Peer Review File

Manuscript Title: A wide-orbit giant planet in the high-mass b Centauri binary system

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referee #1 (Remarks to the Author):

Dear Authors and Editor,

I have reviewed with great interest this manuscript. The convincing detection of an extreme mass ratio companion (likely planetary) in a circumbinary orbit around intermediate mass stars is a novel and groundbreaking discovery. While I have a few minor concerns that I think should be addressed by the authors, overall I think it meets the criteria for publication in Nature (B. original and significant, C. valid approach, D. high quality data, E. robust conclusions, G. appropriate references, H. clearly written). Suggestions for improvement follow:

- Planet vs BD? I think the authors have shown that object is very likely below the deuterium burning limit (and certainly falls into the category of "planet-like" mass ratios.) To be clear, I believe the object is interesting and noteworthy whether it's 5 Mjup or 20Mjup. I just think a few things could be more clearly explained / stated:
a) In the discussion of Regianni et al 2013, Nielsen et al 2019, it might be useful to include the statistics of Schalufman 2018 and Wagner et al 2019, which discuss the frequency of gas giant companions at close and wide distances as a function of mass. By mass, it would live in the star-like part of the parameters (i.e. the low mass tail), whereas by mass ratio, it would appear more planet like. Brief clarification in the text on these issues in the first paragraph on page 5 would be useful. In the Supplemental information, this issue is discussed further, but the model description is opaque (and cites an unpublished paper). I also worry about the parameters quoted for the stellar model. Companion frequencies vary across 30-800 au separation range. I'm also surprised by the peak at such large radii (400 au) , as I think the peak separation of binaries decreases with increasing primary mass and is less than ~50 au above 1 Msun (Moe & Di Stefano 2017, Moe & Kratter 2021)

b) In the description of the mass determination for the companion, the issues of age and distance uncertainty aren't discussed as clearly as for the stars themselves. Is the spread due to these uncertainties less than the model uncertainties?

- System Architecture:
a) the authors give the sky-plane inclination of the orbit. Can any constraints be placed on the likely orientation of the binary? The authors mention some tentative RV trends and information on rotational velocities. Can they rule out or in that the binary and planetary planes are aligned or misaligned?

b) eccentricity: I do not think $e<0.4$ is "near circular". The quoted 17% probability of obtaining $e < 0.4$ in a thermal distribution is also not a compelling argument against capture — especially when comparing this with the overall occurrence rate of such system (<1%) I am not arguing that capture is a compelling explanation, just that $e \sim 0.4$ is substantial, and could easily be consistent with dynamical interactions of some kind.

c) Dynamics of Circumbinaries: I think the formation discussions really undersell the circumbinary nature of the system. First, the presence of the binary opens up many new pathways for orbital excitation beyond the normal planet-planet scattering scenarios. Scattering and ejection can be driven by close encounters with the binary, or even just with regions of overlapping mean motion resonance (Holman & Wiegert 1999, Mudryk & Wu 2006, Smullen et al 2016, Quarles et al 2018). 500 AU is a wide orbit for any disk formation mechanism (gravitational instability or core accretion), so the idea that scattering might have occurred is relevant. Note that even if the orbital period ratio is enormous today, previous interactions are possible, and the inner binary could have had a wider orbit in the past at the time of formation.

- Formation:

Overall I think the discussion of the formation pathways is clear, concise, and nuanced. The only suggestion I have is to address the mass constraints for both models a bit further. 10 Mjup is certainly very large for core accretion models around solar type stars, but accounting for the increased disk mass one might expect for a 10 Msun system, is it really any harder to explain than Jupiter? The timescales at fixed separation are faster in a massive disk around a 10 Msun star — both due to increase in \Omega and increase collisional frequencies. Similarly, from the opposite direction, if formed by gravitational instability, the disk in question would have been at least 1-2 Msun. The fragment mass predicted by analytic theory and in simulations is well above 10 Mjup. (e.g. Rafikov 2005, Kratter et al 2010, Forgan et al 2013). On this same note, for intermediate and massive stars, gravitational instability is invoked as a method for binary formation as well, so one could argue that disfavoring gravitational instability is disfavoring a stellar origin (though again 500au is still a bit wide for in-situ disk fragmentation).

On a side note ,the statement (ext. data fig 6) that the object has an unusually low ratio relative to other planets in wide obits is misleading as this could still be a selection bias. We have limited observational constraints on ~1 Mjup objects from direct imaging surveys.

