COMMENTS TO THE AUTHOR:  
  
Reviewer #1: This article explores the evolution of short-period gaseous exoplanets loosing mass to their host stars via Roche-lobe overflow. The goal is to compare the observed population of short period planets to expectations based on theoretical calculations of coupled orbital/structural evolution of mass-transferring planets. The scientific objectives are timely and well-posed, and the results are honestly stated. For these reasons I think the article is worthy of publication. Although the methods and results are similar to recent papers by Valsecchi et al., the question is interesting enough to warrant the publication of this separate investigation.  
  
There is one significant issue that should be addressed before the paper is published: the treatment of angular momentum transfer between an accretion disk and the planetary orbit. I do not believe that the current approximation of total angular momentum transfer back to the orbit is valid, and I think the authors need to explore this issue in slightly more depth. Less efficient angular momentum transfer back to the orbit will shorten RLO evolution timescales (impacting comparisons between observations and expectations), and may even alter the stability of mass transfer. The authors should more carefully estimate the amount of angular momentum returned to the orbit, and determine how this will affect the evolution of the system.  
Most of my other comments are minor, and are enumerated below:  
  
  
Figure 1:  
Since the x-axis of Figure 1 assumes P\_Roche = 9.6 hours, it would be less confusing if the vertical dashed line were drawn and labeled at this period.

>> We’ve added vertical lines showing both P\_Roche, 0 = 9.6 and 12.6 hours to clarify.  
  
"The HAT-P11 system is also noteworthy for a possible 6:1 commensurability between the orbital and stellar rotation periods."  
There is very little evidence for high order commensurabilities between the spins of stars and the orbits of their exoplanets, and no theoretical reason to expect them (that I am aware of). I find this sentence to be slightly misleading, and probably not necessary.

>> We neglected to add the appropriate reference to Béky, Holman et al. ApJ 788(1), 2014 which discusses the observational evidence as well as theoretical justification for a 6:1 commensurability between HAT-P-11’s spin period and the planet’s orbital period. We have now added that reference.

>> Among other possibilities, they explore the idea that differential rotation in HAT-P-11’s photosphere could result in a particular latitude’s having a rotation period commensurate with the planet’s orbit. Then some magnetic or other interaction with the planet could cause a star spot to preferentially form and persist along that latitude, giving rise to the long-lived commensurability observed.

Figure 2:  
How do the authors determine a/a\_Roche for the exoplanets on this plot? The ratio a/a\_Roche is directly related to the density of the planet and the orbital period. Are the authors assuming a density of 1 g/cm^3? This should be stated clearly, because the density of exoplanets can vary greatly and can create scatter on this plot.

>> For that Figure 3, a\_Roche was estimated from the available system parameters using data harvested from exoplanets.org on 2015 Jul 8. We’ve added a note to the manuscript to indicate this.  
  
"1. Mass transfer completely conserves angular momentum, in which case gas escaping the planet forms a thin accretion disk around the star and rapidly transfers all of its angular momentum back to the planet before falling onto the star."  
  
I do not think this assumption is justified. Angular momentum can be lost from the orbit for at least three reasons: 1. Mass transfer may not be conservative. Let's ignore this for simplicity, even though non-conservative mass transfer could be important. 2. The transferred mass may not form an accretion disk since the star's radius is a considerable fraction of the Roche limit. This is known as direct impact accretion. I don't think it occurs here but it would be best for the authors to demonstrate this. 3. Even if the transferred mass does form an accretion disk, it can't transfer all of its angular momentum back to the orbit because, once again, the star's surface is a considerable fraction of the Roche limit. The accreting gas therefore still has substantial angular momentum when it hits the surface of the star, and this angular momentum is lost from the orbit.  
  
Perhaps it is the case for planets that the loss of orbital angular momentum does not destabilize the mass transfer process, but simply shortens the evolutionary timescale. Nonetheless, I think this issue should be addressed since the evolutionary timescales may be comparable to the ages of the systems. It may be necessary to re-run the orbital evolutions presented in this paper to account for the loss of orbital angular momentum. Some good references can be found in literature on mass transferring white dwarfs: articles by authors including Marsh, Nelemans, Verbunt, Rappaport, and Lubow might be useful.  
  
