Dear Dr. Shafter,

We thank you for organizing the review of our paper, and also
thank the referee for constructive criticism. We have considered
the referee's remarks carefully and revised our manuscript
accordingly.

Below, we have reproduced the relevant portions of the referee's
report, alongside our responses. A list of changes is appended to
the manuscript using the trackchanges AASTeX macros.

Sincerely,

Luke Bouma

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REFEREE COMMENT

Overall: It would help if the authors used hours or days
throughout the paper in a consistent manner. The period is
referred to as 0.5d and 11.98h just a couple paragraphs apart
(e.g. in Section 5.1). The abstract uses hours, most plots use
days.

RESPONSE

> Throughout the manuscript, we have shifted the units wherever
> possible to hours.
>
> Exceptions include the ephemeris (which is quoted in days), and
> the periods listed in Figures 3 and 6, which are quoted in
> days. The former is standard convention. The latter was because
> we felt it important to list the periods used to generate the
> figures at a higher precision, and quoting higher precision in
> units of hours seems atypical.

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REFEREE COMMENT

Introduction: The introduction takes a light/jovial tone, e.g.,
did they actually wish it were a planet, or is it more that they
aimed to confirm it and found the opposite? I would normally
suggest making more formal. However, I enjoyed reading it. I'm
sorry the authors ruined their own favorite planet.

RESPONSE

> So are we! Our aim was to clarify what could be happening in
> the system, and hopefully this contribution is valuable for
> that. When a planetary door closes, a window on young star
> variability opens...

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REFEREE COMMENT

Introduction: I do not think this is youngest hot Jupiter. That
should be true just for transiting systems, and it was the
youngest at discovery, but the two RV planets from ESPaDOnS are
younger (Donati et al. 2016 and Yu et al. 2017). Both are hot
Jupiters and in Taurus. I believe the age of Tap 26 is more
ambiguous due to age spreads (multiple groups?) in Taurus, but
the age of V830 Tau is certainly <3 Myr.

RESPONSE

> Thank you for this correction. We have qualified the relevant
> sentence as the youngest known *transiting* hot Jupiter, and
> added a footnote to cite the V830 Tau and TAP 26 results.

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REFEREE COMMENT

Introduction: I also think it wasn't the first H-alpha detection.
That belongs to LkCa15, which was imaged in H-alpha in 2015
(Sallum et al. 2015). Although that planet might not exist, so
this comment is probably OK. It would still be the first close-in
planet either way. I'd suggest weakening the statement slightly
to err on the side of caution.

RESPONSE

> We followed the referee's suggestion by stating that PTFO 8-8695
> would have been the first close-in planet for which Hα emission
> was detected.

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REFEREE COMMENT

Introduction: Barnes et al. (2013) claimed that the planet and star precessed on a period of 300–600 days, and that this could somewhat fit variable depths and missing transits. The Ciardi et al. 2015 paper uses this as their model, in addition to gravity darkening as mentioned in the current introduction. I suggest mentioning the precession of the planet in the introduction, as many papers used the precession model or at least discussed it. The line "...the gravity-darkened star precessed..." seems to allude to this, but it's unclear from the rest of the text.

RESPONSE

> We have changed the wording of the last sentence of the first
> paragraph in the introduction to use the word
> "precess", rather than "torque".

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REFEREE COMMENT

Section 2: The first paragraph discusses the full frame images, while the next section discusses the 2m data. Why did the authors use the FFIs when they have the (higher quality) 2m data?

RESPONSE

> For the essential analysis of the study (i.e., extracting and
> fitting the long and short signals), we used the 2m SPOC
> lightcurve.
>
> Our discussion of the FFI lightcurves in Section 2 was motivated
> by the discontinuity in the light curve at BJD 2458488.3,
> which was eventually excluded from further analysis. We wanted
> to compare the FFI and 2m light curves to see whether this jump
> was an instrumental systematic or a software systematic.
>
> A secondary reason to introduce the concept of FFIs in Section
> 2 is that they are used to measure the variability amplitudes
> of the other members of 25 Ori-1 in Section 4.3.

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REFEREE COMMENT

Section 2.2.2: "These details concerning the group membership for one object may seem excessive to those accustomed to the simple distinction between "young cluster members" and "old field stars". I suggest removing this. It's not adding anything and sounds like the authors are making assumptions about the reader.

The membership of this object is essential for an age assessment,
and can/should be updated given the new Gaia data. Since most of
the object class in question are at similar ages, this directly
relates to the study. If it had turned out to be ~100 Myr, we
would all be more skeptical (although ~100 Myr stars like this
are known, they are more rare).

RESPONSE

> We appreciate this suggestion and have omitted this sentence.

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REFEREE COMMENT

Section 3/4: This deals mostly with identification of the second
star. However, the second star appears to be an assumption of
section 3.2. If the periods come from two stars that could be
separated (e.g., wide binaries), it would have been best to do so
rather than try to model both by including fs and fl terms. So
it is odd that the binary search is after the light curve model
in the paper. It might make more narrative sense to move the
light curve model to after section 4, since 4 motivates 3.2.

RESPONSE

> We considered re-organizing the paper as the referee suggested,
> but ultimately decided against it. We chose to present the
> TESS data, and show the 2 signals, which raises the possibility
> of binarity - which is then investigated further with Gaia.
> The referee suggests showing the Gaia evidence for binarity, and
then
> showing the TESS data with 2 signals. To us, there does not seem
> to be any advantage to inverting the order.

