Peer Review File

Manuscript Title: Atmospheric molecular blobs shape up circumstellar envelopes of AGB stars

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referees' comments:

Referee #1 (Remarks to the Author):

The manuscript presents high angular resolution millimeter imaging of the well-studied carbon-rich AGB star IRC+10216 with ALMA. Thanks to the proximity of the target and the high angular resolution of ALMA, the data show inhomogeneous, clumpy structures in the innermost circumstellar envelope. Observational studies of where and how dust forms are crucial for clarifying the mass-loss mechanism in AGB stars. The result presented in this manuscript that dust formation and the atmospheric structure are intrinsically clumpy is therefore important for the solution of this long-standing problem. Furthermore, the images obtained in different molecular lines reveal distinct morphology, which the authors interpret in terms of inhomogeneities in density and temperature. These data are valuable for further understanding chemistry in the atmosphere shocked by pulsation and/or convection.

However, although the authors claim that their results are the first to spatially resolve anisotropic formation of dust and molecule in the atmosphere of AGB stars, non-spherical structures in the atmosphere and dust formation very close to the star have been detected in the recent years. For example, visible polarimetric imaging has revealed clumpy dust clouds very close to the star, within 2 stellar radii, with spatial resolutions comparable to or better than the present paper (Khouri et al. 2016, A&A, 591,A70; 2020, A&A, 635, A200; Ohnaka et al. 2016, A&A, 589, A91; 2017, A&A, 597, A20). Although the authors cite one of the papers, Ohnaka et al. (2016), as "non-spherically symmetric structures are commonly observed further out in the circumstellar envelope", the above papers detected clumpy dust clouds within ~2 stellar radii, slightly closer to the star than probed in the present study. As for the gaseous component, Takigawa et al. (2017, Sci. Adv., 3, eaao2149), Vlemmings et al. (2017, Nature Astronomy, 1, 848), and Khouri et al. (2018, A&A, 620, A75) probe the inhomogeneous molecular gas within several stellar radii around AGB stars with ALMA using different molecular lines. IR interferometric imaging has also revealed inhomogeneous structures in the dust-forming atmosphere of AGB stars with several milliarcsecond resolution (Wittkowski et al. 2017, A&A, 601, A3; Paladini et al. 2018, Nature, 553, 310; Ohnaka et al. 2019, ApJ, 883, 89; Chiavassa et al. 2022, A&A, 658, A185) as well as in red supergiants (Montarges et al. 2021 as cited in the paper and Ohnaka et al. 2017, Nature, 548, 310; Climent et al. 2020, A&A, 635, A160; Norris et al. 2021, ApJ, 919, 124).

Therefore, while the presented observations for this particularly well-studied carbon-rich AGB star are definitely valuable, I do not think that the significance of the paper is sufficiently high for publication in Nature. I agree in the conclusion that not only binary companions but also the anisotropy found in the present work leads to structures on larger scales. However, such anisotropy was already shown in the previous works.

The authors report the data and the analysis in a sufficiently detailed and transparent manner, and I found no apparent flaw. The error bars are appropriately described in each figure. However, there are some points that need clarification as reported below.

* Results, p5
The continuum emission was fitted with a Gaussian, but the fitting procedure is not entirely clear. According to the text, it appears that the fit was done to the image, not to the uv data, and the authors fitted to the "continuum image deconvolved from the beam". It is not clear whether the deconvolution means the usual CLEAN process or something else such as deconvolution of the CLEANed image using the CLEAN beam. This should be more clearly described.

* Figure 2
The horseshoe is seen at ~25 mas from the Gaussian centre in the residual map. This means horseshoe-shaped extra emission in front of the star. I think that it is worth discussion, because it may be caused by hot dust clouds or it may be similar to the hot spot found by Vlemmings et al. (2017) in the 338 GHz continuum toward the oxygen-rich AGB star W Hya.

The beam size is 27x19 mas (FWHP) according to the text. However, the beam shown in Fig. 2 is 40--50 mas (major axis), which should be clarified.

* Discussion, p12
The authors try to explain HCN/SiS with a chemical equilibrium model. However, in Supplementary discussion: shocks and outflows, they use a non-equilibrium chemistry model for shocked atmospheres. Therefore, the adoption of the chemical equilibrium in the interpretation of HCN/SiS needs justification and some discussion about possible effects of non-equilibrium chemistry, which I think would be easy, because they have such models.

* Supplementary discussion: continuum extended emission
As the authors correctly point out, the velocity information obtained from the molecular line maps may teach us the wind acceleration and the 3-D position of the clumps. However, the authors do not discuss the dynamical structures seen in the maps. It would be very valuable to include discussion on the dynamics seen in the different molecular lines, even if it is qualitative.

The authors describe the clumps discovered in the 2 micron speckle imaging. However, subsequent observations show clear time evolution of those clumps (Weigelt et al. 2002, A&A, 392, 131).

* Figure 9 shows a number of miniature spectra, but the conclusion that they present from this figure is that absorption is observed in front of the star, and emission is seen beyond the radio photosphere. But this is already seen in Fig. 8. I think that it is more reasonable to show the HCN data in the same way as Fig. 8.

* The authors mention UV radiation enhancement in the SW. However, it is not clear whether the interstellar UV photons penetrate down so close to the star through the optically thick circumstellar

dust envelope even if it is clumpy. If the UV radiation does not reach the region probed by the present study, it would be irrelevant for explaining the anisotropy anyway.

* Figure 7
It would be necessary to explain whether the line profiles shown in the figure correspond to the flux integrated over the entire model image extending (at least) to 10 stellar radii (as shown in the top panels) or perhaps they are the spectra computed at the stellar disk centre and convolved with the 0.02" resolution. The model spectra show narrow features both in absorption and in emission. Explanation of the reason the shock gives rise to narrow features both in absorption and emission would be necessary. For this, it is probably helpful to plot the velocity structure as a function of radius. Then it is possible to see where the sharp features at specific velocities originate.

The signature of shocks is not seen in the observed data. Does this mean that the shocks are much weaker than predicted by the model? More discussion about this point would be valuable because shocks play an important role in dust formation.

* An unidentified line is reported, and the channel maps are shown. Given the similarity to the images of SiC2, I wonder if they can discuss its possible origin, even if it is not definitive.

Referee #2 (Remarks to the Author):

I have carefully read the manuscript "The uncharted atmosphere of an evolved AGB star: new challenges for circumstellar models." The authors present new observational evidence for the presence of small-scale spatial inhomogeneities inside the atmosphere of evolved star IRC+10 216, suggesting that such inhomogeneities are present at the initial stages of dust formation and thus that the formation of dust grains and molecules in AGB stars is not an isotropic process.
This finding calls into question the claim that asymmetrical structures in the envelopes of AGB stars are fully explained by the presence of a stellar companion. It also calls into question the validity of several modeling assumptions, including those of (1) isotropically expanding stellar winds; (2) isotropic physical conditions; and (3) isotropically distributed fractional molecular abundances.
The authors argue that these inhomogeneities, detected in spatially resolved observations of molecular emission in the deep atmosphere, disrupt isotropic stellar winds. In combination with other known physical processes (such as interaction with a companion), these small-scale anisotropies produce the morphologies observed in AGBs and circumstellar envelopes.

I think the findings themselves are worthy of publication in Nature, but understanding the novelty and scientific importance of these results based on the manuscript, as currently written, took a lot of effort for me, even as an expert in a closely related field. Presenting arguments in a way that is exciting and accessible to a general audience is part of publishing in Nature (per their stated criteria). With this in mind, I believe the authors should devote some time to extricating the punchline from the technical details of the observations (most of which could be moved to Methods) in the main body of the text. I also think that the authors do not spend enough text debunking alternative hypotheses for their observations or explaining the significance of their discovery.

I provide general and specific comments below.

General comments:
The title includes the phrase “challenges for circumstellar models,” but a focus on models does not seem appropriate. These findings disrupt our current understanding of dust grain formation and the origin of complex circumstellar morphologies; this is the significant result, in my view. Further, computational practicalities may very well prevent modelers from incorporating deep atmospheric anisotropies anyway. Not knowing about such anisotropies (until now) is not necessarily, or even likely to be, the limiting factor in achieving high physical fidelity with simulations of evolved stellar atmospheres. Our theoretical and numerical treatments of fundamental physical processes, like radiative transfer and convection, in these regimes are approximate, and just because the standard modeling prescriptions are not (completely) physical does not mean it is worthwhile or even possible to change the physical assumptions in the models. The results are significant without tying them to modeling inadequacies (of which there are many), and I think they should be divorced from this.

Why do the stellar winds have to be isotropic? Is this not also an assumption of convenience?

Have you actually run simulations that confirm the connection between small-scale anisotropies and the development of large-scale arcs or clumps in the AGB atmosphere or post-AGB circumstellar envelope?

Specific comments:
Page 2:
“It is inside…of AGB stars that is initiated…” awkward phrasing

Page 3:
“Successfully resolve for the molecular…” ungrammatical

“...usually adopted by models” Which models?

Results, Page 5:
Why one fifth of the synthesized beam? Are the contributions from these two sources of uncertainty (Gaussian fit and fifth of the beam) equivalent? Are uncertainties due to instrumental systematics folded into “uncertainty of one fifth of the beam”?

Page 6:
It is very interesting that the identified components could be dusty gas clumps expelled by IRC+10 216. In Discussion, the authors briefly touch on the idea that they may be probing a mechanism related to the production of the dust cloud responsible for the “Great Dimming” of Betelgeuse in 2019–2020. I think this merits greater attention and a more prominent location in the text.

The entire concept of a “radius” for such stars is questionable. As the authors note, the definition of the radius is wavelength-dependent. Would a different measurement for the size of the radio and/or

IR photosphere change the results? Has this been explored?

Page 8:
"This has led to suggest that…" → this has lead to suggestions that

Discussion:
I am satisfied with the authors' arguments against alternative explanations for non-spherically symmetric emissions. However, we would do well to remember that finding no evidence to support an alternative hypothesis is not the same as ruling out that hypothesis definitively.

Page 12:
Again, does the presumed/measured radius affect your conclusions? This is important to address given that the novelty of the results is tied in part to the depth at which the atmosphere is being probed.

"Even a moderate variation of 10% in the temperature…can lead to orders of magnitude variations in the fractional abundances…"
This is an area of enormous uncertainty in simulations and notoriously poor treatment in stellar models.

Page 13:
"Therefore, phenomena such as convection cells at the surface of the star would produce variations in the temperature with angle…lead to anisotropies seen in the emissions…"
The complex interplay between convection and chemical stratification in the outer layers of evolved stars is well established. Is this the first time it has been observed directly (at this depth)?

Methods
I have investigated Agundez et al (2020) hoping for more information on the chemical model. Their code is not publicly available. Though "closed-source" software is, unfortunately, common in astronomy, a truly rigorous evaluation of the science is not possible without direct access to this tool. I do not think this should prevent publication, but it is something to keep in mind.

