Peer Review File

Manuscript Title: A close-in giant planet escapes engulfment by its star

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referee #1 (Remarks to the Author):

This paper presents asteroseismic modelling of the planet hosting red giant star 8 Ursae Minoris, convincingly showing that the star is core helium burning, with the implication being that the planet could not have survived the previous evolution of the star in its current orbit. This may have important implications for the study of planetary systems around white dwarfs, since it shows that close in planets can survive the RGB evolution of their host star and potentially go on to orbit the white dwarf remnant and perhaps even perturb lower mass objects into orbits close to the white dwarf, leading to metal pollution.

This is a particularly active and exciting area of research at present and this object certainly contributes to this. One concern I have is that this system appears to have quite a complex evolutionary history if the binary pathway is to be favoured, which I would argue is probably the more likely scenario (as opposed to second generation formation). As such, it may not be representative of the progenitors of most planet hosting or metal polluted white dwarfs and so it's unclear exactly what the implications of this result are in a larger context (e.g. are the authors claiming that the known short period planets around white dwarfs are also the result of stellar mergers? I don't think so). Nevertheless, I believe the result is compelling enough to warrant publication in Nature.

There is appropriate use of statistics and treatment of uncertainties throughout, including figure captions.

Overall the paper is well written and easy to follow. I have no major issues with it but do have some points that the authors should address:

1) Does the star not expand further on the AGB (particularly during thermal pulses) than on the RGB and is the planet likely to survive this evolutionary phase as well? Some discussion of the expected future evolution of the system would be appreciated, in particular is this system likely to end up as a planet in orbit around a white dwarf? In the simulated binary merger scenario what is the expected mass of the final remnant in the fiducial model? Is it the same as the final core mass shown in extended data figure 9 (which would be very low for a white dwarf) or would you expect the core to continue growing on the AGB?

2) The argument regarding the short amount of time available for the planet to have tidally circularized since the host star reached its maximum size would be made far stronger by quoting an estimate of what this timescale actually is, or at least an estimate of the order of magnitude.

3) The binary merger scenario outlined in this paper appears to initially create a so-called EL CVn binary (see Maxed et al. 2014, MNRAS, 437, 1681). Recent results have shown that these kinds of binaries are usually (perhaps always) the inner binaries of hierarchical triples (e.g. Lagos et al. 2020, MNRAS, 499L, 121). Could the outer companion be stellar in nature? Even if this isn't the case, the strong need for a stellar companion in this scenario (96 per cent of very close main-sequence binaries are found to be hierarchical triples - Tokovinin et al. 2006, A&A, 450, 681) means that this is worth mentioning and the possible effects this may have on the evolution of the planetary system.

4) Is it possible to constrain the rotation rate of the star/core from the asteroseismology? The merger scenario would imply that the core should be quite rapidly rotating, indeed the entire star is spun up during the stable mass transfer phase, so one would expect this star to be more rapidly rotating than similar stars that have evolved in isolation.

5) Binary evolution plots are typically shown in terms of orbital period, obviously in this case plots with separation in au are useful to compare to the planetary orbit so I am not suggesting to change this, but perhaps adding orbital period to the right-hand axis of the top panel in extended data figure 8 and the bottom panel of extended data figure 9 would aid in comparing this system to known populations of binaries.

6) In the fiducial binary merger model what is the change in the planetary semi-major axis after the first mass transfer event, i.e. what was the original value (beta=0.6 implies it is not fully conservative, so the mass loss would have affected the planet's orbit)? How does this compare to known circumbinary planets?

7) In extended data figure 3 the purple and blue lines are a little difficult to distinguish for those with color blindness. I suggest the use of a different color (the purple/green combination works well in other figures).

Referee #2 (Remarks to the Author):

Dear Editor,

I have read and reviewed the manuscript called "A close-in Jovian planet orbiting a helium-burning red giant star" by Hon and collaborators.

The study reports a new analysis of the structure of a planet's host star, 8 Ursa Minoris. Based on recent TESS photometry, the authors were able to use asteroseismology techniques to constrain the internal structure and evolution state of the star, which is claimed to be a core-helium burning giant.

The analysis reported in the manuscript seems adequate and convincingly demonstrates that the star is indeed a core-helium burning giant. The estimation of the uncertainties on the various planetary and stellar parameters also seems thorough.

The crux of the matter is that the actual location of the planet is below the radius that the star is expected to have reached during its ascent on the red giant branch, which raises the question of how the planet managed to survive up to that stage.

On that point, I have several comments:

- on Figure 3: since the right panel is a depiction of the boundary on Rtrgb as a function of stellar mass and metallicity, I am not sure what the two curves mean for each model or how to understand the following sentence in the caption: "Each code's boundary is drawn with two lines that indicate the boundary's uncertainty, which is propagated from the uncertainty of 8 UMi's mass and metallicity". Could the author please clarify this point?

- beginning of page 3: The authors briefly mention how unlikely the survival to a common envelope evolution would be. Considering that many of the following arguments rely on it, I think it would be helpful to remind to the non-expert reader how much time is expected to have elapsed since the star reached the tip of the red giant branch (including the measured uncertainties on the stellar parameters). In the same vein, it would be nice to actually provide an estimate of the survival time of the planet in a common envelope scenario to help the comparison.

