Dear Dr. Federman,

The manuscript, "Precision Determination of Atmospheric Extinction at Optical and Near IR Wavelengths", D. L. Burke, et al. ApJ #80422 has been revised and resubmitted following consideration of the report of the referee. Specifically:

1. p. 4, 6-7 lines from the bottom. In practice, it is not really necessary to extrapolate the observed extinction curve to the top of the atmosphere; it is only necessary to account for the airmass differential between target star and the photometric reference standards. Ideally, this will be a short interpolation and not a long extrapolation.

- We have removed the sentence that refers to extrapolation to the top of the atmosphere, and left only the sentence from the original text that comments on the need to interpolate between the target and a standard star.

2. p. 5, last sentence of Sec. 1. If on the basis of their experience, e.g., with Sloan, the authors could provide some quantitative estimates of the fraction of 1%, 2%, better, and worse nights, some people would find those numbers useful for planning purposes, or maybe just for general interest.

- The numerical values included in this sentence were intended only to reflect what we perceive as the anecdotal experience of most astronomers. We do not think it appropriate for us to try to present SDSS data in this paper, so have simply removed the reference to specific numbers.

3. p. 8, Fig. 2. Personally, I have trouble seeing the difference between some of these colors. The curves would be more meaningful to me as chains of distinct symbols rather than as solid curves of identical thickness and similar darkness.

- We did indeed try other methods of presenting these curves, but the use of non-solid lines in Figure 1 does not work well because of the rapid vertical fluctuations in the molecular absorption lines; and we want to maintain continuity in Figure 2.

4. p. 10. Is "fluxuations" a word? Is "fluctuations" meant, or is this some technical term completely beyond my experience?

- The spelling of "fluctuations" has been corrected.

5. p. 10. Equation: is A_mols a typo for A_mola, or is it a different (as yet undefined) quantitity?

- A_mols is a label for the molecular (Rayleigh) scattering template. A reference has been added to the text in the second paragraph in section 2.1 (and also in the caption of Figure 1).

6. p. 11. Eq. 5, I seriously question the wisdom of expanding the approximation in terms of Delta HA and Delta delta. A given Delta HA means a completely different angular distance in the

north from what it means in the south, and Delta HA takes all values between -12 and +12 simultaneously at the pole. I think a wiser parameterization would be in terms of Delta cos(alt)sin(az)and Delta cos(alt)cos(az). (Basically proportional to the Cartesian x,y of the point where the line of light passes through any given layer of the atmosphere.) This is well defined, symmetric both N-S and E-W, and continuous and smooth through both pole and zenith.

- This is an excellent point. We have redone the analysis using the form suggested by the referee and updated Figure 8 and the text. The results are not changed significantly (the east-west variation in extinction by water vapor is measurable, but not large), but the form suggested by the referee is clearly preferred. We have similarly tried this form for the spatial variation of the optical depth for aerosols; in all cases the spatial dependence of the aerosols are found to be consistent with zero for these observations.

7. p. 13, Table 2. For mapping telluric absorption features, spectroscopists have traditionally used rapidly rotating O and B stars. These have no strong or sharp spectral features, can be quite bright, and provide lots of UV photons for those pesky blue-insensitive CCDs. Is there a reason why the authors have included no such stars here, in favor of much fainter white dwarfs, and AFG stars that can have strong and likely sharp absorption features of their own?

- The most structurally complex signatures of atmospheric absorption occur toward the red end of the optical range, so we used stars with spectra that emphasize this region. There is no reason in principle not to use O and B stars, but the white dwarf stars are well-known Southern standards (from the ESO list) with magnitudes fairly well matched to the AFG stars used as probes. Our use of an integrated Kolmogorov-Smirnov statistic to fit the sources of atmospheric absorption most significant in the blue makes the analysis rather insensitive to features in the stellar spectra that have small effective widths - and Balmer lines are easily modeled.

8. p. 15, first line: I suggest replacing the phrase "differential atmospheric refraction" with "atmospheric dispersion." To an astrometrist, "differential atmospheric refraction" refers to the variation of refraction angle with zenith distance, resulting in different images scales parallel to and perpendicular to the horizon. I encourage the authors to maintain this distinction in their nomenclature.

- We have made this change.

9. Couple of typos noted on p. 17: "A first set ... were fitted" should be "A first set ... was fitted" and "Metalicities" should be "Metallicities".
Also, on p. 23 "A typical set ... are shown" should be "A typical set ... is shown."
If I see more typos I will not mention them, but I suggest that one or more co-authors should give the text a critical read.

- We have fixed these typos, and proof-read the text.

10. We need more information on the quality of these nights APART from the results achieved with the full model fit. In particular, what r.m.s. fitting residuals result if a single constant value

of K (Eq. 3 on p. 4) is computed for each filter on each night? The statement is made that "the y band correction varied over this one night by as much as 50 mmag at fixed airmass." It is not at all clear whether "by as much as" means r.m.s., peak-to-peak, or anything really; whether this was generally a good, typical or poor night; or how these variations would be perceived in other filters where the atmospheric tranmission is higher and smoother with wavelength than the notoriously miserable y filter.

- We have expanded the discussion of this in the first paragraph of section 5.1. We also note that Stubbs, et al (2007) cited in the first paragraph of section 2.1 provide a survey of many years of atmospheric data that were used to guide construction of our fitting model.  We have made a more pointed note of this in the second paragraph of the conclusions in section 6.

11. It would be useful if the authors could provide estimated values of K (Eq. 3 on p. 4) for a much wider variety of commonly used filters, and their range of variation. Can Fig. 1 and the extinction model be extrapolated even approximately to shorter wavelengths, given the relative smoothness of the model, even if there are at present no observations to confirm the physical data? How do UBVRI compare to ugri, for instance, in their sensitivity to atmospheric variability (I expect U is much worse than u, but it would be good to understand the other filters better). Stroemgren uvby are concentrated in the spectral regime where only Rayleigh scattering matters. Is this a particular strength? Is the very broad-band Washington CMT1T2 system particularly difficult to calibrate? If on a given night I can measure, for instance, only some of K_U, K_B, K_V, K_R, and K_I, with what reliability could I predict the others? I understand that the limited nights of observing used here prob ably do not come close to sampling the possible range of observing conditions, but if the authors could call upon their theoretical understanding and previous observing experience to discuss these points, I think a less Sloan- and LSST-centric discussion would be valuable for others.

- We agree with the referee that a study of the relative strengths and weaknesses of various standard filters is an interesting topic, but believe this discussion is beyond the intent and scope of the work presented in the manuscript.  Our results demonstrate a technique for measuring properties of the atmosphere that effect astronomical observations in a variety of ways and with importance that depends on the science goals of the research being performed.  We have opted to limit the paper to discussion of the technique, though investigation of wider implications of the measurements might make a rather nice follow-on study and paper.