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response to referee's report - MNRAS: MN-06-0094-MJ

Reviewer's Comments:

I found this a very interesting paper to read, with some pleasing results and conclusions. It consists of a detailed analysis of high resolution X-ray data for Zeta Orionis taken with Chandra. This work is important in that Zeta Ori has been used as something of a counter-example to a developing prevailing view of the origin of the X-ray emission in single O stars. This paper demonstrates that, on closer inspection, Zeta Ori is in fact a good example of this viewpoint rather than a counter-example. The paper also highlights some of the outstanding issues with the basic model, and has some broad comments about the possible explanations for them.

The paper is well written, coherent and concise and needs relatively little modification, and as such the paper certainly merits publication.

My comments are detailed below, the vast bulk are minor, involving rewording or action to improve the clarity of the paper or diagrams. The only comment of substance concerns the beta law (see below).

p.1 As a tongue-in-cheek comment, in the first paragraph you cite a record number of papers on O star winds, but manage to miss out a reference to the classic CAK paper. I will leave that to the authors conscience.

Well, the list of citations in the first paragraph (Lucy & White 1980,...) relates to the line-force instability, not the line-driven winds themselves. We have now added a sentence clarifying that this general model exists within the context of CAK winds (and referenced the classic paper). This change induced us also to break off the end of that paragraph - on alternative wind-shock scenarios - into a new paragraph.

p.2 (and elsewhere): "XMM" should be referred to as "XMM-Newton"

OK; fixed.

p.2 "beta velocity law" - this concept is introduced without reference (see also comment below).

OK. We've now referenced Lamers and Cassinelli's textbook. This is such a standard concept now (within the field) that this reference seems appropriate (and indeed, although many concepts in that

particular textbook are themselves referenced back to the original research papers, this particular one is introduced in Ch. 2 without reference). It is introduced at the beginning of Ch. 2, with several variations described, and representative values and the corresponding run of $v(r)$ are plotted in Fig. 2.1. on p. 10.

p.2 "overall X-ray properties of Zeta Ori..." provide a reference for this.

We've added Cassinelli and Swank's 1983 paper on Einstein SSS observations of this star and Berghoefer, Schmitt, and Cassinelli's big survey of ROSAT all-sky observations of OB stars to the reference list, and referenced WC2001 as well as these papers where we quote the overall X-ray properties. Note that CS1983 discuss the basic properties (including L_x) and conclude that there is a harder component to the X-ray spectrum ($T_x \sim 15 \times 10^6$ K). WC2001 quote lower temperatures (and indeed the lack of any significant S XV emission in the Chandra spectrum indicates that there isn't much plasma with $T > 10^7$ K); the harder component in the SSS data may very well be due to that instrument's relatively poor spatial resolution (and thus blending with nearby sources). The ROSAT all-sky survey indicates L_x/L_{bol} slightly higher than 10^{-7} and a quite cool X-ray temperature (about a quarter keV). These references are also invoked in the discussion section.

p.3 The authors refer to Waldron and Cassinelli (2001) as the "discovery paper for Zeta Ori". I would reword or clarify this statement - this paper presented the first high resolution X-ray spectrum for this star.

Absolutely. This has been reworded.

p.3 (and elsewhere) Lyman alpha and Halpha - these should be $Ly\alpha$ rather than Ly_alpha etc.

OK, fixed.

p.4 (and elsewhere) - a couple of mathematical expressions used within a paragraph are rather small and maybe better off on separate lines (example of s on p.4 and τ on p.6).

OK, we have now typeset these two equations on separate lines. We considered doing the same for a few more equations - including the definition of "x" on p. 3 and some of the ones near the top of p. 5, but decided against it. We will heed the advice of the copy editor, though, if it's deemed we should do the same for more of these equations. Finally, we note that the equation for τ_{star} is already prominently typeset on its own line when it is introduced (on p. 4), so perhaps this

second statement of its definition is unnecessary (we have left it in for now, though, pending further input from the referee, editor, or copy editor).

p.5 Concerning the assumption of a $\beta=1$ wind law. This is the assumption that was adopted in previous work on Zeta Pup (Kramer et al. 2003). Have you investigated how important an assumption this is - does using $\beta=0.8$ (or $\beta=2$) make any substantial difference. My gut feeling is that it is not a major issue (for a plausible range of β), but would appreciate some comments on this.

