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Porosity vs. Mass-Loss-Reduction:
issues related to OFH2006 and its impact on our work using quantitative model-fitting of line profiles to jointly constrain porosity length and wind optical depth

We have been doing some model-fitting experiments, using the Chandra MEG spectrum of zeta Pup, to explore the extent to which porosity length and wind opacity can be discerned via the observed line profile shapes. The philosophy is to use the usual statistical analysis techniques to put formal constraints on the parameters h_inf and tau_star. Even if the two cases (reduced optical depth (or mass-loss rate) and transport through a porous wind) cannot be discerned, this approach will at least allow us to rule out large regions of h_inf – tau_star parameter space and also allow us to quantify the porosity lengths required to accommodate the literature mass-loss rates (or any other specific mass-loss rates).

In the course of doing these model-fitting experiments and writing up the results, it has become clear that we need to at least consider including some of the following analyses: a porosity length formalism for anisotropic porosity; detailed calculations of the atomic opacities for the cold wind (to go from tau_star values to mass-loss rates, among other things); a quantitative analysis of the porosity characteristics of real winds (including but perhaps not limited to a presentation of Luc's latest 2-D LDI simulations and x-ray line profiles calculated from them). We also may gain a significant amount of additional discriminatory power by including the very long XMM RGS observation of zeta Pup in the analysis.

While working on all of this, Oskinova, Feldmeier, and Hamann's paper on the analysis of line profiles in four Chandra spectra (incl. zeta Pup) in the context of their fragmented wind model was finally accepted. It has changed in some significant ways from the submitted version that has been available on astro-ph since March. The purpose of the document you are reading is (a) to summarize my impressions of OFH2006 and (b) consider how we might craft our current paper to address some of the issues raised by OFH2006.

I have already laid out, above, the outline of our manuscript. You'll note that it has become rather broad – including Luc's simulations and opacity calculations (using Joe MacFarlane's codes). I would like to invite both Luc and Joe to work on this with us.

You can see the current version of the manuscript at:

http://astro.swarthmore.edu/~cohen/projects/porosity/draft5.pdf

There are other related documents in that directory, including some of Stan's notes about anisotropic porosity in the context of porosity length formalism, some of which will find their way into our manuscript. I'll refer to these and some other documents in that directory later in this document, and some of them are explicitly referenced in the draft manuscript – see, especially, iso-vs-aniso-por.pdf and lida-vs-stan.pdf and wind_opacity_issues_sep06.pdf.

Next, let me very briefly summarize my overall impression and a few important points/criticisms of OFH2006. I'll follow with a more detailed critique, and then close with implications for our work.

OFH2006: "High resolution X-ray spectroscopy of bright O type stars," MNRAS, in press; astro-ph/0603286 (v.4)

As we've all discussed before, this group has had some nice insights about x-ray transport through a fractured medium (as they call it). Feldmeier has done very nice work and had many important insights related to the LDI simulations. Hamann has done good work on wind modeling (spectral, UV/R-T), which should transfer over nicely to this work. Oskinova's approach to data leaves something to be desired. But the weird thing is that OFH2006 really doesn't exploit Hamann's expertise/modeling capabilities in a productive way, and Feldmeier's insights about the LDI and the wind structure it produces seem to be only very narrowly exploited.

My brief summary of the paper is as follows:

- It still doesn't present any model fitting. In fact, if anything, the modeling is even more tied to a single picture of wind structure (they've eliminated the one free parameter from their model, and now impose a fixed clump release frequency), so there's nothing to fit. And they still don't do any statistical analysis of goodness of fit between their model profiles and the data. Have a look at, e.g. Fig. 8. Are these fits even "good"?
- The wind opacity calculations show a strong wavelength dependence (see Fig. 5), which they invoke as support for their

claim that the highly clumped wind dominates the transport/profile effects. The logic is that they don't find a trend in profile shape with wavelength, but since their opacities have a strong wavelength dependence, the effectively gray opacity from optically thick clumps comports with the data. However, have a look at those opacities. The hallmark of photoelectric opacity is the presence of ionization edges in the wavelength-dependent opacity. One for each abundant ion of each abundant element. Fig. 5 shows only a single K-shell edge. This isn't right. I have summarized the situation, including other, similar opacity calculations from the literature in a separate document: http://astro.swarthmore.edu/~cohen/projects/porosity/wind_opacity_issues_sep06.pdf.

