Peer Review File

Manuscript Title: CO gas produced by a giant impact in the inner region of a young system

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referees' comments:

Referee #1 (Remarks to the Author):

This paper by T. Schneiderman and co-authors reports a detection of CO around HD 172555. HD 172555 is a member of the ~20Myr old Beta Pictoris Moving Group hence a well determined age, and it was claimed previously as a recent site of planetary scale collisions as evidenced by unusual amount/composition/size of dust grains in orbit around the star. The newly reported CO detection further corroborates the claim of a recent planetary collision around this star which mimics the last stage of terrestrial planets' evolution in our Solar System. A theory of planet formation and evolution predicts that the last stage of terrestrial planet formation is through violent collisions. This paper not only presents the detection of gas which was likely released from the atmosphere of collided planets, but also compares clearly possible scenarios of unusual presence of dust and gas. It was demonstrated clearly that the only viable scenario is the collisional release of gas and dust from planetary size bodies. This result is important and has immediate influence to many people in astronomy and planetary science communities and it is well justifiable to be published in Nature. Thorough understanding of how our Solar System was formed and evolved is the utmost importance in astronomy, and the result from this paper firmly establishes HD172555 as the first, unambiguous star displaying a planet-planet collision outside of our Solar System. Therefore, I recommend a fast acceptance and publication of this article in Nature.

The paper is well written and organized, and all technical analyses are sound. Other than some minor remarks shown below, I do not have any major comments.

* at line 143: Authors can add the retrograde rotation of Venus as one more evidence of giant impacts in the inner planetary region.

* In the ALMA observation section, the expression of "flagging antennas" seems to be too colloquial. If it was meant to be "removing data from a certain subset of antennas", then writes it so instead of describing as "flagging antennas".

* At lines around 256. The best-fit stellar model has Teff=7840+-30 K in the paper. The 30K uncertainty is likely the resultant uncertainty from the MultiNest method. However, this uncertainty seems to be too small. When this referee performed an SED fitting of HD 172555 using the same input stellar spectral models (e.g., PHEONIX), the obtained best-fit Teff was 7590K with an uncertainty of 50K. Using a different stellar model (e.g., Castelli and Kurucz 2004), Teff can be even lower (7520K). The difference in Teff of about 200-300K is unlikely affect the result of the paper, but. for the claimed small uncertainty of Teff in the paper, it is worthwhile to elaborate on the effect of different Teffs (hence the luminosity).

* Throughout the paper, authors use "exocomets" when describing comets originating from an outer, cold disk of HD 172555 resembling the Kuiper-Belt in our Solar System. This choice of word can be potentially confusing readers because the prefix "exo-" can wrongly imply the origin of such comets as external to the star (I.e., comets entering from interstellar space). I suggest replacing all appearances of "exocomets" with "comets" to remove any potential misunderstanding.

Referee #2 (Remarks to the Author):

This paper presents archival data from ALMA utilized to detect an unresolved CO debris ring co-orbiting with a dust disc around HD 172555 at radial separations from 6-9 AU, based on modeling. The authors discuss various scenarios to explain the origin of the CO disk, asserting that the data favor an interpretation of a giant impact between 2 bodies of masses ~ 8M_Earth each at least 0.2 Myr ago, in which one impactor fuels the debris and the other survives. The key to discriminating between the scenarios is the spectral features of CO.

The work presented is original and certainly of interest and significance to the debris disc community. The use of the CO data to constrain recent giant impacts in this young disc as well as others certainly merits publication. The work is thorough and quite concise, with only a few areas which I noted would benefit from some increased detail.

Comments on the manuscript

In Figure 1, is there any comment to be made regarding the second point source adjacent to the continuum peak? 2sigma may or may not be significant, but no comment is made.

While the chi-squared values shown in Figure 2 clearly favour the warmer, lower opacity CO emission, the fits to the spectral profile seem to both leave room for improvement. Can the authors comment on the requirements to better match the spectrum?

Line 165: "... timescale, require the release of an amount of CO2 corresponding to just 9-23% of the total present in the Venusian atmosphere"

Whilst this sounds like a relatively small amount, it is not clear to me how that compares to the amount expected in a debris field in the scenario outlined in the paper. How much of the atmosphere of an 8MEarth planet would be in place at 23 Myr?

