

*Clumping in Hot Star Winds*

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## General Discussion

Moderator team: A.F.M. Moffat, J. Hillier, W.-R. Hamann, S. Owocki

**Cassinelli:** Could you explain why clumping affects the red wing of the line? Does it have something to do with iso-velocity surfaces, i.e. more scattering of photons from the back side of the star?

**Hillier:** In WR stars and P Cygni stars the electron scattering wings (proportional to density) are always too strong in homogeneous winds when we fit the emission line strengths (generally proportional to density squared) with our models. The only way to reduce the strength is to introduce clumping. In P Cygni stars, with low outflow velocities, the wings are fairly symmetric about the emission line whereas in WR stars the wings are mainly seen on the red, as originally predicted by Auer and van Blerkom. While it is easy to deduce that  $f < 1$  (say 0.1) it is quite difficult to determine an accurate value. In O stars the electron scattering wings created in the wind are usually too weak to provide meaningful constraints.

As to your question the simplest explanation can be provided by assuming that we have an outflow, electron thermal motions are negligible, the photon is initially emitted near the core at zero velocity, and scatters off an electron moving with velocity  $v$ . To a good approximation the electron scattering process is coherent in the electron restframe, and hence the incident photon will be redshifted in the electron's frame. Since in the observer's frame the electron can be either moving towards or away from the observer, the photon will pick up an additional red- or blueshift. For a photon scattered in the same direction as the incident photon, the net shift will be zero, but for a photon scattered through 180 degrees the redshift will correspond to  $2v$ . In Wolf-Rayet stars the outflow velocities dominate over electron thermal motions, but in P Cygni the reverse is true. The same effect can be seen when we look at emission lines, arising in the central source, that are scattered off out-flowing dust in the Homunculus associated with Eta Carinae. The reflected emission lines are always redshifted.

**Hamann:** Regarding the question whether *all* hot-star winds are clumped, I want to nominate possible exceptions. The Galactic WN star WR 2 is the only one for which Cheneé and Moffat (this meeting) could not find any line-profile variability. The spectrum of WR 2 looks different from all other Galactic WN spectra because of the round shape of the

emission-line profiles that cannot be reproduced by any of our models. But, strikingly, the spectrum can be nicely matched by a model after convolution with rotational broadening of  $v \sin i = 1900$  km/s (see Hamann et al. 2006). I know that just a convolution is not an adequate model for a rotating stellar wind, but still this may indicate that WR 2 is a very rapidly rotating star close to its breakup limit. Maybe that this rapid rotation leads to a different wind dynamics where the line-driven instability cannot develop? Recently we realized that there is a counterpart in the LMC, Brey 6, which shows a very similar spectrum with the same round-shaped profiles, which are again reproduced with a rotationally broadened model spectrum.

**Schnurr:** Regarding the rotationally broadened lines of WR 2 and Brey 6: can you reproduce the spectrum by taking a normal line spectrum and diluting it?

**Puls:** Round-shaped profiles can be explained by optically thick shells, as already shown by John Castor in 1970 (MNRAS).

**Pollock:** Just out of interest: WR 2 is one of the apparently single WN stars that does have X-ray emission.

**Moffat:** If WR 2 is a fast rotating WR, could this be a candidate for a GRB since it is believed that approximately one in  $10^3$  WR stars can become a GRB. There we have one star among  $\sim 300$  WR stars known in the Galaxy.

**Owocki:** Well, it may be interesting that observations can be fit with such a huge rotation, but of course it makes little physical sense to have an outflowing wind simply be forced to have such a huge rigid-body rotation. If it were from a magnetic field, then there should be complex structure from regions of wind confinement, and surely it would become more oblate than spherical. So I am at a loss to know how to develop a dynamical model for this.

**Cassinelli:** Regarding the DAC effect: plateaus in velocity do not lead to the density increase that I am interested in. Is the DAC effect analogous to the weak shock in the wings of a bow shock?

**Hillier:** How universal are DACs?

**Prinja:** It now seems that in all cases of line-driven winds the spectral signature of migrating

DACs are seen in the UV resonance lines: this includes O stars, B supergiants, PN central stars and WR stars (WR24). The only time-series data of variable UV wind-formed lines that do not show DACs, are the disk winds from cataclysmic variables.

**Owocki:** As I noted above, it may be that DACs are the consequence of relative velocity plateaus that seem to occur naturally after kinks that form ahead of some slower moving compression. In this view, they would be a characteristic feature of structure in a line-driven flow. I did not know that disk winds from cataclysmic variables did not show DACs, and I will have to think why. Perhaps it means they are not primarily line-driven?

