Peer Review File

Manuscript Title: Observation of the Antimatter Hypernucleus 4 Λ̄H

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referee #1 (Remarks to the Author):

General:
The paper is rather well written and conveys a clear and original message. It reports the first observation of the antihyperhydrogen-4 (a bound state of two neutrons, a proton and a Lambda hyperon), the currently heaviest antinucleus measured.
The STAR Collaboration uses for these results data from Au+Au, U+U, Ru+Ru and Zr+Zr collisions measured at the BNL RHIC accelerator complex. The evidence for the observed signal is estimated by a p-value method and leads to a significance of 4.7sigma, which is in the high-energy physics language just slightly below the significance of 5sigma, that is needed to claim the discovery of a new particle. In the case of the antihyperhydrogen-4, the particle itself was known since long and the existence of the antiparticle state is expected. Therefore, the significance of the signal here should be sufficient.
In addition to the pure evidence of the signal, the paper reports the lifetime measurements of hypertriton, antihypertriton, hyperhydrogen-4 and antihyperhydrogen-4 and by that checks the CPT symmetry (fundamental symmetry in quantum physics that expects properties of matter and antimatter to be equal). The measured lifetime of hypernuclei and antihypernuclei agree in uncertainties.
Furthermore, the production mechanism of these objects are studied by comparison of some production yield ratios (ratios of rapidity densities) with predictions from a thermal model.

The manuscript requires some changes to be suitable for the broad readership of Nature. For instance some parts are still too detailed and should go to the methods part, e.g. lines 46-48 and lines 65-66. In contrary, some information is missing, like the formula for the invariant mass, that is known to most physicists but a repetition at least in the methods would be good. For non-physicists the words “energy and momentum conservation“ help to set the quantity in the proper context. There are also some inconsistencies in the language usage, e.g. hyphenation is not used consistently throughout the manuscript - for example (line 6 should read): High-energy nuclear-collision rate (see for instance: https://www.grammarbook.com/punctuation/hyphens.asp).

The article emphasizes the importance of the existence of antimatter and the asymmetry at the beginning but does not come back to that point at the end. A closed story loop is normally nicer for a general readership. Namely, that the initially raised questions are somehow answered - at least to some extend.

For me a clear conclusion on the production mechanism is needed: does the thermal model work for the hyperhydrogen-4 or is coalescence the proper model? If the latter is the case, then I would expect some additional supporting material, e.g. model calculations that work better than the thermal model. To phrase it differently, the production mechanism section is the weakest part of the manuscript and one could opt that this is actually not needed here. The observation of the antihyperhydrogen-4 and the CPT tests would be sufficient for a publication for a general scientific readership.

Detailed comments on the manuscript:

I am a bit confused about the Data availability statement and the Code availability statement. Data at CERN and particular the LHC data is supposed to be available for the public, even with some delay. The code of STAR is available through https://www.star.bnl.gov/webdata/dox/html/index.html or am I wrong here?

References:

Please check refs. 42 and 43 where the symbols of the hypernuclei look odd.
Ref 46 should read Rayet, M. and Dalitz, R.H.
Ref. 50 should read Hildenbrand AND since that are only two authors it should be Hildenbrand, F. and Hammer, H.-W.

Some references are missing:

You missed the STAR search upper limit paper on strangelets (suppose to be around ref. 58) - even if it is rather old, there is no better upper limit from colliders yet:
@article{STAR:2005wmk,
author = "Abelev, B. I. and others",
collaboration = "STAR",
title = "{Strangelet search at RHIC}",
eprint = "nucl-ex/0511047",
archivePrefix = "arXiv",
doi = "10.1103/PhysRevC.76.011901",
journal = "Phys. Rev. C",
volume = "76",
pages = "011901",
year = "2007"
}

On the Kalman Filter package I would propose this two additional references:
@article{Ju:2023xvg,
author = "Ju, Xin-Yue and others",
title = "{Applying the Kalman filter particle method to strange and open charm hadron reconstruction in the STAR experiment}",
doi = "10.1007/s41365-023-01320-1",
journal = "Nucl. Sci. Tech.",
volume = "34",
number = "10",
pages = "158",
year = "2023"
}

@article{Ablyazimov:2015pka,
author = "Ablyazimov, T. O. and Zyzak, M. V. and Ivanov, V. V. and Kisel, P. I.",
title = "{Kalman filter-based fast track reconstruction for charged particles in a Compressed Baryonic Matter experiment using parallel computing on a multicore server at the Laboratory of Information Technologies, Joint Institute for Nuclear Research}",
doi = "10.1134/S1547477115030012",
journal = "Phys. Part. Nucl. Lett.",
volume = "12",
number = "3",
pages = "423--427",
year = "2015"
}

In principle one could also reference to http://www.gsi.de/documents/DOC-2007-May-14-1.pdf

The hypernuclei community has its own data base (supposed to be equivalent to PDG, CODATA and BNL Nuclear Data Center):
@misc{HypernuclearDataBase,
title = {Chart of Hypernuclides --- {H}ypernuclear Structure and Decay Data},
author = {P. Eckert and P. Achenbach and others},
year = 2021,
note = {\href{https://hypernuclei.kph.uni-mainz.de}{hypernuclei.kph.uni-mainz.de}}
}

Bethe-Bloch - since you have some references on the origin of antimatter production, it might be good to have also the original sources of the energy-loss formula:

```
@article{bethe,
author = {H. Bethe},
title = {{Bremsformel f\"ur Elektronen relativistischer Geschwindigkeit}},
volume = {76},
pages = {293},
year ={1932},
journal = {Z. Phys.}
}

@article{bloch,
author = {F. Bloch},
title = {{Zur Bremsung rasch bewegter Teilchen beim Durchgang durch Materie}},
volume = {408},
pages = {285},
year = {1933},
journal = { Ann. Phys.}
}
```

As review (summarizing run1 results) on antihypernuclei at the LHC I would suggest:

```
@article{Braun-Munzinger:2018hat,
author = {Braun-Munzinger, Peter and D\"onigus, Benjamin},
title = "{Loosely-bound objects produced in nuclear collisions at the LHC}",
eprint = "1809.04681",
archivePrefix = "arXiv",
primaryClass = "nucl-ex",
doi = "10.1016/j.nuclphysa.2019.02.006",
journal = "Nucl. Phys. A",
volume = "987",
pages = "144--201",
year = "2019"
}
```

and there is an update for run2:

```
@article{Donigus:2020fon,
author = {D\"onigus, Benjamin},
title = "{Selected highlights of the production of light (anti-)(hyper-)nuclei in ultra-relativistic heavy-ion collisions}",
doi = "10.1140/epja/s10050-020-00275-w",
journal = "Eur. Phys. J. A",
volume = "56",
number = "11",
pages = "280",
year = "2020"
}
```

Importance of excited states of nuclei (in the thermal model) is discussed here:
@article{Vovchenko:2020dmv,
author = {Vovchenko, Volodymyr and D\"onigus, Benjamin and Kardan, Behruz and Lorenz, Manuel and Stoecker, Horst},
title = "{Feeddown contributions from unstable nuclei in relativistic heavy-ion collisions}",
eprint = "2004.04411",
archivePrefix = "arXiv",
primaryClass = "nucl-th",
doi = "10.1016/j.physletb.2020.135746",
journal = "Phys. Lett.",
volume = "B",
pages = "135746",
year = "2020"
}
and the only paper naming clearly the need of taking the excited state of the hyperhydrogen-4 into account is this:
@article{Donigus:2023hiw,
author = {D\"onigus, Benjamin},
title = "{Hypernuclei at relativistic energies}",
doi = "10.1051/epjconf/202327604002",
journal = "EPJ Web Conf.",
volume = "276",
pages = "04002",
year = "2023"
}

Detailed comments on the text:
line 6: High energy nuclear collision rate -> High-energy nuclear-collision rate
line 11: ultrarelativistic heavy ion collisions -> ultrarelativistic heavy-ion collisions
Figure 1: “lineage of antimatter nuclear cluster“ is a bit misleading since the positron is also indicated and antiproton and antineutron are nucleons but definitely not nuclear clusters, perhaps antinuclear constituents and clusters (?)
line 32: heavy ion collisions -> heavy-ion collisions
line 34: not clear if strange quarks are known to a general audience
lines 41/42: relativistic heavy ion collisions -> relativistic heavy-ion collisions
lines 44/45: center of mass energy -> center-of-mass energy
lines 46-48: The data sets and triggers used are too detailed for the text - suggest to move this to the methods
lines 56/57: Bichsel function is a specific method to parametrize the Bethe-Bloch formula, that describes the energy loss, and might be known to non-physicists as well, better name the latter here and put the Bichsel function to the methods - that applies also to the caption of Figure 2
Figure 2: (A) <dE/dx> versus -> (A) Average energy loss <dE/dx> versus
lines 65/66: usage of the Kalman-Filter package is too detailed, better move to methods
lines 69-72: might be confusing even for a physicist that is outside high-energy physics

line 74: invariant mass spectra -> invariant-mass spectra
line 75: invariant mass distributions -> invariant-mass distributions
lines 74-76: here I would mention that the calculation is based on energy and momentum conservation - and in the methods please put the formula
Figure 6: Invariant mass distributions -> Invariant-mass distributions
Figure 6: invariant mass regions -> invariant-mass regions
Figure 6: how are the uncertainties of the background calculated? Even when they are determined by the rotated background they look too small somehow; in particular, looking at the signal region of the two lower plots (4LH and anti4LH) there seems to be some structure…
lines 84-90: did you try to use the asymptotic calculator from RooFit/RooStat? That should give a better estimate than the S/sqrt(S+B) formula and is commonly used and accepted for discoveries at the LHC
line 92: early -> initial (?)
line 96: CPT theorem might not be known to everyone, perhaps invert the logic: quantum physics expects matter and antimatter to have the same properties and then name it CPT theorem
line 105-109: what is written there is true, but requires quite some modeling - perhaps mention that here
line 110: lifetimes of (anti)hypernuclei 3LH, 4LH, anti3LH and anti4LH are -> lifetimes of the (anti)hypernuclei 3LH, 4LH, anti3LH and anti4LH are (the is needed since you particular hypernuclei here)
line 114/116: what nominal mass m is used here? The one extracted from the invariant mass, some literature value or the Mainz data base value (that might be good to cite somewhere)?
line 122: formula is actually inconsistent with itself (last part is not depending on t) - implicit conversion from t to L, which is actually the investigated quantity
lines 122-124: might be confusing for non-experts
lines 127/128: needs brackets before 16 and after 10(sys.) & before 18 and after (46(sys.) since otherwise only the last value (the systematic uncertainty) has the unit picoseconds
Figure 4: why only statistical uncertainties in (A)? What generates the difference in dN/d(L/betagamma) for particle and antiparticle? Is it only muB?
Figure 4 (B): how is the average lifetime calculated (PDG routines?)? Do they take into account the newly presented results? The free L lifetime needs a reference (PDG presumably)
lines 131-132: both statements are rather vague and not useful for the broad audience that is expected to read this article
line 133: relativistic heavy ion collisions -> relativistic heavy-ion collisions
line 137: heavy ion collisions -> heavy-ion collisions
lines 145/146: these quantum numbers are also important for the thermal model, only the mass is needed in addition there
line 148: relativistic heavy ion collisions -> relativistic heavy-ion collisions
line 152: also a rather vague statement that is not helping a non-expert readership
line 155: 1.5c is very misleading! The mentioned quantity is actually beta_T x gamma and that is not having any unit per se, better remove the c here on both sides
line 165: again very vague
line 168: what does it mean that the Au+Au results are different now (by 1.9sigma)? Is there any understanding or do you think it is of purely statistical origin?
Figure 5: vertical bars and boxes -> vertical bars and shaded boxes

Figure 5: The comparison with the ALICE results is confusing since there the muB is close to zero and the temperature is sightly lower there AND the reference in the legend is wrong, the ratios of the hypertriton are shown in PLB (ref. 20)
Figure 5: Are the thermal model lines for the (anti)hyperhydrogen-4 with or without the excited state? If so the legend and/or the caption should mention that the predictions have been modified accordingly (ref. 53 did not take the excited states into account)
line 170: antimatter-over-matter particle yield ratios -> antimatter-over-matter particle-yield ratios

Methods:
There are some details missing that would help the reader to understand the actual measurements better. For instance the non-photonic electron trigger is not known to many people. If you don't want to extend the description, then perhaps consider some additional reference(s).
Are the different triggers properly simulated? If so, how much do the different triggers actually bias and is that taken into account in the systematical uncertainties?

Is there any systematic uncertainty added for the absorption of antinuclei and antihypernuclei? Since the hypertriton is large (ultimate halo nucleus) this should be mentioned at least in the methods
somehow.

lines 46ff.: The rotational method details should be added here (angles and number of trials) and the actual estimation of the uncertainty of the background should also be explained a bit. As mentioned above, the fluctuations of the rotational background is definitely underestimated in the numbers given in Fig. 3 of the manuscript. Please add somewhere the formula for the invariant mass.

Table 1: The cuts on the chi2 values from the Kalman Filter package are not straightforward to understand. Could you add a supporting plot or point to a proper reference?

lines 61ff.: Perhaps you mention the p-value in the actual manuscript. Is known by all scientist that apply statistical methods normally.

Figure 2: Why is the efficiency of (anti)hyperhydrogen so much smaller than the one of the (anti)hypertriton? In principle, one would assume the (anti)alpha daughter would be cleaner and have a high efficiency.

The lifetime shown in Fig. 4 is that the value given in line 104? If yes, which one Lambda or anti-Lambda?

Figure 3: Did you apply any check, e.g. look-elsewhere effect? There are some fluctuations visible that also have a relatively high significance…

Absorption is not mentioned at all as source of the uncertainty, is that somehow incorporated? If yes, please mention otherwise try to estimate it under some assumptions.

