General Response:

We are grateful to the referee for a very detailed and helpful review. The referee’s major concern is that our work largely just reproduces W96. We think this concern is based on a misunderstanding – for which we accept full responsibility – regarding the nature of our simulations. The key point is that – unlike W96 – we do **not** treat radiative heating and cooling in the optically thin limit. Instead, we treat the interaction of the irradiating X-ray flux with the disk atmosphere/wind fully self-consistently, i.e. the ionization state of the plasma, the attenuation of the radiation field and the heating of the plasma by the radiation are all taken into account. The only radiative effects we do not deal with are (i) heating associated with the radiation field produced by the wind itself; (ii) radiation pressure. Thus our simulations really are “radiation-hydrodynamic” – our coupled radiation+hydro operator splitting algorithm implicitly solves the equations of RHD in the relevant limit. Since attenuation effects can be very strong in the acceleration region of the outflow, we believe our simulations are significantly more realistic than those in W96 and represent a genuine advance.

However, we fully accept that (i) we failed to make this key aspect of our simulations clear in the text, and (ii) we did not sufficiently explore how our results compare to those obtained by W96. The explanation for (i) is that

we tried hard not to burden the present paper with technical details already discussed in our last paper, but in the process ended up omitting key information as well. With regards to (ii), we had originally considered having a full section of the paper dedicated to a comparison with W96. However, we felt that this was too technical and unwieldy, but – here again – we apparently threw out the baby with the bathwater. In the revised version, we hope we have got the balance right.

*The authors present numerical simulations of thermally driven winds from low mass X-ray binaries, examining the effects of varying the X-ray luminosity from 0.04 to 0.6 L\_Edd.  Direct comparisons are made with recent observational data and synthetic iron absorption line profiles are computed, and this serves as a valuable contribution to the field.  However, I cannot recommend the paper for publication without some major revisions.  The main reason is simply that the effects of varying the X-ray luminosity were studied thoroughly by Woods et al. (1996; hereafter W96).  While the authors cite this work, it is not to acknowledge that W96 carried out an even more detailed study of luminosity dependence.  Of the three results quoted in the authors’ abstract, results (ii) and (iii) were in fact obtained by W96 but no credit was given and the authors made no attempt to confirm other key results of W96 or to draw meaningful comparisons with analytic theory as W96 did.*

As noted above, the revised version explains the distinction between the RHD simulations carried out here and the HD simulations carried out in W96 much more carefully. We hope this makes it clear why our results are new and relevant, even where they broadly agree with those of W96. In addition, we now compare to and reference W96 much more thoroughly, including in the abstract.

*Abstract  
1. Results (ii) and (iii).    
See the abstract and Section 5.1.2 of W96, where result (ii) was first obtained.   See Figure 29 for their version of your result that ‘observable absorption features are preferentially produced along high-column equatorial sightlines”.  Please revise your abstract and text accordingly.  Discussion as to whether or not your results agree with their Fig 29, which shows significant columns for high ionization stages, should be included in your Section 4.3.*

We have included a brief comparison with W96s predictions of column density – although in section 4.1 where it seemed to naturally fit with the discussion of our figures showing EW variation with angle. We have also added a line in the abstract to make it clear from the start that our results echo earlier work.

*Table 1  
Please add values of the critical luminosity (eqn 4.4 in W96) to ease comparison with that work.*

Done.

*Section 2  
1.  Relation between thermal instability and wind launching  
Based on my reading to Higginbottom & Proga (2015), I do not understand the statement “In order to attain the high temperatures necessary to launch a thermally driven winds, material in the the disc atmosphere must therefore reach and exceed ξcool,max. Such gas becomes thermally unstable and heats up rapidly.”  This paragraph overall reads like thermal instability is necessary to launch winds, yet cases C and D in the 2015 paper had no thermal instability and yet thermally driven winds were still launched.  Please clarify.*