**Townsend:** When considering the role played by pulsation in producing clumped winds, it is important to remember that pulsational instabilities are rather universal in massive stars. The instability strips associated with the  $\kappa$  mechanism (either iron bump or deep iron bump), and with strange modes, cover most of the hot, high-mass HRD. Of course, we do not see a coherent pulsation signature in massive stars. But that could very well be, because the instability excites a few thousand pulsation modes, each a small amplitude, rather than a couple of individual modes to a detectable amplitude. The effectively incoherent velocity perturbations caused by these thousands of modes could be what we observe as macroturbulence and could seed the line-driven instability at the wind base, leading to clumping.

**Puls:** I just want to mention that Lefever, Puls and Aerts (2007) performed an investigation of periodically variable B supergiants in order to investigate the possibility of the  $\kappa$  mechanism. Interestingly, we choose 12 comparison stars which were not known to be periodically variable. After our investigation, it turned out that 9 of these 12 are very similar to the confirmed variables. This shows that at least among B supergiants pulsations are more the rule than the exception.

**Feldmeier:** I have a question to Rich Townsend: You spoke about the 2000 or so excited pulsation modes in a massive star. For acoustic modes there are the famous results by Poincare and others that when you feed an atmosphere with a spectrum of sound waves, there is a large amount of energy that accumulates at the acoustic cutoff period. And such a strong oscillation signal is indeed seen on sun. Would something similar happen for pulsations, i.e. that they pump energy into one single frequency? This could be interesting to understand the recurrence time of DACs.

**Townsend:** There are two cutoffs when we consider non-radial pulsation: one at high frequencies and one at low frequencies. For the sorts of modes that are unstable in massive stars, the frequencies fall well inside these cutoffs, and so in principle we

expect complete reflection of the modes at the stellar surface. However, this analysis (and all others to date) neglects the effect of wind outflow. The only exception is the PhD thesis of Steve Cranmer. Steve did a very insightful local analysis of wave propagation in an expanding isothermal atmosphere and showed that complete wave reflection is impossible. Some fraction of the wave energy will always leak out into the wind, possibly seeding the line-driven instability.

**Massa:** I have no explanation for the recurrence time of the DACs in  $\zeta$  Pup. They are what they are. In HD 64760, the “bananas” have roughly the same ionization structure. Perhaps Alex Fullerton can add something. He is the one who did the time series analysis.

**Fullerton:** Concerning the “spiral” modulations in HD 64760: Fourier analysis of the periodic modulations shows that lower ions lag the higher ions very slightly. This is consistent with higher ions being preferentially (but not exclusively) located along the leading edge of the spiral feature. So, in addition to the “every second spiral” ionization effect which Derck Massa mentioned, there seems to be a small ionization shift across a spiral feature.

**Hamann:** Concerning the DACs, we have shown in a widely ignored paper (Hamann et al. 2001) that the corotating interaction region (CIR) model cannot explain their remarkably slow frequency drift. A simple kinematical model reveals that the apparent acceleration of a CIR feature is even higher than the clump acceleration without rotation. However, the observations can be understood if the DACs are formed by structures which are travelling upstream in the wind with something like the Abbott speed.

**Feldmeier:** Twice the Abbott speed!

**Hamann:** Okay, twice the Abbott speed according to your newest result about the motion of kinks.

**Schnurr:** So far, continuum-driven winds have not been mentioned in this meeting. So, I wonder whether continuum-driven LBV outbursts produce clumps, as are seen in LBV nebulae?

**Townsend:** Stan Owocki has done quite a bit of work on this issue. For a star above the Eddington limit (from which we expect continuum-driven mass loss) the whole envelope is above the Eddington limit, not just the surface layers. The star will therefore try to drive the whole envelope off in a massive wind outflow. This will invariantly fail, due to effects such as photon tiring, and the outflow will fragment and partly fall back to the star. But now that we have a clumpy wind, it is possible for porosity to allow a steady (albeit non-homogenous) outflow. Owocki, Gayley and Shaviv (2004) demonstrated how porosity can modulate the continuum

radiative force, and allow the atmosphere of a super-Eddington star to transition from hydrostatically bound to accelerating outward.

