N87-17622

JUPITER: NEW ESTIMATES OF MEAN ZONAL FLOW AT THE CLOUD LEVEL

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The presentation by Limaye is largely contained in a paper appearing in the special issue of *Icarus* (1986; 65, 335-352). The abstract of that paper is reproduced here:

Previous estimates of the mean zonal flow on Jupiter from Voyager images by Ingersoll et al. (1981, J. Geophys. Res. *86, 8733-8743) and by Limaye et al. (1982,* J. Atmos. Sci. *39, 1413-1432) showed good agreement in the locations of the easterly and westerly jets but differed somewhat in magnitude. Recent measurements of the high-speed jet located near 24 deg N (planetographic) latitude by Maxworthy (1984,* Planet. Space Sci. *32, 1053-1058) from high spatial and temporal resolution Voyager images indicate that both Ingersoll et al., and Limaye et al. underestimated the magnitude of the jet by more than 30-40 m s^{-1}. In an attempt to examine the differences in the magnitude of the Jovian jets determined from Voyager 1 and 2 images, a new approach to determine the zonal mean east-west component of motion was investigated. The new technique, based on a simple, digital pattern matching approach and applied on pairs of mapped images (cylindrical mosaics) yields a profile of the mean zonal component that reproduces the exact locations of the easterly and westerly jets between ± 60 deg latitude. Not only do the jet magnitudes but also the wings of the jets agree remarkably well from mosaic pair to pair. Further, the latitudinal resolution is five (mid-latitudes) to eight times (equatorial) greater than previous results. Results have been obtained for all of the Voyager 1 and 2 cylindrical mosaics. The correlation coefficient between Voyager 1 and Voyager 2 average mean zonal flow between ± 60 deg latitude determined from violet filter mosaics is 0.9978. A slight latitude offset (averaging + 0.15 deg) possibly due to navigation errors, is detectable in the Voyager 1 data. Independent cloud motion measurements in two high resolution image pairs (orange and violet) acquired from Voyager 1 cameras agree well with the average mean zonal flow for the fastest Jovian jet at 23.8 deg N latitude. Comparison with Maxworthy's results suggests longitudinal variations in cloud motions approaching about 20 m s^{-1} as well as some possible sampling problems. In particular, the jet magnitude is about 163 ± 9 (RMS) m s^{-1}, which compares well with 182 ± 10 m s^{-1} reported by Maxworthy. There is excellent agreement in the location of the peak magnitude as well as its shape.*

DR. STONE: I guess we can take one or two comments. Larry Sromovsky.

DR. SROMOVSKY: I know these results are hot off the press. It's a little bit premature to expect a complete study of a lot of these phenomena, but I'd like to inject a note of caution in interpreting these as time variations in the jet until some of the morphological possibilities are also explored. It'd be interesting to determine, given the vertical shear or estimated vertical shear from thermal gradients, what cloud top altitude change would be required to explain that difference. That would be one thing to look at, as well as the scale dependence of this kind of correlation method, which we've already looked at to some degree and I know suggests that this is a good way to measure the winds.

DR. ROSSOW: Sanjay, I guess I missed which of the three techniques you actually used.

DR. LIMAYE: All three techniques essentially give the same answer. Mostly I used the minimum cumulative absolute brightness difference method.

DR. ROSSOW: Since this is mosaic data, how do you remove the effects of the edges in the mosaic?

DR. LIMAYE: They did a very good job of blending in, and I didn't worry too much about it.

DR. ROSSOW: How did they do that?

DR. LIMAYE: It's averaged over overlapping map sections and within a map section the data from the image with the best navigation is kept. For the most part this blending worked quite well. However, there are problems. I'm not saying the mosaics are perfect. I forgot to mention that the brightness normalization also introduces some brightness aliasing over each map region (72 deg longitude). Ideally the longitudinally averaged normalized brightness from mosaics should be the same from mosaic to mosaic. But it seems to vary somewhat, a percent or so from mosaic to mosaic. It appears to be within a couple of percent at least. But these kinds of small problems don't seem to affect the correlation between line scans too much. As I said, I didn't really throw out any data except along that really high speed jet where, because the resolution is not very high, sometimes some correlations are not as good as others and you'll get the wind going 100 m s^{-1} the wrong way. Only a small number of results had to be thrown out because they were too far off from the "expected" or average results. You can also look at the meridional shears as a quality control check. It works--you can't use linear correlation, you shouldn't use linear correlation because the normalization is not perfect. If you look at some of the regions and do a line plot, you'll see the effects of the individual frames being normalized somewhat. And I use that for the difference and it seems to work very well.

DR. BELTON: Regarding the magnitudes of the eddy motions which you should now be able to measure, can you tell me what kind of magnitudes they are and whether you think they will now be statistically significant?

DR. LIMAYE: The variability in the zonal component, or the eddies determined relative to the new mean are not too different from their magnitudes determined in the past, except in some latitude regions where due to sampling problems the eddies may not have been previously estimated properly. Latitudinal shear within a given bin used in the past had not been thought to be much of a problem, at least for the zonal component. The problem comes when you try to correlate the u-component eddies with the meridional component eddies and then you run into navigation and sampling problems.

DR. BELTON: The difference is that you were getting, you showed it in your first slide, navigational errors in the RMS deviations of the meridional velocities--aren't the primed (meridional) velocities much smaller than that?

DR. LIMAYE: No, they're about the same order. The navigational contribution to the RMS variation about the mean meridional velocities does not appear to be large, probably on the order of about 1 m s^{-1}. So most of the eddies you see in the v-component are, within sampling problems, about the same magnitude as the RMS deviations. Another point to note is that meridional component variability is only about half that of the zonal component variablility, i.e., the v-eddies appear to be roughly only half as large as the u-eddies.

