Reviewer's Comments:

The paper describes the location of the water, carbon monoxide, and carbon dioxide snow lines under the influence of radial drift and inward viscous gas accretion. Numerical models are presented for four different disk models, highlighting different aspects that influence the location of the snow line. The main result is that the snow lines moves in by up to 50-60% for most particle sizes when considering the inward gas and dust flow.

The paper is well-written and the outlined approach is clear. The conclusions seems fairly robust given the context in which they are presented, with the exception of the assumption of inward radial gas flow, which may not be valid under all circumstances and this may change some of the conclusions. There are also a number of other issues that need to be addressed before publication.

The paper merits publication in ApJ, provided the following issues are addressed:

We thank the referee for a thoughtful and detailed review. Most of the comments were related to clarification of some concepts, as well as more detailed explanations about the validity of some of our assumptions.

MAJOR

The paper assumes the radial flow of gas is directed inward, towards the star (Eq. 13), in contrast with Ali-Dib et al. 2014 that assume an outward flow. Since this is a critical assumption in the paper, this issue needs to be addressed. An inward flow may be a reasonable assumption for the water snow line that is close to the star, but this is not immediately clear for CO/CO2 snow lines that are located further out. In reality, the movement of gas is likely a diffusive process, but with a net flow that is inward in the inner disk and outward in the outer disk. The authors need to investigate whether the expected direction of the gas flow is indeed inward for each of the snow lines and discuss how this assumption affects their conclusions if this is not (always) the case.

We have checked that for our fiducial disk model, the nebular gas always flows inward for the active disks. If that weren't the case and the gas drifted outward at the CO2 and CO snowlines, this would only change the drifting direction of the smallest particles in our model, which are well-coupled to the gas. We have added an explanation along these lines at the end of section 2.2.

The inward movement of the water snow line is significant smaller in a viscous disk than in a passive disk (Figure 5). Since the former is the most likely scenario, the quoted number of 60% in the abstract seems overstated. In general, a location of 0.7 AU for the water snow line in a disk model based on the minimum mass solar nebula seems unrealistic, so care should be taken with conclusions based on the model that does not include viscous heating. The actively heated model should be preferred as being the most complete one, and at least mentioned in the abstract. It is also not clear why this model is not shown in figures 2 and 3. There were a number of papers published this year on the location of the water snow line in viscous+irradiated disks that could be used for guidance: Baillié et al., Mulders et al., Bitsch et al.

We agree that the actively heated model is the most realistic of the three, despite the steady-state assumption which is less realistic due to accretion rates decreasing over time. We have modified the abstract to reflect that the quoted numbers are for a steady-state accretionally heated disk, and have changed the percentage for the H2O snowline accordingly (to ~40%), both in the abstract and in the main text where we quote these numbers (towards the end of section 4). Right after quoting the innermost snowline locations in the main text, we note that we already mentioned that the water snowline is significantly closer to the host star compared with solar system models, and explained why. We also note that for the viscous disk, we calculated the static snowline location using the same temperature profile as that of the viscous disk, for consistency purposes. We added a footnote to reflect this when quoting the water snowline location in a static disk. We do not show the actively heated model in Figures 2 and 3 because it would complicate these already very complex figures, and it would not add any qualitative information – the only difference is that the water snowlines would be further out from the star compared to the active disk, as we show in Figure 5. We clarified our reasons for not showing these plots when we describe Figure 2 in section 3.

The calculated gas C/O ratios (colored lines in figure 5) are not correct. Between the static and dust snow line, gas-phase abundances are modified by the release of additional gas through desorption of drifting solids, not by additional adsorption onto dust grains. Inside the CO2 and H2O snow line, this drives the C/O ratios down, not up as is shown. The assumption in section 4 that "the abundance relative to hydrogen for each volatile is fixed" cannot be used in conjunction with radial drift. Two possible solutions are evident: calculating the amount of vapor released by drifting grains (equation 18a+b) or finding an approximation that reflects that the C/O ratios go down, not up.

We think there might be some confusion here. We agree with the referee that the arrows should go down inside the H2O and CO2 snowlines, and this is what Figure 5 reflects – the arrows are indeed going down. Specifically, we explain this in section 5: "Before discussing the quantitative aspects of this plot, it is essential to acknowledge that our estimates for the C/O ratios in the active disks ignore the movement of the desorbed ices with the accreting gas—the relative fluxes of the volatiles in

