Peer Review File

Manuscript Title: A rotating white dwarf shows different compositions on its opposite faces

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referees' comments:

Referee #1 (Remarks to the Author):

I have read with great care the manuscript "A rotating white dwarf with two faces" by I. Caiazzo and collaborators. This work reports the discovery of a rotating white dwarf that only shows hydrogen absorption lines on one side of its surface and only helium lines on the other side. This is an interesting finding, but I have major concerns that may prevent the publication of this work in Nature.

1. Novelty and importance of the discovery

As mentioned in the manuscript, we already know at least one white dwarf, GD 323, for which we have conclusive evidence for the presence of an inhomogeneous surface composition (Pereira et al. 2005). Of course, ZTF J203349.8+322901.1 is a more extreme case as it shows only H lines on one side and only He lines on the other side, while GD 323 shows variations but no complete disappearances/reappearances of spectral lines. But all in all, the difference between GD 323 and ZTF J203349.8+322901.1 appears to be one of degree and not one of kind. ZTF J203349.8+322901.1 does not represent a new class of objects.

Since 2005, only 7 publications have cited the work of Pereira et al. where the inhomogeneous surface composition of GD 323 was established. Moreover, over those 17 years, GD 323 has not led to any advance in our understanding of spectral evolution. I am therefore very skeptical when the authors explain the astrophysical importance of ZTF J203349.8+322901.1 by asserting that it may help "shed light on the still poorly understood physical mechanisms that underpin spectral evolution in white dwarfs." Why would ZTF J203349.8+322901.1 be so important to our understanding of white dwarf spectral evolution if GD 323 was not? To be clear, I believe that this is a very interesting discovery, but I doubt it is important or novel enough to warrant publication in Nature.

2. Magnetic field and shallow spectral lines.

A key aspect of ZTF J203349.8+322901.1 is its shallow spectra lines, which current atmosphere models cannot reproduce. The authors' favoured explanation for the observed shallowness of the absorption lines is the presence of an undetected magnetic field. In the main text (lines 89-90), the author justify this hypothesis by citing two conference proceedings. The first one is by Hardy et al. (2020) and the second one is by Rolland & Bergeron (2015). I have read both proceedings and I have found no mention of how magnetic fields weaker than a few MG can make the absorption lines shallower. I see absolutely no justification for the authors' hypothesis in the references they give: this is very concerning.

In the Supplementary Material (lines 418-422), the authors now speculate that the magnetic field may affect the temperature stratification of the atmosphere in a way that weakens the absorption lines. Again, no evidence is given for this hypothesis. I deem this scenario very implausible. If an undetected magnetic field could really have such an effect on the atmospheric structure, then surely a stronger field would also have a large effect too. However, white dwarfs with multi-MG fields are routinely modelled without any correction to the temperature structure and as far as I know weak lines are not an issue. Furthermore, if there really were a strong effect on the temperature structure even when the field is too low to be detected, then we would surely see this weak lines problem in many more white dwarfs harbouring weak undetected fields.

3. Inhomogeneous vs mixed homogeneous atmosphere models.

In their model atmosphere analysis, the authors explain that they tried using mixed H/He atmosphere models but that this did not result in a better fit to the observed spectra. As far as I can tell, the models used by the authors assume a homogeneous H/He mixture. This is clearly inappropriate for a star like ZTF J203349.8+322901.1: the whole point of this work is that this is a star with an inhomogeneous atmospheric composition!

This inhomogeneous surface composition could possibly explain the weak lines problem, a scenario that is neither explored nor mentioned by the authors. Calculating such models is relatively straightforward given that the authors already have access to a white dwarf atmosphere code. For magnetic white dwarfs, this kind of inhomogeneous atmospheric modelling has been done for at least 20 years (Euchner et al. 2002, https://arxiv.org/pdf/astro-ph/0205294.pdf). The same proven approach could be used here, except that H/He instead of the magnetic field strength would vary over the surface.

4. WD 1832+089

I have found no mention in the manuscript of WD 1832+089. This is a DBA white dwarf in the same effective temperature range as ZTF J203349.8+322901.1, it is also very massive, and it shows periodic variations in its photometry with a period of about 6 minutes (Pshirkov et al. 2020, https://arxiv.org/pdf/2007.06514.pdf). This is likely another object in the same class as GD 323 and ZTF J203349.8+322901.1.