- 2nd paragraph of page 3: I agree with the authors that it would require extremely fine tuned parameters to explain the current system with a tidal migration that would have halted just at the right location. However, for the dynamical scattering followed by tidal circularization scenario, it would be nice to actually quote plausible timescales and to compare them with the age of the system since the tip of the RGB (see previous comment).

Then the authors try to explain the existence of the system using a peculiar scenario involving the merger of an initial binary star.

Although it seems that this scenario can meet the various constraints provided by the observation of the system, I think it is difficult to get a feel for how (un)likely or finely tuned the initial conditions on the binary need to be. In particular when the tidal scenario is rejected on such a kind of principle. It would be really helpful if the authors could roughly estimate the fraction of binary stars that would fall in a relatively close area of the parameter space.

Finally, this system is presented as a missing link between main-sequence planet-hosting stars and planet-hosting white dwarfs. But, there is no discussion of how such a system could actually evolve (or not) into the kind of planetary system observed around white dwarfs. I think that a discussion should be added about that. Such a discussion would be crucial to help the reader place this discovery in the big picture.

With all that in mind, I think that after the various clarifications proposed here are made, the manuscript will meet the standard for publication.

other comments:

- Please give more details in the caption to extended figure 7. At the moment it is difficult to understand what the curves actually mean, which curve sets upper or lower limits and what the shaded region means.

Referee #3 (Remarks to the Author):

Hon et al. report the detection of asteroseismic oscillations in the planet-hosting star 8 UMi that indicate it is a helium-burning red giant. This is surprising because the star evidently hosts a giant planet, detected by radial velocities, that would have been engulfed by the star before its current phase if standard single-star evolution had proceeded as usual. The authors argue that this implies the host star is the product of mass transfer and mergers of an inner binary system, demonstrating a new evolution pathway for post-main-sequence planetary systems.

This is an extremely interesting result, and the authors have done a superb job of presenting a plausible and well-supported explanation. I will be excited to see the community's reaction and whether other explanations emerge. I believe this will be a high-impact discovery and should be published in Nature.

Before publication, however, I do have some comments that I would like to see addressed. I have four moderate/major points, and a number of less important suggestions.

Moderate/major:

1. The authors use very strong language to describe the detection of the planet orbiting 8 UMi, saying things like "confirming the planetary nature of the radial velocity variations" and "The long term stability of 8 UMi’s 93-day radial velocity variations confirms an orbiting companion rather than stellar activity". I am okay with the authors using the word "confirmed" to describe the radial velocity detection itself, since they have now provided an independent dataset to the discovery data that shows the same signal. However, I do not think using the word "confirmed" is appropriate for the planetary interpretation. Usually in the literature, "confirming" a planet means detecting it with an independent detection method (like transits + RVs, or RVs + astrometry), which has not been done here yet. This is particularly important because the inclination of the planet's orbit has not been constrained, so the companion's mass may be significantly larger than the minimum mass found in the RV analysis. I recommend the authors replace the word "confirmed" in these cases with "strongly suggests" or "supporting" to more accurately reflect these uncertainties.

2. The authors leave the true mass of the orbiting companion unconstrained, as is often the case for radial velocity planet detections. I agree that this is most likely a planetary-mass companion, but as has recently been demonstrated by Gaia observations of another famous RV detection published in Nature (https://ui.adsabs.harvard.edu/abs/1989Natur.339...38L/abstract), sometimes promising planet signals can instead be caused by binary stars with nearly face-on orbits(https://ui.adsabs.harvard.edu/abs/2022AJ....164..196W/abstract). I recommend that the

authors attempt to constrain the true mass of the 93 day companion using constraints from either Hipparcos astrometric measurements, or the residuals of Gaia DR3 astrometry (which appear to have no strong excess astrometric noise, consistent with a planetary interpretation). A quantitative exploration would strengthen the paper.

3. Along the lines of my previous point, I would like to see some discussion of how a significantly larger companion mass would change the formation scenarios discussed in the paper. How massive must the 93 day companion be for common envelope evolution to be a viable formation pathway? Is this consistent with the constraints from Hipparcos and Gaia astrometry?

4. I would like to see a more fleshed-out step-by-step description of the binary evolution scenario in the letter itself on Page 3. I think this is the most interesting take-away from this work, and it should not be buried in the Methods/Supplementary Information!

Minor points:

5. Page 2: "a minimum distance of 5 AU (67% confidence, see Supplementary Information for further details)." -- does the 67% confidence refer to the semimajor axis, or the confidence level in the detection of the trend? I see from reading the supplementary information that it is likely the semimajor axis, but it might help to clarify this here.

6. Page 3: "Survival of the planet in a common envelope evolution " - remove the word "evolution" here.

7. Page 3: "Tentative evidence of an outer companion from 8 UMi's radial velocity data may suggest scattering processes9, 29 favouring this scenario." -- It would be helpful to readers to quantify "tentative" here.

