Reply to comment by D. A. Hansen et al. on "The 2003 North American electrical blackout: An accidental experiment in atmospheric chemistry"

Lackson T. Marufu,[1] Brett F. Taubman,[2] Bryan Bloomer,[1] Charles A. Piety,[1] Bruce G. Doddridge,[1] Jeffrey W. Stehr,[1] and Russell R. Dickerson[3]

Received 7 January 2005; revised 25 February 2005; accepted 16 April 2005; published 28 May 2005.

Citation: Marufu, L. T., B. F. Taubman, B. Bloomer, C. A. Piety, B. G. Doddridge, J. W. Stehr, and R. R. Dickerson (2005), Reply to comment by D. A. Hansen et al. on ''The 2003 North American electrical blackout: An accidental experiment in atmospheric chemistry,'' *Geophys. Res. Lett.*, *32*, L10813, doi:10.1029/2005GL022385.

[1] We thank *Hansen et al.* [2005] for their interest in our letter, "The 2003 North American electrical blackout: An accidental experiment in atmospheric chemistry" and appreciate the opportunity to expand upon the original, necessarily brief publication. We begin by making corrections to some apparent factual misrepresentations in their comment and then go on to provide a point-by-point response.

[2] • Contrary to *Hansen et al*'s [2005] contention we do not base our inferences and conclusions on a single comparison of measurements performed on August 15, 2003 and August 4, 2002. We also compare measurements inside to measurements outside the blackout area on the same day, August 15, 2003.

[3] • Reductions in power plant emissions that we report were not "estimated" as stated in the comment but are based on measurements made by power plants and reported to the USEPA. This was pointed out and referenced in the paper.

[4] • We do not conclude, as suggested in the comment, that the observed improvements in air quality went on to benefit much of the "United States" but rather "much of the eastern United States". These are completely different statements.

[5] *Hansen et al.* [2005] state the most important limitation of our study as being "... failure to consider the variability associated with concentrations of atmospheric species". They further state that similarity in synoptic weather patterns does not mean similarity in concentrations. However, the authors do not spell out what concentration variability considerations we overlooked nor do they attempt to qualify the statements. Complexities of atmospheric processes notwithstanding, it is not unreasonable to assume that when factors that drive transport and, subsequent physical and chemical transformation of pollutants in the air masses are similar, the concentrations of both primary and secondary atmospheric pollutants should be similar, unless of course, as obtained on August 15, 2003, a major primary pollutant source is perturbed.

[6] *Hansen et al.* [2005] compare observed ozone maps from EPA's AirNow archives for August 4, 2002 (our control day) and August 14, 2003; a day they contend was synoptically similar to our experimental day (August 15, 2003). They go on to argue that since O_3 levels over Maryland, Pennsylvania, and New Jersey on August 4, 2002 were higher than on August 14, 2003 our observed changes in air quality following the blackout could have been a result of phenomena other than reduction in power plant emissions. This argument does not hold because in choosing the control day (August 4, 2002) we did not only consider synoptic-scale air motion but all factors that drive atmospheric chemical and physical processes, including temperature, insolation, and humidity. The average regional surface high temperature for August 14, 2003 ($\sim$ 29°C) was about 4°C lower than that for August 4, 2002 ($\sim$33°C) or August 15 2003 ($\sim$33°C). *Ryan et al.* [1999] show that in this temperature range, a 4°C difference can account for as much as 35 ppb in O_3, probably enough to account for the unspecified disparity in O_3 abundance that Hansen et al. report. Ozone concentrations on August 14, 2003 cannot, therefore, be compared to August 4, 2002 or August 15, 2003. Local meteorology cannot be responsible for the differences in air quality observed in our study because surface O_3 maps from EPA's AirNow archives also show the same differences at the regional level.

[7] Regarding the regional representativeness of measurements conducted over a single location, we would like to point out that unlike surface measurements, which *Hansen et al.* [2005] probably had in mind, aircraft vertical profiles (surface – 3 km) are more regional measurements that capture regional signatures. Moreover, our overall deductions are not based only on the profiles conducted over central PA but on transects and profiles conducted over PA, northern Virginia, and Maryland as well. The $PM_{2.5}$ comparisons, which Hansen et al. go on to make in support of the representativeness argument, are wrong for the following reasons:

[8] 1) As alluded to earlier, the days they compare (August 14 and August 15, 2003) are not synoptically similar.

[9] 2) The $PM_{2.5}$ data presented are 24-hour averages, therefore August 14, 2003 data were also affected (lowered) by the blackout, which started at 4:00 ET on the same day. Thus a comparison of 24-hour average $PM_{2.5}$ concentrations

[1]Department of Meteorology, University of Maryland, College Park, Maryland, USA.
[2]Department of Chemistry, University of Maryland, College Park, Maryland, USA.
[3]Also at Department of Chemistry, University of Maryland, College Park, Maryland, USA.

0094-8276/05/2005GL022385$05.00

from August 14 and 15, 2003 cannot be used as a measure of the magnitude of the impact of the blackout.