Detailed comments:
line 19: inconsistency: anti-nuclei before antinuclei
line 33: distance of closest approach -> distance-of-closest approach
Table 1: He DCA < 1 cm -> shouldn't it be larger than some value?
line 46: invariant mass distribution -> invariant-mass distribution
line 78: (Anti-)hypernuclei before always (Anti)hypernuclei
line 92: invariant mass distribution -> invariant-mass distribution
Fig. 3: in particular the important antihyperhydrogen-4 plots are not as convincing as the figure in the manuscript
Fig. 4: originates the difference between particle and antiparticle here only from the baryo-chemical potential?
line 107: the high-statistics data from the J-PARC emulsion experiment show some cascade like decays also in 4LH and 3LH, but presumably the analysis presented here is safe, since the mass of the multistrange hypernuclei decaying into 4LH and 3LH are higher and thus there production is suppressed compared to the 4LH and 3LH
line 110: |Rapidity| -> |y|
line 110: same issue as in the manuscript pT/m is actually the transverse betagamma and that has no unit (1.5c does not make sense here)
Table 2: the small uncertainties of the background counts are not clear
line 113: was confused by M^2/Q^2 for some time, Q is supposed to be Z, as in Fig. 2 of the manuscript - only then the unit makes sense. Actually, would be good to mention that this is calculated from the time-of-flight information and the measured momentum.
line 114: better use a symbol for efficiency, e.g. \epsilon - mainly because the 1/epsilon is easier to read (and understand) than 1/efficiency weight
line 116: 1/\epsilon weight
line 117: invariant mass distribution -> invariant-mass distribution
line 125: R is 10 fm -> not clear why that should be. For all discussed collision systems?
Fig. 5: Style is compeltely different compared to all other figures, please unify - STAR Collaboration before was only STAR
Table 3: What are the corresponding uncertainties?

Referee #2 (Remarks to the Author):

In the present manuscript the authors report on the first experimental observation of the A=4 anti-4-Lambda-H antihypernucleus, together with measurement of its lifetime, in relativistic heavy-ion collisions. Also presented in this work are new measurements of the A=3 3-Lamba-H, anti-3-Lambda-H, and 4-Lambda-H lifetimes. By comparing the lifetimes of A=3(4) hypernuclei and their corresponding antihypernuclei, no violation of CPT (matter-antimatter) symmetry is observed.

This is certainly an impressive achievement, representing a great technical progress in the field. However, in my opinion, it is a bit unfortunate to frame the whole analysis in the context of CPT symmetry breaking. Given the large reported experimental uncertainties due to the (expected) small number of detected events for A=4 anti-4-Lambda-H and 4-Lambda-H, such a measurement (likely) will not have a significant impact on CPT violation studies in strong-interaction baryonic sector.

Moreover, the paper is very technical and I doubt that it is accessible for anybody outside of the high-energy collisions community. A better ballance between explaining the basic concepts and providing details of the data analysis should be found. In particular, I have difficulties in understanding how exactly the (anti)hypernuclear lifetimes were obtained from their two-body charged mesonic decay modes. Do the "efficiency-corrected yields" also contain the two-body branching ratios (BRs) to account for the remaining (anti)hypernuclear decay modes? I might be completely confused. The additional decay modes contributing to the lifetimes and BRs are mentioned for the first time in the later section on yield ratios. There, the sentence is also quite ambiguous and could be misinterpreted that the BRs were measured in Ref. [42] which is not the case. The branching ratios are quite poorly known and the adopted value for A=3 of R3=0.25 seems to come from [48] which is a (although comprehensive) theoretical calculation using rather dated input nuclear and hypernuclear interactions available in 1998. This might be a coincidence and Ref. [42] does not seem to be very helpful in tracking down the origin of the adopted value. Similarly, the BR for 4-Lambda-H was measured in emulsion and bubble-chamber experiments during 1961-1970 and the quoted uncertainties are purely statistical. The approach in Ref. [42] was to consider and propagate quite significant uncertainties in BRs. It seems that it was not done in the present work. Certainly, a more detailed discussion would be appropriate. Moreover, it is to be noted that equal two-body BRs for hypernuclei and antihypernuclei were tacitly assumed, while searching for violation of matter-antimatter symmetries later. In this context I would like to mention the recenlty published extension of Ref. [50] on hypertriton lifetime: [F. Hildenbrand, H. W. Hammer, Eur. Phys. J. A 59, 280 (2023)], as well as additional recent comprehensive analysis in [A. Pérez-Obiol, D. Gazda, E. Friedman, A. Gal, Phys. Lett. B 811, 135916 (2020)] and [D. Gazda, A.~Pérez-Obiol, A. Gal, E. Friedman, arXiv:2310.03087 [nucl-th]].

I also think that a more detailed discussion of the obtained lifetime values would be appropriate. Especially for the case of 3-Lambda-H and 4-Lambda-H hypernuclei which can be compared with the previous measurements. Although only their differences with respect to the anti-hypernuclear counterparts are given in the text, their absolute values (shown in Fig. 4) differ from STAR Collaboration's own measurements from the last year [42] and from 2018 [41].

Referee #3 (Remarks to the Author):

I would like to congratulate the authors for analyzing such a huge amount of data and tackling the problem of the hyper-nuclei and antihypernuclei production in heavy ion collisions.
The paper presents the measurements of hyper-nuclei and antihypernuclei by the STAR experiment, including the first observation of the antiH4L, the heaviest antihypernucleus observed so far. The analysis spans over four different collisions systems (AuAu, UU, RuRU, and ZrZr) in order to get a large enough signal of the antiH4L to claim its observation. While the title focus on the discovery of H4L, the measurement and discussion of the production of H3L is also very interesting and important.

The findings are both intriguing and significant. However, as demonstrated in Figure 1 of the authors' paper, the groundwork for uncovering heavier anti(hyper)nuclei was already laid by their previous discovery of antiHe4, as published in Nature 473, 353. In my view, this paper represents a notable yet incremental advancement, making it more suitable for a specialized physics journal such as Nature Physics.

Finally, I have some questions about the analysis and the presentation of the results that I would like to see addressed before the publication:
- it is mentioned in the manuscript that the detection efficiency is obtained from a GEANT3 simulation. Already for the antinuclei (hence the daughter particles of the antihypernuclei), we know that GEANT3 is not an appropriate transport code without any tuning. Were the interaction cross sections implemented in GEANT3 for antinuclei modified to reproduce the available measurement of antinuclei interaction cross sections with material? Alternatively, what is the effect of using GEANT4, that has a representation of the interaction cross sections closer to the measurements? Was there any study on what is the effect of the interaction cross section of the H3L and H4L on the efficiency? All these questions are relevant for the efficiency correction and its systematic uncertainty.
- Is the uncertainty on the B.R. of the hyper-nuclei considered in the systematic uncertainty?
- The H3L lifetime is considerably larger than in the previous paper from STAR (PRC(2018) in the figure), given that the detector is the same, I guess most of the systematic would cancel out. How much is the difference in terms of number of sigmas? Is there any explanation for this difference?
- the rotational background for antiH4L presents a structure close to the peak (see for instance panel D of Figure 3): given that with rotational background the statistics could be improved, are these present as well when enhancing the statistics?
- In the comparison with the previous measurements there is no mention of the treatment of the systematic uncertainties (line 168, for instance), were this canceled out or not? It would be good to comment on the treatment of such cases at least in the method section.
- It looks like that for antiHe4/He4 the Gaussian error propagation has been used for the old results, this looks not appropriate given that the ratio becomes compatible with 0 but the antiHe4 yield is not compatible with 0.
- The PV is requested to be within 2 cm in radius from the beam line, is this the beam spread or due to the PV resolution? What is the secondary vertex reconstruction spatial resolution and how does this compare with the binning used for the lifetime analysis?

Comments on the text:

- General: there is a lot of emphasis in the abstract and throughout the text on the similarities with the early Universe. I think that this is not the main point of the paper and it should be tuned down and/or revised. There are many differences between the early Universe and the collisions at RHIC: the matter-antimatter asymmetry we observe in HIC is due to the baryon transport from beam to mid-rapidity, while the puzzle of the matter dominance in the universe concerns the baryogenesis and possible CPT violation mechanisms and the matter-antimatter balance changes quite a lot depending on the rapidity where we perform measurements in HIC.

Figure 1: while I understand that this is a schematic figure with selected antiparticles, it looks like this is cherry-picking to show an increase of mass with time. However, when including all antibaryons (or even the heaviest antiparticle seen, the tbar) this picture would change a lot. I would suggest to either remove this figure or to remove the positron to keep only the antinuclei/antihypernuclei.

- line 28: "Antimatter readily annihilates with matter, making it difficult to create antimatter nuclear clusters in the Universe" -> "Antimatter readily annihilates with matter, making it difficult to *observe* antimatter nuclear clusters in the Universe"
- Figure 2A: the bands for nuclei are not visible for data, only the expected values are visible. This is not the case in the paper on the antialpha discovery. What is the source of the large background? It would be good to explain this in the text or in the caption.
- Line 71: "This bias due to fluctuations is much smaller for 3Î›Â¯H because of its large signal significance", the bias is smaller, but given the impact of this paper it is necessary to either quantify it with a systematic uncertainty or avoid it altogether by optimizing the selections on a MC for H3L.
- Line 97: you introduce here the acronym for CPT, but the first time it was mentioned was on line 39. It would be good to introduce it earlier.
- Line 165-167: this is such an interesting point that I suggest to elaborate on that. Is the trend going from small to large collision system significant when canceling out the correlated sources of systematic uncertainties?
- Line 192: here you mention the discrepancy of the H3L/He3 from the SHM and you mention that this could be due to the low binding energy. I would request a quantification of the discrepancy in terms of number of sigmas and to mention the role of the wave function in the coalescence model, that indeed could explain this discrepancy (see e.g. https://arxiv.org/abs/1807.05894)
- "According to the coalescence model and statistical thermal model, antinucleus and antihypernucleus production yields decrease by about 3 orders of magnitude for each additional atomic
mass number" could you add a reference to a coalescence model prediction where this penalty factor is predicted? (for example https://arxiv.org/abs/1512.00692)

Comments on the additional material:

- Table 1: the selections look all reasonable, but that on the chi2_primary for He3. What is the meaning of a selection of Chi2_primary<2000?
- Line 105: "However, they are slightly longer than the value from the Particle Data Group 263Â±2 ps. This is expected because the V0DCA cut can not exclude all Lambda from Xi and Omega decays." I would expect that the feed-down would also spoil the exponential distribution, do you have a MC study that substantiates this statement?
- Figure 5: the systematic uncertainties on the single pt bins are fluctuating bin by bin, this is not expected. Do you have an explanation for this?

Author Rebuttals to Initial Comments:

We thank the referees for reviewing our paper and giving the helpful questions, comments and suggestions. They help us improve the paper in many aspects, ranging from the analysis procedures to the language as well as fitting the general audience of Nature. Along with the updated paper draft and Methods, we have prepared the replies to the questions and comments below. The original referees' comments are in black, while the replies to each point are in blue.
Thank you.

Referee #1 (Remarks to the Author)

General:
The paper is rather well written and conveys a clear and original message. It reports the first observation of the antihyperhydrogen-4 (a bound state of two neutrons, a proton and a Lambda hyperon), the currently heaviest antinucleus measured.
A: Thanks.

The STAR Collaboration uses for these results data from Au+Au, U+U, Ru+Ru and Zr +Zr collisions measured at the BNL RHIC accelerator complex. The evidence for the observed signal is estimated by a p-value method and leads to a significance of 4.7sigma, which is in the high-energy physics language just slightly below the significance of 5sigma, that is needed to claim the discovery of a new particle. In the case of the antihyperhydrogen-4, the particle itself was known since long and the existence of the antiparticle state is expected. Therefore, the significance of the signal here should be sufficient.
In addition to the pure evidence of the signal, the paper reports the lifetime measurements of hypertriton, antihypertriton, hyperhydrogen-4 and antihyperhydrogen-4 and by that checks the CPT symmetry (fundamental symmetry in quantum physics that expects properties of matter and antimatter to be equal). The measured lifetime of hypernuclei and antihypernuclei agree in uncertainties.
Furthermore, the production mechanism of these objects are studied by comparison of some production yield ratios (ratios of rapidity densities) with predictions from a thermal model.

The manuscript requires some changes to be suitable for the broad readership of Nature. For instance some parts are still too detailed and should go to the methods part, e.g. lines 46-48 and lines 65-66. In contrary, some information is missing, like the formula for the invariant mass, that is known to most physicists but a repetition at least in the methods would be good. For non-physicists the words "energy and momentum conservation" help to set the quantity in the proper context. There are also some inconsistencies in the language usage, e.g. hyphenation is not used consistently throughout the manuscript - for example (line 6 should read): High-energy nuclear-collision rate (see for instance: https://www.grammarbook.com/punctuation/hyphens.asp).

A: Thank you. Some details are removed from the main draft. Explanation of e.g. invariant mass are added. The use of hyphenation is checked and standardized. But in Line 6, it's "create" rather than "rate", so the hyphenation between "nuclear" and "collisions" is not needed.

The article emphasizes the importance of the existence of antimatter and the asymmetry at the beginning but does not come back to that point at the end. A closed story loop is normally nicer for a general readership. Namely, that the initially raised questions are somehow answered - at least to some extend.

A: The paper's very first sentence is rewritten so that it emphasizes the importance of the study on antimatter, rather than only on matter-antimatter asymmetry. The theme of the whole paper, as written in the title, is the observation of this new antimatter particle, plus some studies of its properties, including the CPT symmetry test through its lifetime measurement. At the end of the "Lifetimes and matter-antimatter symmetry test" section, it is stated that the lifetime differences between antihypernuclei and hypernuclei are consistent with zero, which is a new test of the matter-antimatter and CPT symmetry.

For me a clear conclusion on the production mechanism is needed: does the thermal model work for the hyperhydrogen-4 or is coalescence the proper model? If the latter is the case, then I would expect some additional supporting material, e.g. model calculations that work better than the thermal model. To phrase it differently, the production mechanism section is the weakest part of the manuscript and one could opt that this is actually not needed here. The observation of the antihyperhydrogen-4 and the CPT tests would be sufficient for a publication for a general scientific readership.

A: The "yield ratios" section is rewritten, so that it does not sound like we are trying to distinguish either the coalescence or thermal model works better. Instead, coalescence is introduced as a commonly accepted physics picture of (anti)hypernuclei and (anti)nuclei production, while the statistical thermal model can also describe the production yields well with limited parameters. And our measurement is consistent with the expectations of both the coalescence picture and the statistical thermal model. The section is also shorter, getting rid of some unnecessary details, and adding some physics background so the general readers can understand more easily.