8. Page 3: Since the Li abundance is an important piece of evidence, I recommend the authors try to quantify their confidence that the Li abundance is anomalously large. I recommend looking at Figure 1 of Soares-Furtado et al 2021 (https://ui.adsabs.harvard.edu/abs/2021AJ....162..273S/abstract), who tabulate the mean and scatter lithium abundance as a function of HR diagram position. Reading off their plot, I estimate roughly 2-3 sigma confidence that UMi's Li abundance is higher than typical stars.

9. Methods Page 11: "This firmly excludes the presence of a $\sim$ 5G mean longitudinal magnetic field strength expected for an active red giant rotating at a period of 90 days15." -- how much scatter is in the empirical relations used here to connect a 90 day rotation period to a 5 G magnetic field strength?

10. Supplementary Information Page 14: "The improvement in WAIC in using the trended model surpasses the standard error in WAIC differences between the two models (dWAIC = 3.84), and is thus significant" - I am not particularly familiar with the WAIC for model comparison. Could the authors please also give some other metrics for assessing the confidence of the detection, like the Bayesian Information Criterion, or a frequentist confidence level?

Author Rebuttals to Initial Comments:

Responses to Referees

We thank the referees for their constructive feedback on our manuscript. The following are our responses to comments from the referees, which are in **bold.**

Referee #1:

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1) This is a particularly active and exciting area of research at present and this object certainly contributes to this. One concern I have is that this system appears to have quite a complex evolutionary history if the binary pathway is to be favoured, which I would argue is probably the more likely scenario (as opposed to second generation formation). As such, it may not be representative of the progenitors of most planet hosting or metal polluted white dwarfs and so it's unclear exactly what the implications of this result are in a larger context (e.g. are the authors claiming that the known short period planets around white dwarfs are also the result of stellar mergers? I don't think so).

Our response:

In a larger context, our result suggests a reassessment of the survivability of post-main sequence exoplanetary systems. Given the ubiquity of stellar multiplicity and its demonstrated role in preserving close-in planets around giants, there may be a non-negligible occurrence rate of short period planets surviving up to stages as late as the asymptotic giant branch phase. This, in turn, will influence our understanding of exoplanet demographics around evolved systems, which have been mostly limited to outer (distant) planetary systems. This implication is separate from that of the system being progenitors of known white dwarf systems, whose connection we have not explored in detail (see next response). To maintain the clarity of our paper, we have removed text discussing a link to white dwarf systems.

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2) Does the star not expand further on the AGB (particularly during thermal pulses) than on the RGB and is the planet likely to survive this evolutionary phase as well? Some discussion of the expected future evolution of the system would be appreciated, in particular is this system likely to end up as a planet in orbit around a white dwarf? In the simulated binary merger scenario what is the expected mass of the final remnant in the

fiducial model? Is it the same as the final core mass shown in extended data figure 9 (which would be very low for a white dwarf) or would you expect the core to continue growing on the AGB?

Our response:

After the merger, it is expected that the resulting core helium-burning star would evolve similarly to an ordinary 1.5 $M_{\odot}$ clump star, which mirrors observations of 8 UMi. Following the evolution of a typical clump star, stellar evolution models predict that the star will ascend the asymptotic giant branch. The planet is not expected to escape engulfment during the thermal pulse phase since the star will grow to distances beyond 1 au. Under our interpretation that the merger remnant evolves similarly to a 1.5 $M_{\odot}$ red clump star, the star will eventually form a carbon-oxygen white dwarf of approximately 0.6 $M_{\odot}$. We have included a description on the eventual fate of the 8 UMi system in the main text.

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3) The argument regarding the short amount of time available for the planet to have tidally circularized since the host star reached its maximum size would be made far stronger by quoting an estimate of what this timescale actually is, or at least an estimate of the order of magnitude.

Our response:

We estimated the tidal circularization timescale using a constant Q formalism (Goldreich and Soter et al. 1966, Icarus, 5, 375), including dissipation both in the planet and the star, and both during the helium flash and at the present day. We find that this timescale is ~60 Gyr today and was ~1600 kyr at the time of the helium flash, if the planet were to be brought right to the outskirts of the star right at this time (this minimizes this timescale). The former is clearly longer than the core helium-burning timescale of ~100 Myr as estimated from canonical stellar evolution, and the latter is much larger than the ~50 kyr timescale for the star to descend from its maximum radius to its current-day value. We have added a small reference in the main text to the circularization timescale exceeding all other timescales, and a detailed description of the above calculation in the Methods section under 'Exploring survival scenarios'.

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4) The binary merger scenario outlined in this paper appears to initially create a so-called EL CVn binary (see Maxed et al. 2014, MNRAS, 437, 1681). Recent results have shown that these kinds of binaries are usually (perhaps always) the inner binaries of hierarchical triples (e.g. Lagos et al. 2020, MNRAS, 499L, 121). Could the outer companion be stellar in nature? Even if this isn't the case, the strong need for a stellar companion in this scenario (96 per cent of very close main-sequence binaries are found to be hierarchical triples - Tokovinin et al. 2006, A&A, 450, 681) means that this is worth mentioning and the possible effects this may have on the evolution of the planetary system.

