

Magnetic character of the low-energy enhancement in ^{70}Zn

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This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Referee #1

(Remarks to the Author)

The authors report on measurements of the low-energy enhancement of the photon-strength function in ^{70}Zn . This enhancement of up to an order of magnitude has been observed over the last two decades in some nuclei, but is not present in other nuclei and is not explained by current theoretical models. This is particularly important to astrophysics, as this enhancement occurs at energies that are relevant to astrophysics, increasing neutron-capture cross sections (particularly for exotic neutron-rich nuclei, where the neutron-separation energies are lower) and thus changing elemental abundances in r-process nucleosynthesis calculations. In particular, although it has been shown that the enhancement has a dipole nature, it was not possible to say if it is E1 or M1 and theoreticians have considered both multipolarities.

The present paper uses two well-established methods: the Oslo Method, which extracts the nuclear level density and the gamma strength function simultaneously, but requires an external normalisation and the Shape Method, which is used to provide that normalisation. Using calorimetry avoids the issue of the Pandemonium Effect, where a large number of weak unobserved transitions, can lead to an underestimation of the gamma-ray strength. The novel use of an ion trap to separate the 6- ground state and the 1+ isomer as separate beams, allowed the analysis to be performed for two different populations of spin and parity. Here, the difference of parity is crucial, as it reveals a significant difference between the two parities. Comparison with theory shows this to be due to the suppression of E1 strength due to the lack of low-lying negative-parity levels and a low-energy M1 enhancement.

As far as I know, this is the first time that it has been shown experimentally, that the enhancement is of M1 nature and this is an important result, which should inform the development of theories to explain this enhancement, which are much needed for astrophysical models. As such I recommend publication with the few minor corrections below.

Fig. 1: it would be useful to include the spins of the ground state and isomer in the legend.

Fig. 2: in the figure, E1 is written in purple and M1 is written in green, but in the caption, it is the other way round.

Fig. 3: in the legend, the solid black line is given as E1+M1+LEE, but in the caption, it is only given as E1+M1.

Line 62: it would be good to give both the term "gamma-ray strength function" and the other commonly used term "radiative strength function" as both are found in the literature.

Line 244: Fig. 5 (a) and (d) should be referenced in the caption in the same way the other subfigures are referenced.

Line 252: The "Ť" (calligraphic T) does not show up on the PDF. It appears correctly in the .docx file, so this maybe an issue for the editors. The authors should pay particular attention to this in the proofs.

Line 307: As these are git trees, it would be useful to reference the specific commit that was used in the analysis.

Line 385: The issue number for ref. 17 is wrong. It should be 108 not 16.

Line 395: "neodymium" is misspelled.

Referee #2

(Remarks to the Author)

This is a nice paper, implementing a beautiful new idea in order to address the electromagnetic decay of atomic nuclei. Aside from providing

important insights on nuclear structure, this has a direct impact on neutron capture rates, and, therefore, on a variety of societal applications and astrophysics.

In particular, the authors address the nature of the so-called low-energy enhancement (LEE) in the gamma-ray strength function,

which is, essentially, the probability for gamma emission during the nuclear decay as a function of the gamma energy. The LEE has drawn a considerable amount of theoretical and experimental attention since its discovery about 20 years ago. It consists in an enhancement of low-energy gammas in some nuclei with respect to the expectations based mainly on the electric dipole (E1) strength arising from the decay of the giant dipole resonance, a conspicuous mode of collective nuclear excitation. The main result of the paper is the identification of the nature of the LEE as magnetic dipole (M1) transitions.

This work is an important and novel step towards the identification of M1 gamma rays as responsible for the LEE, at least in some nuclei. However, it is also fair to say that, arguably, most bets were already on M1: first, the dipole nature of the radiation had already been experimentally established. Secondly, most theoretical calculations pointed already to the magnetic nature of the gamma rays. Many of the relevant papers addressing these facts are correctly cited by the authors. I do not believe that this is in detriment of the novel nature of the present paper.

The basic idea is beautiful and simple, but, in order to be implemented, a sophisticated combination of different experimental techniques had to be put to work:

Isomer separation and selection, and total absorption spectrometry: by selecting the parity of the decaying nucleus (either the 6- ground state or the 1+ isomer), the E1 transitions of energies in the range of the LEE are suppressed in the isomer decay as compared with the ground state decay. The remaining gamma rays in the isomer decay are then assigned to M1 radiation. In order for the LEE=M1 conclusion to be solid, it is essential that this assignment, as well as the fit of the energy dependence of the E1 and M1 contributions are robust. This can be clearly seen in Fig. 3, which I consider to be the most important figure in the paper.

The paper is well written and very clear, all the relevant technical aspects are well explained, either explicitly, or by pointing to the relevant literature.

Let me now make some questions and comments.

1) Why ^{70}Zn ? Of course the gs vs isomer structure of the parent ^{70}Cu is essential, but, are there other interesting candidates?

2) No mention is made of E2 transitions, which can also connect states of the same parity. I think a brief discussion is in order, in particular for ruling it out as a candidate for the nature of the isomer decay.

3) After mentioning some relevant previous papers exploring the nature of the LEE, the authors claim that "...no clear conclusion has been drawn on whether the LEE is of electric or magnetic character (E1 or M1)." This is arguably true, but it would be fair to acknowledge that almost all the theoretical studies predict M1 (with some exceptions, like [30]).

4) The authors point to [39] to check the distribution of the excitation energies of ^{70}Zn produced in the beta decay. Since this reference is not particularly easy to find (it is a PhD thesis), I will like to have a few words about how it looks like. Not much, just a mention about the "typical" range of excitation energies populated.

5) Authors: "We use Gogny+Hartree-Fock-Bogoliubov (HFB) calculations of the NLD from the Hauser-Feshbach code TALYS..." I guess they mean something like "We use Gogny+Hartree-Fock-Bogoliubov (HFB) calculations of the NLD used by the Hauser-Feshbach code TALYS...", since TALYS itself doesn't calculate NLD's.

6) In Fig. 2 (c) the authors use theoretical calculations for the NLD. It would be interesting to compare them to the known discrete levels of well assigned spins and parities. In particular, with the current situation Fig 2(c) doesn't seem to be exactly compatible with the states in 2 (a,b).

7) Authors: "To qualitatively demonstrate the suppression of the E1 strength function we first model the experimental γSF extracted following the β decay of the 6- ^{70}Cu gs state (blue band in Figure 1)". I'd like a word about how the decay is "modeled". Maybe with TALYS? Where does the width of the corresponding band come from?

- 8) Authors: "An additional exponential component was added to account for the M1 LEE." Although this is rather common practice, it would be good to mention whether there is any theoretical support for this energy dependence, as opposed to being a purely phenomenological fit. In addition, it seems to me that the fit of the energy dependence of the M1 strength is one of the most important elements on which the robustness of the conclusions is based, since it allows for the extrapolation to smaller energies, where the gs and isomer decays are no longer different. I feel that the authors should comment on this, and assess (even if only qualitatively) the uncertainty introduced by this procedure. This is my main concern: that the mechanism at play for lower energies (up to about 2 MeV) might be different, for example, decays between excited states from an excitation energy region where positive and negative parity states are better balanced. In this situation, E1 transitions might recover and be present at these lower energies, unless I'm missing something.
- 9) Authors: "The amplitude of the E1 and M1 exponential LEE components were allowed to freely vary." I'm a bit puzzled about the variation of the E1. The authors claim that "the shape of the E1 strength was based on the Gogny+HFB with QRPA model (using the D1M version of the Gogny force) by Goriely et al." Can they vary this, and still keep the good reproduction of the GDR shown in Fig. 1?
- 10) Authors: "To quantify the impact of the parity asymmetry on the extracted γ SF we determined an E1 reduction factor for each γ -ray energy". I admit that the nature and rationale for this procedure are a bit obscure to me. How is this reduction factor extracted, exactly? I think the current version is too vague on this respect. The primary gammas in the energy region of interest populate only positive parity states, with the exception of a 3038 keV 5- and a 1859 keV 3-. If the primary gammas are the ones to be modeled, it seems that one would only need to compute the discrete transitions to these states as a function of the continuous initial excitation energy... On the other hand, is it really needed in order to establish the M1 nature of gammas in the energy range of interest? The contribution of the reduced E1 to the total strength seems to be quite negligible according to Fig. 3.
- 11) Fig 3 summarizes the main results of the paper, associated with the gs and isomeric decays. I wonder if anything is known experimentally in that energy region associated with the decay from a compound nucleus produced in some different way, where the initial parity is not constrained.
- 12) In the Methods section, the authors write "We assume, as we do with the standard Oslo method, that the primary γ -decays into Lj are dipole transitions. For the matrix populated from the β decay of ^{70}Cu (Figure 5 (a)), we restrict our analysis to two discrete states: EL1 = 885 keV and EL2 = 1759 keV, where both states have identical spin $L_{1,2} = 2^+$ ". Since the parity of the compound nucleus is positive in this case, the transition could also be E2. If it is reasonable to rule this out, the authors might want to state it explicitly.

Summing up, this is a good, novel, and relevant paper, regardless of the solidity of the main conclusion, which is the assignment of the LEE to M1 transitions. Concerning this essential point, I would like to have addressed my few points above, most importantly point 8) regarding the extrapolability of the M1 assignment below ~ 2 MeV.

Referee #3

(Remarks to the Author)

I have been asked to provide a review for the manuscript "On the Nature of Low-Energy Electromagnetic Radiation in Atomic Nuclei" submitted by E.K. Ronning et al. to Nature. While I commend the authors on performing this challenging and important measurement for the field of nuclear physics, that allowed them to access the gamma-ray strength functions (gSF) resulting from the decay of different classes of states (with different spin and parity), I do not think that based on their observation they can claim conclusively that the low-energy upbend is of magnetic character. Either additional experimental data, like polarization measurements (which are not available from this experiment), or supporting theoretical (microscopic) calculations of the two gSF resulting from the ^{70}Cu g.s. and ^{70}Cu isomer decays need to be provided to allow the authors to make such a strong claim. The experiment is also blind in a significant gamma-ray energy range, i.e., over a range of about 5 MeV (mainly because of the Q value of the decays). A significant deviation from the (g,n) data is observed just around 10 MeV, where especially the gSF "determined" for the ^{70}Cu g.s. decay deviates from the data. This could influence the shape of the gSF at lower energies as the authors do not know whether additional components would need to be added.

Furthermore, I would like the authors to comment to what extent their findings violate the Brink-Axel hypothesis, as the two gSF, shown in Fig. 1 of their manuscript, are clearly different. This discussion is missing entirely. Also, most available data point to the LEE only being part of the gSF in the decay channel, which would violate the Brink-Axel hypothesis per se. Because of that, its influence on nucleosynthesis processes, as mentioned in the introduction of the manuscript, might be entirely different than shown in some work where it was assumed that the LEE is also part of the g.s. gSF.

The main concern though is that there are significant differences between the results presented in this manuscript and those

presented in the cited PhD thesis (available online) that led to the results of this manuscript.

Some additional comments before my closing remarks:

- 1) Lines 75ff: The authors fail to mention the important work of Richter et al. on the scissors mode. At least, the review article by Heyde, von Neumann-Cosel, and Richter should be cited, Rev. Mod. Phys. 82, 2365 (2010).
- 2) Line 80: Probably this review article should be cited for the PDR as well: D. Savran, T. Aumann, A. Zilges, Prog. Part. Nucl. Phys. 70, 210 (2013), as at least from an experimental perspective it was the first extensive review. There are several others, which were published in recent years, that are commonly cited.
- 3) Lines 85ff: "The presence of the LEE has significant impact on predictions of nuclear reaction rates; ..." Probably a more nuanced discussion should be adopted, mentioning how the LEE would impact nucleosynthesis processes if it was only part of the decay gamma-ray strength function. See comment above.
- 4) Lines 95ff: As indicated above, I strongly feel that the claim that the authors "conclusively" showed that the LEE in ^{70}Zn is of M1 character is too strong.
- 5) Line 105 (assumption of allowed beta decays): Can the contribution from forbidden beta decays be estimated? Does this assumption bias the interpretation of the data? Do measured $\log(ft)$ values support this assumption?

The authors chose to report data on the $1+$ isomer decay and $6-$ g.s. decay, even though data for the $3-$ isomer decay appear to be available, too. They are part of the cited PhD thesis. The cited PhD thesis also shows the beta feeding compared to shell-model predictions (Fig. 4.11 of the thesis). While for the $3-$ and $6-$ decays good (excellent) agreement is observed, the agreement is much worse for the $1+$ isomer. The latter is, however, used in this manuscript to discuss the LEE. Troubling is that Fig. 5.20 of the thesis shows very different results than Fig. 4.11, as I understand after the SuN upgrade to SuN++, deteriorating the agreement with theory for the $1+$ isomer further and also significantly changing the result for the $3-$ isomer. Can the authors comment on these deviations and how they would impact their results?

6) Line 108 and GDR data: The authors refer to the data as (g,n) data. The (g,p) threshold opens ~ 11.1 MeV, the $(g,2n)$ threshold around 15.7 MeV, thus, both should contribute to the shape of the gSF in the region of the IVGDR. Are these decay channels known or do they contribute to the uncertainty of the data? The authors report the gSF in ^{70}Zn , so $^{70}\text{Zn}(g,X)$ should be sufficient. Why is $^{68}\text{Zn}(g,n)$ also used?

7) Figure 1:

i) The main concern (!) is that Fig. 1 of the manuscript is very different from Figure 7.1 in the cited PhD thesis. Obviously, the normalization was changed. Ambiguities with the normalization are also discussed in the manuscript. The same discussion (alpha scaling) is presented in the thesis, with a very different outcome though; that means Fig. 6 of the manuscript and Fig. 6.16 of the thesis, which compare the Oslo-method and shape-method results, are also very different and likely ambiguous. Obviously, the "data" was further tweaked to obtain agreement of the two gSF for energies lower than 2 MeV, which likely does significantly impact the interpretation of the data and the fit presented in Figure 3 of the manuscript; i.e., only after this tweak a suppression of the proposed E1 part will be observed and lead to the claim that the LEE part is the same for both gSF. In the thesis, one might claim that there is no LEE for the gSF observed from the ^{70}Cu ground state, or that it is strongly suppressed.

ii) The reduction factor applied to the E1 gSF should be shown as a function of E_g , possibly as an inset to Figure 1.

iii) Fits in Fig. 1: How was the width of the M1 component determined? How does the E1 fit compare to IVGDR data? Is anchoring of data at ~ 15 MeV a flaw because (g,p) might already contribute (see comment above)?

iv) The extracted level density determined from the ^{70}Cu g.s. decay is questionable, too, and points to problems with the normalization as the number of discrete levels, including the ground state, is not reproduced. The NLD determined for the decay of the $1+$ isomer looks better at energies where level counting seems still possible (Fig. 7.4 in the thesis). The disagreement with the predicted level densities, as shown in the thesis, also partly questions the argument presented for Fig. 2 (c). Some of the same authors have suggested normalization to theory predictions in their earlier studies. How would that change the results presented in this manuscript?

v) Given the sketch in Fig. 2, why should one expect that the M1 strength is the same for the two cases (a) and (b); i.e., that it is the same in the two gSF shown in Fig. 1? As sketched in Fig. 2, more high-energy M1 transitions should be observed from states in the quasi-continuum in case (b) than in case (a). Still, the authors do not vary the M1 strength. So, this effect might also counteract the suggested E1 suppression for the gSF resulting from the ^{70}Cu isomer decay, which as said, depending on the normalization of the data, might not be as pronounced. This needs to be explained better if there is any resolution.

