Peer Review File

Manuscript Title: Magnetorotational hypernovae are another astronomical r-process site

Redactions – Mention of other journals

This document only contains reviewer comments, rebuttal and decision letters for versions considered at *Nature*. Mentions of the other journal have been redacted.

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referees' comments:

Referee #1 (Remarks to the Author):

The reviewer is Timothy Beers

I have reviewed this paper carefully, and believe that the results reported, strong similarities of the observed light and heavy chemical abundances to expectations from a rapidly rotating magnetorotational hypernova, are of sufficient importance and potential impact to merit publication in Nature.

I only have a few specific comments, which I urge the authors to consider in carrying out their revision.

1) Although this star has, apparently, the highest abundances of Th and U known for an r-process-enhanced star, and thus clearly a member of the class of stars known as "actinide-boost" stars, I did not see any mention of this fact in the draft. Since there are only a handful of such stars known (and this would be the most metal-poor of them) I find the oversight curious.

2) There is a recent paper by Placco et al. (2020, ApJ, 897, 78) of the r-process-enhanced star RAVE J183013.5..455510. This star, with [Fe/H] = -3.57, is also very N-rich, and likely Th-rich, but the latter result is only an upper limit due to the contamination from carbon, which is also quite high. Assuming the Th actually is high, this star would ALSO be an actinide-boost star. The high C is clearly in contrast to the star reported herein, which might be of interest. In any case, the authors of that paper ALSO suggest a similar mechanism (with detailed predictions from the model of Choplin et al. 2020 cited in the present manuscript) might well be responsible. So, I think that the authors should call out this star, and see if they can put it into the context of their claim for the SMSS target.

3) The authors should reconsider using the light green dashed line to represent the NSM prediction in Figure 2. Another color would be very much preferred, and far easier on the reader, especially

if they suffer from color blindness.

4) In Figure 4, the filled dots on the panels appear gray, not black, which should be fixed up as well. In this figure, one can also notice the similarity of several of the elements in CS 31082-001, also an actinide-boost EMP star, to the SMSS target, with the exception of N and Zn. This star also does not have high C. These interesting similarities (and differences) merit being called out in the main text, along with the Placco et al. result. In fact, I would encourage the authors to put in a panel showing the C abundances for the stars, including the RAVE target. Having error bars for the stars which are called out in the legend (including the Placco et al. star) would be useful as well.

The methods and other arguments from these authors appear sound to me, and are not in any way controversial.

Referee #2 (Remarks to the Author):

This article presents a detailed analysis of a unique star; an extremely metal-poor giant with chemical abundances that resemble the predicted pattern of a single, zero metallicity, magnetorotational hypernova (MRHN). The authors identify their analysis as the first evidence that MRHN are an additional r-process nucleosynthetic site, and which occur earlier than neutron star mergers, which makes this finding timely and original for publication in **[redacted].** It is worth noting that this field has had increased attention recently, with the confirmation of GW170817A as a binary neutron star merger, with a detected kilonova signature, thought to be associated with creation of the r-process elements.

This team is expert at best practices in analyzing the spectra of extremely metal-poor stars and interpreting them. The steps that they took to discover and analyze this object are clearly explained; starting from their SkyMapper photometric survey, followed by low resolution spectroscopy at the Siding Spring Observatory, followed by a high resolution spectrum taken with the Magellan Telescope, and (due to the recognized uniqueness of this star) they also obtained a high resolution, high signal-to-noise spectrum at the VLT using UVES to reach the most blue wavelengths to observe more of the r-process element spectral lines. The quality of their spectra is excellent, as is necessary to achieve high precision stellar abundances, which has been demonstrated by this team in the past. Important corrections for non-LTE effects have been included for several elements.

Uncertainties have been calculated in the stellar spectral parameters (effective temperature, surface gravity, microturbulent velocity, and metallicity), and element abundance uncertainties were obtained using "standard procedures". There is no doubt that this group is expert in these methods, and their standard procedures do follow best practices. A few points could be clarified for this manuscript though: (1) Were all of the uncertainties per element added in quadrature? (2) It is not clear if the Gaia parallax distance (and uncertainty) was used to select between the giant and dwarf solution from their isochrone fits. (3) How was the giant solution selected for this star, and could that have a larger systematic uncertainty in the analysis, than the Delta(log g)= +/-0.3 that was adopted for the errors analysis?