RLO Results from MESA:  
What is the initial spin period chosen for the star? It probably has little effect on the evolution since the star should quickly spin down to periods longer than the orbital period, but should be stated. The link to the MESA inlists appears to be broken.

>> We plan to post the inlists after the article is accepted for publication. We have added the following line to the first paragraph of Section: “All host stars have $M\_\star = 1\ {\rm M\_{Sun}}$, a Sun-like initial rotation velocity (2 km/s), and $Q\_\star = 10^5$.”

Figures 4 and 6:  
The x-axis is mislabeled (wrong units).

>>Fixed. Thanks.  
  
Just above section 4:  
we'd --> we would

>> Changed

Just after equation 10:  
The next equation should be numbered, and a citation should be provided for this mass-radius relation.

>> Done  
  
Paragraph beginning: "Using Figure 8, …"  
I find this description and Figure 8 a little confusing. The way it is written, it sounds like there is something important about the ratio rho\_p/P\_Roche. Really rho\_p and P\_Roche are just related by the requirement of filling the Roche lobe. It would help to relabel the lines and rewrite the text such that this doesn't appear to be a ratio. Also, it seems a planet won't get stranded at an inflection point of the contours, but rather at the extrema of the countours tangent to the appropriate M\_core. "Inflection point" should be changed in the paragraphs below, as well.  
  
Near top of text on page 18: "can the planet from 82 to 66 hrs"  
Omitted word

>> Sentence now reads “In fact, tidal decay with the planet's current mass and $Q\_\star = 10^5$ can move the planet from 82 to 66 hrs in about 2 Gyrs …”.

Figure 10:  
Based on Figure 8, P\_Roche,max occurs when f\_env ~0.5. Naively, planets may get stranded near this value of f\_env. So why should we expect planets with f\_env < 0.1 to lie near the solid line in Figure 10? I suppose this would happen if the planet is initially stranded at f\_env=0.5 and then photoevaporated down to f\_env<0.1, but of course this depends on the efficiency of that process.

>> Photoevaporation may remove the rest of the atmosphere, but it doesn’t have to, unless we are considering whether a planet known not to have a substantial atmosphere is a remnant core. We’ve modified the second paragraph appearing after Figure 8 to include the following:

>> As $f\_{\rm env} \rightarrow 0.5 \approx 10^{-0.3}$, that $P\_{\rm Roche}/\rho\_{\rm p}$ contour turns over, $\rho\_{\rm p}$ increases while $P\_{\rm Roche}$ decreases, and presumably the planet would either move back in toward to the star or would be stranded near this $P\_{\rm Roche, max} = 19\ {\rm hrs}$. The rest of the atmosphere may be shed by photoevaporation, depending on the rate of evaporative mass loss. In any case, for this simple scenario, a planet with a known $M\_{\rm core}$ (and which may or may not retain a substantial atmosphere) should have a period near the inflection point in contour that passes nearest its $M\_{\rm core}$-value.

Are there any known exoplanets that have been estimated to have f\_env ~0.5 (I'm guessing this would be very difficult to determine), and if so, where do they lie on this plot? Where is Kepler 21b and GJ 1214b?

>> The referee is correct: for planets with substantial gaseous envelopes, determining fenv (or Mcore) is difficult since the mass-radius relationship is not terribly sensitive to those parameters in that range of compositions. Lopez & Fortney (2014) estimate that GJ 1214 b has fenv = 3.83%, as we say on p. 17 of the manuscript. Kepler-21 b has a mass and radius consistent with no gaseous envelop, as we say on p. 18.   
  
EDITOR'S COMMENT:  
- If the mention to the commensurabiity of HAT-P11 is kept in the paper, the analysis of Béky, Holman wt al. ApJ 788(1), 2014 should be mentionned.

>> Thanks. We have added this missing reference.