The manuscript entitled "Atmospheric Circulation of Venus and its Rotation Evolution" by Zhieng Yang, deals with computations of the time evolution of the Venus length of day due to tides and atmospheric motions.

The main results is that the atmospheric contribution dominates the others and that, as far as I understand, this effect is able to explain the decrease of a quick rotation of 15hr to zero and even a reversal of the rotation, explaining that Venus does not rotates the same way as the other planets.

Of course, such a result would be very important and I would strongly encourage publication of this manuscript despite of its poor writing and lack of rigour in the explanations. However, and although I am not a specialist of the Liouville equation but rather of the atmsopheric part of this strory, I fill that there is a strong misunderstanding for this main result.

I will expose in details the reason why I believe that the main results is wrong. Of course, the author may show that the misunderstanding is just on my side. However, if so, I really think that the paper would require a major rewritting, changing units to SI (centuries does not make things eaisier to understand when astrophysical time scales are involved), emphazising on the physical assumptions and phenomenon beyond the equations, reinforcing the logical link between equations.

Let us come to the central point. Without entering in the details of the Liouville equations, I can understand equation (24)

$$\Delta(LOD) = \frac{LOD_0}{\Omega I_{33}} \Delta h_3$$

.

I would rewrite it by using notations $_o$ for old and $_n$ for new

$$LOD_n - LOD_o = \frac{LOD_o}{\Omega_n I_{33}} \Delta h_3$$

which gives, after changing from LOD to $\Omega = 2\pi/LOD$:

$$\Omega_o I_{33} = \Omega_n I_{33} + \Delta h3$$

This equation just states that, assuming that there was no wind at time t_o, the total angular momentum at t_o ($\Omega_o I_{33}$) is equal to the total angular momentum now, which is the sum of angular momentum of the solid planet plus atmosphere at rest ($\Omega_n I_{33}$) plus the wind contribution h_3. Note that in I_{33} is not a "change of the maximum principal rotation inertia" as stated in the middle of page 7 but the monoment of inertia itself of the solid planet plus atmosphere.

I don't understand how the author goes from this equation to the estimation of $\dot{\Omega}_{\rm atm}$ (eq 25) (by the way, the explanation for the numerical result of 1293"/century is very vague). The conservation of angular momentum of solid planet plus atmosphere can be used to infer some modifications of the rotation of the solid planet. Howver, this requires that we know something not only on the atmospheric winds but also on there time evolution. By the way, an atmosphere with a very strong superrotation as Venus, may exchange zero angular momentum with the solid planet if, at the surface, easterlies balance westerlies. There are good reasons to think that this is the case on a long time average on Venus.

Indeed, it is very easy to see that the atmosphere, even with very strong winds, can not store a large enough angular momentum to explain such a drsatic change of Venus rotation. In fact, the principal moment of inertia of the solid planet is by about four to five orders of magnitude larger than that of the atmosphere. Even if the winds were changed globally by a factor of 100, it would only change the rotation of the solid planet by a small fraction.

So the atmosphere can not store enough angular momentum to modify the rotation of the solid planet beyond small fluctuations (those fluctuations can be of course larger than on Earth and Mars due to the larger atmospheric mass). The atmosphere could play a role on the long therm by transmitting a torque (for instance a gravitational torque exerted on a thermal tide) to the solid surface through surface drags.

If this major result of the paper is eronneous, I thing that the paper can not be accepted for publication, even after the major rewritting it would require.