Dear Dr. Endl,

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. Figures supporting the verification of our SB2 injection-recovery test, and supporting our radial velocity semi-amplitude/period upper limits have also been uploaded for inspection.

Sincerely,

Luke Bouma

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REFEREE COMMENT

It appears that TOI 837 b was previously noted as a candidate planet and IC 2602 member in Nardiello et al. 2020 (https://ui.adsabs.harvard.edu/abs/2020MNRAS.495.4924N/abstract). If the authors were unaware, it is still worth mentioning this in the text.

RESPONSE

> Thank you for noting the omission. We have added the relevant
> citation in Section 2.1.

****
REFEREE COMMENT

Sec. 2.1
A number of validation tests are listed, but no detail is provided about how these tests work. For example, what is the ghost diagnostic test?

RESPONSE

> For the sake of brevity, we prefer to continue with our
> refererence to Twicken et al., 2018. The optical ghost
> diagnostic test in the SPOC pipeline (Twicken et al., 2018,
> Section 3.7) flags threshold crossing events that are likely to
> be caused by reflections of light from bright sources between
> CCDs. Since few people know what it is, and it will therefore

> confuse rather than clarify, we have omitted it from the
> manuscript.

****
REFEREE COMMENT

Sec. 2.1
Have the authors performed a by-eye search for additional
transits in the light curve?

RESPONSE

> We did perform a by-eye search for additional transits in the
> light curve, and we did not find any compelling events. The
> logical next step -- quantifying our sensitivity through
> injection-recovery simulations -- would be an interesting
> exercise. For it to be valid, care would need to be taken to
> marginalize out dependence on the detrending method (e.g.,
> David et al 2019 and 2020's experience which led to the
> four-planet V1298 Tau system). Given the required level of
> effort, we feel this exercise to be outside the scope of the
> present analysis.

****
REFEREE COMMENT

Sec. 2.2
Given that Gaia typically resolves binaries at separations > 1
arcsec, is the blending scenario between Star A and TOI 837
plausible?

RESPONSE

> Referring to the "Gaia" line in the bottom sub-panel of our
> Figure 6 (which came from Rizzuto et al., 2018), at 2.3
> arcseconds of separation between Star A and TOI 837, Gaia
> typically resolves binaries to brightness contrasts of ΔG ~=
> 5.0. The actual brightness difference between the two is 4.7
> G-band magnitudes. So yes, we believe that the blending
> scenario is plausible, primarily because Star A is much fainter
> than TOI 837, placing it near the resolution threshold.

****
REFEREE COMMENT

Sec. 2.2
It seems another line of evidence against Star B (rho=5.4 arcsec)

as the host is the difference imaging centroid test mentioned in Section 2.1, which they state localizes the transit to within 2 arcseconds of the target.

RESPONSE

> Correct -- we have modified the wording in Sec 2.4 to clarify
> this point.

****

REFEREE COMMENT

Given that TOI 837 and Star A have a large contrast but similar distances, I would expect the two stars to have a large difference in their expected densities. Can the mean stellar density from the transit fit be used to further argue that the planet orbits TOI 837?

RESPONSE

> Our thanks for this point. We have added a paragraph at the end
> of Section 2.4.1 to this effect, though we continue to place
> the strongest weight on our tests from the seeing-limited
> photometric timeseries data.

****

REFEREE COMMENT

The orientation of Figure 2 is confusing. The axes are labeled as right ascension and declination, but the lines of constant RA and Dec are at an angle.

RESPONSE

> We have updated Figure 2, and opted to drop the axis labels
> entirely. We updated the caption to emphasize the orientation
> as given by the compass.

****

REFEREE COMMENT

Sec. 2.3
Suggest swapping the order of Figures 3 & 4, as the high resolution imaging is discussed before the ground-based light curves.

RESPONSE

> Done, thank you for the suggestion.

****
REFEREE COMMENT

Sec. 2.5.1
For the secondary spectral line injection, it is not clear whether a synthetic spectrum was added to the raw spectrum before deriving the LSD profile - or whether a synthetic LSD profile was added to the true profile. It is not clear that the two procedures would yield the same results.