References
This manuscript does seem to cite in-group work preferentially.
Citations to several works by Andrea Chiavassa, Sofia Ramstedt, Matthias Maercker, and Michael J Ireland would be appropriate.

The following specific citations would also be appropriate:
Norris et al. 2012 (Nature)
Hofner & Freytag 2019 (A&A)
Joyce et al. 2020 (ApJ)
Harper et al. 2020 (ApJ)
Harper et al. 2021 (AJ)

Hofner & Freytag 2022 (arxiv)

Referee #3 (Remarks to the Author):

Summary of the key results

I reviewed the paper “The uncharted atmosphere of an evolved Asymptotic Giant Branch (AGB) star: new challenges for circumstellar models”. The manuscript presents new ALMA observations of the carbon-rich Mira star IRC+10216. The data are taken at 4 different spectral windows with spectral resolution around 0.550 km/s, the maximum angular resolution is about 20 mas, hence extremely high. The scientific target is very nearby and well-studied source, already object of another study published on Nature volume 467, pages 64–67 (2010) where water was detected for the first time. Despite its relative closeness, the presence of a binary companion in the atmosphere of IRC+10216 is argument of discussion in the community.
The ALMA data here presented, and interpreted in terms of chemical models, resolve spatially the molecular emission in the vicinity of the radio photosphere. Inhomogeneities are clearly observed, and the authors argue that such inhomogeneities cannot be explained only in terms of binaries or sub-stellar companions, as in their data there is no signal of the companion hunted by a large part of the community. Later in the text the argument of the companion comes back as explanation of the offset in proper motion. This should be clarified in the text.
The key conclusion of this work is that anisotropies are for the first-time observable in the mass-loss process, contradicting the “axiom” that mass-loss from evolved stars is spherically symmetric. I acknowledge that the data are spectacular and deserve a publication in a high-profile journal, however I have a doubt about the axiom of spherically symmetric mass-loss which is outdated and does not represent what the community believes since quite a while (see my comment below to the abstract). Moreover there is an consistency between the presence/not presence of the binary companion which should be solved.

Originality and significance: if not novel, please include reference
AGB stars such as IRC+10216 are relevant to various fields of Astrophysics because of their mass-loss enriching the interstellar medium of the various products of stellar nucleosynthesis. In literature it is often assumed that AGB stars lose mass via an isotropic wind. This statement is written by the authors in line 5 of the abstract, and it would be good to add a reference (see also comment in the section “suggested improvements”). In the last 10-15 years, instrument advancement and high angular resolution observations unveiled a different picture. Maercker et al. 2012 ALMA observations of RScl showed a very beautiful spiral inside the well-known (round) detached shell. During the years we gathered more and more evidence that in fact the environment of evolved stars is not as spherically symmetric as we thought, and indeed asymmetries show up. On the other hand, such asymmetries are usually explained as a signature of a companion of stellar or substellar origin. Decin 2020Sci...369.1497D observed a sample of AGB O-rich stars using ALMA. The spatial resolution of the data is less than the one presented here, in fact the spatial scales probed are between ~0.24′′and ~1′′. Maps of SiO and CO molecules show several morphologies such as spirals, and

bipolar outflows explained as the signature of binary companions interacting with the expanding outflow of the AGB. All the companions so far found around AGB stars are orbiting at a distance larger than 10 stellar radii. First generation of dynamic model atmosphere correctly cited by the authors (Winters, Höfner early works) are 1D, hence assume spherically symmetric / isotropic wind. Liljegren et al. A&A 619, A47 (2018) used 3D-star-in-a-box (CO5BOLD) models of convection as inner boundary for 1D -DARWIN winds. The authors show that anisotropic CO5BOLD central star affects the wind, however they do not present synthetic observations hence, it is not clear if the asymmetric structures predicted could be observed. From the observational point of view there have been works showing asymmetric structures not clearly linked to binaries for example Ohnaka et al. A&A 589, A91 (2016) reports clumpy dust clouds in the environment of WHya using VLT/SPHERE and VLTI/AMBER. They conclude that "the detection of the clumpy dust clouds close to the star lends support to the dust formation induced by pulsation and large convective cells as predicted by the 3D simulations for AGB stars." The work here presented shows clear evidence that not only the dust as Ohnaka mentioned in his work, but also the molecular emission is affected by the local asymmetries induced by pulsation and convection in the stellar vicinity. In fact, the work here presented goes beyond and shows that the chemistry is also different because of the different density of the medium. Van de Sande & Millar 2022MNRAS.510.1204V showed how chemistry can change if a stellar companion is present in the atmosphere, now we know that it is the case also for single stars. For all the arguments listed above, this work can be considered a breakthrough.

Data & methodology: validity of approach, quality of data, quality of presentation

The data acquired are very high angular resolution (~20mas) ALMA data mapping the molecular emission beyond the radio-photosphere up to 5 stellar radii. Images are reconstructed using various methods and shown for the continuum, HCN, SiS, and SiC2. The interpretation is mostly qualitative and based on the data. Anisotropies are clearly detected, and the authors conclude that they cannot be explained only with companions (not detected in their data), rather by the interplay of convection and pulsation changing the temperature and pressure of the environment causing the formation of different chemical species.
I think the scenario that the author presents are reliable, however I also think it would be hard to see directly in such data a planet or a sub-stellar companion. Do we have prediction of what would a planet or a stellar companion do if within the first 5-10 stellar radii? Can we for example exclude 100% that the broken arc (not the arc itself) seen in the data are not the effect of a tiny object which is too small from producing more "apocalyptic" scenarios (i.e., emission lines, uv-flux, and so on)? The author mentions chemical equilibrium models and radiative transfer described in Methods and supplementary material to support their conclusions; however, I find that such models are not sufficiently presented in the main text (I know the problem of limited space), they could play a more important role in convincing the reader about the conclusions of the authors. Probably a couple of sentences about the main results of the models in the main text could help making a stronger case for the "single star" scenario.

Suggested improvements: experiments, data for possible revision; references; clarity and context

Below I will provide some comments about suggested improvements.

• Abstract, line 5: “assumed to be isotropic”. As a scientist in this field, I interpret “AGB winds are isotropic” as something isotropic on a global scale, meaning without a preferential direction. This picture is described in the review by Hoefner and Olofsson 2018 (https://link.springer.com/content/pdf/10.1007/s00159-017-0106-5.pdf). The mass-loss as “locally” isotropic is a very old concept which nobody believes since already a few years. A single star emits material in clumps and patches, and the mass-loss is isotropic only if the ejection is averaged over a very long time. There are several proofs that the wind from single stars is not locally symmetric (see the review above), and this work is among one of the nicest proofs itself.
This line should be rephrased to mirror better the reality of the field.
• Page 2, last paragraph, references in line 3 (Hofner, Winters): again, in line with what I wrote above the 1D models are from the 90s. They are still used because they are simple to calculate, but nowadays it is known that the winds are not locally symmetric. Therefore, I would say that no one assumes that winds are symmetric, especially in the stellar vicinity, unless there are computational reasons for it.
• Page 3, “here we present the first observational evidence that… winds are not isotropic” This is an unfortunate wording because there are already several other examples such as Ohnaka 2016. It might be the first case for the ALMA data with respect to the series of works of Decin et al. and references therein.
• Page 6, beginning: there is a reference to Weigelt work. The paper of Weigelt presents evolution over 10 years of structures in the inner atmosphere of IRC+10216. I wonder if any of the structures here presented/detected could relate to the ones from this work (if any it should go in the appendix of course, and yes, I know the star is variable).
• Discussion, line 7: magnetic activity. The reference here is again Weigelt, but in the meantime the field has evolved and magnetic field on the surface of these stars is hardly detected (only example so far is Lebrè et al. 2014 for X Cygn, though the work is very debated). See page 68 of the review Hoefner and Olofsson; I suggest removing the magnetic field hypothesis from here and leave only convection. If the authors insist on keeping the magnetic field reference they should at least update it with the Lebrè 2014 one. By the way, I do not understand why the authors cite reference 8 as dynamic models with magnetic field. The word “magnetic” appears in that publication 2 times: one is a question, the second one refers to magneting coupling and rotation and refers to the core of the star interplaying with the surface.
• Page 13, at the very beginning “On the other hand SiC2 is suspected… Therefore phenomena…” I do not understand the connection between the SiC2 possible precursor of SIC dust formation, and the sentence afterwards. Maybe removes “therefore” or rephrase.
• Page 13, third line: the sentence “convection… would produce variations in temperature with angle” is misleading. The variations are in the direction of the convective cell ejection, but also do not forget that there is also pulsation at play.
• Page 13: Furthermore, the anisotropies reported here change our view of dust formation… This sounds as an extrapolation, though a very nice one. The dust species observed so far in in C-stars are SiC and amorphous carbon. Are you saying that one of them forms because of the anisotropies? Or are you suggesting that the various species in O-rich stars are explained by your argument? But then IRC+10216 is not an O-rich star. Perhaps what is meant is that different kind of dust species can co-exist at the same distance from the star because the conditions for dust formation are different due to the anisotropies.
• Chemical model: 919 gaseous species=> does this include both atoms and molecules? Are we

showing only HCN, SiS, C2 and SiC2 because these are the species probed by the data? If yes please say it explicitly.

• Supplementary discussion: the first section suggests that the offset in proper motion might be due to the binary nature of the star. While in the rest of the text they exclude the binary… so I wonder how the binary here suggests affects the conclusion of the rest of the work. This should be specified.

• Page 27, last three sentences “The effect….” There are no shocks in the data, can this be due to the specific pulsation phase of the data? Would you expect to see such shocks?

Author Rebuttals to Initial Comments:

ANSWERS TO REFEREE #1:

Referee #1 (Remarks to the Author):

The manuscript presents high angular resolution millimeter imaging of the well-studied carbon-rich AGB star IRC+10216 with ALMA. Thanks to the proximity of the target and the high angular resolution of ALMA, the data show inhomogeneous, clumpy structures in the innermost circumstellar envelope. Observational studies of where and how dust forms are crucial for clarifying the mass-loss mechanism in AGB stars. The result presented in this manuscript that dust formation and the atmospheric structure are intrinsically clumpy is therefore important for the solution of this long-standing problem. Furthermore, the images obtained in different molecular lines reveal distinct morphology, which the authors interpret in terms of inhomogeneities in density and temperature. These data are valuable for further understanding chemistry in the atmosphere shocked by pulsation and/or convection.