The "Theoretical Modelling" in the Methods section is important and well explained. The authors detail their calculations and assumptions for comparisons with 25-40 solar mass MRHN models, with a 15 solar mass supernova, and with a neutron star merger that occurs in a pre-enriched Galaxy (time delayed model). The authors are clear that the 25 solar mass MRHN has the best fit statistical fit to the data, and it is interesting that this is true for their preferred elements, for all of the elements, and even with/without their non-LTE corrections. One question that arises from these comparisons is (1) could a few stars may have contributed to the abundance pattern, perhaps with the MRHN dominating? This is discussed in the literature, as star formation tends to form groups of stars, which also help to retain the expelled gas used in the next generation of star formation (i.e., for this star). A second question arises between lines 401-423, where the details of the MRHN model yields are discussed; (2) several assumptions need to be made to scale the models for this EMP star, which seem reasonable, but are they consistent with one another. For example, if all were incorporated simultaneously (rapid rotation can affect core masses, leading to different explosion energies, symmetries, mixing, etc.), could different yields be encountered and how is that currently folded into the errors (or fitting) analysis?

Overall the paper and methods sections are concise and clear. The authors have selected an appropriate set of references to previously published work, especially given that there is an enormous amount of literature on these subjects.

Author Rebuttals to Initial Comments:

AUTHORS

We appreciate the helpful comments and questions from the two referees.

REFEREE #1

1) Although this star has, apparently, the highest abundances of Th and U known for an r-process-enhanced star, and thus clearly a member of the class of stars known as "actinide-boost" stars, I did not see any mention of this fact in the draft. Since there are only a handful of such stars known (and this would be the most metal-poor of them) I find the oversight curious.

AUTHORS

The actinide boost nature of this star was noted in the Methods, line 386. We initially included this in the main text, but it added no additional information and the age estimate is not a useful constraint. We have added text noting that our object is the most iron-poor of the actinide boost stars, but prefer to keep this in the Methods section.

REFEREE #1

2) There is a recent paper by Placco et al. (2020, ApJ, 897, 78) of the r-process-enhanced star RAVE J183013.5..455510. This star, with [Fe/H] = -3.57, is also very N-rich, and likely Th-rich, but the latter result is only an upper limit due to the contamination from carbon, which is also quite high. Assuming the Th actually is high, this star would ALSO be an actinide-boost star. The high C is clearly

in contrast to the star reported herein, which might be of interest. In any case, the authors of that paper ALSO suggest a similar mechanism (with detailed predictions from the model of Choplin et al. 2020 cited in the present manuscript) might well be responsible. So, I think that the authors should call out this star, and see if they can put it into the context of their claim for the SMSS target.

AUTHORS

We appreciate being made aware of this interesting object but believe that the two objects are rather different. While they both have comparable metallicity and high [N/Fe], there are some major differences. As noted by the referee, the C abundances are clearly different between the two stars. The Th limit is [Th/Fe] < +1.35 compared to the Th measurement in SMSS 2003-1142; [Th/Fe] = +2.10. So while the RAVE target may be another actinide boost star, Th is not detected. Eu is also different by an order of magnitude; [Eu/Fe] = +0.69 vs. +1.70. While the RAVE object is interesting and worth mentioning, it is not obvious how to make any clear connection between the two stars. We have updated the manuscript to include the text below.

"Production of neutron-capture elements by the explosion of a massive rotating star has also been suggested as one of the possible explanations of the abundance distribution in the r-process rich star RAVE J183013.5-455510[47]. That object, however, has considerably different abundances when compared to SMSS 2003-1142, e.g., SMSS 2003-1142 has much lower C and higher Eu and Th abundances."

REFEREE #1

3) The authors should reconsider using the light green dashed line to represent the NSM prediction in Figure 2. Another color would be very much preferred, and far easier on the reader, especially if they suffer from color blindness.