Referee #2 (Remarks to the Author):

Dear Dr. Sage,

I have read the manuscript entitled “A wide orbit giant planet in the 10 solar-mass b Centauri” by Janson and collaborators, submitted to Nature for consideration in the main journal. The authors detail the discovery of a planet orbiting two intermediate-to-high mass stars, which would represent the lowest planet-to-host(s) mass ratio of any directly imaged planet, at 10^-3. The uniqueness of the discovery is best captured by Extended Data Figure 6, showing how unusual this planet is among the thousands of exoplanets discovered to date.

All in all, the authors provide convincing evidence that they have in fact detected a planet in orbit around b Cen, thanks to their multiple epochs of observations and an archival observation taken by another team several decades ago. I believe this discovery is worthy of publication in the journal Nature; however, I have several comments that I would like addressed before I can recommend the manuscript for publication. If the authors can address these concerns, I would be happy to recommend the article for publication.

Major comments:

The authors note that the other team discarded the discovery as a background star owing to its colors and the planetary atmosphere models available at the time. The authors state that a modern analysis shows the colors are consistent with a planet (of 11 Jupiter masses). However, the authors do not provide more details concerning this discrepancy, and as a reader I’m left to wonder what specifically changed in the models since the previous analysis. I suspect many, if not all, readers in the field will ask this question. I suggest the authors elaborate on this discrepancy in the models in the supplemental information section.

By the way, I looked up the cited paper (Shatsky & Tokovonin 2002), and I could not find where it was stated that the object had colors inconsistent with a planet. Instead, I found that the object had a "O" and a "c" flag in the data tables. "O" means the object was determined to be an "optical (background) companion", rather than a "physical companion", but here is a quote from that paper: "All new components which are fainter than K = 12 or J = 13 are considered as optical." That seems like a conservative detection criteria, rather than anything to do with planetary models. Could these authors elaborate on this in their response to this referee report?

The authors spend a good deal of text describing how difficult it is to form a planet like b Cen (AB) b given it's distance from the host and low eccentricity. It reads as if this is unique to b Cen (AB) b, when if fact there are several known directly imaged planets which do not easily fit into existing planetary formation paradigms, most notably the HR 8799 planets. I ask that the authors place this discussion in the context of other directly imaged planets, and whether b Cen (AB) b is in fact unique with regard to its orbital distance and eccentricity.

Furthermore, the discussion on the formation of b Cen (AB) b appears to completely ignore the binary nature of the host. It reads as if the authors are interpreting the binary host as a single high-mass host. But presumably the binary nature of the host would affect the orbital dynamics and evolution of the planet, but this is not discussed at all. We know that binary stars result in large disk gaps (see the literature on binary stars and transitional disks); how would this affect the interpretation? I would naively assume the capture scenario is different for a single vs binary host, given there is the opportunity to exchange angular momentum with the binary star. I ask that the authors consider the binary nature of the host in the discussion of planet formation scenarios. If it is reasonable to treat the binary as a single star of equivalent mass, then this needs to be better justified in the text, preferably in the main section.

Minor comments:

The title is fine, but I wonder if it could be improved to better capture the uniqueness of the system. "A wide orbit giant planet in the 10 solar-mass b Centauri" does not mention the binary host, and even implies that the host is a single star, which is inaccurate. Also, technically, since "wide" modifies "orbit" and not "giant" or "planet" it should be hyphenated to "wide-orbit".

Figure 2 and Extended Data Figures 3 and 4: These are confusing figures. It is difficult to appreciate the plot in the frame of the host star, and I have trouble thinking about the planet as if it were a background star ("if bkg"). Perhaps the frame of the sky would be better, showing the host motion, the expected planet motion (including orbit), and the measured position of the planets? It would also be helpful to have inserts (small plots inside the large plot) zooming in on the points and showing their full error bars.

Figure 3: It is difficult to find b Cen (AB) b on the plot. Could it be plotted with more relief? Or maybe fade all of the other points a bit.

Extended Data Figure 6 is useful for distinguishing this planet from other directly imaged planets. Perhaps this figure would be more appropriate in the main section rather than the extended section.

I found Extended Data Figure 5 unnecessary, as it is virtually identical to simply stating the mass determination and 1-sigma Gaussian errors as text. Instead, I would much rather see a plot capturing the various parameters that go into the mass determination and their various degeneracies. Perhaps a contour plot showing initial entropy versus planet color with mass contours and likelihood contours? I realize that is complex, but it is appropriate for a journal like Nature.

Referee #3 (Remarks to the Author):

This paper presents detection and characterization (photometric and astrometric) of a planetary mass companion to the tight, high-mass binary system b Centauri AB.

The authors make a strong case that this directly imaged planet is superlative in several ways that make it interesting from a planet formation theory perspective. Perhaps the most compelling is its presence around a B star binary, but it's extreme separation and mass ratio will also be of interest to the community.

