**We thank the referee for a thoughtful and thorough report. The suggestions were mostly along the lines of clarification, emphasis of caveats and comparison to previous work, which we have taken seriously. In response, we made significant edits as described below. Regarding the models themselves, the referee was also concerned about our claim that self-gravity is the main difference between our analytic and numerical models. The attached supplemental figure described below should alleviate this concern. We have added some clarifying text to the manuscript on this point as well (also described below).**

**We hope that we have allayed the referee’s main concerns about the reliability and applicability of our quantitative results. While some of our approximations are more severe (or different) than some may like, we feel that a diversity of model approaches helps clarify important processes in planet formation.**

The paper presents numerical calculations and an analytical model of the contraction of a gaseous envelope bound to a planetary core. The aim is to characterize the critical core mass for rapid gas accretion, as a function of various planet/nebula parameters. The authors are especially interested in the possibility of giant planet formation at large orbital radii. I think that the paper provides a valuable framework for planetary formation models that can complement more sophisticated and time-consuming calculations typically applied in studies of giant planet formation. Although the results are qualitatively useful, it is more difficult to judge their applicability in quantitative terms, due to the approximation involved. In view of this, the authors should make an effort to discuss the issues outlined below. I think that there are limitations in the interpretation of the results, some of which are also mentioned in the text. These limitations should be identified and discussed  
in the introduction and/or summary. There are also some technical aspects of the numerical/analytical models that need clarifications.  
  
One problem that is only briefly mentioned is core formation. In fact, if there was a mechanism to form a planetary core at large orbital distances on a reasonable timescale, there would not be much to object to the idea that the core accretion model may operate at those distances. It has long been recognized that the phase of thermal contraction is mostly determined by the internal physics/microphysics of the envelope and not by the external state of the nebula. Grain opacity has been identified as the single most determinant factor that governs the cooling rate of an envelope. Therefore, if envelope contraction can occur fast enough at 5AU, it would do so at 100AU as well. It is indeed true that the point of the paper is that it takes a smaller core at large separations to trigger rapid contraction, but the perceived problem is that it takes longer to form a core of a given mass the farther the core is from the star. But because of the different formation timescales, it may not  
be appropriate to use a single nebula model and disk lifetime at all distances. Some comments may be in order about these issues. **We reemphasize in the summary that our brief treatment of core formation is purposeful due to the large uncertainties that currently exist in the mechanisms of core growth. We also mention alternative models for core growth, such as pebble accretion. We do acknowledge the concern of ongoing planetesimal accretion, and are careful to emphasize that gas accretion onto low mass cores that are no longer growing is only a possibility, and can certainly be prevented by ongoing accretion of solids. Differences in disk lifetime are now addressed (also a separate point below). Other important disk parameters (density, temperature, opacity, and composition to some degree) are addressed so that our results are not completely tied to a single disk model. Fortunately, the weak dependence on disk density makes parameter exploration slightly less important than it would otherwise be.**

The statement of the paper is that, by neglecting heating sources in the envelope, these models provide the shortest possible timescale for the formation of a giant planet. But this statement bears the question: can the critical core mass be smaller than the isolation mass (e.g., Pollack et al. 1996)? The authors recognize that a minimal supply of energy to the envelope (e.g., in the form of gravitational energy delivered by the accretion of solids) can inhibit contraction, increasing the critical core mass. But if isolation is not reached, accretion of solids should occur. Therefore, in giant planet formation calculations, the crossover mass is always larger than the isolation mass. For the disk assumed in the paper, the isolation mass Miso increases with orbital radius, Miso ~ a^(3/4), and it is possible that at large distances from the star the values of the critical mass found here would be smaller than the corresponding isolation mass (under the usual assumption that the  
gas-to-dust mass ratio is initially uniform). Obviously, one could assume that the isolation mass is always smaller than the critical core mass Mcrit (by an appropriate choice of the initial surface density of solids, i.e., of a distance-dependent dust-to-gas mass ratio). Nonetheless, this issue should be discussed and the working assumption Mcrit > Miso explicitly stated. **The summary to the paper already included a significant emphasis on the possibility that cores do not isolate. We have added to this a specific discussion of the quantitative scaling of M\_iso, and how this real obstacle might be overcome. The alternative of fast growth through pebble accretion and core migration / scattering is re-emphasized, with a specific mention of how this affects the isolation mass.**