In conclusion, the claim presented in this manuscript is too strong and the data might not unambiguously support it. Without a doubt, proving experimentally that the LEE is of M1 character and that it is not a fundamental part of the gSF would be important for the field of nuclear physics and nuclear astrophysics. However, the manuscript cannot be accepted in its current form and a revision is needed. The authors must satisfactorily address the concerns.

Version 1:

Reviewer comments:

Referee #1

(Remarks to the Author)

The authors have addressed all of my comments. I recommend publication.

Referee #2

(Remarks to the Author)

I thank the authors for their very careful and detailed address of all of my initial concerns. As I wrote in my initial review, I considered all of them to be of relatively minor importance, with the exception of what I labeled as point 8). This query pointed to the robustness of their conclusions, and it has been addressed with special care by the authors. I have found their clarifications quite illuminating, and I now tend to hold their main conclusions as a rather strong and convincing result. All the other points were also answered to my full satisfaction. I believe this paper would be a very valuable addition to the relevant literature associated with nuclear electromagnetic decay.

Referee #3

(Remarks to the Author)

The authors provided a detailed response to my initial report. I do, however, still have concerns about the uniqueness of their result and the strong assumptions, i.e., model dependency leading to their final conclusion about the LEE being of M1 character. In their comment #8, Referee #2 expressed these concerns as well and I again feel that the authors' answer is solely based on arguments coming from model expectations rather than experimental data. I do not think that they can exclude a low-energy E1 component from their data. Furthermore, the theory arguments come from a theory (HFB+QRPA at best) that does not include complex configurations. Such configurations do influence both the level density in the quasicontinuum and the strength fragmentation. They will also impact the decay behavior between states in the quasicontinuum as wavefunctions are more mixed. The authors themselves state, including in their response letter, that E1 transitions are typically "stronger" than M1 transitions. In fact, below particle-emission thresholds, this is not necessarily true. Gamma widths can be quite comparable or be even larger for M1 transitions. But for the sake of the argument let us assume that this statement was true at all energies, then the experimental data cannot exclude a significant E1 contribution to the upbend (LEE) as E1 transitions should then be stronger between states in the quasicontinuum. As the observed counts in their primary matrix are modeled to be the product of the level density and gamma-ray transmission coefficients, stronger E1 transitions might counteract the lower density of the states needed to observe E1 transitions and, thus, lead to the same result as a higher level density and weaker M1 transitions. Of course, as also pointed out by the other referees, most theories do expect the LEE to be of M1 character (mostly shell-model calculations, which cannot correctly describe E1 strengths because of restricted model spaces) and to result from low-energy transitions between states in the quasicontinuum. I am concerned that the authors' interpretation of their data and resulting normalizations between the gSF from the Cu-70 g.s. and Cu-70 isomer are biased by expectations for the LEE and theory predictions. To support publication, I would, thus, urge the authors to, at minimum, refrain from using words like "conclusively" in their presentation.

A few more comments.

a) Response letter (not in order).

- "... we do present the comparison of a microscopic gSF model ...", as also pointed out above, this model is very restricted. The QRPA only includes 1p-1h excitations. Furthermore, I am certain that the QRPA does not predict the LEE. Typically, the QRPA also only predicts the gSF built on the ground state and cannot model decays. Extended models are very recent and it is unlikely that these were already added to TALYS or used in this work. I am also sure that this model would not predict the suppression of the E1 transitions. Do the mentioned shell-model calculations actually get that?

- "... this work since we were able to provide an interpretation based on experimental observations.", as mentioned above, the authors normalized their data based on expectations from a theoretical model, their data are, thus, not a clear experimental observation. The data was "manipulated".

- "This gSF has a suppressed E1 gSF and is not a "true" gSF (containing the full E1 and M1 strengths), thus it is unphysical to extrapolate it to the GDR.", while I appreciate that the authors deleted this extrapolation, the reduced E1 component is still derived from the tail of the GDR. So, technically, while not plotted in Fig. 1 of the manuscript, a part of the tail of the GDR is still part of the fit in Fig. 3 and the strength of the part is determined by anchoring the data to the (γ, n) data and scaling it by the parity asymmetry reduction factor shown in the response letter. This is an additional bias and the outcome would also be different if there were another low-energy E1 "bump" from, e.g., a mode like the PDR.

- "The referee asks, "how was the width of the M1 component determined?" ...", the LEE component does not have a width. So, this question was asking about the width of the M1 component at higher energies, not the M1 LEE component.

- "[...] In Fig. 1, we do not attempt to disentangle the contributions of E1 and M1 fit but assume GLOs are some combination

of the two components." What does that mean? How was the extrapolation to the GDR region done? Which GLOs were fitted? Obviously, the components are shown individually in Fig. 3 (HFB "predictions") and were "scaled" such (?) to describe the "data". Does the black curve in Fig. 3 also describe the GDR data? I would like the authors to show the plot expanded to the GDR region and to add the comparison to the manuscript.

- "As a result, we currently do not consider our result to be a violation of the gBA hypothesis." The gBA hypothesis states that "that the g-ray strength function depends only on the transition energy, not on the specific initial or final excitation energy, spin, or parity." → In my opinion, the authors show that the gSF depends on initial and final spin and parity (both of which they select in their experiment), or alternatively, they show that the statistical model description breaks down for their case. So, is the "different availability of states" a violation of the gBA hypothesis?

- "However, as our interpretation of the LEE developed, it became clear that the gSF extracted from $^{70}\text{Cu}^{m2}$ was not the full gSF, but a gSF with suppressed E1 transitions. Therefore, this gSF was renormalized in magnitude so there was agreement at low energies." ... yes, this is exactly the problem. The renormalization was a choice guided by theoretical expectations, some of which might not be correct. Without this renormalization and the connected suppression of the E1 strength, the result would be quite different as shown in the PhD thesis.

b) Revised manuscript

- line 43: "... and how the nucleus is structured." This is a funny formulation. Maybe "how nuclear structure emerges" or "impacts our understanding of [...] and nuclear structure."

- line 44: "... which show conclusively", please avoid "conclusively" and choose softer claim. Any difference in the two extracted gSF is interesting enough in my opinion.

- line 78f: "... to arise from the collective behavior of the nucleus, ..." This is very vague. Please be more specific here.

- line 96: Please avoid "conclusively".

- line 113: "... or in previous publications." The PhD thesis is not a publication and the manuscript submitted to PRC has not been published yet.

- lines 136ff: "... does not appear to violate the gBA hypothesis, ..." Maybe have a more nuanced discussion here. See my comments above.

- line 173: "... because those transitions are not allowed to decay to..." Transitions do not decay, states do. Please correct.

- line 177: "... we loop over ..." You loop where? TALYS, Shapelt, Sorting Code? Please be more precise.

In summary, I think that the authors performed a remarkable experiment which warrants publication. However, I want them to address my comments listed above, implement changes accordingly, and to avoid the use of words like "conclusively" in their manuscript since the renormalization of their data intimately depends on the interpretation of the data they are seeking for. Work out the model dependency of the interpretation more clearly and then I support the publication.

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The authors would like to thank the referees for taking the time to review our manuscript and provide helpful and constructive feedback. Please find below detailed answers to the proposed questions and concerns. Our replies are in purple and any “quotations from the text” are presented in blue. We hope the referees find our replies and corrections to be satisfactory.

Referee #1 (Remarks to the Author):

The authors report on measurements of the low-energy enhancement of the photon-strength function in ^{70}Zn . This enhancement of up to an order of magnitude has been observed over the last two decades in some nuclei but is not present in other nuclei and is not explained by current theoretical models. This is particularly important to astrophysics, as this enhancement occurs at energies that are relevant to astrophysics, increasing neutron-capture cross sections (particularly for exotic neutron-rich nuclei, where the neutron-separation energies are lower) and thus changing elemental abundances in r-process nucleosynthesis calculations. In particular, although it has been shown that the enhancement has a dipole nature, it was not possible to say if it is E1 or M1 and theoreticians have considered both multipolarities.

The present paper uses two well-established methods: the Oslo Method, which extracts the nuclear level density and the gamma strength function simultaneously, but requires an external normalization and the Shape Method, which is used to provide that normalization. Using calorimetry avoids the issue of the Pandemonium Effect, where many weak unobserved transitions, can lead to an underestimation of the gamma-ray strength. The novel use of an ion trap to separate the 6- ground state and the 1+ isomer as separate beams, allowed the analysis to be performed for two different populations of spin and parity. Here, the difference of parity is crucial, as it reveals a significant difference between the two parties. Comparison with theory shows this to be due to the suppression of E1 strength due to the lack of low-lying negative-parity levels and a low-energy M1 enhancement.

As far as I know, this is the first time that it has been shown experimentally, that the enhancement is of M1 nature and this is an important result, which should inform the development of theories to explain this enhancement, which are much needed for astrophysical models. As such I recommend publication with the few minor corrections below.

The authors would like to thank the referee for their summary of our work and for their recommendation for publication. We have made all the suggested corrections to the manuscript. Please find these modifications detailed below.

Fig. 1: it would be useful to include the spins of the ground state and isomer in the legend.

We have added the spins of the ground and isomeric states to the legend. We have also removed the orange extrapolation line to address comments/questions from referee #3. The updated Figure 1 and legend are shown below:

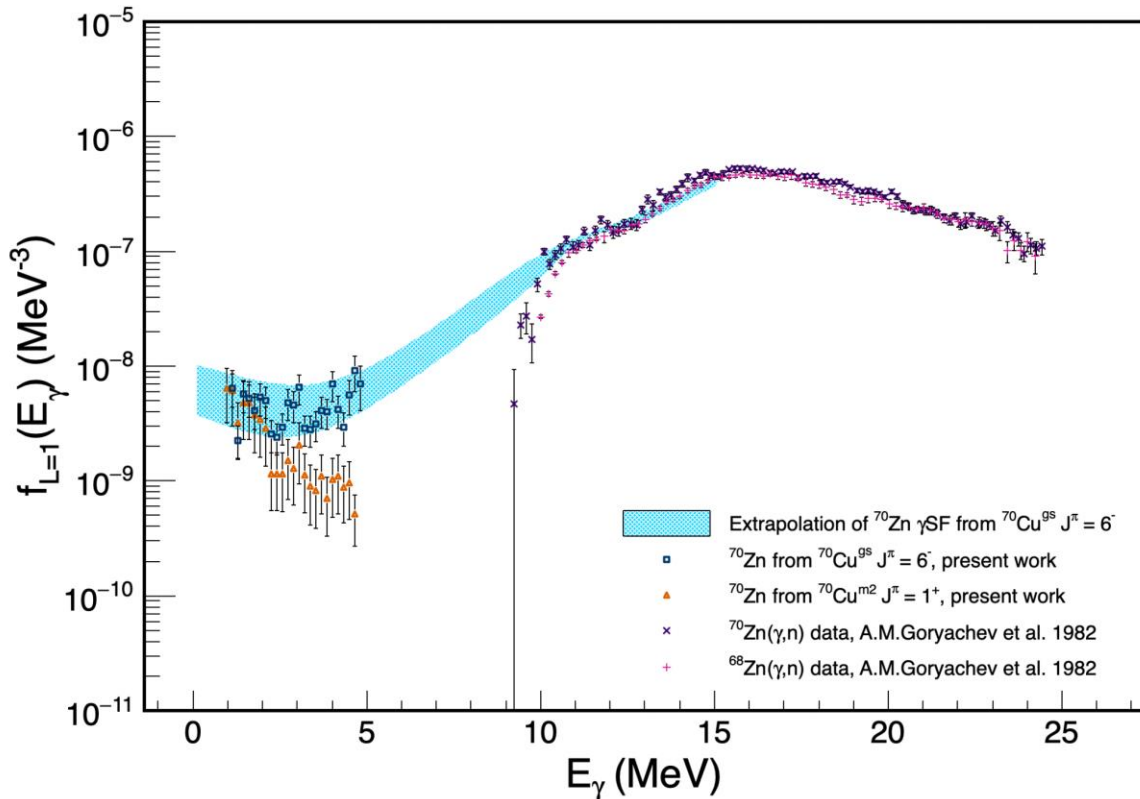


Figure 1: The γ -ray strength functions of ^{70}Zn extracted from the β decays of $^{70}\text{Cu}^{m2}$ (orange triangles) and $^{70}\text{Cu}^{gs}$ (blue squares). The data from the β decay of $^{70}\text{Cu}^{gs}$ are normalized in magnitude and extrapolated to GDR data from $^{70}\text{Zn}(\gamma, n)^{69}\text{Zn}$ and $^{68}\text{Zn}(\gamma, n)^{67}\text{Zn}$ measurements by Goryachev *et al.*⁴⁴, shown in the blue band. The absolute normalization of the data following the β decay of $^{70}\text{Cu}^{m2}$ is relative to the blue band.

Fig. 2: in the figure, E1 is written in purple and M1 is written in green, but in the caption, it is the other way round.

The caption has now been modified to read:

"Here transitions of M1 character are shown in green, and transitions of E1 character are shown in purple."

Fig. 3: in the legend, the solid black line is given as E1+M1+LEE, but in the caption, it is only given as E1+M1.

The caption has now been modified to read:

"Here the E1+M1+LEE γ SF from Gogny+Hartree-Fock-Bogoliubov (HFB) calculation in TALYS 1.96⁵¹ is shown as the solid black line."

Line 62: it would be good to give both the term "gamma-ray strength function" and the other commonly used term "radiative strength function" as both are found in the literature.