However, although the authors claim that their results are the first to spatially resolve anisotropic formation of dust and molecule in the atmosphere of AGB stars, non-spherical structures in the atmosphere and dust formation very close to the star have been detected in the recent years. For example, visible polarimetric imaging has revealed clumpy dust clouds very close to the star, within 2 stellar radii, with spatial resolutions comparable to or better than the present paper (Khouri et al. 2016, A&A, 591,A70; 2020, A&A, 635, A200; Ohnaka et al. 2016, A&A, 589, A91; 2017, A&A, 597, A20). Although the authors cite one of the papers, Ohnaka et al. (2016), as "non-spherically symmetric structures are commonly observed further out in the circumstellar envelope", the above papers detected clumpy dust clouds within ~2 stellar radii, slightly closer to the star than probed in the present study. As for the gaseous component, Takigawa et al. (2017, Sci. Adv., 3, eaao2149), Vlemmings et al. (2017, Nature Astronomy, 1, 848), and Khouri et al. (2018, A&A, 620, A75) probe the inhomogeneous molecular gas within several stellar radii around AGB stars with ALMA using different molecular lines. IR interferometric imaging has also revealed inhomogeneous structures in the dust-forming atmosphere of AGB stars with several milliarcsecond resolution (Wittkowski et al. 2017, A&A, 601, A3; Paladini et al. 2018, Nature, 553, 310; Ohnaka et al. 2019, ApJ, 883, 89; Chiavassa et al. 2022, A&A, 658, A185) as well as in red supergiants (Montarges et al. 2021 as cited in the paper and Ohnaka et al. 2017, Nature, 548, 310; Climent et al. 2020, A&A, 635, A160; Norris et al. 2021, ApJ, 919, 124).

Therefore, while the presented observations for this particularly well-studied carbon-rich AGB star are definitely valuable, I do not think that the significance of the paper is sufficiently high for publication in Nature. I agree in the conclusion that not only binary companions but also the anisotropy found in the present work leads to structures on larger scales. However, such anisotropy was already shown in the previous works.

Answer:

We thank the referee for the detailed list of articles. We want to note that we were aware of most of these papers while we cited only a few in our manuscript. We gave only a few of them because they are mostly focused on the same scientific results. That was probably not the best approach to the background and for that we apologise. Since a lot of text has been moved to the Methods, there is more room now to include most of these references, which we did.

We also thank his/her agreement in our conclusions about the combined effect of mechanisms leading to the formation of large scale structures. We do not agree about the perceived significance of our work, which seems novel, significant, and very relevant from different points of view. We do respect the criticism about this concern. Nevertheless, this subject has been the object of several high-impact journal publications, in particular 3 Nature, 1 Nature Astronomy, 1 Science, and 1 Science advance, in just four years (between 2017-2021), and several papers have recently come out describing 3D hydrodynamical models of AGBs with binary companions, which also indicates that our publication will be timely.

Asymmetries and anisotropies have already been observed in several evolved stars at different scales, including very high spatial resolution observations of the atmospheric environment of irregular variables and mostly oxygen rich stars.

Red Supergiants and Yellow Hypergiants have only irregular, small-amplitude variations and are not pulsating in a similar way than AGBs. In general, their winds are optically thin and radiation pressure on dust grains may not play an important role in their driving (see e.g. Bennett, P.D. Chromospheres and Winds of Red Supergiants: an empirical look at outer atmospheric structure. ASPC, Volume 425, Hot and Cool: bridging gaps in massive star evolution (2010)). Many other different mechanisms have been studied but RSGs and YHGs are, because of this, not representative cases of mass losing stars.

Concerning the rest of the AGB papers cited, we argue that most of the targeted sources are low mass loss rate and semiregular variables (W Hya, SW Vir, R Crt, and R Dor) plus Mira, whose circumstellar environment is evidently influenced by Mira B, where Khouri et al. 2018 showed dust trails connecting Mira A and B. To our knowledge, only studies of low mass loss rate of AGB stars have been published. Additionally, a large portion of those papers focus on one of the ingredients, either dust or gas, and only on a limited spatial scale. We cover both, continuum/dust and molecular emission and we provide a complete view of the circumstellar envelope of IRC+10216.

We also want to mention the strong chemical bias towards oxygen rich objects in the aforementioned papers. Among the sources targeted in that list of papers, we find R Dor, W Hya, R Crt, SW Vir, Mira, VX Sag, and the supergiants Antares, Betelgeuse, V602 Car, and AZ Cyg, all oxygen rich. Only one S-type (Pi1 Gruis), and one carbon star R Scl have been observed so far. R Scl observations presented by Wittowski et al. (2017) are nIR interferometric images towards the stellar disk, they do not present observations of the outflowing material in the atmosphere of a carbon star as we do. Moreover, R Scl is a semiregular variable and its present mass loss rate is one order of magnitude lower than that of IRC+10216. Properties of the wind driving mechanisms change due to the chemical differences and it is particularly important to show these anisotropies occuring in carbon stars.

IRC+10216 is the most iconic and studied carbon star, and the closest high mass loss rate TP-AGB to the Earth. It is considered an archetypal AGB in the studies of carbon rich stars, and also quite often in the study of oxygen rich stars. It is a very regular variable star with a high mass loss rate of ~10e-5 msun/yr. Here we present results for both dust and gas, and we show in our figures how these structures are configured at different spatial scales from a few tens of mas to much larger scales as seen in the spectacular combined maps of CO showing those irregular structures. Our work offers a much more complete answer to the formation of complex morphologies in circumstellar envelopes. Convective cells (most likely) also shape the winds of evolved stars. We have not found a deep discussion about this in any of the aforementioned papers. Moreover, we also conclude that the chemistry is directional, in the sense that due to these physical anisotropies, the same species may condensate at different distances depending on the occurrence of these anisotropies. Therefore, different species can be found in different directions/angles. To our knowledge, nobody has explored that different chemical compositions may be found at the same time and at the same distance from the star but at different directions/angles, not only as a simple mix of condensates but a real heterogeneous distribution where, for example, Si-C dust has been formed to the East but not to the West in a certain moment of the stellar evolution. For all these reasons, we firmly think that our work has a significant impact and presents impressive observational products so it is worth a Nature publication.

The authors report the data and the analysis in a sufficiently detailed and transparent manner, and I found no apparent flaw. The error bars are appropriately described in each figure. However, there are some points that need clarification as reported below.

* Results, p5
The continuum emission was fitted with a Gaussian, but the fitting procedure is not entirely clear. According to the text, it appears that the fit was done to the image, not to the uv data, and the authors fitted to the "continuum image deconvolved from the beam". It is not clear whether the deconvolution means the usual CLEAN process or something else such as deconvolution of the CLEANed image using the CLEAN beam. This should be more clearly described.

Answer: The fit was done to the image not the uv data. By “deconvolved from the beam” we meant that this is the fit output from the Gaussian fit CASA task that gives the image component size deconvolved from the beam (it also offers the size convolved with the beam). Since we are not carrying out an accurate work in the description of the continuum position and the extension of the bulk of the emission, the validity of this approach should be good for our purposes and it should be similar to what can be obtained from the fit of the uv-data.

* Figure 2
The horseshoe is seen at ~25 mas from the Gaussian centre in the residual map. This means horseshoe-shaped extra emission in front of the star. I think that it is worth discussion, because it may be caused by hot dust clouds or it may be similar to the hot spot found by Vlemmings et al. (2017) in the 338 GHz continuum toward the oxygen-rich AGB star W Hya.

Answer: The horseshoe emission is seen at ~25 mas from the centre, where the distance has been measured to the approximate midpoint (width-wise) of the horseshoe. It would be difficult to locate the emission in terms of a reference from the star if we do not define a radius first. That distance is slightly larger than the IR radius measured by Ridgway and Keady, 1988, but shorter than the measured radio photosphere by Menten et al. (2012). The horseshoe is rather extended and it seems to lie along the edge of the IR stellar disk from West to South (counterclockwise). The nature of this horseshoe structure is unclear, it could be dust clouds to the East and North. Vlemmings et al. (2017) argued that the hotspot seen in their data of W Hya is too small compared to the scale of convective cells, which is not our case. The size of this structure is compatible with the size of convective cells as reported by Freytag et al. (2017, A&A...600A.137F). However, with our data we cannot rule out the nature of this structure but it is not a symmetric feature. We have incorporated a brief sentence to indicate possible origins for this structure.

* The beam size is 27x19 mas (FWHP) according to the text. However, the beam shown in Fig. 2 is 40--50 mas (major axis), which should be clarified.

Answer: The 27x19 mas value is correct but it was wrongly shown in the image due to an unfortunate factor of 2, which comes from the usage of the FWHP as semi-axes size in the figure GILDAS/GREG script. Now this is correctly shown.

* Discussion, p12
The authors try to explain HCN/SiS with a chemical equilibrium model. However, in Supplementary discussion: shocks and outflows, they use a non-equilibrium chemistry model for shocked atmospheres. Therefore, the adoption of the chemical equilibrium in the interpretation of HCN/SiS needs justification and some discussion about possible effects of non-equilibrium chemistry, which I think would be easy, because they have such models.

Answer: The main aim of the shock models is to predict any emergent feature in the line profiles so synthetic spectra can be compared to the observations. We note that the presence of shocks is a likely possibility but uncertain since calculated line profiles are different from those observed. Chemical equilibrium is likely valid at the star photosphere due to the high pressures and temperatures. At some stellar radii, chemical equilibrium probably does not hold anymore. The purpose of the chemical equilibrium calculations is to see the dependence of the abundances of different molecules to changes in the temperature at the photosphere (which are expected due to anisotropies caused by hydrodynamic processes such as convection cells). In this sense, we think that chemical equilibrium reflects better this sensitivity of the chemical composition with temperature at the photosphere than shock models, which are inherently more uncertain due to both the underlying physical structure and the incompleteness of the chemical kinetics network.

* Supplementary discussion: continuum extended emission
As the authors correctly point out, the velocity information obtained from the molecular line maps may teach us the wind acceleration and the 3-D position of the clumps. However, the authors do not discuss the dynamical structures seen in the maps. It would be very valuable to include discussion on the dynamics seen in the different molecular lines, even if it is qualitative.

Answer: We have not discussed the kinematics of the emission besides the images that are used to illustrate that no obvious signs of shocks were found in our data. While this is an interesting point, it deviates from the main focus of this paper. We would like to note that we already have more high angular resolution data from this source with impressive results about the kinematics. This has been left for a forthcoming paper where some of us will be able to show the time evolution of the emission (continuum and gas) as we have observations of different epochs.

*The authors describe the clumps discovered in the 2 micron speckle imaging. However, subsequent observations show clear time evolution of those clumps (Weigelt et al. 2002, A&A, 392, 131).

Answer: We have included the main conclusions from this paper in the corresponding Section (Methods).

* Figure 9 shows a number of miniature spectra, but the conclusion that they present from this figure is that absorption is observed in front of the star, and emission is seen beyond the radio photosphere. But this is already seen in Fig. 8. I think that it is more reasonable to show the HCN data in the same way as Fig. 8.

Answer: We understand that Figure 8 is easier to interpret and, probably, more appealing, visually speaking, than Figure 9. We think that is in any case a subjective point given that even some of us prefer Figure 8 while others prefer Figure 9. Our point was to offer a different view that could help to carefully inspect different features coming from the three main regions here depicted, that is in front of the star, towards the radio photosphere, and beyond the radio photosphere. We have included two new figures in the Extended data so both species can be seen with both styles.