The data are unequivocal in my opinion as to the bound and planetary nature of the companion, and the methodology and conclusions appear quite robust. The paper is very well-written and organized in general. I would categorize all of my suggestions below as clarifying and relatively minor in nature, and I see no barriers to quick publication of this work.

General Comments:

There are a few bits of context that I think should appear earlier and perhaps in more detail in the paper:
1. There is some good broad context about wide-orbit companions and the frequency of single and binary planet hosts at the very end of the paper (in the "Formation Analysis" section) that I think should appear much earlier, certainly in the main body of the paper. I'd also suggest fleshing this section out with more specific comparisons to previous directly imaged and otherwise detected (RV) planetary mass companions. I wondered about HD106906 and the frequency of directly imaged planets around binary host stars, in particular.
2. More generally speaking, I think the significance of the fact that this is a tight binary system can/should be discussed more directly somewhere near the beginning of the paper. The possible range of separations for the binary pair is discussed in angular separation units, but I think a range of potential physical separations and a discussion of the possible influence (or, more likely I believe, lack of influence) of the binary pair on the evolution of the system, would be beneficial.
3. I think there is a bit of missing context about the observed and/or predicted radial extent of disks around very massive stars. Is there an expectation that there is enough material at 550au and/or that conditions are likely to be favorable at this location for planet formation via any mechanism?
4. [minor, but] I think the angular and orbital separation of the planet, as well as the distance to the system, should appear earlier in the paper.

And some more detailed comments/suggestions.

Abstract:
Line 25: how wide is wide? This is a good place to provide some of the numbers, as suggested in #4, above.

Introduction:
PDF page 3, line 25 ◊ the statement that the colors of the companion firmly establish it as an exoplanet seems a bit of a stretch. Is its location in the CMD not fairly consistent with field BDs?

Line 31 -33 ◊ It might be helpful to state explicitly somewhere in here that the lower mass star does not have a confirmed spectral type. It took me a while to understand why it doesn't.

PDF Page 4, line 15 ◊ the reasoning for the system age of 15+/- 2 Myr is described in detail in the extended methods section, but I think it should be stated at least briefly here that this is based on an extremely high membership probability for the UCL moving group

Lines 19-21 ◊ I think separate comparisons to the highest mass single stellar host and the highest combined binary mass host would be more helpful here than just the "three times higher" reference.

PDF Page 5, line 15 ◊ "orbit fitting code". I'm not sure what the convention is for the body of a Nature paper, but I wondered immediately here what kind of orbit fitting code (based on later descriptions, I infer a combination of rejection sampling and MCMC).

Line 19-20 ◊ This range of eccentricities extends pretty high, in my opinion (to 0.41), suggesting that "near-circular" is a bit of a stretch

Line 30-31 ◊ A description of why this mass range has not been previously probed with direct imaging, as well as a description of how thoroughly it has been explored with RV methods, would be quite beneficial context here

PDF Page 7, lines 16-18 ◊ It's unclear why membership in UCL implies that it was never part of a dense stellar environment. I assume that it is a timescale argument based on the young age of UCL, but I think this should be clarified.

Lines 28-30 ◊ This is a place where some more specific RV statistics would be beneficial. The statement that the RV distribution implies NO planet hosts above 3 solar masses is a bit different from the framing elsewhere in the paper that the frequency distribution is dropping off steeply at high masses. This is admittedly nit-picky, but 0 is different from very low. I also believe it needs a qualifier. Aren't the RV planet frequency rules usually explicitly for planets within some number of au (10?)? I think this is what is meant by "cannot be extrapolated to the full system architecture", but I think that it needs a qualifier earlier (e.g. "upper stellar limit for hosting giant exoplanets within 10au")

Methods page 4, lines 3-4 ◊ I don't follow the link between the stated axis ratio limit of 0.88 and 16mas (which is not a ratio).

Lines 29-30 ◊ I believe the reference should be to orbitize, with OFTI as a module within that code

Methods Page 5, Lines 1-27 ◊ I had a great deal of difficulty following this section and would suggest restructuring and clarification throughout. It's basically the only section of the paper that wasn't clear and well-structured in my opinion, and as a result, I couldn't quite decide whether I should buy the error bars on position.

Methods Page 6 , lines 4-5 ◊ "since it is partially calibration limited". What is it? Limited by what calibrations?

Line 11 ◊ If you are adopting the full range of uncertainties in the age and initial entropy then shouldn't you adopt the full range of masses and luminosities as the uncertainty rather than the standard deviation? How much does this change the results?

Methods Page 7, Lines 2-3 ◊ " from a mass ratio from …. 1000au". This phrasing is hard to follow.

Lines 13-14 ◊ Though I appreciate the difference in them, both probabilities are quite low. It could perhaps be emphasized here that these systems are quite rare, perhaps with some statistics from the BEAST sample.