We have modified the text to now read:

"The resulting measured γ -ray energy emission is no longer discrete but instead follows a largely continuous distribution as a function of energy, known as the radiative strength function or γ -ray strength function (γ SF)^{8,9}. "

Line 244: Fig. 5 (a) and (d) should be referenced in the caption in the same way the other subfigures are referenced.

We have modified the figure caption for Fig. 5 to make sure it mentions all sub-panels in the figure. Please find the modified figure caption below:

"Figure 5: The raw, unfolded, and primary E_x vs E_γ matrices for ^{70}Zn , populated via the β decay of $^{70}\text{Cu}^{\text{m}2}$ in panels (a)-(c) and $^{70}\text{Cu}^{\text{gs}}$ in panels (d)-(f). The binning of panel (a) is 20 keV/bin and of panels (d) and (e) are 10 keV/bin. Panels (b) and (c) have binning of 80 keV/bin and panel (f) has binning of 60 keV/bin. The presence of diagonals corresponding to a strong population of certain levels in ^{70}Zn is highlighted in each raw matrix in purple and these diagonals were used for the Shape method. The $2^+_{1,}$ and $2^+_{2,}$ states are highlighted in the raw matrix populated from $^{70}\text{Cu}^{\text{m}2}$ (a), and the $4^+_{1,}$ and $4^+_{2,}$ states are highlighted in the raw matrix populated from $^{70}\text{Cu}^{\text{gs}}$ (d). Panels (b) and (e) correspond to the detector response-corrected "unfolded" matrices. The region outlined in black in panels (c) and (f) represent the regions in the primary matrix from which the γ SF was extracted with the β -Oslo method."

Line 252: The "T" (calligraphic T) does not show up on the PDF. It appears correctly in the .docx file, so this maybe an issue for the editors. The authors should pay particular attention to this in the proofs. We thank the referee for noting this discrepancy between the PDF and the .docx file. The authors will pay attention in the proofing stage to make sure this is appropriately translated.

Line 307: As these are git trees, it would be useful to reference the specific commit that was used in the analysis.

The "Code Availability" section now reads as follows:

"**Code Availability:** The codes used for the analysis of the present data are publicly available on github: <https://github.com/oslocyclotronlab/oslo-method-software>, <https://github.com/dennismuecher/Shapelt>. The last commit to the Oslo codes at the time of download by the authors was commit 21e45ce. The last commit to Shapelt at the time it was downloaded by the authors was commit b7666a8. It should be noted that the response functions for SuN++ were created for this analysis and are not present in the referenced commit here."

Line 385: The issue number for ref. 17 is wrong. It should be 108 not 16.

The revised reference now reads:

"Wiedeking, M. et al. Low-Energy Enhancement in the Photon Strength of ^{95}Mo . *Phys. Rev. Lett* **108** 162503 (2012)."

Line 395: "neodymium" is misspelled.

The spelling for "neodymium" has been corrected:

"Guttormsen, M. et al. Evolution of the γ -ray strength function in neodymium isotopes. *Phys. Rev. C*. **106**, 034314 (2022)."

Referee #2 (Remarks to the Author):

This is a nice paper, implementing a beautiful new idea in order to address the electromagnetic decay of atomic nuclei. Aside from providing important insights on nuclear structure, this has a direct impact on neutron capture rates, and, therefore, on a variety of societal applications and astrophysics.

In particular, the authors address the nature of the so-called low-energy enhancement (LEE) in the gamma-ray strength function, which is, essentially, the probability for gamma emission during the nuclear decay as a function of the gamma energy. The LEE has drawn a considerable amount of theoretical and experimental attention since its discovery about 20 years ago. It consists in an enhancement of low-energy gammas in some nuclei with respect to the expectations based mainly on the electric dipole (E1) strength arising from the decay of the giant dipole resonance, a conspicuous mode of collective nuclear excitation. The main result of the paper is the identification of the nature of the LEE as magnetic dipole (M1) transitions. This work is an important and novel step towards the identification of M1 gamma rays as responsible for the LEE, at least in some nuclei. However, it is also fair to say that, arguably, most bets were already on M1: first, the dipole nature of the radiation had already been experimentally established. Secondly, most theoretical calculations pointed already to the magnetic nature of the gamma rays. Many of the relevant papers addressing these facts are correctly cited by the authors. I do not believe that this is in detriment of the novel nature of the present paper.

The basic idea is beautiful and simple, but, in order to be implemented, a sophisticated combination of different experimental techniques had to be put to work: Isomer separation and selection, and total absorption spectrometry: by selecting the parity of the decaying nucleus (either the 6- ground state or the 1+ isomer), the E1 transitions of energies in the range of the LEE are suppressed in the isomer decay as compared with the ground state decay. The remaining gamma rays in the isomer decay are then assigned to M1 radiation. In order for the LEE=M1 conclusion to be solid, it is essential that this assignment, as well as the fit of the energy dependence of the E1 and M1 contributions are robust. This can be clearly seen in Fig. 3, which I consider to be the most important figure in the paper.

The paper is well written and very clear, all the relevant technical aspects are well explained, either explicitly, or by pointing to the relevant literature.

Let me now make some questions and comments.

The authors would like to thank the referee for their nice summary of our manuscript on the novelty of the technique and for their comments and questions. Please find below detailed responses to each question and comment. We hope our responses and modifications are satisfactory.

1) Why ^{70}Zn ? Of course the gs vs isomer structure of the parent ^{70}Cu is essential, but, are there other interesting candidates?

^{70}Zn was chosen, for a number of technical reasons and to provide a compelling case to demonstrate the M1 nature of the LEE including: (1) the presence of multiple isomeric states in ^{70}Cu spanning a range of spins and both parities as recognized by the referee, (2) a Q-value appropriate for the β -Oslo and Shape methods, (3) the ability to produce large quantities of ^{70}Cu at a FRIB, and (4) a decay into a stable nucleus to avoid the complexity of background subtraction. Now that we have successfully demonstrated this technique it can be extended to other interesting isomeric candidates exhibiting a range of spins or parties. For example, the β decay of ^{128}In from the ground state (spin 3^+) and first isomeric state (spin 8^-) could be used to perform a similar test in the γ SF of ^{128}Sn . In addition, there are many known isomeric states of varying spins and parity along the Y and Co isotopic chains that could be used to probe strength functions systematically along neutron-rich Zr and Ni elemental chains, respectively. Many of the isotopes mentioned have the appropriate Q-values, isomer energy separations, and production rates to feasibly perform these experiments.

2) No mention is made of E2 transitions, which can also connect states of the same parity. I think a brief discussion is in order, in particular for ruling it out as a candidate for the nature of the isomer decay.

We thank the referee for pointing out the lack of discussion regarding E2 transitions. We have added text to the paper to address this concern.

It is true that given the spin assignment of $^{70}\text{Cu}^{m2}$ (1^+), the assumption of allowed Gamow-Teller β decay (population of 0^+ , 1^+ , and 2^+ states), and the low-lying structure of the ^{70}Zn level scheme presented in Fig. 2a,b, that E2 decays are a possible decay mode, particularly at low energies. However, when we apply the β -Oslo and Shape methods, we extract our data in the region of the quasi-continuum, where dipole transitions are known to dominate the decay. We have added a sentence in the methods section to address this assumption:

"We assume, as we do with the standard Oslo method, that the primary γ -decays into L_j are dipole transitions, as dipole transitions are known to dominate decay in the quasi-continuum⁶⁶."

We know that the E2 component of the γ SF is orders of magnitude lower in strength than the E1 and M1 components (see Ref. 66) and that the LEE is of dipole nature (see Ref. 35). The predicted high-energy γ -ray strength functions for E2 transitions are expected to be roughly three orders of magnitude below the E1 γ SF (thus, two orders of magnitude below the M1 component of the γ SF presented in Fig. 3 of the manuscript). These expectations align with experimental demonstrations that strength functions are dominated by dipole transitions. As a result, we feel comfortable with the assumption that E2 decays are negligible here, and we hope the clarifications we have added to the manuscript address these concerns.

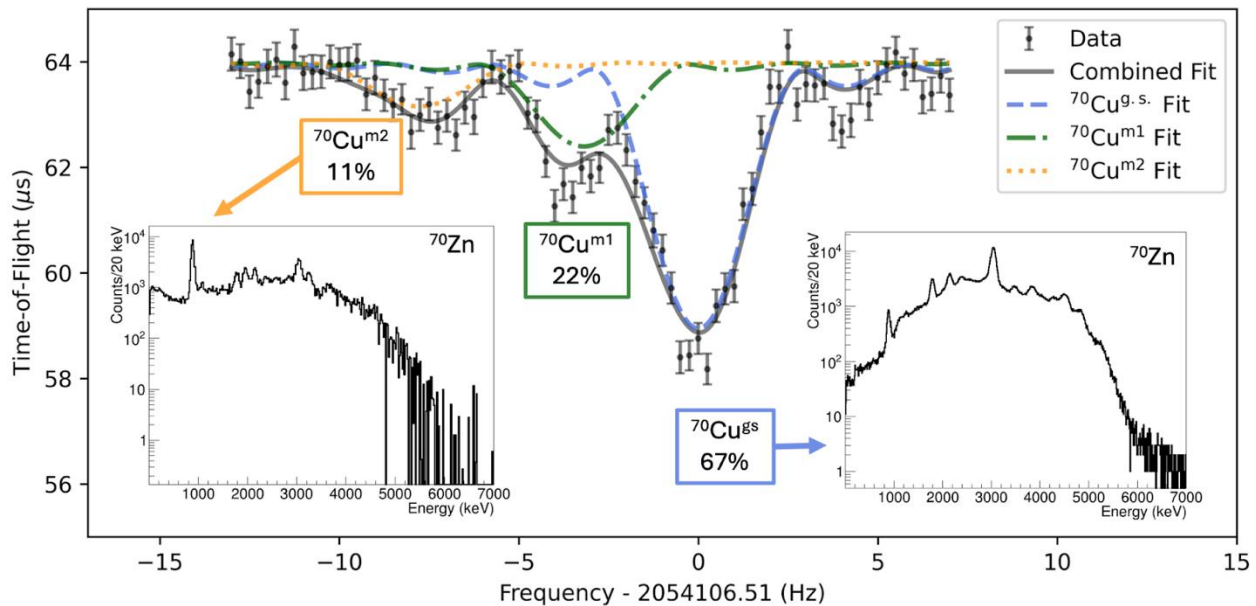
3) After mentioning some relevant previous papers exploring the nature of the LEE, the authors claim that "...no clear conclusion has been drawn on whether the LEE is of electric or magnetic character (E1 or M1)." This is arguably true, but it would be fair to acknowledge that almost all the theoretical studies predict M1 (with some exceptions, like [30]).

We thank the referee for bringing up this point and we have modified the manuscript to read as follows:

"Many theoretical studies^{20,28-34} predict the LEE to result from M1 transitions (with some exceptions³⁰), but these results have yet to be confirmed experimentally. In 2013 Larsen *et al.*³⁵ showed that the LEE is of dipole nature, but despite significant experimental efforts³⁶, no clear conclusion has been drawn on whether the LEE is of electric or magnetic character (E1 or M1)."

4) The authors point to [39] to check the distribution of the excitation energies of ^{70}Zn produced in the beta decay. Since this reference is not particularly easy to find (it is a PhD thesis), I will like to have a few words about how it looks like. Not much, just a mention about the "typical" range of excitation energies populated.

We thank the referee for pointing this out, and we have added another citation here to a manuscript which is currently under review at Physical Review C. We also point to Figure 4 of the methods section (copied below), where the ^{70}Zn total absorption spectra from $^{70}\text{Cu}^{m2}$ and $^{70}\text{Cu}^{gs}$ are presented as insets. In this figure we see the β decay feeding from $^{70}\text{Cu}^{m2}$ and $^{70}\text{Cu}^{gs}$. Here, we see more counts at low excitation energies in the ^{70}Zn spectrum populated from the β decay of $^{70}\text{Cu}^{m2}$ and more counts at high excitation energies in the ^{70}Zn spectrum populated from the β decay of $^{70}\text{Cu}^{gs}$. In both cases we see counts in these spectra until ~ 6.5 MeV, close to the β -decay Q-value (where we are limited by statistics).



We have modified this section of the text to include more details; it now reads as follows:

"The relative population of different excitation energies within the Q_β window following the decay of either parent state can be obtained from total absorption spectroscopy, which was measured in the present data set (see the Methods section, where the total absorption spectra are inset into Figure 4 or previous publications^{42,43}). In general, the β decay from $^{70}\text{Cu}^{m2}$ favors decays to lower-energy positive parity states in ^{70}Zn , whereas the β decay from $^{70}\text{Cu}^{gs}$ favors decays to higher energy negative parity states in ^{70}Zn , though experimentally observed feeding is present up until around Q_β in both cases. Following β decay, the excited states decay through the emission of photons until the ground state is reached."

5) Authors: " We use Gogny+Hartree-Fock-Bogoliubov (HFB) calculations of the NLD from the Hauser-Feshbach code TALYS..." I guess they mean something like "We use Gogny+Hartree-Fock-Bogoliubov (HFB) calculations of the NLD used by the Hauser-Feshbach code TALYS...", since TALYS itself doesn't calculate NLD's.

We thank the referee for pointing out that our language needed to be more precise. This statement has been modified to read as follows:

"We use Gogny+Hartree-Fock-Bogoliubov (HFB) calculations⁴⁷ of the NLD contained in the Hauser-Feshbach code TALYS⁴⁸ to describe the ratio of the negative and positive parity states in ^{70}Zn , shown in Figure 2c."

6) In Fig. 2 (c) the authors use theoretical calculations for the NLD. It would be interesting to compare them to the known discrete levels of well assigned spins and parities. In particular, with the current situation Fig 2(c) doesn't seem to be exactly compatible with the states in 2 (a,b).

From the evaluated nuclear data sheet in Ref. 41, there are a total of 50 total known states in ^{70}Zn with firm spin and parity assignments from roughly 0-4.5 MeV. 26 of these states fall within the assumed spin-range following the β decay of $^{70}\text{Cu}^{\text{m}2}$ ($J=0-3$) and 21 of these states fall within the assumed spin-range following the β decay of $^{70}\text{Cu}^{\text{gs}}$ ($J=4-8$). Three of these states have the assigned spin-parity of $3^+, 4^+, 5^+$ and therefore they are excluded from the discussion below, as we cannot assign them to either the ground state or isomeric decay.