5. Synthetic spectra

There is no figure which compares the synthetic spectra to the observed spectra of ZTF J203349.8+322901.1. This should definitely be shown explicitly.

6. Mass of ZTF J203349.8+322901.1

The mass of ZTF J203349.8+322901.1 is only given as "roughly 1.2 solar masses". The authors should clearly state their mass determination with error bars and including the uncertainty on the core composition (both a CO and an ONe core composition are plausible in this mass range).

7. Constraint on the magnetic field

The authors should clearly state what is their constraint on the magnetic field of ZTF J203349.8+322901.1. I have only found vague statements in the text. It is straightforward for the authors to measure an upper limit on the magnetic field using the spectra they have acquired.

8. Mass of the MESA model

The MESA model discussed in the Supplementary Material has a mass of 1.025 Msun. Earlier in the text, we learn that ZTF J203349.8+322901.1 has a mass of roughly 1.2 Msun. So why not use the correct mass? A higher mass will increase the surface gravity and change the pressure stratification.

9. Convective mixing and convective dilution

At line 152, convective mixing and convective dilution appear to be conflated. Those are two separate mechanisms in the context of white dwarf spectral evolution.

10. Caption of Figure 4

“The shaded area indicates the location of the DB gap according to the same models”. The evolutionary models cited above do not predict the location of the DB gap. I don’t know what the authors are referring to.

Referee #2 (Remarks to the Author):

This paper presents the discovery of Janus, a two-faced white dwarf where one face is dominated by hydrogen and the other by helium. This is an exciting discovery that is clearly worth consideration for publication in Nature. The authors present
convincing spectroscopy data on the nature of this object, and discuss potential ways to explain the unusual surface composition of this object.

I have two major comments that should be addressed before acceptance.

1- The authors assume a pure hydrogen or pure helium composition in their model fits, but it is not clear that either face is pure in composition. The spectra at phases 0 and 0.5 are clearly unusual; they show absorption features much broader than expected from pure hydrogen or pure helium atmosphere models.

The paper states:
"The strength of the lines can be reduced if the atmosphere has a mixed composition; however, if we increase for example the hydrogen content in our models so that in the mixed atmosphere spectrum the helium lines appear as weak as in the observed spectrum at phase 0.5, the model spectrum also shows strong hydrogen lines, which we do not see in the observed spectrum. The same goes for the hydrogen face. A mixed composition is therefore insufficient to explain the weakness of the lines."

Can the authors show some of these model fits to the spectra? It is true that a mixed composition may be insufficient, but it could still be a part of the picture. The current draft presents the spectroscopy data, but does not show any fits to the line profiles at all. Even if the authors cannot match the spectra perfectly, it would be interesting to see what limits they can place on the H/He ratio of the two sides based on the absence
of the H or He lines on either face of this star.

There is another statement:
"if the magnetic field induces a small difference in temperature between the two faces, it would be enough to explain the difference in composition: on the hot side, the mixing region is not yet extended into the hydrogen layer and we still see hydrogen, while at the colder side mixing has already diluted the hydrogen and we see only helium."

The paper does not clearly demonstrate that we see only helium on one side and only hydrogen on the other side. Presenting some of the model fits to the spectra and providing upper limits on the H (He) abundance on the He (H) face would be very valuable.

- Pereira, Bergeron, Wesemael (2005)

https://ui.adsabs.harvard.edu/abs/2005ApJ...623.1076P/abstract
discussed the variability in GD 323 and considered specific geometries including pure helium equatorial belts and and pure hydrogen polar caps. If the H ocean idea shown in Extended Data Figure 5 is correct, this is exactly what you would expect, and it may help fit the observed spectral lines. The paper would benefit from a discussion of this possibility (e.g., some exploratory fits to the observed spectra may be helpful) and if there is any evidence of such a configuration in the spectroscopy data.

Minor comments:
"of the order of 1000 - 1300 km/s," >> "of the order of 1000 km/s,

- Table 1 does not include a mass estimate for Janus. Please provide a mass estimate for CO and ONe core composition.

- "The absorption lines do not show any Zeeman splitting or wavelength shifts, so we know that the magnetic field on either face cannot be higher than a fewMG."

One can typically do better than a few MG based on low resolution spectra of DA white dwarfs. The way it is written, it is not clear if the authors have really tried to model the observed spectra and empirically determined that the field strength cannot be higher than some number or not.