* The authors mention UV radiation enhancement in the SW. However, it is not clear whether the interstellar UV photons penetrate down so close to the star through the optically thick circumstellar dust envelope even if it is clumpy. If the UV radiation does not reach the region probed by the present study, it would be irrelevant for explaining the anisotropy anyway.

Answer: We agree with this comment. According to our data, we cannot affirm to what extent UV photons may play a role, if any role is played, at this inner region. Since this comment can be misleading we have removed it in the revised version.

* Figure 7
It would be necessary to explain whether the line profiles shown in the figure correspond to the flux integrated over the entire model image extending (at least) to 10 stellar radii (as shown in the top panels) or perhaps they are the spectra computed at the stellar disk centre and convolved with the 0.02" resolution. The model spectra show narrow features both in absorption and in emission. Explanation of the reason the shock gives rise to narrow features both in absorption and emission would be necessary. For this, it is probably helpful to plot the velocity structure as a function of radius. Then it is possible to see where the sharp features at specific velocities originate.

Answer: The main purpose of this figure was to show how the predicted spectra would be as if it were observed with a spatial resolution similar to that found in our observations. We used the shock-model predicted abundances at different stellar phases to feed our radiative transfer code and produce the emergent spectra as observed with 0.2’’ resolution centred at the source. The reason behind the narrow features both in emission and in absorption at different velocities is due to the complex combination of the kinematics, with sudden changes of velocities, and the excitation. It is a combination of the amount of gas at each relative position and velocity and the temperature. We have commented this in the methods and we have also included the velocity profiles to this figure.

*The signature of shocks is not seen in the observed data. Does this mean that the shocks are much weaker than predicted by the model? More discussion about this point would be valuable because shocks play an important role in dust formation.

Answer: The non-detection of shock signatures in our data is compatible with the existence of shocks but at lower velocity than our resolution so they are smoothed in our data. It may be also an effect of the opacity and excitation or spatial resolution. This fact does not affect our conclusions, considering that shocks, if present, would be a signature of the existence of convective cells and pulsation in order to obtain an anisotropic distribution. We have included a brief discussion in the corresponding section.

* An unidentified line is reported, and the channel maps are shown. Given the similarity to the images of SiC2, I wonder if they can discuss its possible origin, even if it is not definitive.

Answer: We do not have a good candidate for this line. It would be tempting to speculate on the carrier based on the distribution of the emission of the unidentified line compared to that of SiC2. It could be a spectral feature of a vibrationally excited mode of SiC2 or a feature from another Si-C-bearing molecule. That would be in any case a speculation that we think that can be left aside.

ANSWERS TO REFEREE #2:

Referee #2 (Remarks to the Author):

I have carefully read the manuscript "The uncharted atmosphere of an evolved AGB star: new challenges for circumstellar models." The authors present new observational evidence for the presence of small-scale spatial inhomogeneities inside the atmosphere of evolved star IRC+10 216, suggesting that such inhomogeneities are present at the initial stages of dust formation and thus that the formation of dust grains and molecules in AGB stars is not an isotropic process.
This finding calls into question the claim that asymmetrical structures in the envelopes of AGB stars are fully explained by the presence of a stellar companion. It also calls into question the validity of several modeling assumptions, including those of (1) isotropically expanding stellar winds; (2) isotropic physical conditions; and (3) isotropically distributed fractional molecular abundances.
The authors argue that these inhomogeneities, detected in spatially resolved observations of molecular emission in the deep atmosphere, disrupt isotropic stellar winds. In combination with other known physical processes (such as interaction with a companion), these small-scale anisotropies produce the morphologies observed in AGBs and circumstellar envelopes.

I think the findings themselves are worthy of publication in Nature, but understanding the novelty and scientific importance of these results based on the manuscript, as currently written, took a lot of effort for me, even as an expert in a closely related field. Presenting arguments in a way that is exciting and accessible to a general audience is part of publishing in Nature (per their stated criteria). With this in mind, I believe the authors should devote some time to extricating the punchline from the technical details of the observations (most of which could be moved to Methods) in the main body of the text. I also think that the authors do not spend enough text debunking alternative hypotheses for their observations or explaining the significance of their discovery.

Answer: We appreciate these comments from the referee. We have revised the manuscript with the purpose of presenting our results in a much more accessible and exciting way. Additionally, part of the technical details have been moved to the Methods, alleviating the main text. We have paid attention to the parts about alternative hypotheses, which are now better supported by the argumentation and additional material. We hope the scope of the paper and its breakthrough results are now described in a better way.

I provide general and specific comments below.

General comments:
The title includes the phrase "challenges for circumstellar models," but a focus on models does not seem appropriate. These findings disrupt our current understanding of dust grain formation and the origin of complex circumstellar morphologies; this is the significant result, in my view. Further, computational practicalities may very well prevent modelers from incorporating deep atmospheric anisotropies anyway. Not knowing about such anisotropies (until now) is not necessarily, or even likely to be, the limiting factor in achieving high physical fidelity with simulations of evolved stellar atmospheres. Our theoretical and numerical treatments of fundamental physical processes, like radiative transfer and convection, in these regimes are approximate, and just because the standard modeling prescriptions are not (completely) physical does not mean it is worthwhile or even possible to change the physical assumptions in the models. The results are significant without tying them to modeling inadequacies (of which there are many), and I think they should be divorced from this.

Answer: We have suggested a new title that conveys better the scientific results from our work.

Why do the stellar winds have to be isotropic? Is this not also an assumption of convenience?

Answer: Well, of course it is unreal to think that they could be perfectly isotropic. This argument is linked to the spherical appearance that AGB circumstellar envelopes, in general, show when observed with low angular resolution such as the IRAM 30m telescope. That also connects to the general assumption of spherical symmetry in most of the modeling works done so far. Spherical symmetry can then be considered as an assumption of convenience, generally speaking. However, as we prove with our work, the analysis of high angular resolution observations based on such assumptions, even when the images show clear deviations from that symmetry, are incomplete (e.g. Khouri et al. 2018, Vlemmings et al. 2017, Khouri et al. 2016, see also our answer to referee #3). Our analysis of the HCN/SiS ratio shows that such assumptions may lead to wrong estimates by significant amounts, one order of magnitude perhaps, and maybe more if the chemical effect is considered. That chemical effect may also have an important impact when the analysis of images of polarised light as scattered by dust are analysed if, as we demonstrate, the grain composition has chemical differences in different directions.

Have you actually run simulations that confirm the connection between small-scale anisotropies and the development of large-scale arcs or clumps in the AGB atmosphere or post-AGB circumstellar envelope?

Answer: We have run simulations to illustrate this connection. We have reconsidered the overall philosophy of our manuscript and considered that this was an important topic to develop. We have included an extended discussion about this issue and developed some models, which are presented at the end of the Methods.

Specific comments:
Page 2:
"It is inside…of AGB stars that is initiated…" awkward phrasing

Answer: this has been removed according to one of the requests by the editor

Page 3:
"Successfully resolve for the molecular…" ungrammatical

Answer: this has also been removed

"...usually adopted by models" Which models?

Answer: this has also been removed

Results, Page 5:
Why one fifth of the synthesized beam? Are the contributions from these two sources of uncertainty (Gaussian fit and fifth of the beam) equivalent? Are uncertainties due to instrumental systematics folded into "uncertainty of one fifth of the beam"?

Answer: no, they do not contribute equally, in fact the beam uncertainty dominates this value but we wanted to have these two sources of uncertainty added in quadrature as a standard approach to the calculation. A precise estimate of the uncertainty, as done in astrometry, would require a formal derivation that is not needed for our purposes. From the general formula, the position uncertainty is proportional to (beam_size/(2*SNR)). For a clear detection (SNR=5) we would have (beam_size/10), but we have taken a more conservative value of beam/5 which should reflect that a poorer positional accuracy (of a factor of 2 worse) is obtained with extended configurations of the interferometer due to atmospheric phase fluctuations. A reference for the calculation of the position uncertainty is Reid, M. J., et al. Astrophysical Journal v.330, p.809. We have included a comment in the Methods about this technical detail and the reference.

(The following link also gives useful information:
https://help.almascience.org/kb/articles/what-is-the-absolute-astrometric-accuracy-of-alma)

Page 6:
It is very interesting that the identified components could be dusty gas clumps expelled by IRC+10 216. In Discussion, the authors briefly touch on the idea that they may be probing a mechanism related to the production of the dust cloud responsible for the "Great Dimming" of Betelgeuse in 2019–2020. I think this merits greater attention and a more prominent location in the text.

Answer: As commented before, we have re-written certain parts of the text to make it more impactful. This specific point has been revised and we give it now a more specific weight in the discussion.

The entire concept of a "radius" for such stars is questionable. As the authors note, the definition of the radius is wavelength-dependent. Would a different measurement for the size of the radio and/or IR photosphere change the results? Has this been explored?

Answer: Yes, as it is correctly pointed out by the referee, the concept of radius is ill-defined not only due to the wavelength dependency but also as an intrinsic problem of pulsating stars. The rigorous definition of photosphere is linked to the concept of opacity, which is wavelength dependent, this is a subtle nuance that might be skipped by non astrophysicists but we believe is of general knowledge in the community. That being said, an ill-defined radius does not affect our calculations. The exact physical conditions should change along the stellar variability curve, meaning that densities and temperatures will vary from phase to phase. Nevertheless, we consider a whole phase for the star so anisotropies (e.g. different abundances for a given species, direction/angle-wise speaking and considering a given distance from the star) would exist, as we know also from our observations of CW Leo at different epochs (priv. comm.). Our chemical models would be perfectly valid considering that a specific chemical model for a given location in space for a specific stellar phase represents the situation at a given instant. A complete description of the situation would require a coupling of an hydrodynamical model with the chemistry which is out of the scope of our paper.

Page 8:

“This has led to suggest that…” → this has lead to suggestions that

Answer: changed.

Discussion:
I am satisfied with the authors’ arguments against alternative explanations for non-spherically symmetric emissions. However, we would do well to remember that finding no evidence to support an alternative hypothesis is not the same as ruling out that hypothesis definitively.

Answer: We agree with that statement. We tried to be as careful as possible in our wording and ways to express our conclusions.

Page 12:
Again, does the presumed/measured radius affect your conclusions? This is important to address given that the novelty of the results is tied in part to the depth at which the atmosphere is being probed.

Answer: As we have explained three questions before, our conclusions are not affected by the measured radius. If the radius is shifted inwards or outwards that would affect the radial distribution of molecules and dust, that is, at which distance from the star a given molecule is in gaseous or solid state (i.e. has condensed onto the dust grains) and its predicted abundance, but the anisotropies would still be there.

“Even a moderate variation of 10% in the temperature…can lead to orders of magnitude variations in the fractional abundances…” This is an area of enormous uncertainty in simulations and notoriously poor treatment in stellar models.