There are no observed negative parity states in the ^{70}Zn until the 3^- state at 2.859 MeV, which we illustrate in Fig 2a, b. Below 3 MeV, there are 16 states with assigned spin-parity, 12 within the spin range $J=0-3$ and 4 within the spin range $J=4-8$. While it is true that at energies below 3 MeV the Gogny+HFB model predicts there to be negative parity states where there are none observed, the general trend presented in the figure remains true: there are more positive parity states in the spin-range populated by $^{70}\text{Cu}^{\text{m}2}$ than there are by $^{70}\text{Cu}^{\text{gs}}$.

Above 3 MeV, there are a total of 31 states in ^{70}Zn with assigned spin-parity, 15 to the spin range $J=0-3$ and 16 to the spin range $J=4-8$. In the spin range $J=0-3$, 11 of the states have positive parity and 4 of the states have negative parity. In the spin range $J=4-8$, 6 of the states have positive parity and 10 have negative parity. The resulting ratios of negative parity states over positive parity states are as follows: 0.36 for $J=0-3$ (populated by the β decay of $^{70}\text{Cu}^{\text{m}2}$) and 1.67 for $J=4-8$ (populated by the β decay of $^{70}\text{Cu}^{\text{gs}}$). We also find this to be consistent with what is presented in Figure 2c, where, at high excitation energies in ^{70}Zn there is nearly a 1:1 ratio of negative to positive parity states compared in the spin range $J=4-8$ compared to a much lower ratio of negative to positive parity states in the spin range $J=0-3$. The presence of only 31 total states with firmly assigned spin-parity over a range of 3-4.5 MeV makes it difficult to graphically display a comparison in the same way as can be done with the Gogny+HFB calculations in Fig 2c, but we think that this is an important component of the discussion and should be addressed in the manuscript. We reproduce the discussion of Figure 2 from the manuscript below, with an addition regarding experimental data in bold.

"The discrepancy in the parity of states is illustrated by the low-energy level scheme of ^{70}Zn , reproduced in Figures 2a and 2b⁴¹. Below approximately 3 MeV there are only two negative parity states. We use Gogny+Hartree-Fock-Bogoliubov (HFB) calculations⁴⁵ of the NLD contained in the Hauser-Feshbach code TALYS⁴⁶ to describe the ratio of the negative and positive parity states in ^{70}Zn , shown in Figure 2c. The ratio is zero at low energy, matching the expectation based on discrete levels where there are few negative parity states in the nucleus, and approaches 1 toward higher excitation energies where there should be an equal number of positive and negative parity states. The two lines in Figure 2c are to distinguish the parity asymmetry for the two different spin ranges $J=0-3$ (orange, $^{70}\text{Cu}^{\text{m}2}$) and $J=4-8$ (blue, $^{70}\text{Cu}^{\text{gs}}$). Shell model calculations using the jun45 ⁴⁷ and jj44b ⁴⁸ Hamiltonians produce qualitatively similar ratios for the parity asymmetry as a function of excitation energy. **Experimental levels with firmly assigned spin-parity exhibit similar behavior, where above 3 MeV the ratio of negative parity states to positive parity states is higher for higher spins $J=4-8$ than it is for spins $J=0-3$.**"

7) Authors: "To qualitatively demonstrate the suppression of the E1 strength function we first model the experimental γ SF extracted following the β decay of the 6^- ^{70}Cu gs state (blue band in Figure 1)". I'd like a word about how the decay is "modeled". Maybe with TALYS? Where does the width of the corresponding band come from?

We thank the referee for this question, as we believe the original text denoting the 6^- ^{70}Cu ground state data as the "blue band in Figure 1" to be inaccurate. We change the text here to denote these data as the "blue squares in Figure 1" (see below):

"To qualitatively demonstrate the suppression of the E1 strength function we first model the experimental γ SF extracted following the β decay of the $6^{-} {}^{70}\text{Cu}^{\text{gs}}$ state (blue squares in Figure 1)."

The blue band in Figure 1 represents an extrapolation of the experimental data (blue squares) to the tail of the GDR using a generalized Lorentzian. What is presented in Figure 1 is not "modeling" but an extrapolation. The width of this band is meant to represent an extrapolation that contains the full error bars of the experimental strength function.

However, when we present the data in Figure 3, we do model the γ SF using strength functions available in TALYS. We take the shape of the E1 strength to be from the Gogny+HFB with QRPA model γ SF in TALYS and we model the M1 strength as a generalized Lorentzian. The LEE is modeled as an exponential decay function. The details are recorded in the text as follows:

"Here, the shape of the E1 strength was based on the Gogny+HFB with QRPA model (using the D1M version of the Gogny force) by Goriely *et al.*⁵¹ and the shape of the M1 strength is modeled by a generalized Lorentzian⁵²."

8) Authors: "An additional exponential component was added to account for the M1 LEE." Although this is rather common practice, it would be good to mention whether there is any theoretical support for this energy dependence, as opposed to being a purely phenomenological fit. In addition, it seems to me that the fit of the energy dependence of the M1 strength is one of the most important elements on which the robustness of the conclusions is based, since it allows for the extrapolation to smaller energies, where the gs and isomer decays are no longer different. I feel that the authors should comment on this, and assess (even if only qualitatively) the uncertainty introduced by this procedure. This is my main concern: that the mechanism at play for lower energies (up to about 2 MeV) might be different, for example, decays between excited states from an excitation energy region where positive and negative parity states are better balanced. In this situation, E1 transitions might recover and be present at these lower energies, unless I'm missing something.

We thank the referee for bringing up several good points and we address them separately below.

First, we agree that theoretical support should be added to explain why we model the LEE with an exponential. There are multiple theoretical calculations that have attempted to predict the low-energy enhancement in nuclear systems (see Refs. 20, 28-34 from the manuscript). In them the LEE is expected to have an energy dependence that can be modeled as an exponentially increasing function. We have modified the manuscript to include a statement referencing this behavior:

"An additional exponential component was added to account for the M1 LEE, as the LEE is expected to have an energy dependence that can be modeled by an exponential function^{20,28,29,31-34}."

Now we address the referee's questions and concerns about the energy dependence of the M1 strength. In the current analysis we take the theoretical strengths provided by the Gogny+HFB with QRPA model (using the D1M version of the Gogny force, see Ref. 51) with their expected M1 LEE without any modification to their respective shapes. The only parameters that are allowed to vary in the fit are the absolute magnitude of the M1 LEE and the E1 components (the high-energy M1 component is fixed relative to the high-energy E1 component based on previous results in this mass region as mentioned in Ref. 10). It should also be noted that the relative magnitude between the high-energy E1 and M1 components could also be altered but our data is not particularly sensitive to this variable, so we choose to keep this restricted to suggested literature values.

The E1, M1 and M1 LEE that were used in Fig. 3 are shown below (Figure A) in comparison to the blue band presented in Fig. 1. The sum of the three individual components used in the analysis presented

in Fig. 3 are within the uncertainties presented in Fig. 1 over the energy range of interest provided the experimental data.

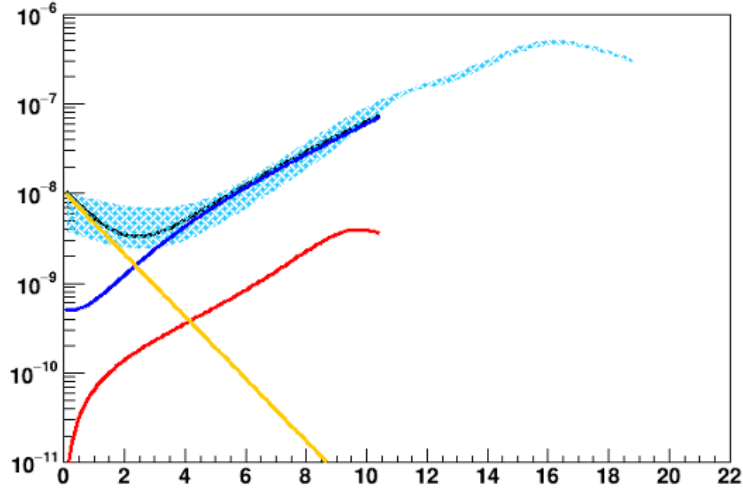


Figure A: Comparison of E1(blue), M1(red), M1 LEE (yellow), and sum (black) compared to the blue band used in Fig. 1 of the original manuscript. The sum of the three components used in the analysis is within the uncertainties of the blue band of Fig. 1.

If the analysis is repeated and the M1 strength is allowed to vary in slope, a slightly different M1 LEE is obtained for the γ SF extracted following the decay of the 6^- state. This leads to a slightly different version of Fig. 3. When we apply the subsequent analysis steps, the result is the figures shown below (Figures B and C). The referee will find that in Figure B the modified M1 LEE still matches the experimental data obtained from the decay of the 1^+ state within the experimental errors and this change to the fitting procedure does not impact the overall conclusions of the paper.

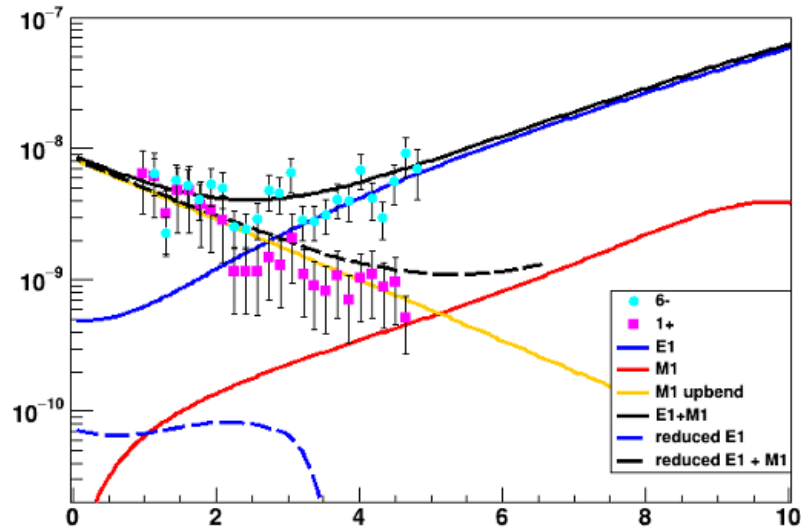


Figure B: An equivalent analysis presented as presented in Fig 3. in the manuscript when the shape of M1 LEE is allowed to vary. The reduced γ SF changes slightly but is still within the uncertainty of the experimental γ SF extracted from the 1^+ state.

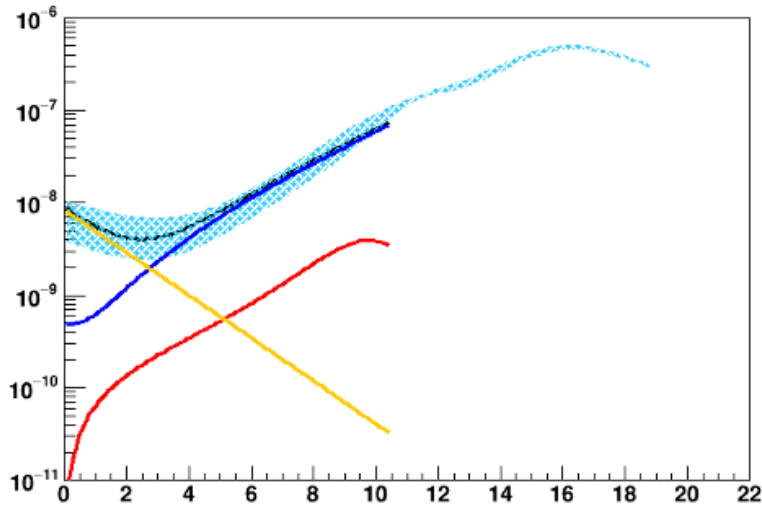


Figure C: Same as Fig A above but the shape of the M1 LEE is allowed to vary. There is no significant change.

Lastly, the referee is again correct that at high excitation energies, the spin and parties could become better balanced. However, based on the predicted parity ratios from Ref. 47 even at the Q_β ($Q_\beta = 6.59$ MeV) the ratio between negative and positive parity states is still 0.64. It appears that a region where there is a more equal parity distribution is not available in the 1^+ decay. Even if such a region were accessible, the theoretical expectations for the E1 strength at low energy is vanishingly small (blue curve in Fig 3 in the original manuscript). To assess the possibility that the LEE is E1 in nature instead of M1 we performed the analysis presented in Fig 3 with an assumed E1 LEE. The resulting analysis is presented in the Figure D below. It is not possible to get the $1^+ \gamma$ SF to match with the expected shape of an assumed E1 LEE in line with the conclusions of the manuscript.

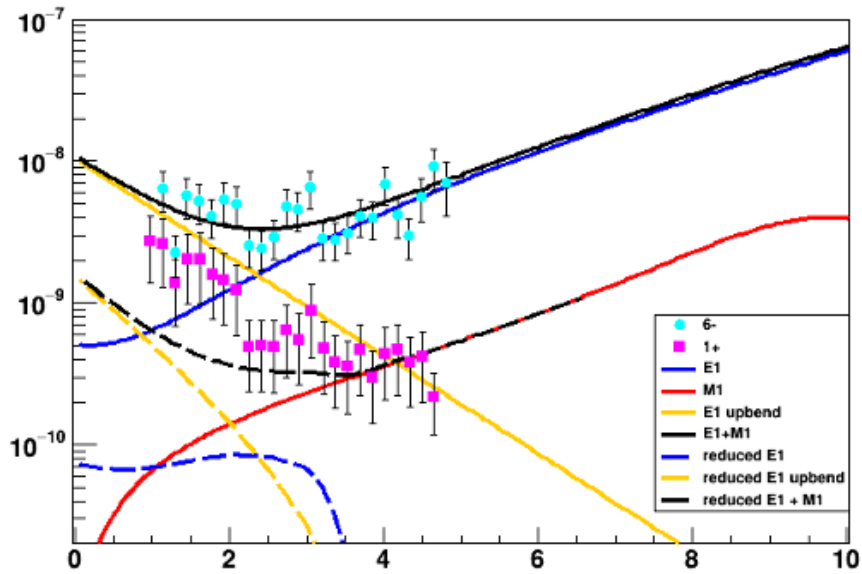


Figure D: Analysis from Fig 3 in the manuscript assuming an E1 LEE. It is not possible to match the dashed black line to the shape of the strength function with the experimental data extracted from the decay of the 1^+ state.

To clarify this, we have added the following text to the manuscript near line 185:

"The dashed black line matches the shape of the experimentally measured γ SF from the 1^+ β decays of $^{70}\text{Cu}^{m2}$; **it should be noted that this behavior holds if the M1 LEE exponential is allowed to vary in slope within the error of the experimental data at low energies.** Further, the difference in shape between the ^{70}Zn γ SF extracted from the $^{70}\text{Cu}^{m2}$ β decays and $^{70}\text{Cu}^{gs}$ β decays is attributable to the suppression of the E1 strength and the existence of the M1 exponential LEE."