**Cassinelli:** Regarding continuum-driven winds: Rico Ignace and I studied the IR line profiles from ISO to derive a velocity law of WR stars. The WR should be small,  $\sim 1R_{\odot}$  from evolution models, but  $\sim 10R_{\odot}$  from line profile wind analyses. It seems there must be a slowly accelerating outflow deep down and a transition to a rapid accelerating wind at larger radii.

**Ignace:** In describing the inner, slowly accelerating WR wind, the necessary conditions appear to be very delicate. Surely, WR stars have non-zero rotation. Do you have an idea of how rotation will affect this delicate force balance? I mean, in light of the fact that most WR stars are not intrinsically polarized.

**Massa:** Can I get a consensus of what a wind might look like? When Vink produced a grid of mass loss rates versus stellar parameters that was great since it gave observers a paradigm to test. It seems to me that when I talk to different theorists that I get a general picture of bow shocks and that these may be clumped in some way to make coherent features such as DACs and “bananas”. Is that correct? What does the panel think?

**Lobel:** The CIRs in our hydrodynamical models are large-scale density and velocity structures that produce slowly drifting features in the P Cygni profiles called DACs. For the DACs we modelled in HD 64760 there is no shock surface. There is only a kind of “smooth” density wave. If the radiative driving mechanism is unstable in these CIRs, they may fall apart in “clumps”, but this idea for the origin of clumps is not going to resolve the  $\dot{M}$  discrepancies because such density waves do not require large changes of  $\dot{M}$ . The hydrodynamic simulations show that changes of  $\dot{M}$  of less than 1% suffice to produce the CIRs. We need the CIRs to make the DACs observed in many O and B type stars, so the CIR should be a stable feature in the wind that is not easily disrupted. If it can be broken up, it should build up again very fast. The coherent CIR structure results from a steady mass injection at the base of the wind which I think is very different from the source of the clumps we are talking about.

**Blomme:** CIRs can survive a limited amount of breakup due to the instability mechanism. But they would not survive a large amount of instability. The DACs would disappear.

**Hillier:** I wonder whether DACs could be produced not by an outflow, but by some type of coherent phenomena (density wave). By analogy with a traffic jam, the pile up of cars at the traffic jam is determined mainly by local conditions, and not by where the cars are from.

**Feldmeier:** Referring to Joe Cassinelli’s bow shock model that was mentioned by Derck: I think this model runs into the problem first raised by John Hillier in his ROSAT paper 1993: once you have created clumps, the interclump density is much lower than the CAK density: the material is sitting in the clump and is lost between the clumps, where now the density is very low. This is indeed seen in the hydrodynamic wind simulations. Now when you have this thin material crashing into the clump at high speeds, it may create sufficiently high temperatures for X-ray emission, but it does not produce sufficiently large X-ray flux.

**Cassinelli:** I envision the high density of these clumps as arising at the contact surface of a driven wave, giving  $\rho = \text{Ma}^2 \rho_{\text{wind}}$ . This high density shell breaks up owing to, say, Rayleigh-Taylor instability and forms clumps onto which the wind collides producing the bow shock. The clump probably would have the same velocity from front edge to rear edge so there is no line driving force on it but rather only the continuum radiation force on  $\pi R_{\text{clump}}^2$  (and the drag force of the wind passing by).

**Hamann:** In your (Joe Cassinelli’s) sketch you have placed the bow shocks of clumps around their bottom side. This seems to be the common understanding, and is supported by the hydrodynamic modelling by e.g. Achim Feldmeier, where smaller cloudlets are strongly accelerated and ram from below into bigger, denser shells. However, the original idea by Lucy & White (1980) to explain the X-ray emission had been that the clumps are ploughing through a slower ambient wind, the bow shocks thus preceding the clumps. I would like to reconsider this scenario, for two reasons. First, in the Monte-Carlo X-ray modelling by Lida (Oskinova et al.) we found no reasonable fit of the observed emission if the emitters are always attached to the bottom side of clumps. Second, there is no reason to assume that the radiative acceleration of blobs is smaller than for lower-density material. Rather on the contrary, hydrodynamic plus non-LTE modelling by Götz Gräfener has shown that in denser material the excited atomic energy levels are more populated, while at low densities all ions tend to be in their ground state. Moreover, ionization degrees are lower in the denser blobs, again providing more line opacities. These density effects are not taken into account in the present time-dependent models by Stan Owocki and Achim Feldmeier. I can imagine that the densest clouds are actually accelerated more rapidly than the ambient gas of lower density, forming their bow shock on the upper side.