- "If we assume the magnetic field to be stronger on one side (as in a offset dipole structure)," G183-35 would be an excellent example of a star with a magnetic field stronger on one side, and could be mentioned in the discussion.

- "Spectral variations, although not as extreme as in Janus, have been observed in another white dwarf, GD 323, which is also close to the red edge of the DB Gap"
There are other examples. For example Eisenstein et al. (2006) lists "An example near 45,000 K is PG1210+533, which shows variable H, He I, and He II line strengths, probably modulated with an (unknown) rotational period"

- Figure 1 caption: The solid lines in the left panel are not described.

- Extended Figure 1:
The scale is different between the left and right panels. It would be better (for a fair comparison) to use the same y scale for all panels.

- Model fits: The authors use the Gaia parallax measurement of 2.5 +- 0.4 mas in their model fits. Bailer-Jones et al. (2021) give a geometric distance of 455 pc with 1sigma lower and upper limits of 374 and 666 pc. The distance estimate from Bailer-Jones
seems better than a simple inversion of the Gaia parallax. Hence, a discussion of the 1/varpi versus the distance from Bailer-Jones et al. would be useful.

- The terms in Equation 1 on page 30 should be explained. For example, what is alpha_T?

Author Rebuttals to Initial Comments:

Response to Referee #1

We would like to thank the referee for reading the paper with care, for the thoughtful comments and for their celerity in submitting the referee report. Below are our replies to the different comments, highlighted in purple.

1. Novelty and importance of the discovery

As mentioned in the manuscript, we already know at least one white dwarf, GD 323, for which we have conclusive evidence for the presence of an inhomogeneous surface composition (Pereira et al. 2005). Of course, ZTF J203349.8+322901.1 is a more extreme case as it shows only H lines on one side and only He lines on the other side, while GD 323 shows variations but no complete disappearances/reappearances of spectral lines. But all in all, the difference between GD 323 and ZTF J203349.8+322901.1 appears to be one of degree and not one of kind. ZTF J203349.8+322901.1 does not represent a new class of objects.

Since 2005, only 7 publications have cited the work of Pereira et al. where the inhomogeneous surface composition of GD 323 was established. Moreover, over those 17 years, GD 323 has not led to any advance in our understanding of spectral evolution. I am therefore very skeptical when the authors explain the astrophysical importance of ZTF J203349.8+322901.1 by asserting that it may help "shed light on the still poorly understood physical mechanisms that underpin spectral evolution in white dwarfs." Why would ZTF J203349.8+322901.1 be so important to our understanding of white dwarf spectral evolution if GD 323 was not? To be clear, I believe that this is a very interesting discovery, but I doubt it is important or novel enough to warrant publication in Nature.

The white dwarfs GD 323 and ZTF J2033 are indeed most likely members of the same class, as we mention in the paper. GD 323, however, shows only subtle variations in the strength of the absorption lines; ZTF J2033 is novel and exciting because the elements on its surface appear completely separated, as the lines of hydrogen and helium disappear completely at opposite phases. This extreme behavior presents much harder and more interesting challenges for modelling the white dwarf and places much stronger constraints on the models for this new class white dwarfs

Often in science the first discovery of a new type of object remains almost unnoticed until a second object of the same class is discovered. In fact, more than one object is needed to establish a new class, and in this case, the second object characterizes the class in a much stronger fashion. Furthermore, although a single double-faced white dwarf is very interesting in itself and might inspire several theoretical models to explain its structure and evolution, an entire class of objects undergoing the transition between DA and DB would be extremely helpful because their characteristics (temperatures, masses, magnetic field strengths etc.) would inform us on the physical processes underpinning the transition.

The publication of the discovery of ZTF J2033 will inspire researchers to look for more objects of this new class. The current observational landscape is much different from what was available in 2005, when the spectral variability of GD 323 was discovered. We now have several high-cadence all-sky surveys (PTF, ZTF, TESS and soon Rubin) that can detect photometric variability in white dwarfs down to faint magnitudes and over the entire sky, and soon the new spectroscopic surveys (SDSS V, DESI, WEAVE, 4MOST etc.) will provide spectral classification for hundreds of thousands of white dwarfs, dramatically increasing the number of known helium-dominated white dwarfs that we can follow up to look for variability. These surveys will allow us to discover and build a sample of double-faced white dwarfs, which in turn will shed light on the spectral evolution of white dwarfs.

