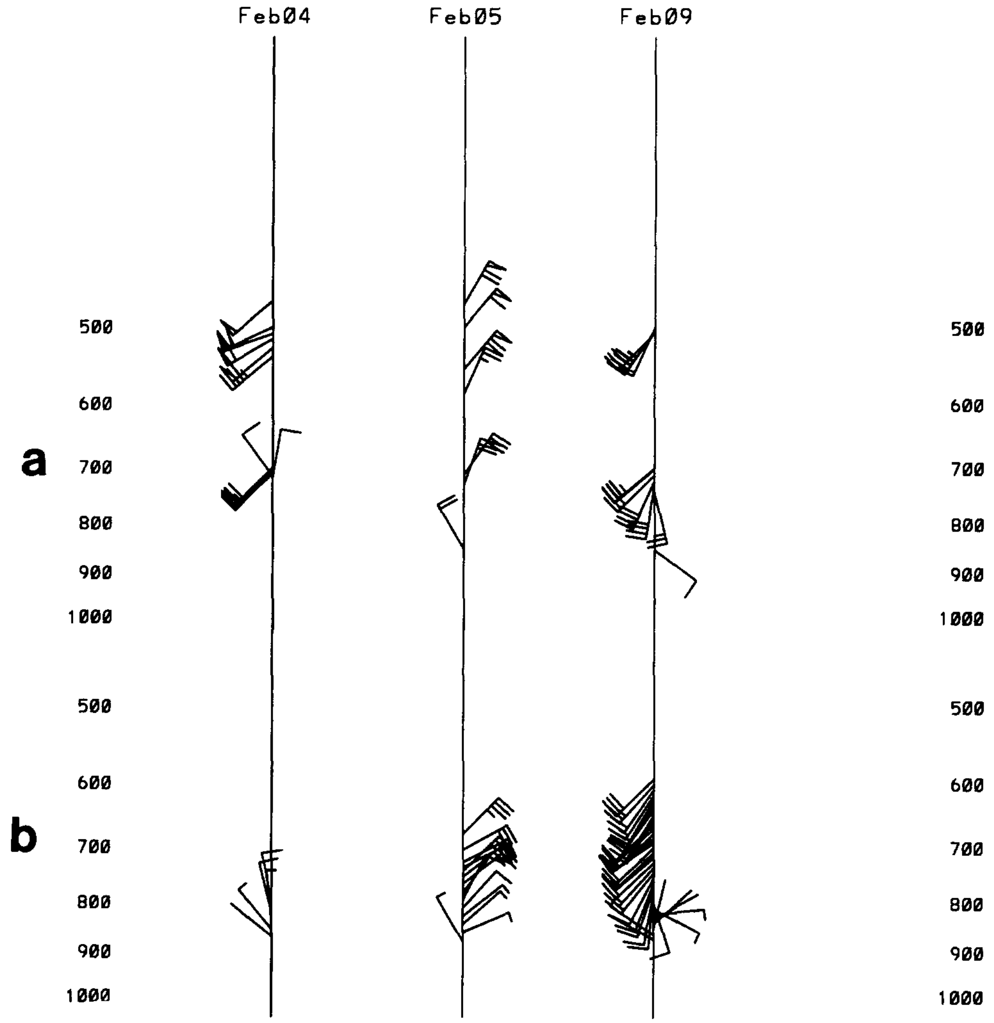

Fig. 3. Vertical profile of winds for the 12Z soundings on 4–5 February and 9 February 1987 at (a) Winslow, AZ; (b) Page, AZ. The ordinate shows pressure in mb.

The response by Davis and Gay also includes the following comment; "the air mass passed over highly-polluted regions of California before moving into the Great Basin" (which a review of synoptic maps does not support for 4–13 February 1987). Such movement of mass, however, cannot be inferred directly from either their analysis or ours. Pressure fields (and their height fields) *propagate* such that while air advection occurs simultaneously and does modify the pressure field through temperature advection, it is well known that synoptic features move as waves (e.g. Rossby waves; Holton, 1992). For this reason, we are applying a regional atmospheric numerical model (RAMS; Pielke *et al.*, 1992) and a dispersion model to refine and quantify our interpretation of transport paths of pollutants (Uliasz *et al.*, 1994). Our synoptic classification scheme is the first step in such an analysis which permits us, over short time periods (i.e. 12 h intervals) and using the synoptic pressure field, to conclude the direction from which pollution is transported, but does not allow quantitative source apportionment.

For the 9–14 February 1987 period during WHITEX, transport from the northeast direction was demonstrated by the tracer data (i.e. see Malm *et al.*, 1989) and from the time lapse camera located at Hopi Point. We urge Davis and Gay to view these time lapse images for that time period which graphically show the decoupling of the below inversion northeast and easterly flow from the above inversion southwest flow during much of that time period.