**Cassinelli:** Regarding the depth of formation of X-rays: In the bow shock picture, an important contribution to the width (HWHM) is from the sideways (lateral flow) past the curved front of the bow. Also the high ions form deep and that is okay in your

picture because the continuum optical depth is  $\lesssim 1$ . The O VII lines form at the larger radius, as you show.

**Ignace:** In terms of anticipating the structured flow expected from fully 3D time dependent simulations, Puls and I discussed the possibility that one might have something like the patch method (“1D sectors”) but with partially overlapping sectors (more or less). In this case there would still be clump collisions, but this would lead to “shredding” and a cascade of structure with lateral spatial scale. However, perhaps such effects will be governed by Rayleigh-Taylor instability if this is faster.

**Puls:** I just want to remind you that the observed “macroturbulence” in hot supergiants ( $3 v_{\text{sound}}$ ) has not been explained so far. We do not know where it comes from and what its relevance with respect to clumping is.

**Hamann:** David Cohen has argued that porosity effects may be negligible for the attenuation of X-rays from O stars like  $\zeta$  Pup. But look at our diagram showing the radius where the radial optical depth becomes unity. At wavelengths of the observed X-ray emission line, this radius lies at several stellar radii, even if taking the clumping-reduced mass loss rate of  $\log \dot{M} = -5.56$ . However, from the width of the X-ray lines, as well as from the expectation that the strongest shocks are located in the acceleration zone, one should expect that the X-rays are produced below a radius of about  $2 R_*$ . Only porosity can explain how these X-rays manage to emerge from the star.

**Leutenegger:** The opacity plot Wolf-Rainer Hamann showed for  $\zeta$  Pup is oversimplified. Using realistic opacities and line profile models shows that there is no inconsistency between profile widths (velocities) and the continuum optical depth of the wind.

**Pollock:** I have a way of reducing the line widths which none of you seem to like and that is to get a shock to randomize  $v_\infty$ . Velocity randomization is what shocks do. Then you share the velocity from one dimension into three and the component along the line of sight is naturally narrower than X-ray material moving with the bulk velocity of the wind. In  $\zeta$  Ori, the maximum velocity seen on both, the red and blue sides of the line, is about 80 or 90% of the maximum expected values (Pollock 2007).

**Hillier:** It is clear that when making inferences from X-ray analyses, and discussing the appropriateness of different results, that a detailed comparison of the adopted wind opacity must be done. This means verifying the cross-sections, adopted ionization structure, and abundances. This is particularly important because of the recent suggestion by Asplund that CNO abundances in the sun should be revised downwards.

**Hamann:** Is there still a P v problem and how can it be solved?

**Fullerton:** To the issue of whether there is still a P v problem: My hope is that we have all agreed that there is a problem; that the discrepancy is real. Beyond that, there are a large number of issues that remain to be resolved. These concern, e.g., the exact ion fraction of P v, the effect of X-rays and the extent to which “macroclumps” are present and optically thick to resonance-line radiation. Beyond an ill-defined feeling that an order of magnitude reduction in  $\dot{M}$  is probably too large, I have not sensed any consensus on the resolution of these issues. So my answer is that the problem remains; and in fact we do not know what the mass loss rates of O stars are to the desired accuracy.

**Puls:** I also think that there is a P v problem: As we have heard, P v can be made consistent with  $\dot{M}$  from other diagnostics, but the degree of the reduction in  $\dot{M}$  depends crucially on our assumptions. If we have microclumping, this reduction is of the order of ten, and if we include macroclumping, we might be able to have a reduction of “only” a factor of three. Due to the difficulties in the statistical modelling (porosity, vorosity etc.), we have to carefully test these models, agree on them and apply them to a wide range of parameters and diagnostics. Only then we will be able to say which  $\dot{M}$  reduction P v actually implies.

**Prinja:** We should note that it is not just a “P v problem” but also extends to sulphur and silicon, the latter is the case in B supergiants. All their diagnostics point to a need for low  $\dot{M}$ , compared to the smooth wind case.

**Gräfener:** As far as I see it there is no observational evidence against the low P v mass loss rates plus microclumping. We saw that the O star spectra are convincingly reproduced (Bouret, this volume) and also that the X-ray line profiles favor microclumping plus low mass loss (Cohen, this volume, and Leutenegger, this volume). Moreover, my own wind models with large microclumping factors nicely reproduce the low mass loss rates.