AUTHORS

We appreciate the suggestion and have selected new colors for this figure.

REFEREE #1

4) In Figure 4, the filled dots on the panels appear gray, not black, which should be fixed up as well. In this figure, one can also notice the similarity of several of the elements in CS 31082-001, also an actinide-boost EMP star, to the SMSS target, with the exception of N and Zn. This star also does not have high C. These interesting similarities (and differences) merit being called out in the main text, along with the Placco et al. result. In fact, I would encourage the authors to put in a panel showing the C abundances for the stars, including the RAVE target. Having error bars for the stars which are called out in the legend (including the Placco et al. star) would be useful as well.

AUTHORS

We have updated the figure to have black dots, the C abundance and the new RAVE target. While CS 31082-001 has some similarities, this figure confirms that SMSS 2003-1142 is unique in terms of N and Zn, which is the unique signature of the magnetorotational hypernova. We prefer not to delve any further into detailed comparisons with individual objects.

REFEREE #2

Uncertainties have been calculated in the stellar spectral parameters (effective temperature, surface gravity, microturbulent velocity, and metallicity), and element abundance uncertainties were obtained using "standard procedures". There is no doubt that this group is expert in these methods, and their standard procedures do follow best practices. A few points could be clarified for this manuscript though: (1) Were all of the uncertainties per element added in quadrature? (2) It is not clear if the Gaia parallax distance (and uncertainty) was used to select between the giant and dwarf solution from their isochrone fits. (3) How was the giant solution selected for this star, and could that have a larger systematic uncertainty in the analysis, than the Delta(log g)= +/-0.3 that was adopted for the errors analysis?

AUTHORS

1. Errors were added in quadrature.

2. Gaia parallaxes were not included in the analysis. The dwarf/giant discrimination is readily provided from the low-resolution spectra. The EDR3 parallax indicates an absolute magnitude in G of -0.4, which confirms the “giant” solution.

3. The surface gravity was adopted using isochrones as described in the text. In this case, the dwarf gravity is 4.6 and the giant gravity is 2.4. The ANU 2.3m spectra readily allow us to discard the dwarf solution.

We have updated the text accordingly.

REFEREE #2

The "Theoretical Modelling" in the Methods section is important and well explained. The authors detail their calculations and assumptions for comparisons with 25-40 solar mass MRHN models, with a 15 solar mass supernova, and with a neutron star merger that occurs in a pre-enriched Galaxy (time delayed model). The authors are clear that the 25 solar mass MRHN has the best fit statistical fit to the data, and it is interesting that this is true for their preferred elements, for all of the elements, and even with/without their non-LTE corrections. One question that arises from these comparisons is (1) could a few stars may have contributed to the abundance pattern, perhaps with the MRHN dominating? This is discussed in the literature, as star formation tends to form groups of stars, which also help to retain the expelled gas used in the next generation of star formation (i.e., for this star).

AUTHORS

In response to this point, we added some discussion on this in the Methods section, and added a brief sentence in the main text as well. Note that in many previous works on the abundance fitting of EMP stars (e.g., Ishigaki et al. 2018), it has been assumed that there is only one enrichment source at this metallicity.

REFEREE #2

A second question arises between lines 401-423, where the details of the MRHN model

yields are discussed; (2) several assumptions need to be made to scale the models for this EMP star, which seem reasonable, but are they consistent with one another. For example, if all were incorporated simultaneously (rapid rotation can affect core masses, leading to different explosion energies, symmetries, mixing, etc.), could different yields be encountered and how is that currently folded into the errors (or fitting) analysis?

AUTHORS

It will be not possible, even in the near future, to incorporate all of these effects simultaneously in simulations due to the different timescales and dynamic ranges. Our required conditions for MRHN are not inconsistent with one another, and we believe our model is reasonable. The detailed abundance pattern could be different in (future) self-consistent simulations; this point has been discussed in the Methods section (in relation to Ye) but we made this aspect clearer in the revised paper. We also added a brief sentence in the main text as well.