Figures:

Figure 3:
I'd suggest adding labels with names for some of the other planetary mass objects that you might compare this system to here, especially HD 106906 (also widely separated) and 51 Eri (similar

mass ratio). Maybe also other UCL companions?

Extended data figure 2:
I'd find it helpful to see a range of orbital fits (I believe orbitize will do this for you) to the astrometry in addition to the posteriors.

Extended data figure 6: I'd suggest putting this figure in the main body of the paper. I think it's particularly useful for a general audience.

Author Rebuttals to Initial Comments:

Referee #1 (Remarks to the Author):

Dear Authors and Editor,

I have reviewed with great interest this manuscript. The convincing detection of an extreme mass ratio companion (likely planetary) in a circumbinary orbit around intermediate mass stars is a novel and groundbreaking discovery. While I have a few minor concerns that I think should be addressed by the authors, overall I think it meets the criteria for publication in Nature (B. original and significant, C. valid approach, D. high quality data, E. robust conclusions, G. appropriate references, H. clearly written). Suggestions for improvement follow:

Ref 1: - Planet vs BD? I think the authors have shown that object is very likely below the deuterium burning limit (and certainly falls into the category of "planet-like" mass ratios.) To be clear, I believe the object is interesting and noteworthy whether it's 5 Mjup or 20Mjup. I just think a few things could be more clearly explained / stated:

a) In the discussion of Regianni et al 2013, Nielsen et al 2019, it might be useful to include the statistics of Schalufman 2018 and Wagner et al 2019, which discuss the frequency of gas giant companions at close and wide distances as a function of mass. By mass, it would live in the star-like part of the parameters (i.e. the low mass tail), whereas by mass ratio, it would appear more planet like. Brief clarification in the text on these issues in the first paragraph on page 5 would be useful. In the Supplemental information, this issue is discussed further, but the model description is opaque (and cites an unpublished paper). I also worry about the parameters quoted for the stellar model. Companion frequencies vary across 30-800 au separation range. I'm also surprised by the peak at such large radii (400 au) , as I think the peak separation of binaries decreases with increasing primary mass and is less than ~50 au above 1 Msun (Moe & Di Stefano 2017, Moe & Kratter 2021)

Auth: We have added discussion about the Schlaufman (2018) and Wagner et al. (2019) studies and their relations to mass distributions and stellar masses in the formation discussion in the Methods section. The model described in that section accounts for the variation in companion frequency as a function of mass ratio and orbital separation; we now specify this more explicitly in the text. The peak at 400 au for wide companions is based on the de Rosa (2014) results, and follows the natural shift of the general companion distribution toward increasing separations with increasing mass, from M-type stars through FGK and up through A-type stars. For very massive stars (most emphasized for O-type stars, continuing down through B-type stars) there is then a second peak of predominantly close-in companions, which contributes to shifting the average separation inward for early type stars in the Moe & di Stefano (2017) study. We do not think that the inner peak will have any large impact at wide separations. However, as these examples highlight, the models are necessarily based on extrapolations, particularly as the frequency of high-contrast companions to B-type stars isn't currently known, and is exactly one of the issues that BEAST will address. More precise calculations can therefore be made after the survey is done. We have added text in the Methods section to highlight this aspect.

Ref 1: b) In the description of the mass determination for the companion, the issues of age and distance uncertainty aren't discussed as clearly as for the stars themselves. Is the spread due to these uncertainties less than the model uncertainties?

Auth: We have clarified in the Methods section that the distance uncertainty is implicitly accounted for in the conversion from apparent to absolute magnitude (the impact is small, since the uncertainty in the parallax is only 3%). The age uncertainty is described in the same section.

Ref 1: - System Architecture:

a) the authors give the sky-plane inclination of the orbit. Can any constraints be placed on the likely orientation of the binary? The authors mention some tentative RV trends and information on rotational velocities. Can they rule out or in that the binary and planetary planes are aligned or misaligned?

Auth: It is not yet possible to derive information about the binary orbit orientation, but with dedicated observations this can be done in the future. We have plans for both radial velocity and interferometric observations for this purpose. There is a measurement of the projected rotational velocity for the primary star, but without a measurement of its rotational period or equivalent value, its rotational plane cannot be uniquely determined.

Ref 1: b) eccentricity: I do not think e<0.4 is "near circular". The quoted 17% probability of obtaining e < 0.4 in a thermal distribution is also not a compelling argument against capture — especially when comparing this with the overall occurrence rate of such system (<1%) I am not arguing that capture is a compelling explanation, just that e ~ 0.4 is substantial, and could easily be consistent with dynamical interactions of some kind.