**Owocki:** Does Achim Feldmeier want to comment on the intercloud lengthscale?

**Feldmeier:** From your and my simulations, the biggest shells or shell fragments have a radial distance of order  $1 R_*$  for reasonable base perturbation periods. But you suggest that for isotropic clumps one would need separations of even a few  $R_*$ , and I cannot see how to achieve this. But for the pancake-shaped clumps a distance of  $1 R_*$  between clumps may be sufficient to explain the observed X-ray emission line profiles.

**Owocki:** Well, the pancake shape does help to increase the effective porosity, but only marginally.

And I would argue that dynamically they would be likely to break up into clumps with comparable lateral and radial size. Based on the simulations done so far, it seems that stochastic structure arising from the line-driven instability is likely to have too small a porosity length to explain the X-ray profile symmetry. Perhaps larger scale structure seeded by, e.g. pulsations, would work better, but that seems a less universal mechanism.

**Townsend:** I would like to emphasize that we can still get a porous wind with clumps that are close together. So long as the clumps are optically thick (i.e. "macroclumping"), all that determines the radiative transfer properties of the wind is the porosity length  $h = L^3/l^2$ , where  $L$  is the clump separation and  $l$  is the clump size. So, with small  $L$  we can still obtain large  $h$ , as long as  $l$  is small, too. Of course, with very small clumps, it can be problematic to ensure that the clumps remain optically thick. The density in the clumps (relative to the interclump medium) must be very large, which may be unphysical.

**Hamann:** I want to emphasize that the criterium whether porosity effects will make the medium effectively more transparent, does not only depend on the average clump separation  $L$ . The effect occurs when the optical depth across a clump becomes larger than unity, and this optical depth scales also with the density contrast:  $\tau_C = \kappa_D D^{2/3} L$ .

**Owocki:** Yes, assuming that the  $\kappa_D$  is the wind opacity for the *mean* density (with units of inverse length), then the quantity  $D^{2/3}L$  is just what I call the "porosity length". If  $l$  is the clump size then  $D = (L/l)^3$  is the inverse of what I have called the volume filling factor,  $f = 1/D$ , giving the porosity length  $h = l/f$ , and so clump optical thickness  $\tau_C = \kappa_D h$ . For a given mean opacity  $\kappa_D$ , the key to having a significant porosity effect is thus to have a porosity length  $h$  that is large enough to make the individual clumps optically thick. A key point, though, is that the (continuum) photon mean free path between optically thick clumps is also set by this porosity length  $h$ , and not, for example, by the clump separation scale  $L$ .

**Feldmeier:** I have a similar question to Stan Owocki as George Sonneborn, but even without rotation: What would you expect for the shape of the clumps? In your paper with Luc Dessart you find shell fragments, maybe similar to Joe Cassinelli's suggestion of clumps with bow shocks. I remember that a few years ago you made an interesting suggestion that the wind structure may look like a 1D spray or stream coming out of a tube of toothpaste, which is very curly and goes to all different lateral directions. Do you think such a spaghetti stream could stay coherent, or would it break up laterally into separate clumps?

**Owocki:** If I understand you correctly, this is essentially what Luc Dessart has done. But the key approximation has been to ignore the lateral radiation forces, which in principal may regulate the lateral scale at which Rayleigh-Taylor instabilities can operate.

**Runacres:** The moving box models described in my talk can certainly be extended to 2D: a first toy model would be to examine whether clump collisions in 2D still produce density enhancements predicted by the 1D models, or whether instabilities may inhibit such density enhancement. A further idea would be to build on the Dessart & Owocki (2003, 2005) models and extend them to large distances ( $\sim 250 R_*$ ).

**Owocki:** Yes, this would be interesting. For the radial direction, the computational box could have the quasi-periodic boundary conditions developed by the pseudo-planar model, but also use real periodic conditions in the lateral direction. In this way, clumps would have another degree of freedom and not be forced to repeatedly collide as in 1D models, and this would certainly be more realistic.

**Hamann:** Stan Owocki has argued that clumps move strictly radially, because high lateral velocities would have been observed. I want to contradict here, as I think that lateral velocity components cannot be distinguished observationally from the component of the radial motion projected on the line of sight.

**Blomme:** The material essentially moves out radially. Only close to the surface of the star do you see some effect of rotation (through angular momentum conservation).

**Ignace:** Comparing bow shocks for radiative vs. adiabatic cooling: for hypersonic flow, our simulations (Cassinelli et al.) give bow shocks that are roughly parabolic. Raga has an analytic solution for a similar scenario but with strong radiative cooling and the bow shock (downwind) is much more "confined", implying less lateral flow.

