Dear Dr. Jackson,

We thank you for organizing the review of our paper, and also thank the referees for constructive criticism. We have considered the remarks of both of the reviewers carefully and revised our manuscript accordingly.

Below, we have reproduced the relevant portions of the reviews' reports,
alongside our responses. The changes are bolded in the attached ms_boldchanges.pdf file, using the trackchanges AASTeX macros.

Sincerely,

Luke Bouma

****
Reviewer #1:

REVIEWER COMMENT

Line 57 (evaporation): Consider adding citation to Lopez et al. (2012) or similar

RESPONSE

> Done, thank you.

REVIEWER COMMENT

Line 78 (Kepler ages): Consider adding citation to Walkowicz & Basri (2013)

RESPONSE

> Done, thank you.

REVIEWER COMMENT

Line 93: Clarify origin of name delta Lyr, given there are two pre-existing names, and how they overlap. It might also be worth mentioning Kerr's Cep-Her association as in prep just to save future readers the headache of cross-identifying them, either here or in Sec 2.1

RESPONSE

> delta Lyra cluster was the first name suggested by Stephenson
> (1958), and it is the default name listed in SIMBAD. It was also

> the name used in the title of Eggen's 1967 paper.
>
> The cluster was called Stephenson-1 by people other than
> Stephenson, including by Kounkel & Covey 2019. The Kounkel & Covey
> 2019 "Theia" catalog identifiers are simply given to any cluster in
> their catalog, in addition to any previous names that existed.
>
> The overlap between the earlier lists (Stephenson 1958, Eggen 1968,
> Kharchenko et al. 2012) and the Theia 73 list from Kounkel & Covey
> 2019 is only present in the stars closest to delta Lyr itself. For
> instance, see the 30 stars shown in Figure 1 of Eggen 1968. The
> broader "complex" shown in Figure 1 of our manuscript was to our
> knowledge first reported to be a set of kinematically associated
> stars by Kounkel & Covey -- inasmuch as an "HDBScan group" implies
> kinematic association.
>
> Regarding Ronan Kerr's view of the delta Lyr cluster as a component
> of the larger Cep-Her complex, we would prefer to leave this as
> Ronan's result to present. There's significant work here in
> designing the clustering method and verifying that it works.
> There's also work in understanding the overall structure of the
> broader association (which is different in nuanced ways from the
> gray Theia 73 points in Figure 1). Given these complications, and
> the potential importance of the complex given its proximity and
> age, we really would prefer to leave this for the in prep R. Kerr
> work. And similarly, the introduction of the term "Cep-Her
> complex" is probably best left for that future work, where it can
> be mentioned that the "del Lyr cluster" is a subset of this
> complex.

REVIEWER COMMENT

Line 165: Consider mentioning the axes on which data was cleaned
(e.g. color precision)

RESPONSE

> Done - thank you. We have added the sentence: "This also involved
> cuts on the photometric signal to noise ratio, the number of
> visibility periods used, the astrometric χ^2 of the
> single-source solution, and the $G_{\rm BP}-G_{\rm RP}$ color
> excess factor."

REVIEWER COMMENT

Line 179 (reddening): What does "reasonably well" mean? Something
like 1-2 sigma?

RESPONSE

> Thank you for catching the imprecise wording. The revised text now
> reads as follows: "These values are within a factor of two of
> previously reported values in the literature (citations), and are
> all small enough that the choice of whether to use them {\it vs.}
> other extinction estimates does not affect our primary
> conclusions."
>
> More quantitatively, from the Lallement maps we find
> $E(B-V)=\{0.084\pm0.041, 0.020\pm0.003, 0.045\pm0.008,
> 0.032\pm0.006\}$ for UCL, IC 2602, the Pleiades, and the delta Lyr
> cluster respectively. The relevant literature reports:
>
>   Gaia+2018 E(B-V):
>   Pleiades 0.045
>   IC2602 0.031
>
>   KC2019: E(B-V)
>   Pleiades 0.071
>   IC2602 0.078
>   del Lyr cluster 0.059
>
>   Bossini+2019 E(B-V)
>   Pleiades/Melotte22 0.045
>   IC2602 0.031
>   del Lyr cluster 0.051
>
>   Randich+2018 E(B-V):
>   IC2602 0.032 - 0.092
>
>   Pecaut & Mamajek 2016 Table 7 UCL members E(B-V)
>   UCL 0.059
>
> hence our assessment of "it's within a factor of two, and doesn't
> matter much." Given that all these clusters are within ~400
> parsecs, this perhaps isn't too surprising.

REVIEWER COMMENT

Fig 3: It may be helpful to include one of the even younger clusters
on the CMD (i.e. to bracket delta Lyra), though perhaps it makes the
plot too busy.