RESPONSE

> We have updated the manuscript to clarify that the SB2
> grid-search injection and recovery exercise was conducted with
> the secondary signal injected into the LSD profiles, not the
> spectra.
>
> To test the validity of this technique, we injected a second
> set of lines to the original spectra, and compared the
> resulting derived LSD profiles. To inject a second set of lines
> to the raw spectrum, we made use of a high SNR CHIRON spectrum
> of a G0 RV standard star, scaled its flux, and blended it with
> the averaged observed spectrum of TOI-837. We then performed
> the LSD routine on the blended spectrum to derive its LSD
> broadening profile as we would for a normal observation.
>
> The attached figure shows the resulting LSD profiles from our
> injection test. The left column (black) shows the LSD profiles
> when the secondary signals were injected directly into the LSD
> profile. The right column (red) shows the results of our
> blended spectrum analysis. There is minimal difference between
> the two injection techniques.

****
REFEREE COMMENT

Sec. 3.1.3
The outer separation of the "Not SB2" constraint in the top panel of Fig. 6 seems optimistic. Wouldn't the velocity separation of a binary with a=100 AU be less than the broadening of 15 km/s?

RESPONSE

> Correct -- thank you raising for this point. We have amended
> the outer limit in Figure 6 for the "Not SB2" associated

> companion constraint to the distance at which the Keplerian
> orbital velocity falls below the rotational broadening. The
> text in Sec 3.1.3 has been accordingly.
>
> This weaker constraint now leaves a miniscule region of the HEB
> parameter space open. We do not think that this warrants a
> recalculation of the false positive probability for the system,
> but nonetheless we have added a footnote in the first paragraph
> of Section 3.2 to draw attention to this point.

****
REFEREE COMMENT

Sec. 3.1.4
It is not clear how the range of K-P parameter space stated was
explored in detail. MCMC sampling is inefficient for multimodal
distributions, and given the sparse sampling of the RV time
series, it seems likely that multiple modes exist. Was K fit
independently over a grid of periods? Orbital inclination would
also be an important parameter to include. If models were used to
transform mass ratio limits to contrast limits this should be
explicitly stated.

RESPONSE

> Prompted by this comment, we have amended our method for sampling
> K-P parameter space to one that we feel is more robust against
> inefficient sampling and the multimodal nature of the long-period
> detection problem. The last paragraph of Section 3.1.4 has been
> amended to the following:
>
> """
> To place limits on the properties of a possible bound hierarchical
> companion, we performed the following injection-recovery
> exercise. We simulated 10^6 two-body systems with random orbital
> phases and inclinations, and drew their semi-amplitudes and periods
> from logarithmic distributions: $K\ [{\rm m\,s^{-1}}] \sim
> \log\mathcal{U}(1,10^7)$, and $P\ [{\rm days}] \sim \log
> \mathcal{U}(1, 10^{15})$. Again assuming circular orbits, we then
> analytically evaluated what the radial velocities would have been at
> the observed FEROS times if the system had the assumed parameters.
> We then calculated what the linear slope would have been for each
> simulated system. If the absolute value of the slope exceeded our
> 3-σ limit of $|\dot{\gamma}| < 0.82\,{\rm m\,s}^{-1}\,{\rm
> day}^{-1}$, we assumed that we would have detected such a system.
> Figure~\ref{fig:fpscenario} shows the resulting limits; weakened
> sensitivity at harmonics of the baseline occur at lower masses and
> smaller projected separations than shown on the plot. The conversion
> from mass to brightness contrast was performed using the same

> isochrone models and assumptions as in Section 3.1.2.}
> """
>
> As a supplement for the referee's inspection, we also attach a plot
> showing the simulated "detections" and "non-detections" directly in
> the Msini vs semi-major axis plane (yellow points are detected;
> purple are not).

****
REFEREE COMMENT

Sec. 3.2
The following sentence is somewhat imprecise: "The relevant
populations need to be modeled at the Monte Carlo level." Monte
Carlo is a sampling method, whereas the "modeling" step lies in
the choice of probability distributions for the variables being
sampled. Also, is it the sampling that gives a more reliable
estimate of the FPP or is it the Bayesian framework, which
incorporates priors from population occurrence rates?

RESPONSE

> Thank you for this point. We have revised the relevant
> sentences in Sec 3.2 to clarify that the probabilistic
> framework is the important aspect, not the sampling technique.

****
REFEREE COMMENT

Does the secondary eclipse non-detection apply to a specific
phase (e.g. 0.5) or across all phases?

RESPONSE

> All phases -- we have revised the text in Sec 3.2 to clarify
> this point.

****
REFEREE COMMENT

Sec. 4.1.2
The statement that TOI 837 lies along the empirical cluster
isochrone and this limits the presence of a companion to be <50%
the brightness seems to be misstated. I believe the authors mean
that TOI 837 appears to lie below the equal-brightness binary
sequence (which sits 0.75 mag above the single star sequence).
This limits a hypothetical companion to be less than the

brightness of the target star (or less than 50% the total system
brightness).