Answer: Well, with this affirmation we want to underline that even a moderate change in the physical conditions may lead to an important change of the fractional abundances as shown by our chemical models comparing in particular SiS and HCN. That would be a physical situation where in certain regimes from a 10% change we could move from a part of the P-T diagram where the species condensate or not, as shown in our previous Fig. 10.

Page 13:
“Therefore, phenomena such as convection cells at the surface of the star would produce variations in the temperature with angle…lead to anisotropies seen in the emissions…” The complex interplay between convection and chemical stratification in the outer layers of evolved stars is well established. Is this the first time it has been observed directly (at this depth)?

Answer: As commented by referee #1, there have been publications showing anisotropies in the emission of molecules and dust grains at different scales. However, in all of these works we find a lack of closure for all the arguments presented so far. None of them relate the chemical segregation as a consequence of convective cells emerging at random spots along the surface of the star. 3D-hydro-chemical models would be the next frontier to reach in the simulations of AGB circumstellar envelopes. We have included several recent references to this topic (Silke, M. et al. 2022, arXiv:2206.12278, see also our answer to referee #3).

Methods

I have investigated Agundez et al (2020) hoping for more information on the chemical model. Their code is not publicly available. Though “closed-source” software is, unfortunately, common in astronomy, a truly rigorous evaluation of the science is not possible without direct access to this tool. I do not think this should prevent publication, but it is something to keep in mind.

Answer: The source code behind the chemical equilibrium calculations is available upon request to M. Agúndez. This code has been used to study the chemistry of exoplanet atmospheres (Agúndez et al. 2012, A&A, 548, A73, Agúndez et al. 2014, A&A, 564, A73) and AGB atmospheres (Agúndez et al. 2020, A&A, 637, A59). It is also implemented in the exoplanet atmosphere retrieval code TauREx, which is publicly available (Al-Refaie et al. 2022, ApJ, 932, 123).

References
This manuscript does seem to cite in-group work preferentially. Citations to several works by Andrea Chiavassa, Sofia Ramstedt, Matthias Maercker, and Michael J Ireland would be appropriate.

The following specific citations would also be appropriate:
Norris et al. 2012 (Nature)
Hofner & Freytag 2019 (A&A)
Joyce et al. 2020 (ApJ)
Harper et al. 2020 (ApJ)
Harper et al. 2021 (AJ)
Hofner & Freytag 2022 (arxiv)

Answer: We have included some of the references suggested by the referee trying to stay below 30 references within the main text.

Referee #3 (Remarks to the Author):

Summary of the key results

I reviewed the paper "The uncharted atmosphere of an evolved Asymptotic Giant Branch (AGB) star: new challenges for circumstellar models". The manuscript presents new ALMA observations of the carbon-rich Mira star IRC+10216. The data are taken at 4 different spectral windows with spectral resolution around 0.550 km/s, the maximum angular resolution is about 20 mas, hence extremely high. The scientific target is very nearby and well-studied source, already object of another study published on Nature volume 467, pages 64–67 (2010) where water was detected for the first time. Despite its relative closeness, the presence of a binary companion in the atmosphere of IRC+10216 is argument of discussion in the community.
The ALMA data here presented, and interpreted in terms of chemical models, resolve spatially the molecular emission in the vicinity of the radio photosphere. Inhomogeneities are clearly observed, and the authors argue that such inhomogeneities cannot be explained only in terms of binaries or sub-stellar companions, as in their data there is no signal of the companion hunted by a large part of the community. Later in the text the argument of the companion comes back as explanation of the offset in proper motion. This should be clarified in the text.
The key conclusion of this work is that anisotropies are for the first-time observable in the mass-loss process, contradicting the "axiom" that mass-loss from evolved stars is spherically symmetric. I acknowledge that the data are spectacular and deserve a publication in a high-profile journal, however I have a doubt about the axiom of spherically symmetric mass-loss which is outdated and does not represent what the community believes since quite a while (see my comment below to the abstract). Moreover there is an inconsistency between the presence/not presence of the binary companion which should be solved.

Answer: We thank the referee for this summarized vision of the paper. The presence of a binary companion has been discussed by different authors. Kim, H. et al., 2015 (ApJ, 804, Issue 1, L10) reported the detection of a point-like source in HST images lying 0.5 arcsec SE from IRC+10216 that could be the companion star. That is a plausible explanation but it is not a confirmation of the detection of the companion. In fact, these authors refuted this hypothesis in a following paper, Kim et al. 2021 (ApJ, 914, 35). Our observations cannot rule out the presence of a binary companion. In fact, the position we measured for CW Leo does not match the predicted position one would obtain taking into account the position and proper motion estimates by Menten et al. (2012).

However, in terms of the circumstellar environment the situation is different. Guelin M. et al., 1993, A&A 280, L19; Cernicharo, J. et al. 2015, A&A, 575, A91; Decin et al. 2015, A&A, 574,A5 discuss the different signatures seen in the circumstellar shells of IRC+10216 as an effect of binary interactions, such as off-centered shells and spirality. Please note that the structures we see in the maps have an age of 4-5 years approximately, while the binary companions discussed so far in the literature have orbital periods of 700 yr (Cernicharo et al 2015 & Guelin et al. 2018) and 55 yr (Decin et al. 2015). In case of existence, these companion stars cannot induce a periodicity for the atmospheric anisotropies of a much lower period of just a few years.

Moreover, the signatures of the interaction between two stars surrounded by circumstellar material are often observed as circumbinary disks/toroids and bipolar or multipolar outflows. None of these are seen in our data of the close circumstellar environment of IRC+10216. The models presented by Decin et al. 2015 predict a very clear spiral within the central 5''x5'' FOV. We can directly compare that to our Figure 3 (following the new numbering, check bottom left HCN panel), where a more complex structure is seen. Partial spiral arms may be hinted there but they are not complete arms and some blobs/clumps are clearly seen. Our Figures 2 and 3 show a much more complex situation where different molecules trace different structures. We interpret these as a possible consequence of convective cells emerging at random locations along the stellar surface.

Convective motions of material cause anisotropies in the temperature/density conditions leading to a chemical segregation in the gas depending on the direction/angle from the star where this is located. We have been more cautious when discussing this scenario and binary effects in the revised version. At atmospheric level, it is hard to interpret the spatial distribution of SiS, HCN, and SiC2 seen in our data as a sole consequence of the presence of a binary star. Inspecting carefully our Fig. 2, for example, how could one connect under that framework the fact that SiC2 is only seen towards the Eastern hemisphere as two blobs while four blobs are seen in the SiS line and a more ubiquitous distribution of HCN? Again, we cannot think of an interpretation to these morphologies as direct consequences of a binary companion.

Originality and significance: if not novel, please include reference

AGB stars such as IRC+10216 are relevant to various fields of Astrophysics because of their mass-loss enriching the interstellar medium of the various products of stellar nucleosynthesis. In literature it is often assumed that AGB stars lose mass via an isotropic wind. This statement is written by the authors in line 5 of the abstract, and it would be good to add a reference (see also comment in the section "suggested improvements"). In the last 10-15 years, instrument advancement and high angular resolution observations unveiled a different picture. Maercker et al. 2012 ALMA observations of RScl showed a very beautiful spiral inside the well-known (round) detached shell. During the years we gathered more and more evidence that in fact the environment of evolved stars is not as spherically symmetric as we thought, and indeed asymmetries show up. On the other hand, such asymmetries are usually explained as a signature of a companion of stellar or substellar origin. Decin 2020Sci...369.1497D observed a sample of AGB O-rich stars using ALMA. The spatial resolution of the data is less than the one presented here, in fact the spatial scales probed are between ~0.24″and ~1″. Maps of SiO and CO molecules show several morphologies such as spirals, and bipolar outflows explained as the signature of binary companions interacting with the expanding outflow of the AGB. All the companions so far found around AGB stars are orbiting at a distance larger than 10 stellar radii. First generation of dynamic model atmosphere correctly cited by the authors (Winters, Höfner early works) are 1D, hence assume spherically symmetric / isotropic wind. Liljegren et al. A&A 619, A47 (2018) used 3D-star-in-a-box (CO5BOLD) models of convection as inner boundary for 1D -DARWIN winds. The authors show that anisotropic CO5BOLD central star affects the wind, however they do not present synthetic observations hence, it is not clear if the asymmetric structures predicted could be observed. From the observational point of view there have been works showing asymmetric structures not clearly linked to binaries for example Ohnaka et al. A&A 589, A91 (2016) reports clumpy dust clouds in the environment of WHya using VLT/SPHERE and VLTI/AMBER. They conclude that "the detection of the clumpy dust clouds close to the star lends support to the dust formation induced by pulsation and large convective cells as predicted by the 3D simulations for AGB stars." The work here presented shows clear evidence that not only the dust as Ohnaka mentioned in his work, but also the molecular emission is affected by the local asymmetries induced by pulsation and convection in the stellar vicinity. In fact, the work here presented goes beyond and shows that the chemistry is also different because of the different density of the medium. Van de Sande & Millar 2022MNRAS.510.1204V showed how chemistry can change if a stellar companion is present in the atmosphere, now we know that it is the case also for single stars. For all the arguments listed above, this work can be considered a breakthrough.

Data & methodology: validity of approach, quality of data, quality of presentation

The data acquired are very high angular resolution (~20mas) ALMA data mapping the molecular emission beyond the radio-photosphere up to 5 stellar radii. Images are reconstructed using various methods and shown for the continuum, HCN, SiS, and SiC2. The interpretation is mostly qualitative and based on the data. Anisotropies are clearly detected, and the authors conclude that they cannot be explained only with companions (not detected in their data), rather by the interplay of convection and pulsation changing the temperature and pressure of the environment causing the formation of different chemical species. I think the scenario that the author presents are reliable, however I also think it would be hard to see directly in such data a planet or a sub-stellar companion. Do we have prediction of what would a planet or a stellar companion do if within the first 5-10 stellar radii? Can we for example exclude 100% that the broken arc (not the arc itself) seen in the data are not the effect of a tiny object which is too small from producing more “apocalyptic” scenarios (i.e., emission lines, uv-flux, and so on)?