Detailed comments on the manuscript:

I am a bit confused about the Data availability statement and the Code availability statement. Data at CERN and particular the LHC data is supposed to be available for the public, even with some delay. The code of STAR is available through https://www.star.bnl.gov/webdata/dox/html/index.html or am I wrong here?

A: We confirmed the STAR routine about Data and Code availability and modified the Code Availability.

Here the raw data are not available to the public. These include the "DAQ" data directly obtained by the Data Acquisition system during the experiment, and the offline processed data ("MuDst" and "picoDst" in STAR jargon), which include for example the reconstructed charged particle track information. "DAQ" files can not be read without specific STAR software. "MuDst" and "picoDst" files ideally can be read by anyone who knows CERN ROOT software, but still need quite complicated skills to analyze in order to obtain right physics results. Similar to CERN experiments, STAR does not make the raw, detector data available to the public. The experiment's host facility, however, keeps this data available for collaboration. The code to process this raw data is publicly accessible at https://github.com/star-bnl/. The link you pointing out is rather outdated. At this time, STAR has not yet released reduced datasets ("picoDSTs") to the public.

Derived data supporting the findings of this study will be publicly available in the HEPData repository, upon publication of the paper. Here the derived data are the analysis results appearing in the figures and tables in this paper and the supplementary "Methods" document. And those code used for this analysis to obtain the derived published data from the "MuDst" and "picoDst" data, are not available to public as STAR routine. STAR also archives copies of all analysis codes, It permit any knowledgeable member of STAR Collaboration reproduce the entire analysis.

References:

Please check refs. 42 and 43 where the symbols of the hypernuclei look odd.

Ref 46 should read Rayet, M. and Dalitz, R.H.

Ref. 50 should read Hildenbrand AND since that are only two authors it should be Hildenbrand, F. and Hammer, H.-W.

A: Done.

Some references are missing:

You missed the STAR search upper limit paper on strangelets (suppose to be around ref. 58) - even if it is rather old, there is no better upper limit from colliders yet:

@article{STAR:2005wmk,
author = "Abelev, B. I. and others",
collaboration = "STAR",
title = "{Strangelet search at RHIC}",

eprint = "nucl-ex/0511047",
archivePrefix = "arXiv",
doi = "10.1103/PhysRevC.76.011901",
journal = "Phys. Rev. C",
volume = "76",
pages = "011901",
year = "2007"
}

A: The whole "Yield ratios" section is largely rewritten. This sentence about strangelets is not in the new draft.

On the Kalman Filter package I would propose this two additional references:
@article{Ju:2023xvg,
author = "Ju, Xin-Yue and others",
title = "{Applying the Kalman filter particle method to strange and open charm hadron reconstruction in the STAR experiment}",
doi = "10.1007/s41365-023-01320-1",
journal = "Nucl. Sci. Tech.",
volume = "34",
number = "10",
pages = "158",
year = "2023"
}

@article{Ablyazimov:2015pka,
author = "Ablyazimov, T. O. and Zyzak, M. V. and Ivanov, V. V. and Kisel, P. I.",
title = "{Kalman filter-based fast track reconstruction for charged particles in a Compressed Baryonic Matter experiment using parallel computing on a multicore server at the Laboratory of Information Technologies, Joint Institute for Nuclear Research}",
doi = "10.1134/S1547477115030012",
journal = "Phys. Part. Nucl. Lett.",
volume = "12",
number = "3",
pages = "423--427",
year = "2015"
}

A: Done. And we move the sentence and citation about Kalman-Filter into Methods.

In principle one could also reference to http://www.gsi.de/documents/DOC-2007-May-14-1.pdf

A: This is a note without a proper citation format. Since we already have 4 citations for KF particle, we would prefer to not have this one.

The hypernuclei community has its own data base (supposed to be equivalent to PDG,

CODATA and BNL Nuclear Data Center):
@misc{HypernuclearDataBase,
title = {Chart of Hypernuclides --- {H}ypernuclear Structure and Decay Data},
author = {P. Eckert and P. Achenbach and others},
year = 2021,
note = {\href{https://hypernuclei.kph.uni-mainz.de}{hypernuclei.kph.uni-mainz.de}}
}
A: Done.

Bethe-Bloch - since you have some references on the origin of antimatter production, it might be good to have also the original sources of the energy-loss formula:
@article{bethe,
author = {H. Bethe},
title = {{Bremsformel f\"ur Elektronen relativistischer Geschwindigkeit}},
volume = {76},
pages = {293},
year ={1932},
journal = {Z. Phys.}
}
A: Done.

@article{bloch,
author = {F. Bloch},
title = {{Zur Bremsung rasch bewegter Teilchen beim Durchgang durch Materie}},
volume = {408},
pages = {285},
year = {1933},
journal = { Ann. Phys.}
}
A: Done.

As review (summarizing run1 results) on antihypernuclei at the LHC I would suggest:
@article{Braun-Munzinger:2018hat,
author = {Braun-Munzinger, Peter and D\"onigus, Benjamin},
title = "{Loosely-bound objects produced in nuclear collisions at the LHC}",
eprint = "1809.04681",
archivePrefix = "arXiv",
primaryClass = "nucl-ex",
doi = "10.1016/j.nuclphysa.2019.02.006",
journal = "Nucl. Phys. A",
volume = "987",
pages = "144--201",
year = "2019"
}

and there is an update for run2:
@article{Donigus:2020fon,
author = {D\"onigus, Benjamin},
title = "{Selected highlights of the production of light (anti-)(hyper-)nuclei in ultra-relativistic heavy-ion collisions}",
doi = "10.1140/epja/s10050-020-00275-w",
journal = "Eur. Phys. J. A",
volume = "56",
number = "11",
pages = "280",
year = "2020"
}
A: Done. We add these two citations to the abstract and the introduction section.

Importance of excited states of nuclei (in the thermal model) is discussed here:
@article{Vovchenko:2020dmv,
author = {Vovchenko, Volodymyr and D\"onigus, Benjamin and Kardan, Behruz and Lorenz, Manuel and Stoecker, Horst},
title = "{Feeddown contributions from unstable nuclei in relativistic heavy-ion collisions}",
eprint = "2004.04411",
archivePrefix = "arXiv",
primaryClass = "nucl-th",
doi = "10.1016/j.physletb.2020.135746",
journal = "Phys. Lett.",
volume = "B",
pages = "135746",
year = "2020"
}
and the only paper naming clearly the need of taking the excited state of the hyperhydrogen-4 into account is this:
@article{Donigus:2023hiw,
author = {D\"onigus, Benjamin},
title = "{Hypernuclei at relativistic energies}",
doi = "10.1051/epjconf/202327604002",
journal = "EPJ Web Conf.",
volume = "276",
pages = "04002",
year = "2023"

}
A: Done.

Detailed comments on the text:
line 6: High energy nuclear collision rate -> High-energy nuclear-collision rate

A: It's "create" rather than "rate" here, so the 2nd suggested hyphenation is not needed.

line 11: ultrarelativistic heavy ion collisions -> ultrarelativistic heavy-ion collisions
A: Done.

Figure 1: “lineage of antimatter nuclear cluster“ is a bit misleading since the positron is also indicated and antiproton and antineutron are nucleons but definitely not nuclear clusters, perhaps antinuclear constituents and clusters (?)
A: The caption is changed to "Masses vs. discovery years of selected antimatter particles, including the positron, antinucleons, antiLambda, and antimatter (hyper)nuclear clusters." to avoid confusion.

line 32: heavy ion collisions -> heavy-ion collisions
A:Done.

line 34: not clear if strange quarks are known to a general audience
A: A couple of sentences are added explaining the strange quark.

lines 41/42: relativistic heavy ion collisions -> relativistic heavy-ion collisions
A: Done.

lines 44/45: center of mass energy -> center-of-mass energy
A: Done.

lines 46-48: The data sets and triggers used are too detailed for the text - suggest to move this to the methods
A: We moved details about data sets and triggers into Methods, and keep only one brief sentence about the data sets in the main draft.

lines 56/57: Bichsel function is a specific method to parametrize the Bethe-Bloch formula, that describes the energy loss, and might be known to non-physicists as well, better name the latter here and put the Bichsel function to the methods - that applies also to the caption of Figure 2
A: Bichsel function is not just a specific method to parametrize the Bethe-Bloch formula. The former consider the thickness of the medium layer as an input to calculate the energy loss, while the latter does not.

Figure 2: (A) <dE/dx> versus -> (A) Average energy loss <dE/dx> versus
A: Done.

lines 65/66: usage of the Kalman-Filter package is too detailed, better move to methods
A: Done.

lines 69-72: might be confusing even for a physicist that is outside high-energy physics

A: The subsentence starting from "instead of antihyperH4, in order to ...", and the next sentence, is moved to the "Methods" for more expert readers.

line 74: invariant mass spectra -> invariant-mass spectra

A: Done.

line 75: invariant mass distributions -> invariant-mass distributions

A: Done.

lines 74-76: here I would mention that the calculation is based on energy and momentum conservation - and in the methods please put the formula

A: Done.

Figure 6: Invariant mass distributions -> Invariant-mass distributions

A: Done.

Figure 6: invariant mass regions -> invariant-mass regions

A: Done.

Figure 6: how are the uncertainties of the background calculated? Even when they are determined by the rotated background they look too small somehow; in particular, looking at the signal region of the two lower plots (4LH and anti4LH) there seems to be some structure…

A: Thank you for this good question.

The uncertainties of the background histogram were automatically generated by ROOT after they were filled, so it's assumed that the rotated background candidates are uncorrelated. Triggered by your question, we did some further checks and found that this assumption is not true. Each helium track is randomly rotated for 50 times in order to increase the background statistics. However, not all combinations of rotated helium tracks and pion tracks can pass the topological cuts. In a certain event, the combinations that pass the cuts tend to be from the same (anti)He4 track and the same pion track, with rotation angles close to a certain angle, thus have close invariant masses. In other words, the invariant masses of background candidates from the same event are correlated. Simply increasing the rotaion times with limited (anti)He4 events will not help. This makes the rotational background invariant-mass spectrum's fluctuations larger than the nominal underestimates.

The "structures" around the signal regions are fluctuations, which looks prominent since the uncertainties are underestimated. We can see in the data how a structure is generated by a certain single event.

We have recalculated the uncertainties of the background histogram in the plot using a bootstrapping method, in which (anti)hypernucleus candidates in the different sub-samples are from different events, so that there is no correlation between different subsamples. Table 2 in Methods is also updated with the newly calculated background count uncertainties.

BTW, we find the original Table 2 in Methods is for all triggers rather than for MB trigger, as stated in the caption and text. We have changed the Table with right numbers.

lines 84-90: did you try to use the asymptotic calculator from RooFit/RooStat? That should give a better estimate than the S/sqrt(S+B) formula and is commonly used and accepted for discoveries at the LHC

A: Thanks for your suggestion.

We try to use the RooStat::AsymptoticCalculator to calculate the significance. And the results, named Z_shape, are added in the draft.

We have also updated the significances calculated with signal and background counts (named Z_count now) using a unified formula from Ref. 31, replacing the S/sqrt(S+B) for (anti)hypertriton and self-defined equivalent Gaussian significance for (anti)hyperH4.

This should make the significance calculation better accpected by the community, and also makes the explanations easier.

line 92: early -> initial (?)

A: Done.

line 96: CPT theorem might not be known to everyone, perhaps invert the logic: quantum physics expects matter and antimatter to have the same properties and then name it CPT theorem

A: Done. We rewrote these sentences in accordance with this logic.

line 105-109: what is written there is true, but requires quite some modeling - perhaps mention that here

A: A reference paper, Physics Letters B 811, 135916 (2020), is added as an example relating hypernucleus lifetime and Y-N interactions.

line 110: lifetimes of (anti)hypernuclei 3LH, 4LH, anti3LH and anti4LH are -> lifetimes of the (anti)hypernuclei 3LH, 4LH, anti3LH and anti4LH are (the is needed since you particular hypernuclei here)

A: Done.

line 114/116: what nominal mass m is used here? The one extracted from the invariant mass, some literature value or the Mainz data base value (that might be good to cite somewhere)?

A: We used 2.991GeV for (anti-)H3L and 3.922GeV for (anti-)H4L.

We added the reference to the Mainz data base to the draft following your suggestion.

line 122: formula is actually inconsistent with itself (last part is not depending on t) - implicit conversion from t to L, which is actually the investigated quantity

A: Here the t is the decay time in the rest frame of the (anti)hypernucleus. Then in the lab frame, the decay time is t*gamma due to the relativistic time dilation, and decay length L = t*gamma*v = t*gamma*beta*c, t/tau = t*gamma*beta*c / (tau*gamma*beta*c) =

L/(beta*gamma)/(c*tau), which appears in the last part of the formula in the paper. So L, which is directly measured as the distance between the collision point and the decay point, is related to the decay time t. "in the rest frame of the (anti)hypernucleus" and "the Lorentz factor of relativistic time dilation" are added in Line 117-118 to make it more clear.

lines 122-124: might be confusing for non-experts

A: We remove this sentence. The details of sysmetic error are described in Methods.

lines 127/128: needs brackets before 16 and after 10(sys.) & before 18 and after (46(sys.) since otherwise only the last value (the systematic uncertainty) has the unit picoseconds

A: Done

Figure 4: why only statistical uncertainties in (A)? What generates the difference in dN/d(L/betagamma) for particle and antiparticle? Is it only muB?

A: We didn't calculate the systematical uncertainties for each dN/d(L/beta gamma)point in Panel A. In stead, we only calculate the systematical uncertainties for the finally measured lifetimes. When doing the systematical uncertainty estimation, we do some variations, and each variation leads to a dN/d(L/beta*gamma) v.s. L/beta*gamma distribution, which is fit to extract a lifetime result. The differences among these extracted lifetimes are used to estimate the systematical uncertainties. The advantage of this treatment is that the correlation among different points in Panel A will be automatically properly considered.
The difference in dN/d(L/betagamma) for particle and antiparticle is due to muB.