9) Authors: "The amplitude of the E1 and M1 exponential LEE components were allowed to freely vary." I'm a bit puzzled about the variation of the E1. The authors claim that "the shape of the E1 strength was based on the Gogny+HFB with QRPA model (using the D1M version of the Gogny force) by Goriely et al." Can they vary this, and still keep the good reproduction of the GDR shown in Fig. 1?

We thank the referee for pointing out the unclear language in describing this part of the analysis. The referee is correct that the magnitude of the E1 strength is fixed by a good extrapolation between the 6^- data and the GDR, shown in the blue band of Fig. 1. The experimental data points for the 6^- and 1^+ γ SF shown in Figs 1 and 3 are identical. In the analysis presented in Fig. 3, the model for the E1 was taken from the Gogny+HFB with QRPA model and the sum of the three components used in the analysis for Fig. 3 are within the uncertainties band in Fig 1 as shown in Fig A of the response to question 8.

To clarify this, we have added the following text to the manuscript:

"The amplitude of the E1 and M1 exponential LEE components were allowed to freely vary and the non-exponential M1 component was fixed at approximately an order of magnitude weaker than the E1 strength. This order of magnitude difference between E1 and M1 strengths is expected based on other nuclei within this mass region¹⁰. The total γ SF obtained by summing the contributions from E1, M1, and M1 exponential LEE is presented as the solid black line in Figure 3. **This line falls within the blue band in Figure 1.**"

10) Authors: "To quantify the impact of the parity asymmetry on the extracted γ SF we determined an E1 reduction factor for each γ -ray energy". I admit that the nature and rationale for this procedure are a bit obscure to me. How is this reduction factor extracted, exactly? I think the current version is too vague on this respect. The primary gammas in the energy region of interest populate only positive parity states, with the exception of a 3038 keV 5^- and a 1859 keV 3^- . If the primary gammas are the ones to be modeled, it seems that one would only need to compute the discrete transitions to these states as a function of the continuous initial excitation energy... On the other hand, is it really needed in order to establish the M1 nature of gammas in the energy range of interest? The contribution of the reduced E1 to the total strength seems to be quite negligible according to Fig. 3.

The referee is correct that the high-energy primary E1 transitions from a highly excited state to any of the discrete states below ~ 3 MeV (apart from the 3038 keV $J^\pi=5^-$ or the 1859 keV $J^\pi=3^-$) would not be allowed and this is indeed the reason for the dramatic suppression of the E1 strength. It is the absence of the E1 transitions that allows us to conclude the M1 nature of the LEE.

The reduction factor is calculated by taking the parity information into account. As an example, assume a 4 MeV primary γ -ray transition. This 4 MeV primary γ -ray transition could be emitted from states with excitation energies from 4 MeV and above. For each excitation energy that could emit a 4 MeV γ ray a reduction factor is computed. In the present experiment, the emission of a 4 MeV γ ray is

not possible from any excitation energy since the states that the transition would go to are all positive parity. Therefore, the multiplicative E1 reduction factor for a 4 MeV γ ray is 0 and this γ ray does not appear in the E1 reduced strength function shown by the dashed blue line in Fig. 3.

However, if we consider instead a 100 keV primary γ ray, such a low energy primary γ ray could be emitted from many excitation energies. Again, assuming an excitation energy of 4 MeV, the reduction factor would depend on the availability of states at $4 \text{ MeV} - 0.1 \text{ MeV} = 3.9 \text{ MeV}$. The ratio of negative to positive parity states at 3.9 MeV according to LD5 is 0.21 so we take this to mean that E1 transitions would be a factor of five less intense compared to potential M1 transitions because fewer states are available. If we look at a higher excitation energy of 6 MeV, the ratio of positive to negative parity states at 5.9 MeV is approximately 0.55. The relative importance of the reduction factor at each excitation energy is taken from the relative population of each excitation energy extracted from the total absorption spectrum for the 1^+ state (see Fig. 4 in the methods section). Of course, in the actual analysis these are energy bins both for the excitation energies and the emitted γ rays instead of exact energies since our knowledge of the level scheme in ^{70}Zn is incomplete. In this manner we calculate a reduction factor for each γ -ray energy in the γSF and apply it to the E1 strength resulting in the dashed blue line shown in Fig. 3.

To clarify the purpose of the E1 reduction factor we have modified the following text in the manuscript: "To account for and quantify the impact of the parity asymmetry on the extracted γSF we determined an E1 reduction factor for each γ -ray energy. For a given primary γ -ray energy (E_γ) we loop over all excitation energies (E_x) where it is energetically possible to emit a primary E_γ . Then we determine the reduction factor based on the parity asymmetry at the energy of $E_x - E_\gamma$ to account for the lack of negative parity levels at the destination excitation energy. **Above $E_\gamma = \sim 3.5 \text{ MeV}$ the multiplicative E1 reduction factor is 0, as there are no available negative parity states in the nucleus to accommodate the emission of a primary E1 γ -ray at these energies.** The reduction factor at each E_x is weighted by the population of that specific E_x in β decay, taken from total absorption spectroscopy results⁴¹. The final reduction factor for each E_γ was applied to the E1 strength function, leading to the dashed blue line in Figure 3."

11) Fig 3 summarizes the main results of the paper, associated with the gs and isomeric decays. I wonder if anything is known experimentally in that energy region associated with the decay from a compound nucleus produced in some different way, where the initial parity is not constrained.

There are data in this energy region produced from a $^{70}\text{Zn}(p,p')$ reaction. These data were analyzed with the Oslo method as part of Erlend Lima's M.S. thesis from the University of Oslo [1]. As these data are presented in a thesis and have not been finalized, we do not discuss them in the current manuscript. However, when we compare the data presented in the M.S. thesis to the data we present here, we find very good agreement between the ^{70}Zn γSF from the β decay of $^{70}\text{Cu}^{\text{gs}}$ and the ^{70}Zn γSF from the (p,p') measurement. These results will be presented in a subsequent publication.

[1]. E. Lima. Measurements and Calculations of the Nuclear Level Density and γ Strength Function of ^{70}Zn . *PhD. Thesis*, University of Oslo (2020).

12) In the Methods section, the authors write "We assume, as we do with the standard Oslo method, that the primary γ -decays into L_j are dipole transitions. For the matrix populated from the β decay of $^{70}\text{Cu}^{\text{gs}}$ (Figure 5 (a)), we restrict our analysis to two discrete states: $EL1 = 885 \text{ keV}$ and $EL2 = 1759 \text{ keV}$, where both states have identical spin $L_{1,2} = 2^+$ ". Since the parity of the compound nucleus is positive in this case, the transition could also be E2. If it is reasonable to rule this out, the authors might want to state it explicitly.

We thank the referee for this comment, and we agree that this assumption should be addressed. The following text has been added to the methods section:

"We assume, as we do with the standard Oslo method, that the primary γ -decays into L_j are dipole transitions, as dipole transitions are known to dominate decay in the quasi-continuum⁶⁴."

We add here Ref 64: J. Kopecky and M. Uhl, Phys. Rev. C 41, 1941 (1990), where a discussion of dipole, quadrupole, and octapole contributions to the γ SF is presented. The authors determine that the quadrupole and octapole contributions to the γ SF are very small compared to the dipole contributions and they are negligible in the energy region we are experimentally accessing here.

Summing up, this is a good, novel, and relevant paper, regardless of the solidity of the main conclusion, which is the assignment of the LEE to M1 transitions. Concerning this essential point, I would like to have addressed my few points above, most importantly point 8) regarding the extrapolability of the M1 assignment below ~ 2 MeV.

We thank the referee again for their comments, questions, and constructive feedback. We hope that we have provided sufficient responses (particularly with regard to point 8) to satisfy the referee's concerns.

Referee #3 (Remarks to the Author):

I have been asked to provide a review for the manuscript "On the Nature of Low-Energy Electromagnetic Radiation in Atomic Nuclei" submitted by E.K. Ronning et al. to Nature. While I commend the authors on performing this challenging and important measurement for the field of nuclear physics, that allowed them to access the gamma-ray strength functions (gSF) resulting from the decay of different classes of states (with different spin and parity), I do not think that based on their observation they can claim conclusively that the low-energy upbend is of magnetic character.

We thank the referee for taking the time to review our manuscript and their rigorous attention to detail. We provide a thorough response below to all of the referee's concerns and questions. We hope that our answers provide sufficient proof to convince the referee of the robustness of the data, analysis, and interpretation we present in this manuscript.

Either additional experimental data, like polarization measurements (which are not available from this experiment), or supporting theoretical (microscopic) calculations of the two gSF resulting from the ^{70}Cu g.s. and ^{70}Cu isomer decays need to be provided to allow the authors to make such a strong claim.

We agree with the referee that additional measurements and calculations are always beneficial to enhance an argument. While we cannot provide polarization measurement data from this experiment, we do present the comparison of a microscopic γ SF model (Gogny+HFB with QRPA model (using the D1M version of the Gogny force from Ref. 51) in Figure 3 of the manuscript. Additionally, several theoretical studies have been done in attempt to provide an interpretation of the upbend and most of them predict it should be of M1 nature. Additional theoretical calculations were beyond the scope of this work since we were able to provide an interpretation based on experimental observations. We have added the following text in the manuscript to highlight these theoretical models "Many theoretical studies^{20,28-34} predict the LEE to result from M1 transitions (with some exceptions³⁰), but these results have yet to be confirmed experimentally."

The experiment is also blind in a significant gamma-ray energy range, i.e., over a range of about 5 MeV (mainly because of the Q value of the decays). A significant deviation from the (g,n) data is observed

just around 10 MeV, where especially the gSF “determined” for the ^{70}Cu g.s. decay deviates from the data. This could influence the shape of the gSF at lower energies as the authors do not know whether additional components would need to be added.

The referee is correct in pointing out that the data obtained from this experiment are in a more limited energy range than other Oslo-type experiments. With the statistics of the experiment and Q_β of ^{70}Cu , we can measure the γ SF from approximately 1-5 MeV; these data are displayed in Figure 1 of the manuscript as the blue square and orange triangular points. While it does not extend to high energies, the region we measure (1-5 MeV) does span the range of the LEE, which occurs at energies below 3 MeV.

We would also like to address the referee’s comment about deviations in the “determined” γ SF from the (g,n) data at around 10 MeV. We would like to make it clear that we do not claim that the γ SF at such high energies is “determined” by our experimental data. The blue and orange bands included in Figure 1 of the submitted manuscript are extrapolations of the experimental data to high energies as a possible way to connect our data to the existing high-energy data. We have made a modification to Figure 1 to address this concern, where we have removed the extrapolation of the data from $^{70}\text{Cu}^{m2}$. This γ SF has a suppressed E1 γ SF and is not a “true” γ SF (containing the full E1 and M1 strengths), thus it is unphysical to extrapolate it to the GDR.

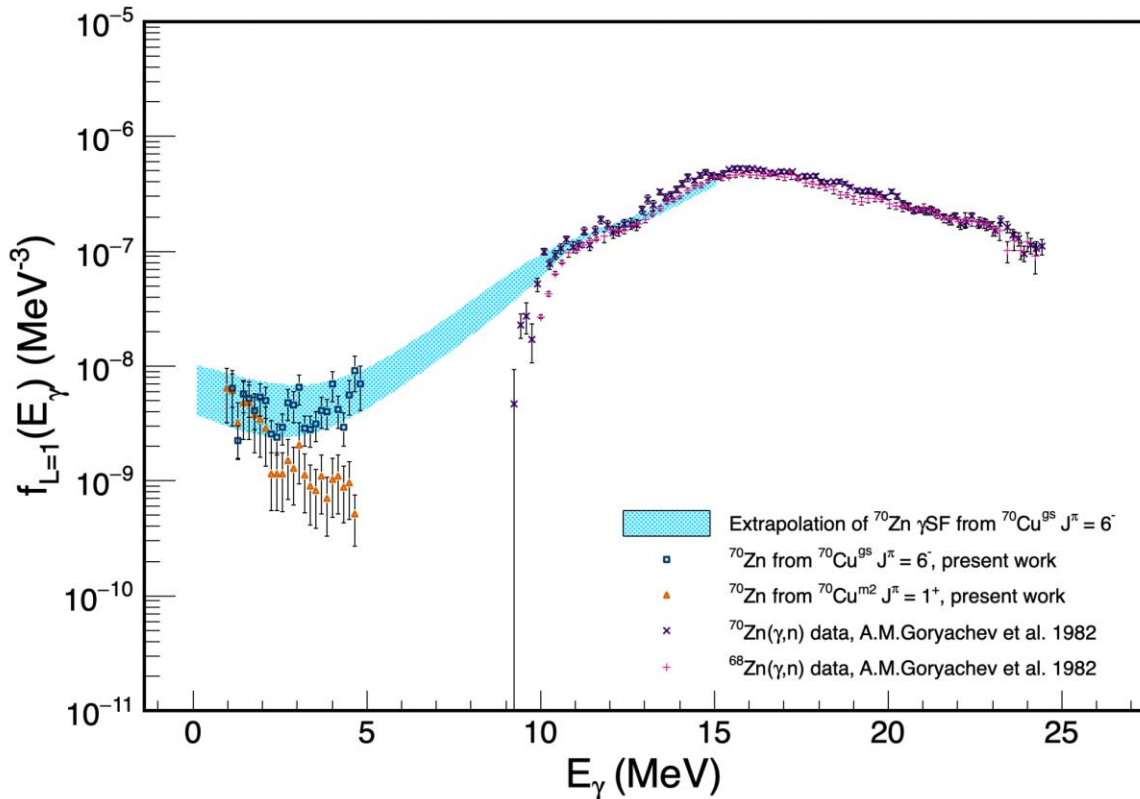


Figure 1: The γ -ray strength functions of ^{70}Zn extracted from the β decays of $^{70}\text{Cu}^{m2}$ (orange triangles) and $^{70}\text{Cu}^{gs}$ (blue squares). The data from the β decay of $^{70}\text{Cu}^{gs}$ are normalized in magnitude and extrapolated to GDR data from $^{70}\text{Zn}(\gamma, n)$, ^{69}Zn and $^{68}\text{Zn}(\gamma, n)$, ^{67}Zn measurements by Goryachev *et al.*⁴⁴,

shown in the blue band. The absolute normalization of the data following the β decay of $^{70}\text{Cu}^{\text{m}2}$ is relative to the blue band.

