This paper presents a new and uniform analysis of the Chandra/HETGS archival spectra for 15 single O-stars, using emission line profiles to determine the mass loss rate and the minimum radius of X-ray line formation. Most of these stars have some published results, but this appears to be the first time all of them have their fundamental wind parameters determined from the same line formation model in a uniform way. Furthermore, an attempt was made to justify a common (and usually unstated) assumption that continuum opacity is a constant function of radius for the relevant wavelength range. Given that the mass loss rate is an important value for stellar and galactic evolution, and that the minimum radius of X-ray formation is a key parameter to know in embedded wind shock theoretical models, this paper represents an important synthesis of some of the best high-resolution O-star X-ray spectra available to date and is worthy of publication.

The presentation would be clearer with a bit of restructuring, especially regarding the opacity vs radius discussion. There should be more comparison to other measured mass loss rates, not solely what is expected theoretically, because the theoretical rates have typically been higher than measured for these stars. Figure 4, which is key, would be better cast with "interesting" axes: while \tau_\star is fit, it is \dot M which is desired.

Some key references are missing.

These are major issues, to be described in detail below, which should be addressed before this is published. Other minor questions or suggestions will follow.

Major issues:

1)

Most of Section 3.3 provides detailed description of opacities and the dependence on radial position in the wind in order to justify factorization of \kappa from the integral in Eq.3. This discussion is important since this assumption is commonly made in the literature, but neither explicitly stated nor justified. The original formulation of \tau_\star was by MacFarlane et al (1991; ApJ380:564) (their \tau_0), in which the integral was a delta function since they were concerned with a thin, expanding shell. This convention has been adopted since, and generalized to radially extended winds, but often left implicit.

Section 3.3 would be much better supported (and perhaps shortened) if Section 3.1 were rearranged and the radial dependence made explicit. Eq.3 should come before Eq.2, and should be written with \kappa_\nu(r) shown within the integral:

$$
\tau_\nu(p,z) = \frac{\dot M}{4 \pi R_\star v_\infty}
\int{ dz\, \frac{ \kappa(r) R_\star}{r^2 (1-R_\star/r)^\beta}}
$$

Eq.4 should come next, followed by simplyfying assumptions, namely, adoption of a mean \kappa, leading to Eq.2 (which logically comes last). While the opacity is traditionally written simply as κ, a more formal indication of the mean and dependence would be acheived by using $\bar\kappa_\nu$ as an obvious reminder of the radial average and frequency dependence. (It could be written this way once, then "henceforth κ for simplicity...").

Given this, it would be much clearer that \tau_\star includes an approximation, and Section 3.3 will justify that approximation.

A further remark could be made regarding analytic solutions. Owocki and Cohen (2001) noted that the integral in Eq.3 is analytic for integer \beta. This is a great advantage for numerical computation of profiles, and motivates the choice of \beta=1 (which is also physically justified). It seems that the integral is also analytic if \kappa(r) is polynomial in r. The authors might wish to verify this and perhaps include it in an appendix. If it is so, they could also take an explicit \kappa(r) dependence and more rigorously show the effect (though they make a good case that in the wavelength regime of interest here, the radial dependence and resulting effects are much smaller than uncertainties in the data).

2)

Figure 4 provides key results, specifically the mass loss rate determination from \tau_\star. However, \dot M is rather implicit, being hidden in the scale factor on the opacity curves. Since \dot M is derived from each \tau_\star, and since there is no new information in each opacity curve (they are the same but for a scale factor), it might be more instructive to plot wavelength vs \dot M, and the mean \dot M (essentially ratio residuals). This would take out the "uninteresting" opacity trend and clearly show the quality of the \dot M determination. A label could give the theoretical mass loss rate, so the y-axis scale would not have to accomodate the sometimes disparate theoretical vs measured rates. Such a plot would also be more commensurate with Fig.5, which shows the other key parameter directly, R_0, the minimum radius of X-ray formation.

3)

Some relevant references are missing, or mis-attributed:

MacFarlane et al (1991) should be cited in the introduction or early in Section 3, since they first described the profile from an expanding shell.

The work of Waldron and Cassinelli (2007; ApJ668:456) should be cited, since they analyzed 12 of the 15 X-ray spectra included here. In particular, they derived a minimum radius of X-ray formation from the f/i ratios (under the assumption of local, not distributed, emission). There should be some discussion of how the current R_0 relates to their values.

Section 3.2 cites Leutenegger et al (2006) for UV photoexcitation of f/i. This should instead refer to Blumenthal, Drake, and Tucker (1972; ApJ172:205), with the former as an application for some of these specific spectra.

Section 3.3 states that

> "... this correlation between \tau_\star and \kappa was not noted in the initial analyses of Chandra grating spectra, it has recently been shown for the high signal-to-noise spectrum of \zeta Pup that if all lines in the spectrum are considered ... then the wavelength trend in the ensemble of \tau_\star values is consistent with the atomic opacity (Cohen et al. 2010a)."