Auth: We have removed the "near circular" interpretative statement, and now merely refer to the number itself in the text. We certainly agree that 17% is a far from negligible probability and tried to communicate that in the original manuscript, but we have now further softened the statement to emphasize this more clearly.

Ref 1: c) Dynamics of Circumbinaries: I think the formation discussions really undersell the circumbinary nature of the system. First, the presence of the binary opens up many new pathways for orbital excitation beyond the normal planet-planet scattering scenarios. Scattering and ejection can be driven by close encounters with the binary, or even just with regions of overlapping mean motion resonance (Holman & Wiegert 1999, Mudryk & Wu 2006, Smullen et al 2016, Quarles et al 2018). 500 AU is a wide orbit for any disk formation mechanism (gravitational instability or core accretion), so the idea that scattering might have occurred is relevant. Note that even if the orbital period ratio is enormous today, previous interactions are possible, and the inner binary could have had a wider orbit in the past at the time of formation.

Auth: We have now included a discussion on binarity and corresponding scattering in the text.

Ref 1: - Formation:

Overall I think the discussion of the formation pathways is clear, concise, and nuanced. The only suggestion I have is to address the mass constraints for both models a bit further. 10 Mjup is certainly very large for core accretion models around solar type stars, but accounting for the increased disk mass one might expect for a 10 Msun system, is it really any harder to explain than Jupiter? The timescales at fixed separation are faster in a massive disk around a 10 Msun star — both due to increase in \Omega and increase collisional frequencies. Similarly, from the opposite direction, if formed by gravitational instability, the disk in question would have been at least 1-2 Msun. The fragment mass predicted by analytic theory and in simulations is well above 10 Mjup. (e.g. Rafikov 2005, Kratter et al 2010, Forgan et al 2013). On this same note, for intermediate and massive stars, gravitational instability is invoked as a method for binary formation as well, so one could argue

that disfavoring gravitational instability is disfavoring a stellar origin (though again 500au is still a bit wide for in-situ disk fragmentation).

Auth: From a mass perspective, core accretion would be reasonable, but a remaining issue in the context of massive stars and disks is the timescale issue alluded to in the text: The disk dispersion should be rapid, while core accretion is expected to be quite slow. We have added a discussion about the issue of fragment mass in the Methods section.

Ref 1: On a side note ,the statement (ext. data fig 6) that the object has an unusually low ratio relative to other planets in wide obits is misleading as this could still be a selection bias. We have limited observational constraints on ~1 Mjup objects from direct imaging surveys.

Auth: We have now clarified in the caption that the statement intends to refer to only to the detected population of planets.

Referee #2 (Remarks to the Author):

Dear Dr. Sage,

I have read the manuscript entitled “A wide orbit giant planet in the 10 solar-mass b Centauri” by Janson and collaborators, submitted to Nature for consideration in the main journal. The authors detail the discovery of a planet orbiting two intermediate-to-high mass stars, which would represent the lowest planet-to-host(s) mass ratio of any directly imaged planet, at 10^-3. The uniqueness of the discovery is best captured by Extended Data Figure 6, showing how unusual this planet is among the thousands of exoplanets discovered to date.

All in all, the authors provide convincing evidence that they have in fact detected a planet in orbit around b Cen, thanks to their multiple epochs of observations and an archival observation taken by another team several decades ago. I believe this discovery is worthy of publication in the journal Nature; however, I have several comments that I would like addressed before I can recommend the manuscript for publication. If the authors can address these concerns, I would be happy to recommend the article for publication.

Major comments:

Ref 2: The authors note that the other team discarded the discovery as a background star owing to its colors and the planetary atmosphere models available at the time. The authors state that a modern analysis shows the colors are consistent with a planet (of 11 Jupiter masses). However, the authors do not provide more details concerning this discrepancy, and as a reader I'm left to wonder what specifically changed in the models since the previous analysis. I suspect many, if not all, readers in the field will ask this question. I suggest the authors elaborate on this discrepancy in the models in the supplemental information section.

Auth: The very red colours of many young directly imaged planets is thought to be related to them having very dusty conditions in their photospheric regions.

(As the next reply describes, we have removed the colour statement from the text).

Ref 2: By the way, I looked up the cited paper (Shatsky & Tokovonin 2002), and I could not find where it was stated that the object had colors inconsistent with a planet. Instead, I found that the object had a "O" and a "c" flag in the data tables. "O" means the object was determined to be an "optical (background) companion", rather than a "physical companion", but here is a quote from that paper: "All new components which are fainter than K = 12 or J = 13 are considered as optical." That seems like a conservative detection criteria, rather than anything to do with planetary models. Could these authors elaborate on this in their response to this referee report?