We are surprised that Davis and Gay are reluctant to grasp the value of the synoptic surface pressure field. The lower tropospheric Great Basin High is known to be dominant climatic feature in the southwest U.S.A. during the winter. Since this high is generally located north of Page and the Grand Canyon, why should there be any disagreement that flow will have an easterly component, at least up to the depth of the Great Basin High?

Rather than criticize our technique, Davis and Gay could incorporate the surface pressure data into their radiosonde-derived scheme, and perform a new analysis. If this is done objectively, it would combine the strength of their scheme in defining the free tropospheric synoptic flow, and ours in terms of defining aspects of the synoptic structure in the lowest levels of the atmosphere. Moreover, by comparing such a statistical approach with the results from a prognostic model such as RAMS (which because of limited computa-

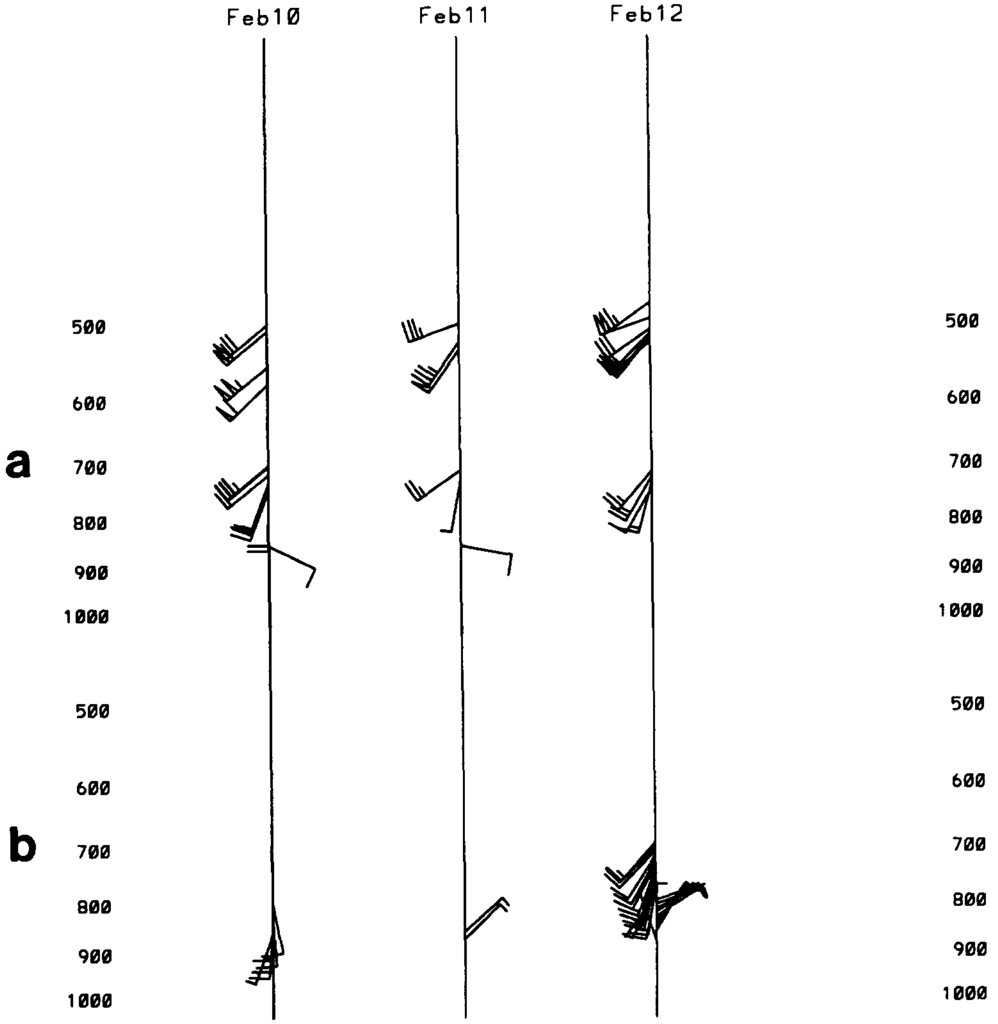

Fig. 4. Vertical profile of winds for the 12Z soundings on 10–12 February 1987 at (a) Winslow, AZ; (b) Page, AZ. The ordinate shows pressure in mb.

