Reply

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We shall reply to the points raised by Rossow, Fels and Stone (RFS) and make additional comments where appropriate.

1. Global mean temperature structure

RFS state that the model dynamics does not have much influence on the equator-to-pole temperature difference, and that a major part of the interaction between the dynamics and the temperature structure is missing.

a) That the computed motion fields are important in determining the thermal structure is evident from Figs. 21 and 24 of Young and Pollack (1977, hereafter YP) and the associated discussion (pp. 1341–1342). When $\bar{u}_\phi$ is small the parameterized diffusion of heat is negligible, and the mean meridional circulation balances the mean differential solar heating. As $\bar{u}_\phi$ increases, the equator-to-pole temperature difference increases concurrently. This temperature increase is clearly associated with the zonal wind increase and is not the response of the zonal wind to an applied equator-pole temperature contrast.

b) It is true that when $\bar{u}_\phi$ is large, the mean differential solar heating was balanced by the parameterized eddy terms. We already discussed this at some length in the paper (pp. 1341–1342 of YP). As we stated there, when $\bar{u}_\phi$ is large, baroclinic eddies will probably be important in transporting heat, and we did not resolve them nor realistically parameterize them. Therefore, in this situation, the quantitative details of the meridional temperature structure are uncertain. Nevertheless, the equator-to-pole temperature contrast when $\bar{u}_\phi$ is large is the result of approximate cyclostrophic balance; if $\bar{u}_\phi$ decreased, the equator-to-pole temperature contrast would also.

c) As discussed on pp. 1335–1337 of YP, we made computations where all the nonlinear advective terms were removed from the momentum and energy equations, so that the balance in the energy equation in steady state was almost entirely between the parameterized eddy terms and the differential solar heating. The resulting steady equilibrium flow configuration was from subsolar to antisolar point. It is obvious from Figs. 17 and 18 of YP that the thermal structure with the nonlinear terms retained is quite different from one corresponding to a simple steady Hadley flow from subsolar to antisolar point. Thus, it is clear that the diffusion terms are not fixing the thermal structure and the motion field simply responding to this structure, but in fact the motion field is determining to a large degree the thermal structure.

2. Formulation of momentum diffusion

a) The zonally averaged momentum advection must be balanced by the dissipative terms in steady state. We discussed this at some length in the paper (pp. 1320, 1335 of YP).

Unavoidably, therefore, the exact details of our solutions are affected by our choice of momentum diffusion. But the question of whether or not generation of $\bar{u}_\phi$ depended on our diffusion formulation was answered when we performed the calculation without the nonlinear terms (pp. 1335–1337 of YP); $\bar{u}_\phi$ decayed to zero and the flow characteristics were not at all like those obtained in our general solutions. *Therefore, the damping terms do not generate nor maintain the mean zonal winds in our model nor determine most of the flow field characteristics.*

b) It is true that forces are present in the model which result from neglecting the dependence of density with altitude in the diffusion terms. But as we stated above based on calculations deleting the nonlinear advective terms, it was established that these forces were not as important as the terms we treated explicitly, i.e., they were small compared to the nonlinear forcing. Furthermore, we know of no reason for believing the formulation suggested by RFS is any more relevant to Venus than the one we used.

c) RFS present a simple analytical example and infer from this that we would have obtained a much smaller mean zonal wind if we had explicitly accounted for a variable density in the diffusion formulation. Their result neglects two important facts: 1) the momentum forcing itself is a complicated function of $\bar{u}_\phi$; and 2) the vertical diffusion time over more than one atmospheric scale height is much greater than any of the other relevant time scales. The atmospheric scale height in our model was at least 10 km except for the altitude range between 55 and 64 km. The vertical diffusion has an associated diffusion time scale over 10 km of $\sim 1.3 \times 10^8$ s; over 20 km it is 2×10^9 s. It is clear from Figs. 1, 5 and 10 of YP that between 30 km and the top boundary the growth of $\bar{u}_\phi$ is limited on a time scale *much shorter* than the time scale associated with diffusion over more than one scale height. Therefore, *any density dependence of the momentum diffusion formulation acting over several scale heights cannot be of major importance in determining the results presented in YP*. The growth of $\bar{u}_\phi$ is limited primarily by the nonlinear dependence of the forcing on $\bar{u}_\phi$ itself.

Under the above circumstances one would expect the values of $\bar{u}_\phi$ presented in YP to be relatively insensitive to the magnitude of the momentum coefficient for values of the coefficient not too close to the critical value. Near values of the diffusion coefficients given in Table 1 of YP, $\bar{u}_\phi$ depends on the diffusion coefficient raised to ~¼ power.

d) The fact that increasing the diffusivities by a factor of 3 led to decay of $\bar{u}_\phi$ established that there is a critical value of the damping above which the nonlinear instability which amplified $\bar{u}_\phi$ in our calculations would not occur. Near this value one would expect sensitivity of the flow to values of the diffusion coefficients. As we stated in the paper (p. 1337), we have not determined quantitatively where this region is for Venus because of the diffusion formulation we used. Nevertheless, this result does indicate that a critical value of the damping exists, a result also in qualitative agreement with the result of Busse (1972) and Thompson (1970) for nonlinear instability involving only eddy stresses.

3. Truncation effects

a) Within the context of the model presented in the paper we had adequate resolution, i.e., the modes neglected would not have altered our solutions. We checked this by a limited number of higher resolution runs.