Line 178: “This highlights the importance of gas release in post-impact dynamics, and opens the possibility of using gas..”

This sentence (coming right at the end) is somewhat awkwardly worded. I suggest something like: “Furthermore, this discovery reveals the importance of gas release in post-impact dynamics and highlights the potential of using gas….”

Methods.

ALMA observations.
I would recommend giving the distance explicitly, rather than stating “at the known distance of the system”, so the reader doesn’t have to hunt backward.

Millimetre dust mass.
The subtraction of stellar mm emission based on extrapolated photospheric profiles has been noted to be problematic in various ALMA observations. Thus, that extrapolated mass could be viewed as a minimum expected stellar contribution to the mm mass. This should be acknowledged and referenced. HD 172555 hasn’t been targeted for mm stellar emission specifically, so a measured value isn’t available. What are the implications to your analysis if the millimetre dust

mass present is lower than your calculated value?

Optically thin CO ring modeling.
Line 229: "... the unitless spectrum by a factor linked to the integrated flux of the line, which we leave as a free parameter in the fit"

"linked to" seems a bit non-specific. Is it a constraint or a derived value?

Delivery from an outer belt.
Line 292; "the latter leaves". This sentence only discusses gas drag, so the comment about "the latter" is not needed.

High eccentricity comet population.
The assumption of a 100 au planetesimal belt clearly results in CO emission too far from what is observed, but looking at the dispersion in location of planetesimal belts around A stars in the Matra study, it would seem that many are larger, but some are smaller by 10%. This doesn't seem enough to bring the perihelions in close enough to reconcile with the data. Most of the belts however are not narrow; they are in some cases wider than the radial separation reported by Matra et al. Therefore, it would seem plausible that some fraction of bodies could reach the 7.5 au location of the CO disk, though this would likely make the CO ring too wide. Is this the case? The 100 au assumption here should be clarified in terms of dispersion and width of the rings for the A stars of Matra et al.

Line 312: "Therefore, these larger radii make an eccentric exocomet scenario inconsistent with the available observations."

"these larger radii" could be clarified with "The fact that the expected radius of a CO ring would significantly exceed the observed value makes the eccentric… "

In situ giant impact constraints.
Are the timescales for symmetrisation the same for dust and CO?
It is apparent that it is really the dust that is setting this timescale since elsewhere the authors state that the dust requires the time since the giant impact to be at least 0.2 Myr.

Constraints from debris survival.

Line 369: "... we first note that for a leftover planet with mass below ~1.8 M, the predominant (eventual) outcome of an encounter will be accretion, whereas for masses above that threshold, the predominant (eventual) outcome will be ejection"

This statement is not referenced and no details as to how this value was derived are given. Similarly, below the statements about the re-accretion and ejection timescales are also not sufficiently supported.

The authors should check for noun-verb agreement for "data" throughout.

Author Rebuttals to Initial Comments:

Response to referee #1

1. Referee comment: at line 143: Authors can add the retrograde rotation of Venus as one more evidence of giant impacts in the inner planetary region.

Response: The suggested change has been incorporated.

2. Referee comment: In the ALMA observation section, the expression of “flagging antennas” seems to be too colloquial. If it was meant to be “removing data from a certain subset of antennas”, then writes it so instead of describing as “flagging antennas”.
Response: The expression ‘flagging antennas’ is the standard language in radio calibration. It does indeed mean ‘removing data from a subset of antennas’. We recognize the concern the referee has over the colloquialism of the phrase, so have modified the text to read as follows: *For both the continuum and CO line emission, we removed data from antennas 7, 19, and 25. These data were taken in a hybrid configuration, with the three flagged antennas far from the compact group of antennas; removing these data significantly improves the imaging*