In Section 2, some of the assumptions should be better justified. For example, I understand the reason for keeping the luminosity constant in the radiative layer of the analytic model, but why is it constant in the numerical calculation as well? Is there a physical/numerical reason that requires this choice? Isn't this approximation basically constraining the derivative dT^4/dm, where T is the temperature and dm the mass increment m(r+dr)-m(r**)? We further justify this assumption in section 2.5, and reference the explanation in assumption #4. In brief, this approximation drastically simplified the numerical model (turning PDEs into ODEs), allowing easier exploration of parameter space.**  
  
The radiative layer should operate as a 'thermal blanket', regulating the loss of heat generated in the envelope interiors. This layer must operate differently here since the luminosity is constrained to be equal to that at the boundary of the convective-radiative layer. Besides joining the convective layer to the nebula, what is the role of the radiative layer in regulating the cooling rate of the envelope? **We added an explanation to section 2.5, together with the justification of assumption #4. Because the conditions in the radiative layer help determine conditions at the RCB, they are essential in regulating the cooling rate. This fundamental feature of the radiative zone does not go away with the constant luminosity assumption, though the regime of validity of our models (which we test) is limited.**  
  
About assumption #6 in Section 2: How is it enforced? How is the radiative layer kept 'cool enough' to avoid sublimation? I understand that the radiative layer of the analytical model is basically isothermal with a temperature equal to the nebula temperature, but how is this condition enforced in the numerical calculations? Would this assumption be valid if the luminosity of the radiative layer was actually calculated? Moreover, why did the authors choose to apply an interstellar grain opacity in a protoplanetary disk, which apparently has lost all its solids contents (no solids accretion)? Typically, dust in the envelope is provided by ablation of accreted solid material. Instead, here, it appears that the opacity is due to dust entrained in the accreted gas. It this correct? If so, is a full interstellar grain opacity still applicable this far along during the disk evolution? Have the authors looked at the opposite extreme case of a radiative layer devoid of dust (which would  
seem more applicable in this study, given the absence of solid material)? **The radiative layer remains cool enough to avoid sublimation as a direct result of the numerical integration, for the cool disk temperatures we are interested in and our assumption of an inner convective – outer radiative region (i.e., without inner radiative windows). We added an explanation along these lines in the text of assumption #6, and deferred additional discussion about the opacity choices to section 6.2. As mentioned in section 4.2 in the description of Figure 3, our choice of interstellar opacity is conservative, and we acknowledge that the dust opacity in our regime is likely to be lower due to dust settling. While section 6.2 already discusses some caveats in assuming a power-law opacity and the possibility of radiative windows, we also now mention the possibility of a radiative layer devoid of dust.**

In section 2.2, it is discussed how the physical radius of the envelope is chosen, introducing the Bondi and Hill radii. The authors set the envelope radius equal to the Hill radius, Rhill. However, when calculating the envelope masses, they use the smaller of the Bondi (Rbondi) and Hill radii. I do not understand the reason for using these two different lengths. This approach seems inconsistent. If the boundary is set at Rhill, the envelope mass should be computed within Rhill. If the authors believe that the envelope mass should only be considered within MIN(Rbondi,Rhill), then the boundary should be placed at that distance. If I were to judge from the bottom panel of Fig.1, I would say that there are at least 1-2 Earth masses worth of gas between Rbondi and Rhill in those cases. The envelope mass estimates should be made consistent with the choice of the envelope boundary. **We justify our different choices for outer boundary and atmosphere mass in section 2.2, after equation 9 (outer boundary), and in the last paragraph of 2.2 (enclosed mass choice).**  
  
