

Dear Dr. Jackson,

We thank you for organizing the review of our paper, and also thank the referee for their helpful comments. 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

REFeree COMMENT

Section 2.1, Paragraph 2.

It would be helpful if the authors briefly describe the exact meaning of "high quality" in the context of the transit times used in the analysis.

RESPONSE

> Thank you for this prompt. We have removed the phrase "high
> quality", as its precise definition is not of particular
> importance for this study. It is defined in Section 3.4 of
> Baluev+ 2019, through an empirical cutoff that Baluev+ made
> based on the sampling cadence and the light-curve residual
> RMS.

REFeree COMMENT

Section 3.1

In this section it is stated that the assumption of "a Gaussian likelihood and sampling over the posterior probability distributions" is made. This assumption needs further justification given the variety of data sources included. In particular, it is later stated in Section 3.1 that the cause of the high reduced χ^2 values "must be some additional signal or noise". Alternatively, it could indeed mean that the Gaussian likelihood assumption is not completely valid.

RESPONSE

> We expect that the high reduced chi2 values are likely due to
> either underestimated statistical uncertainties on transit
> timing measurements, or that they are caused by systematic
> errors. We do not think that the specific choice of a
> Gaussian likelihood vs. an alternate choice of likelihood is
> necessarily at issue.
>
> We made every effort to ensure the integrity of our included
> times, for instance by rejecting times that did not clearly
> specify the nature of the barycentric/heliocentric
> correction, or the nature of the leap-second correction (UTC
> vs TDB). However we ultimately need to trust timestamps that
> were recorded by observers other than ourselves. Despite
> these efforts, some observations might have systematic errors
> (e.g., of order 1 minute for leap-second corrections), but we
> cannot be sure which ones.
>
> Our approach for dealing with this issue was to introduce a
> free parameter that uniformly inflates the uncertainties of
> derived parameters (e.g., dP/dt) by the factor that would
> ensure reduced chi2 = 1. Any suggestions on better
> methodologies are welcome, however it is not clear to us that
> there is any way around the fundamental issue of needing to
> trust that observers can record time correctly.
>
> We have updated the wording in the manuscript to improve the
> clarity with which we present these issues.

REFeree COMMENT

Section 3.3

The discussion and figure in this section are very good. However, it is not sufficiently demonstrated that a stellar companion is ruled out when considering the possible angular separations of the companion resulting from being near inferior or superior conjunction. This is particularly important for this system since even a small degree of coplanarity of the companion with WASP-4b would result in an edge on orbit that makes the companion difficult to detect when near conjunction locations. Also, the very last sentence of this section "Figure 4 shows the result." should describe the results of the figure in more detail.

RESPONSE

> We agree; we erroneously neglected this issue in the
> submitted manuscript. Thank you for bringing it to our
> attention.

>
> In the revised manuscript, we amended our methodology to
> probabilistically include the speckle imaging limits. The
> procedure is discussed in the text, but in brief, for each
> simulated companion (which has known orbital parameters), we
> calculated its projected separation on-sky at the time of the
> imaging observation. We then multiply the RV and speckle
> imaging posteriors, yielding contours in a revised Figure 4.
>
> This revision led to slightly weaker upper limits on the
> companion mass (200M_{Jup} → 300M_{Jup}), but did not otherwise
> affect the primary conclusions of the study.
>
> The comment regarding the last sentence has also been
> addressed; thank you.

REFeree COMMENT

Section 4.1

Paragraph 1. The paragraph implies that the Doppler effect is the sole cause of the timing derivative. However, and given the 2 sigma discrepancy, the wording should allow for the possibility that the timing derivative is actually a result of a combination of the described effects.

RESPONSE

> We have modified the wording in this paragraph to clarify
> that this is a possibility, but that at present the dominant
> physical effect is the line-of-sight acceleration.

REFeree COMMENT

Section 4.2

Paragraph 3. "hot Jupiters is expected" should be "hot Jupiters are expected".

RESPONSE

> Corrected, thank you.