3. Referee comment: At lines around 256. The best-fit stellar model has Teff=7840+-30 K in the paper. The 30K uncertainty is likely the resultant uncertainty from the MultiNest method. However, this uncertainty seems to be too small. When this referee performed an SED fitting of HD 172555 using the same input stellar spectral models (e.g., PHEONIX), the obtained best-fit Teff was 7590K with an uncertainty of 50K. Using a different stellar model (e.g., Castelli and Kurucz 2004), Teff can be even lower (7520K). The difference in Teff of about 200-300K is unlikely affect the result of the paper, but. for the claimed small uncertainty of Teff in the paper, it is worthwhile to elaborate on the effect of different Teffs (hence the luminosity).
Response: T_{eff} does not matter so much as the luminosity. The luminosity distribution is derived from integrating the fitted models. The area under the photometry that is integrated to get the luminosity does not change significantly for the T_{eff} that is fit.
With that said, the value difference is likely the result of different fitting codes, assumptions, zero points, and photometric points used.

4. Referee comment: Throughout the paper, authors use “exocomets” when describing comets originating from an outer, cold disk of HD 172555 resembling the Kuiper-Belt in our Solar System. This choice of word can be potentially confusing readers because the prefix “exo-” can wrongly imply the origin of such comets as external to the star (I.e., comets entering from interstellar space). I suggest replacing all appearances of “exocomets” with “comets” to remove any potential misunderstanding
Response: Exocomets are commonly interpreted as small bodies releasing gas and/or dust in other planetary systems, and now defined as such in a recent review of the subject (Strom et al. 2020) which spurred from a meeting between the Solar System comet and exocomet community. The prefix ‘exo’ is also commonly applied in astronomy to bodies outside of our Solar System, in the same way as the word ‘exoplanet’. What the reviewer has in mind would usually be termed an interstellar object, such as 1I/‘Oumuamua and 2I/Borisov, rather than an exocomet. However, we understand where the referee is coming from, so we clarified that the exocomets originate from within the system by drawing the analogy with the Kuiper belt - in the text location where the word is first mentioned.

Response to referee #2

1. Referee comment: In Figure 1, is there any comment to be made regarding the second point source adjacent to the continuum peak? 2sigma may or may not be significant, but no comment is made.
   Response: The 2σ peaks are background noise and not significant. This commentary has been added to the figure caption.

2. Referee comment: While the chi-squared values shown in Figure 2 clearly favour the warmer, lower opacity CO emission, the fits to the spectral profile seem to both leave room for improvement. Can the authors comment on the requirements to better match the spectrum?
   Response: The difference between the best fit data and the model is within the current noise level. To better match the spectrum, more sensitive ALMA data is needed to reduce the noise level. A definitive answer to the mass-temperature degeneracy can be found through taking observations of multiple CO transitions, as the ratio of emission can be used to definitively pin down the gas temperature.

3. Referee comment: Line 165: "... timescale, require the release of an amount of CO2 corresponding to just 9-23% of the total present in the Venusian atmosphere"
   Whilst this sounds like a relatively small amount, it is not clear to me how that compares to the amount expected in a debris field in the scenario outlined in the paper. How much of the atmosphere of an 8MEarth planet would be in place at 23 Myr?
   Response: We want to avoid speculation about the nature of the atmosphere in place, given the current data quality. We simply note that the CO_2/CO budget is consistent with today's composition of Venus, and indeed with the composition of the Earth, when including carbonate rocks.

4. Referee comment: Line 178: "This highlights the importance of gas release in post-impact dynamics, and opens the possibility of using gas.."
   This sentence (coming right at the end) is somewhat awkwardly worded. I suggest something like: "Furthermore, this discovery reveals the importance of gas release in post-impact dynamics and highlights the potential of using gas…."
   Response: The suggested change has been incorporated

5. Referee comment: ALMA observations.
   I would recommend giving the distance explicitly, rather than stating "at the known distance of the system", so the reader doesn't have to hunt backward.
   Response: The suggested change has been incorporated

6. Referee comment: Millimetre dust mass.
   The subtraction of stellar mm emission based on extrapolated photospheric profiles has been noted to be problematic in various ALMA observations. Thus, that extrapolated mass could be viewed as a minimum expected stellar contribution to the mm mass. This should be acknowledged and referenced. HD 172555 hasn't been targeted for mm stellar emission specifically, so a measured value isn't available. What are the implications to your analysis if the millimetre dust mass present is lower than your calculated value?