This is a good point. We are, unfortunately, constrained by some practical issues. The optical depth integral requires numerical solutions for non-integer β values - see Owocki and Cohen (2001). In that paper, we compare some $\beta=1$ models to (rather extreme) $\beta=3$ models - see Fig. 2. The qualitative differences in the profiles are not huge, even in those cases. If you look at Fig. 2.1 in Lamers and Cassinelli's book (see above), the $\beta=0.8$ - which is the standard assumption/fit for O star winds - differs very little, in terms of simply the velocity profile, from the $\beta=1$ case we assume here. The slightly more rapid acceleration of the $\beta=0.8$ model would likely move R_{\min} inward, but just a very small bit (given the $\sim 10\%$ difference in the velocity in the two models near $1.5 R_{\star}$) - and certainly less than the error on the derived model parameters.

In any case, the actual velocity law of the x-ray emitting plasma is not known (independently) and very well may be somewhat different from that of the bulk wind (although not likely too different - see the hydro simulation snapshots we reference elsewhere in the paper).

Perhaps we should add a note (a footnote?) to sec. 4, explaining why we use $\beta=1$. We have not done this at this point, but would be open to doing so if the editor or referee recommended it.

Finally, we note that the new work by Puls et al. (2006), which we now discuss and reference in Sec. 5, shows β values for their sample of O giants and supergiants that are even closer to unity than the standard value of 0.8 (see their Tables 8 and 9, for example).

p.10 Table 3 - I would be inclined to bolster the caption for this table, defining a bit more what the columns are.

Good idea. We have added some explanatory text in a new note at the bottom of the table.

p.10 Table 4 - to be picky, you are not consistent in use of the zero in the errors.

OK, I think we've corrected this now by removing the leading zeros in three instances. We chose to exclude leading zeros in this table but keep them in Table 3 because of the small and crowded typesetting necessitated by having different upper and lower uncertainties for most of the values in Table 4.

p. 9-11 Quite a few of the diagrams are too small on the version I received, with the labels almost illegible. They should be larger (or redrawn). Also you do not specify the units of R_{\min} in the caption or the label of Fig. 6.

We agree, and have now made most of the figures span two columns. We feel that the issue wasn't that the labels were inherently too small, but rather that the figures themselves were too small. In most cases, this is best remedied by having the figures span an entire page. If the copy editor (or the referee!) feels differently, we will remake figures on a case-by-case basis.

Regarding the second point, we've now indicated the units (in the y-axis label) of the R_{\min} plot in Fig. 6.

Other changes:

- We added a sentence to the abstract pointing out the nearly order of magnitude reduction in wind optical depths – compared to those derived from published mass-loss rates and assuming a smooth wind – inferred from our modeling.
- In the first paragraph at the top of p. 2, where we discuss coronal models and their possible applicability to massive stars, we've added two sentences which include references to Mullan and MacDonald (2005) and Mullan and Waldron (2006). We note that this second paper actually used the initial assertions of symmetric and unshifted x-ray emission lines in the Chandra grating spectrum as a primary motivation for applying dynamo/coronal models to zeta Ori.
- Removed one of the Feldmeier et al. references from the third paragraph of sec. 5
- In the left-hand column on p. 6, where we discuss the expected τ_{star} values, we have made a small modification to the quoted opacity values in Fig. 4 of Cohen et al. (1996).

- In the discussion of clumping and mass-loss overestimates (where the Bouret et al. and Fullerton et al. papers are referenced), we've added a reference to and discussion of a new paper by Puls et al. on this subject.
- We've added new references in the discussion section (Naze et al. 2002, Cooper et al. 2004, Freyer et al. 2006) which provides yet more evidence for mass-loss rate overestimates. This additional text, plus that described in the previous point, have induced us to break this paragraph into two.
- In the right-hand column on p. 6, we've added a third Oskinova, Feldmeier, & Hamann reference – to a paper submitted to MNRAS which elaborates on the 2005 conference proc. analysis. We'd be fine not referencing the conference paper, but since the journal papers hasn't yet been accepted (to our knowledge) and isn't yet posted to astro-ph, we feel more comfortable (and that it is more fair to those authors) if we reference both. (This discussion now spills over to p. 7)
- We've broken up the penultimate paragraph of sec. 5, and added a very brief discussion (last two sentences of what is now the third-from-last paragraph of that section) of the trend seen in x-ray temperature (using the shock velocity statistics as a proxy) in numerical simulations of the line-force instability.
- And we've added two new sentences to the end of the (now) penultimate paragraph, further addressing the issue of clumping's effect on line profiles.
- In the second paragraph of the Conclusions section, we've removed the word "unphysical" from our description of coronal models as applied to O stars (but kept "ad hoc").
- We corrected one of the grant numbers in the acknowledgments section.