Answer: That’s a very good question. We have included a few more references in the manuscript concerning this part of the discussion that were missed and also a few that have recently come out. Fundamental models of the production of spiral patterns, arcs, and disks in the circumstellar material were presented by Mastrodemos & Morris (1998ApJ...497..303M, 1999ApJ...523..357M). All these models revealed a recognisable degree of symmetry in the patterns generated in the circumstellar material that we do not observe in our data. There are more recent models dealing with the interplay between a mass losing AGB star and a binary companion. Decin et al. (2015, A&A...574A...5D) presented sub-arcsec resolution (~0.5’’) ALMA data of IRC+10216 where they suggest the presence of spiral structure in its inner wind. The data was accompanied by a 3D model made with SHAPE for which they calculated the radiative transfer for CO. The resulting brightness distributions were inserted as input for the CASA SIMOBSERVE task to calculate synthetic channel maps and PV diagrams. They tested out several parameters related to the orbital properties of the system and they ruled out the possibility of a Jupiter-like or brown dwarf secondary based on arcsec scale signatures in the PV diagrams. However, their arguments for this statement are inconclusive. They also assumed a mass of 4 msun for IRC+10216, deduced from the isotopic ratios of Mg and the output of AGB nucleosynthesis models (Guelin et al. 1995 A&A...297..183G). According to De Nutte et al. (2017A&A...600A..71D), the initial mass of IRC+10216 would be 1.6 msun as derived from the oxygen isotopic ratios. We note that these mass estimates based on the isotopic ratios are extremely uncertain due to the intrinsic uncertainty of the observational derivation of the isotopic ratio and the uncertainties in the nucleosynthesis models. For example, according to Karakas & Lugaro (2016ApJ...825...26K), the Mg isotopic ratio (25Mg/26Mg = 1+-13) derived by Guelin et al. (1995 A&A...297..183G) would be compatible with models of solar metallicity stars with masses between 1 and 6 msun. Moreover, Decin et al. did not explore what happens at atmospheric scales since their spatial resolution was at most 10 times worse than ours.

Malfait et al., 2021 (A&A, 652, A51) also presented Smooth Particle Hydrodynamic models of binary interaction with AGBs. They explored models of a 1.5 msun AGB accompanied by a 1 msun star at 6 au (semi-major axis). The secondary induces a motion of the stars around the centre of mass of the system while generating a spiral shock, and perturbates the AGB wind material. Different patterns can be observed in their models, from perfect spirals to more irregular configurations but none of their simulations reproduce our observations. Moreover, it would still be very difficult to reconcile the predicted density profile with the chemical segregation that we observe.

Similar models were presented by Silke et al. 2022 (arXiv220612278M). These authors point out the need of a true 3D coupling of the chemistry with the hydrodynamical models and refer to the asymmetries that can be caused by a porous envelope, leading to enhanced photochemistry in the inner envelope (Van de Sande et al. 2022).

Recently, Aydi & Mohamed (2022, MNRAS.513.4405A) have presented 3D hydrodynamical models of the circumstellar environments of evolved stars affected by the presence of nearby sub-stellar companions. They concluded that if the companion is at d>4 R*, a single spiral arm should be seen, which can be ruled out from our data. For a much closer separation to the companion, a more complex structure of multiple spirals is observed, whose configuration depends on many orbital and system parameters that are explored. We are very surprised about the results presented by these authors, which show the complex structures that can be formed at scales of tens of AUs. Nevertheless, we note again that the stellar outflow at the atmospheric level is not an isotropic wind, as it can be seen in the images we present. Aydi & Mohamed (2022) used in their models a wind/shocks simulated as a piston (with a sinusoidal modulation) that are spherically symmetric. They also discuss that the detailed models of Freytag, Liljegren & Höfner (2017) show that convection cells and fundamental-mode pulsation will result in chaotic shock wave structures around the AGB star, shifting from spherical symmetry.

As far as we have investigated, the binary models could explain regular and irregular structures at arcsec scales but not down to tens of milli arcsec scales. Moreover, they all assume isotropic mass loss and isotropic wind emerging from the star which we can affirm is not the case as we show in the maps of molecules such as SiS or SiC2.

The author mentions chemical equilibrium models and radiative transfer described in Methods and supplementary material to support their conclusions; however, I find that such models are not sufficiently presented in the main text (I know the problem of limited space), they could play a more important role in convincing the reader about the conclusions of the authors. Probably a couple of sentences about the main results of the models in the main text could help making a stronger case for the "single star" scenario.

Answer: We agree that the chemical models should occupy a more central place in the manuscript. We would like to make a precision here. We do not claim that IRC+10216 is a single star, in fact several empirical proofs exist suggesting that it is a binary system. However, the dust (continuum) and molecular emission morphologies observed at atmospheric scales cannot be explained by the presence of a companion. We are witnessing the signatures of convective cells that cause anisotropies in the density and temperature profiles leading to anisotropic abundances and morphologies that also depend on the species due to their different chemical behavior. We have reorganised the discussion in the manuscript and we hope that the chemistry occupies a more central place.

Suggested improvements: experiments, data for possible revision; references; clarity and context

Below I will provide some comments about suggested improvements.

• Abstract, line 5: "assumed to be isotropic". As a scientist in this field, I interpret "AGB winds are isotropic" as something isotropic on a global scale, meaning without a preferential direction. This picture is described in the review by Hoefner and

Olofsson 2018 (https://link.springer.com/content/pdf/10.1007/s00159-017-0106-5.pdf). The mass-loss as "locally" isotropic is a very old concept which nobody believes since already a few years. A single star emits material in clumps and patches, and the mass-loss is isotropic only if the ejection is averaged over a very long time. There are several proofs that the wind from single stars is not locally symmetric (see the review above), and this work is among one of the nicest proofs itself.
This line should be rephrased to mirror better the reality of the field.

Answer: The abstract has changed so that sentence has been removed.

• Page 2, last paragraph, references in line 3 (Hofner, Winters): again, in line with what I wrote above the 1D models are from the 90s. They are still used because they are simple to calculate, but nowadays it is known that the winds are not locally symmetric. Therefore, I would say that no one assumes that winds are symmetric, especially in the stellar vicinity, unless there are computational reasons for it.

Answer: We agree that computationally speaking there are reasons to adopt spherical symmetry. However, we pose if this is a valid approach when dealing with models of AGB stars investigating the formation of spirals and other structures in the vicinity of the star. While nobody believes that winds are symmetric, (almost) everybody assumes that in their models even when asymmetries are being reported.

• Page 3, "here we present the first observational evidence that… winds are not isotropic" This is an unfortunate wording because there are already several other examples such as Ohnaka 2016. It might be the first case for the ALMA data with respect to the series of works of Decin et al. and references therein.

Answer: We agree with this statement. We refer to our answer to referee #1 where we discuss this issue.

• Page 6, beginning: there is a reference to Weigelt work. The paper of Weigelt presents evolution over 10 years of structures in the inner atmosphere of IRC+10216. I wonder if any of the structures here presented/detected could relate to the ones from this work (if any it should go in the appendix of course, and yes, I know the star is variable).

Answer: It is an interesting comparison. If we assume the star is in the A clump shown by Weigelt et al. (2002, A&A...392..131W), the other three clumps B, C, and D would resemble the residuals in our continuum images. However, if we attend to the scales shown by Weigelt et al. the separation between A and B is ~200 mas which would be out of the FOV shown in our continuum image. Thus, we conclude that they are probably different clumps or dust clouds.

• Discussion, line 7: magnetic activity. The reference here is again Weigelt, but in the meantime the field has evolved and magnetic field on the surface of these stars is hardly detected (only example so far is Lebrè et al. 2014 for X Cygn, though the work is very debated). See page 68 of the review Hoefner and Olofsson; I suggest removing the magnetic field hypothesis from here and leave only convection. If the authors insist on keeping the magnetic field reference they should at least update it with the Lebrè 2014 one. By the way, I do not understand why the authors cite reference 8 as dynamic models with magnetic field. The word "magnetic" appears in that publication 2 times: one is a question, the second one refers to magnetic coupling and rotation and refers to the core of the star interplaying with the surface.

Answer: We agree with the referee that an explanation based on magnetic fields would be improbable as it would not be supported by current research. We removed that reference.

• Page 13, at the very beginning "On the other hand SiC2 is suspected… Therefore phenomena…" I do not understand the connection between the SiC2 possible precursor of SIC dust formation, and the sentence afterwards. Maybe remove "therefore" or rephrase.

Answer: We removed the connector between the two sentences.

• Page 13, third line: the sentence "convection… would produce variations in temperature with angle" is misleading. The variations are in the direction of the convective cell ejection, but also do not forget that there is also pulsation at play.

Answer: Maybe there is a difference in how we understand the same process or there was a semantic problem in our way of writing this part. What we meant is that convective cells occur at random locations all across the stellar surface and that affects the temperature distribution when compared in different angles. We could have a situation, for example, where a convective cell emerges only from the North but not in any other direction. It would be fair to say that if we take a distance of e.g. r=2R* and evaluate the temperature along that surface, there is an angular/directional dependence. We have changed the word "direction" by "angle" which we hope makes this clearer now.

• Page 13: Furthermore, the anisotropies reported here change our view of dust formation… This sounds as an extrapolation, though a very nice one. The dust species observed so far in in C-stars are SiC and amorphous carbon. Are you saying that one of them forms because of the anisotropies? Or are you suggesting that the various species in O-rich stars are explained by your argument? But then IRC+10216 is not an O-rich star. Perhaps what is meant is that different kind of dust species can co-exist at the same distance from the star because the conditions for dust formation are different due to the anisotropies.

Answer: This connects to the previous question. If we agree that at a given distance from the stellar centre the temperature can be different depending on the angle/direction, different types of dust will form depending on the local conditions. That means that we could find amorphous carbon dust efficiently formed in a certain direction after the emergence of a convective cell while SiC dust is not. Under the same conditions, M-type stars will behave in a similar way so convective cells may favour the formation of silicates in a given direction but not in any other at a given instant.

• Chemical model: 919 gaseous species=> does this include both atoms and molecules? Are we showing only HCN, SiS, C2 and SiC2 because these are the species probed by the data? If yes please say it explicitly.

Answer: Indeed, it includes atoms and molecules, and, yes, we only show the relevant molecules. We stated this clearly in the text.

• Supplementary discussion: the first section suggests that the offset in proper motion might be due to the binary nature of the star. While in the rest of the text they exclude the binary… so I wonder how the binary here suggests affects the conclusion of the rest of the work. This should be specified.

Answer: Concerning this topic, we refer to our previous answers about the different signatures that a binary companion causes in: a) the orbital motion of IRC+10216 and, b) the circumstellar environment. We hope it is clearer now.

• Page 27, last three sentences “The effect….” There are no shocks in the data, can this be due to the specific pulsation phase of the data? Would you expect to see such shocks?

Answer: Observationally, the presence of infalling material, specific features in the line profiles, or emission lines whose excitation mechanism requires the existence of shocks, are signatures of the occurrence of shocks in the innermost regions of pulsating AGB stars (Vlemmings et al. 2017, NatAs...1..848V, Khouri et al. 2019, A&A...623L...1K, Winters et al. 2022, A&A...658A.135W). We have not seen any indication of shocks in the explored atmosphere of this archetypal AGB, which does not rule out the existence of shocks due to the convective motions and the pulsation of the star. As we answered to referee #1, it could be a lack of resolutio.

