GENERAL DISCUSSION

Conti: I would like to address the variability question. As an observer of O type stars for 10 years and of WR type stars for a couple of years, the most important conclusion that one can make about variability is that it's very small. You have to look hard to see it. Earlier, Anne Underhill reminded us that Wolf and Rayet in 1867 described the spectra of three WR stars. These are as extreme an example of mass-losing stars as you can find and they looked more-or-less like they do 100 years later.

I would like to first talk about the observations given earlier by Dr. Vreux on variability: The star BD+40°4220 went from a very strong absorption to a very strong Hα emission in a day, an appreciable fraction of the binary period of 6.6 days. This kind of behavior is well known in other close binary systems and not very well understood, but I would suggest that this kind of extreme behavior has something to do with the double nature of the star. More observations on this system, which is quite faint, would be very, very important. As to the B supergiant, according to Jean-Marie, the scale on that spectrogram was the same as the scale on the BD star. The spectral change was only from weak emission to weak absorption. If this profile was in λ4686 (not only Hα) it would be called an O(f) type. It's a star where the emission and absorption processes in Hα are just about in balance and maybe changes can occur in a few minutes. The fact that it's central emission may be even telling us that it is more like a Be star rather than a star which has a P Cygni profile. This kind of variability is not going to be found in many stars because in most stars the Hα line is either very strongly in emission or very strongly in absorption.

Unknown Voice: Bull ...

Conti: The brickbats are finally arriving .... Anyway, I should point out that Andrillat and Vreux did observe three other stars and that they saw no changes. I would say in most stars, most of the time, even if you look very hard you see no changes above the 10% level. Substantial emission line variability is rare. Niemela and I have a recent paper on ζ Puppis where changes are pretty convincing: You pull the plate out of the hypo and it looks a little different than it did a few months previously.

I would like to give a brief summary as to what I believe are the variability timescales. Photographic methods only show changes above the 10% level: What we usually see is a "pimple" rather than a gross change of the profile. ζ Pup and λ Cep appear to have optical emission line variations in periods of days. That variability you can see when you pick up the plate and look at it (more than 10%). Their variability may be due to rotation but we don't understand the connection.

Delta Ori seems to have a time change in the UV lines of hours, mostly a disturbance in the P Cygni line of N V that propagated outward

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(a density enhancement according to York et al. 1977). There may be a lot more of these kinds of data found as more Copernicus observations are made.

And finally you heard earlier about HD 193793, the variable infrared source. This Wolf-Rayet star ejected a dust shell in the timescale of months. These are variabilities we know of now that are relatively large effects. None of these are periodic as far as we know.

Underhill: About observations, which Conti brought out, in connection with the points that Dick has brought up, the problem is to observe something significant. Changes are significant but perhaps very difficult to observe. Up to now we have only had the ground-based observations. It is our chief source of supply and will continue to be. Space observations give us some clues. You have to ask what portions in the star are visible in the available lines or continuum and how easy it is to observe changes. Now the infrared free-free continuum, and anything longer than 6000 Å in a hot star, is almost insensitive to the details of a model, so you can fit it with anything. (This is the reason I could get angular diameters.) Emission lines are more sensitive, different ones in different ways, to electron densities, temperatures and velocity distributions. Where these come from is Dick's problem and my problem is "how do you get the heating." Dick quite reasonably pointed out that one must start on the inside of the star.

Now to make this more evident I call your attention to the "pimples" Peter referred to earlier; if any of you have teen-age children, you will know that "pimples" can appear, they look very small, they cause great social problems, but they can be due to quite deep lying disturbances.

Conti: Yes, but the teen-ager is still a teen-ager.

Underhill: The really exciting thing is that our observations allow us to see, fairly accurately, significant photometric changes in line shapes and continua that we could not measure before. How can you account for them? The standard wind models are radiation-driven models. You cannot start a flow with radiation, but it may keep one going. Another interesting point which the UV line analysis always brings up is that the envelope temperature is 50,000 K or greater.

Zeta Pup (O4ef) is one of the very few stars whose effective temperature is near 50,000 K. Most of the stars known to have supersonic winds have effective temperatures of less than 32,000 K. You can look at any models: at 30,000 K, half the flux comes out longward of 1400 Å. Very little comes out shortward of 900 Å. I do not believe that radiation pressure by itself will be very important for getting the high velocities. Where does the wind originate?

Another really interesting problem is the difference in rate of mass flow between a Wolf-Rayet star, an O9.5 supergiant and a late O or a B0 near main-sequence star. There is a very large difference, a factor

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of 10^3 or more. Many of these stars have about the same effective temperature, 30,000 ± 1000 K. Typically M_{bol} of most of these stars differs by less than 2 magnitudes, see the following table which has been derived from the material which Divan, Doazan, Prévot-Burnichon and I reported here and from data in the literature on Wolf-Rayet stars.