RESPONSE

> Please see camd_with_UCL.png attached. This does make the plot a
> bit too busy, in our opinion.

REVIEWER COMMENT

Page 3: Consider re-organizing. Based on the 1st paragraph, I thought

the quantitative analysis would only deal with Pleiades and IC 2602
and one might infer reddening was only calculated for these clusters
(whereas I assume the latter are also corrected for reddening prior
to the analysis described). My suggestion would be to introduce all
the clusters first. It would also be helpful to list the age of IC
2602 at some point in the text.

RESPONSE

> Thank you for this suggestion. The first paragraph of Section
> 2.2.1 has been reorganized to now introduce all five of the
> relevant clusters: UCL, IC 2602, del Lyr cluster, mu-Tau, and the
> Pleiades. The second paragraph has been modified to account for
> this change. The extinction for UCL is now also listed in the
> text. So is the adopted age of IC 2602.

REVIEWER COMMENT

Line 202 (binaries): Add RUWE citation(s). Nice approach to remove
radial_velocity_error based on each cluster's distribution. It should
be noted somewhere in this discussion that the target itself does
not meet the RUWE limit and so wouldn't appear on the plot by
default.

RESPONSE

> We've added a footnote to the Gaia DPAC memo by Lindegren on the
> RUWE.
>
> The stars shown in Figure 3 are those discussed in the first
> paragraph of Section 2.2.1. (The binarity cut has not yet been
> applied -- which is why the binary sequence is visible for the δ Lyr
> cluster). So for the latter point, we have not made any changes.

REVIEWER COMMENT

Line 209: I don't follow starting "assembled a piecewise grid of
hybrid isochrones..." What does "hybrid" mean here? Once the splines
for each cluster are defined, how does one interpolate between them?
Are the ages then determined for each star and combined?

RESPONSE

> The manuscript relies on the citation to Gagne+2020 for the full
> description of this method. The "hybrid" isochrone I_i is the
> linear combination of any two reference isochrones. The cited
> Equation 6 of Gagne+2020's mu-Tau paper gives the definition:
>
>   I_i = α_i * I_45 + (1-α_i)*I_112,
>

> for α_i a coefficient between 0 and 1, I_45 the reference 45 Myr
> isochrone spline (from Tuc-Hr, Columba, and Carina in Gagne's
> paper), and I_112 the Pleiades isochrone spline.
>
> In our case, given our different reference clusters, the definition
> is modified to
>
>   I_i = α_i * I_16 + (1-α_i)*I_38,   (age from 16-38 Myr)
>   I_i = α_i * I_38 + (1-α_i)*I_112, (age from 38-112 Myr)
>
> where I_38 is the IC 2602 isochrone spline, and I_16 is the UCL
> isochrone spline.
>
> The main assumption of this approach is that between the reference
> times set by the adopted clusters, the isochrones evolve linearly
> in time through color-absolute magnitude space. If we were to
> attempt this linear interpolation all the way from UCL to the
> Pleiades without putting any intermediate point, the validity of
> this assumption would be crucial to test. Since we include IC
> 2602, and it overlaps almost directly on the CAMD with the δ Lyr
> cluster, the only useful quantity that this method really gives us
> is a formal uncertainty on the age. (Since the age the method
> returns will be very close to whatever age we adopt for IC 2602).
>
> To better illustrate the method, we've attached a figure to the
> response, "age_interpolation_example.png". This shows the CAMD data
> from the clusters after all the cleaning steps, their reference
> splines (constructed from bins over a select color range), and the
> interpolated splines spaced every 4.4 Myr between UCL and IC2602,
> and every 14.8 Myr betwen IC2602 and Pleiades. While we do
> interpolate over a much finer grid, this is not shown to improve
> the clarity of the figure. The age of δ Lyr is determined by using
> all the stars in the displayed color range, through the referenced
> Equation 7 of Gange+2020, which is a Gaussian likelihood that
> treats the interpolated isochrone as the "model" and the δ Lyr
> cluster's isochrone as the "data". The statistical uncertainty on
> the age is then the standard deviation of a Gaussian fit to the
> posterior probability (in log-age; it becomes asymmetric when
> transformed to linear age). We've added a few sentences to the
> manuscript to clarify this latter point.

REVIEWER COMMENT

Line 230 (age): Consider adopting a systematic error (perhaps
corresponding to the range noted for the extremes of the ages) in
addition to the random (assumed to be the quoted +6/-5 error).
Relatedly, how are the quoted errors derived?