Our response:

In the main text, we now discuss the binary evolutionary scenario in the context of EL CVn binaries and suggest that the companion may be stellar in nature. Following this interpretation, the system discussed in the paper may have once been a hierarchical triple that contained a circumbinary planet orbiting the central binary while being perturbed by the outer third star. For the planet to have survived until the binary merger, it must have met criteria for dynamical stability within the triple system that is additionally dependent on the semi-major axis of the outer third star (e.g., Busetti et al. 2018, A&A, 619, A91). After the merger, the planet now exists on an S-type orbit, and its long term orbital stability is defined by criteria outlined in Holman & Wiegert (1999, AJ, 117, 621) . The binary system then comprises 8 UMi and its unresolved outer companion; while the period of the outer companion is not currently well-constrained, astrometry places a lower bound on the outer companion to be as low as 5 au, leaving the possibility for a system as compact as that seen for the gamma Cephei system (Torres, 2007, ApJ, 654, 1095).

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5) Is it possible to constrain the rotation rate of the star/core from the asteroseismology? The merger scenario would imply that the core should be quite rapidly rotating, indeed the entire star is spun up during the stable mass transfer phase, so one would expect this star to be more rapidly rotating than similar stars that have evolved in isolation.

Our response:

Given a sufficiently long time series, it would be – in principle – possible to measure frequency splittings of the observed dipole modes caused by rotation. However, this cannot be done unambiguously with the current amount of data at hand for 8 UMi. The total length of the time series used for 8 UMi in this study spans 320 days, which translates into a nominal frequency resolution of 0.036 μHz. Core helium-burning red giants are known to have core rotation rates corresponding to frequency splittings between 0.01-0.1 μHz (e.g., Mosser et al., 2012, A&A, 548, A10; Tayar et al. 2019, ApJ, 887, 203), and so a finer frequency resolution is still needed at this point to cover the full range of possible rotation rates and definitively identify if the core rotation is fast or slow.

Complicating this analysis further is spectral leakage from gaps within TESS observing sectors that result in a low duty cycle (320d / 1085d) ~ 30%. The sidelobes from this spectral leakage effectively causes frequency ringing (i.e., Gibbs artifacts) that would impede the identification of genuine fine frequency splittings in the power spectrum. Nonetheless, we anticipate that more observations of 8 UMi over subsequent TESS observing cycles, which when combined with analysis of the spectral window function should be able to pinpoint the star's core rotation rate in future.

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6) Binary evolution plots are typically shown in terms of orbital period, obviously in this case plots with separation in au are useful to compare to the planetary orbit so I am not suggesting to change this, but perhaps adding orbital period to the right-hand axis of the top panel in extended data figure 8 and the

bottom panel of extended data figure 9 would aid in comparing this system to known populations of binaries.

Our response:

In the top plot of Extended Data Figure 8, we have added a second y axis on the right hand side indicating period, and have indicated the evolution of the binary's orbital period with a second curve indicating the binary's orbital period (which accounts for the mass lost in non-conservative mass transfer). We have also added a second y axis on the bottom plot of Extended Data Figure 9 to indicate the final orbital period.

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7) In the fiducial binary merger model what is the change in the planetary semi-major axis after the first mass transfer event, i.e. what was the original value (beta=0.6 implies it is not fully conservative, so the mass loss would have affected the planet's orbit)? How does this compare to known circumbinary planets?

Our response:

Indeed, the planet's semi-major axis should be expected to evolve in a way that conserves angular momentum ($M \sim a^{-1}$) —in the fiducial model, the semimajor axis increases by 0.13 au over the course of the evolution, assuming that its final value matches the present-day orbit of 8 UMi b (an increase in ~40% from its initial value). We have updated Extended Data Figure 8 to take into account the impact of this evolving planetary orbit on the maximum allowed binary orbit. The fiducial model still satisfies the constraint set by the planet's dynamical stability.

The planetary orbit (both pre- and post-mass transfer) draws comparisons with Kepler-38 (P = 105 d, a = 0.46 au) and Kepler-47b (P = 49 d, a = 0.3 au). While these two planets orbit binaries with mass ratios (q = 0.25) lower to our fiducial model, higher mass ratio systems such as Kepler-35 (q = 0.91) and Kepler-1647 (q = 0.79) exist. Had 8 UMi b existed as a circumbinary planet, it would have orbited a very short period P = 2 d) binary initially. The lack of observed circumbinary planets orbiting binaries with P < 7 d, however, remains an open question (e.g., Hamers et al. 2016, MNRAS, 455, 3180 ; Moe & Kratter, 2018, ApJ, 854, 44), which for 8 UMi may be related to its existence as a possible hierarchical triple. This is a prospect we defer to follow-up work.

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8) In extended data figure 3 the purple and blue lines are a little difficult to distinguish for those with color blindness. I suggest the use of a different color (the purple/green combination works well in other figures).

Our response:

We have modified the colors of the previously purple and blue lines to appear more color-blind friendly.

Referee #2:

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1) on Figure 3: since the right panel is a depiction of the boundary on Rtrgb as a function of stellar mass and metallicity, I am not sure what the two curves mean for each model or how to understand the following sentence in the caption: "Each code's boundary is drawn with two lines that indicate the boundary's uncertainty, which is propagated from the uncertainty of 8 UMi's mass and metallicity". Could the author please clarify this point?