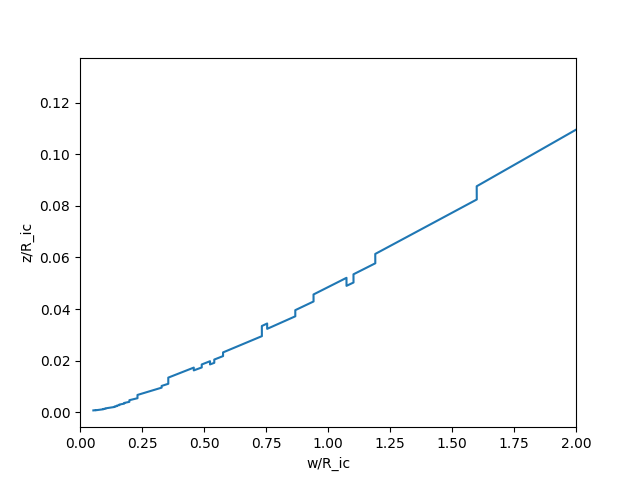
It is true that, as shown in Higginbottom & Proga (2015) thermal instability is not a necessary requirement for a thermal wind. However, the SED we use in our simulations does give rise to a thermal instability, and so the analysis which gives rise to a maximum ionization parameter on the `cool branch’ is applicable. That is, we can be sure that gas on the cool branch will not be outflowing. We seek to only model the outflow, and so our choice of density (and hence ionization parameter) is intended to ensure we minimise the amount of gas in this state. It is not possible to know a-priori what the ionization parameter will be (because of absorption between the central source and the cell in question). We have included some text in the paper to clarify that the instability is not a requirement for the flow, rather it is a result of the SED used, and thus must be dealt with.

*2. Relation between midplane conditions and launching surface.  
The discussion on what to choose for the ionization parameter on the disk midplane is confusing, especially the statement “it also avoids the inclusion of static material in the simulation”.  Accretion disks are optically thick and therefore not governed by an optically thin gas photoionization parameter.  Is this merely a computational requirement to avoid having to treat the disk physics separately?  Please clarify why the ionization parameter inside the disk must be related to that in the wind.  I would think you are simply placing the tau=1 surface for X-rays at the midplane  – a razor thin disk approximation.*

We agree with the referee that this section was not perfectly clear. The referee is correct that the setting of the midplane density is designed to avoid including the bulk of the hydrostatic disk. We have clarified this. The optically thin ionization parameter is ONLY used for setting the mid-plane density – using it for this purpose is conservative, in the sense that it guarantees that we always include the full acceleration region in the flow (at the cost of also including some hydrostatic material). We hope our rewording has made this clearer.

*Section 3  
1. Convex disk structure  
“As a result, the vertical height at which ξcool,max is reached increases with radius. Since this height marks the effective boundary between the static disc and the outflow, the net effect is a thin, slightly convex disc structure.”  
This is hard to visualize.  Please include a figure showing this structure.*

Below is a figure showing the location of this boundary in w/z coordinates for the L\_edd simulation – which shows the clearest convexity. The convex nature is clearest at small w. We are happy to develop this figure for inclusion in the report if the referee feels this is necessary.



*2. Corona  
Please assess the following result quoted in the abstract of W96: “We find the radial extent of the corona to be independent of luminosity, as predicted by BMS, extending out to about 0.25 R\_IC; this is a direct consequence of Compton heating.”  Do you reproduce this key result?*

We do not see the same detailed behaviour – although we did not set out to model the corona in detail. Our simulations are intended to capture the fast outflows, which we expected (in part through looking at W96) to be mainy generated outside the coronal region.

CK: This needs some more detail, plus a note that this is now discussed in the paper itself.

*Section 4.1  
1. Mass flux densities  
W96 found excellent agreement between their mass flux densities and the analytic theory of Begelman, McKee, & Shields (1983) – see Figures 10 and 16 and the text on page 780.  The theoretical values are quoted in Section 4 of W96.  It may be useful to also make a direct comparison with theory, but I recommend at least trying to assess why the profiles in your Figure 2 are qualitatively different than the ones in Figure 16 of W96.  In particular, their abstract quotes an exponential rise in mass flux density in the corona, independent of luminosity.  Do you see this also?*

We think the referee might have misread the y-axis label on our Fig 2 or their Fig 16. Our figure shows the normalized mass-loss rate per unit area as a function of **polar angle**. Their figure shows mass-loss rate per unit area as a function of **radius along the disk plane**. The two can’t really be compared directly.

CK: In an ideal world, we’d make a figure like their Fig 16 for one or more of our simulations and show/discuss this (at least in the response, possibly in the paper).