RESPONSE

> We're comfortable quoting the statistical error derived from the
> standard deviation of the logarithmic age posterior since this
> distribution is roughly Gaussian (see the attached
> del_lyr_cluster_age_posterior.png). The worry with quoting a
> systematic error, e.g., "The age of the δ Lyr cluster is
> 38^{+6}_{-5} (stat) $^{+6}_{-8}$ (sys)" is that the systematic
> uncertainties are not based on any probability distribution that
> we can easily assess -- they're estimated based on the extremes of
> previous literature age measurements. Quoting this systematic
> range next to a number that has been derived based on a more
> formal approach (and that has statistical meaning within that
> approach) somehow misses this point. We would prefer to leave the
> discussion in the manuscript as-is in order to communicate the
> spread of ages from different measurement approaches (which
> reflects a systematic uncertainty in the overall age scale),
> rather than quoting separate statistical and systematic
> uncertainties.

REVIEWER COMMENT

Line 235: May be worth mentioning the restriction to the 391 stars in
the beginning of sec 2.2.2.

RESPONSE

> Done, thank you.

REVIEWER COMMENT

Line 278: "Indistinguishable" -- from each other or from the Pleiades?

RESPONSE

> From each other, and the manuscript has been updated; thank you!

REVIEWER COMMENT

Line 286: I thought I remember Curtis or Douglas citing binarity as a
possibility for stars above to slow sequence as well, but I don't
have a reference on-hand.

RESPONSE

> Thank you for this point -- we've updated the text with a
> reference to Stauffer+16. In their Section 5.1, they discuss the
> case of some unresolved F+K binaries in the Pleiades which show
> anomalously long K2 rotation periods.

REVIEWER COMMENT

Section 3.2: The text mentions statistical and systematic
uncertainties but I only see one set of errors listed in Table 1.
Were these uncertainties combined or was only one adopted in the end?

RESPONSE

> They were combined in quadrature (see Line 331 of the original
> manuscript, "Reported uncertainties are a quadrature sum of the
> statistical and systematic components."

REVIEWER COMMENT

Line 391: It would be helpful to state the star's RUWE in the text
here.
Additionally, I believe RUWE acornym was already defined previously.

RESPONSE

> Done, thank you.

REVIEWER COMMENT

Section 3.2: Comment on whether the binary companion should effect
the rotation measurement of the host star (either here, or in the
rotation section).

RESPONSE

> We've added the following sentence at the beginning of Section 4.1,
> when the Kepler light curve is introduced
>
> """
> The resulting photometry is dominated by a quasi-periodic starspot
> signal with a peak-to-peak amplitude that varies between 2\% and
> 8\%. \added{Given that the secondary companion's brightness in the
> Kepler band is 1\% to 2\% that of the primary, source confusion for
> the rotation signal is not expected to be an issue.}
> """
>
> The rationale is that the rotation amplitude on the secondary would
> need to be a factor of >two times its intrinsic brightness for any
> confusion to happen, and the primary would need to not show any
> rotation signal. Such large variability amplitudes have not been
> observed except for disked stars (e.g., Rebull+2020, Figure 10),
> and there is no evidence for a secondary period in the Lomb Scargle
> periodograms.

REVIEWER COMMENT

One note about the transit analysis: The MuSCAT3 data was introduced as supporting evidence but the conclusion (that the transits appear achromatic) was never explicitly stated.

RESPONSE

> Thank you -- we have reworded this section to improve the clarity
> with which this point is made.

REVIEWER COMMENT

Line 725: The fast rotation period and flares could plausibly effect the completeness.

RESPONSE

> These completeness statistics were computed using KeplerPORTS (the
> Burke & Catanzarite 2021 citation). KeplerPORTS corrects for red
> noise in the light curves by using the CDPP over different
> timescales -- see Section 3.3 of Burke, C. J. & Catanzarite, J.
> 2017, Planet Detection Metrics: Per-Target Detection Contours for
> Data Release 25, KSCI-19111-002. This correction is performed
> using both a) the slope of the RMS CDPP versus transit duration for
> for "long" transit durations (the average of 7.5, 9.0, 10.5, 12.0,
> 12.5, and 15.0 hours) and b) the same slope for "short" transit
> durations (2.0, 2.5, 3.0, 3.5, and 4.5 hours). We retrieved these
> values from the DR25 Kepler occurrence rate products hosted on the
> NASA Exoplanet Archive. For Kepler 1627, they were CDPP_long =
> 0.25478 and CDPP_short=0.79505. Per Figure 2 of the KSCI-19111-002
> memo, these do indeed indicate a noisy star -- no surprises.
>
> We've added a sentence to the manuscript to clarify that we are
> talking about per-target detection contours, which should correct
> for the impact of at least the fast rotation period, inasmuch as it
> manifests in the slope of the CDPP versus transit duration. We
> have also modified the wording for clarity, and have added a
> citation to Burke & Catanzarite's 2017 KSCI-19111-002 memo.
>
> To determine whether this CDPP-based detection efficiency
> correction is accurate given the exact red-noise properties of
> Kepler-1627 (i.e., the rotation and flares), a full
> injection-recovery analysis of the light curve with a pipeline of
> our own would be necessary. This effort is outside our scope, for
> now.