gaseous and solid form will affect the relative abundance of C and O in gas and dust throughout the disk. As demonstrated in Figure 4, this will not affect the snowline locations for particles of a given size, but will change the shape of the C/O curves in between the various snowlines. For example, for the disk parameters and particle sizes displayed in Figure 5, water molecules in solid particles drift up to ~1000 times faster across the H2O snowline than do molecules of CO and CO2 vapor that are entrained in the accreting gas. This differential inward motion will result in an increased oxygen gas abundance inside the H2O snowline, and thus a (in some cases much) lower gaseous C/O ratio in this region." We only discuss readsorption in the context of small particles that form snowlines outside the static snowline, and which are therefore not true snowlines, as explained in the text. The abundance of each volatile is *initially* fixed compared to the hydrogen abundance, and we added this in the text. Indeed radial drift will change the abundances, but as we state in section 5, we do not take into account the relative fluxes of the desorbed ices and the overall nebular gas, which is why the curves in Figure 5 represent upper limits for the C/O ratio, as stated in the text. We will address this issue in a future paper, and we added a sentence to reflect that in item 7 of the summary.

THROUGHOUT PAPER

"the disk": Protoplanetary disk properties evolve in time and their properties vary from star to star. It is fine to pick a fiducial disk model for this investigation, but one should be aware that some of the conclusions will depend on the chosen disk model. In particular, the grain sizes quoted in abstract and conclusions are model-dependent and this should be clearly articulated, unless they can be expressed in a dimensionless size that is independent of the chosen disk structure. This same holds for the respective locations of the snow lines.

We agree with the referee's comment and added in the abstract the fact that the quoted grain sizes are for our fiducial disk model. We also added in the main text in section 5, right before we quote the snowline radii, that for grain size distributions with a different maximum particle size, one can pick out the appropriate snowline location from the plot. We also note that we explicitly state in the next paragraph that the quoted snowlines are those appropriate to our model.

The term "active disk" for an evolving irradiated disk without viscous heating is confusing. In the current manuscript, the terms "active" and "passive" refer to both heating (eq. 8) and dynamics (disk models), and are not defined in the abstract. The authors should try to find less ambiguous terms. I would suggest "viscous" and "(ir)radiated" when referring to heating mechanisms. Perhaps "irradiated", "evolving", and "viscous" are terms that better reflect both geometry and heating mechanisms for the "passive", "active", and "accretionally heated" models, respectively.

We agree with the referee that the terminology used in defining the disks was confusing, and we thank the referee for the suggestions, which we have implemented throughout the paper as well as in the figures. We note, however, that we retained the "steady-state" terminology when talking about other cited works in the introduction, as well as in the appendix where we discuss the Shakura-Sunyaev steady-state disk solution, which is a well-established terminology.

The ("active disk") does not take into account time-dependencies in the mass accretion rate and irradiated mid-plane temperature (through evolving surface density and stellar luminosity). Inclusion of these effects will lead to the inward movement of the snow lines throughout the disk lifetime. It is not clear from the current draft how this effects conclusions that snow line locations are independent of time. The water snow line may move fast at early times. The authors could verify that the timescales for moving the snow line in through drift are shorter than its intrinsic movement.

We had already discussed the effects of changes in mass accretion rate and stellar luminosity in items 7 and 8 of section 5.2, respectively, but we agree with the referee that we should state that we ignore these changes in our models early on in the paper. We made this clarification in the last paragraph of section 2.1, as well as when discussing about stellar luminosity in section 5.2.

MINOR

Introduction:
- It would be good to also mention the Alma DCO+ observations of the snow line.

We thank the referee for the suggestion – added a citation to the DCO+ observations in the introduction.

- "This 'conveyor belt' model is also used by Ciesla & Cuzzi (2006)". The current paragraph doesn't reflect that much of the work is based on the pioneering work by Ciesla & Cuzzi and also Cuzzi & Zahnle 2004. Please rephrase.

Rephrased and added the Cuzzi & Zahnle (2004) citation.

- "These observations are currently lacking an interpretive framework that takes into account all important dynamical and chemical processes." Since the paper does not present such a framework or outlines a way towards one, it is not clear how the work presented here fits in to this bigger picture.

This statement serves as a motivation for our paper and for future papers, in which we will add other dynamical and chemical effects, with the eventual goal of inferring properties about the disk by comparing our results with current and future ALMA observations of snowlines, and eventually of giant planet atmospheres with JWST. Since this paper is the first one in a series and is just setting the stage, we cannot yet make very robust comparisons with observations. However, we already make some inferences about the disk around TW Hya at the end of section 4, as well as about the H2O snowline location in TW Hya at the end of section 5.1. We thus believe that this big picture statement is appropriate.

- The claim of super-stellar C/O ratio in WASP-12b by Madhusudhan et al. 2011 are no longer supported by the available observations. The text should reflect that there is currently no unambiguous evidence for super-stellar C/O ratios in hot jupiters.

We clarified this and added citations to the relevant papers.

- "One combination that has not yet been considered is the combination of radial drift and viscous gas accretion" This contradicts earlier statements about the work in the Ali-Dib et al. (2014) paper. Maybe the authors mean INWARD gas accretion?