RESPONSE

> Thank you for raising this point. Our original writing was in
> error, because we meant something slightly stronger. If TOI 837
> were an equal-brightness binary system, it would be 0.75
> magnitudes above the single star sequence. However TOI 837 is
> not only not on the equal-brightness binary sequence -- it is
> not at all elevated with respect to the single-star sequence. A
> binary companion with say half the brightness of the primary
> would make the system 0.44 mags brighter than the single star
> sequence. Based on the intrinsic scatter in the HR diagram,
> this is also ruled out. We (imprecisely) meant this second
> point, and have modified the text accordingly.

****
REFEREE COMMENT

Sec. 4.2.4
It is not immediately clear that the mass-temperature-age
combination of ~1.2 Msun, 5960 K, and 30-45 Myr is incorrect. The
mass-temperature relation for stars in this mass range is not the
same on the pre-main sequence as it is on the main sequence.

RESPONSE

> Looking at the MIST isochrones, it seems that a
> solar-metallicity, 1.1Msun star reaches the ZAMS around
> log(age) of 7.5, or ~32 Myr. So, TOI 837 seems likely to be
> roughly at the ZAMS, rather than particularly early in its
> pre-main sequence contraction.
>
> Regardless, we have modified the wording here to reflect that
> our preference for the stellar parameters from "Method 2" is
> linked to our ability to more easily quantify its uncertainties
> (statistical and systematic -- see below).

****
REFEREE COMMENT

Sec. 4.2.4
The authors are encouraged to explore the effect of adopting a
different set of evolutionary models. The Dartmouth models of
Feiden (2016), which include a magnetic field prescription, have
been shown to more accurately reproduce the parameters of
pre-main sequence stars. In the mass range of TOI 837 the effects

may not be dramatic, but are likely to be significant compared to
the quoted uncertainty on e.g. the stellar mass. I would expect
that a 10% uncertainty on the stellar mass would be more
realistic (in the sense that it would accommodate systematic
uncertainties) than the currently quoted 1% uncertainty.

RESPONSE

> Thank you for this suggestion, which we agree is important. We
> amended Section 4.2.4 to explore the use of the PARSEC models
> in addition to the MIST models. (We checked the Feiden 2016
> Dartmouth models, but could not find synthetic Gaia-band
> photometry, which would have complicated the comparison between
> between the Gaia photometry and the models.) The differences
> between MIST and PARSEC are now described in the text, and are
> used to estimate systematic uncertainties on the stellar
> parameters. These uncertainties are now reported in Table 4,
> and they are also propagated through the transit fits (Table 5;
> Figure 1; Figure 4; Figure 10).
>
> Compared to the initially submitted TESS-only transit fit, the
> wider priors on R* and logg* exacerbated the impact parameter
> vs Rp/R* degeneracy. With the wider stellar density prior,
> impact parameters much greater than 1 became statistically
> allowed, though not favored, if the planet-to-star radius
> ratio were to be very large. Our ability to break the
> degeneracy was broken! Given only the TESS data + the
> isochronal stellar parameter priors with systematic
> uncertainties included, only a one-sided limit on the planet
> radius can be derived.
>
> One way to "fix" this issue would have been to impose a prior
> constraint that Rp<3Rjup, based on our mass upper limit of
> ~2Mjup and the fact that all known planetary and stellar
> objects follow mass-radius relations that would require the
> planet to be smaller than 3Rjup. However, this approach would
> be somewhat atypical, and even when doing it, we found with our
> updated stellar uncertainties that the TESS-only model (with Rp
> limit prior) gave:
>
>   TESS-only, Rp<3Rjup assumed, systematic star uncertainties:
>   r_planet [Rjup]: 0.836 +0.208 -0.121
>   b: 0.957 +0.027 -0.017
>
> where we quote median, 86th, and 14th percentiles. Conversely,
> the fit to the combined TESS+ground data yielded a planet
> radius more precise by a factor of ~2x:
>
>   TESS+ground, broad Rp/R* prior, systematic star uncertainties:
>   r_planet [Rjup]: 0.768 +0.091 -0.072