REVIEWER COMMENT

Line 729: Considering Owen 2020, contraction could also contribute to some extent to the larger radii of small planets; might this be

significant?

RESPONSE

> Thank you for this point -- we've added a sentence in the
> discussion to mention this possibility.

REVIEWER COMMENT

Appendix A paragraph beginning at line 1158: May be worth mentioning that delta Lyr may also be part of one of these heirarchical associations.

RESPONSE

> Done, thank you.

REVIEWER COMMENT

Figure 13 caption: Specify planetary orbital period since technically there is also a stellar orbital period involved.

RESPONSE

> Done, thank you.

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Reviewer #2:

REVIEWER COMMENT

Abstract
line 24: Kepl-1627Ab cannot be a "mini-Neptune" if it is same size of Neptune. Replace with "Neptune-size" Yes, it falls within a distribution of objects in a period-radius diagram with an average that is smaller than Neptune, but claiming affiliation with smaller objects implies more than we know about this planet, e.g. its mass.

RESPONSE

> Point well-taken. R_p = 3.82 +/- 0.16 R_earth, so yes, Kepler-1627
> Ab is the same size as Neptune within 1-sigma. All instances in
> the text that referred to Kepler-1627 Ab as a mini-Neptune have
> been modified to call it a "Neptune-sized planet", including the
> manuscript's title.

REVIEWER COMMENT
----
line 25: Kepler target stars were chosen for their suitability for

transiting detection of planets in general, and for planets that are
Earth-analogs in paraticular. That involves radius, brightess.
Distance and spectral types were not direct selection criteria; this
was before Gaia.

RESPONSE

> Thank you for this point. Batalha+2010 (ApJL) discuss the
> selection function. The sentence in L25 currently states that
> "The star was observed by the Kepler space telescope because it is
> nearby (d=329pc) and it resembles the Sun". More directly, yes,
> the star was chosen because it was thought that it might enable
> the detection of small planets in the habitable zone. However the
> underlying reason for why the star enables such detections is
> based on the star's brightness and assumed radius, which both
> factor into the transit SNR (SNR ~ δ/σ ~ (Rplanet/Rstar)^2 *
> (1/σ(Kepmag)); for σ(Kepmag) the relative precision as a function
> of Kepler magnitude). Our claim therefore holds, albeit
> indirectly: a star of the same color that was too far away would
> not have been selected because it would be too faint. If it were
> the wrong color, it also wouldn't have been selected. But perhaps
> we are getting a bit too pedantic: this sentence also serves the
> more important purpose of informing the reader what kind of star
> this paper is about: a star that is at a distance of ~330 parsecs,
> and that resembles the Sun (except for its age!).

REVIEWER COMMENT
----
line 28: "main" --> "prime"

RESPONSE

> Done here and throughout the manuscript, thank you.

REVIEWER COMMENT

Intro

Line 47: (First paragraph on p 2) the time-scale of 1e8 is associated
with one particular set of photoevaporation models; core-powered mass
loss proposes timescales of Gyr(s) (see the Ginzburg paper that was
cited). More recent photoevaporation models also propose 1 Gyr
timescales. It would be good to revise/update this paragraph.

RESPONSE

> The sentence corresponds to a claim that mini-Neptunes are
> "expected to shrink in size by factors of several over their first
> 10^8 years".   The Owen 2020 models are then cited because this is
> their main claim: e.g., Figure 1 top panel of Owen 2020. The range

> in initial raddi from that study come from differences in
> assumptions about whether or not the boil-off phase (Owen & Wu
> 2016) happens, and differences in assumption about the envelope to
> core mass ratio.
>
> Nonetheless, the point is well-taken. We've added a sentence that
> clarifies that the outflows are expected to persist for 1e8 to 1e9
> years, and have added citations to Gupta & Schlichting 2020, Rogers
> & Owen 2021, and King & Wheatley 2021.

REVIEWER COMMENT

Section 2

Line 65: it is convention to italicize names of space missions like
TESS and K2

RESPONSE

> Yes, and we leave the imposition of this convention to the
> copy-editors at the journal.

REVIEWER COMMENT

Lines 142-144: perhaps rephrasing this would clarify that the authors
did not use Kepler-1627 as a fiducial star about which to select
cluster members, and that they restricted the analysis to one of
several sub-clusters or clumps which contained Kepler-1627.

RESPONSE

> We have rephrased Lines 142-144 to clarify the meaning, which is
> indeed what the reviewer has stated.