The results from models used here seem very sensitive to the radius of the convective-radiative boundary (R\_RCB). How is this radius affected by the applied values of the mean molecular weight (mu) and the polytropic index (gamma)? Are there differences between the values of R\_RCB used in the analytic model and those obtained from the numerical solutions? The value of gamma directly affects the density (hence mass) and temperature (through the adiabatic gradient) in the convective layer. How does the critical core mass depend on the assumed value of gamma? **We discuss the differences in R\_RCB between the numerical and analytic models in section 4.2, in the paragraph where the luminosity drop is discussed. We added the R\_RCB dependence on mean molecular weight in 5.1.1. We also added a note at the end of section 5.1.1. that the influence of gamma on R\_RCB and M\_crit is deferred to Piso, Youdin & Murray-Clay (2014, in prep.)**  
In Section 3.1, where the two layer model is described, it seems that the outer boundary of the envelope is set equal to the Bondi radius, Rbondi. In Section 2.2, it is stated that the outer boundary is equal to the Hill radius, Rhill, so why is Rbondi (and modified radius, Eq. 22), not Rhill involved in the derivation of the analytical structure? The same applies to the results in Section 3.2, which involve Rbondi rather than Rhill. Also, most of the following discussion relies on the Bondi radius, as if it was the envelope radius. **We explain the choice of outer boundary for the analytic model at the beginning of section 3. The modified Bondi radius in eq. 22 does not represent an outer boundary for the atmosphere in the analytic solution, but an important physical scale, written in a way that simplifies the analytic expressions. Independent of the precise location of the outer boundary (R\_B, R\_H or idealized at infinity), the Bondi radius remains an important physical scale in atmospheric structure (we are not the first to find this).**

In Section 3.2, it is stated that the total energy is concentrated close to the core if gamma < 3/2, but at temperatures of a few times 10^3 K, gamma is likely to be ~5/3. I do not understand the issue raised here. **Here we were merely commenting on a property of the polytropic solutions – we are not stating that gamma < 3/2 is likely. We clarified our model assumptions to emphasize that we actually use gamma = 7/5, and that a more detailed treatment of EOS is being considered (the follow up paper is in progress).**   
  
In Section 3.3, the authors use a disk lifetime of 3Myr to estimate the critical mass. It is known that the timescale for gas dissipation in protoplanetary disk is very difficult to measure, due to the difficulty of observing reliable gas tracers. It is also known that gas disks may live longer than a few Myr, say ~10Myr (e.g., Bell et al. 2013). The authors should discuss the consequences of possible longer disk lifetimes. It seems to me that a lifetime of 10Myr would double Mcrit in Eq.(36) (Mcrit ~ td\*\*(-3/5)). **This point is correct, and has been added in the last paragraph of section 5.2. Of course, some fraction of the disk life is required for core growth, as we have also re-emphasized.**

I do not follow the physical argument behind the choice of the factor f (Matm=f\*Mc) applied in Eqs.(37a,b). I am not sure I understand how and why the 'modified crossover mass' is introduced and the requirement that it is 10% of the core mass. It seems to me that the lack of self-gravity in the analytical model prevents its applicability beyond some envelope mass, well before the crossover mass (Matm=Mc) is reached. But even if that model was applicable for Matm < ~Mc/10, the evolution for larger envelope masses would not be accurate. Then, how would one get an estimate of the critical planet mass? **We revised the discussion to emphasize that f\*Mc scaling is not a physical model, but merely a crude method to speed up the analytic model. The purpose is not to make the analytic model look better than it is, we are quite clear that the neglect of self-gravity is a severe approximation that leads to errors. Rather the rescaling facilitates comparison of the parameter dependences (which agree better than the absolute magnitude). Ideally we would like to compare our numerical results to an analytic theory that includes self-gravity, but alas no such theory is available to us.**   
  