Here, we use the IVGDR to scale the experimental γ SF in magnitude. We will elaborate on this procedure in our response to point 7 below. The IVGDR in no way impacts the γ -ray energy dependence of the γ SF. The slope of the experimental γ SF presented in the manuscript is fixed by the Shape Method, not the IVGDR, nuclear level density, or any other external normalization parameter. Therefore, these extrapolations do not affect any of the conclusions in the present manuscript related to the nature of the LEE.

We have also added the following text in the methods section to clarify this normalization:

"The ^{70}Zn γ SF extracted following the β decay of $^{70}\text{Cu}^{\text{gs}}$ was normalized in absolute magnitude to GDR data from $^{70}\text{Zn}(\gamma,n)^{69}\text{Zn}$ and $^{68}\text{Zn}(\gamma,n)^{67}\text{Zn}$ measurements by Goryachev et al.⁴⁴ and the ^{70}Zn γ SF extracted from the β decay of $^{70}\text{Cu}^{\text{m}2}$ was normalized relative to the data from the β decay of $^{70}\text{Cu}^{\text{gs}}$, to maintain consistency with the interpretation that this γ SF has a suppressed E1 component."

Furthermore, I would like the authors to comment to what extent their findings violate the Brink-Axel hypothesis, as the two gSF, shown in Fig. 1 of their manuscript, are clearly different. This discussion is missing entirely. Also, most available data point to the LEE only being part of the gSF in the decay channel, which would violate the Brink-Axel hypothesis per se. Because of that, its influence on nucleosynthesis processes, as mentioned in the introduction of the manuscript, might be entirely different than shown in some work where it was assumed that the LEE is also part of the g.s. gSF.

The referee is correct that the two strength functions presented in Figures 1 and 3 of this manuscript are different, however this is because we are probing different components (M1 vs E1+M1) of the γ SF, and not because we are showing a violation of the generalized Brink-Axel hypothesis (gBA). In our current interpretation our results, the E1 contributions of the γ SF from $^{70}\text{Cu}^{\text{m}2}$ are suppressed due to a lack of available states and not due to a modification of the underlying γ SF. When E1 transitions are available (as they are for the γ SF extracted from the decay of $^{70}\text{Cu}^{\text{gs}}$) the γ SF has both E1 and M1 components (as shown in Fig 3). A similar phenomenon could be observed if one designed an experiment to isolate the E1 and M1 components of the γ SF in a nucleus which contains a scissors resonance. As it has been shown that the scissors resonance results from M1 transitions (see Refs. 11-14), the isolated E1 component of this γ SF in this case would not include the scissors mode, but the isolated M1 γ SF would. These two γ SFs would not be a violation of the gBA, just different components of the same strength function. As a result, we currently do not consider our result to be a violation of the gBA hypothesis.

Further, a M.S. thesis by Erlend Lima at the University of Oslo presents the ^{70}Zn γ SF extracted from the Oslo method from a $^{70}\text{Zn}(p,p')$ reaction [1]. These data are presented in a thesis and have not been finalized for publication, so we do not present them here. However, when we compare these data from the M.S. thesis with our data, we find very good agreement between the ^{70}Zn γ SF from the β decay of $^{70}\text{Cu}^{\text{gs}}$ (which includes the full E1 and M1 strength) and the ^{70}Zn γ SF from the (p,p') measurement (which should also include the full E1 and M1 strength). It should be noted that the spin selection of states in ^{70}Zn is different between the two population mechanisms – β decay from $^{70}\text{Cu}^{\text{gs}}$ and the (p,p') reaction. The agreement with the Oslo γ SF and the γ SF from $^{70}\text{Cu}^{\text{gs}}$ indicates in principle that the Brink-Axel is not violated and affirms our interpretation that the E1 contributions in the γ SF from $^{70}\text{Cu}^{\text{m}2}$ are suppressed.

The authors agree that the matter of the gBA in this case is something that merits further investigation and discussion in the community.

We have added the following text to the manuscript to address Brink-Axel:

“There are few available negative parity states at low excitation energy in ^{70}Zn and as a result, the E1 γ SF inferred following the decay of the $1^+ ^{70}\text{Cu}^{m2}$ is significantly suppressed. In this interpretation of selective suppression of the E1 γ SF, the difference in the two γ SF from $^{70}\text{Cu}^{m2}$ and $^{70}\text{Cu}^{gs}$ β decay does not appear to violate the generalized Brink-Axel hypothesis,^{45,46} but instead reflects a different availability of states.”

At the end of the manuscript we have also added:

“With the nature of the LEE finally fully established for the case of ^{70}Zn , we can focus our efforts into addressing remaining questions regarding the γ SF, such as how the amplitude and shape of the LEE might depend on nuclear structure, how we can predict this behavior⁵³, and how it might be related to the generalized Brink-Axel hypothesis.”

Regarding the LEE in astrophysical calculations, we direct the referee to our response to their comment #3.

Referenced Masters Thesis:

[1]. E. Lima. Measurements and Calculations of the Nuclear Level Density and γ Strength Function of ^{70}Zn . *PhD. Thesis*, University of Oslo (2020).

The main concern though is that there are significant differences between the results presented in this manuscript and those presented in the cited PhD thesis (available online) that led to the results of this manuscript.

While many details of the analysis presented in this publication were also presented in the thesis of E. K. Ronning, the authors would like to be clear that this analysis was not completed at the time this PhD thesis was submitted or accepted. While we appreciate that the referee took the time to review the referenced materials, we want to stress that the analysis of these data and our interpretation of the M1 LEE were finalized after the submission of this thesis. While we understand the referee's concerns, we hope we can provide a satisfactory explanation to their questions below.

Some additional comments before my closing remarks:

1) Lines 75ff: The authors fail to mention the important work of Richter et al. on the scissors mode. At least, the review article by Heyde, von Neumann-Cosel, and Richter should be cited, *Rev. Mod. Phys.* **82**, 2365 (2010).

We thank the referee for pointing out the omission of this reference. The following citation has been added:

“Heyde, K., von Neumann-Cosel, P., and Richter, A. *Rev. Mod. Phys.* **82** 2365 (2010).”

2) Line 80: Probably this review article should be cited for the PDR as well: D. Savran, T. Aumann, A. Zilges, *Prog. Part. Nucl. Phys.* **70**, 210 (2013), as at least from an experimental perspective it was the first extensive review. There are several others, which were published in recent years, that are commonly cited.

We thank the referee for also pointing out the omission of this reference. The following citation has been added:

"Savran, D., Aumann, T., and Zilges, A. *Prog. in Part. and Nucl. Phys.* **70** 210 (2013)."

3) Lines 85ff: "The presence of the LEE has significant impact on predictions of nuclear reaction rates; ..." Probably a more nuanced discussion should be adopted, mentioning how the LEE would impact nucleosynthesis processes if it was only part of the decay gamma-ray strength function. See comment above.

Below please find our response to this point, and also the referee's comment: "Also, most available data point to the LEE only being part of the gSF in the decay channel, which would violate the Brink-Axel hypothesis per se. Because of that, its influence on nucleosynthesis processes, as mentioned in the introduction of the manuscript, might be entirely different than shown in some work where it was assumed that the LEE is also part of the g.s. gSF."

We state in the submitted manuscript: "The presence of the LEE has a significant impact on predictions of nuclear reaction rates; it increases radiative neutron-capture reaction cross sections compared to theoretical expectations, and a better predictive power regarding which nuclei contain the LEE is imperative to modeling astrophysical nucleosynthesis^{26,27}."

The reviewer is correct that most available data point to the LEE being part of the γ SF decay channel, however when modeling the radiative neutron capture cross sections and nuclear reaction rates that we mention in the manuscript, this fact does not change the impact of the LEE on astrophysical nucleosynthesis. For capture cross section and reaction rate calculations, the γ SF is used to describe the γ -emission channel after the formation of the compound nucleus. Therefore, the γ SF decay channel is the one that is used in these calculations, and the presence of the LEE does impact the resulting cross section and reaction rates. We refer the referee to Refs. 26 and 27 of the manuscript for examples of how the LEE increases radiative neutron capture cross sections compared to theoretical expectations. It is well documented that neutron capture cross sections are an essential component to modeling astrophysical nucleosynthesis, and having the predictive capability of where one should expect the LEE to appear would increase our ability to model these processes.

Regarding discussions about Brink-Axel violations in astrophysical nucleosynthesis, we believe this is beyond the scope of our result. While it is certainly important to understand, the presence (or not) of the LEE in the decay and excitation channels requires knowledge about the origin of the LEE which we do not have. While we cannot provide a clear interpretation at this time, we are making an important first step in determining the nature of the LEE in this manuscript, which will prompt future investigation into these issues.

4) Lines 95ff: As indicated above, I strongly feel that the claim that the authors "conclusively" showed that the LEE in ^{70}Zn is of M1 character is too strong.

We hope that our answers to the previous concerns and subsequent questions help provide clarity on the robustness of our claim.

5) Line 105 (assumption of allowed beta decays): Can the contribution from forbidden beta decays be estimated? Does this assumption bias the interpretation of the data? Do measured $\log(ft)$ values support this assumption?

For the β Oslo and Shape method analysis, we extract data from the quasicontinuum region, where we do not expect there to be any significant contribution from forbidden β decays. In this region of the level scheme, the level density is high and we expect the fastest and most favorable (allowed) decays

to dominate, and thus contributions from forbidden decays are negligible. The measured $\log(ft)$ values from Ref. 41 above 3.3 MeV support this assumption, where a majority of the reported $\log(ft)$ values have $\log(ft) < 6$ and are indicative of allowed transitions. We have added the following sentence to the methods section to address our assumptions about forbidden decays:

“The ^{70}Zn level densities extracted from $^{70}\text{Cu}^{m2}$ and $^{70}\text{Cu}^{gs}$ β decay are partial level densities, assumed to result from spins associated with allowed Gamow-Teller decay and one dipole transition ($J = 0 - 3$ for $^{70}\text{Cu}^{m2}$ and $J = 4 - 8$ for $^{70}\text{Cu}^{gs}$). **We assume allowed Gamow-Teller β decay because forbidden decays are roughly 3 to 4 times slower than allowed decays and therefore much less probable to occur in the quasi-continuum region.** Each partial NLD is normalized to low-lying discrete levels.”

The authors chose to report data on the 1+ isomer decay and 6- g.s. decay, even though data for the 3- isomer decay appear to be available, too. They are part of the cited PhD thesis.

While it is correct that as part of this experiment the β decay from $^{70}\text{Cu}^{m1}$ was also measured, the resulting excitation energy vs γ -ray energy matrix (like those presented in Figure 5 of the manuscript) did not have enough statistics to reliably perform the β -Oslo or Shape methods. Therefore no attempt was made to extract the γ SF from these data and the analysis was not presented in the referenced PhD thesis, nor is it presented in this manuscript.

The cited PhD thesis also shows the beta feeding compared to shell-model predictions (Fig. 4.11 of the thesis). While for the 3- and 6- decays good (excellent) agreement is observed, the agreement is much worse for the 1+ isomer. The latter is, however, used in this manuscript to discuss the LEE. Troubling is that Fig. 5.20 of the thesis shows very different results than Fig. 4.11, as I understand after the SuN upgrade to SuN++, deteriorating the agreement with theory for the 1+ isomer further and also significantly changing the result for the 3- isomer. Can the authors comment on these deviations and how they would impact their results?

The first total absorption spectroscopy analysis referred to here by the referee was completed as part of a separate experiment conducted at the NSCL. During that experiment a mixed beam of all three spin parity states ($^{70}\text{Cu}^{gs}$, $^{70}\text{Cu}^{m1}$, and $^{70}\text{Cu}^{m2}$) was delivered to the SuN total absorption spectrometer and the ^{70}Zn spectra corresponding to the decays from each spin-parity state were isolated from a series of implantation/decay curves. We believe that the extracted feeding from all three spin-parity states shown in Fig. 4.11 of the thesis show good agreement with shell model calculations. Please find the figure reproduced from the PhD thesis below. The red dashed line and the blue dot-dashed line represent two shell model Hamiltonians, which show good agreement with the experimental data, particularly above 3.3 MeV, which is the energy region where the β -Oslo method and Shape methods are applied.

[Figure redacted]

The referee then points out discrepancies between the extracted β -feeding values from the figure above (Fig. 4.11 of the PhD thesis) and Fig 5.20 of the PhD thesis, where the β -feeding values from the experiment discussed in this manuscript are presented. This figure is reproduced below.

[Figure redacted]

While the referee is correct that the cumulative β -feeding from $^{70}\text{Cu}^{m2}$ is different at low energies, the discrepancy is with the direct β -decay feeding to the 885 keV state. There was also an additional level accounted for in this analysis that was left out of the previous analysis, which also contributed to the change in the feeding pattern. The difference in the calculated feeding to these two individual states is propagated through the rest of the cumulative feeding, causing the behavior in the figure above. A discussion of this behavior can be found in the referenced PhD thesis. Despite this discrepancy, we still observe very good agreement with the jj44b shell model Hamiltonian, whereas the results from the previous experiment seem to lie in the middle of the jun45 and jj44b Hamiltonians. It can also be seen in this figure that good agreement is observed for the cumulative feeding between the SuN and SuN++ data and both sets of shell model calculations above 3.3 MeV. We do not consider any states below 3.3 MeV in the β -Oslo or Shape method in this case, so these deviations do not impact our overall results.

Regarding the β decays from $^{70}\text{Cu}^{m1}$, we would like to direct the referee to Figure 5.19 of the aforementioned PhD thesis, where the agreement between the experimental data and fits to produce the above plots is contained. For the decays from $^{70}\text{Cu}^{m2}$ and $^{70}\text{Cu}^{gs}$ we see good agreement between the fits and the experimental data. For the decays from $^{70}\text{Cu}^{m1}$, the fits were not in agreement with the experimental data. One can observe that fits “underassign” feeding in the region where the largest discrepancies between the SuN and SuN++ data are observed. While the authors acknowledge that these fits should be improved, we do not think this bears any impact on the results we show in the present manuscript. The β -feeding values presented in the PhD thesis of E. K. Ronning are not related to the γ SF results shown here, particularly regarding the decay of $^{70}\text{Cu}^{m1}$, as no Oslo or Shape Method analysis was performed on these data.

6) Line 108 and GDR data: The authors refer to the data as (g,n) data. The (g,p) threshold opens ~ 11.1 MeV, the (g,2n) threshold around 15.7 MeV, thus, both should contribute to the shape of the gSF in the region of the IVGDR. Are these decay channels known or do they contribute to the uncertainty of the data? The authors report the gSF in ^{70}Zn , so $^{70}\text{Zn}(g,X)$ should be sufficient. Why is $^{68}\text{Zn}(g,n)$ also used?