Our response:

We now clarify that the curve or boundary corresponds to the instance where R_{tRGB} = planet's semi-major axis. Because R_{tRGB} depends strongly on the mass and metallicity of an evolutionary track, the boundary serves to show that R_{tRGB} across a broad range of mass and metallicity values is consistently larger than 8 UMi b's semi-major axis. There is some uncertainty in our measurement in the semi-major axis, and therefore the boundary is drawn with two lines to capture the $\pm$ 1σ variation of that measurement. We have rewritten the caption and relabelled the text in the Figure to better clarify these points.

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2) beginning of page 3: The authors briefly mention how unlikely the survival to a common envelope evolution would be. Considering that many of the following arguments rely on it, I think it would be helpful to remind to the non-expert reader how much time is expected to have elapsed since the star reached the tip of the red giant branch (including the measured uncertainties on the stellar parameters). In the same vein, it would be nice to actually provide an estimate of the survival time of the planet in a common envelope scenario to help the comparison.

Our response:

We estimate the upper limit of the time since the star reached the tip of the red giant branch to be the core-Helium burning timescale – this is because we know that 8 UMi is still burning core helium. This timescale is approximately 100 Myr, and we now quote this timescale in the main text when discussing the tidal circularization scenario.

A single-star MESA model of a 1.5 $M_{\odot}$ star takes ~1.7 Myr to evolve from R = 0.5 au to the tip of the RGB. Modeling the drag force on the planet as a simple ram pressure in a medium with the average density of the star at engulfment, we can estimate the survival time of the planet in a common envelope as its inspiral time when the star expands in radius to the planet's orbit. This yields a survival time $t \sim E/\dot{E} \sim 10^2$ yr. We have also added these timescales to the main text, alongside a reference to MacLeod et al. (2018, ApJL, 853), where a similar calculation for the latter is done.

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3) 2nd paragraph of page 3: I agree with the authors that it would require extremely fine tuned parameters to explain the current system with a tidal migration that would have halted just at the right location. However, for the dynamical scattering followed by tidal circularization scenario, it would be nice to actually quote plausible timescales and to compare them with the age of the system since the tip of the RGB (see previous comment).

Our response:

We estimate the tidal circularization timescale using a constant Q formalism (Goldreich and Soter et al. 1966, Icarus, 5, 375), including dissipation both in the planet and the star, and both during the helium flash and at the present day. We find that this timescale is ~60 Gyr today and was ~1600 kyr at the time of the helium flash, if the planet were to be brought right to the outskirts of the star right at this time (this minimizes this timescale). The former is clearly longer than the core helium-burning timescale of ~100 Myr as estimated from canonical stellar evolution, and the latter is much larger than the ~50 kyr timescale for the star to descend from its maximum radius to its current-day one. We have added a small reference in the main text to the circularization timescale exceeding all other timescales, and a detailed description of the above calculation in the Methods section under 'Exploring survival scenarios'.

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4) Then the authors try to explain the existence of the system using a peculiar scenario involving the merger of an initial binary star.

Although it seems that this scenario can meet the various constraints provided by the observation of the system, I think it is difficult to get a feel for how (un)likely or finely tuned the initial conditions on the binary need to be. In particular when the tidal scenario is rejected on such a kind of principle. It would be really helpful if the authors could roughly estimate the fraction of binary stars that would fall in a relatively close area of the parameter space.

Our response:

This particular type of binary, namely one that produces an A- F- dwarf star – white dwarf in a tight orbit, is also known as an EL CVn-type binary (Maxted et al. 2014, MNRAS, 437, 1681). A previous study (Chen et al. 2017, MNRAS, 467, 1874) has examined the formation of EL CVn binaries via simulations, and the study's Figure 3 details the parameter space (e.g., mass ratios, initial binary periods) that would lead to a first, stable mass transfer phase when the donor reaches the base of the red giant branch, which is the scenario we posit for 8 UMi. To summarize, there is flexibility in both the donor mass (M_1) as well as the mass ratio between donor and secondary ($q_0 = M_1/M_2$) that will allow our scenario to be feasible.

Donors that undergo stable mass transfer at the base of the red giant branch can be produced for donor masses between 1.1-1.25 $M_\odot$. This range allows the merger to happen without requiring orbits of the binaries after the mass transfer to reach periods of 10 days or longer, as there is still the planet orbital stability constraint to consider. Within this range, there are a range of initial periods (1.9 d < $P_{\rm init}$ < 2.4 d) and mass ratios ($1 < q_0 < 1.5$) that will allow the 8 UMi scenario to arise. We anticipate various combinations of donor masses and mass ratios within this range that can allow the final mass of the stellar merger remnant to reach 1.5 $M_\odot$ with a core mass of 0.5 $M_\odot$ – which is 8 UMi's observational constraint.