- I think they've fixed up their anisotropic porosity formalism, but I am still skeptical of a few things. Primarily… have a look at the iso-vs.-aniso profiles in Fig. 16. How in the world can they be so different? As before, there isn't enough information in their paper to figure out exactly how they calculate their profiles (and I'll remind you that Lida was forced to admit to Stan last Spring that the code they used to calculate their models didn't use the exact formalism that was in their manuscript, at that time, anyway…) I'm not sure the problems in the wings of their profile models identified when we did code-code comparisons (see: http://astro.swarthmore.edu/~cohen/projects/porosity/lida-vs-stan.pdf) has been fixed.
- Of course, the biggest conceptual problem with OFH2006 is their lack of a convincing argument – or much of any argument at all – that the big porosity lengths they find/assume are realistic (they do reference Dessart and Owocki 2003, but without any mention of the small clumping scale).

I will elaborate on some of these points and address a few other topics in more detail now.

The Intro is pretty straightforward, though I have some minor objections to a few things – parroting the Waldron/Cassinelli claim of very low formation radii from f/i ratios; asserting that it's inconsistent to assume mass-loss rate reduction due to clumping but to then use a smooth wind x-ray profile model (without any mention of the clump scale) – this comes up later in the paper too; it's as if they want to conceal from the reader the key difference between clumping and porosity; a difference of which they are well aware. There are some re-worded phrases in the intro (and elsewhere) that indicate that the referee wouldn't let them claim that they'd "fit" the data ("…thus we do not infer the model parameters from line fitting, but instead…" and "With all model parameters defined [fixed], we model each line and compare it with the observation.").

They have changed their methodology from the submitted version, and now have eliminated the one free parameter of their model, the clump release frequency. They now take "the time interval between subsequent clumps passing the same point…we adopt the wind flow time." So, they've set h_inf to R_star.

As an aside, related to Nolan's talk, I'd point out that the trend he showed – x-ray hardness correlates with spectral subtype – is not evident in these four stars (see Fig. 1).

They adopt a relatively high mass-loss rate for zeta Pup – 4.2e-6 (from Puls et al. 2006) – see Tab. 2.

They present the f/i ratios of three He-like species on pp. 5-6. They use the relatively lame, but recent, modeling of Porquet et al. (2001). They find no need to invoke small formation radii (see Fig. 3), but don't call out Waldron and Cassinelli or, for that matter, even reference Maurice's recent paper on f/i ratios (which I shared with Lida before it was accepted). Hmmm… They now show Mg XI in Fig. 2 as a representative complex, instead of the Ne IX which, as I pointed out to Lida, is hopelessly blended with various iron lines.

They emphasize that their model conserves mass (2nd paragraph of sec. 5).

The PoWR wind models are discussed at the end of sec. 5. It's strange, since they clearly are quite detailed, including diffuse x-ray emission in the ionization balance, and they go to the trouble to match the FUV spectrum of zeta Pup. They even discuss the chemical composition's effect on kappa. But… looking either at Tab. 4 or Fig. 5, you can easily see that they don't have a mixture of elements in there. Or if they do, they don't include K-shell edges of anything except one ionization stage of nitrogen. If you look at any of the other calculated x-ray opacities in the literature, you'll see multiple edges in this range, not just from C, O, etc. but from various ionization stages of each abundant element. (Ionization shifts the K-edge to the blue, as it reduces the shielding of the nucleus by valence electrons.) Again, see http://astro.swarthmore.edu/~cohen/projects/porosity/wind_opacity_issues_sep06.pdf and note how the confluence of edges conspires to keep the opacity relatively flat between ~0.5 and >1 keV.

They claim at the beginning of sec. 6 that they are using the same model and code as Oskinova et al. (2004). They use R_min ~ 1.5 Rstar. Emission is considered to be produced only up to 5 Rstar, but the transport (and attenuation) goes out to 300 Rstar. They emphasize that the mass and solid angle of each shell fragment (i.e. clump) is conserved.

The model comparisons to the zeta Pup data are presented for six lines in Fig. 8 on page 9. Note that the big change in the model, from the original version of this paper – namely, fixing the parameter n_o so that the time between passing clumps is a flow time, which reduces the effects of porosity, as it lowers the clump spacing by about a factor of 5, compared to the earlier version of the manuscript. It's not clear that this model provides very good fits to the zeta Pup data. Look especially at the wings of the lines. The model – data agreement is somewhat worse in the other stars (see Figs. 9 and 10).