*2.  Quasi-spherical nature of the disk winds  
It is not true that ‘any thermally driven outflow should be (quasi-)spherical’.  Your argument that thermal expansion should lead to quasi-spherical outflows because there is ‘no intrinsically preferred direction’ trivializes the role of pressure gradients in shaping the flow.  As stated by W96 (pg. 779), “at disk temperatures, radial force balance involves primarily gravity and rotation.  Upon being heated, the pressure gradient becomes large and the flow turns radial and becomes compressed.”  The paper by Font et al. (2004) revealed that quasi-spherical flow is no longer obtained when the density profile along the disk is less steep than 1/r^2 – the flow is more biconical.  \*

We accept that we were perhaps a little too casual here. What we were trying to get at is that, in the far-field regime – i.e. at large distances – we expect the flow to become increasingly spherical/radial, as non-radial pressure gradients become weaker. This seems consistent with Font et al, for example, who note

“*In addition, both models appear to be approaching spherical symmetry at large radii (the density contours are nearly spherical while the streamlines are approximately radial). This is an expected result. Figs. 4 and 5 compare the results for the PDW model flow against the other two cases and the Parker wind solution. “*

We have modified the text to make it clear that we are only talking about the large-distance limit here.

*Section 4.2  
1. Constant wind efficiency result  
This is interesting.  However, it depends on the accretion rate, which is not modeled.  Please provide the formula for determining M\_acc, as I did not find it in H18, which is referenced, and mention this is not a self-consistently determined ratio.  Also, to further establish this result, the viscous timescale should be compared with the disk depletion timescale, M\_disk/Mdot, since density and therefore M\_disk changes with luminosity.  If at higher luminosities matter is lost from the disk faster than it is brought in, then the disk will be unstable and this result no longer meaningful.  This would alter your discussion on the comparisons with observations.*

This is all correct. The mass accretion rate is simply related to the luminosity via L=\eta \dot{M}\_{acc} c^2, where \eta is the mass accretion efficiency. This is mentioned in the first para of section 4.2. There is no formula beyond this one for determining Mdot\_acc. We take the referee’s point regarding the viscous timescale and hence the disk depletion timescale. However, this is not directly related to our work, since the luminosity is set by the accretion rate in the **inner disk** and onto the compact object. By contrast, the part of the disk we are modelling – and from which the wind arises – is the **outer disk**. Our models implicitly assume that the rate at which mass is supplied from the companion is Mdot\_wind + Mdot\_acc, of which Mdot\_acc arrives at the compact object. We have clarified this now in Section 4.2.

It is true that large wind massloss rates can destabilise the disk however, even if the throughput remains high enough to generate the assumed luminosity – and we refer to this mechanism in the introduction (Shields + 86 reference). In fact, our wind massloss rates are not enough to destabilise the disk according to Shields.

*Section 4.3  
1. Methods of computing absorption lines.  
“These are calculated using the ray-tracing technique described in Higginbottm & Proga 2015”  
Looking at this paper, I see the Einstein coefficient determining the oscillator strength is set to 1 and hydrogen number density used in place of ion density, yet different line profiles and equivalent widths are obtained for different ions of Fe.  If the methods have been improved, a detailed description should be provided.  Otherwise, a similar caveat as included in the 2015 paper (“these spectra are in no way an accurate representation”) should be given.*

Thanks for pointing this out – the problem is mainly that we included the incorrect reference. We actually use the much more detailed (and accurate) line calculation method in Higginbottom+ 2017. We have changed the reference and added a note to make it clear that we use the correct ion density.

*2. The “step change in line profile shape around L = 0.2 L\_Edd”.  
This is potentially an important result but cannot be established simply by writing down equation 2 and noting that 0.16 is close to 0.2.  If a more convincing analysis cannot be provided, it should be stated that the reason for this noticeable change in line profile behavior is unclear.*

This is fair. The obvious way to confirm our suggested interpretation would be to run additional models with luminosities bracketing L\_crit. However, this would require substantial additional computation effort which we feel goes beyond the scope of the present paper. We have therefore changed the text to make it clear that, in the absence of additional calculations, our suggested interpretation is necessarily speculative.

*Minor comments  
W96 also include an optically thin heating & cooling term, yet they do not refer to their simulations as ‘RHD’.  The authors should clarify what qualifies their simulations as being ‘radiation hydrodynamic’ when they don’t actually solve the equations of radiation hydrodynamics (cf. Mihalas & Mihalas 1984).*

Our heating and cooling terms are calculated through a coupled full radiation transport step (in our own code), and therefore are not optically thin approximations. This permits our simulation take account of frequency dependent attenuation between the source and any cell. This method was set out in detail in Higginbottom+2018 and referenced here. However, as noted above, we fully agree that we did not provide sufficient information in the present paper. We have included more detail on our method in the revised version.

*Section 4.5  
The Done (2018) paper credits your equation (3) as being due to a more general equation from Proga & Kallman (2002).  These authors should therefore retain credit for this result*.

We have included a reference to the earlier work

*Figure 7  
I do not see any crosses.*

Thank you – should have been “stars” in the caption – fixed.