**Owocki:** Backfalling clumps can arise in MHD models when material is trapped in closed loops. Infall can also occur if the wind becomes overloaded, for example from rapid rotation or temporal modulation in base driving. But in ordinary hydrodynamic models of CAK winds with or without instability, we see no evidence of infalling clumps.

As for lateral velocity, the analyses of WR emission bumps by Carmelle Robert and by Sebastian Lepine seem to show that the bumps have smaller width at line center than at line wings, which is consistent with a much smaller velocity dispersion in the lateral than radial direction. Indeed, this feature is reproduced quite well in Luc Dessart's "patch" models with *no* lateral velocity. Physically, it seems reasonable that the lateral velocity should be much smaller since the radiative driving is primarily radial.

Until one accounts for lateral radiation transport, the main lateral force is from gas pressure, which can only give a lateral velocity dispersion up to the sound speed. So from both theory and observations, it seems the radial velocity dispersion is greater than that in the lateral direction.

**Cassinelli:** At the very high temperatures of 10 to 15 million K at the peak of the bow shock, radiation losses are far below the cooling runaway. So the adiabatic shock structure should be appropriate.

**Hamann:** Before I spoil any relations, I want to emphasize that all of you who are doing the wind hydrodynamic modelling: Stan Owocki, Achim Feldmeier, Joachim Puls and others, are doing the best calculations that are feasible today, and even more than that (*laughter*). Nevertheless, we should always remain critical and compare the model predictions with the observational evidence. I feel that there are still a couple of severe discrepancies which should keep us alert that the models might not be fully adequate yet:

1. Mass loss rates from CAK-type stationary wind models are said to be in very good agreement with observed mass loss rates from OB stars. However, when  $\dot{M}$  is revised downwards by more and more clumping, I wonder why there is not too much wind driving now predicted by the models.

**Puls:** The predicted mass loss rates became also lower because of the revision of stellar parameters from line blanketing!

**Hamann:** Okay, but at least if mass loss is revised downward further, by more than a factor of three compared to the unclumped H $\alpha$  diagnostics, you will get into troubles. But I have three more points:

2. The velocity law predicted by the models approaches the terminal speed sooner than is observed. The best fits are obtained with a gradual further acceleration extending to larger distances from the star.

3. In the time-dependent hydrodynamic models, the shocks and clumps develop in the acceleration zone and slowly die out at large distances. The observations seem to indicate (for O stars at least) that the clumps already exist close to the sonic point, and survive into the radio region.

4. Clumping properties have been shown in this workshop to be amazingly universal, from WR stars to OB stars, even to the low-mass central stars of Planetary Nebulae. If clumping is caused by the line-driven instability: would you not expect a significant dependence on the parameters, i.e. on mass loss rate and luminosity, which vary over orders of magnitude?

**Puls:** Actually, the observed O supergiant mass loss rates from H $\alpha$  are a factor of two to three larger than the stationary hydrodynamic predictions. This factor has already been mentioned by Puls et al.,

Markova et al. and Repolust et al. (2003, IAUS212; A&A 2004) and clumping was invoked to explain the difference. If the presently observed  $\dot{M}$  are decreased by a factor of three, there is no contradiction to models. If they are decreased by a factor of ten, then there will be a big discrepancy.

**Vink:** With respect to empirical mass loss rates from H $\alpha$  versus theoretical line acceleration calculations: I think one should be aware that in parallel to the realization that clumping may affect the H $\alpha$  mass loss rate in O stars (with  $f_{cl} \sim 4 - 5$ , i.e.  $\dot{M}$  H $\alpha$  reductions of  $\sim 2 - 3$ ), it has also been shown that line blanketing reduces O star temperatures (Crowther, Martins etc.), which affects the empirical luminosities and counteracts the clumping effect. As a result, at this point in time there is no systematic difference between moderately reduced  $\dot{M}$  (by a factor 2-3) H $\alpha$  rates and theoretical O star predictions by Vink et al. (2000), Pauldrach et al. (2003), and Krtečka & Kubát (2004). If the *real* clumping factor turns out to be  $\gtrsim 5$ , then we produce too much driving with our current line force models.

**Fullerton:** This is a question for the atmosphere modellers: how robust is the result that the clumping must start very near the photosphere? I understand that it appears to be required to get the best fits. But we here also heard that clumping is implemented in the simplest possible way. Could this result be an artifact of the implementation of clumping? It is a crucial issue.

