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LETTER TO THE EDITOR

Reply to G. Visconti

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The authors wish to thank G. Visconti for the attention he paid to their work. It gives us an opportunity to come back to it. The comments are actually all on points we had discussed in our paper giving the reader a warning on how a particular assumption was related to reality and what limitations and drawbacks it had. Thus, the comments should be considered as numerical illustrations of the points, but in several cases the illustrations "are incorrect", using his words, including one arithmetical error.

The first comment is about single atmospheric temperature T_a for both upward and downward IR fluxes. On p. 175 on the upper right, we discuss the limitations of the assumption and note that it can give underestimations in the sought values of T_a and in the surface temperature T_s. On page 176, in the middle left, we had given $T_s = 282$K and said that "to obtain $T_s = 288$K, one must significantly decrease D (the transmission, as G. Visconti is demonstating) or α (the atmospheric absorption), or both". We may comment on this, by saying that with $D = 0.14$, albedo 0.3 and surface absorption of the solar radiation $\alpha_s = 0.45$, from our formula (2.7), one should obtain $T_s = 279.4$K, not 282.6K he is presenting. We rounded numbers up to three significant digits, so our 282K, which was obtained for $L_s = 0.5$ is actually 282.38 because for the solar constant 1370 $\mathrm{Wm^{-2}}$, $T_{e0} = 278.78$K ≈ 279K. We said quite clearly that the single value of T_a is defective for normal conditions, but with a situation approaching the isothermy, because of the atmospheric heating and surface cooling, the model should work better. Actually, it has the correct asymptote for large optical depth τ and gives results for intermediate τ comparable to Turco et al. (1983) and several others.

The second comment concerns the application of our model to CO_2, dust storms and smoke. We do not prove our formula, because a model cannot be proven, but needs to be tested to see how it works. The testing grounds are the temperatures for the normal earth; we got some underestimations for T_a and T_s, the temperatures for the normal Mars; the results obtained were about right; the *changes* in T_s for doubling CO_2 in the earth's atmosphere which are $\delta = 1.4$K are again somewhat underestimated; the changes in T_a and T_s for the dusty Martian atmosphere are in good agreement with observations; changes in T_s and T_a for the Earth's atmosphere with dust compare well (with some underestimation) with the Pollack et al. (1983) multi-layer radiative convective model. Our philosophy was that if the model does not work too badly at such widely different conditions, it should be reasonable in predicting effects of large amounts of smoke. Commenting about CO_2, Visconti has some grounds: we should here not use $T_s = 288$K because it is not consistent with optical properties of the model at normal conditions, but using $T_s = 282$K gives $S(2CO_2) = 1.5$K, improving only slightly the performance of the model.

Now, about the ratios of the optical thicknesses in solar, τ_s, and IR, τ, Visconti says that we assumed incorrect ratios of τ_s and τ. Actually, we did not, at this point, assume anything, but after review of a substantial amount of the literature for optical properties of smoke, we took $\tau_s = 10\tau$ and for dust $\tau_s = 4\tau$, having mainly data for Mars (see references in Golitsyn and Ginsburg, 1985). After our paper was submitted, the NRC (1985) Report was released, where optical properties of smoke and dust were discussed at length. The ranges there include our values having come somewhat closer to the upper range of the values. This ratio depends primarily on the size distribution of the particles and their complex refractive index, so the examples given by Visconti do not have a direct relation to the actual dust storms and fire smoke we are dealing with. In any case, G. Visconti is right that the larger the ratio, the stronger the cooling, as follows from common sense and our results (see our Fig. 2b where curves for dust and smoke illustrate the point very well).

Visconti is right with his objection on how we deal with albedo and again the essence of his comments can be found at the end of Section 3. However, some additional clarification may be needed here. It does make sense to report cases with $A = 0.3$, i.e., the albedo unchanged in comparison with normal conditions because, as we noted in our paper, the mineral dust is increasing in the albedo and much of the dust above is smoke in several scenaria by Turco et al. (1983). Perhaps we should mention it specifically at the corresponding points, but having discussed the different influences of smoke and dust on albedo of the system, we hoped, evidently wrongly, that the reader was familiar with the fact that bombs produce dust (the point is also discussed at the lower right of p. 178). But we must admit, here, that we have overlooked several important words on line 8 from above, in the right-hand column of p. 177. The complete sentence should be as follows: "Using more accurate numerical and analytical calculations *based on methods more sophisticated than the one* by Sagan and Pollack (1967), we computed asymptotic values of A when $\tau_s \to \infty$". In relation to Sagan and Pollack (1967), we may add that their theory is good only for aerosols with weak absorption and not for a very large extinction optical depth; that is why we used other methods to obtain our approximation formula (3.4).

The last optical comment by Visconti is that our approximation of transmission (not absorption α_s, as he says) is rather crude. We used the same word "crude" for the way we deal with the optical properties of the aerosol. He is correct that for the single scattering of albedo $\omega < 1$ (evidently substantial absorption), the effect is not important for large optical depth. For the aerosol with $\omega = 0.9$, we have used $r_s = 2(1 - \omega)/M$, where $M = 0.5$ is the average cosine of the sun's zenith angle. Visconti does not present how he makes his diurnal averaging, so we would not reproduce his 0.295, but with $\omega = 0.5$ and $\tau_s = 2$, we obtain 0.449, a value close to his 0.4748.

The final comment is a surprise as to why the first author, who had developed some general arguments on the effects of dust on the atmospheric circulation, neglected them here. The reference to the comment is incorrect; it should be Golitsyn (1973), where some arguments were presented concerning dust storms on Mars (but not smoke!). Visconti says that the heating of the atmosphere would enlarge the Hadley circulation. It is again a proper comment and most of our Section 5 is devoted exactly to this problem, i.e., how a single Hadley cell appears as the main feature of the atmospheric circulation both in the case of dust storms and in numerical models of nuclear winter, and how the phenomenon was simulated in special laboratory experiments.

All the formulae and the Figures in our paper were obtained in 1983 and many much more detailed results have been obtained since then. But we believe that being analytical, our results still have some methodical value easily revealing the important features of the temperature regime of the atmosphere with absorbing aerosol. With some underestimation of temperatures in normal conditions and temperature changes with an increase of the aerosol optical depth, only one important feature is missing in our model, which is a slight heating of the surface for small optical depth, but for that, one needs a multilayer model with aerosol in lower levels. Such a multilayer model with direct treatment of the radiation transfer has been developed and its results for $\tau_s < 1$ are in a good agreement with the simplified model under discussion.

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