2. Magnetic field and shallow spectral lines.

A key aspect of ZTF J203349.8+322901.1 is its shallow spectra lines, which current atmosphere models cannot reproduce. The authors' favoured explanation for the observed shallowness of the absorption lines is the presence of an undetected magnetic field. In the main text (lines 89-90), the author justify this hypothesis by citing two conference proceedings. The first one is by Hardy et al. (2020) and the second one is by Rolland & Bergeron (2015). I have read both proceedings and I have found no mention of how magnetic fields weaker than a few MG can make the absorption lines shallower. I see absolutely no justification for the authors' hypothesis in the references they give: this is very concerning.

Of the 16 white dwarfs analyzed by Rolland & Bergeron (2015), 10 show disagreement between the spectroscopic temperatures and the photometric values (the lines in the spectra are too weak compared to the temperatures inferred from the SEDs). The authors suggest that the objects might be unresolved binaries; however, to explain the weakness of the lines with a binary companion, the companion would have to be featureless, and such a high incidence of DC companions is quite suspicious. The point of the citation is that this disagreement between the photometric and spectroscopic temperatures is very common in magnetic white dwarfs (we added a sentence to the paper to clarify this point).

The authors of the other paper (Hardy et al. 2020, a conference proceeding), gave an update on the same work at the white-dwarf conference in Tübingen in August of this year. In their talk, they described how this discrepancy between the photometric temperatures and the strength of the lines is present in the sample that they analyzed for a large fraction of objects, both for low- and for high-magnetic field white dwarfs. We could change the citation to private communication if the referee and the editor consider it more appropriate.

In the Supplementary Material (lines 418-422), the authors now speculate that the magnetic field may affect the temperature stratification of the atmosphere in a way that weakens the absorption lines. Again, no evidence is given for this hypothesis. I deem this scenario very implausible. If an undetected magnetic field could really have such an effect on the atmospheric structure, then surely a stronger field would also have a large effect too. However, white dwarfs *with multi-MG fields are routinely modelled without any correction to the temperature structure and as far as I know weak lines are not an issue. Furthermore, if there really were a strong effect on the temperature structure even when the field is too low to be detected, then we would surely see this weak lines problem in many more white dwarfs harbouring weak undetected fields.*

As we commented above, although many multi-MG fields white dwarfs are indeed modelled without any correction to the temperature structure, many white dwarfs with different magnetic field strengths present a problem of weak lines instead, and therefore it is not implausible to assume that the magnetic field might affect the strength of the lines in some cases. We have attempted to make it more clear in the manuscript that we put forward the presence of magnetism as a possible reason for the shallow lines and emphasize that it is a possibility that requires further investigation.

3. Inhomogeneous vs mixed homogeneous atmosphere models.

In their model atmosphere analysis, the authors explain that they tried using mixed H/He atmosphere models but that this did not result in a better fit to the observed spectra. As far as I can tell, the models used by the authors assume a homogeneous H/He mixture. This is clearly inappropriate for a star like ZTF J203349.8+322901.1: the whole point of this work is that this is a star with an inhomogeneous atmospheric composition!

We agree with the referee that the surface's composition is not homogeneous; however, we tried a mixed composition to see if for example a mixed composition in the hydrogen-dominated spot would explain the weakness of the lines on the hydrogen phase. We also tried an inhomogeneous composite surface (see below). In both cases, if the fraction of the helium-dominated surface is contributing enough flux - or if the abundance of helium in the atmosphere is high enough – to dilute the hydrogen lines as in the observed spectrum, we would see helium lines in the spectrum too (and viceversa).

This inhomogeneous surface composition could possibly explain the weak lines problem, a scenario that is neither explored nor mentioned by the authors. Calculating such models is relatively straightforward given that the authors already have access to a white dwarf atmosphere code. For magnetic white dwarfs, this kind of inhomogeneous atmospheric modelling has been done for at least 20 years (Euchner et al. 2002, https://arxiv.org/pdf/astro-ph/0205294.pdf). The same proven approach could be used here, except that H/He instead of the magnetic field strength would vary over the surface.