Figure 4 (B): how is the average lifetime calculated (PDG routines?)? Do they take into account the newly presented results? The free L lifetime needs a reference (PDG presumably)

A: The method used to calculate the average lifetime follows the reference:
Roger Barlow, Phystat2003, SLAC, Stanford,California, 250 (2003)
It doesn't take into account the new results presented by this paper. "calculated from previous results" are added to the draft to make this clear.
Reference[34] has been added for the Lambda lifetime.

lines 131-132: both statements are rather vague and not useful for the broad audience that is expected to read this article

A: The whole "Yield ratios" section is largely rewritten. This sentence is shortened, mentioning only the (anti)(hyper)nucleus production mechanism, which is discussed in more details in the following part of this paragraph.

line 133: relativistic heavy ion collisions -> relativistic heavy-ion collisions

A: Done.

line 137: heavy ion collisions -> heavy-ion collisions

A: The whole "Yield ratios" section is largely rewritten. This sentence is rewritten without "heavy ion".

lines 145/146: these quantum numbers are also important for the thermal model, only the mass is needed in addition there

A: The whole "Yield ratios" section is largely rewritten. This sentence is not in the new draft. Only baryon number and strangeness dependences are introduced in this paragraph, with more explanations, so it's easier for a general audience. The mass (binding energy) effect, which is a much smaller effect, is only mentioned in the result discussion part (Line 177) in explanation for the hint of smaller measured hypertriton / He3 ratio than the thermal prediction.

line 148: relativistic heavy ion collisions -> relativistic heavy-ion collisions

A: The whole "Yield ratios" section is largely rewritten. This sentence is removed.

line 152: also a rather vague statement that is not helping a non-expert readership

A: This sentence is rewritten to make it more clear.

line 155: 1.5c is very misleading! The mentioned quantity is actually beta_T x gamma and that is not having any unit per se, better remove the c here on both sides

A: Done.

line 165: again very vague

A: This sentence is rewritten to make it more clear, that the ratio is expected to increase with the system size.

line 168: what does it mean that the Au+Au results are different now (by 1.9sigma)? Is there any understanding or do you think it is of purely statistical origin?

A: The newly measured hypertriton / He3 ratio in U+U and Au+Au collisions is lower than previous STAR result in Au+Au by 2.8 sigma. The newly measured antihypertriton / antiHe3 ratio in U+U and Au+Au collisions is lower than previous STAR result in Au+Au by 1.9 sigma. The previous analyses used a mixture of MB and central triggered events. As stated in the paper, the ratios are expected to be higher in central events. The two analyses are also done in slightly different p_T ranges. But these differences are not enough to explain the magnitude of the observed difference between the measured ratios. We have investigated possible sources for the ratio deviations and have found no other explanations except for statistical fluctuations so far.

Figure 5: vertical bars and boxes -> vertical bars and shaded boxes

A: Done.

Figure 5: The comparison with the ALICE results is confusing since there the muB is close to zero and the temperature is sightly lower there AND the reference in the legend is wrong, the ratios of the hypertriton are shown in PLB (ref. 20)

A: It's correct that the muB and temperature are different between LHC and RHIC, but the influence of this difference on the ratio is small (several percent level) comparing with the

uncertainties of current data, as shown in the referenced statistical thermal model Phys. Lett. B 697 (2011) 203. So putting ALICE results on the plot also provides some additional confidence that the analysis is solid with all particle yields somewhat as expected.
The legend of PLB(2016) is corrected.

Figure 5: Are the thermal model lines for the (anti)hyperhydrogen-4 with or without the excited state? If so the legend and/or the caption should mention that the predictions have been modified accordingly (ref. 53 did not take the excited states into account)
A: The thermal-model paper here (Ref. 60) does not have the result of (anti-)H4L. We asked the author to calculate it for us. It has taken into account the (anti-)H4L excited state in the calculation. In Line 175 of the draft, it's mentioned that the thermal model has taken into account the spin degeneracy effect.

line 170: antimatter-over-matter particle yield ratios -> antimatter-over-matter particle-yield ratios
A: Done.

Methods:
There are some details missing that would help the reader to understand the actual measurements better. For instance the non-photonic electron trigger is not known to many people. If you don’t want to extend the description, then perhaps consider some additional reference(s).
A: "designed to trigger electrons from charm and bottom quark decays" is added in the description of the non-photonic electron trigger.

Are the different triggers properly simulated? If so, how much do the different triggers actually bias and is that taken into account in the systematical uncertainties?
A: Events triggered by the “central” or “non-photonic electron” triggers were not used in the yield ratios analysis to avoid potential biases. Only the minimal bias (MB) triggered events are used for the yield ratios measurement. As its name suggests, the bias can be ignored.
For lifetime measurements, we use all triggers mentioned in the Methods.
Since the trigger will only bias on the event level, and will not affect the particle decay properties, the measurement of the lifetime should not be influenced.

Is there any systematic uncertainty added for the absorption of antinuclei and antihypernuclei? Since the hypertriton is large (ultimate halo nucleus) this should be mentioned at least in the methods somehow.
A: Thanks for this good and important question. This is something we hadn't noticed before.
Since STAR official simulation software is based on GEANT3, which does not properly consider (anti)nucleus absorption by materials, the absorption of (anti)He3 and (anti)He4 are not considered in the previous version of the paper. In the updated version of the paper, we simulate the (anti)He3 and (anti)He4 absorption using GEANT4, and further

correct their track efficiency from STAR official simulation. This momentum dependent correction is ~<3% for nuclei and ~<5% for antinuclei. This correction is mentioned in Methods. Since even the whole magnitudes of these corrections are smaller than most of the existing systematic uncertainties on track reconstruction efficiency, we decide not to add an additional systematic uncertainty item for this.

The absorption of (anti)hypernuclei are not considered. Since (anti)hypertriton and (anti)hyperH4 decay length is on the order of c*tau ~= 6cm, the TPC structures, with inner radius of about 40 cm, are too far away to be considered. The materials that the (anti)hypernuclei may ineract with are the beam pipe (Be) and the insulation gas (N2) outside the beam pipe. Assuming conservatively, that the cross section of (anti)hypertriton with Be and N nuclei is the area of a circle with a diameter of 10 fm (the typically considered hypertriton size), we can estimate that the maximum interaction ratio of an (anti)hypertriton with the beam pipe and 1cm-thick of N2 gas are about 0.7% and 0.004%, respectively. These are much smaller than the currently considered systematic uncertainties from other sources, thus can be neglected. The following sentence is added to the Methods "The fraction of (anti)hypernuclei absorbed by the beam pipe (Be) and insulation gas (N$_{2}$) are estimated to be minimum and can be neglected.".

lines 46ff.: The rotational method details should be added here (angles and number of trials) and the actual estimation of the uncertainty of the background should also be explained a bit. As mentioned above, the fluctuations of the rotational background is definitely underestimated in the numbers given in Fig. 3 of the manuscript. Please add somewhere the formula for the invariant mass.

A：We add several sentences about the rotational background and a formula of the invariant mass.

As mentioned in the answer to a previous qeustion, the uncertainties of the rotational background hisogram in the last-version plots are under-estimated due to correlations among candidates in the same event. The uncertainties are recalcuated now with a bootstrapping method, in which (anti)hypernucleus candidates in different the sub-samples are from different events, so that there is no correlation between different subsamples, and the figures are updated accordingly.

Table 1: The cuts on the chi2 values from the Kalman Filter package are not straightforward to understand. Could you add a supporting plot or point to a proper reference?

A：We add a plot to illustrate this in the Methods.

lines 61ff.: Perhaps you mention the p-value in the actual manuscript. Is known by all scientist that apply statistical methods normally.

A: As mentioned above, we have updated the significances calculated with signal and background counts using a unified formula from Ref. 31, replacing the S/sqrt(S+B) for (anti)hypertriton and self-defined equivalent Gaussian significance for (anti)hyperH4. This also makes the Significance Calculation section in Methods much cleaner.

Following your previous suggestion, we also try to use the RooStat::AsymptoticCalculator

to calculate the significance considering signal invariant-mass shape. And the results are added in the draft.

Figure 2: Why is the efficiency of (anti)hyperhydrogen so much smaller than the one of the (anti)hypertriton? In principle, one would assume the (anti)alpha daughter would be cleaner and have a high efficiency.

A: The PID conditions for (anti)He3 and (anti)He4 are different. Basically, for (anti)He3, the TOF information, which is used to deduce m^2, is only used when a track is matched to a TOF hit. So the (anti)He3 selection does not suffer from TOF efficiency. However, for (anti)He4 selection, unless the dE/dx is significantly (3.5 sigma) away from the He3 dE/dx band, TOF (and m^2) information is required, in order to reject (anti)He3, which has a much larger yield. So the TOF effeiciency goes into the (anti)He4 selection efficiency and the final (anti)hyperH4 efficiency. This makes the (anti)hyperH4 efficiency lower than (anti)hypertriton.

The lifetime shown in Fig. 4 is that the value given in line 104? If yes, which one Lambda or anti-Lambda?

A: Yes. Maybe the particle symbol is too small in the Fig. 4. We have adjusted it.

Figure 3: Did you apply any check, e.g. look-elsewhere effect? There are some fluctuations visible that also have a relatively high significance…

A: We try to avoid look-elsewhere effect by limiting the number of free parameters in the new particle search, basically to 0. The collision point selection variables and track quality selection variables are chosen according to STAR routine. The (anti)He4 and pi+- particle identification criteria are decided according to the nSigma and m^2/Z^2 distributions of the target and background particles. The topologic selection variables are chosen by optmizing antihypertriton signal, which has very large statistics and significance, rather than by optmizing the signal of the searched (anti)hyperH4 particle. The hyperH4 mass is known. And we observe both hyperH4 and antihyperH4 signals at the known hyperH4 mass. So everything is already settled and there is basically no free parameter space left when we look at the antihyperH4 signal. The significance value of the observation can not come from a huge free parameter space, as in the look-elsewhere effect.

Yes. We also noticed that there are some up-fluctuations in both this Fig. 3 in the methods and Fig. 3 of the main paper. The most noticeable one is a bump-like structure around 3.95 GeV/c^2 in both He4 pi- and antiHe4 pi+ invariant mass plots. The combined significance is about ~3. So from the current data we can not tell whether it's purely fluctuations or it's from (partial reconstruction of) some other particle decay.

Absorption is not mentioned at all as source of the uncertainty, is that somehow incorporated? If yes, please mention otherwise try to estimate it under some assumptions.

A: Thanks for this good question. As mentioned in the previous answer, the absorption of (anti)He3 and (anti)He4 are not properly considered in the previous version of the paper. This is corrected in the updated version of the paper, and mentioned around Line 99 in Methods. Since even the whole magnitudes of these corrections are smaller than most of

the existing systematic uncertainties on track reconstruction efficiency, we decide not to add an additional systematic uncertainty item for this.

Detailed comments:
line 19: inconsistency: anti-nuclei before antinuclei
A: Done.

line 33: distance of closest approach -> distance-of-closest approach
A: Done.

Table 1: He DCA < 1 cm -> shouldn’t it be larger than some value?
A: Since the decay daughters (anti)He3 and (anti)He4 are much heavier than the other decay daughter pion, the momentum and track direction of (anti)He3 and (anti)He4 are very close to the parent (anti)hypernuclei, which means the He DCA due to decay is small. Considering also that the STAR detector, which has only TPC for tracking for the used data sets, can not provide a very precise track position information, we found that a lower-limit He DCA cut does not help to improve the signal. Here the He DCA < 1cm cut is just used to reject knock-out He. That's also why the cut only applies to He but not antiHe.

line 46: invariant mass distribution -> invariant-mass distribution
A: Done.

line 78: (Anti-)hypernuclei before always (Anti)hypernuclei
A: Done.

line 92: invariant mass distribution -> invariant-mass distribution
A: Done.

Fig. 3: in particular the important antihyperhydrogen-4 plots are not as convincing as the figure in the manuscript
A: This is expected when dividing a ~<5 sigma signal into 3 L/(beta gamma) bins. Some signal candidates in the main draft plot may also be outside the 3 L/(beta gamma) ranges, further decrease the signals seen in Fig. 3 in Methods.

Fig. 4: originates the difference between particle and antiparticle here only from the baryo-chemical potential?
A: If you mean the difference between the absolute yields, yes.
If you mean the difference between the measured Lambda and anti-Lambda lifetimes, they differ by 1.5 sigma considering only the statistical uncertainties, which is not large even if there is no other effect except purely statistical fluctuations. The baryon chemical potential cause the particle yield relationship 0.8 ~ anti-Lambda/Lambda < anti-Chi/Chi < anti-Omega/Omega ~< 1. As a result, Chi/Lambda < anti-Chi/anti-Lambda, Omega/Lambda < anti-Omega/anti-Lambda. So the anti-Chi and anti-Omega feeddown influence on the measured anti-Lambda lifetime is slightly larger than the Chi and Omega feeddown effect

on the measured Lambda lifetime. This may explain the longer measured anti-Lambda lifetime than the measured Lambda lifetime in Fig. 4, both suffer from the feeddown effect. But we have not studied quantatively how much this difference between the feeddown influences on anti-Lambda and Lambda lifetimes is.

line 107: the high-statistics data from the J-PARC emulsion experiment show some cascade like decays also in 4LH and 3LH, but presumably the analysis presented here is safe, since the mass of the multistrange hypernuclei decaying into 4LH and 3LH are higher and thus there production is suppressed compared to the 4LH and 3LH

A: Yes. No A=3 or A=4 multi-strange hypernuclei are discovered so far. In heavy-ion collisions at RHIC energy, the production yield decrease by 2-3 orders of magnitude for each additional A. So even some multi-strange hypernuclei with larger A can decay to hypertriton and hyperH4, their contribution at RHIC will be neglectable.

line 110: |Rapidity| -> |y|

A: Done.

line 110: same issue as in the manuscript pT/m is actually the transverse betagamma and that has no unit (1.5c does not make sense here)

A: Done.