The Ali-Dib et al. (2014) paper also includes particle coagulation and diffusion, which we state when we talk about this paper, and which we do not include in our model. We clarified this, however, by adding that we are studying these two effects in isolation (i.e., ONLY drift and gas accretion).

Section 2:

Disk model ordering: it would make sense to list disk models in order of increasing complexity: static, passive, active, active heated. The static and passive disk are identical in terms of density and temperature, they could be combined in one heading in section 2.1. This would also make it more clear that the choice of density and temperature for the passive disk is based on that in Oberg et al. 2011.

We agree with the referee that this ordering makes more sense, and therefore we combined the static and irradiated disks into one heading at the beginning of section 2.1.

Passive disk: One could also note here that a slope flatter than r^-1.5 is more consistent with the temperature profile for a steady state gas disk.

Agreed and added in the "Static and Irradiated disk" heading.

It is not clear if the active disk has the same surface density profile as the static/passive disk. A plot comparing the two surface densities would be helpful, or a statement at what time the active disk surface density matches the passive disk.

We compared the two profiles and found that they match in the inner disk at t ~ 5e4 years, but they diverge past a few AU due to the exponential cutoff in the surface density of the evolving disk. We added this statement at the end of the description of the evolving disk.

Eq 4 & 5: T is used for time, but is already used for temperature in the rest of the paper.

There was an overbar on top of T in Eq. 4 and 5, but indeed it wasn't easily distinguishable, so we replaced by "tilde t". There was also a factor of 2-gamma missing in the denominator in Eq. 4 (so that the surface density integrated across the disk matches to total disk mass), so we rectified that as well.

"Calculating the mid-plane temperature self-consistently for an active disk is non-trivial." This statement is not correct. It is in fact irradiation that is non-trivial, see for example Chambers 2009.

We clarified this statement in the first sentence of the "Viscous disk" heading.

Active disk: the equations presented here do not take into account two time-dependencies: mass accretion rate and irradiated temperature. This should be clearly stated.

As noted in our response to the last comment in the "THROUGHOUT PAPER" section of the referee report, we stated this fact, along with further explanations, right after we discuss the viscous disk at the end of section 2.1.

After eq 14: please cite original references for binding energies.

Done.

Last paragraph: Text would be clearer if the second sentence "In these case ..." was moved before (1) and (2)

We agree and have restructured the beginning of this paragraph accordingly.

What about the time scales at which the snow line moves in due to changes in accretion rate and stellar luminosity? How do these compare?

We agree and have added a discussion of this to the end of section 2.1. Also see reply to previous comment on time variable luminosity for more details.

Section 3:

Footnote 4: This is an important caveat: these statement is not true for the water snow line in a viscous disk. This should be in the main text.

We added the footnote in the main text and clarified that the snowline locations will vary between the three disk models for a given initial particle size.

Last sentence: This statement is only true if also ignoring time dependence of the mass accretion rate, disk temperature, and stellar luminosity.

We added this clarification in the last sentence, and referred to section 2.1.

Section 4:

"If grains have grown to radii larger than ~7 m, then the 7m snowline applies" This does not seem to be the case for significantly larger particles (Figure 2). Please clarify.

We clarified that this is the case for particles larger than 7 m that still drift and desorb.

"snowline in all disks is significantly closer to the host star compared with Solar system models". The snow line location depends mainly on the mass accretion rate and dust opacity, see for example Min et al. 2011 for a comparison between different models. A different choice may have yielded a value that better matches the solar system snow line. On the other hand, Mulders et al. 2015 find that typical sun-like stars may have snow lines closer in than our solar system.

We thank the referee for these very helpful suggestions and references, which we have incorporated in the discussion of the snowline location in the Solar system compared to our model.

"moving the snowline location outwards" -> "... inwards"

Corrected, thank you for pointing out the typo!

The TW Hya snow line matches that for small grains: This may indicate that coagulation and fragmentation is important. It would be interesting to compare the coagulation-fragmentation time scale with that of the desorption time scale to see if this is the case. Alternatively, the flow may not be inward at 30 AU, see also major point.

As clarified in the response to the major point, the flow is always inward at 30 AU for our model. Comparing the coagulation, fragmentation and desorption timescales would be interesting indeed – we thank the referee for the suggestion, and we plan to address these effects in a future paper.

Section 5:

paragraph 2&3: Mention that the conclusion pertaining disk age and mass are only valid if the snow line itself doesn't move with time or disk mass.

Noted at the end of the third paragraph.

Discussion:

1. "grain growth will eventually push the snowline location outwards" If grains grow to sizes where they don't drift or sublimate, the snow line is successfully 'frozen in' to the planetesimal population, and stops moving. It is not clear from the text why this implies an outward movement of the snow line.