We refer to the GDR data as “(g,n)” data because this is how it is categorized in the EXFOR database and the associated reference. The reference for these data (Ref. 44 from the manuscript) states that “multiplicity curves were used to separate the contributions of (g,n) and (g,2n) reactions”. We

interpreted this statement to mean that the data included in the EXFOR database were specifically from the (g,n) channel.

We also chose to include $^{68}\text{Zn}(n,g)$ to illustrate that the $^{70}\text{Zn}(n,g)$ data were not anomalous and that these data were a reasonable choice for the normalization of the magnitude of the γSF .

7) Figure 1:

i) The main concern (!) is that Fig. 1 of the manuscript is very different from Figure 7.1 in the cited PhD thesis. Obviously, the normalization was changed. Ambiguities with the normalization are also discussed in the manuscript. The same discussion (alpha scaling) is presented in the thesis, with a very different outcome though; that means Fig. 6 of the manuscript and Fig. 6.16 of the thesis, which compare the Oslo-method and shape-method results, are also very different and likely ambiguous. Obviously, the “data” was further tweaked to obtain agreement of the two gSF for energies lower than 2 MeV, which likely does significantly impact the interpretation of the data and the fit presented in Figure 3 of the manuscript; i.e., only after this tweak a suppression of the proposed E1 part will be observed and lead to the claim that the LEE part is the same for both gSF. In the thesis, one might claim that there is no LEE for the gSF observed from the ^{70}Cu ground state, or that it is strongly suppressed.

Normalization is a standard component of any Oslo-type analysis, and it must be treated with care. We hope the referee will find a very detailed response to their concerns below.

We would first like to address the referee’s mention of “ambiguities” with the normalization discussed in the manuscript. We state in the manuscript that: “Using the β -Oslo method produces a γSF with a broader E_γ range than the Shape method, but it requires external normalization points to determine the slope of the γSF , which leads to a large uncertainty in its shape. In cases where it is possible to apply both the Shape method and β -Oslo method to the same data set, the data from the β -Oslo method can be normalized with the model independent slope extracted via the Shape method, resulting in a larger E_γ range without the need of external normalization factors, save for a scaling parameter applied to the γSF .”

As we are fortunate enough here to have a situation where we can apply both the β -Oslo and Shape method to the same data set, the only normalization factor we present in the γSF analysis is a scaling parameter that adjusts the γSF in magnitude. The shapes of the γSF presented in Fig. 1 and Fig 3 are not ambiguous. We explain this below.

The resulting γSF and NLD from any Oslo analysis must be normalized using the following equations:

Equation 1 (NLD): $\rho(E_\gamma - E_0) = A(E_\gamma - E_0) \exp(-B(E_\gamma - E_0))$

and

Equation 2 (γSF): $\gamma(E_\gamma) = A(E_\gamma) \exp(-B(E_\gamma))$

Where $\gamma(E_\gamma)$ is the γ ray transmission coefficient related to the γSF by $\gamma(E_\gamma) = \frac{1}{2\pi} \frac{\gamma(E_\gamma)}{E_\gamma^2}$

Values A and B from equations 1 and 2 correspond to scaling factors. Parameter A will scale the NLD in magnitude (typically to known discrete levels) and parameter B scales the transmission coefficient (therefore also the γSF) in magnitude. Here, we scale the γSF in magnitude with reference to IVGDR data. While it is true that because of the long extrapolation, this scaling can have some ambiguity and impact the absolute γSF magnitude, however for the purposes of the LEE discussion presented here these concerns are not relevant, as discussed below in response to point 7iii.

The slope parameter (α) is contained in both the NLD and γ SF normalization equations. Here, we determine the slope parameter empirically by using the Shape method. We direct the referee to Refs. 37 and 38 from the manuscript for detailed methodology regarding how the slope parameter is determined with the Shape method. Details regarding the specific extraction of the slope parameter using these data are found within the Methods section of the manuscript.

With regard to the discrepancies between the data presented in the PhD thesis and this manuscript, the authors would like to make it clear that the only difference between Figure 1 from our paper and Figure 7.1 from the PhD thesis (and by extension Figure 6 from our paper and Figure 6.16 from the PhD thesis) is that the ^{70}Zn γ SF from $^{70}\text{Cu}^{\text{m}2}$ has been rescaled in magnitude. The ^{70}Zn γ SF from $^{70}\text{Cu}^{\text{gs}}$ has not been altered. Furthermore, the authors would like to make it clear that energy dependence (slope) of the two γ SFs was not altered. The referee mentions that the discussion of the determination of the slope from the Shape method yields different results in the PhD thesis and our manuscript, however this is not the case. The details regarding the extraction of the model-independent slope from the Shape method presented in the PhD thesis and in the methods section of this manuscript are the same, and the slope of the γ SF figures in the PhD thesis is the same as the slope presented in this manuscript.

At the time the PhD thesis was published, we were uncertain of how to scale the two γ SFs with reference to one another. When the thesis was written, our assumption was that the high energy portion of the γ SF (3-5 MeV) should be consistent between the two states. As a result, the two γ SFs were normalized so that they agreed at higher energies and had consistent extrapolation to the IVDGR. This is the normalization the referee finds presented in Figures 7.1 and 6.16 from the PhD thesis. At this time, we did not have an interpretation of what the difference between the two γ SFs meant. However, as our interpretation of the LEE developed, it became clear that the γ SF extracted from $^{70}\text{Cu}^{\text{m}2}$ was not the full γ SF, but a γ SF with suppressed E1 transitions. Therefore, this γ SF was renormalized in magnitude so there was agreement at low energies.

Now we would like to address the referee's point that this impacts the interpretation of the data presented in the manuscript, and that "one might claim that there is no LEE for the γ SF observed from the ^{70}Cu ground state, or that it is strongly suppressed". Despite the change in absolute magnitude scaling, there is still a significant difference in the low-energy portion of the γ SFs around the region of the LEE. This is seen in both Figures 7.1 and 6.16 from the PhD thesis referenced by the referee. From the relatively flat slope of the γ SF for the ground state, it is clear the LEE is present. Typically, if a γ SF does not have the LEE or it is strongly suppressed, it will trend towards zero at low energies. We direct the referee to Figure 1 of Ref. [1] and Figure 8a of Ref. [2] below for examples of such behavior. The strongly negative slope of the γ SF from the isomer indicates an incredibly strong LEE. As the parity asymmetry argument from the manuscript does not change, the normalization presented in the PhD thesis illustrates a situation where there is an enhancement in the M1 transitions observed, rather than a suppression of the E1 transitions (please see our response to question 7v for an elaboration on the suppression of E1 transitions). As a result, our interpretation that the LEE is the result of M1 transitions remains the same, regardless of the normalization of the γ SF in magnitude.

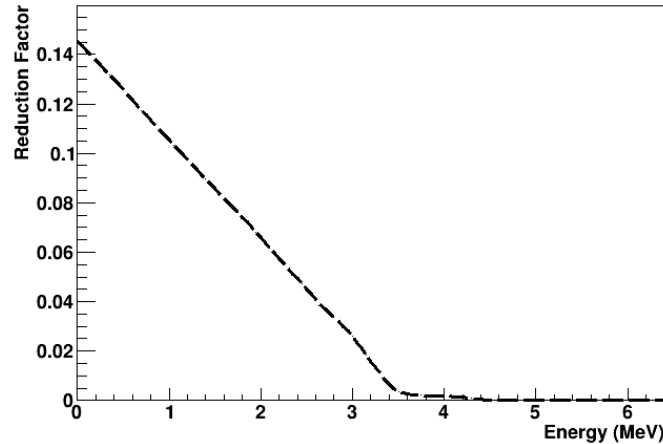
In summary, the energy-dependence (slopes) of the two γ SFs we present in this manuscript were determined empirically and in a model-independent way by using the Shape method. The shape of the γ SF is dictated by its slope, and the two γ SFs we present here have different slopes. Thus, the scaling of the γ SF in absolute magnitude does not change our interpretation of our results.

[1] Spyrou, A., et al. First study of the $^{139}\text{Ba}(n,\gamma)^{140}\text{Ba}$ reaction to constrain the conditions for the astrophysical i process, *Phys. Rev. Lett.* **132**, 202701 (2025).

[2] Markova, M., et al. Nuclear Level Densities and γ -ray Strength Functions of $^{111,112,113}\text{Sn}$ Isotopes studied with the Oslo Method, *Phys. Rev. C* **108**, 014315 (2023).

ii) The reduction factor applied to the E1 gSF should be shown as a function of E_g , possibly as an inset to Figure 1.

The referee will find the reduction factor applied to the E1 γ SF in the figure below:



Here we show the reduction factor applied to the E1 γ SF as a function of energy. This factor was determined from the population of a specific E_x from β decay, taking from total absorption spectroscopy results. The application of these reduction factors to the E1 γ SF results in the dashed blue line of Figure 3 of the manuscript.

We have added the following text to the manuscript in relation to the reduction factor: “To account for and quantify the impact of the parity asymmetry on the extracted γ SF we determined an E1 reduction factor for each γ -ray energy. For a given primary γ -ray energy (E_γ) we loop over all excitation energies (E_x) where it is energetically possible to emit a primary E_γ . Then we determine the reduction factor based on the parity asymmetry at the energy of $E_x - E_\gamma$ to account for the lack of negative parity levels at the destination excitation energy. **Above $E_\gamma = \sim 3.5$ MeV the multiplicative E1 reduction factor is 0, as there are no available negative parity states in the nucleus to accommodate the emission of a primary E1 γ -ray at these energies.** The reduction factor at each E_x is weighted by the population of that specific E_x in β decay, taken from total absorption spectroscopy results⁴¹. The final reduction factor for each E_γ was applied to the E1 strength function, leading to the dashed blue line in Figure 3.”

iii) Fits in Fig. 1: How was the width of the M1 component determined? How does the E1 fit compare to IVGDR data? Is anchoring of data at ~ 15 MeV a flaw because (g,p) might already contribute (see comment above)?

The extrapolations presented in Figure 1 utilize two generalized Lorentzians to fit the γ SF data (the blue square and orange triangles) to the IVGDR. An exponential decay function was added and used to fit the LEE. Two fits were performed, one to the lower points of the error bars on the experimental data and one to the upper points on the error bars of the experimental data. The region between these two fits encapsulates the blue (for the ^{70}Zn γ SF from $^{70}\text{Cu}^{\text{gs}}$) and orange (for the ^{70}Zn γ SF from $^{70}\text{Cu}^{\text{m2}}$) bands shown in Figure 1.

As mentioned in our response to the referee's comment about the gBA, we see good agreement between the γ SF from $^{70}\text{Cu}^{\text{gs}}$ and data from the $^{70}\text{Zn}(\text{p,p}')^{\text{Oslo}}$ analysis. We also know that this γ SF should contain both E1 and M1 components (see our response to part v). Thus, we consider the γ SF from $^{70}\text{Cu}^{\text{gs}}$ decay to be the "true" γ SF, and this is the γ SF which should be used to compare to other data for application to nuclear astrophysics. The data from $^{70}\text{Cu}^{\text{m2}}$ with the reduced E1 component are not representative of the full γ SF and this is why the original extrapolation to the IVGDR is lower and why we have removed it from Figure 1 for presentation in our updated manuscript.

The referee asks, "how was the width of the M1 component determined?" If by the M1 component, the referee is referring to the LEE, this width was determined by fitting the exponential decay function to the low energy data points. In Figure 1, we do not attempt to disentangle the contributions of the E1 and M1 fit but assume the GLOs are some combination of the two components. As we do not disentangle the E1 or M1 components here, use these values in any subsequent analysis presented in the manuscript, and present the extrapolations simply to show that we can reasonably connect our experimental data with the GDR, we do not find the anchoring of data at ~ 15 MeV to be a flaw.

iv) The extracted level density determined from the ^{70}Cu g.s. decay is questionable, too, and points to problems with the normalization as the number of discrete levels, including the ground state, is not reproduced. The NLD determined for the decay of the 1+ isomer looks better at energies where level counting seems still possible (Fig. 7.4 in the thesis). The disagreement with the predicted level densities, as shown in the thesis, also partly questions the argument presented for Fig. 2 (c). Some of the same authors have suggested normalization to theory predictions in their earlier studies. How would that change the results presented in this manuscript?

When performing an Oslo-type analysis, the nuclear level density and γ -ray strength function are extracted simultaneously, and we appreciate the referee's interest in the level density results from this experiment. In the situation we present in this manuscript, the slope of the γ SF is fixed by the Shape method, which has no relation to the NLD. In fact, the use of the Shape method removes ambiguities in the normalization of the NLD, as the slope of the γ SF and NLD are the same (see the response to point 7i). However, we understand that as we present γ SF data, it is reasonable for one to want to make sure the NLD is not unphysical.

The NLD figure referenced here by the referee (Fig 7.4 from the PhD thesis) is not the direct output from the Oslo method. This figure is included in the PhD thesis as part of a discussion on the nuclear spin-distribution and (n,g) cross-section constraints and the authors do not find it relevant to the present manuscript or integrity of our NLD data.

We state in the submitted manuscript:

"The ^{70}Zn level densities extracted from $^{70}\text{Cu}^{\text{m2}}$ and $^{70}\text{Cu}^{\text{gs}}$ β decay are partial level densities, assumed to result from spins associated with allowed Gamow-Teller decay and one dipole transition ($J = 0 - 3$ for $^{70}\text{Cu}^{\text{m2}}$ and $J = 4 - 8$ for $^{70}\text{Cu}^{\text{gs}}$). Each partial NLD is normalized to low-lying discrete levels."

The figure referenced by the referee (Fig 7.4) is an extrapolation of the partial NLD to the total NLD. We would therefore like to direct the referee to Figure 6.17 of the referenced PhD thesis (reproduced below). This figure shows the partial nuclear level densities from each β decaying state. The referee will find very good agreement between the experimental partial NLDs presented here and the known discrete levels.

[Figure redacted]

[Text redacted]

The referee mentions disagreement with the predicted level densities. The referee will find in Figure 6.17 from the PhD thesis that there is a significant disagreement between four of the TALYS models (Constant Temperature, Back-shifted Fermi Gas, Generalized Superfluid Model, and the Skyrme+HFB model). However, both the Gogny+HFB and Temperature dependent Gogny+HFB models match the slope (and in the case of $^{70}\text{Cu}^{m2}$ the magnitude) of the partial NLDs.