REVIEWER COMMENT

Figure 1: the first three panels would be more effective if they were
plotted at the same scale and with appropriately aligned axes. As
such the upper right panels are too small to be useful. Consider
leaving out the Sun (which is irrelevant) so as to have a more
appropriate choice of scale to how the details of the cluster. The
purpose of the fourth panel (tangential velocities) and the second
line in the caption are unclear to me. Aren't the tangential
velocities known from Gaia proper motions and parallaxes? Why
calculate them with the assumption of common space motion? If that is
done, what does it show, anyway?

RESPONSE

> Thank you for these important points of feedback.

>
> The Sun has been included to clarify the direction along which
> parallax uncertainties are expected to produce erroneous
> clusterings (analogous to the "Fingers of God" effect in the
> extragalactic literature). This is relevant because the HDBScan
> clustering method used by Kounkel & Covey in their 2019 paper
> likely was susceptible to this systematic effect. A sentence has
> been added to the caption of Figure 1 to clarify this point.
>
> The {X,Y}, {Z,X}, and {Z,Y} panels are plotted at different scales
> in order to keep the aspect ratios roughly correct. The reported
> KC2019 cluster members span $\{\Delta X, \Delta Y, \Delta Z\}$ of roughly {400, 600, 200}
> parsecs, which grows to ~{600, 600, 200} parsecs when including
> the Sun. The {Z,X} and {Z,Y} panels should therefore roughly have
> aspect ratios of 1:3 in order for the parsec to not be stretched
> too much in either dimension. The top-down {X,Y} view is the most
> informative dimension, which is why it is enlarged related to the
> edge-on views (and its 1:1 aspect ratio is set by the same
> consideration).
>
> Finally, the tangential velocities are important because they tell
> us whether the structures are kinematically coherent. Open
> clusters typically have velocity dispersions of order ~1km/s.
> Meingast et al. 2021 (A&A 645 84) for instance suggest tangential
> velocities <2 km/s (from the mean cluster velocity) as a plausible
> cutoff for selecting candidate cluster members. One needs to
> compute these as relative tangential velocities instead of just
> looking at the proper motions because the proper motions are
> changed by line-of-sight projection effects, especially when the
> clusters appear stretched over tens of degrees on-sky. See for
> instance Figure 2 of Meingast+2021, or Figure 14 of Bouma+2021 (AJ,
> 162, 197). In our case, the tangential velocities indicate that
> the spread in Kounkel & Covey 2019's reported cluster members is
> ~10km/s, which is large enough that it does not make obvious
> kinematic sense to consider all of these stars to be part of the
> same structure.
>
> We've added a few sentences in the manuscript to clarify these
> points.

REVIEWER COMMENT

Figure 3 bottom: Can the IC 2602 and 2391 rotation periods be added
to this plot? Lines 276-278 mention that these and cite Douglas et
al. 2021 (although the reference does not seem to be complete). Isn't
it interesting that the interlopers all fall on the Praesepe rotation
sequence?

RESPONSE

> Thank you for pointing out the missing link in the reference to
> Douglas et al. 2021, which should have pointed to
> https://ui.adsabs.harvard.edu/abs/2021tsc2.confE.157D/abstract,
> and has been updated in the revision. The rotation periods from
> this work, which includes IC 2602, IC 2391, and a few other 30-50
> Myr clusters, are not yet public.
>
> The interlopers falling on the Praesepe sequence is indeed a bit
> odd, but it's really only ~3-5 stars. A much bigger set of
> interlopers didn't have detected rotation periods at all! (See
> final paragraph of Section 2.2.2). At the other reviewer's
> request, we've also added a sentence noting that some of these
> stars might be unresolved F+K binaries.

REVIEWER COMMENT
----
p 5 lines 258-261: rather than simply throw the TESS values out in
favor of the Kepler ones, the authors should compare them to evaluate
the accuracy and reliability of the TESS values.

RESPONSE

> Thank you for this point -- please see kepler_vs_tess_prot.png in
> the attached response, which shows the 28 referenced stars that
> overlap. 5 of the 28 stars have Kepler rotation periods above 10
> days, of which none were correctly recovered by TESS (3 are chosen
> at the half-period harmonic). Of the 23 stars with periods below
> 10 days, 21 of the TESS periods agree with the Kepler rotation
> periods, and 2 are at the double-period harmonic.
>
> We've updated the text to reflect this point, which doesn't affect
> any of the conclusions given that almost all the δ Lyr cluster
> stars have rotation periods below below 10 days.

REVIEWER COMMENT

Section 3

page 7 line 391: report the RUWE value (yes its in the Table but
don't make the reader work)

RESPONSE

> Done, thank you.

REVIEWER COMMENT

Section 4.1.3 and Figure 6

The significance of this slope is unclear and a sigma is not a good
way to evaluate it. Perform a ratio of chi-squared or F-test to
evaluate it vs. a flat line.