Auth: Fig. 6 in Shatsky & Tokovinin (2002) shows the motivation behind their classification criteria. They assess their point sources on the basis of whether they make sense in relation to the isochorones and empiricial data the authors had available. For J ~ 12 mag and fainter, the colours of the point sources start providing a poor match to the existing isochrones. If any of those candidates had matched well to the isochrones, it would presumably have been interpreted as a possible companion, but none of them do. Meanwhile, since we now know that any physically real companion would generally have been much redder than previously expected, we also know that such companions could not have been correctly identified when using the isochrones implemented in Shatsky & Tokovinin (2002). In any case, since the referee found this section of the text confusing, many readers probably would as well, and the diversion about isochrones is not really relevant to the present analysis. Hence, we have simplified the text and removed the reference to isochrones. We instead simply note that all targets fainter than J = 13 mag were discarded as a population in the 2002 study.

Ref 2: The authors spend a good deal of text describing how difficult it is to form a planet like b Cen (AB) b given it's distance from the host and low eccentricity. It reads as if this is unique to b Cen (AB) b, when if fact there are several known directly imaged planets which do not easily fit into existing planetary formation paradigms, most notably the HR 8799 planets. I ask that the authors place this discussion in the context of other directly imaged planets, and whether b Cen (AB) b is in fact unique with regard to its orbital distance and eccentricity.

Auth: We have now included this contextual discussion in the main text.

Ref 2: Furthermore, the discussion on the formation of b Cen (AB) b appears to completely ignore the binary nature of the host. It reads as if the authors are interpreting the binary host as a single high-mass host. But presumably the binary nature of the host would affect the orbital dynamics and evolution of the planet, but this is not discussed at all. We know that binary stars result in large disk gaps (see the literature on binary stars and transitional disks); how would this affect the interpretation? I would naively assume the capture scenario is different for a single vs binary host, given there is the opportunity to exchange angular momentum with the binary star. I ask that the authors consider the binary nature of the host in the discussion of planet formation scenarios. If it is reasonable to treat the binary as a single star of equivalent mass, then this needs to be better justified in the text, preferably in the main section.

Auth: We have added discussion regarding the binarity of the star and the relation to formation in the main text.

Minor comments:

Ref 2: The title is fine, but I wonder if it could be improved to better capture the uniqueness of the system. "A wide orbit giant planet in the 10 solar-mass b Centauri" does not mention the binary host, and even implies that the host is a single star, which is inaccurate. Also, technically, since "wide" modifies "orbit" and not "giant" or "planet" it should be hyphenated to "wide-orbit".

Auth: We have changed the title, it now reads: "A wide-orbit giant planet in the high-mass b Centauri binary system"

Ref 2: Figure 2 and Extended Data Figures 3 and 4: These are confusing figures. It is difficult to appreciate the plot in the frame of the host star, and I have trouble thinking about the planet as if it were a background star ("if bkg"). Perhaps the frame of the sky would be better, showing the host motion, the expected planet motion (including orbit), and the measured position of the planets? It would also be helpful to have inserts (small plots inside the large plot) zooming in on the points and showing their full error bars.

Auth: We have produced a figure that follows all of these suggestions, except for that the reference frame is still fixed on b Cen itself instead of the on the field. The primary reason for keeping the reference frame like this is that the orbital fits look more intuitive in a star-centered reference frame than in one fixed on the field. Since this figure contains all of the astrometric information, it replaces the previous figures showing the astrometry for the individual point sources around b Cen.

Ref 2: Figure 3: It is difficult to find b Cen (AB) b on the plot. Could it be plotted with more relief? Or maybe fade all of the other points a bit.

Auth: We have re-plotted the figure to make the b Cen (AB)b data point more visible.

Ref 2: Extended Data Figure 6 is useful for distinguishing this planet from other directly imaged planets. Perhaps this figure would be more appropriate in the main section rather than the extended section.

Auth: We have moved the figure to the main section, as suggested (its updated label is "Figure 2"). At the same time we moved the previous Figs 2 and 3 into the Methods section, since they are more technical and directed at expert readers, while the new Figure 2 is more important for setting the context to both expert and non-expert readers.

Ref 2: I found Extended Data Figure 5 unnecessary, as it is virtually identical to simply stating the mass determination and 1-sigma Gaussian errors as text. Instead, I would much rather see a plot capturing the various parameters that go into the mass determination and their various degeneracies. Perhaps a contour plot showing initial entropy versus planet color with mass contours and likelihood contours? I realize that is complex, but it is appropriate for a journal like Nature.

Auth: The planet colours are highly model-dependent, so they are not used in the analysis (bolometric luminosity is used instead) and we prefer not to plot them in this context. However, we have replaced the old figure with a new one that is more along the lines suggested by the referee: The new figure shows mass versus initial entropy, along with some upper and lower limits implied by population synthesis studies.