**Najarro:** Implementation is important. In our current prescription we are using two or three parameters and this means that we cannot easily derive a precise radial value for where clumping starts. To do so, a step function should be implemented. Concerning the region where clumping sets on, we are obtaining two sorts of regions, either very close to the photosphere (close to the sonic point) or at much higher velocities ( $\sim v_{\infty}/3$ ).

**de Koter:** Someone mentioned “free parameter heaven”. Indeed, in principle, the radial stratification structure of clumping offers many free parameters; one  $f_{cl}$  at every radius. So it is essential that in constraining  $f_{cl}$  the diagnostics should significantly overlap. Note, for instance, that the UV line formation region would only overlap with, say, 10% of the mm-flux zone. Overlap needs to be stronger. Is this the case?

**Hillier:** I have several comments.

1. Observations by Moffat of  $\zeta$  Pup show that the variability starts at low velocities in that star (and that the velocity law is consistent with  $\beta = 1$ ).

2. The X-ray modelling finds that the X-ray emission starts at  $1.5 R_*$ , and hence that the shock structure is well developed at this radius. This clearly implies that the clumping and wind structure must start at

lower radii.

3. With smooth winds ( $f = 1$ ) many difficulties in modelling O stars were encountered. The adoption of the volume filling factor approach allowed many of these difficulties (i.e. P v resonance line, O v 1371) to be simultaneously alleviated. While there are discrepancies, these are at a much smaller level than previously.

**Schnurr:** Referring to what Paco said about the possibility to constrain the properties of wind clumping. The same is true for WR winds, i.e. we are probing different regions of the wind, and can tell what the clumping has to be in different parts of the wind from lines, electron scattering wings, and the spectral energy distribution (Spitzer/Herschel).

**Puls:** I guess all modellers agree that we need a stratification of clumping. The important point, however, as raised by Wolf-Rainer is whether clumping starts close to the wind base. In WR stars, this question cannot be “directly” answered, because one cannot see so far down. If, indeed, clumping sets in very deep, the standard description of a void inter-clump medium might influence the result (as long as photospheres are not completely clumped as well).

**Massa:** In regards to lateral coherence, I have never seen a feature simply vanish at intermediate velocity so, at least for DACs, there is no evidence for small lateral structures in UV wind line variability.

**Vink:** To assess the role of clumping on stellar evolution I do not think we should look too much at GRB, as they are really quite exceptional (high rotation, low  $Z$ , binarity?, etc.). A factor of two reduction in absolute  $\dot{M}$  will already have an effect on canonical stellar evolution models (e.g. Meynet et al. 1994).

**de Koter:** Concerning Jorick’s remark how a factor of three reduction would affect massive star evolution: I do think that a factor of three decrease would prevent Galactic stars to spin down significantly on the main sequence. If so, partial cumulative projected radial velocities for SMC, LMC and Galactic stars should be similar for young clusters, if the initial  $v_{\text{rot}}$  distribution does not depend on the environment.

**Ignace:** We observe GRBs, and some are associated with supernovae, and so connected with massive stars. To what extent is the frequency of GRBs dependent on mass loss issues discussed here for single stars, or can all GRBs be explained with effects from binary star evolution?

**de Koter:** I am no expert in binary evolution. However, it is discussed that the binary fraction may be less at lower  $Z$ . If so, the possibility of having GRBs as the result of single star evolution becomes more important, perhaps even essential.

**Vink:** Regarding stellar evolution. It is crucial to determine the precise offset between UV, X-ray diagnostics and the  $\rho^2$  diagnostic from H $\alpha$ . If it is only a factor of two to three, the agreement with theory is good and stellar evolution models that use the Vink et al. (2000)  $\dot{M}$  rates would not be affected. If the *real* empirical mass loss rates are a factor of five to seven lower compared to H $\alpha$ , and a factor of two to three lower than theory, then stellar evolution will be affected quite dramatically.

**Gräfener:** In our recent WN analyses (Hamann et al. 2006) we found that the luminosities of the Galactic H-free WN stars are all below the Humphreys-Davidson-limit, while H-rich WNL stars (WNLha) tend to lie above it. This could mean that the H-free WN stars are all post RSG/YSG stars, and there would be no need for large O star mass loss to explain their existence. According to our hydrodynamic models the WNLha stars have large masses and are thus still in the H-burning phase.