Table 2: the small uncertainties of the background counts are not clear

A: We use the rotation method to obtain the background invariant-mass distribution. And the rotation is done with a random angle between 30 and 330 degrees, for 50 times. Thus the obtained background invariant-mass distribution has much larger statistics and smaller uncertainties. The background counts, obtained by integrating the background histogram in the signal invariant-mass ranges, also has smaller uncertainties than the signal counts. This rotation method is now described in Methods.

line 113: was confused by M^2/Q^2 for some time, Q is supposed to be Z, as in Fig. 2 of the manuscript - only then the unit makes sense. Actually, would be good to mention that this is calculated from the time-of-flight information and the measured momentum.

A: Q is now replaced by Z.

It has been mentioned in the main draft that M^2/Z^2 is calculated from the rigidity, track length and time of flight measured by the TPC and TOF detectors.

line 114: better use a symbol for efficiency, e.g. \epsilon - mainly because the 1/epsilon is easier to read (and understand) than 1/efficiency weight

A: Done.

line 116: 1/\epsilon weight

A: Done.

line 117: invariant mass distribution -> invariant-mass distribution

A: Done.

line 125: R is 10 fm -> not clear why that should be. For all discussed collision systems?

A: The shape of the Blast-Wave function is not sensitive to the value of R. It is only sensitive to parameters beta and T. Change of R will influence the absolute yield, or the overall magnitude of the p_T spectrum. Here we are only interested in the shape of the p_T spectrum, which is needed to obtain the average efficiency in a wide p_T range. Thus we fix R at 10fm, and fit the summed p_T spectra from a mixture of collision systems with only beta and T as the free parameters.

Fig. 5: Style is compeltely different compared to all other figures, please unify - STAR Collaboration before was only STAR

A： Done. Since these p_T spectra are from a mixture of collisions and not normalized by number of events, we consider it only a technical plot to obtain the p_T spectrum shapes, rather than a physics result. So the "STAR" and collision system information are removed from the plot.

Table 3: What are the corresponding uncertainties?

A: Uncertainties are added in the new version.

Referee #2 (Remarks to the Author):

In the present manuscript the authors report on the first experimental observation of the A=4 anti-4-Lambda-H antihypernucleus, together with measurement of its lifetime, in relativistic heavy-ion collisions.
Also presented in this work are new measurements of the A=3 3-Lamba-H, anti-3-Lambda-H, and 4-Lambda-H lifetimes. By comparing the lifetimes of A=3(4) hypernuclei and their corresponding antihypernuclei, no violation of CPT (matter-antimatter) symmetry is observed.

This is certainly an impressive achievement, representing a great technical progress in the field. However, in my opinion, it is a bit unfortunate to frame the whole analysis in the context of CPT symmetry breaking. Given the large reported experimental uncertainties due to the (expected) small number of detected events for A=4 anti-4-Lambda-H and 4-Lambda-H, such a measurement (likely) will not have a significant impact on CPT violation studies in strong-interaction baryonic sector.

A: Thank you for the suggestion. The paper's very first sentence is rewritten so that it

emphasizes the importance of the study on antimatter, rather than only on matter-antimatter asymmetry. The theme of the whole paper, as written in the title, is the observation of this new antimatter particle, plus some studies of its properties, including the CPT symmetry test through its lifetime measurement.

Moreover, the paper is very technical and I doubt that it is accessible for anybody outside of the high-energy collisions community. A better ballance between explaining the basic concepts and

A: Thanks for the suggestion. The whole paper draft is updated, moving some technical detals to Methods, or even completely removing them. Also, for some key concepts and key physics, descriptions and explanations are added, so that it's easier to be followed by a general audience.

In particular, I have difficulties in understanding how exactly the (anti)hypernuclear lifetimes were obtained from their two-body charged mesonic decay modes.

Do the "efficiency-corrected yields" also contain the two-body branching ratios (BRs) to account for the remaining (anti)hypernuclear decay modes?

I might be completely confused. The additional decay modes contributing to the lifetimes and BRs are mentioned for the first time in the later section on yield ratios.

A: In this analysis, the decay lifetime is not measured by adding up decay widths of all channels. Instead, it uses the simple fact that the particle decay time follows an exponential distribution, as shown by Fig. 4A and the function in Line 125: N(t) = N0*exp(-t/tau). Here the tau is the overall lifetime, rather than the reversed decay width for a certain channel. This is because all decay channels will cause the decaying-particle number to decrease and follow the exponential distribution. In this method, the branching fractions does not needed to be corrected, because it will only scale down the decay time (=L/(beta gamman c)) distribution down by a constant, and thus will not change the lifetime tau extracted from the function in Line 125.

There, the sentence is also quite ambiguous and could be misinterpreted that the BRs were measured in Ref. [42] which is not the case.

The branching ratios are quite poorly known and the adopted value for A=3 of R3=0.25 seems to come from [48] which is a (although comprehensive) theoretical calculation using rather dated input nuclear and hypernuclear interactions available in 1998. This might be a coincidence and Ref. [42] does not seem to be very helpful in tracking down the origin of the adopted value.

Similarly, the BR for 4-Lambda-H was measured in emulsion and bubble-chamber experiments during 1961-1970 and the quoted uncertainties are purely statistical.

The approach in Ref. [42] was to consider and propagate quite significant uncertainties in BRs. It seems that it was not done in the present work. Certainly, a more detailed discussion would be appropriate. Moreover, it is to be noted that equal two-body BRs for hypernuclei and antihypernuclei were tacitly assumed, while searching for violation of matter-antimatter

symmetries later. In this context I would like to mention the recenlty published extension of Ref. [50] on hypertriton lifetime: [F. Hildenbrand, H. W. Hammer, Eur. Phys. J. A 59, 280 (2023)], as well as additional recent comprehensive analysis in [A. Pérez-Obiol, D. Gazda, E. Friedman, A. Gal, Phys. Lett. B 811,135916 (2020)] and [D. Gazda, A.~Pérez-Obiol, A. Gal, E. Friedman,
arXiv:2310.03087 [nucl-th]].

A: Thanks. There are not very precise measurements of the decay branching ratio of hypertriton and hyperH4, due to the exsitence of some neutral decay channels and low statistics. We use branching fractions 0.25 and 0.5 following the conventions in previous analyses of heavy-ion experiments STAR, ALICE and E864. It seems that the first paper using these values is PHYSICAL REVIEW C 70, 024902 (2004) from E864 Collaboration, which uses these two values from theoretical calculations, Ref. [53, 62, 63, 64] in the new draft. Ref. [53] is the old Ref. [48] that you mentioned.

These are clearly explained around Line 156 in the new draft now. And the B.R. assumptions are repeated in the caption of Fig. 5. Since Fig. 5 is already pretty dense of information, we think a result vs. assumed B.R. plot, as in some other measurement results, is not proper here for a general audience. Using the same convention of B.R. allows an easy comparison of different measurements. The yield (ratio) results can also be easily converted if more accurate and reliable measurements on B.R. are available in the future.

I also think that a more detailed discussion of the obtained lifetime values would be appropriate. Especially for the case of 3-Lambda-H and 4-Lambda-H hypernuclei which can be compared with the previous measurements. Although only their differences with respect to the anti-hypernuclear counterparts are given in the text, their absolute values (shown in Fig. 4) differ from STAR Collaboration's own measurements from the last year [42] and from 2018 [41].

A: The 4 lifetime results in this analysis are all consistent with the average of previous results for hypertriton and hyperH4, repectively. Considering STAR results only, the new (anti)hyperH4 results are both consistent with PRL 2022 (Ref. [42] in the previous draft). The (anti)hypertriton results are both consistent with PRL 2022 and Science 2010. Only PRC 2018 (Ref. [41] in the previous draft) has some tension with the new (anti)hypertriton results. The deviation is 2.3 sigma for hypertriton and 1.7 sigma for antihypertriton. Considering that PRL 2022, which is the precisest among the STAR results, also has some tension with PRC 2018, it seems that PRC 2018 is a somewhat outlier. So far we did not find any explanation for the deviation of PRC 2018 from other results.

To make it more rigorous, we add "most" in Line 127: "our results are consistent with most existing measurements ...". We think additional discussions on this may make the draft defocused for non-experts.

Referee #3 (Remarks to the Author):

I would like to congratulate the authors for analyzing such a huge amount of data and tackling the problem of the hyper-nuclei and antihypernuclei production in heavy ion collisions.

A: Thanks.

The paper presents the measurements of hyper-nuclei and antihypernuclei by the STAR experiment, including the first observation of the antiH4L, the heaviest antihypernucleus observed so far. The analysis spans over four different collisions systems (AuAu, UU, RuRU, and ZrZr) in order to get a large enough signal of the antiH4L to claim its observation. While the title focus on the discovery of H4L, the measurement and discussion of the production of H3L is also very interesting and important.

The findings are both intriguing and significant. However, as demonstrated in Figure 1 of the authors' paper, the groundwork for uncovering heavier anti(hyper)nuclei was already laid by their previous discovery of antiHe4, as published in Nature 473, 353. In my view, this paper represents a notable yet incremental advancement, making it more suitable for a specialized physics journal such as Nature Physics.

A: Thank you for the statement and suggestion. It's a matter of taste whether the discovery of antimatter hyperH4 is significant enough to be published in Nature. But we believe this paper will certainly interest many readers of Nature. AntihyperH4 is the heaviest antimatter (hyper)nuclei observed so far. It's the second observed of its kind - antimatter hypernuclei - 14 years after the observation of the 1st antihypernucleus. I think it's a discovery worthy to be known by a general audience, considering that the second observed antimatter particle - antiproton - is worthy of a Nobel prize. AntiHe4 is the 4th antinuclei discovered, after antideuteron, antitriton and antiHe3. So in some sense, the discorvery of antihyperH4 is not more incremental than that of antiHe4. Of course both antiHe4 and antihyperH4 have 4 antibaryons, but one is an antinucleus, while the other is an antihypernucleus. The latter envolves the strangeness degree of freedom, and enables the CPT symmetry test through its lifetime measurement, although still with quite large uncertainties so far as shown in the paper. Comparing with antiHe4 identified with a single track, antihyperH4 is reconstructed with a pair of daughter tracks, meaning much less reconstruction efficiency and much more difficulty on background suppression. That's why it takes another 13 years and great effort for STAR to come to the discovery of antihyperH4 after antiHe4.

Finally, I have some questions about the analysis and the presentation of the results that I would like to see addressed before the publication:

- it is mentioned in the manuscript that the detection efficiency is obtained from a GEANT3 simulation. Already for the antinuclei (hence the daughter particles of the antihypernuclei), we know that GEANT3 is not an appropriate transport code without any tuning. Were the interaction cross sections implemented in GEANT3 for antinuclei modified to reproduce the available measurement of antinuclei interaction cross sections with material? Alternatively, what is the effect of using GEANT4, that has a representation of the interaction cross sections closer to the measurements? Was there any study on what is the effect of the interaction cross section of the H3L and H4L on the efficiency? All these questions are relevant for the efficiency correction and its systematic uncertainty.

A: Thanks for this good and important question. This is something we hadn't noticed before.

Since STAR official simulation software is based on GEANT3, which, as you mentioned, does not properly consider (anti)nucleus absorption by materials, the absorption of (anti)He3 and (anti)He4 are not properly considered in the previous version of the paper. In the updated version of the paper, we simulate the (anti)He3 and (anti)He4 absorption using GEANT4, and further correct their track efficiency from STAR official simulation. This momentum dependent correction is ~<3% for nuclei and ~<5% for antinuclei. This correction is mentioned in Methods.

The absorption of (anti)hypernuclei are not considered. Since (anti)hypertriton and (anti)hyperH4 decay length is on the order of c*tau ~= 6cm, the TPC structures, with inner radius of about 40 cm, are too far away to be considered. The materials that the (anti)hypernuclei may ineract with are the beam pipe (Be) and the insulation gas (N2) outside the beam pipe. Assuming conservatively, that the cross section of (anti)hypertriton with Be and N nuclei is the area of a circle with a diameter of 10 fm (the typically considered hypertriton size), we can estimate that the maximum interaction ratio of an (anti)hypertriton with the beam pipe and 1cm-thick of N2 gas are about 2% and 0.004%, respectively. These are much smaller than the currently considered systematic uncertainties from other sources, thus can be neglected. The following sentence is added to the Methods "The fraction of (anti)hypernuclei absorbed by the beam pipe (Be) and insulation gas (N$_{2}$) are estimated to be minimal and can be neglected.".

- Is the uncertainty on the B.R. of the hyper-nuclei considered in the systematic uncertainty?

A: No, the uncertainty on the B.R. is not considered. The 0.25 and 0.5 are not measured results, but theoretical calculations from Ref. [53, 62, 63, 64] in the new draft, which are conventionly used in previous analyses of heavy-ion experiments STAR, ALICE and E864. So they do not come with uncertainties. Also there are not very precise measurements of the decay branching ratio of hypertriton and hyperH4, due to the exsitence of some neutral decay channels and low statistics. So it's not easy to quote B.R. numbers with proper uncertainties. We have made the related descriptions around Line 156 more clear in the new draft, and the B.R. assumptions are repeated in the caption of Fig. 5. So the yield (ratio) results can be easily converted if more accurate and reliable measurements on B.R. are available in the future. If the uncertainty on the B.R. should be presented, it should certainly be presented separately from other systematical uncertainties and only applies to some of the measured ratios. Considering that Fig. 5 is already pretty dense of information, we think adding an uncertainty on B.R. is not very helpful for a general audience.

- The H3L lifetime is considerably larger than in the previous paper from STAR (PRC(2018) in the figure), given that the detector is the same, I guess most of the systematic would cancel out. How much is the difference in terms of number of sigmas? Is there any explanation for this difference?

A: The deviation between this analysis H3L lifetime and STAR PRC(2018) is 2.3 sigma; The

deviation between this analysis antiH3L lifetime and STAR PRC(2018) is 1.7 sigma. Here the systematical uncertainties of the two measurements are assumed to be independent, since there are many differences between these two analysis (e.g. the use of KF particle vs. DCA method for the (anti)hypertriton reconstruction), and it's not easy to estimate the cancellation of systematic uncertainties. Considering that PRL (2022), which is the precisest among the STAR results, is consistent with out new results and also has some tension with PRC (2018), it seems that PRC (2018) is a somewhat outlier. So far we did not find any explanation for the deviation of PRC (2018) from other results.