Referee #3 (Remarks to the Author):

This paper presents detection and characterization (photometric and astrometric) of a planetary mass companion to the tight, high-mass binary system b Centauri AB.

The authors make a strong case that this directly imaged planet is superlative in several ways that make it interesting from a planet formation theory perspective. Perhaps the most compelling is its presence around a B star binary, but it's extreme separation and mass ratio will also be of interest to the community.

The data are unequivocal in my opinion as to the bound and planetary nature of the companion, and the methodology and conclusions appear quite robust. The paper is very well-written and organized in general. I would categorize all of my suggestions below as clarifying and relatively minor in nature, and I see no barriers to quick publication of this work.

General Comments:

There are a few bits of context that I think should appear earlier and perhaps in more detail in the paper:

Ref 3: 1. There is some good broad context about wide-orbit companions and the frequency of single and binary planet hosts at the very end of the paper (in the "Formation Analysis" section) that I think should appear much earlier, certainly in the main body of the paper. I'd also suggest fleshing this section out with more specific comparisons to previous directly imaged and otherwise detected (RV) planetary mass companions. I wondered about HD106906 and the frequency of directly imaged planets around binary host stars, in particular.

Auth: We have added a discussion of b Cen (AB)b in the context of other exoplanets such as HD 106906 in the main text.

Ref 3: 2. More generally speaking, I think the significance of the fact that this is a tight binary system can/should be discussed more directly somewhere near the beginning of the paper. The possible range of separations for the binary pair is discussed in angular separation units, but I think a range of potential physical separations and a discussion of the possible influence (or, more likely I believe, lack of influence) of the binary pair on the evolution of the system, would be beneficial.

Auth: This discussion is now included.

Ref 3: 3. I think there is a bit of missing context about the observed and/or predicted radial extent of disks around very massive stars. Is there an expectation that there is enough material at 550au and/or that conditions are likely to be favorable at this location for planet formation via any mechanism?

Auth: We now include a discussion of disk sizes, referencing Beltran & de Wit (2016), which lists observed disk sizes in the hundreds to thousands of au, encompassing the orbit of b Cen (AB)b.

Ref 3: 4. [minor, but] I think the angular and orbital separation of the planet, as well as the distance to the system, should appear earlier in the paper.

Auth: As mentioned in the next reply, we now include the separation of the planet in the abstract. We avoid angular separations in the main text, but we have now included this value for b Cen (AB)b early on in the Methods section.

Ref 3: Abstract:

Line 25: how wide is wide? This is a good place to provide some of the numbers, as suggested in #4, above.

Auth: We have included the actual separation, formulated in terms of the Sun-Earth distance (rather than with the explicit term “astronomical units”), since the abstract needs to be written with a non-expert audience in mind.

Ref 3: Introduction:

PDF page 3, line 25 the statement that the colors of the companion firmly establish it as an exoplanet seems a bit of a stretch. Is its location in the CMD not fairly consistent with field BDs?

Auth: Yes; the statement about establishing the candidate as an exoplanet was intended to refer to the whole section preceding it and not the CMD in isolation, but we now recognize that its placement can easily lead to the interpretation that it only refers to the preceding sentence. We have clarified the sentence to account for this.

Ref 3: Line 31 -33 It might be helpful to state explicitly somewhere in here that the lower mass star does not have a confirmed spectral type. It took me a while to understand why it doesn’t.

Auth: The corresponding paragraph has been re-written, and we have clarified the uncertain properties of the lower-mass star.

Ref 3: PDF Page 4, line 15 the reasoning for the system age of 15+/- 2 Myr is described in detail in the extended methods section, but I think it should be stated at least briefly here that this is based on an extremely high membership probability for the UCL moving group

Auth: We have now included such a statement.

Ref 3: Lines 19-21 □ I think separate comparisons to the highest mass single stellar host and the highest combined binary mass host would be more helpful here than just the “three times higher” reference.

Auth: We have now included this information.

Ref 3: PDF Page 5, line 15 □ “orbit fitting code”. I’m not sure what the convention is for the body of a Nature paper, but I wondered immediately here what kind of orbit fitting code (based on later descriptions, I infer a combination of rejection sampling and MCMC).

Auth: We have added a reference to the Methods section to clarify that this is mentioned elsewhere in the manuscript.

Ref 3: Line 19-20 □ This range of eccentricities extends pretty high, in my opinion (to 0.41), suggesting that “near-circular” is a bit of a stretch

Auth: We have removed the “near-circular” interpretative statement, and now only state the eccentricity limit itself.