RESPONSE

> The second paragraph of Section 4.1.3 has been modified to mention
> the difference in BIC, and the corresponding approximate Bayes
> factor.
>
> The slope of $-21 \pm 10\,{\rm min\,day}^{-1}$ is similar to that
> found by \citet{holczer_time_2015}. \added{The χ^2 of the
> best-fit line is 306.1, with $n=140$ data points. An alternative
> model of a flat line yields $chi^2=315.6$. The difference in the
> Bayesian information criterion (BIC) between the two models is
> ${\rm BIC}_{\rm flat} - {\rm BIC}_{\rm line} = 4.5$, which
> corresponds to a Bayes factor of $\approx$9.4. According to the
> usual \citet{kass_bayes_1995} criteria, this is ``positive''
> evidence for the model with a finite slope. We view it as
> suggestive evidence at best, particularly given the excess scatter
> in the transit timing measurement uncertainties.}
>
> We have also modified the caption of Figure 6 to clarify that this
> is not a detection.

REVIEWER COMMENT

Lines 530-542: does option (ii) include a transit fit as well? I
assume it does but it would be good to clarify. And is option (i) the
2nd and 4th order polynomial? Perhaps just state this?

RESPONSE

> Yes, option (ii) includes the transit -- we've updated this in the
> text for clarity.
>
> Yes, option (i) is the 2nd or 4th order polynomial. We've updated
> the text to state this more directly as well. Thank you!

REVIEWER COMMENT

Line 561-562: do the authors mean "lack of statistical significance"?

RESPONSE

> Yes -- we've added this to the text. Thank you.

REVIEWER COMMENT

Section 4.2

Analysis of the motion of the photocentroid has been widely used to help validate Kepler planets. Can this be used here to distinguish between the possible A and B host stars?

RESPONSE

> Kepler-1627B is ~0.16 arcseconds East of Kepler-1627A. For the
> sake of argument, assume it contributes 2% of the flux to the
> aperture (an overestimate), and neglect other stars in the scene.
>
> Taking the origin at Kepler-1627A, the center of light outside of
> transit is then roughly (0.98*0 arcsec + 0.02*0.16 arcsec) = 0.0032
> arcsec due East of the primary.
>
> During transit, if the transit happens on the primary, the center
> of light is roughly ((0.98-0.002)*0 arcsec + (1-(0.98-0.002))*0.16
> arcsec) = 0.0035 arcsec due East of the primary -- i.e., it is
> closer to the secondary by 0.3 milliarcseconds, since the primary
> is getting fainter.
>
> If the transit instead happened on the secondary, the center of
> light would be roughly (0.98*0 arcsec + (0.9*0.02)*0.16 arcsec) ~=
> 0.00288 arcsec due East of the primary, where a 10% intrinsic
> eclipse depth on the secondary is required to fit the observed
> transit depth. In this scenario, the centroid would shift closer
> to the *primary* by 0.3 milliarcseconds.
>
> Examining the NASA SPOC Data Validation Report (DVR), there are two
> available photocentroid tests, the first based on the flux-weighted
> centroid analysis, and the second based on the difference image
> centroid offset test. The latter test yields offset uncertainties
> of ~100 milliarcsec for Kepler-1627, which is far above the
> relevant signal. The former test yields centroid peak RA and DEC
> offsets consistent with zero, with uncertainties of 0.9 and 1.1
> milliarcsec respectively (page 19 of 552 in the DR25 DVR). So,
> given that the quoted centroid precision uncertainties are a factor
> of ~3 larger than the expected shift, it is not possible to use the
> test in a way that distinguishes the two host stars in a
> statistically convincing way.

REVIEWER COMMENT

Line 604-608: The statement that the density from the transit duration is inconsistent with that expected for the M dwarf "B" cites Table 2 and Choi+2016, but that Table does not appear to provide any information on "B", and the Choi+2016 reference is just for the Mesa models. A density of 2 g/cm3 (or lower) is possible for an M dwarf on the pre-main sequence, depending on the mass, age and model used.