To further provide estimates to the occurrence of this kind of binary system, we report in Methods the frequency of companions per decade of orbital period of short-period (< 3 d) binary systems with $q > 0.3$ to be 0.017 ± 0.007 (Moe and Di Stefano 2017, ApJS, 230, 15). We also report the estimated occurrence rate of long period (2.2 d < $P_{\rm init}$ < 10 d) EL CVn binaries in the Galaxy (similar to our predicted history of 8 UMi) to be ~ 0.01/yr based on the Chen et al. 2017 simulations.

To estimate the formation rate of EL CVn binaries, Chen et al. 2017 perform a population synthesis assuming a Miller & Scalo (1979, ApJS, 41, 513) IMF, described approximately by the Eggleton–Fitchett–Tout (1989, ApJ, 347, 998) formula, and a constant SFR = 5 $M_\odot$/yr. They find that, for the last few Gyr, the production rate of EL CVns is ~0.015/yr. In comparison, using the same IMF and SFR, it can be inferred that the formation rate of stars between 1 and 2 $M_\odot$ is ~0.6/yr. Taking the ratio of these two numbers, it can be inferred that ~2.5% of degenerate-core clump stars should have undergone a similar process to the fiducial binary scenario described in this paper. This number is roughly consistent with estimates from astrophysical transients, close binaries, and red giant to clump binarity (see Rui & Fuller 2021).
We have added a description of the above calculation to the Methods section, under the "Modelling the binary scenario" header.

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5) Finally, this system is presented as a missing link between main-sequence planet-hosting stars and planet-hosting white dwarfs. But, there is no discussion of how such a system could actually evolve (or not) into the kind of planetary system observed around white dwarfs. I think that a discussion should be added about that. Such a discussion would be crucial to help the reader place this discovery in the big picture.

Our response:

Because we have not explored in detail the role of 8 UMi-like systems in forming progenitors of known white dwarf (WD) systems, we have omitted mention of the WD connection in the main text. This will enhance the clarity of our article, for which the big picture is the extended survivability of post-main sequence exoplanetary systems, and our resulting reassessment of exoplanet demographics around evolved star systems.

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6) Please give more details in the caption to extended figure 7. At the moment it is difficult to understand what the curves actually mean, which curve sets upper or lower limits and what the shaded region means.

Our response:

We now provide more detail in the caption. Specifically, we now explain that the contours indicate 67 % highest density intervals of permissible mass and separation values of the outer companion as estimated from RV residuals (purple), and from measurements of 8 UMi's Gaia DR3-Hipparcos astrometric acceleration (green). The overlap between the two contours form the shaded region that represents the joint constraint between RV and astrometry measurements.

Referee #3:

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1) The authors use very strong language to describe the detection of the planet orbiting 8 UMi, saying things like "confirming the planetary nature of the radial velocity variations" and "The long term stability of 8 UMi's 93-day radial velocity variations confirms an orbiting companion rather than stellar activity". I am okay with the authors using the word "confirmed" to describe the radial velocity detection itself, since they have now provided an independent dataset to the discovery data that shows the same signal. However, I do not think using the word "confirmed" is appropriate for the planetary interpretation. Usually in the literature, "confirming" a planet means detecting it with an independent detection method (like transits + RVs, or RVs + astrometry), which has not been done here yet. This is particularly important because the inclination of the planet's orbit has not been constrained, so the companion's mass may be significantly larger than the minimum mass found in the RV analysis. I recommend the authors replace the word "confirmed" in these cases with "strongly suggests" or "supporting" to more accurately reflect these uncertainties.

Our response:

We have replaced instances of “confirmed” with words of lesser strength to better represent the uncertainty in the planetary interpretation. Changed instances are the following:

<u>Main Text</u>
Second paragraph, first line: “...variations confirms an orbiting companion…” -> “...variations suggests an orbiting companion…”

Second paragraph, final line: “...confirming the planetary nature of the radial velocity variations.” -> “...further supporting the planetary nature of the radial velocity variations.”

Methods

Stellar Activity, second line: “We further established the planet's existence…” -> “We further confirmed the planet's existence”

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2) The authors leave the true mass of the orbiting companion unconstrained, as is often the case for radial velocity planet detections. I agree that this is most likely a planetary-mass companion, but as has recently been demonstrated by Gaia observations of another famous RV detection published in Nature (https://ui.adsabs.harvard.edu/abs/1989Natur.339...38L/abstract), sometimes promising planet signals can instead be caused by binary stars with nearly face-on orbits(https://ui.adsabs.harvard.edu/abs/2022AJ....164..196W/abstract). I recommend that the authors attempt to constrain the true mass of the 93 day companion using constraints from either Hipparcos astrometric measurements, or the residuals of Gaia DR3 astrometry (which appear to have no strong excess astrometric noise, consistent with a planetary interpretation). A quantitative exploration would strengthen the paper.