Table 1. Comparison of measured 800 mb winds at Page, Arizona with 800 mb winds at Winslow, Arizona, average surface winds as computed by Davis and Gay, and subjective and objectively evaluated winds

Date	Measured		Model-derived			Comparison			
	Page (800 mb)	Winslow (800 mb)	Area avg. (sfc)	Sub. (sfc)	Obj (sfc)	Page vs Winslow	Page vs area avg.	Page vs subjective	Page vs objective
9 Feb 87	S	S	SW	SE	SE	y	y	y	y
10 Feb 87	S	S	SW	SE	SE	y	y	y	y
11 Feb 87	NE	NE	S	C	SE	y	n	—	n
12 Feb 87	E	SW	SW	C	SE	n	n	—	y
13 Feb 87	W	W	SW	SE	S	y	y	n	n
14 Feb 87	—	SW	SE	SE	NW	—	—	—	—
14 Jan 87	N	NW	SW	NE	E	y	n	y	n
15 Jan 87	SE	SW	W	NE	E	n	n	n	y
16 Jan 87	—	NW	NW	NW	N	—	—	—	—
17 Jan 87	N	N	NW	NE	NE	y	y	y	y
18 Jan 87	N	N	W	NE	E	y	n	y	n
3 Feb 87	SW	—	SW	C	S	—	y	—	y
4 Feb 87	N	N	N	NE	E	y	y	y	n
5 Feb 87	NE	N	N	E	E	y	y	y	y
Totals						9/11 82%	7/12 58%	7/9 78%	7/12 58%

Note. Directions are deemed to be in agreement (y) when they are within one heading on an 8-point compass or 45°. n denotes lack of agreement;—indicates missing data; C denotes calm winds; sfc indicates data from the surface

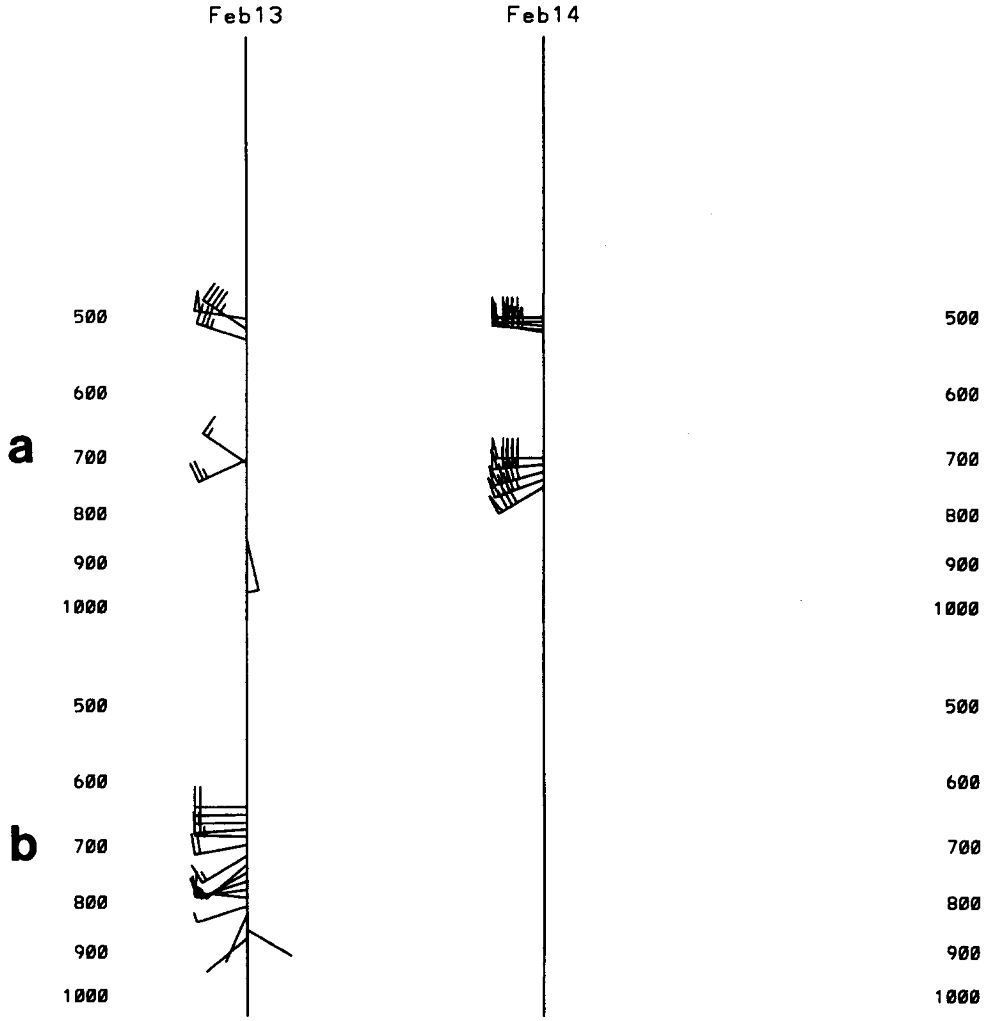

Fig. 5. Vertical profile of winds for the 12Z soundings on 13–14 February 1987 at (a) Winslow, AZ; (b) Page, AZ. The ordinate shows pressure in mb.

tional resources is being integrated for only one year), an estimate of interannual variability in dispersion meteorology could be obtained.