- the rotational background for antiH4L presents a structure close to the peak (see for instance panel D of Figure 3): given that with rotational background the statistics could be improved, are these present as well when enhancing the statistics?

A: Thanks for the suggestion. We tried to increase the number of rotation times from 50 to 300, and find the background histogram will not become smoother and smoother as we rotate more and more times. This is because the number of events with identified (anti)He4 is not large. When rotating the (anti)He4 track, not all combinations of rotated helium tracks and pion tracks can pass the topological cuts. In a certain event, the combinations that pass the cuts tend to be from the same (anti)He4 track and the same pion track, with rotation angles close to a certain angle, thus have close invariant masses. In other words, the invariant masses of background candidates from the same event are correlated. Simply increasing the rotaion times with limited (anti)He4 events will not help.

- In the comparison with the previous measurements there is no mention of the treatment of the systematic uncertainties (line 168, for instance), were this canceled out or not? It would be good to comment on the treatment of such cases at least in the method section.

A: We don't consider the cancelation of systmeatic uncertainties between the new and previous measurement. This is because there are many differences between these two analyses: the decay vertex reconstruction methods are completely different (K-F particle vs. helix DCA); the track number of hits requirement is different (20 vs. 25/15 for He/pion); the (anti)He3 PID selection criteria are different; the tracking algorithm in STAR offline data processing changed... It's thus difficult (at least very complicated) to consider cancelation of systmeatic uncertainties between these two analysis.

- It looks like that for antiHe4/He4 the Gaussian error propagation has been used for the old results, this looks not appropriate given that the ratio becomes compatible with 0 but the antiHe4 yield is not compatible with 0.

A: Thank you for this good comment. We update the result with an asymmetric error, obtained using a Monto Carlo method.

- The PV is requested to be within 2 cm in radius from the beam line, is this the beam spread or due to the PV resolution?

A：Neither. This is due to the radius of beam pipe (~3cm). 2cm is within the radius of beam pipe. So this cut rejects beam-material-collision events while taking full use of beam-beam collision events.

What is the secondary vertex reconstruction spatial resolution and how does this compare with the binning used for the lifetime analysis?

A: The secondary vertex reconstruction spatial resolution is determined by the TPC track pointing resolution, on the order of 1mm. When the decay angle is small, the resolution along the decay length direction can be larger, about several mm. This is smaller than the bin width in the lifetime measurements. The decay vertex resolution effect is automatically corrected when applying the efficiency correction. This is because when calculating efficiency vs. l/(beta gamma), we use the reconstructed l/(beta gamma) for the numinator (the successfully reconstructed (anti)hypernuclei) and true l/(beta gamma) for the denominator (all generated (anti)hypernuclei). Thus the numinator histogram already has the smearing effect due to decay vertex resolution. When applying the efficiency correction, the decay vertex resolution effect is also corrected, as long as the MC input lifetime is not much different from the real lifetime.

Comments on the text:

- General: there is a lot of emphasis in the abstract and throughout the text on the similarities with the early Universe. I think that this is not the main point of the paper and it should be tuned down and/or revised. There are many differences between the early Universe and the collisions at RHIC: the matter-antimatter asymmetry we observe in HIC is due to the baryon transport from beam to mid-rapidity, while the puzzle of the matter dominance in the universe concerns the baryogenesis and possible CPT violation mechanisms and the matter-antimatter balance changes quite a lot depending on the rapidity where we perform measurements in HIC.

A: Thanks for this comment. Yes the HIC and early universe have some similarities as well as differences. We have modified the paper draft to avoid potential misunderstandings by the audience. The paper's very first sentence is rewritten so that it emphasizes the importance of the study on antimatter, rather than only on matter-antimatter asymmetry. We mainly want to emphasize the similarities between the HIC and the early universe on the quark-gluon degrees of freedom and the very close amount of matter and antimatter, which is very different from the current universe. These are mentioned in Lines 5-7 and 30-32. These help to explain to the audience why HIC is a good tool to create antimatter particles. And We think this connection is also interesting in iteself to the general audience of Nature. Of course, as you pointed out, the mechanisms generating the matter-antimatter asymmetry in HIC and in early universe are totally different. The matter-antimatter asymmetry in the universe and related explorations are discribed in Lines 93-106. The matter-antimatter production yield imbalance in HIC and its cause are described in Lines 166-168. With these, We think the audience can well understand both the simliarities and differences between the HIC and the early universe.

Figure 1: while I understand that this is a schematic figure with selected antiparticles, it looks like this is cherry-picking to show an increase of mass with time. However, when including all antibaryons (or even the heaviest antiparticle seen, the tbar) this picture would change a lot. I would suggest to either remove this figure or to remove the positron to keep only the

antinuclei/antihypernuclei.

A: Thanks. We try to show in the figure the antimatter particles that are mostly relevent to this study: antimatter (hyper)nuclear clusters, as well as antinucleons and antiLambda which constitute them. The antiLambda is newly added to the plot, triggered by this question. The positron is also included, both because it's the most important 1st-observed antimatter particle, and because it's mentioned in the draft. We prefer to keep this plot so the general audience can have a sense how more and more complicated antimatter (hyper)nuclear clusters were made and observed in history. The figure caption is changed to "Masses vs. discovery years of selected antimatter particles, including the positron, antinucleons, antiLambda and antimatter (hyper)nuclear clusters." to avoid confusion.

- line 28: "Antimatter readily annihilates with matter, making it difficult to create antimatter nuclear clusters in the Universe" -> "Antimatter readily annihilates with matter, making it difficult to *observe* antimatter nuclear clusters in the Universe"

A: Done.

- Figure 2A: the bands for nuclei are not visible for data, only the expected values are visible. This is not the case in the paper on the antialpha discovery. What is the source of the large background? It would be good to explain this in the text or in the caption.

A: Thanks for the question. In the dE/dx plot in the antialpha-discovery paper, the events are pre-selected by the STAR High Level Trigger for (anti)He3 and (anti)He4 candidates. So the (anti)He3 bands look very prominent. In the original Figure 2A in this paper, there are a lot of background tracks which are reconstructed with poor quality, for example merged tracks. These poor-quality tracks are reduced as the multiplicity decreases. So we updated Fig. 2A with isobar (Ru+Ru, Zr+Zr) only data, which has lower multiplicity, and the (anti)He3 bands can be seen.

- Line 71: "This bias due to fluctuations is much smaller for 3Î› Â¯H because of its large signal significance", the bias is smaller, but given the impact of this paper it is necessary to either quantify it with a systematic uncertainty or avoid it altogether by optimizing the selections on a MC for H3L.

A: For the lifetime and yield ratio measurements, this kind of bias is included in the systematic uncertainty from "topological selection", which is estimated by varying the topological cuts. For the calculation of the signal significance, since antihypertriton is not a new particle to discover, the upward bias on the order of 1 out of a significance value of 14 is not very important.

- Line 97: you introduce here the acronym for CPT, but the first time it was mentioned was on line 39. It would be good to introduce it earlier.

A: Thanks for the suggestion. We remove the "CPT symmetry" in Line 39 of the old version of the draft. Only "matter-antimatter symmetry" is kept in the introduction. In the new draft, "CPT" firstly appears in Line 99 with full explanation of its meaning.

- Line 165-167: this is such an interesting point that I suggest to elaborate on that. Is the trend going from small to large collision system significant when canceling out the correlated sources of systematic uncertainties?

A: After canceling out the correlated sources of systematic errors, the difference between the (anti)hypernucleus/(anti)nucleus ratios in the large and small systems are as the following:

H3L/He3(AuAu+UU) - H3L/He3(Isobar) = 0.085 ±0.047(stat.) ±0.045(sys.)

antiH3L/antiHe3(AuAu+UU) - antiH3L/antiHe3(Isobar) = 0.097 ± 0.074(stat.) ± 0.122(sys.)

They are both positive, with 1.3 and 0.7 sigma deviations from 0. So it's not very significant. The reason for this system size dependence, according to the Ref. [66, 67], is that (in simple words) when the whole collision-system size is even smaller than the hypertriton size, the overlap of the hypertriton internal wave functions with those of nucleons are reduced, and the coalescence chance gets smaller. This is certainly an intersting point, as you pointed out, for heavy-ion experts, supporting the coalescence production mechanism of hypernuclei. However, for a general audience, this may be too detailed. So we only cite the references, providing a reason to conduct the ratio measurements for large and small systems separately, in order to have a better comparison with existing results.

- Line 192: here you mention the discrepancy of the H3L/He3 from the SHM and you mention that this could be due to the low binding energy. I would request a quantification of the discrepancy in terms of number of sigmas and to mention the role of the wave function in the coalescence model, that indeed could explain this discrepancy (see e.g. https://arxiv.org/abs/1807.05894)

A: In AuAu+UU system, the H3L/He3 ratio is lower than the statistical thermal model prediction by 1.6 sigma, the H3L/He3 ratio is lower than the statistical thermal model by 0.7 sigma. We didn't add these values to the paper draft as they are not very significant. And we add the suggested reference paper as an explanation for the difference. Similarly as above, although this may be insteresting for heavy-ion experts, we did not add too much description about the theoretical model explanations because it may become a distraction for the general audience.

- "According to the coalescence model and statistical thermal model, antinucleus and antihypernucleus production yields decrease by about 3 orders of magnitude for each additional atomic mass number" could you add a reference to a coalescence model prediction where this penalty factor is predicted? (for example https://arxiv.org/abs/1512.00692)

A: Thanks. We add this paper as a reference. The whole "Yield ratios" section is largely rewritten. This sentence is also changed.

Comments on the additional material:

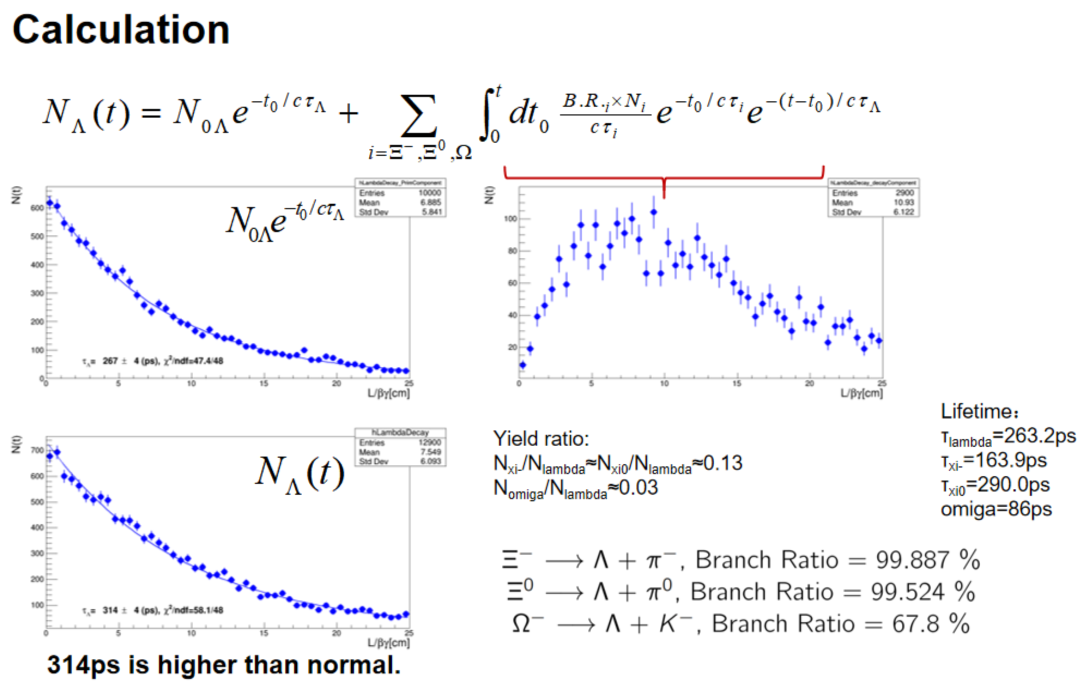
- Table 1: the selections look all reasonable, but that on the chi2_primary for He3. What is the meaning of a selection of Chi2_primary<2000?

A: Since the decay daughters (anti)He3 and (anti)He4 are much heavier than the other decay daughter pion, the momentum and track direction of (anti)He3 and (anti)He4 are

very close to the parent (anti)hypernuclei, which means the He DCA due to decay is small. Considering also that the STAR detector, which has only TPC for tracking for the used data sets, can not provide a very precise track position information, we found that a lower-limit He Chi2_primary cut does not help to improve the signal. Here the very loose Chi2_primary<2000 cut is just used to reject background tracks that are too far away from the primary vertex, for example, from pile-up events. This explanation is added to Methods.

- Line 105: "However, they are slightly longer than the value from the Particle Data Group 263Â±2 ps. This is expected because the V0DCA cut can not exclude all Lambda from Xi and Omega decays." I would expect that the feed-down would also spoil the exponential distribution, do you have a MC study that substantiates this statement?

A: Yes, with feeddown component the dN/d(L/(beta*gamma)) is not a perfect exponential distribution anymore. We did a MC study as shown below. The top left and top right plots show the primary and feeddown Lambda dN/d(L/(beta*gamma)), respectively. The bottom plot shows the combined.



Since the feeddown component is small, the combined dN/d(L/(beta*gamma)) distribution is still not far from an exponential. As shown in the bottom plot, fitting with an exponential function, the chi2/ndf is still close to 1 (58.1/48). We can still use the exponential function to fit, and obtain a higher apparent lifetime than its true value. For real data, as shown in Figure 4 in Methods, there is also no visible deviation from the exponential fit.

- Figure 5: the systematic uncertainties on the single pt bins are fluctuating bin by bin, this is not expected. Do you have an explanation for this?

A: The larger systematic uncertainties for some p_T bins is mostly due to statistical fluctuations when varying the topological selections. Since these bin-by-bin systematic uncertainties are not used to obtain the final results anyway, we remove them from Fig. 5. The systematic uncertainties from the assumed p_T shapes, which are used to calculate the average efficiency, are estimated by fitting the p_T spectra in different ranges.