Our response:

The astrometry for 8UMi is consistently well-behaved in that its Renormalized Unit Weighted Error (RUWE) is below 1.4 for both DR2 (ρ_{DR2} = 1.09) and DR3 (ρ_{DR3} = 0.80). One approach that we adopted is to use the Gaia DR2 RUWE following Belokurov et al. (2020, MNRAS, 496, 1922) to estimate the photocenter wobble, $\delta\theta \approx 0.53\, \sigma_{\varpi}\, N^{0.5}(\rho_{DR2}\ -1)^{0.5}$, where σ_{ϖ} is the star’s parallax error and N is the number of good along-scan Gaia observations. Assuming a face-on orbit (as we are considering the scenario where the companion is more massive than a Jovian with very small inclination), the perturbation can be expressed – following Penoyre et al. (2020, MNRAS, 495, 321) – also as $\delta\theta = (a|q - l_r|)((1+q)(1+l_r))^{-1}$, where $q = M_{planet}/M_{\star}$ is the mass ratio and $l_r = L_{planet}/L_{\star} \sim 0$. is the luminosity ratio. Assuming the companion contributes wholly to the RUWE value, we estimate an upper bound mass of approximately 43 M_J , which rules out stellar companions. We additionally apply the statistics of randomly distributed angles, which show there is a ~99% chance that 8 UMi b has an orbital inclination angle greater than 7.3°, at which it is less massive than a brown dwarf (13 M_J). We include these constraints in the Main Text, and describe these calculations in the Methods section under the ‘Exploring survival scenarios’ header.

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3) Along the lines of my previous point, I would like to see some discussion of how a significantly larger companion mass would change the formation scenarios discussed in the paper. How massive must the 93 day companion be for common envelope evolution to be a viable formation pathway? Is this consistent with the constraints from Hipparcos and Gaia astrometry?

Our response:

While the observations could in principle be explained by a massive, super-planetary companion on a very inclined orbit, we argue that a common-envelope formation scenario for this system is not possible even for arbitrarily massive companions for the following two reasons:

[a] Structurally, the host star is very similar to a normal core helium-burning star with a large envelope. However, because red giants far up the RGB (e.g., at the time that the companion would be engulfed) are dominated by their convective envelopes, they are expected to behave approximately as $\gamma = 5/3$ polytropes whose radii increase as mass is lost (this is the same reason why, in the fiducial binary scenario, the first stage of mass transfer must happen low on the RGB to be stable). Therefore, if the companion were to cause a successful common envelope, it is likely to unbind most of the envelope and leave a helium star/WD rather than a normal clump star (i.e., it should not be possible to only unbind part of the envelope of a slightly more massive RG and terminate the CEE). This is not what is observed.

[b] In the standard common envelope formalism, the initial orbital energy $-GM_pM_1/2a_i$ "decreases" to $-GM_pM_c/2a_f$, where M_1 is the mass of the red giant and M_c is the mass of its core only. This change depends on the change in the mass of the star M (i.e., it decreases as the envelope is removed) as well as the orbital separation (which typically dominates the dependence in these calculations). However, in this case, M_c/M_1 ~ 1/6, but a_f/a_i ~0.47/0.7 ~ 2/3 (assuming, generously, that the planet is engulfed right as the star reaches the tip of the RGB). In this case, the companion's final orbital energy would actually increase because the fractional decrease of $a_f - a_i$ is outweighed by the decrease in mass $M_1 - M_c$. Independent of M_p, the change in orbital energy therefore cannot unbind the envelope. We have included a discussion of the above points in the Methods section under the 'Exploring survival scenarios' header.

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4) I would like to see a more fleshed-out step-by-step description of the binary evolution scenario in the letter itself on Page 3. I think this is the most interesting take-away from this work, and it should not be buried in the Methods/Supplementary Information!

Our response:

We have migrated details of the binary evolution simulation from Supplementary Information to Methods, and expand on the binary evolution scenario in the main text. For the latter, we now describe our simulations as involving the binary evolution of two low-mass stars that initially orbit each other at a 2-day period before merging once forming a white dwarf- red giant pair. We now describe the progenitor of this system as possibly belonging to the so-called EL CVn binaries (Maxted et al. 2014, MNRAS, 437, 1681), and we also elaborate on the expected evolution of the star after the merger process.

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5) Page 2: "a minimum distance of 5 AU (67% confidence, see Supplementary Information for further details)." -- does the 67% confidence refer to the semimajor axis, or the confidence level in the detection of

the trend? I see from reading the supplementary information that it is likely the semimajor axis, but it might help to clarify this here.

Our response:

The confidence level is for the semi-major axis. We now clarify this in the main text.

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6) Page 3: "Survival of the planet in a common envelope evolution " - remove the word "evolution" here.

Our response:

We have replaced the word “evolution” here with “event” (viz. “common envelope evolution”).

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7) Page 3: "Tentative evidence of an outer companion from 8 UMi’s radial velocity data may suggest scattering processes9, 29 favouring this scenario." -- It would be helpful to readers to quantify "tentative" here.

Our response:

This sentence has been omitted from the text without loss of clarity in the discussion of scattering processes as a possible explanation for 8 UMi’s existence.

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8) Page 3: Since the Li abundance is an important piece of evidence, I recommend the authors try to quantify their confidence that the Li abundance is anomalously large. I recommend looking at Figure 1 of Soares-Furtado et al 2021 (https://ui.adsabs.harvard.edu/abs/2021AJ....162..273S/abstract), who tabulate the mean and scatter lithium abundance as a function of HR diagram position. Reading off their plot, I estimate roughly 2-3 sigma confidence that UMi's Li abundance is higher than typical stars.