We added a few plots in the methods section to show that the weakness of the lines cannot be explained neither with a mixed homogeneous composition nor with an inhomogeneous composite atmosphere. If we take for example phase 0.5, where only helium lines are present, and if we construct an inhomogeneous surface, in which part of the flux comes from a hydrogen-dominated region on the surface and part of the flux comes from a helium-dominated region (like a spot or a belt), in order to weaken the helium lines as in the observed spectra, the contribution to the flux of the region dominated by hydrogen has to be so high that strong hydrogen lines would appear in the spectrum.

4. WD 1832+089

I have found no mention in the manuscript of WD 1832+089. This is a DBA white dwarf in the same effective temperature range as ZTF J203349.8+322901.1, it is also very massive, and it shows periodic variations in its photometry with a period of about 6 minutes (Pshirkov et al. 2020, https://arxiv.org/pdf/2007.06514.pdf). This is likely another object in the same class as GD 323 and ZTF J203349.8+322901.1.

In the paper mentioned by the referee, the authors do not present any evidence of spectral variability. We agree that WD 1832 is a good candidate for being another object in the same class of GD 323 and ZTF J2033. However, confirming it would require spectroscopic follow-up, and in this paper, we would rather focus on the discovery of ZTF J2033, leaving the analysis of other candidates to future work. Nonetheless, we added a section of possible candidates in the methods section.

5. Synthetic spectra

There is no figure which compares the synthetic spectra to the observed spectra of ZTF J203349.8+322901.1. This should definitely be shown explicitly.

We added figures in the method section.

6. Mass of ZTF J203349.8+322901.1

The mass of ZTF J203349.8+322901.1 is only given as “roughly 1.2 solar masses”. The authors should clearly state their mass determination with error bars and including the uncertainty on the core composition (both a CO and an ONe core composition are plausible in this mass range).

We added estimates using CO and ONe models from the literature.

7. Constraint on the magnetic field

The authors should clearly state what is their constraint on the magnetic field of ZTF J203349.8+322901.1. I have only found vague statements in the text. It is straightforward for the authors to measure an upper limit on the magnetic field using the spectra they have acquired.

Zeeman splitting depends strongly on the geometry of the magnetic field and sometimes splitting is not evident even at magnetic field strengths of a MG or more (especially in such weak and broad lines). For this reason, we cannot provide a strict upper limit on the field of the white dwarf. We added a small section in the methods, where we cite the example of WD

J0103-0522: a white dwarf whose lines are blueshifted, indicating a field strength of about 5 MG, but that does not show any Zeeman splitting.

8. Mass of the MESA model

The MESA model discussed in the Supplementary Material has a mass of 1.025 Msun. Earlier in the text, we learn that ZTF J203349.8+322901.1 has a mass of roughly 1.2 Msun. So why not use the correct mass? A higher mass will increase the surface gravity and change the pressure stratification.

We rerun the MESA model for a 1.26 Msun white dwarf. There are indeed some changes in the required hydrogen mass.

9. Convective mixing and convective dilution

At line 152, convective mixing and convective dilution appear to be conflated. Those are two separate mechanisms in the context of white dwarf spectral evolution.

By that phrase we meant that at that temperature the convective mixing in the helium layer is becoming stronger, which leads to the convective dilution. We changed it to “convective instabilities” to make it clearer.

10. Caption of Figure 4

“The shaded area indicates the location of the DB gap according to the same models”. The evolutionary models cited above do not predict the location of the DB gap. I don’t know what the authors are referring to.

The location on the CMD of the boundary temperatures (50,000 and 30,000) is derived from the models. We clarified it in the caption.

Response to Referee #2

We would like to thank the referee for reading the paper with care, for the thoughtful comments and for their celerity in submitting the referee report. Below are our replies to the different comments, highlighted in purple.

1- The authors assume a pure hydrogen or pure helium composition in their model fits, but it is not clear that either face is pure in composition. The spectra at phases 0 and 0.5 are clearly unusual; they show absorption features much broader than expected from pure hydrogen or pure helium atmosphere models.

The paper states:
"The strength of the lines can be reduced if the atmosphere has a mixed composition; however, if we increase for example the hydrogen content in our models so that in the mixed atmosphere spectrum the helium lines appear as weak as in the observed spectrum at phase 0.5, the model spectrum also shows strong hydrogen lines, which we do not see in the observed spectrum. The same goes for the hydrogen face. A mixed composition is therefore insufficient to explain the weakness of the lines."