Reviewer Reports on the First Revision:

Referee #1 (Remarks to the Author):

I thank the authors for improving the manuscript and answering all my comments. The new manuscript reads very nice and I think after some polishing it can be accepted for publication in Nature. The topic of the title of the manuscript is not the main reason to argue like this, since the significance of a discovery was set to 5sigma and here only 4.7 sigma are reached. In fact the STAR Collaboration reported 5.5sigma at the Strangeness in Quark Matter conference in 2022.
I nevertheless think that the manuscript is interesting for a broad scientific audience (not only physicists) because of the three different measurements that are reported here. It discusses the production and the comparison with models, the lifetime of the antihyperhydrogen-4 together with the corresponding CPT test.

I am slightly confused that the significance using the asymptotic formula gives about the same value as the S/sqrt(S+B) formula that was used before. Normally the significance is higher with the proper formula. I would in any case still spell out in the manuscript or at least the methods that the 4.7sigma correspond to a p-value of 1.3e-06, since an interested biologists can not judge the number of sigmas easily but catch a p-value easier.

Detailed comments on the manuscript:
line 5: to date. -> yet. (?)
line 30: However relativistic -> However, relativistic
line 41/49: Symbols are used but hypernuclei are not properly introduced (perhaps use a footnote for that?)
line 53: TPC reconstructs the three-dimensional tracks and measures their -> TPC is used to reconstruct the three-dimensional tracks and to measure their
line 68: significant not so clear, consider to leave
line 69: optimized for best 3barLbarH signal is not clear, perhaps larges number of signal (?)
Figure 4: caption has twice represent close by
line 137: is commonly believed -> not so commonly, perhaps better: is often imagined (?)
Figure 5: would be interesting to see, where the thermal model lines for 155/156 MeV would be compared to the data
line 178: wave-function size -> extension of the wave functon(?)
line 185: great step forward -> significant step forward
line 305: year should be 2023
line 306: Should have at least a CDS number (or CERN Report number) to allow to find it

line 322: Suggest to also cite here:@article{Reichert:2022mek,
author = {Reichert, Tom and Steinheimer, Jan and Vovchenko, Volodymyr and D\"onigus, Benjamin and Bleicher, Marcus},
title = "{Energy dependence of light hypernuclei production in heavy-ion collisions from a coalescence and statistical-thermal model perspective}",
eprint = "2210.11876",
archivePrefix = "arXiv",
primaryClass = "nucl-th",
doi = "10.1103/PhysRevC.107.014912",
journal = "Phys. Rev. C",
volume = "107",
number = "1",
pages = "014912",
year = "2023"
}

Detailed comments on the methods part:
Figure 1: of a hypernucleus. -> of a hypernucleus and the according selection criteria.
lines 102/103: you should better describe what went into finding the absorption can be neglected. A size of 5-11 fm should lead to a significant absorption cross section and therefore to some absorption.
Figure 3: Consider to add the significances for each signal
line 122: No particle yet discovered decays to -> No particle yet discovered decays weakly to
line 141 at -> to
Figure 5: not clear why the Blast-Wave curves of 3LH and anti-3LH are different when they use the same parameters…
Figure 5: Still confusing without proper legend in the Figure
line 159: +-20 ~ 30 is not clear

Referee #1 (Remarks on code availability):

The code is not really usable without the data as described in one answer by the authors. Nevertheless, having the code public is very good.

Referee #2 (Remarks to the Author):

I thank the authors for responding in detail to most of my comments and appreciate their willingness to review and modify the manuscript. There are certainly many improvements in the revised version which further improved the presentation of the impressive experimental achievements.

However, I still find even the revised manuscript very technical and thus better suited to be published in a specialized physics journal. The bulk of the paper is written as a collection of facts describing the data analysis, rather than trying to build a narrative for readers unfamiliar with heavy-ion-collision experiments. Moreover, in my opinion, although this paper reports on important and significant results, they represent (to some extent) incremental advancement upon previous discoveries of A=2 anti-deuteron, A=3 anti-hypertriton, and A=4 anti-4He. The new measurements of the anti-L3H and anti-L4H lifetimes and their relation to matter-antimatter symmetry tests are the main 'selling points' of this paper. Unfortunately, the clear message of their analysis is quite overshadowed by the section on production yield ratios. This necessarily brings a large amount of additional technical details and; furthermore, the results do not allow any clear conclusions to be made. In my opinion, it would be better to leave it out in favor of adding more background information on matter-antimatter symmetry which would make it more accessible to readers.

Listed below are few specific remarks on the text:

- Line 35: The introduction of strangeness, hyperons, and hypernuclei seems to me quite short. It is also a bit misleading as it seems to me to put quarks on the same footing as asymptotic (detectable) particles.

- Line 60: m, Z is left undefined

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- Line 113: "transverse momentum" might not be known to wider audience

- Line 119: It might not be clear at all, what is being "reconstructed"

- Line 121, 122: It might not be clear what "reconstruction of hypernuclei" actually means

- Line 124: "expression" -> ~ exponential decay law (formula)

- Line 129: It would be useful, for reference, to state the lifetime values, only only their differences.

- Line 130: What is the difference between CPT symmetry and matter-antimatter symmetry in this context?

Fig.4: It would be appropriate to incorporate the L3H lifetime value of Ref. [36] as well. It should be also mentioned how the averages of past lifetime measurements were calculated or a Ref. given.

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- Line 140-143: It would be 'safer' to frame it in terms of strange baryons rather than quarks, with not so clear definition of mass.

- Line 146: "chemical freeze-out temperature" and "baryon chemical potential" might not be known to the audience

- Line 149: "different collision systems", "mixture collisions" - I find these terms quite abstract/technical and unnecessary to introduce in order to say that these were collisions of nuclei with different mass numbers.

- Line 154: "rapidity" might not be known to the 'target' audience

- Line 155: I strongly suggest to avoid the word "conventions" when it comes to adoption of particular values of the 2-body decay branching ratios. There is nothing conventional about it. I understand that this choice can be motivated in order to be consistent with previous analyses but it should be mentioned at least in the Supplemental Material. The values of R3,R4=0.25,0.5 are not universally accepted by the hypernuclear community. There is STAR's own value of R3=0.32(5)(8) [PRC97] and several other measurements, also for R4 - see https://hypernuclei.kph.uni-mainz.de/. I do not see a good reason to discard their uncertainties altogether. It should be at least mentioned, as well as that R3,4(anti hypernucleus) = R3,4(hypernucleus) was assumed.

Figure 5: I find it quite unfortunate to call composite nuclei particles. Also "... with the same baryon number" - the baryon number is positive(negative) for (anti)particles.

- Line 178: "wave function size" is quite ambiguous; perhaps "wave function" -> rms radius or be more precise as ~ "the spatial extent of L3H wave function"

Methods

- Line 43-45: The definition of \chi^_topo,NDF,primary is not clear, as well as their relationship with "likelihood"

- Line 88: It's not clear in what sense "(Anti)hypernuclei are simulated"

- Line 113: Is the V_{0}DCA the same as DCA_{V_0} in Fig. 1?

- Line 129: "M/Z" here while "m/Z" previously

Referee #2 (Remarks on code availability):

There is no manuscript-specific code (or instructions) which would allow one to reproduce the results presented in the manuscript.

Referee #3 (Remarks to the Author):

I would like to thank the authors for their work and for addressing the comments. I am in general happy with their replies. I would like to come back to some of their answers and to suggest a few more minor changes to the text.

General on the analyses: is the measured value of the hypertriton and H4L lifetimes used in the MC for the calculation of the efficiency correction?

Answers:

- On the B.R. of the decay: I am fine with not cluttering too much the plot with the B.R. uncertainties, I would still mention what are the typical uncertainties related to those in the text around line 156. For instance, from the R3 of the hypertriton ALICE estimated in Phys. Rev. Lett. 128, 252003, an uncertainty of 0.02 for the H3L->He3pi decay B.R.

Manuscript:
line 27: heaviest antimatter nuclear and hypernuclear -> use just hypernuclear
line 33: some antimatter to decouple from matter -> most antimatter to decouple from matter
line 79: introduce Nsig and Nbg here, in line 83 they are used but not defined
line 87: "The significances are also calculated without setting certain signal invariant-mass regions." this sentence is rather unclear, while it is better formulated in the method section. In my taste it is not necessary to repeat it here the comparison among the two methods.
line 135: "or on the order of 10^12 K" -> "that correspond to a 10^12 K"
line 177: ", if really exists," -> ", that is currently not statistically significant," or remove altogether this sentence

Additional material:
line 158: "On the other hand, we note that the total uncertainties of those ratio measurements are large, of the order of ±20 ~ 30% themselves." -> I am not sure I get your point here: it is true that the uncertainties themselves are large, but the comparison still yields a significant difference. I would suggest to rephrase this sentence to further elaborate your point or to remove this sentence. As you write the differences are 2.8 and 1.9 σ. Being two independent ratios, I guess that the combination of these discrepancy will be larger than 3σ. That looks like a large discrepancy, how much the effect of the centrality selection and pt coverage would reduce it?

Author Rebuttals to First Revision:

Thank you very much for the good comments and suggestions. Most of the suggestions are taken, which we feel further improve the draft. Please find the replies to the comments and suggestions inline below.

Referee #1 (Remarks to the Author):

I thank the authors for improving the manuscript and answering all my comments. The new manuscript reads very nice and I think after some polishing it can be accepted for publication in Nature. The topic of the title of the manuscript is not the main reason to argue like this, since the significance of a discovery was set to 5sigma and here only 4.7 sigma are reached. In fact the STAR Collaboration reported 5.5sigma at the Strangeness in Quark Matter conference in 2022.

A: In SQM2022, it is the "STAR Preliminary" result of this analysis. At that time, we underestimated the background, because we obtained the scale factor of the rotational background by a least-chi^2 fit to the real candidate histogram. When the statistics are low, it's known that the least-chi^2 fit tends to get a lower fit result. Now, we use the ratio of side-band integral values between candidate histogram and the rotational background histogram as the scale factor. So the estimated background counts increases, and the calculated significance decreases.

I nevertheless think that the manuscript is interesting for a broad scientific audience (not only physicists) because of the three different measurements that are reported here. It discusses the production and the comparison with models, the lifetime of the antihyperhydrogen-4 together with the corresponding CPT test.

I am slightly confused that the significance using the asymptotic formula gives about the same value as the S/sqrt(S+B) formula that was used before. Normally the significance is higher with the proper formula. I would in any case still spell out in the manuscript or at least the methods that the 4.7sigma correspond to a p-value of 1.3e-06, since an interested biologists can not judge the number of sigmas easily but catch a p-value easier.

A: We didn't use the S/sqrt(S+B) formula for H4L and anti-H4L in the 1st version of the paper. Instead, we used a self-defined numerically calculated "equivalent Gaussian significance", which almost equivalent to the asymptotic formula, because both are based on p-values. We did use the S/sqrt(S+B) formula for H3L and anti-H3L in the 1st version, and the calculated significances are lower than the numbers from the asymptotic formula in the 2nd version.
Following your suggestion, we have added the p-values in the new (3rd) version of the draft.

Detailed comments on the manuscript:

line 5: to date. -> yet. (?)

A：Done.

line 30: However relativistic -> However, relativistic

A：Done.

line 41/49: Symbols are used but hypernuclei are not properly introduced (perhaps use a footnote for that?)

A: Done, we have added a footnote here.

line 53: TPC reconstructs the three-dimensional tracks and measures their -> TPC is used to reconstruct the three-dimensional tracks and to measure their

A: This paragraph, including this sentence, is largely rewritten, following another suggestion to make the paper more narrative for readers unfamiliar with heavy-ion-collision experiments.

line 68: significant not so clear, consider to leave

A: Done.

line 69: optimized for best 3barLbarH signal is not clear, perhaps larges number of signal (?)

A: Here we mean maximizing the significance, instead of maximizing the number of signal counts.

Figure 4: caption has twice represent close by

A: We have swapped the words "represent" and "show" so the same words are not used close by.

line 137: is commonly believed -> not so commonly, perhaps better: is often imagined (?)

A: Done.

Figure 5: would be interesting to see, where the thermal model lines for 155/156 MeV would be compared to the data

A: The thermal model is from the reference Phys. Lett. B 697,203–207 (2011). The parameter T=164MeV are obtained by fitting measured hadron yields (like pion, kaon, proton ...). The thermal results in Fig. 5 is not calculated by us, so we can not easily get the results with T=155MeV.

line 178: wave-function size -> extension of the wave functon(?)

A: Changed to "the spatial extent of H3L wave function", according to another suggestion.

line 185: great step forward -> significant step forward

A: Done.

line 305: year should be 2023

A: Done. And this reference is moved into "Methods" due to limit on number of references.

line 306: Should have at least a CDS number (or CERN Report number) to allow to find it

A: We have added the CERN Report Number. And this reference is moved into "Methods" due to limit on number of references.

line 322: Suggest to also cite here: @article{Reichert:2022mek,

author = {Reichert, Tom and Steinheimer, Jan and Vovchenko, Volodymyr and D\"onigus, Benjamin and Bleicher, Marcus},

title = "{Energy dependence of light hypernuclei production in heavy-ion collisions from a coalescence and statistical-thermal model perspective}",

eprint = "2210.11876",

archivePrefix = "arXiv",

primaryClass = "nucl-th",

doi = "10.1103/PhysRevC.107.014912",

journal = "Phys. Rev. C",

volume = "107",

number = "1",

pages = "014912",

year = "2023"

}

A: The maximum number of references required by the editor is 60. We already have more than 70 references for the last version. Although we'd like to cite this paper, we can not add it anymore.

Detailed comments on the methods part:

Figure 1: of a hypernucleus. -> of a hypernucleus and the according selection criteria.

A: Done.

lines 102/103: you should better describe what went into finding the absorption can be neglected. A size of 5-11 fm should lead to a significant absorption cross section and therefore to some absorption.

A: We made some rough estimations. We assume the radius of H3L is 5fm. Its estimated maximum cross section is 3.14*5*5(fm^2). When H3L flies 1cm in insulation nitrogen gas with density of 1.25e-3 g/cm^3, the probability of the reaction is 2.1e-5. Since the typical decay length is only a few centimeters, the reaction probability should be negligible. We did similar estimation for the beryllium beam tube with the thickness of only 0.4mm, the result absorption probability is also very small.