DR. STONE: They're about the same order.

DR. LIMAYE: The point to note is that this technique yields the longitudinally averaged zonal component. If you average over a few rotations then you have a zonal and time mean value for the average east-west component of motion. But the technique does work on smaller chunks of data, so there is a possibility of obtaining the "regional" average mean zonal component. Some experimentation is required to determine how small such regions have to be. And it would be a really useful technique to use with the Space Telescope image data for Jupiter and Saturn. I think we have the ability to monitor the mean zonal component over a long time period from a very small number of observations and we can do it efficiently at all (observed) latitudes. From the Voyager mosaics it has been possible to determine the zonal component profile between about 55-60 deg latitude. It appears somewhat unfortunate that the mosaics are in cylindrical projections. One reason the technique appears to fail at higher latitudes may be that the scan lines become too wide at about 55-60 deg latitude, so the actual resolution is quite poor. Of course you have foreshortening in the original images also. So because of unresolved features the results are not very reliable. If you had a different projection you could perhaps extend to higher latitudes, and I'd like to see some of that work get funded.

DR. ALLISON: Sanjay, you know there's a lot of longitudinal structure along the south edge of the equatorial jet. In particular there is this feature that we put on the conference logo which is called J1 in Reta Beebe's catalogue. Do you have any sense of the measurements being loaded on to that feature and being an important part of the temporal change of the equatorial jet at that latitude? Also, do you have any sense of wave dispersive character in any of the structure of the equatorial jet?

DR. LIMAYE: I have not examined the results and systematically compared them to morphology that you see, or don't see, in the equatorial region. I did look to see how sensitive the technique is to the input data. Not all the mosaics are perfect, there are some missing regions. Sometimes you'll get only about 72 degrees worth of data in a mosaic. And since I essentially ran the computer programs blindly without paying too much attention to the gaps or other problems with the mosaics, it is only when I look at the results that I notice some differences which ultimately depend on the image data. If the morphology changes, then the results can change too. And you do see some differences when some regions are missing or when a different filter image may be present in the mosaic. So if I don't take the whole 360 degrees there are some perturbations. So what that tells you is that there are longitudinal differences that appear to be significant. At this point, regarding the 7 deg jet, the morphology did change in that neighborhood over the Voyager observation period, but it is not clear how it affects the zonal component results. I'm not saying the zonal component results are absolutely accurate at that latitude, and I have to go back and study all the mosaics to see what happened. So I can't answer your question completely.*

DR. LEOVY: I'd like to ask another question about the eddies, the isotropy or the anisotropy of the eddies is an interesting diagnostic feature. Can you say anything about the relationship between RMS u' and the RMS v'?

DR. LIMAYE: Based on the mosaic results only I do not have any new information. Based on our old results from cloud tracking in similar mosaic images, I would defer my answer to Larry, who is going to talk more about eddies in his paper, which follows mine. One comment I would like to make is that we can take care of some of the sampling problems we have had in determining the zonal circulation at least. The mean meridional component remains a problem, but we could try a mixed approach. We could use the new mean zonal component, and say ignore the mean meridional flow, since it is difficult to obtain from the mosaic data due to its small magnitude. We can then digitally remove the mean zonal component from a mosaic by displacing individual scans in the longitudinal direction proportional to the mean zonal component at that latitude, so that at least longitudinally the clouds are "frozen" in space. This can eliminate one major problem in measuring the meridional component of motion, that of tracking a feature in a pair of images. If you remove the mean flow from one of them so they are essentially co-located, then all you're left with is the eddy component. And I think we might do a better job of measuring the eddies in both the zonal and meridional component that way than by the conventional method of measuring individual cloud motions in original images of Jupiter.

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*It now appears that the change in the 7 deg S jet is in reality a difference in the apparent magnitude of the jet in different color filters from which the mosaics were generated. The mosaics for the first 70 Jovian rotations used almost exclusively blue filter images. All subsequent mosaics (through rotation 112 for Voyager 1 and rotations 266-408) were generated from mostly violet filter images. The morphology in the region just south of the equator is somewhat different, but there is no doubt about the blue-violet difference as it is apparent at the exact time the input images change from blue to violet filter. (See the paper in *Icarus* for additional details.)
-Sanjay Limaye

DR. SROMOVSKY: I'm a little surprised that everybody keeps asking about eddy activity in measurements based on a technique which is inherently unable to see it, and further, that there are many theorists here who are interested in the stability of the jets (and this data is very good at showing curvature in the jet profile), but nobody's asked anything about that.

DR. INGERSOLL: I'd like to ask about that.

DR. LIMAYE: I did compute it, and as I said this is hot off the press. I didn't have enough time to prepare the last slide, but then I thought it would be obvious. I do have a very high latitudinal resolution so that makes the job somewhat easier initially but something that makes the job more difficult is that, it is not immediately clear over what latitude scale I should calculate the derivatives. We're very lucky that 1 degree turned out to be a reasonable choice three years ago when you folks and we computed both du/dy and d^2u/dy^2 in that the errors in u were small enough over the 1 degree bin so that we got a reasonable d^2u/dy^2 profile. In this case the standard error is holding at about 1 m s^{-1} for both Voyager 1 and Voyager 2 results but that is still not small enough for me to take mean meridional derivatives at the highest latitudinal resolution possible from the data. To reduce the error in the derivatives I have to increase the distance over which to calculate the derivatives. And if I increase the distance to a few scans, up to 0.25 degree away, the derivatives are still larger than previous estimates. All the westward jets are unstable by a long shot.

DR. INGERSOLL: By a factor of 2?

DR. LIMAYE: Or even higher. I wish I had a slide but I don't. But it's even higher.

DR. STONE: In defense of the theorists, you really need to know three-dimensional structure to answer anything about stability.