Reviewer Reports on the First Revision:

Referees' comments:

Referee #1 (Remarks to the Author):

I see that the authors have made great efforts to address the points raised by the referees and overhauled the manuscript. They justify that the results are worth publishing in Nature by noting that IRC+10216 is a very well-studied carbon-rich object with a high mass-loss rate, and most of the previous studies are about oxygen-rich AGB stars with low mass-loss rate. However, one could justify the novelty of a result by claiming that it is the first for a certain subclass of objects such as C-rich/O-rich and high/low mass-loss rate. In my opinion, given that the non-spherically symmetric structures within a few stellar radii have already detected in several AGB stars as noted in my previous referee report, this sort of justification for the originality and novelty is not sufficient for publication in Nature, even if the results are highly interesting and are important contribution for solving the mass-loss problem. The authors also stress the detection of the directional chemical segregation. However, the previous ALMA observations already show very different morphology for different molecular emission for the same objects, if not discussed separately as directional chemical segregation: for instance, AlO and 29SiO (E_up/k ~= 80 and 73 K, respectively) in W Hya reported by Takigawa et al. (2017, Science Advances 3, eaao2149) and SiO and H2O (E_up/k = 3552 and 3462 K, respectively) in Mira from Wong et al. (2016, A&A 590, A127). In either case, E_up of two lines is similar, as SiS and HCN have similar E_up, and the published channel maps already show different morphology. Therefore, also in this regard, the novelty of the directional chemical segregation is not well justified.

Below are comments of somewhat minor nature.

* Horseshoe emission
The authors mention that its size approaches that of the convective cells predicted by the 3D simulations. It is a viable scenario, but since this is not conclusive, it would be better to formulate in a more conservative way. Besides, it is not clear whether the authors conclude that the horseshoe emission is from the gas in large convective cells or dust clumps formed due to convective cells or they cannot distinguish from their data.

* Figure 2
The color scale goes down only to 0. However, the absorption due to the gas in front of the star appears negative in the continuum-subtracted images. Therefore, the color scale should be changed to show the absorption as well.

* Figure 9
I understand the authors' point of view. But it is very difficult to recognize any features in the spectral line profile (other than it is absorption or emission) in those miniature spectra. Maybe they can show the spectra at larger spatial interval so that each spectrum can be shown bigger.

Referee #2 (Remarks to the Author):

I have reread the manuscript now entitled “Atmospheric molecular blobs shape up circumstellar envelopes of AGB stars”---a title which much more accurately captures the key result of this work and helps to address my primary concern regarding the framing of the manuscript.

The authors have made good effort towards extricating the punchline from the technical details, and I find the text much more readable.

I feel that my previous comments were satisfactorily addressed, and so I recommend this manuscript for publication in Nature.

Minor comments:
Page 2:
“...by not completely understood phenomena…” suggest rephrasing

“the archetypal and closest to the Sun high mass-loss rate AGB-TP star” → “the archetypical, high-mass-loss-rate TP-AGB star that is also closest to the Sun.”

Page 3:
No comments

Page 4:
No comments

Page 5:
No comments

Page 6:
First paragraph of page 6 is much improved.

Page 7:
“...incompleteness of the chemical kinetics network…” Can the nature of this incompleteness be elaborated upon, e.g., in Methods? The impact of this could be significant, and at least deserves more explanation.
In general, I remain concerned about the lack of open access to the models used in this analysis. If the source code used to obtain results is to remain behind a “contact the author” wall (just because this is a common practice in astronomy does not mean it is a good one), it is incumbent upon the authors to provide enough detail for reproducibility within the text (or appendices, supplemental materials, etc.)

Page 8:
The discussion of alternative hypotheses is much improved.

Page 9:

Also much improved.

Referee #3 (Remarks to the Author):

Dear colleagues,
First I would like to congratulate with the authors for the data and the results here shown.I read the new version of the paper called “Atmospheric molecular blobs shape up circumstellar envelopes of AGB stars”. I have only one remaining major concern which I hope can be taken into account.
The main conclusion of the paper is that the emerging convective cells modulate the temperature and density of the gas causing chemical anisotropies. However, AGB stars are known to pulsate, and the dynamic process at 1 stellar radii is not only convection, but convection plus pulsation. So I am left wondering if the authors are suggesting that there is no effect of pulsation, and indeed only convection is shaping the inner envelope and the chemistry. In Hoefner & Freytag A&A 623, A158 (2019) sect 3.3 one reads:
“The non-spherical shock fronts in the 3D models, triggered by convection and pulsations, lead to a patchy distribution of dust in the atmosphere. Grain growth is more efficient in high-density regions, and new dust is therefore concentrated in the wakes of outward-propagating shocks, apparent as arc-like structures in the central cross-sections shown in Fig. 3 (rows 1 and 3). Seen face-on, these structures correspond to partial dust cloud layers, covering an area of the stellar surface similar to the shock. “
In the current version of the paper the title hints at convection as only responsible, and later in the text there are a few contradictions. Please see the items below:
- in abstract they write “We interpret this by the presence of large convective cells in the photosphere, which modulate the temperature and density of the gas and cause the observed chemical anisotropies.
- page 5: “ This anisotropy in the formation of dust grains and molecules very probably occur as a consequence of localized ejections of matter due to large-scale convective motions in the photosphere of the star “
- At page 6 the authors refer to the combination of pulsation and convection of the Betelgeuse Nature paper explaining the Great dimming, while in the sentence right after later they omit the pulsation again: “Similarly, we argue that we are witnessing the anisotropic formation of dust and molecular gas in the
high mass-loss rate AGB star IRC + 10◦216 due to temperature and density anisotropies caused by large
convective cells13. “ We should remember that Betelgeuse and IRC+10216 are different objects, and the pulsation of IRC+10216 as mira is arguably more significant than in Betelgeuse which is a semiregular variable.
At the end of page 7 again only convection is mentioned.
At page 8 the authors exclude the binarity as explanation for the observed structures and mention also that there is no observation of shocks. Are the shocks here mentioned the single effect of the pulsating atmosphere? If yes, then I propose this is stated clearly.
Later in the text the authors also mention that the non-detection of the shocks could be due to the insufficient spatial resolution “... which, in any case, would be a consequence of convective motions

in the
photosphere”. I do not understand the second part of the sentence and its relation to the previous statement. Again it seems the effect of pulsation is ignored in this part of the text. Finally: is it possible to quantify the S/N needed for the detection of the shock?
In the Methods at page 6 shocks are connected to pulsation. Again, there is a discussion about why the shock is not observed, however I wonder if the non-detection is enough to exclude completely the role of pulsation in the shaping. Are we sure that with convection as lonely player the material would be lifted far enough from the surface to meet the conditions for the formation of given chemical species? Also how does the observed wind develop if pulsation would not play a role at all?
Finally at page 11 the authors mention that the simulation supporting their observations consist of a model with *an AGB outflow* and randomly ejected blobs. Is the AGB outflow the effect of pulsation?
I think there is sufficient evidence in the literature that we are facing the interplay between the two dynamic processes. I hope the authors will consider these comments and clarify the role of pulsation in the paper so that the text becomes more consistent. If they believe that pulsation has no role at all then this should be stated clearly.
Thanks a lot for your work and your effort. Best Regards

Author Rebuttals to First Revision:

REFEREE 1:

I see that the authors have made great efforts to address the points raised by the referees and overhauled the manuscript. They justify that the results are worth publishing in Nature by noting that IRC+10216 is a very well-studied carbon-rich object with a high mass-loss rate, and most of the previous studies are about oxygen-rich AGB stars with low mass-loss rate. However, one could justify the novelty of a result by claiming that it is the first for a certain subclass of objects such as C-rich/O-rich and high/low mass-loss rate. In my opinion, given that the non-spherically symmetric structures within a few stellar radii have already detected in several AGB stars as noted in my previous referee report, this sort of justification for the originality and novelty is not sufficient for publication in Nature, even if the results are highly interesting and are important contribution for solving the mass-loss problem. The authors also stress the detection of the directional chemical segregation. However, the previous ALMA observations already show very different morphology for different molecular emission for the same objects, if not discussed separately as directional chemical segregation: for instance, AlO and 29SiO (E_up/k ~= 80 and 73 K, respectively) in W Hya reported by Takigawa et al. (2017, Science Advances 3, eaao2149) and SiO and H2O (E_up/k = 3552 and 3462 K, respectively) in Mira from Wong et al. (2016, A&A 590, A127). In either case, E_up of two lines is similar, as SiS and HCN have similar E_up, and the published channel maps already show different morphology. Therefore, also in this regard, the novelty of the directional chemical segregation is not well justified.

As we answered in our first revision, the sources cited by the referee are not good representatives of Mira variability class to generalise on the molecular emission of AGBs, as W Hya is a semi-regular variable, and Mira circumstellar surroundings are affected by multiple companions. IRC+10216 is, as we noted, an archetypal AGB star (plus C-rich and high mass loss rate) and, probably, the most studied evolved star so far. We argued on the impact of our results, concerning not only on the wind launching mechanisms but also on the chemistry of the innermost region of the circumstellar surroundings, where dust is formed. We firmly think that our results have the significance and novelty that is worth publishing in Nature.

Below are comments of somewhat minor nature.

**** Horseshoe emission***

The authors mention that its size approaches that of the convective cells predicted by the 3D simulations. It is a viable scenario, but since this is not conclusive, it would be better to formulate in a more conservative way. Besides, it is not clear whether the authors conclude that the horseshoe emission is from the gas in large convective cells or dust clumps formed due to convective cells or they cannot distinguish from their data.

We have revised the phrasing. The nature of this emission structure is uncertain and we cannot assert its origin.

**** Figure 2***

The color scale goes down only to 0. However, the absorption due to the gas in front of the star appears negative in the continuum-subtracted images. Therefore, the color scale should be changed to show the absorption as well.

The color scale and the LUT have been changed to improve the visualisation.

**** Figure 9***

I understand the authors' point of view. But it is very difficult to recognize any features in the spectral line profile (other than it is absorption or emission) in those miniature spectra. Maybe they can show the spectra at larger spatial interval so that each spectrum can be shown bigger.

The scale of the map of spectra has been changed to improve the visualisation.

REFEREE 2:

I have reread the manuscript now entitled "Atmospheric molecular blobs shape up circumstellar envelopes of AGB stars"---a title which much more accurately captures the key result of this work and helps to address my primary concern regarding the framing of the manuscript.

The authors have made good effort towards extricating the punchline from the technical details, and I find the text much more readable.

I feel that my previous comments were satisfactorily addressed, and so I recommend this manuscript for publication in Nature.

Minor comments:
Page 2:
"...by not completely understood phenomena…" suggest rephrasing

We are not sure about this comment, if the referee finds this sentence vague or it is due to another reason. Nevertheless, the summary paragraph has changed and this sentence has been removed.

"the archetypal and closest to the Sun high mass-loss rate AGB-TP star" → "the archetypical, high-mass-loss-rate TP-AGB star that is also closest to the Sun."

Done

Page 3:
No comments

Page 4:
No comments

Page 5:
No comments

Page 6:
First paragraph of page 6 is much improved.

Page 7:
"...incompleteness of the chemical kinetics network…" Can the nature of this incompleteness be elaborated upon, e.g., in Methods? The impact of this could be significant, and at least deserves more explanation.