The question is whether we had so much damping at higher wavenumbers that we left out important physical processes which would have given us totally different results. We don't think so as discussed below.

b) From Baines (1976), in order for a mode to be barotropically unstable the magnitude of that mode must be above a critical value relative to the mode corresponding to solid body rotation (his Tables 1, 2, 3). For the modes retained in our model this critical amplitude is of the order unity. Under this criterion all our modes, with the exception of the T_3^0 mode mentioned by RFS, *are quite stable*.

c) By Baines' criterion the T_3^0 mode in our solution I is unstable in the upper region of the model—it exceeds the critical amplitude by a factor of ~1.6. However, Baines' calculations also show that even though the T_3^0 mode is unstable, it probably retains most of its original energy; the barotropic modes do not extract energy efficiently from it. RFS state that Baines' calculations retain only a few specific modes and imply more resolution would give totally different results. This statement is rather surprising, since the numerical integration done by Baines upon which the above conclusion is based has rhomboidal truncation number 10, a resolution accuracy comparable to that used in Rossow and Williams (1979) involving a 128 × 128 grid-point array. Therefore, even if we had represented the unstable barotropic waves, we would not expect our mean velocity profile to change. This conclusion is born out by the observations of Mariner 10. Even though the latitudinal profile of the observed mean zonal wind is barotropically unstable (Travis, 1978), the profile, which is qualitatively similar to our solution I profile, is observed. A qualitatively similar profile is implied by the preliminary Pioneer Venus DLBI[1] wind measurements (Counselman *et al.*, 1979). If barotropic instability was efficient at spreading out the latitudinal profile it is difficult to imagine why one observed such a profile; meridional transport of zonal momentum, probably by the mean meridional circulation, is more efficient in maintaining the profile than the barotropic modes are in spreading the profile out. This is a situation, probably crucial to the dynamics of Venus, which the model of Rossow and Williams (1979) neglects.

d) RFS imply that the YP model may not correctly simulate any of the essential nonlinear interactions. However, it is easy to show that if the initial conditions have the hemispheric symmetry assumed in YP, then the equations retain that symmetry for all time. Therefore, the model of YP has not artificially eliminated any nonlinear interactions that the equations would otherwise produce, i.e., the solutions are as general as the initial conditions from which they started.

e) By the above statements we do not mean to imply that a better or improved model should not treat the barotropic modes we have neglected. We feel, however, as we stated in the paper (pp. 1342, 1348), that the modes which are the most important to include in future calculations are baroclinic modes, or some hybrid of barotropic and baroclinic modes. The reason is that the equator-to-pole heat transport was accomplished in the model by horizontal diffusion in several of the solutions after $\bar{u}_\phi$ had grown to large values. Barotropic modes are likely to transport heat up, not down, the thermal gradient (cf. Pfister, 1979) and so will probably act to increase the equator-to-pole temperature contrast. Therfore, it is probably necessary to explicitly resolve baroclinic modes in future calculations if meridional heat transport is not to be parameterized.

[1] DLBI = Differential long baseline interferometry.

It should be noted though that in order to do this we would have to decrease our diffusion coefficients sufficiently so that the higher wavenumber baroclinic modes would not be damped, and it is not clear we can successfully do this at present.

f) The damping of barotropic and baroclinic modes, and higher wavenumber processes no doubt leads to the fact we get very steady solutions for $\bar{u}_\Phi$, contrary to observation. Second, the equator-to-pole temperature contrast in the upper atmosphere which is intimately related to $\bar{u}_\Phi$ would be affected by modes we have not treated; we expect this to affect the magnitude and specific spatial dependence we would get for $\bar{u}_\Phi$, but not the overall scenario by which $\bar{u}_\Phi$ is generated. Most of this we discussed in the paper (see pp. 1342, 1345, 1348).

g) RFS make the statement that only three modes in our model are not strongly damped and imply we should only expect to see these three modes. It is true modes having harmonic indices 3 and 4 are damped much more than modes with indices 1 and 2. However, it is quite clear we can get modes for $\bar{u}_\Phi$ that are significant that do not correspond to solid body rotation (our solution I for $\bar{u}_\Phi$), and that higher modes (such as those with zonal wavenumber $m = 2$) contribute to the vertical velocity patterns given in Figs. 17 and 18 of YP.

We isolated the zonal wavenumber $m = 1$ poloidal component of the flow in Fig. 27 of YP, not because $m = 2$ modes weren't important, but because we wanted to illustrate the $m = 1$ component and the fact that under certain circumstances it produced a Y-feature. In order for this component to produce a Y-feature, a mode with harmonic index higher than 1 must be present, as it clearly is from the figure. In our model this mode has harmonic index 3, i.e., it is the S_3^1 mode. We discussed this mode and why it must be the result of nonlinear interactions on p. 1339 of YP. Without this mode present in Fig. 27, there could obviously be no latitude at a fixed longitude where the vertical velocity changed sign, and hence no Y-feature. Thus, contrary to the statements of RFS, higher modes do contribute significantly to the flow patterns presented in YP.

4. Summary

Contrary to the suggestions of RFS, the Venus model of YP includes the effects of dynamics on the thermal structure, although as pointed out in YP we have not included explicitly some of the transport mechanisms; the diffusion formulation does not spuriously generate the large mean zonal winds obtained by YP; the spectral truncation, while quite limited, is adequate for deducing the qualitative nature of the flow fields and studying the mechanism by which the large mean zonal wind is generated.

The model of YP represents a preliminary step toward modeling the circulation of the Venus atmosphere. There is no doubt that in order to quantitatively describe the circulation, many of the approximations and assumptions made in YP need to be removed. This we will be doing in the near future using a more advanced model.

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