Can the authors show some of these model fits to the spectra? It is true that a mixed composition may be insufficient, but it could still be a part of the picture. The current draft presents the spectroscopy data, but does not show any fits to the line profiles at all. Even if the authors cannot match the spectra perfectly, it would be interesting to see what limits they can place on the H/He ratio of the two sides based on the absence of the H or He lines on either face of this star.

We added a section in the methods as well as a few plots to show that the weakness of the lines cannot be explained neither with a mixed homogeneous composition nor with an inhomogeneous composite atmosphere. In both cases, if the fraction of the helium-dominated surface is contributing enough flux - or if the abundance of helium in the atmosphere is high enough – to dilute the hydrogen lines as in the observed spectrum, we would see helium lines in the spectrum too (and viceversa).

There is another statement:
"if the magnetic field induces a small difference in temperature between the two faces, it would be enough to explain the difference in composition: on the hot side, the mixing region is not yet extended into the hydrogen layer and we still see hydrogen, while at the colder side mixing has already diluted the hydrogen and we see only helium."

The paper does not clearly demonstrate that we see only helium on one side and only hydrogen on the other side. Presenting some of the model fits to the spectra and providing upper limits on the H (He) abundance on the He (H) face would be very valuable.

- Pereira, Bergeron, Wesemael (2005)
https://ui.adsabs.harvard.edu/abs/2005ApJ...623.1076P/abstract
discussed the variability in GD 323 and considered specific geometries including pure helium equatorial belts and and pure hydrogen polar caps. If the H ocean idea shown in Extended Data Figure 5 is correct, this is exactly what you would expect, and it may help fit the observed spectral lines. The paper would benefit from a discussion of this possibility (e.g., some exploratory fits to the observed spectra may be helpful) and if there is any evidence of such a configuration in the spectroscopy data.

Unfortunately, the disagreement between the strength of the lines in the spectra on both sides and the temperature that we infer from the SED precludes us from this kind of modelling. This is because ratio of hydrogen to helium in a mixed atmosphere or the ratio of the surface covered by pure hydrogen vs pure helium in the case of a non-homogeneous atmosphere, can only be constrained by the strength of the lines of each element in the spectrum.

Minor comments:
"of the order of 1000 - 1300 km/s," >> "of the order of 1000 km/s,

We changed it in the text.

- Table 1 does not include a mass estimate for Janus. Please provide a mass estimate for CO and ONe core composition.

We added the estimate in the methods and in the table.

- "The absorption lines do not show any Zeeman splitting or wavelength shifts, so we know that the magnetic field on either face cannot be higher than a fewMG."

One can typically do better than a few MG based on low resolution spectra of DA white dwarfs. The way it is written, it is not clear if the authors have really tried to model the observed spectra and empirically determined that the field strength cannot be higher than some number or not.

Zeeman splitting depends strongly on the geometry of the magnetic field and sometimes splitting is not evident even at magnetic field strengths of a MG or more (especially in such weak and broad lines). For this reason, we cannot provide a strict upper limit on the field of the white dwarf. We added a small section in the methods, where we cite the example of WD J0103-0522: a white dwarf whose lines are blueshifted, indicating a field strength of about 5 MG, but that does not show any Zeeman splitting.

- "If we assume the magnetic field to be stronger on one side (as in a offset dipole structure)," G183-35 would be an excellent example of a star with a magnetic field stronger on one side, and could be mentioned in the discussion.

Thank you for the suggestions. Many magnetic white dwarfs show variation of field strength over the rotation period, but G183-35 is probably the nicest example. We added a mention in the paper.

- "Spectral variations, although not as extreme as in Janus, have been observed in another white dwarf, GD 323, which is also close to the red edge of the DB Gap"
There are other examples. For example Eisenstein et al. (2006) lists "An example near 45,000 K is PG1210+533, which shows variable H, He I, and He II line strengths, probably modulated with an (unknown) rotational period"

We added it to the paper.

- Figure 1 caption: The solid lines in the left panel are not described.

Thank you for catching this, we added it to the caption.

- Extended Figure 1:
The scale is different between the left and right panels. It would be better (for a fair comparison) to use the same y scale for all panels.

We changed the axis.

- Model fits: The authors use the Gaia parallax measurement of 2.5 +- 0.4 mas in their model fits. Bailer-Jones et al. (2021) give a geometric distance of 455 pc with 1sigma lower and upper limits of 374 and 666 pc. The distance estimate from Bailer-Jones seems better than a simple inversion of the Gaia parallax. Hence, a discussion of the 1/varpi versus the distance from Bailer-Jones et al. would be useful.