Type of Star	Typical M_{bol}	Typical T_{eff}	$\dot{M}$ ($M_\odot$ yr^{-1})
Wolf-Rayet (not WN7 or WN8)	-7.7	31,000 K	$\geq 10^{-5}$
09.5 supergiant (α Cam, δ Ori A, ζ Ori A, HD 188209)	-8.8	30,800 K	$\approx 10^{-6}$
B0 main-sequence (κ Aql, λ Lep, 1 Cas)	-7.4	29,300 K	$< 10^{-8}$

How do these slightly differing values of M_{bol} and T_{eff} account for mass-flow rates differing by 10^3? There has to be another more significant factor. The only way that I can understand it is to postulate that the rate of mass loss is connected somehow with those subatmospheric velocities that Dick Thomas is talking about, or with magnetic fields. The proposed X-rays, which Cassinelli so beautifully showed could account for many of our observations, have got to come from somewhere. These effects may be only observed as "pimples" but they're defining almost everything we're observing.

Thomas: Peter has explained that he considers that if Hα goes from absorption to emission he thinks it a minor perturbation. Also in T Tauri, for sodium D to change from absorption to emission in a few minutes, he would say that's a minor change. For me it's a major thing in life.

Conti: Yes, Dick, but O-stars are not T-Tauri stars.

Abbott: I would like to answer the point raised by Anne Underhill concerning whether sufficient flux is available to drive the wind by line radiation pressure. Although I have not treated the problem dynamically, I can say that on the range of T_{eff} from 30,000 K to 50,000 K, enough flux and the lines needed to absorb it are theoretically available to get the observed mass loss rates.

The important parameter is not the flux but 2 times the flux. For example, in a 30,000 K star, this product peaks at ~1200 Å. For cooler stars, the radiation force presumably becomes progressively less important.

Morton: Did you consider just resonance lines, or do you include also subordinate lines?

Abbott: Both. However, the major contribution comes from lines for which there are no downward transitions. These are sometimes at relatively high excitations.

Morton: Some subordinate lines are not observed to have velocity shifts (i.e., they are photospheric only). They presumably would not drive the wind.

Abbott: Not necessarily. As an example, a T_{eff} = 40,000°K star with $\dot{M} \sim 10^{-6}$ M yr^{-1} at the point $v(r) = (1/2)v_\infty$ has 60% of the force coming from 100-200 optically thick lines, and 40% coming from the cumulative contribution of very many optically thin lines.

Sreenivasan: I have noted that essentially all the models proposed by the panel necessitate heating of the envelope in one form or another. Could anyone suggest the source of this heating?

Castor: One possibility is that there are radiatively driven sound waves that are unstable. These are then dissipated in the wind and produce the heating. However, this is highly uncertain.

Sreenivasan: Would including this increase the mass loss rate?

Castor: It would not. Essentially the mass loss rate is determined by the high velocity part of the flow, a balance between radiation force in the lines and gravity. In this region the gas pressure then is very small. Mass loss rates are not sensitive to the temperature, at least up to values of a few times 10^6 degrees.

Hearn: This, of course, is in the Castor model. In the hot coronal model, the mass loss is determined by the energy transport in the coronal levels. Similarly, I would think, for Dick's models.

Thomas: No, by convection in the subphotosphere.

Snow: I'd like to raise a somewhat different point about velocity laws. Most of the panel's models have velocity laws which are more-or-less single valued, as evidenced by the P Cygni profiles. However, some stars are observed to have detailed structure in some ions (narrow absorption components). These can be interpreted as "plateaus" in the flow, which are, interestingly enough, relatively stable with time. Can anyone comment on how this could come about?

Hearn: That's a very interesting question. I have no suggestions.

Lamers: If these plateaus are occurring where the radiation pressure is driving the wind, then we must conclude that the radiation force is changing. How could this happen? One possibility would be a spatial variation of the ionization balance. You might reach some critical points in ionization balance. It's a very interesting observation which does need some more work.

Cassinelli: In our model, since the X-rays are the cause of the ionization, I would not expect a strong dependence of ionization with radial distance.

Van Blerkom: Dick Thomas earlier raised the point about rapid changes observed in emission line profiles in a few cases. Since the timescale is a few minutes in one instance this implies something happening very close to the star. To me, this brings about a difficult conceptual problem. We observe P Cygni lines with violet-shifted absorption line velocities extending to 2000 km s^{-1} from line center. These must occur very many stellar radii from the star. If any of these P Cygni lines we've observed change substantially in the duration of a few minutes, I don't think it could be understood.

Thomas: This worries us very much also. However, we suggest that maybe this short timescale variability must mean the entire profile is formed close to the star (a "close coronal" model). Ann Boesgaard has shown me some P Cygni profiles of T Tauri stars. Out of five plates, one showed an inverse profile (redward displaced absorption).

Van Blerkom: So it is a problem.

Thomas: The entire topic is a problem.

Lamers: I think it's only a problem if the variations occur at large velocities. Within a few hundred km s^{-1}, it could well be "chromospheric" variations close to the star.