In Section 4 Fig. 1, it should be explained what physical approximations (in the numerical calculations) lead to a temperature stratification in the envelope interiors that is basically the same, except for the case with the thinnest envelope, for all envelope masses. Would they expect the same temperature at the core regardless of the envelope mass? **We added an explanation in the description of Figure 1 in section 4.1. Since the temperature profile is largely set by the gravitational potential (and EOS), the atmosphere mass is not very relevant near the core.**  
  
In Section 4.2, it is stressed, as also stated in the abstract, that self-gravity becomes important for Matm > 0.1Mc. It seems to me that this conclusion is reached by comparing numerical and analytical results and simply assuming that all differences stem from the lack of self-gravity in the analytic model. Looking at the bottom panel of Fig. 2, it seems to me that the two solutions (numerical and analytical) diverge since the beginning, well before Matm reaches a tenth of the core mass. My understanding is that the analytical model relies on a number of approximations that are relaxed in the numerical calculations. These approximations may also contribute toward the observed differences. The authors ought to better justify the statements about the relevance of self-gravity because it is unclear whether they are supported by the comparison analytic vs numerical solution.

**We have attached a plot which includes a numerical model with self-gravity turned off, for a = 10 AU and Mc =5 Mearth. This plot verifies that the main difference between analytic and full numerical models is indeed self-gravity. Other effects (outer boundary and surface terms) have a noticeable, but much smaller influence. Indeed, the numerical and analytic solutions diverge well before Matm = 0.1 Mc in Fig. 2. By comparing the attached plot and Fig. 2, we see that self-gravity starts to matter at larger Matm / Mc ratios for smaller orbital distances; thus our conservative claim that self-gravity is important when Matm ~ 0.1 Mc. We have added elements of this explanation to section 4.2.**  
In Section 5, it is mentioned that these models provide "the necessary conditions for giant planet formation by core accretion." I think this is an overstatement because of all the approximations involved. For example, using a more suitable opacity, the timescale for runaway growth will be reduced compared to the numbers given here. A fixed nebula model is likely inapplicable to cores forming at various orbital distances and the critical core mass appears to depend on the nebula conditions. I would rather say that these models may provide indications on the timescale for giant planet formation, as a function of various nebula/planet parameters. **We rephrased the opening to section 5 along the suggested lines. We also note that we do explore the effect of atmospheric opacity, among other parameters, in the section. The word “necessary” was removed in the rephrasing in case that seemed over-reaching.**  
In Section 5.1.1, again, to explain some results as a function of the mean molecular weight, the argument is based on the assumption that the envelope radius is the Bondi radius, whereas it should be the Hill radius. I Section 5.1.2, the authors argue that the thermal state of the nebula has a considerable effect on the critical core mass: Is this conclusion affected by the choice of a constant luminosity in the radiative layer? Could a larger temperature gradient in the radiative layer act as a buffer and reduce the effects of the nebula temperature and pressure?

**Regarding mu, that explanation was a bit loose and has been sharpened. R\_B is still an important scale, even when R\_H sets the outer boundary or sphere of influence, as we have explored in detail elsewhere and re-emphasize here. Regarding constant L, this approximation is generally valid for our model, but not universally valid as we state clearly. The effect of a large dL/dr, and thereby a larger dT/dr, on the coupling of evolution to the disk temperature is an interesting question, but not one that we are prepared to answer at this stage. It seems to us intuitively unlikely that the atmosphere would lose memory of the disk temperature completely. Unlike the density, there is likely no region of (near) exponential growth of temperature. However, without detailed models to address this point, it is probably best that we not attempt to deal with it in detail in this paper.**

In Section 5.2, I think that since the core is assumed to form via solids accretion, the authors should mention that the critical core mass is not expected to be smaller than the isolation mass. **We discuss the implications of Mcrit < Miso in the summary.**  
  