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RESPONSE
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> Thank you for catching this -- this citation is indeed deprecated
> and erroneously was not updated during the writing process.  The
> final adopted calculation was based on the stellar masses and radii
> from the Baraffe+2015 models, rather than the MESA models
> (Choi+2016).  These models are available from
> http://perso.ens-lyon.fr/isabelle.baraffe/BHAC15dir/.  We used
> their 2MASS synthetic photometry to estimate the companion's mass,
> as follows.
>
>     m_K = 11.194 (2MASS)
>     m_K,A = 11.31
>     m_K,B = 13.68 (NIRC2)
>     m-M = 5*log10(329.5) - 5 = 7.59
>     -> M_K,A = 3.72
>     -> M_K,B = 6.09
>
> Assuming an age of 40 Myr, this yields M_comp = 0.31 Msun, by
> interpolating between the Baraffe 0.3 and 0.4 Msun values (M_K =
> 6.12 and 5.71, respectively).  Assuming an age of 30 Myr yields
> M_comp = 0.27 Msun (M_K=6.48, and 5.90 for 0.20 and 0.30 Msun).
> The bilinear interpolation at 38 Myr (our nominal age) yields
> M_comp=0.30 Msun, Teff=3408K, R=0.451 Rsun, and log(g)=4.60.
> In our original manuscript, we quoted a companion mass of 0.33
> Msun; this has been corrected in the revision.
>
> Given our age uncertainies (38 +/- 6 Myr), a plausible mass range
> based on these models for the companion is roughly 0.27 Msun to
> 0.33 Msun.  The model densities over the relevant ages and masses
> are
>
>        age    mass        radius      density
>   32 Myr 0.300 Msun, 0.481 Rsun, 3.80 g / cm3
>   40 Myr 0.300 Msun, 0.443 Rsun, 4.86 g / cm3
>   32 Myr 0.330 Msun, 0.506 Rsun, 3.60 g / cm3
>   40 Myr 0.330 Msun, 0.467 Rsun, 4.58 g / cm3
>   32 Myr 0.350 Msun, 0.518 Rsun, 3.55 g / cm3
>   40 Myr 0.350 Msun, 0.479 Rsun, 4.50 g / cm3
>   38 Myr 0.300 Msun, 0.451 Rsun, 4.61 g / cm3
>
> This means that for the argument concerning the stellar density
> inferred from the transit, the worst-case scenario would be that of
> a) the companion being more massive than expected  (~0.35 Msun),
> simultaneously with b) the system being younger than expected (~32
> Myr).  In this scenario, the companion star's density would be ~=
> 3.5 g/cc.  We've updated the manuscript to reflect this point.
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REVIEWER COMMENT

Lines 613-614: it would be good to quantify this, i.e. how faint an
unresolved companion or background star must be based on the star's
position in the CMD.

RESPONSE

> Visually, one can see that the width of the single-star sequence of
> δ Lyr candidates (black points) in the upper-right panel of Figure
> 3 is <~0.2 mag. So, if we saw a star with the same (BP-RP) color
> as Kepler 1627 that were more than ~0.3 mag brighter in M_G, we
> would classify it as a likely photometric binary. This means we
> are able to exclude stars ~0.32* as bright as the primary (in Gaia
> G-band). This corresponds to excluding bound companions more
> massive than ~0.85 Msun (the primary's mass is ~0.95 Msun). The
> SB2 tests do much better if the velocity separation exceeds the CCF
> width (down to 1% as bright as the primary;
> https://ui.adsabs.harvard.edu/abs/2015AJ....149...18K/abstract).
> For brevity, we would prefer to not present this information in the
> text.

REVIEWER COMMENT

Lines 635-638: Here, new observations appear for the first time but
are not described. While the details are given in the appendix, and
the outcome of the observations doesn't define this work, the authors
need to give some basic description of the observations, e.g.
passbands, SNR, resolution, observation times, etc. The authors
should not assume the reader actually know what HIRES and MuSCAT are!

RESPONSE

> Thank you for this suggestion. The main text now reads
>
> "We observed a transit of Kepler-1627 Ab on the night of 2021 Aug 7
> spectroscopically with HIRES at the Keck-I telescope and
> photometrically in $griz$ bands with MuSCAT3 at Haleakal\=a
> Observatory. Details of the observation sequence are discussed in
> Appendix C; Figure 7 shows the results."
>
> The first paragraph of Appendix C has been significantly modified
> to include the requested additional information, including the
> passbands, exposure times, S/N, and instrument resolution.
> Regarding observation times, "the night of 2021 Aug 7" is
> sufficiently precise for the purposes of the text; Tables 5 and 6
> have the BJD-TDB timestamps for those who desire greater precision.

REVIEWER COMMENT

Section 5

line 670: assuming that SNR is not to blame, the problem is not precision, the problem is either measuremnt systematics or astrophysical noise.

RESPONSE

> Thank you for this point; we've updated the text to say
> "signal-to-noise" rather than "precision".

REVIEWER COMMENT
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line 678-683: The Ca II HK activity indicator "S" as well as the H-alpha equivalent width could be obtained from the HIRES spectrum. Have the authors checked fora X-ray or UV (Galex) detection of this star? The star is likely not detected but its worth a look. In the same vein, the authors may wish to comment on whether the star's infrared emission indicates or rule out a substantial debris disk -- the star is detected in WISE (and perhaps Spitzer?)

