General Response:

Firstly, we thank the referee for a very detailed and helpful review. We would like to start our response by answering a minor question, that we feel indicates a significant problem with the way we have written the paper. The referee asks

“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 simulations are radiation hydrodynamic because although we indeed don’t solve the equations of radiation hydrodynamics, we do calculate the transport of radiation through the simulation via our own Monte-Carlo radiative transport code that is linked to zeus and is called periodically to update the heating and cooling rates – they are therefore no longer optically thin rates. We discussed this method in detail in Higginbottom+ 2018, but decided not to include much detail on the method in this paper for brevity. I think on reflection we perhaps should have included a little more detail, and I have therefore increased the details on our method in the paper.

In response to the general comments regarding the comparison of our work to W96, we certainly agree that W96 carried out a much more detailed study of luminosity dependence. We did initially have a section where we attempted a more detailed comparison but since we are carrying out radiation transport as part of our simulations (which is computationally very expensive), we were unable to cover more than a tiny subsection of the parameter space considered by W96. We therefore decided to concentrate the paper on our more targeted comparison with observations. Through our changes made in response to the referee’s other questions, we hope that the status of this work in respect to W96 is clearer.

Might be worth putting something in here that we are particularly interested in winds – and the observational imprint of them and se we leave out the iner corona because it isn’t fast and so cant be seen??

Reviewer's Comments:  
  
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.    
  
Below are my detailed comments for the authors.    
  
  
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.

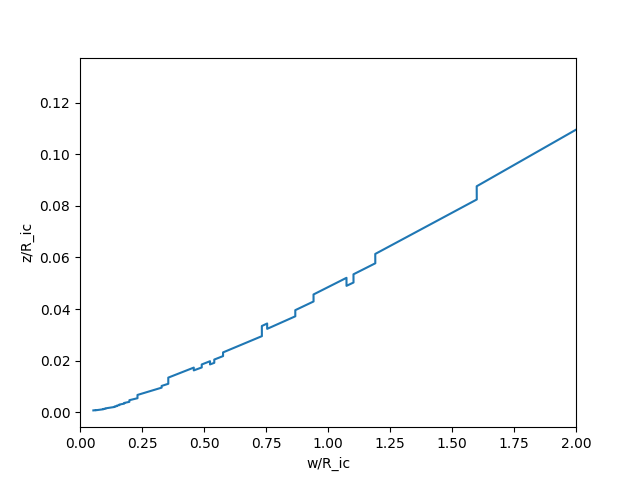
Table 1  
Please add values of the critical luminosity (eqn 4.4 in W96) to ease comparison with that work. Done.  
  
Section 2  
1.  Relation between thermal instability and wind launching  
Based on my reading to Higginbottom & Proga (2015), I do not understand the statement “In order to attain the 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, and 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) and so we use an optically thin estimate as a guide. 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 the inclusion of a disk because we don’t have the physics in our simulation. We have clarified this. In this work we are not in the optically thin limit, because of our monte carlo RT step and so our use of an ionization parameter computed in the optically thin limit to set our midplane density is an approximation – we hope our rewording has made that clearer. In an optically thin simulation (as in H18) this does result in a razor thin disk, we end up with a disk like structure in our simulations – although it it purely hydrostatically supported and so the convex shape (see below) is not really a   
  
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.

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?

I don’t know how to respond to this – our fig2 and their fig16 are showing totally different things. Fundamentally they are interested in how mass leaves the disk – our figure is trying to show how, at large r, the flow is spherical. HELP?

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.  \

Again – I’m at a bit of a loss here. In the comment above, they even quote W96 in saying that the flow turns radial, which is spherical right? Or am I missing something…. Even reading Font, they say “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. “ Their two models are alpha=3/2 and 5/2 on the equation n=n\_g(r/r\_g)^-alpha – so 3/2 is the ‘less steep than 1/r^2 case. Help again???

“    
  
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 implied from the luminosity, and is simply 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. To this end there is no formula beyond this one for determining M\_acc. We take the referees point regarding the viscous timescale and hence the disk depletion timescale however this is not directly related to our work, since the luminosity is related to the mass reaching the inner disk and on-to the compact object. As long as there is a sufficient mass supply into the outer disk, our wind can be generated and still maintain the assumed mass accretion rate. 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.

We have added some clarifying text to section 4.2.

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.

We are indeed unable to provide a more convincing analysis at this time, and so we have adjusted the text accordingly.  
  
  
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 dependant attenuation between the source and any cell. This method was set out in detail in Higginbottom+2018, and referenced although we agree we do not go into much detail in this paper – for brevity. We have included a little more detail on our method.

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.  – I 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.