****
REFEREE COMMENT

Section 4.3: My biggest concern with this analysis is reliance on
the chi^2 from Gaia to assess binarity. This is not the suggested
path from Gaia, as it is subject to a noise model Gaia now knows
has issues. For example, nearly 100% of mid/late M dwarfs have a
high chi^2 (and a large excess astrometric noise), but clearly
they are not all binaries. This is (partially) because of a
color-term in the PSF model that was not initially understood.
Similarly, almost all bright stars have a large RUWE, as do the
bluest stars. The authors correctly use RUWE, the suggested
method, but the RUWE analysis is less convincing and clearly the

authors are leaning on the chi^2.

A good reference on this would be Belokurov et al. 2020, which shows what kinds of binaries RUWE can and cannot detect. This binary might simply be too tight to create an astrometric signal (in which case the RUWE makes sense). The typical cutoff for binarity is a RUWE of ~1.4, see Rizzuto et al. 2018, Ziegler et al. 2019, Pearce et al. 2020, among many others. I suggest removing the chi^2 analysis. If the authors want to keep it, they need to argue why it is a more meaningful metric than RUWE, and attempt to account for color and other parameters that impact chi^2.

RESPONSE

> We appreciate this criticism. We've followed the referee's advice
> and replaced this sub-plot with RUWE instead of reduced chi^2.
> The text in Section 4.3 and captions have been updated accordingly.

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REFEREE COMMENT

Section 4.4: Need a period between velocities and Further.

RESPONSE

> Corrected, thank you.

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REFEREE COMMENT

Section 5.2: That clear offset at (Pl-Ps)/Ps doesn't seem like a coincidence, and the paper reads like the authors agree, but couldn't explain it. I'm not sure I can help much here, although it is interesting. Could this be that both stars are dippers and one turns off while the other turns on by coincidence? Spots and hot-spot clusters can shift in phase as the magnetic fields realign, which creates a new spot cluster at the same period but a different phase. If it is circumstellar material, a tight binary could be shepherding the material, but I think the systems would have to be very tight for that, and it could not last for such a long time.

I think this can be assessed statistically; given the scatter in those points, the odds of lining up to any given line is small.

But without a physical explanation this is hard to test. Probably this requires looking at many dippers as an ensemble. It is not required for the primary analysis of this paper, so I will defer to the authors if they want to dig deeper on this.

RESPONSE

> Indeed, we are not sure whether or not it is a coincidence, but
> we thought it was interesting enough to mention, even though we
> do not have an interpretation.
>
> It seems unlikely in our view that both stars are showing dips.
> Our interpretation of the two smooth photometric signals is
> that one star is rotating at 11.98 hr, and the other star is
> rotating at 10.76 hr.
>
> All of the dips occur with a periodicity of 10.76 hr. (If we
> make Figure 6 but instead phasing by P_long, it yields a mess
> of points). Therefore all the dips are presumably associated
> with the star that has that rotation period.

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REFEREE COMMENT

Section 5.3.2: The difference between the fraction of transient flux dip stars and the binary fraction is more significant than it appears to the authors. High mass ratio binaries would not be included, as they are too faint to detect the second period. Further, at the distance of Upper Sco (~150pc) and assuming Stauffer et al. 2017 would notice/separate two stars wider than a K2 pixel (4"), the presence of two periods would not include binaries wider than 600pc. Eyeballing the Winters et al. 2019 distribution suggests ~20% of binaries are beyond this. Stauffer et al. 2017 also probably checked against existing imaging data for obvious binaries down to 1-2", so likely even more are getting cut.

RESPONSE

> We have updated the text to include this point -- thank you for
> making it.
>
> We have also updated the text to reflect our hesitation about
> whether or not the phenomenon is associated with binarity,
> primarily because multiple photometric periods do not
> necessarily imply binarity.

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REFEREE COMMENT

Section 5.4: The authors conclude this subsection with a reasonable argument that this isn't a protoplanet. Does this same argument not rule out gas/debris at the Keplerian coronation radius? What could maintain such clouds for years/decades and then have them jump phase but at the same period? It's hard to do this if it is not somehow physically attached to the star in some what (e.g., spots can do this).

RESPONSE

> This comment caused us to consider more carefully the merits
> and weaknesses of the two hypotheses we discussed.
> We ended up with a stronger preference for
> clumpy torus model over the spot occultation model. The reasons
> for the preference are given in the text: 1) the spot
> occultation model seems more geometrically contrived than we
> initially thought, and 2) the Halpha excess seen by
> Johns-Krull+16 (and also in unpublished observations by
> Stauffer of scallop shells) seems likely to require hydrogen in
> orbit near the corotation radius.
>
> In response to the question of: "why can the shifting phase be
> explained more easily by dust/gas clumps than the
> protoplanet?", we now reference D'Angelo and Spruit 2012 (see
> in particular their Section 1), which discusses the possibility
> of cyclic accretion, with periods of "stalled" mass
> accumulation outside the corotation radius alternating with
> accreting phases.
>
> In addition to these two models, after reflecting, we became
> persuaded that an accretion hotspot passing behind the star
> could also produce the needed dips in PTFO 8-8695. Although we
> do not favor this explanation, we now also discuss it in
> greater depth in Section 5.4.

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ADDITIONAL CHANGES

A few additional changes are noted in the "List of Changes" appended at the end of the manuscript.

We modified the text throughout to clarify that Stauffer+ 2017 favored the explanation of a clumpy torus of gas and dust at the Keplerian co-rotation radius, rather than our initial (incomplete) description of "clouds of gas" at the same location.

We also modified the labels of the axes in Figure 5a for clarity.

We have also added a few citations that we omitted in the original manuscript.