Ref 3: Line 30-31 □ A description of why this mass range has not been previously probed with direct imaging, as well as a description of how thoroughly it has been explored with RV methods, would be quite beneficial context here

Auth: We have added such a description to the manuscript.

Ref 3: PDF Page 7, lines 16-18 □ It’s unclear why membership in UCL implies that it was never part of a dense stellar environment. I assume that it is a timescale argument based on the young age of UCL, but I think this should be clarified.

Auth: Yes, this has now been clarified in the text.

Ref 3: Lines 28-30 □ This is a place where some more specific RV statistics would be beneficial. The statement that the RV distribution implies NO planet hosts above 3 solar masses is a bit different from the framing elsewhere in the paper that the frequency distribution is dropping off steeply at high masses. This is admittedly nit-picky, but 0 is different from very low. I also believe it needs a qualifier. Aren’t the RV planet frequency rules usually explicitly for planets within some number of au (10?)? I think this is what is meant by “cannot be extrapolated to the full system architecture”, but I think that it needs a qualifier earlier (e.g. “upper stellar limit for hosting giant exoplanets within 10au”)

Auth: We have now clarified that the quoted upper limit is statistical in nature, to avoid giving the impression of a strict limit above which the frequency is mathematically zero. We have also added a semi-major axis qualifier to the statement (~5 au, which is the largest separation at which planets around giant stars has been reported in existing studies), as requested.

Ref 3: Methods page 4, lines 3-4 □ I don’t follow the link between the stated axis ratio limit of 0.88 and 16mas (which is not a ratio).

Auth: We have removed the corresponding paragraph, since it only makes sense if the 2010 interferometric epoch is fully relied upon.

Ref 3: Lines 29-30 I believe the reference should be to orbitize, with OFTI as a module within that code

Auth: We have updated the reference accordingly.

Ref 3: Methods Page 5, Lines 1-27 I had a great deal of difficulty following this section and would suggest restructuring and clarification throughout. It's basically the only section of the paper that wasn't clear and well-structured in my opinion, and as a result, I couldn't quite decide whether I should buy the error bars on position.

Auth: We fully agree that the mentioned paragraph was overly complicated, particularly in light of the fact that the small-scale astrometric binary motion mentioned toward the end of the paragraph was entirely self-sufficient to make the central argument. As a result, we have greatly shortened and simplified the whole paragraph to account for this fact.

Ref 3: Methods Page 6 , lines 4-5 "since it is partially calibration limited". What is it? Limited by what calibrations?

Auth: The sentence was intended to communicate that the photometric precision is not only affected by the photon count uncertainty of the planet, but also by the photometric uncertainty of the primary star, which is used as the photometric reference point. We have clarified this in the text.

Ref 3: Line 11 If you are adopting the full range of uncertainties in the age and initial entropy then shouldn't you adopt the full range of masses and luminosities as the uncertainty rather than the standard deviation? How much does this change the results?

Auth: Our idea is to acquire a representative error bar estimate (the text has been slightly reformulated to explain this more clearly). The brightness is measured in four different bands – if, for example, one band suggests a lower bolometric luminosity than the other three, then adopting the bolometric luminosity that corresponds to the fainter data point alone would arguably put undue weight on an isolated edge case. We consider that taking the standard deviation in the landscape of fitted bolometric luminosities provides the most relevant estimator for the uncertainty as used in a classical sense. The full span of bolometric luminosities as fitted to individual photometric bands ranges from log Lbol of -4.4 to -3.7.

Ref 3: Methods Page 7, Lines 2-3 " from a mass ratio from …. 1000au". This phrasing is hard to follow.

Auth: We have broken up the sentence into a structure that is easier to follow.

Ref 3: Lines 13-14 Though I appreciate the difference in them, both probabilities are quite low. It could perhaps be emphasized here that these systems are quite rare, perhaps with some statistics from the BEAST sample.

Auth: This has now been clarified in the text.

Ref 3: Figure 3:

I'd suggest adding labels with names for some of the other planetary mass objects that you might compare this system to here, especially HD 106906 (also widely separated) and 51 Eri (similar mass ratio). Maybe also other UCL companions?

Auth: 51 Eri does not have reliably measured colours in the relevant bands, but we have included labels for HD 106906 and other relevant Sco-Cen companions in the figure.

Ref 3: Extended data figure 2:

I'd find it helpful to see a range of orbital fits (I believe orbitize will do this for you) to the astrometry in addition to the posteriors

Auth: We have included a range of orbital fits in Extended Data Figure 2, which contains all of the relevant astrometric data for the system (a new figure based on another referee comment).

Ref 3: Extended data figure 6: I'd suggest putting this figure in the main body of the paper. I think it's particularly useful for a general audience.

Auth: We have moved the figure to the main body of the paper, where it is now labelled "Figure 2".