**Hillier:** Analyses of wind-blown bubbles have had problems with the energetic for years, and one possible solution is that mass loss rates have been overestimated. With the lower O star mass loss rates it would be worthwhile exploring this issue in further detail.

**Weis:** I think it also depends highly on how large the bubble is. We distinguish between single-star and total OB association bubbles. For single stars the wind might not be the driving mechanism of the bubble, but only the adiabatic expansion.

**de Koter:** I showed before that for SMC, LMC and Galactic stars our results on clumping in O stars do apparently not depend on metallicity. My question is, does one expect clumping to depend on  $Z$ ?

**Owocki:** In the line driven instability, the growth rate scales with the mean acceleration divided by the ion thermal speed. As such, its formal “direct” dependence on metallicity  $Z$  should be the same as the line force itself, i.e. as  $Z^{(1-\alpha)}$ , where  $\alpha$  is the CAK power index. But that is just the growth rate, and in practise the effect on wind structure is more subtle, depending on the nonlinear evolution, as well as, for example, the overall density and its role is setting the cooling length. Generally, as one lowers  $Z$ , the smaller number of optically thick lines will make the instability less effective, and the reduced density for radiative cooling will tend to make structure stay hot rather than cool into dense clumps.

**Vink:** Regarding the  $Z$ -dependence of the clumping, for O stars, Mokiem et al. (2001) give some hints that the clumping factor may be the same for the SMC, LMC and Galaxy. For WR stars, Marchenko et al. (2007) show a  $Z$ -independent behavior of moving sub-peaks.

**Sonneborn:** What is the limit of “no metallicity dependence”? 0.1 solar? 0.01 solar?

**Hillier:** While clumping is prevalent in all stars, it is not clear that its characteristics are the same. Evidence in LBVs and P Cygni stars seems to indicate that while present, it is less important. Metallicity might provide a  $Z$ -dependence since it affects  $v_\infty$ . Furthermore metallicity affects the cooling of shocked X-ray gas, and hence the shock structure. For example, cooling of shocked X-ray gas will be much more efficient in galactic stars as compared with SMC stars.

**Hamann:** With the hydrodynamic modelling of WR winds, we have found (Gräfener & Hamann 2007) that low-metallicity stars can still drive a strong wind, if they are close enough to the Eddington limit. Even at one thousands of the solar metallicity, high mass loss rates (but only low wind velocities) can be achieved. The helium and CNO lines are obviously capable to provide enough opacity.

**Puls:** Nobody has discussed the consequences of clumping for the emission from wind-wind collisions. Any comments?

**Pollock:** Reducing the mass loss rate in WR 140 will correspondingly increase the plasma cooling lengths which are already of the order or greater than the dimensions of the shocked region.

**Ignace:** In some WR and OB star winds, the OB wind is dominated by the WR wind. So in those cases, reductions in  $\dot{M}$  for the OB winds might not impact the wind collision interaction *except* perhaps how clumping in the WR wind might alter radiative braking effects (e. g. in V444 Cyg).

**Hillier:** The problem with using LBVs, and A stars, is that H can recombine in the radio region, and this

can provide another mechanism for providing structure. Such effects are seen in P Cygni.

**Cohen:** I would urge Joachim, John Hillier and Wolf-Rainer to include detailed X-ray cross sections in their wind models. We need  $\kappa_\lambda$  to better than a factor of two.

**Leutenegger:** The He ionization balance strongly influences the soft X-ray opacity of O star winds.

**Pollock:** I think there is another clumping independent mass loss rate diagnostic which is the residual X-ray flux in eclipse. This presumably simply counts the number of electrons in the wind.

**Sonneborn:** The physics of O/WR mass loss may be relevant to other astrophysical problems where mass loss or outflow is important: the early universe (Pop III stars, reionization of H and EUV escape fraction), AGN/QSO outflows, galactic winds, young stars, and planetary system formation. We should look how what we have learned this week applies or can be extended.

**de Koter:** George Sonneborn very nicely summarizes a number of high profile fields in astronomy that may be expected to show great interest in our knowledge of the mass loss properties of stars. For instance, he refers to T Tauri stars. One should certainly add the Herbig Ae/Be stars, i. e. young intermediate mass stars with protoplanetary disks. Eventually the stellar radiation field clears these disks from gas remnants, transforming them into debris disks. Mass loss may play a role in these processes, though one should not expect to obtain constraints on  $\dot{M}$  from, say, protoplanetary disk versus debris disk statistics.