I think that some comparisons with existing core accretion calculations (e.g., comparing crossover masses and runaway accretion timescales) should be carried out in Section 5. Even though the majority of the cases found in the literature is within 10AU, the comparison would still be useful.   
**We added some comparisons with previous studies in the new section 5.3. We focused the most detailed comparisons on similar models, but also address models with ongoing planetesimal accretion for completeness.**

In Section 6.1, it is mentioned that the nebula flow no longer circulated the planet outside of the Bondi sphere, but the density remains spherically symmetric and hydrostatic (with respect to the core) nonetheless. How is this conclusion reached and why should unbound gas be spherically symmetric around the core? **Here we are quoting results from Ormel (2013), an impressive study which we cite and which is consistent with previous studies of gas flow around a planet, while exploring new regions of parameter space. Intuitively, these results are reasonable because a relatively weak pressure gradient in the disk can affect the flow pattern outside R\_B without affecting the density structure noticeably. The planet’s gravity is not completely negligible outside R\_B (especially when R\_B < R\_H as considered by Ormel and mostly by us), which allows for the (very near) spherical symmetry in density that his solutions find.**

In Section 6.2, the authors should mention that the importance of envelope opacity has been long recognized and that there are already several dedicated studies on this topic**. We mentioned this briefly in section 4.2 when we presented our results for varying opacity, but we added it at the beginning of 6.2 as well for completion.**  
In Section 7, at conclusion #5, should the last two occurrences of 'Matm ~ Mc' rather be 'Matm/Mc'? As I argue above, conclusion #6 remains to be proved. **We corrected the typo, thank you for pointing it out!**  
The authors may want to mention that the decline of the critical mass for envelope collapse with orbital distance was already predicted by the 'radiative' solution of Stevenson (1982), in which the critical mass slowly decreases as the envelope radius increases. The same solution predicts that the critical core mass increases as the opacity grows and reduces as the mean molecular weight increases. **The opacity and mean molecular weight effects are of course not new to our study, they are mainly a way to convince the reader of the reasonableness of our results. More references are added to further emphasize this. We investigated the findings of Stevenson (1982, S82) for orbital distance in detail. The “envelope radius” in Stevenson is a photosphere radius, whose connection to orbital radius is not clear in S82. Moreover, S82 states that “the calculation is insensitive to Ro” (the envelope radius) b/c it appears as log(Ro) (and moreover the cube root). Since a detailed discussion of static radiative solutions is too far afield, we do not feel that our paper is the appropriate place to delve into the details of this model (R06 did investigate these issues in some detail already). However, we do not deny the overall importance of S82 and indeed we emphasize it in many places.**   
  
In the Abstract, I would avoid the second sentence. It is indeed true that the value of 10 Mearth is often quoted, but it refers to an order of magnitude loosely applied to Jupiter and Saturn. It is a well-established fact that the mass for runaway gas accretion depends on several factors. I would mention that the results of this study are based on an interstellar grain opacity **We appreciate the advice and fully agree that the variable nature of the critical core mass is a well-established fact (and we now discuss this in even greater detail in our comparison to previous work). However, the 10 Mearth value remains frequently cited in the literature as a threshold value, often without caveats -- e.g., Lambrechts & Johansen 2012, Ida & Lin 2004 scale the critical core mass to 10 Mearth, Dodson-Robinson et al. 2009 use 10 Mearth as a threshold. In this era of increasing specialization, many conversations with observers or even theorists who are not experts in atmospheric calculations have confirmed to us that deviations from 10 M\_E (and certainly their causes) are not yet widely appreciated. We agree that an interest in reproducing believed values for Jupiter and Saturn is one reason that a greater diversity of values is not appreciated. We feel the community will be better educated if (in only glancing at our abstract) they learn or are reminded that the critical core mass is not fixed at 10 M\_E.**