This is true and we thank the referee for pointing this out – for very large grains, the snowline just reduces to a static disk, so it is true that it would eventually move outwards compared to smaller grains. However, we compare our results with the static snowline, in which case particle growth can either push the snowline inwards or leave it unchanged. We added a footnote in table 1 to reflect this.

2. "Turbulence causes eddies and vertical mixing, which are likely to reduce the radial drift velocity of the solids" This is not immediately clear from the text. Please expand or add a reference.

Reference added.

3. Is it possible to estimate the fragmentation time scale and compare it to the drift and desorption time scale? Perhaps in Section 2?

As mentioned in the response to the referee's comment regarding the TW Hya snowline, fragmentation is not the scope of this work, since our goal was to specifically investigate the effect of radial drift and gas accretion. We acknowledge that fragmentation may have an important effect on the snowline location, and we plan to address this, as well as grain coagulation, in a future paper.

4. "outward" -> "less far inward" or "outward with respect to"

Text changed to "less far inward".

6. "cavities significantly depleted of gas" Given that most transitional disk observations probe dust, it might be good to add a reference for the gas. For example, van der Marel et al. 2015.

Citation added.

7. The mass accretion rate is a dominant factor in setting the location of the water snow line, and some more discussion on this choice is warranted. In particular, how does this value compare to accretion rate measurements of solar mass stars, and what is the age where the disk reaches 10^-8?

Please see the end of section 2.1 for a discussion on this.

Summary:

It would be good to explicitly state what "the model" or "fiducial model" is, since the mentioned grains sizes depend upon it.

We now state explicitly in item 6 what is the grain size range that we consider.

1. add that movement is inward

Added.

2. "Thus for each particle size there is a fixed and uniquely determined H2O, CO2 or CO snowline." This conclusion is at direct odds with that of Baillié et al 2015, who identify snow regions. please clarify or rephrase.

We clarified that this is true for our simplified model, which does not include the effects described in section 5.2 – diffusion, in particular, will

spread the snowline radially and may therefore produce snow regions rather than snowlines.

3. Specify what kind of numerical simulations

We were referring to our own numerical simulation and we clarified that in the text.

6. Add that the drift of the water snow line is less than 60% for a viscously heated disk.

Added.

7. Is conclusion 7 new? If this is just a confirmation of Oberg et al. 2011, this should be stated, and what this paper adds to this conclusion. Mention this is C/O ratio of gas. Also mention the direction and magnitude of the change in C/O when taking into account radial movement of gas, reflecting the changes made to figure 5 and section 2.

We changed and expanded conclusion 7 along these lines. We also removed the sentence "This is consistent with possible detections of superstellar C/O ratios in some exoplanet atmospheres", since indeed these claims have been refuted.

8. mention this is assuming a constant Mdot and Lstar.

Added.

FIGURES

1: not color-blind friendly. This is a very complex plot, is it possible to split it into three separate panels?

'We changed to red to a shade of orange, which we hope will be more color-blind friendly. We agree that the plot is indeed very complex. Unfortunately, we don't think that splitting it into three separate plots would be a good idea, since some of the information the plot contains would be lost that way – we want the plot to show the comparison between the three timescales (drift, desorption, accretion) at each location, as well as the comparison for a given timescale (either drift, desorption or accretion) at one single location. We believe that the caption and the explanation in the main text should clarify any confusion regarding this complex plot.

Would it be possible to show a cut a given radius (at the snow line or a few different radii) showing final semi-major axis as a function of particle size as an intermediary between figure 1 and 2?

2: Line colors are hard to distinguish since a large section of the color bar is not used. Is it possible to increase the contrast?
Why is the heated disk not shown? Also for figure 3.

We made the plots taller, which we think helps a bit with distinguishing the lines. We explained why we didn't add the heated disk as a part of our response to the second major point.

Could skip a few axes tick labels in Figure 1 and 4. Bold tick labels in figure 2 & 3 like in other plots would improve readability.

Done – both fewer axes ticks in figs. 1 and 4, and bold tick labels in figs. 2 and 3.

4: There is only one labeled tick in the top right panel
Left hand panels would be clearer if particle size at t=tdisk would not go to zero

Added another tick to the top right panel x-axis. The particle size does not go to zero at t=tdisk, it just becomes very small (hence the 10^-4 lower limit on the y axes). Perhaps we're not understanding the referee's point here?

5: Can the caption be shorter? A lot of information is included that is better explained in the main text. The arrows are not explained. Curves exterior to the static snow line could simply be replaced by the static snow line.

We have shortened the caption, though we still needed to keep some of the essential information, even if it is already mentioned in the main text, given the complexity of the figure. However, we have removed the curves outside the static snowline and adjusted the main text accordingly.