[10] 3) The simple one-to-one comparison of aircraft profiles and surface measurements of *Hansen et al.* [2005] is not permissible. The two measurement types cover different catchment areas and cannot be compared "simply". Moreover, surface measurements are especially sensitive to local influences at night when the boundary layer is shallow.

[11] 4) Similarly, complexities associated with atmospheric processes require that care be taken when comparing primary and secondary pollutants such as in the case of SO_2 and $PM_{2.5}$.

[12] 5) The $PM_{2.5}$ data reported by *Hansen et al.* [2005] are 24 h average mass concentrations. Although in the mid-Atlantic most of the mass is made up of sulfate, which is also primarily responsible for the scattering values we report, there are limitations to comparing mass and scattering values.

[13] The above shortcomings in Hansen et al.'s $PM_{2.5}$ analysis notwithstanding, the data that they present in Table S1 clearly support our findings. There is a general decline in $PM_{2.5}$ levels following the blackout, which, despite their choice of control days (see bullets 1) and 2) above), is still apparent. All but one represented Pennsylvania regions show a decline in $PM_{2.5}$ when comparing August 14 with August 15, 2003 (southwest Pennsylvania averages −15% (3 sites); south-central Pennsylvania averages −20% (3 sites); northeast Pennsylvania averages −4% (4 sites); southeast Pennsylvania averages −23% (2 sites)). The only Pennsylvania region that does not show a decline is the northwest (+18%, 2 sites), a region that was closer to the affected power plants and would not necessarily be expected to see the downwind impacts.

[14] We do acknowledge that, indeed, a wider geographical coverage in our aircraft measurements on August 15, 2003 would have yielded a more representative picture but the accidental nature of the unplanned experiment precluded such extensiveness and our results are presented within the context of these limitations.

[15] The SO_2 mixing ratio in the 2002 vertical profile shown in Figure 4 of our article changes from 4 ppb at the surface to 2 ppb at 2.5 km, which is not as much of a strong gradient as *Hansen et al.* [2005] described. The profile simply shows evidence that the source is near the surface contrary to Hansen et al.'s contention that SO_2 sources are elevated without specifying the extent of elevation. Power plant stacks are not more than a few hundred meters tall and therefore are "near the surface" when put in the context of profiles that go up to 3 km in the vertical. With a source near the surface and a sink near the top of the PBL (reaction with peroxides in fair weather cumulus clouds) a vertical gradient is expected. The sulfate regional experiment (SURE) data, published in an internal EPRI document, conflict with SO_2 profiles published in the reviewed literature, for example, *Heikes et al.* [1987].

[16] Regarding the said discrepancies in back trajectories and potential differences in emission source strengths for August 4, 2002 and August 15, 2003, we would like to point out that we make no assumptions as to which power plants were scaled back and to what extent. Instead we use actual emission numbers supplied to us by the USEPA and the trajectory "swaths" that incorporate the relevant power plants. Regardless of whether they are the same power plants or different, the observed differences in pollution levels are attributable to differences in power plant emissions upwind of the study area.

[17] It is not clear what *Hansen et al.* [2005] are disputing regarding the fact that we observed no significant changes in CO mixing ratio and particle light absorption over central Pennsylvania, and that these observations are consistent with the observed lack of change in vehicular activity (the main source of CO and soot) upwind of the study area. They contend it is reasonable to assume that in upwind areas heavily impacted by the blackout, traffic activity and hence emissions were reduced. Reasonable as that may sound, the composition of air impacting central Pennsylvania does not support it and we see no point in making "reasonable" assumptions where empirical data exist. If, indeed, vehicular traffic upwind were substantially reduced why were CO and soot unaffected?

[18] Regarding the root mean square forecast error of 10 ppb mentioned in the discussion, we agree; it is possible that the Baltimore/Washington ozone forecast was high by chance. A difference of 3.5 times the RMS error is not impossible, only highly improbable.

[19] In conclusion, we acknowledge that this study was not perfect on account of its unplanned and accidental nature. Most of its limitations arise from this reality and not, as *Hansen et al.* [2005] contend, a lack of analytical rigor. Our results are presented in the context of these imperfections and, fittingly, we do not make sweeping or holistic inferences from them. We also do not make a conclusive indictment of the electrical generating community that EPRI represents. We simply took advantage of a unique opportunity that may never be repeated and drew conclusions from the available data. Measurements downwind of many of the affected power plants, together with the reported (not estimated) emissions from those power plants suggest a clear relationship between the blackout and air quality. The synoptic conditions on the study and control days were similar enough for photochemical processes to be comparable. Any differences in wind conditions were accounted for by the integration of emissions specific to the individual trajectories. Future work will entail a modeling study coupled with surface and aircraft data to attempt to draw more general conclusions about the impact of electrical generation on regional air quality.

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B. Bloomer, R. R. Dickerson, B. G. Doddridge, L. T. Marufu, C. A. Piety, and J. W. Stehr, Department of Meteorology, University of Maryland, 2335 Computer and Space Sciences Building, College Park, MD 20742, USA. (marufu@atmos.umd.edu)

B. F. Taubman, Department of Chemistry, University of Maryland, 2107 Computer and Space Science Building, College Park, MD 20742, USA.