Our response:

Adopting a value of *A(Li)* = 2.0 +/- 0.2, taken as the average and dispersion of the Local Thermodynamic Equilibrium measurements of Charbonnel et al. (2020, A&A, 633, A34) and Kumar et al. (2011, ApJL, 730, L12), we estimate with a 3 sigma confidence that 8 UMi’s A(Li) is above 1.4, which is the upper limit for lithium abundances from both models and GALAH observations of red giants as described in Chanamé et al. (2022, ApJ, 933, 58). We now include this quantification in the main text.

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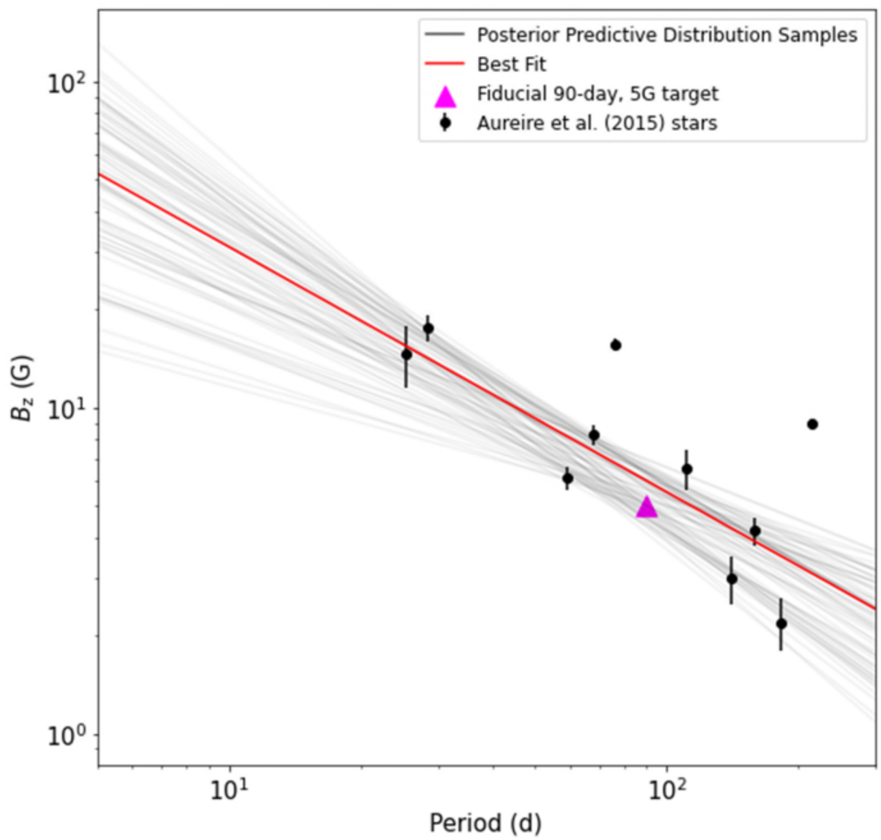
9) Methods Page 11: "This firmly excludes the presence of a ∼ 5G mean longitudinal magnetic field strength expected for an active red giant rotating at a period of 90 days15." -- how much scatter is in the empirical relations used here to connect a 90 day rotation period to a 5 G magnetic field strength?

Our response:

Using data from the Auriere et al. (2015, A&A, 574, A90) paper, we fit a linear function of the form log (Bz) = intercept + grad × log (P), identical to their analysis in Figure 6. The red line in the plot below represents our best fit of grad = -0.75 ± 0.22 and intercept = 2.2 ± 0.3. The uncertainty in the fit is shown with our samples (grey lines) drawn from the posterior predictive distribution from our MCMC exploration of this fit. As shown in the plot, a 90 day rotation period with a 5 G magnetic field strength (pink point) can be expected within the scatter given by the data.



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10) Supplementary Information Page 14: "The improvement in WAIC in using the trended model surpasses the standard error in WAIC differences between the two models (dWAIC = 3.84), and is thus significant" - I am not particularly familiar with the WAIC for model comparison. Could the authors please also give some other metrics for assessing the confidence of the detection, like the Bayesian Information Criterion, or a frequentist confidence level?

Our response:

The Widely-Applicable Information Criterion (WAIC) is also known as the Watanabe-Akaike Information Criterion (both terms are used interchangeably) and functions very similarly to the standard Akaike Information Criterion in indicating model predictive accuracy. As described by Gelman et al. (2013, https://arxiv.org/abs/1307.5928), in Bayesian modelling WAIC is preferred over AIC because the latter uses the maximum likelihood value as a point estimate to evaluate model predictive accuracy, whereas WAIC averages the likelihood of each observation drawn from the posterior distribution, which may be better representative of a model's predictive power. This averaging is similar to cross-validation, and is responsible for the 'standard error of the WAIC differences' as reported in the text.' We have modified the text to describe that we are effectively estimating AIC to identify the model with a better predictive accuracy. Additionally, we modify the units such that it now corresponds to values of deviance (-2 × previously reported score), where the lower the score, the better – in alignment with expectations from an AIC-like score.