RESPONSE

> Regarding the S-values, they are shown in the second panel of
> Figure 7, and are also reported in Table 6. The line profile
> changes shown in Figure 12 suggested that the majority of the RV
> variation observed was due to starspot-induced variations.
> Quantitative measurements of the H-alpha EWs are not expected to
> change this picture, and so we opt to not include them.
>
> Regarding the NIR, UV, and X-ray detections, we searched Vizier
> within a radius of 4 arcseconds.
>
> For the X-ray (i.e., ROSAT), there were no reported detections in
> Vizier.
>
> For the NIR and the possibility of a debris disk, Kepler-1627 is
> detected in W1, W2, W3, and W4 (see
> kepler1627_vizier_photometry.png attached). While W4 might be in
> excess of the usual Rayleigh-Jeans tail, and this could be
> quantified by fitting synthetic stellar atmospheres, this type of
> analysis is best done at the population level, particularly given
> the calibration uncertainties that are present in W4 (e.g., Cotten
> & Song 2016, ApJS 225 15). Since Kepler-1627 has not been
> identified as even a candidate debris disk host in the previous
> literature that performs these population-level infrared excess
> analyses, we opt to not speculate on this topic.
>

> For the UV, Olmedo+2015 reported a coincident source with NUVmag of
> 19.052 +/- 0.017 [AB mags], with a NUV S/N of 65 (2015, ApJ, 813,
> 100), through the GALEX-CAUSE Kepler survey. UV detections can
> sometimes be used as a youth indicator (e.g., Figure 9 of
> Newton+2021 AJ 161 65, which Newton et al attribute from the idea
> in Shkolnik+2011, ApJ 727 6). For Kepler-1627, we have 2MASS J =
> 11.690+/-0.021, and therefore m_NUV-m_J = 7.36. Given the Gaia
> (BP-RP)0 of 0.977, comparing to Figure 9 of Newton+2021 we find
> that Kepler-1627 overlaps with late-G type stars in Psc-Eri, the
> Pleiades, and the Hyades. In other words, the UV flux is not a
> particularly strong diagnostic of the age for Kepler-1627 as an
> individual star.
>
> To substantiate the latter claim, we cross-matched our lists of
> IC2602, Pleaides, and δ Lyr cluster members against 2MASS and
> GALEX. The results are in galex_jmk.png and galex_bpmrp.png,
> attached. IC2602 only got a few matches because it was in a region
> of sky not observed by GALEX. We see that the earliest A and F
> stars in the del Lyr cluster (38 Myr) are brighter in the UV than
> the Pleiades sequence (112 Myr). However, there's not really any
> new information in this statement that goes beyond the work that's
> already been done in the manuscript using Gaia isochrones and the
> stellar rotation. So, for brevity, we would prefer to omit
> this analysis from the manuscript -- but thank you for suggesting
> it!

REVIEWER COMMENT
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lines 699-701 It could be mentioned here that the planet orbital
period and stellar rotational period are not a rational combination.

RESPONSE

> Done, thank you.

REVIEWER COMMENT
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line 723-726: The caveat that should be mentioned here is that this
is for older, main sequence stars.

RESPONSE

> The first reviewer had a similar comment -- our response is the
> same:
>
> These completeness statistics were computed using KeplerPORTS (the
> Burke & Catanzarite 2021 citation). KeplerPORTS corrects for red
> noise in the light curves by using the CDPP over different
> timescales -- see Section 3.3 of Burke, C. J. & Catanzarite, J.

> 2017, Planet Detection Metrics: Per-Target Detection Contours for
> Data Release 25, KSCI-19111-002. This correction is performed
> using both a) the slope of the RMS CDPP versus transit duration for
> for "long" transit durations (the average of 7.5, 9.0, 10.5, 12.0,
> 12.5, and 15.0 hours) and b) the same slope for "short" transit
> durations (2.0, 2.5, 3.0, 3.5, and 4.5 hours). We retrieved these
> values from the DR25 Kepler occurrence rate products hosted on the
> NASA Exoplanet Archive. For Kepler 1627, they were CDPP_long =
> 0.25478 and CDPP_short=0.79505. Per Figure 2 of the KSCI-19111-002
> memo, these do indeed indicate a noisy star -- no surprises.
>
> We've added a sentence to the manuscript to clarify that we are
> talking about per-target detection contours, which should correct
> for the impact of at least the fast rotation period, inasmuch as it
> manifests in the slope of the CDPP versus transit duration. We
> have also modified the wording for clarity, and have added a
> citation to Burke & Catanzarite's 2017 KSCI-19111-002 memo.
>
> To determine whether this CDPP-slope detection efficiency
> correction is *accurate* given the exact red-noise properties of
> Kepler-1627 (i.e., the rotation and flares), a full
> injection-recovery analysis of the light curve with a pipeline of
> our own would be necessary. This effort is outside our scope, for
> now.

****

Other minor changes made to the manuscript include:

* Updated binning interval on lower panels of Figure 5 from 20 to 15 minutes.

* L.G.B.'s affiliation order was reversed, to reflect the primary institution at which this work was performed.