The distance that we get from the parallax and the distance from the Bailer-Jones catalog agree within the error bars, so not much would change in our calculations. Also, the Bailer-Jones distances are calculated assuming in the prior that the stars are main sequence stars, so it is unclear if they are better estimates for white dwarf's distances.

- The terms in Equation 1 on page 30 should be explained. For example, what is alpha_T?

We added a description of the terms.

Reviewer Reports on the First Revision:

Referees' comments:

Referee #1 (Remarks to the Author):

I thank the authors for addressing the concerns raised in my report. I am satisfied with the modifications they have made to the manuscript to address my comments and questions.

Regarding the appropriateness of Nature as a venue for this paper, my view is that the findings reported in this manuscript are not as ground-breaking as some recent white dwarfs related publications in Nature, such as the identification of the signature of core crystallization (Tremblay et al. 2019), the discovery of a giant planet candidate transiting a white dwarf (Vanderburg et al. 2020), or the detection of X-ray emission from a white dwarf accreting planetary material (Cunningham et al. 2022). But I realize that this is a subjective matter and an intrinsically editorial question.

Following the additions/modifications to the manuscript, I have two more comments to make.

(1) The authors invoke a magnetic field to explain the shallowness of the absorption lines compared to the model atmospheres. Magnetic fields are also known to have the opposite effect and create sharper, deeper line cores than predicted by non-magnetic models. A good example is LHS 2534, see Hollands et al. 2021 (Nature Astronomy, 5, 451). I think it is perfectly fine to speculate that magnetic fields are the culprit here, but the authors could acknowledge that the opposite is also very well possible.

(2) The authors give the example of J0103-0522 to explain how a multi-MG can be "hidden" and produce no visible Zeeman splitting. The interpretation according to which J0103 is a magnetic white dwarf is much less solid than what the current text suggests. Similar asymmetries and shifts of the Balmer lines have been observed in at least two other white dwarfs (GD16 and PG1157+004), both known to have He-dominated atmospheres. Spiegelman et al. 2022 (A&A, 659, 157) have shown that under those conditions, an unresolved blend of quasi-molecular line satellites due to high He densities can naturally explain the peculiar appearance of the spectral lines. This is a baseline effect that will impact the spectral lines of high-mass He-rich DAs. It does not require any special magnetic field geometry and therefore seems like a more natural explanation.

Referee #2 (Remarks to the Author):

The authors have responded to all of my comments and revised the paper accordingly. The newly added figures comparing the observed spectra with pure H, He, and mixed atmospheres, as well as the composite models diluted by a black body make the paper stronger.

I agree with the authors that this is an exciting discovery, and that finding the second example of a new class of objects will lead to significant advances in the field. I expect (and hope) that additional Janus-like objects will be discovered in the near-future, and this class of objects will help us understand the spectral evolution of white dwarfs.

Author Rebuttals to First Revision:

Response to Referee # 1

(1) The authors invoke a magnetic field to explain the shallowness of the absorption lines compared to the model atmospheres. Magnetic fields are also known to have the opposite effect and create sharper, deeper line cores than predicted by non-magnetic models. A good example is LHS 2534, see Hollands et al. 2021 (Nature Astronomy, 5, 451). I think it is perfectly fine to speculate that magnetic fields are the culprit here, but the authors could acknowledge that the opposite is also very well possible.

We thank the referee for pointing out the example of LHS 2534. We added a mention in the text.

(2) The authors give the example of J0103-0522 to explain how a multi-MG can be “hidden” and produce no visible Zeeman splitting. The interpretation according to which J0103 is a magnetic white dwarf is much less solid than what the current text suggests. Similar asymmetries and shifts of the Balmer lines have been observed in at least two other white dwarfs (GD16 and PG1157+004), both known to have He-dominated atmospheres. Spiegelman et al. 2022 (A&A, 659, 157) have shown that under those conditions, an unresolved blend of quasi-molecular line satellites due to high He densities can naturally explain the peculiar appearance of the spectral lines. This is a baseline effect that will impact the spectral lines of high-mass He-rich DAs. It does not require any special magnetic field geometry and therefore seems like a more natural explanation.

In the text we now clarify that there are alternative interpretation for the nature of J0103-0522.