>   b: 0.936 +0.013 -0.010
>
> Given the increase from 5 to 12 transits, this is presumably
> because the transit depth is measured more precisely. (Both
> sets of chains are fully converged, with Rhat-1 < 1e-4).
> Despite our initial reservations about using the ground-based
> data for the planet parameters, the combined data do yield a
> more precise fit, and also seem to do a better job at breaking
> the Rp/R* vs b degeneracy. These two points persuaded us to
> adopt it as our preferred solution. Nonetheless, this decision
> is one of preference, and not modelling necessity. We have
> amended Section 4.3 at some length to present the points made
> in this response, and to emphasize the key results from both
> approaches. Consequent changes include:
>
> * Bottom panel of Figure 1 now shows the TESS+ground data
>   phase-folded, instead of TESS-only. Caption was updated
>   accordingly.
>
> * Figure 10 is now the posterior from TESS+ground, instead of
>   TESS-only.
>
> * Figure 11 was updated for the new planet radius.
>
> * The planet radius was updated in the abstract and throughout
>   the manuscript.

****
REFEREE COMMENT

Sec. 4.3
The authors perform a joint fit of the TESS and ground-based
photometry to derive a more precise ephemeris, but fit only the
TESS data for the remaining planet parameters. If the errors on
the respective data sets are properly scaled (including jitter
terms if needed), the extra noise from the ground-based
photometry should not significantly impact the inferred
parameters when including those data. This is merely a comment,
and no extra work is necessary.

RESPONSE

> As described above, we have amended our preferred solution to
> the TESS+ground solution, rather than the TESS-only solution.

****
REFEREE COMMENT

Given the planet's young age, it would be interesting to know if the transit photometry meaningfully constrains the eccentricity (which along with impact parameter could explain the short duration). Was a transit fit including eccentricity performed? If not, there are methods to infer the eccentricity from the posteriors of a circular orbit fit (see Dawson & Johnson 2012).

RESPONSE

> While we agree that the possibility of a non-circular orbit
> would be interesting, we do not expect that given the present
> data we can make credible statements about potential non-zero
> eccentricities. We have updated the last paragraph of Sec 4.3
> to justify this assumption of a circular orbit, and have
> amended the conclusion to mention this as a possible area for
> future work.

****
REFEREE COMMENT

-"The condition for a grazing transit is whether the impact parameter b is below 1 - Rp/R*" → below should be replaced with "above" or "greater than".

RESPONSE

> Corrected, thank you.

****
REFEREE COMMENT

Discussion
More emphasis could be placed on the unusually large size of the planet. Some speculative discussion on the nature of this inflation could be warranted. Figure 11 might also be revised to also reflect the effect of insolation (or the data plotted could be restricted to close-in planets).

RESPONSE

> Thank you for this suggestion. We have added a sub-plot in
> Figure 11 showing planet sizes and orbital periods, with the
> sub-100 Myr old planets emphasized. We considered also
> including a plot with planet insolations on the ordinate
> instead of orbital periods, but the NASA Exoplanet Archive
> column for planet insolation is much less complete, presumably
> owing to the sparsity of available stellar luminosities. We
> have also added some words in the discussion to the effect that

> that the young planets could be large because of i) a selection
> effect or ii) atmospheric escape.

****
REFEREE COMMENT

The paper ends on a note about nodal precession. Is it simple to compute the nodal precession timescale given some reasonable assumptions? This might help determine how likely it is that the effect may be observed in the near term.

RESPONSE

> Our thanks for this comment, which prompted us to calculate the
> precession frequency of the orbital axis due to the stellar
> oblateness using equations from Anderson+2018 and Lai+2018. We
> found that given the observed parameters of the system and an
> assumed stellar Love number (k2) equal to that of the Sun, the
> precession period would be 45,000 years. To complete say 1% of
> a precession cycle, which might roughly be what is needed to
> observe noticeable changes, we would need to wait ~450 years.
>
> We have correspondingly amended the last sentence to omit the
> point on nodal precession, in favor of the point that more
> photometry should improve our ability to constrain the orbital
> eccentricity, and will also let us search for additional
> transiting planets.

****
ADDITIONAL CHANGES

A few additional changes, most of which are noted in the "List of Changes" appended at the end of the manuscript:

* Added citation to Bonomo+2017 in the introduction.

* Citations to Donati+2020 and Damasso+2020 are added to clarify that the planetary nature of the RV-detected hot Jupiters in Taurus has been debated.

* Last paragraph of Sec 2.1: added transition sentence.

* Acknowledgements to K. Anderson, and to the referee, have been added.

* Table 5 has new rows for the ground-based transit nuisance parameters, and the posterior values are updated for the propagation of stellar parameter uncertainties.