Hutchings: I'd like to clarify the observations. I also have extensive data on optical P Cygni lines and I have seen no evidence for short timescale variations in these lines. The timescale over which things are observed to change is invariably longer than the transit time for material to leave the surface and reach the terminal velocity. At small velocities close to the star, things can change quicker. I don't think one has to worry about this.

Thomas: What do you mean that you don't have to worry about it?

Hutchings: The observational evidence does not show short timescale variations occurring at large velocities, while the radius is substantial.

Noerdlinger: Some winds in QSO's have substantial variability in the absorption spectrum, which often seems short compared to sound speed travel time. I think Margaret Burbidge observed this in PHL 5200. Since the absorption feature is that material seen in projection, the projected cross section can be quite small compared to the distance to the object itself. So in stars, you could get a disturbance of the material at high velocity which changes in a small timescale. If the emission portion changes rapidly, then you might be in trouble.

Conti: The point I was trying to make earlier was that the short timescale absorption to emission variability that Jean-Marie showed occurs, in fact, in the center of the line. It's not in a P Cygni feature, and not at a high velocity.

Seggewiss: I have observed changes in P Cygni profiles in a WN star, which moved from -600 to -1100 km s^{-1} in a few days.

Hutchings: This kind of timescale is not really a problem.

Van Blerkom: Is the star a binary?

Seggewiss: No, it's not.

Thomas: I'd like to say again that any kind of variability needs some mechanism to bring it about.

Heap: I have been directed by Rudi Kippenhahn to a theoretical paper by R. Connon Smith [M.N.R.A.S. 148, 275 (1970)] on rotation, circulation currents, and shear. This could be a source of the heating and/or instabilities we have been discussing here.

Does anyone know anything about this?

Ebbets: I'd like to comment on this macroturbulence point. I spent about a year doing my Ph.D. thesis on the question of line widths in O-type stars. By doing a Fourier analysis of the profile, and also using the observed profile as is, it is easy to separate rotation from macroturbulent broadening. In O-type supergiants, with an assumed Gaussian profile for the macroturbulence, I found values of 25-30 km s^{-1}. In some main sequence O-type stars, I find only upper limits of some 13 km s^{-1} for macroturbulence.

Bidelman: τ Sco must have quite a low turbulence to judge from its spectra lines which are quite sharp.

Lamers: The photospheric turbulence is quite small, but that in the wind may be larger.

Stalio: I'd like to ask John Castor about his future work. Do you intend to put expansion and rotation in your wind?

Castor: Yes.

Sreenivasan: With respect to Sally Heap's question, with differential rotation such a physical input seems very natural.

Lamers: With respect to questions about crucial observations, I'd like to say that Cassinelli's coronal model works quite well on all the data. However, τ Sco still has a nagging problem: The ionization decreases outwards. Well, one could say that τ Sco is not a normal star

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and has a mass loss mechanism different from O and B supergiants. How unique is the observation that ionization decreases outwards? Do any other stars show this behavior?

Underhill: The distinction between ionization constant or dropping off outwards may depend critically on the density. The cooling depends closely on the aerodynamic solutions. The density could well drop off faster in the main sequence star, resulting in a decrease in the ionization in these objects, compared to supergiants.

Morton: Is it correct to say the models of Castor, Cassinelli, and Lamers are identical except for the ionization? All seem to have arbitrary ways to get the O VI lines. Dick Thomas is apparently waiting to see what he puts in.

Thomas: Castor's initial assumption was radiative equilibrium, whereas the initial assumption of the coronal models is non-radiative equilibrium and chromospheric-coronal mechanical heating. So there is a fundamental distinction. John has to put in an arbitrary parameter, which has nothing to do with a sub-atmosphere. We don't put in any such arbitrary parameters to get O VI once the sub-atmospheric non-thermal fluxes are fixed.

Morton: Your key point seems to be a connection of the mass flux to the sub-atmosphere.

Thomas: Exactly.

Castor: I am in there with the arbitrary folks. My model is like Henny's, except that I use a small elevation of the temperature to increase photoionization in place of a larger increase that makes collisional ionization important.

Noerdlinger: Dick Thomas pointed out that he believes turbulence must come from the subphotosphere. But, really, any kind of energy input would suffice. Is that correct?

Lamers: Yes.

Thomas: Let's be careful here. We are not talking about how you generate turbulence. A solar analogue clearly comes from the sub-atmosphere. Tony's coronal model is generated by a perturbation which is amplified by the radiation field. Castor needs some arbitrary heating, which he separates from the radiative force. I think we do need mechanical heating somewhere in any case (acoustic waves).

Underhill: Where do the acoustic waves come from?

Conti: I'd like to ask Joe Cassinelli about his coronal model. Is it true that your high temperature region is so thin in extent that it gives you no other observable features other than the required X-rays to ionize the wind?

Cassinelli: We checked to see if Fe XIV would be present but it is essentially not observable.

Conti: You wouldn't have any trouble with He I further out in the wind?

Cassinelli: No, the ionization balance is dominated by the photospheric field and by the cool temperature of the wind, so plenty of He I can be there.

Hearn: The crunch for that model will depend ultimately on its soft X-ray detection.

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