Response: This is a very good point. Actually, we had an error in the text and our mm stellar flux originates from the same PHOENIX model which fitted the optical/IR photometry of the star. PHOENIX models have been shown to be a good fit to mm photometry of Sirius A (White et al. 2019), which is reasonably similar in spectral type to HD172555 (A1 vs A7). Our PHOENIX model produces a 230 GHz flux that matches the one predicted by Eq. 2 of White et al. 2019, where this equation is also based on PHOENIX models. Therefore, we feel that we have used what is a very reasonable estimate of the stellar flux; we finally note that the low SNR of the continuum emission reflects in the quoted large uncertainty on the millimetre dust mass; this uncertainty is likely large enough to encompass comparably small variations in the stellar mm emission

7. Referee comment: Optically thin CO ring modeling.
Line 229: "... the unitless spectrum by a factor linked to the integrated flux of the line, which we leave as a free parameter in the fit"
"linked to" seems a bit non-specific. Is it a constraint or a derived value?
Response: The unitless spectrum is renormalized so that the integral of the spectrum is equal to the integrated flux – a free parameter in the fit. This has been clarified in the text.

8. Referee comment: Delivery from an outer belt.
Line 292; "the latter leaves". This sentence only discusses gas drag, so the comment about "the latter" is not needed.
Response: The suggested change has been incorporated.

9. Referee comment: High eccentricity comet population.
The assumption of a 100 au planetesimal belt clearly results in CO emission too far from what is observed, but looking at the dispersion in location of planetesimal belts around A stars in the Matra study, it would seem that many are larger, but some are smaller by 10%. This doesn't seem enough to bring the perihelions in close enough to reconcile with the data. Most of the belts however are not narrow; they are in some cases wider than the radial separation reported by Matra et al. Therefore, it would seem plausible that some fraction of bodies could reach the 7.5 au location of the CO disk, though this would likely make the CO ring too wide. Is this the case? The 100 au assumption here should be clarified in terms of dispersion and width of the rings for the A stars of Matra et al.
Response: As long as it is at tens of au, where we would expect a cold belt to reside, changing the apocenter of the comet population does not significantly alter our conclusion. For example, changing the apocenter of the inward scattered comets (and therefore belt location) from 100 to as low as 20 au means that the pericentre distance for a pericenter velocity of 14.5 km/s is 10 au instead of 13.2 au. If the inner edge of the CO ring were at 10 au, with most CO originating at or beyond this radius, we would still have at least marginally resolved it given what would be a minimum CO disk diameter of 20 au, (or 0.7") comparable to the resolution of our observations (1.16" x 0.75"). As the referee points out, in this scenario we would expect CO to be released much beyond ~10-13 au as well, which again is not observed. We have modified the text to reflect this.

10. Referee comment: Line 312: "Therefore, these larger radii make an eccentric exocomet scenario inconsistent with the available observations."
"these larger radii" could be clarified with "The fact that the expected radius of a CO ring would significantly exceed the observed value makes the eccentric… "

Response: The suggested change has been incorporated

11. Referee comment: In situ giant impact constraints.
    Are the timescales for symmetrisation the same for dust and CO?
    It is apparent that it is really the dust that is setting this timescale since elsewhere the authors state that the dust requires the time since the giant impact to be at least 0.2 Myr.
    Response: The timescale for CO is unclear because the photodissociation is unknown, and symmetrization would be dependent on this.

12. Referee comment: Constraints from debris survival.
    Line 369: "... we first note that for a leftover planet with mass below ~1.8 M, the predominant (eventual) outcome of an encounter will be accretion, whereas for masses above that threshold, the predominant (eventual) outcome will be ejection"
    This statement is not referenced and no details as to how this value was derived are given. Similarly, below the statements about the re-accretion and ejection timescales are also not sufficiently supported.
    Response: The citation provided in line 375 – Wyatt et al. 2017 is the relevant reference.

13. Referee comment: The authors should check for noun-verb agreement for "data" throughout.
    Response: The suggested change has been incorporated