Figure 3: Consider to add the significances for each signal.

A: Done.

line 122: No particle yet discovered decays to -> No particle yet discovered decays weakly to

A: Done.

line 141 at -> to

A: Done.

Figure 5: not clear why the Blast-Wave curves of 3LH and anti-3LH are different when they use the same parameters…

A; The Blast-Wave curves of H3L and anti-H3L don’t use same parameters. The parameters are obtained by fitting their pT spectra independently.

Figure 5: Still confusing without proper legend in the Figure

A: We have made some adjustments to the legend.

line 159: +-20 ~ 30 is not clear

A: This sentence is now removed following another suggestion.

Referee #1 (Remarks on code availability):

The code is not really usable without the data as described in one answer by the authors. Nevertheless, having the code public is very good.

A: It's STAR routine not to have the code public. And as you echoed, the codes alone are not really useful without the data. Also, the codes are quite complicated, difficult to be used properly by a non-STAR member.

Referee #2 (Remarks to the Author):

I thank the authors for responding in detail to most of my comments and appreciate their willingness to review and modify the manuscript.

There are certainly many improvements in the revised version which further improved the presentation of the impressive experimental achievements.

However, I still find even the revised manuscript very technical and thus better suited to be published in a specialized physics journal. The bulk of the paper is written as a collection of facts describing the data analysis, rather than trying to build a narrative for readers unfamiliar with heavy-ion-collision experiments. Moreover, in my opinion, although this paper reports on important and significant results, they represent (to some extent) incremental advancement upon previous discoveries of A=2 anti-deuteron, A=3 anti-hypertriton, and A=4 anti-4He. The new measurements of the anti-L3H and anti-L4H lifetimes and their relation to matter-antimatter symmetry tests are the main 'selling points' of this paper. Unfortunately, the clear

message of their analysis is quite overshadowed by the section on production yield ratios. This necessarily brings a large amount of additional technical details and; furthermore, the results do not allow any clear conclusions to be made. In my opinion, it would be better to leave it out in favor of adding more background information on matter-antimatter symmetry which would make it more accessible to readers.

A: Follow your suggestion, we have polished the whole article, especially the "(Anti)hypernucleus reconstruction" part. We try to avoid to use the terminology that only experts can understand. Some necessary concepts are also given basic explanations. So that it's more narrative for readers unfamiliar with heavy-ion-collision experiments.

We consider the discovery of a new, most-heavy antimatter hypernucleus itself, the most important selling point. Discovery of antimatter particles is the first step towards studying any of their properties, including any matter-antimatter property differences. The lifetime difference measurement in this paper is not very precise yet, but it opens up the possibility to do similar studies in the future.

The production yield ratio results are also important. After the discovery of this new particle, a most natural following question is, how many are produced? With the yield ratio results, we show the readers that we observed the amount that we anticipated, and confirmed our previous understanding of

(anti)(hyper)nuclei production mechanism in heavy ion collisions. This, in turn, gives us some guidance to make even more complicated antimatter, from heavier antimatter (hyper)nuclei, to more.

Listed below are few specific remarks on the text:

- Line 35: The introduction of strangeness, hyperons, and hypernuclei seems to me quite short. It is also a bit misleading as it seems to me to put quarks on the same footing as asymptotic (detectable) particles.

A: We have rewritten this part, adding some basic concepts about the standard model, quarks and hyperons. We also try to avoid the misleading you mentioned.

- Line 60: m, Z is left undefined

A: The sentence is modified with m, Z defined.

- Line 97: "CPT theorem" applies to Lorentz-invariant local quantum field theories with Hermitian Hamiltonians. CPT symmetry does not come from "quantum physics itself". The text should be phrased more carefully.

A: We have changed "Quantum physic expects" to "One expects".

- Line 107: True in principle but hypernuclear lifetimes also depend on other (quite poorly known) inputs apart from nuclear and hypernuclear interactions which does not make them to be particularly "good" probes of the (hyper)nuclear interactions themselves.

A: We have changed "a good probe" to "an important tool".

- Line 113: "transverse momentum" might not be known to wider audience

A: We add the explanation "the momentum component perpendicular to the beam direction" in the parentheses here. Also this sentence is moved to Methods now.

- Line 119: It might not be clear at all, what is being "reconstructed"
- Line 121, 122: It might not be clear what "reconstruction of hypernuclei" actually means

A: "reconstruction" is now explained around Line 62.

- Line 124: "expression" -> ~ exponential decay law (formula)

A: Done.

- Line 129: It would be useful, for reference, to state the lifetime values, not only their differences.

A: Done.

- Line 130: What is the difference between CPT symmetry and matter-antimatter symmetry in this context?

A: Here the same properties (lifetimes) of matter particles as those of their corresponding antimatter particles, is equivalent to CPT symmetry. We have rephrased the sentences to make it more clear.

Fig.4: It would be appropriate to incorporate the L3H lifetime value of Ref. [36] as well. It should be also mentioned how the averages of past lifetime measurements were calculated or a Ref. given.

A: Rather than simply predicting a L3H lifetime, Ref. [36] (Ref. [29] in the new draft) actually builds a relationship between L3H lifetime and Lambda binding energy by theory. It predicts 4 different values of L3H lifetime, so that there is always one to match one of the existing contradicting experimental

measurement results from STAR and ALICE. Thus we think it's not a simple prediction to compare with our new result.

The averages of past lifetime measurements are calculated by a maximum-likelihood fit. This is added to the figure caption.

- Line 133: The sentence is not clear to me. How the measured yields can be sensitive to the production mechanism, given that there is one underlying mechanism? Does it refer to models?

A: We have changed "is sensitive to" to "carry information about". So that this sentence is in-line with the following contents of the section, that we introduced one most accepted mechanism, and showed that our new data also support it. There are other mechanisms or models, e.g. Int.J.Mod.Phys.E 5 (1996) 1-90. But this paper does not talk about them, because the general readers may not be interested in them.

- Line 140-143: It would be 'safer' to frame it in terms of strange baryons rather than quarks, with not so clear definition of mass.

A: Done. We have changed the comparison between strange and u, d quarks, to that between (anti)hyperons and (anti)nucleons.

- Line 146: "chemical freeze-out temperature" and "baryon chemical potential" might not be known to the audience It is too detail.

A: We adjusted the sentence a little bit, so that the readers will see "The parameters in the statistical thermal model" first, before reading (chemical freeze-out temperature T and baryon chemical potential mu_B) in the bracket. The symbols of these parameters also appear in the figure, so it's better to mention it. If the readers can not quite understand their meanings, it's fine that they just know these are the parameters of the model.

- Line 149: "different collision systems", "mixture collisions" - I find these terms quite abstract/technical and unnecessary to introduce in order to say that these were collisions of nuclei with different mass numbers.

A: We have changed "different collision systems" to "U+U, Au+Au, Ru+Ru and Zr+Zr collision systems" to make the meaning of "collision systems" here clearer.

- Line 154: "rapidity" might not be known to the 'target' audience

A: We add an explanation in parentheses: (i.e., the velocity component along the beam direction is in range of $|v_{z}|<0.604c$).

- Line 155: I strongly suggest to avoid the word "conventions" when it comes to adoption of particular values of the 2-body decay branching ratios. There is nothing conventional about it. I understand that this choice can be motivated in order to be consistent with previous analyses but it should be mentioned at least in the Supplemental Material. The values of R3,R4=0.25,0.5 are not universally accepted by the hypernuclear community. There is STAR's own value of R3=0.32(5)(8) [PRC97] and several other measurements, also for R4 - see https://hypernuclei.kph.uni-mainz.de/. I do not see a good reason to discard their uncertainties altogether. It should be at least mentioned, as well as that R3,4(anti hypernucleus) =R3,4(hypernucleus) was assumed.

A: We rewrote that sentence: "Due to the lack of conclusive theoretical or experimental results, we assume 0.25 as ..."

There are no conclusive theoretical or experimental results on these branching fractions. The theoretical results are usually with certain assumptions. Experiments usually measure the so called R3 and R4, which are not the branching fractions themselves, but the fraction of 2 body decay with pi- in all decays with pi-. The decays with pi0 or other daughter particles are usually not measured, due to experimental difficulties. So to calculate the branching fractions from R3 and R4, further assumptions are needed. For example, we can assume the branching fraction of all pi- channels of the hypernucleus are the same as that of Lambda (63.9%). With this assumption, we can calculate the hypertriton -> He3 pi- branching fraction as R3 * 63.9% = 0.32(5)(8)*63.9% = 0.20(3)(5), consistent the 0.25 assumed in the paper. Here R3 of the STAR PRC97 value you quoted is used. And this assumption is not very well grounded, since we already know that the hypertriton and hyperH4 lifetime is shorter than free Lambda.
In Fig.5, all the B.R. are assumed to be 0.25 and 0.5, which makes the comparison of the yield ratios easier. As stated above, we don't have much better knowledge so far to update these numbers, or quote well-grounded uncertainties on them.

Figure 5: I find it quite unfortunate to call composite nuclei particles. Also "... with the same baryon number" - the baryon number is positive(negative) for (anti)particles.

A: We have changed "particles" to "(anti)nuclei and (anti)hypernuclei". We have changed "with the same baryon number" to "with the same number of (anti)baryons".

- Line 178: "wave function size" is quite ambiguous; perhaps "wave function" -> rms radius or be more precise as ~ "the spatial extent of L3H wave function"

A: Done.

Methods

- Line 43-45: The definition of \chi^_topo,NDF,primary is not clear, as well as their relationship with "likelihood"

A: We are not trying to define these variables here. The definitions can be found in Ref. 5-8. We just would like to explain to the readers the relation between these variables and the geometrical distances.
We have rewritten these sentences, replacing "likelihood" with "deviation", to make it clearer.

- Line 88: It's not clear in what sense "(Anti)hypernuclei are simulated"

A: To make it clearer, these sentences have been modified to "(Anti)hypernucleus decay and the paths of their daughters are simulated…".

- Line 113: Is the V_{0}DCA the same as DCA_{V_0} in Fig. 1?

A: Yes. We changed V_{0}DCA in the text to DCA_{V_0}.

- Line 129: "M/Z" here while "m/Z" previously

A: "M" here is changed to "m" now.

Referee #2 (Remarks on code availability):

There is no manuscript-specific code (or instructions) which would allow one to reproduce the results presented in the manuscript.

A: It's STAR routine not to have the code public. And as you echoed, the codes alone are not really useful without the data. Also, the codes are quite complicated, difficult to be used by a non-STAR member.

Referee #3 (Remarks to the Author):

I would like to thank the authors for their work and for addressing the comments. I am in general happy with their replies. I would like to come back to some of their answers and to suggest a few more minor changes to the text.

General on the analyses: is the measured value of the hypertriton and H4L lifetimes used in the MC for the calculation of the efficiency correction?

A: We used the measured values of the hypertriton and H4L lifetimes from Phys. Rev. Lett. 128, 202301 (2022) in the MC simulations. That measurement has smaller uncertainties than this analysis.

Answers:

- On the B.R. of the decay: I am fine with not cluttering too much the plot with the B.R. uncertainties, I would still mention what are the typical uncertainties related to those in the text around line 156. For instance, from the R3 of the hypertriton ALICE estimated in Phys. Rev. Lett. 128, 252003, an uncertainty of 0.02 for the H3L->He3pi decay B.R.

A: The original sentence in Line 156 might be misleading that these numbers are somewhat precise. So we have changed it to "Due to the lack of conclusive theoretical or experimental results, we assume 0.25 as ...". Quoting specific uncertainty numbers here, as you suggested, will have to be somewhat arbitrary, so we have not done that. Because to calculate the branching fractions from R3, further assumptions are needed. For example, we can assume the branching fraction of all pi- channels of the hypernucleus are the same as that of Lambda (63.9%). But this is not very well grounded, since we already know that the hypertriton and hyperH4 lifetime is shorter than free Lambda.

Manuscript:

line 27: heaviest antimatter nuclear and hypernuclear -> use just hypernuclear

A: Done.

line 33: some antimatter to decouple from matter -> most antimatter to decouple from matter

A: This depends on at what stage of the collision system evolution we are talking about. In the previous sentence in the draft, we mentioned quark-gluon-plasma. If we consider the antiquarks in QGP as antimatter, then at top RHIC energy, more than half of the antiquarks end up in mesons, rather than forming antimatter (antibaryons). In the quark model, a meson consist of a quark and an antiquark. It hard to define meson is matter or antimatter. In this sense, it's not right that "most antimatter to decouple from matter".

Even in QGP, it is a thermalization system with hundreds of MeV temperature. The temperature is much higher than mass of up and down quark. So the quark-antiquark pairs are constantly being created and annihilated.

line 79: introduce Nsig and Nbg here, in line 83 they are used but not defined

A: Done.

line 87: "The significances are also calculated without setting certain signal invariant-mass regions." this sentence is rather unclear, while it is better formulated in the method section. In my taste it is not necessary to repeat it here the comparison among the two methods.

A: Done. We have removed "without setting certain signal invariant-mass regions."

line 135: "or on the order of 10^12 K" -> "that correspond to a 10^12 K"

A: Done.

line 177: ", if really exists," -> ", that is currently not statistically significant," or remove altogether this sentence

A: Done.

Additional material:

line 158: "On the other hand, we note that the total uncertainties of those ratio measurements are large, of the order of ±20 ~ 30% themselves." -> I am not sure I get your point here: it is true that the uncertainties themselves are large, but the comparison still yields a significant difference. I would suggest to rephrase this sentence to further elaborate your point or to remove this sentence. As you write the differences are 2.8 and 1.9 σ. Being two independent ratios, I guess that the combination of these discrepancy will be larger than 3σ. That looks like a large discrepancy, how much the effect of the centrality selection and pt coverage would reduce it?

A: We have removed the sentence.

When changing the centrality selection and pt coverage of the analysis, the H3L/He3 ratio almost doesn't change. The antiH3L/antiHe3 ratio increased by less than 1 sigma. So the centrality and pt coverage differences can not fully explain the difference.