There were, in fact, very early correlations noted between the radius of \tau=1 and wavelength (Cassinelli et al 2001; ApJ554:L55), which is closely related to the wavelength vs \tau_\star shown here. Kramer, Cohen, and Owocki (2003; ApJ592:532) explicitly related the "commonly quoted radius of optical depth unity" (their wording) to \kappa in their eq.3.

4)

While it is good to provide the uniformly determined theoretical mass loss rates for comparison, there are also values determined from data (UV or X-ray, typically) for many of these stars. Some attempt should be made to quote these prior results (without going into an extensive review). E.g., Table 3 could contain an additional column with \dot M from literature (e.g., as in Waldron & Cassinelli 2007).

Minor Comments (in order of occurrence):

[notation: page, Left- or Right-hand column, ~line number]:

p.2, L, 13

The discussion about types of diagnostics is a bit vague, regarding line vs simultaneous broad-band fits. The point seems to be that there is emissivity structure (temperature distribution --- the emission measure, and radial density structure), affecting (primarily) continuum emission and relative line strengths, and there is wind structure affecting (primarily) line profiles (and the latter are of interest in this paper).

Next paragraph: "this X-ray diagnostic": "this" should be made explicit: the line profile? the parameters derived from line profile modeling?

p.2, L, 49: use of "hide" (in quotes) should be explained, and quotes removed since there is nothing suggestive about it. If a clump optical depth is large, then not all ions can see photons, and you can't count ions using photons.

p2, R, 21: typo: is f_cl 3.5^2 or sqrt(12)?

p3, L, 41: "lower sensitivity" needs to be qualified. HEG is more sensitive than MEG below about 3A (though it is of no significance for the current investigation)

p.4, L, 49: "just the normalization": is this equivalent to the line flux? then for consistency with the following text, give the parameter a name: "the normalization, f_{line}, ..."

p.4, R, 39: "... propotional to \kappa, the atomic": add "bound-free opacity" (or continuum opacity) (to clearly distinguish from a line opacity).

p.5, L, 13: "normalization factor", is the line flux? (i.e., not the continuum normalization, which is frozen?)

p.5, L, 23: Regarding line-of-sight velocities, was xi Per velocity significant because of the magnitude, or good signal in the line? Perhaps line-of-sight velocities could be included in Table 1.

p.5, L, 37ff: were any wavelengths free parameters? were line groups constrained to have fixed wavelength offsets?

p.5, R, 49: The paragraph beginning "The actual wind abundances" is awkwardly phrased. It sounds like "uncertainties in and updates to" our knowledge will affect the *observed* profile. Instead, something like:

"Elemental abundances determine the wind opacity and hence, in principle, affect the line profile. Abundances are somewhat uncertain and represent a source of uncertainty in derived parameters. However..."

p.6, L, 35ff: Please comment also on dependence of the LDI mechanism on metallicity (and the velocity law), which, if important, would introduce a non-linear dependence in the determination of \dot M (i.e., one affect of metallicity is a scale factor; can the other be ruled out?)

p.6, , 30: in the figure caption, "is therefore even narrower, in an absolute sense" would be more simply put as:

"is unresolved, and therefore narrower than the instrumental profile which is about 0.02 A (FWHM) or ... km/s ...."

p.7, L, 50: it is not clear how the extra opacity is applied. The function range is 0 to 1. Is it added, times some scale factor?

p.7, R, 49: while it is fairly obvious from context and the citation to Asplund, it wouldn't hurt to qualify abundances as "solar photospheric".

p.8, L, 33: Better to say that the lines are unresolved and thus show the instrumental profile, which is close to Gaussian.

p.11, R, 54: Regarding point 3: here is where other mass loss rate determinations are relevant, as well as theory.

Also, it might be noted that while current X-ray results rely on the profile fitting, there is also information in the overall X-ray emission normalization not accounted for here (i.e., if you know geometry and emission measure you can get N_e and another X-ray-derived \dot M) and that is where the complementary approach of broad-band fitting can be useful.

p.12, R, 31: Q: What is the relevance of the phase of 9 Sgr being such that the primary has zero orbital radial velocity?

p.15, R, 30: Regarding systematic errors, this term usually refers to unknown effects, which if you knew what they were, would be removed. "Systematic effects" (instead of errors) is a fairer term here, since they represent known terms which can be included and assessed with some effort.

p.15, R, 58: The argument about consistency is extremely weak, to the point of being meaningless. The only way to know if the consistency is significant is to repeat the analysis with a different velocity law, in which case, different but consistent values might also have been obtained. If that could not be done, then one could conclude something about the kinematics. Is there any verifiable, quantitative conclusion which can be made?

p.16, L, 62: In what sense are the determined mass loss rates "reliable"? One would need an independent assessment, i.e., the truth, against which to judge. Or a theoretical study showing that fits with one model against simulations with another can definitively show that models are inconsistent. Perhaps simply omit the word, "reliable". (Or is the intent of the statement that rates are consistent from line-to-line for a given stellar spectrum?)