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AUTHORS' REPLY

RESPONSE TO "SECOND RESPONSE TO COMMENTS ON 'A SYNOPTIC CLIMATOLOGICAL ANALYSIS OF AIR QUALITY IN THE GRAND CANYON NATIONAL PARK'"

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In their second response, the major points of Pielke *et al.* can be summarized as follows:

(1) that the geostrophic wind approximation provides a better estimate of the advection of a pollutant plume than observed (i.e. measured) winds over mountainous terrain;

(2) that *cold-core* high pressure systems (including an anticyclone that was present during the WHITEX period of 9–14 February 1987) are responsible for poor air quality in the Grand Canyon;

(3) that our analysis in Davis and Gay (1993a) is not appropriate since it is too far above the surface and plume height; and

(4) that the averaging procedure we used to refute their original criticisms, as discussed in our initial response, is improper.

In this final response, we refute each of these points in turn and offer some final thoughts on the future of air quality in the Grand Canyon region.

GEOSTROPHIC WINDS AND SURFACE OBSERVATIONS

Pielke *et al.* seem intent upon arguing that the geostrophic wind provides the best representation of surface wind flow in the mountainous West. Recall that the geostrophic wind approximation is only valid with straight isobars (so that there is no centripetal acceleration present) and in the absence of friction. We doubt that there is any location in the continental United States where the geostrophic assumption is less appropriate. Pollutant transport, to the best of our knowledge, is carried out by winds that actually occur (i.e. *observed* winds), not by theoretical winds that are rarely observed over mountainous regions like the western U.S.

Furthermore, the National Weather Service (NWS) does *not* routinely contour surface pressure, but sea-level pressure. We commend Pielke *et al.* for admitting that reduction of surface pressure to sea level is problematic, but they apparently do not believe this to be sufficiently serious to alter their use of sea-level pressure data. Station pressure is reduced to sea level based on, among other variables, the average temperature in the column between the station and sea level. Since this column is occupied by soil and rock, the NWS substitutes the 12-h average temperature at the station for the column average. In the intermontane West, station elevations are typically several thousand feet above sea level; the resulting "plateau effect" produces systematic errors in the sea-level pressure estimates that artificially inflate the intensity of the Great Basin High (Saucier, 1955).

The goals of both of our original studies (Davis and Gay, 1993a; Pielke *et al.* (in Malm *et al.*, 1989)) were to relate synoptic/regional-scale meteorological information to air quality variations in and around the Grand Canyon National Park. Pielke *et al.* reliance on surface stations is based upon the density of that network. Unfortunately, it is extremely difficult to extract the key regional/synoptic-scale information from these sites because most are located in valleys where micro-scale influences are common. Specifically, to avoid this problem (and reiterating our purpose for the third time), we *intentionally* used upper-air observations in our analyses. We (once again) refer them to Kalkstein *et al.* (1990), who, in agreement with Davis and Gay (1993a, b), demonstrate that southwesterly flow aloft is predominantly responsible for poor air quality in the region. Kalkstein *et al.* also used wind observations (as measured by anemometers and rawinsondes) rather than the geostrophic approximations of surface flow favored by Pielke *et al.*

COLD-CORE VS WARM-CORE ANTICYCLONES

We are grateful that Pielke *et al.* have finally acknowledged that their so-called "polar highs" are more properly deemed "cold-core highs". We now present evidence that the high pressure system present over the Great Basin during the WHITEX period of 1–14 February 1987 was, in fact, warm-core. In their latest response, Pielke *et al.* state, "..a cold-core high pressure system propagated into the Great Basin from Canada on 1–3 February 1987...". To demonstrate that this analysis is incorrect, we reproduce 500 mb geopotential height maps for western North America and the adjacent Pacific Ocean from 1–8 February 1987, the time period in which the WHITEX anticyclone developed over the Great Basin (Fig. 1a–h). Also included on these maps are the positions of surface fronts and pressure systems.

On 1 February, a ridge was building into the Great Basin from the southwest (Fig. 1a). The tight packing of isoheights along the U.S.–Canada border indicates that the polar jet is