Note at the end of sec. 7.2 they discuss contamination by blends, and warn about the 15.01 Fe XVII line and a nearby iron line at 15.08. Looking at coronal spectra with narrow lines and also the atomdb APED database/line-list, it seems unlikely that the 15.08 line is a significant problem (a peak emissivity of only 5% the 15.014 line, and even that level is only realized in higher-temperature plasmas – the line at 15.08 is from Fe XIX.

They point out that our tau_star values for zeta Ori (derived from model fits to data) imply a mass-loss rate reduction of a factor of 20 – but using *their* opacities. Fair enough, for now.

You'll note on pp. 10-11 that they actually try adjusting a few parameters (Ro for some of the longer-wavelength lines in zeta Oph – see Fig. 13), though they don't do any actual fitting.

They list Lx/Lbol values (and related quantities) in Tab. 5, and comment on the energy budget as well as the relatively low Lx/Lbol values, and wonder if lack of binarity could explain this. However, it seems much more likely to me that they are integrating Lx over a different photon energy range than has traditionally been done (ROSAT response went down to almost 0.1 keV; Chandra to only 0.4 keV). By neglecting the softest x-rays, they underestimate Lx.

In sec. 8.1 they discuss the sensitivity to M-dot (see Fig. 14), and go on to discuss the overall line flux as well as the profile shape. However, interpreting these results depends on assumptions about wind opacity and, of course, they assume a specific, fixed fragmentation frequency. So, these results are highly model dependent. I'm also surprised that Achim would allow the statement at the end of this section to pass – that a comparison between absolute line fluxes from the hydro simulations and the observed line fluxes would be a "very sensitive tool."

Similarly, in sec. 8.2 they do a limited parameter study of beta. But again, there are several very restrictive assumptions in these models. Maybe these parameter studies are instructive for showing how profile morphologies change with a given parameter as *all the other parameter values are held constant*, but they certainly don't tell you anything about what parameter values might provide acceptable fits (and they're intentionally deceiving in this respect).

Sec. 8.4 is quite interesting. OFH lay out a case that isotropic porosity is much more like atomic porosity and that anisotropic porosity is fundamentally different. They claim that spherical clumps naturally still give skewed and shifted profiles, without addressing the issue of clump scale, or porosity length, in the context of isotropic porosity. Look at Fig. 16 – I'm surprised at the contrast between the isotropic and anisotropic profiles. We should try to reproduce this. …but have a look at the Lida-vs-Stan.pdf suite of profiles. There's already a Lida-generated profile for h_inf=1 (Rstar) and tau_star=10. Is their tau_o the same as our tau_star? I don't see it defined explicitly in their paper, but I'm interpreting it as tau_j with j=o – the radial optical depth through the clumped wind, which should be…greater than our tau_star, I guess, since it accounts for the non-constant velocity, but, I can't seem to find it defined explicitly in the paper. In any case, it looks nothing like the anisotropic profile they show in Fig. 16 for any but the optically thin case.

Action items for us:

We should reproduce a few of their models, shown in the paper, with our own code. We can check for consistency and also quantitatively assess how the fits compare to actual best-fit models. We can also get a better handle on the systematic deviations in the line-wings, and try to figure out if they've fixed the problem in their code which we provisionally identified.

One specific comparison we can do is of the isotropic vs. non-isotropic models they show in Fig. 16. This would also give us an opportunity to re-examine our own “piece-wise” anisotropic model, revisit the (1/(1+tau)) vs (1-e^-tau)/tau formalism and re-assess how hard it would be to do the double numerical integral. See Maurice’s notes from late August: http://astro.swarthmore.edu/~cohen/projects/porosity/maurice_anisotropic_porosity_28aug06.txt and the two figures in that same directory: tpzlarge.eps tpzsmall.eps.
We really need to get our own anisotropic porosity formalism and analysis out there. For now, I think it makes sense to incorporate it in the current manuscript, but as this effort progresses, we should consider breaking it off into its own paper.

Finally, we also see the need to model and analyze the x-ray opacities in the winds of hot stars. Again, let’s move forward with this effort in the context of this paper, but keep an open mind about breaking that effort off, too, into a separate paper.