Reviewer's Comments:

The authors have addressed most of my concerns, but there are two main issues that remain unresolved, and a few minor ones based on the changes to the paper.

- Time-dependency of mass accretion rates and water snow lines:

The authors have adequately addressed variations in stellar luminosity, but not those in mass accretion rates and water snowline location. The effect of mass accretion on the inward location of the snow line (factor 4) is much larger than the 40% by drift. This issue has to be addressed differently.

The argument brought forth in section 2.1 that "the mass accretion rate is close to 10^{-8} Msun/yr during the quasistatic accretion phase of the disk" is not correct: mass accretion rates decline with time, leading to a continuous inward movement of the snow line. See for example the numerical time-dependent models of Chambers 2009: figures 1-4 for the decline of mass accretion rate and figure 5 for the inward movement of the snow line with time. Note that Garaud & Lin 2007 calculate the steady-state disk structure for a given mass accretion rate, they do not predict a phase where the mass accretion rate is constant in time.

It is possible that the drift timescale is much shorter than the time scale at which the water snow line moves in due to accretion. I leave it up to the authors to either calculate whether this is the case, or acknowledge in section 2.1 and 5.2 that the location of the water snow line may be determined by the decline in mass accretion rate rather than inward drift.

We have amended our discussion at the end of section 2.1 and item 7 of section 5.2 to reflect the fact that changes in mass accretion rate may determine the water snowline location rather than radial drift. Indeed, the findings of Chambers 2009 show that the water snowline may move inward by up to one order of magnitude during the gas giant formation phase of a few Myr. We acknowledge this caveat in both 2.1 and 5.2.

- Figure 5

The colored solid lines (C/O ratio of the gas) still show an increase interior to the static CO and H_2O snow line. It is not clear to me from the text or the rebuttal how the C/O ratio of the gas is calculated. The text or model does not describe a mechanism that would increase the C/O ratio of the gas interior to the static snow line.
(It does describe one that would decrease it, indicated by the arrows).

If desorption of ices is not taken into account in this figure, the C/O ratio of the gas should not change with respect to the static model (i.e., the black and colored lines should overlap). The current plot seems to assume abundances relative to hydrogen are the same at each radius _after_ ices have migrated, and this is the incorrect assumption that I pointed out in my original report.

Desorption is taken into account in the figure, and fundamentally the elevated C/O ratios interior to the static CO2 and H2O snowlines are simply due to the inward mobility of the snowlines (desorption fronts) due to drift and accretion flows. Qualitatively, this scenario should be robust to changes in total abundances throughout the disk, i.e. at e.g. the "dynamic" CO2 snowlines, the rapid return of CO2 into the gas-phase (during CO2 desorption) will reduce the C/O ratio interior to the CO2 dynamic snowline, while no major change in gas-phase composition, and therefore C/O ratio, is expected between the static and dynamic snowlines.

As the referee notes, we do operate under the simplifying assumption that the total (ice+gas) abundances are the same at each radius after ices have migrated. This is a good approximation for the irradiated disk, given that this model by definition presents a constant influx of particles at any given radius while the gas is static, and thus the ice+gas surface density should remain constant. For the evolving disk, we agree with the referee that this is not a good approximation. In evolving disks, the gas-phase C/O ratio may decrease everywhere interior to the CO2 and H2O desorption fronts due to the decrease in the surface density of solids with time at any given radius. In the steady-state viscous disk, the solid abundances at a fixed radius are constant, given that this model is not time-dependent, but the solid/gas ratio is not constant, which can result in a substantially lower C/O ratio interior to the H2O and CO2 snowlines compared to the static case (as indicated by the arrows in the figure).

These caveats are important and we have explained and clarified them further in the next. We have also clarified that the main purpose of this figure is to show the different snowline locations in static and dynamic disks, and thus where in the disk C/O is reduced or increased rather than providing a quantitative estimate of how big or small that increase/decrease is.

Minor points:

- Flow direction, Section 2.2
"we have found that the radial flow of gas is always directed inward"
How was this result found? This is not clear from the text, some clarification here is required. Which method or calculations was used, or what was the radius where the flow turned outward for the fiducial models?
This would also give there reader an idea how variations in the quoted model parameters may change the flow in the outer regions.

We pointed out explicitly in the text the expressions we used to calculated the gas flow r_dot,gas, and stated the radius at which the flow turns outwards for the evolving disk (~200 AU). Since the gas velocity for the steady-state viscous disk is simply obtained from M_dot = -2 pi r r_dot,gas Sigma, r_dot,gas will implicitly always be positive (the negative sign implies inward movement).

- There seems to be an inconsistency between the disk model parameters: the surface density of the irradiated disk matches at 5 10^4 yrs, but the mass accretion rate of 10^-8 Msun/yr is more consistent with an age of a million years.

The number we obtained was 5 x 10^5 years, which is within a factor of two of a million years; we sincerely apologize for the typo.

- Garaud & Lin 2007 is cited 3 times in section 2.1 Perhaps a more observational paper for the range in mass accretion rate could be used, for example Sicilia-Aguilar et al. 2010 or a more recent paper.

We removed the repeated references to Garaud & Lin (2007) in section 2.1, and added references to Sicilia-Aguilar et al. (2010) and Chambers (2009) instead. We thank the referee for the suggestion.