This case refers to the suitability of the usage of thermochemical equilibrium chemistry versus chemical kinetics. Our TE model is explained in Agúndez et al. 2020 (A&A, 63, A59), as we cited in the Methods. TE principles to describe the chemistry are based on pure thermodynamic properties, where the formation of a species is described by the (standard) formation enthalpies and entropies according to the Gibbs energy. Compilations, such as the NIST-JANAF tables (Chase, 1998, NIST-JANAF Thermochemical Tables, 4th ed.) or the NASA database (McBride et al., 2002, NASA Technical Publication TP-2002-211556), provide accurate information for all the relevant species in an AGB atmosphere under TE conditions. On the other hand, chemical kinetics requires the reaction rates for all the formation and destruction paths of each of the species in the models, which translates into two major complications: i) to have all the formation and destruction routes for all species, and ii) to have accurately determined reaction rates at given temperatures in AGB envelopes. Only part of these rates can be empirically determined and very often they are only valid in a very limited temperature range. It is not unusual to find extrapolation of rates for temperatures that were not probed by experiments due to the technical difficulties linked to the determination of reaction rates at certain temperatures, which is known to lead to significant errors. Moreover, if the model aims to accurately describe the nucleation process, the task is out of the scope of this paper. We have elaborated more on this topic adding a sentence so any reader can understand the limitations of chemical kinetics usage.

In general, I remain concerned about the lack of open access to the models used in this analysis. If the source code used to obtain results is to remain behind a "contact the author" wall (just because this is a common practice in astronomy does not mean it is a good one), it is incumbent upon the authors to provide enough detail for reproducibility within the text (or appendices, supplemental materials, etc.)

We understand the referee's concern. In the particular case of the chemical code and the envelope model, as we mentioned in our first answer, it is a matter of the lack of a proper documented version that could be shared in e.g. GitHub, which would also require to prepare a publication, which is something that is foreseen for the future.

Page 8:
The discussion of alternative hypotheses is much improved.

Page 9:
Also much improved.

REFEREE 3:
Referee #3 (Remarks to the Author):

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- Finally at page 11 the authors mention that the simulation supporting their observations consist of a model with *an AGB outflow* and randomly ejected blobs. Is the AGB outflow the effect of pulsation?

I think there is sufficient evidence in the literature that we are facing the interplay between the two dynamic processes. I hope the authors will consider these comments and clarify the role of pulsation in the paper so that the text becomes more consistent. If they believe that pulsation has no role at all then this should be stated clearly.
Thanks a lot for your work and your effort. Best Regards

This is an important point raised by the referee, and we really appreciate the careful revision that they have done to spot this out. We want to note that the omission of the pulsation in the discussion, as we explained in our cover letter, is somewhat an unfortunate consequence of the removal of part of the introductory material to produce this new version. Following the suggestions by the editor and the referees, we removed the introduction about the basic principles that are widely accepted by the scientific community to produce and sustain an AGB stellar wind (PEDDRO scenario, *Pulsation Enhanced Dust Driven Outflow*). How that correction affected the entire manuscript was incautiously revised as we, used to work in this field, internally assumed that. Pulsation was only presented at the very beginning of the summary paragraph implicitly in the description of TP-AGBs.

As it is noted by the referee, theoretical, observational, and modeling works undoubtedly prove that pulsation is needed to launch and sustain AGB stellar winds at the given measured rates. Therefore, we have rewritten all the parts of the manuscript where ambiguous or incomplete descriptions were presented, as in those listed in the referee's comment. In the Methods, we have removed the two sentences at the end of the first paragraph in page 8 about convective cells and put that last sentence in the conclusions presented in page 9. This change avoids redundancies and presents the anisotropic structures as a result that supports predictions from MHD models by the Stellar Physics group in Uppsala University, as we had referenced in our first revision.

Our observations and their interpretation clearly support the existence of an anisotropic process creating these dust and molecular gas clumps and blobs. In principle, Miras are fundamental mode pulsators which would lead to a more or less spherical outflow (e.g. Wood et al. 2010), thus, convective motions of gas cells are presented in the discussion as main agents causing the anisotropies. Nevertheless, both pulsation and convective motions participate in the formation of the outflow. It is the complex interplay between both that initially configure the circumstellar material that can be further affected by the presence of companions.

We do not rule out the existence of shocks, as mentioned in the text. Shocks should take place due to the occurrence of outflowing and infalling material and also when a convective cell or blob randomly occurs and interacts with that material, as discussed in MHD models (e.g. Höfner & Freytag, 2019, A&A, 623, A158). However, we cannot disentangle the existence of shocks as our data seems insufficient to show direct or indirect imprints of them. There are several factors that may play against the observational detection of the shocks in our data that encompass a complex combination of spatial resolution, excitation, opacity, and turbulence effects. For example, as it is shown in our Figures 6 & 7 in the Methods, the radio-photosphere region seems "quiet" in terms of line emission/absorption. Molecular emission coming from behind the star that is not blocked by it (with impact parameters larger than the stellar radius derived from IR observations) is expected to reach our telescopes. In addition, hot molecular gas is predicted to exist in the region covered by the radio-photosphere as chemical models indicate and a priori detectable emission should come from there, unless the molecular gas is thermalized with the continuum emitting matter. The exception to this statement is the absorption in front of the star, which can be produced by the gas outside the radio-photosphere, and weak emission that comes from the external edge of the radio-photosphere, that can be a consequence of the limited spatial resolution of the observations. All these can be explained if the radio-photosphere is optically thick, in good agreement to the brightness temperature of ~1600K derived by Matthews et al., 2018, ApJ, 156, 15, which suggests that we can only detect the emission of the outer shells of the radio-photosphere in the cm wavelength range (note that the effective temperature derived from IR observations ranges from 2000 to 2500K approximately). The optical depth in the mm could be high enough to block the emission coming from behind the radio-photosphere and thermalize the molecular gas in the radio-photosphere. In this situation, imprints of possible shocks occurring inside the radio-photosphere would be undetectable in the mm range. Witnessing these effects would require shorter wavelength observations, where the optical depth is low enough to allow emission escape from the radio-photosphere. The best wavelength for this study would depend on the physical characteristics of the radio-photosphere. The submm range could be a good starting point but even shorter wavelengths (IR and optical) would help as well. It also can be a resolution problem if the model-predicted narrow features are smoothed in our data, taking into account line-of-sight projection and doppler shift. This combination of factors is quite complex so no accurate predictions can be done about the detectability of shocks in CW Leo atmosphere, but the aim should be to obtain observations with higher spatial resolution, i.e. between one third to a half of the stellar radii ~5-10 mas, and also explore at different wavelengths.

When we stated that "*... which, in any case, would be a consequence of convective motions in the photosphere",* we meant that the existence of shocks would be a consequence of the convective motions, interacting with the pre-existent material, launched by pulsation-radiation pressure, again as supported by MHD models.

Concerning our simulations, indeed, the circumstellar outflow is a consequence of the pulsation of the star. The star ejects shells periodically and we then add the binary companion and the blobs. In order to explore the effect of pulsation, we carried out models that use a periodic function that modulates the ejections, that is increasing/decreasing the contrast density between maxima and minima. It can also be done for the temperature but we chose to keep it constant over time to minimize parametrization. We have explored different factors for such density ratio, up to 100, and different periods. Our models do not predict noticeable changes due to the contrast density factor. High contrast spherical shells surrounding the star would be smoothed out (with the current angular resolution, HPBW~20 mas) and not resolved for pulsation periods shorter than 3 years. We commented on this in the Methods.

Reviewer Reports on the Second Revision:

Referees' comments:

Referee #1 (Remarks to the Author):

The authors try to justify the novelty of their observations of IRC+10216 in terms of the chemical composition, mass-loss rate, and binarity. However, as I already wrote in my previous reports, one can always argue for the novelty by going into sub-classes. The authors also try to justify by claiming that W Hya and o Cet are not good representatives of Mira variability class because W Hya is a semiregular variable, and o Cet is a binary. However, W Hya shows a regular periodicity (e.g., light curves in Woodruff et al. 2008, ApJ, 673, 418 based on the data from AAVSO and Whitelock et al. 2000), and it is sometimes classified as Mira-type (see discussion in Nowotny et al. 2010, A&A 514, A35).

As far as binarity concerned, IRC+10216 is also suspected to be a binary system with a long period as discussed in the paper. The binary separation of o Cet is 80 au (0.8" from Prieur+2022, ApJS, 139, 249 and a distance of 100 pc), wider than the 25 AU of IRC+10216 obtained by Decin+2015. If the elliptical orbit with the 700-day period and 1 solar mass for the primary star and 0.2 solar mass for the companion from Guelin+2018 is assumed, the orbit's semi-major axis is ~80 AU, comparable to o Cet. The orbital period of o Cet of ~500 yr (Prieur+2022) is longer or comparable to that of IRC+10216. The papers (Wong+2016, Kaminski+2016, A&A 592, A42) discuss the region close to the star, as done in the present paper. If these studies are not appropriate for investigating Mira-class variability taking place close to the star due to the binarity, it is also the case for IRC+10216 because its binary separation is smaller or comparable, and the orbital period is shorter or comparable.

In terms of chemical composition and mass-loss rate, there is a paper on ALMA observations of the S-type star, showing clumps close to the star (Danilovich+2021, A&A, 655, A80). It is not completely carbon-rich, and its mass-loss rate (3e-5 solar mass/yr) is still lower than IRC+10216, but the star is between IRC+10216 and other oxygen-rich AGB stars already studied. It is true that IRC+10216 is one of the well-studied AGB stars, perhaps the most studied one. However, there are already papers published on other well-studied AGB stars. The present paper stresses that "the anisotropies reported here change our views on the formation of molecules and dust in the upper atmosphere of the archetypal AGB star IRC+10216". However, given the previous papers on other AGB stars, this does not strike me as a surprise or a completely novel or unexpected result but rather confirmation, albeit interesting and important, for this well-studied AGB star.

For these reasons, in my opinion, justifying by going into subclasses like chemical composition, mass-loss rate, or whether the object is the best-studied or not, is marginal and not sufficient for publication in Nature.

The authors incorporated the minor points that I raised in my previous report except that the color scale in Fig. 3 (right column) still goes down only to zero, although the continuum-subtracted ALMA images should show negative values over the radio photosphere.

Referee #3 (Remarks to the Author):

Dear editor, dear authors,

I feel that my previous comments were satisfactorily addressed, and so
I recommend the paper for publication.
Congratulations to the author for this nice result.
Best Regards

Author Rebuttals to Second Revision:

Dear editor and referees,

We really appreciate all the help and comments that have been received during the whole revision process. According to the last report, there was no major remaining issues. Otherwise, we changed the colour scales in Figure 3 in order to include the only suggested revision in the last report.

Best regards,

Luis Velilla-Prieto and José Cernicharo

on behalf of the authors.