Fig 2 (c) in the present manuscript shows the parity asymmetry in ^{70}Zn from the Gogny+HFB model, one of the models which matches the slope of the partial NLDs. We also find this behavior in shell model calculations, and the experimental data as noted in the manuscript: “Shell model calculations using the jun45⁴⁵ and jj44b⁴⁶ Hamiltonians produce qualitatively similar ratios for the parity asymmetry as a function of excitation energy. Experimental levels with assigned spin-parity exhibit similar behavior, where above 3 MeV the ratio of negative parity states to positive parity states is higher for spins $J=4-8$ than it is for spins $J=0-3$.” As the behavior we report in Fig 2 (c) is supported by multiple theoretical models and current experimental levels, we do not think the use of this model to demonstrate the concept of parity asymmetry in the nucleus is questionable.

Finally, the referee mentions that some of the authors on this paper have used normalizations to theory in earlier studies. Normalization to theory is used in situations where there is no other alternative (either it is not possible to use the Shape method or there is no available information about the level spacing at the neutron separation energy). While the authors acknowledge that normalization to theory can lead to larger uncertainties in some Oslo-type experiments, we do not find this discussion to be relevant to the present analysis, as we were able to use the Shape method to empirically determine the slope of the γSF and NLD.

v) Given the sketch in Fig. 2, why should one expect that the M1 strength is the same for the two cases (a) and (b); i.e., that it is the same in the two gSF shown in Fig. 1? As sketched in Fig. 2, more high-energy M1 transitions should be observed from states in the quasi-continuum in case (b) than in case (a). Still, the authors do not vary the M1 strength. So, this effect might also counteract the suggested E1 suppression for the gSF resulting from the ^{70}Cu isomer decay, which as said, depending on the normalization of the data, might not be as pronounced. This needs to be explained better if there is any resolution.

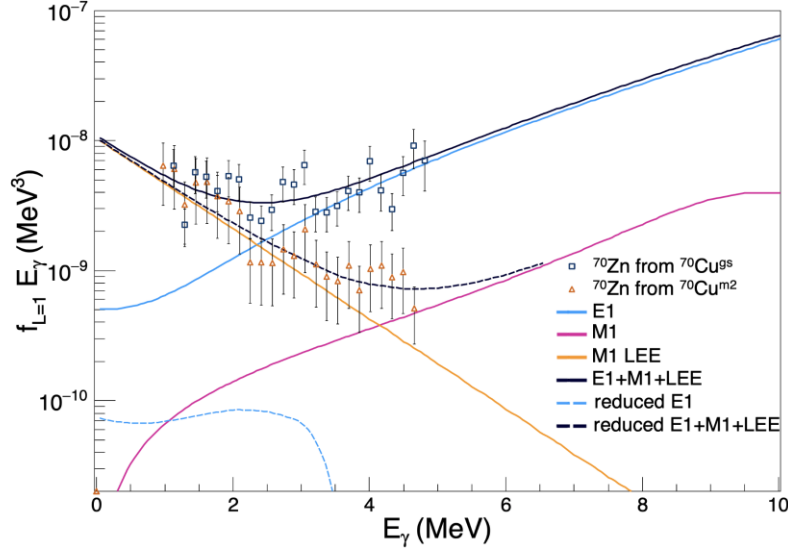
We thank the referee for this question, and we hope we can clarify a few points here about Figure 2 in our response. We will first explain what we show in Fig. 2a,b and how it relates to the LEE, and then explain how the difference between the two γ SF we present here can be interpreted in the context of a suppressed E1 strength and not an enhanced M1 strength.

For the purposes of displaying the level scheme of ^{70}Zn in this manuscript, we have not explicitly included levels above the 5^- level at 3.08 MeV, as in this region the level density is higher and the main purpose of Fig. 2a,b was to illustrate the lack of negative parity levels in ^{70}Zn at low energies. When we perform both the Oslo analysis and the Shape method analysis, we restrict ourselves to the region of the quasi-continuum, meaning that we only have access to primary γ rays that de-excite from levels above ~ 3.3 MeV in this specific nucleus. When investigating the LEE, the relevant γ -ray energies are around $E_\gamma < 3$ MeV, meaning that to probe this feature we are looking at primary γ rays that correspond to transitions between states in the quasi-continuum or just below it.

As we assume allowed Gamow-Teller β decay here, we expect to populate states with spin-parity 5^- , 6^- , and 7^- from the β decay of $^{70}\text{Cu}^{gs}$ in the quasicontinuum. Assuming only dipole transitions, transitions to states with negative parity would be M1 transitions and transitions to states with positive parity would be E1 transitions. From the evaluated nuclear data sheet in Ref. 41, there are many levels above 3.3 MeV populated in the β decay of $^{70}\text{Cu}^{gs}$ that can undergo either E1 or M1 transitions to the lower lying levels in the nucleus. The theoretical prediction shown in Fig. 2c indicates the same behavior, where there should be both negative parity and positive parity states available in the nucleus in the quasicontinuum for decay. Therefore, the low-energy portion of the γ SF should contain both E1 and M1 components.

Following the same assumption of allowed Gamow-Teller β decay for $^{70}\text{Cu}^{m2}$ we should populate states with spin-parity 0^+ , 1^+ , and 2^+ . From the evaluated nuclear data sheet in Ref. 41 and the theoretical predictions in Fig. 2c, we see that in the quasi-continuum here there are far fewer negative states than there are positive states. Operating under the same assumptions about only selecting for dipole transitions, transitions to positive parity states would be M1 transitions and transitions to negative parity states would be E1 transitions. As there are far fewer negative parity states in the nucleus with the appropriate spins to be populated by $^{70}\text{Cu}^{m2}$, we conclude that the E1 strength here is suppressed. At high excitation energies, theoretical calculations and experimental data do not indicate that there are significantly more positive parity levels populated by $^{70}\text{Cu}^{m2}$ than there are negative parity levels populated by $^{70}\text{Cu}^{gs}$, so the M1 strength remains the same.

The high-energy γ ray transitions pointed out by the reviewer are transitions down from the quasicontinuum to the discrete states, and these transitions correspond mostly to the portion of the γ SF where $E_\gamma > 3$. The referee is correct that for the case of the level scheme we show in Fig. 2b, this high energy portion of the γ SF is primarily dominated by M1 transitions, whereas in Fig. 2a this region is primarily dominated by E1 transitions (with some M1 transitions). We direct the referee to Fig 3. (reproduced below) where in fact, the suppressed E1 is functionally zero at $E_\gamma > 3$ and the entire γ SF is dominated in this case by M1 transitions. The opposite is true for the non-suppressed E1, which dominates the γ SF at these high energies. This behavior is consistent with what we display in Fig. 2a, b.



In conclusion, the claim presented in this manuscript is too strong and the data might not unambiguously support it. Without a doubt, proving experimentally that the LEE is of M1 character and that it is not a fundamental part of the gSF would be important for the field of nuclear physics and nuclear astrophysics. However, the manuscript cannot be accepted in its current form and a revision is needed. The authors must satisfactorily address the concerns.

We thank the referee again for their thorough review of our manuscript. We hope that our detailed responses to their concerns above have convinced the reviewer of the robustness of our claim that the LEE is of M1 character, and that we have satisfactorily addressed their concerns with our responses.

The authors would like to thank the referees for taking the time to review our revised manuscript and are happy that referees 1 and 2 recommend moving toward publication. We also appreciate the additional questions from referee 3. Please find below detailed answers to the remaining concerns. Our replies are in purple and any “quotations from the text” are presented in blue.

Referee #1 (Remarks to the Author):

The authors have addressed all of my comments. I recommend publication.

Referee #2 (Remarks to the Author):

I thank the authors for their very careful and detailed address of all of my initial concerns. As I wrote in my initial review, I considered all of them to be of relatively minor importance, with the exception of what I labeled as point 8). This query pointed to the robustness of their conclusions, and it has been addressed with special care by the authors. I have found their clarifications quite illuminating, and I now tend to hold their main conclusions as a rather strong and convincing result. All the other points were also answered to my full satisfaction. I believe this paper would be a very valuable addition to the relevant literature associated with nuclear electromagnetic decay.

Referee #3 (Remarks to the Author):

The authors provided a detailed response to my initial report. I do, however, still have concerns about the uniqueness of their result and the strong assumptions, i.e., model dependency leading to their final conclusion about the LEE being of M1 character. In their comment #8, Referee #2 expressed these concerns as well and I again feel that the authors' answer is solely based on arguments coming from model expectations rather than experimental data. I do not think that they can exclude a low-energy E1 component from their data. Furthermore, the theory arguments come from a theory (HFB+QRPA at best) that does not include complex configurations. Such configurations do influence both the level density in the quasicontinuum and the strength fragmentation. They will also impact the decay behavior between states in the quasicontinuum as wavefunctions are more mixed. The authors themselves state, including in their response letter, that E1 transitions are typically “stronger” than M1 transitions. In fact, below particle-emission thresholds, this is not necessarily true. Gamma widths can be quite comparable or be even larger for M1 transitions. But for the sake of the argument let us assume that this statement was true at all energies, then the experimental data cannot exclude a significant E1 contribution to the upbend (LEE) as E1 transitions should then be stronger between states in the quasicontinuum. As the observed counts in their primary matrix are modeled to be the product of the level density and gamma-ray transmission coefficients, stronger E1 transitions might counteract the lower density of the states needed to observe E1 transitions and, thus, lead to the same result as a higher level density and weaker M1 transitions. Of course, as also pointed out by the other referees, most theories do expect the LEE to be of M1 character (mostly shell-model calculations, which cannot correctly describe E1 strengths because of restricted model spaces) and to result from low-energy transitions between states in the quasicontinuum. I am concerned that the authors interpretation of their data and resulting normalizations between the gSF from the Cu-70 g.s. and Cu-70 isomer are biased by expectations for the LEE and theory predictions. To support publication, I would, thus, urge the authors to, at minimum, refrain from using words like “conclusively” in their presentation.

We thank the referee for taking the time to read over our responses to their original report. We hope that below we can provide clarification on some of the referee's comments and concerns.

First, we understand the referee's concerns about theoretical models for the γ -ray strength function. The referee is correct that QRPA calculations and Shell Model calculations both have pitfalls when modeling the γ SF and the LEE. This is, in fact, the reason that it has been so difficult to determine the nature of the LEE with confidence for so many years, and it is the reason we approached our result from a conceptual perspective and focus on the availability of nuclear levels and parity asymmetry instead of a particular strength function model. We do not base our conclusion about the M1 nature of the LEE based on these γ SF models, but rather on the available spins and parities of levels in ^{70}Zn confirmed by both experimental data and theoretical calculations. We direct the referee to our numbered replies to their response letter below (specifically points 1-5) for detailed explanations regarding the theoretical models in this work, which we hope will ease some of their concerns.

Second, we very much appreciate the referee's suggestion to refrain from using strong statements like "conclusively" in our manuscript. Thus, we have fully removed all such statements. We have also made the referee's suggested changes to the manuscript, please find the according modifications detailed at the end of our response.

Third, please find our responses below to some of the referee's general concerns.

The referee states: "authors themselves state, including in their response letter, that E1 transitions are typically "stronger" than M1 transitions. In fact, below particle-emission thresholds, this is not necessarily true. Gamma widths can be quite comparable or be even larger for M1 transitions. But for the sake of the argument let us assume that this statement was true at all energies, then the experimental data cannot exclude a significant E1 contribution to the upbend (LEE) as E1 transitions should then be stronger between states in the quasicontinuum. But for the sake of the argument let us assume that this statement was true at all energies, then the experimental data cannot exclude a significant E1 contribution to the upbend (LEE) as E1 transitions should then be stronger between states in the quasicontinuum. As the observed counts in their primary matrix are modeled to be the product of the level density and gamma-ray transmission coefficients, stronger E1 transitions might counteract the lower density of the states needed to observe E1 transitions and, thus, lead to the same result as a higher level density and weaker M1 transitions."

We would first like to address the referee's point that we claim that E1 transitions "are typically 'stronger' than M1 transitions". In the introduction to the manuscript, on lines 70+71 we state that "Magnetic dipole (M1) transitions do not have a parity change ($\Delta\pi = 0$) but do have a change in angular momentum ($\Delta I = 1$) and are generally weaker than E1 transitions." The purpose of this line is to generally introduce the broad readership of Nature to E1 vs M1 transitions, and we do not use this sentence to justify any of our arguments later in the manuscript. Furthermore, we rely on previous experimental data to support the assertion that for this nucleus that E1 transitions are typically stronger than M1 strength functions. We state in the manuscript: "The amplitude of the E1 and M1 exponential LEE components were allowed to freely vary and the non-exponential M1 component was fixed at approximately an order of magnitude weaker than the E1 strength. This order of magnitude difference between E1 and M1 strengths is expected based on other nuclei within this mass region¹⁰," and also discuss this point in the referee responses. This comment is regarding the M1 γ SF without the LEE and is supported by the literature in the region (Ref. 10 of the manuscript). We do not make any additional claims about the strength of E1 vs M1 transitions.

The referee's statement about the primary matrix is correct here and applies for the case of the primary matrix resulting from β decay $^{70}\text{Cu}^{\text{gs}}$, where both E1 and M1 transitions can be observed and expected. The statement is still correct for the primary matrix resulting from the β decay of $^{70}\text{Cu}^{\text{m2}}$,

but the stronger E1 transitions here referenced by the referee cannot physically occur based on the states populated by $^{70}\text{Cu}^{m2}$ and low-energy level scheme of ^{70}Zn . The J^π of $^{70}\text{Cu}^{m2}$ is 1^+ , meaning that via allowed GT β decay, states with $J^\pi=0^+, 1^+, 2^+$ can be populated. For an E1 transition to occur from these states, there would need to be an available state with $J^\pi=0^-, 1^-, 2^-, 3^-$ at low energies in ^{70}Zn . The first available state in ^{70}Zn with the proper spin-parity for an E1 transition to occur is at 2.895 MeV. This is what we show in Figure 2b of the manuscript. In the quasicontinuum region we do expect to see both E1 and M1 transitions as the referee suggests, and this is also shown in Figure 2b of the manuscript. The E1 component is shown as the “reduced E1” blue dot-dashed line of Figure 3.

Our results are also supported by the fact that the Shape Method was used in this analysis which isolates transitions into particular spins/parities. Specifically, the Shape Method was applied to the β decay of $^{70}\text{Cu}^{m2}$, populating $0, 1, 2^+$ states and feeding 2^+ discrete states. These transitions correspond to predominantly M1 transitions. On the other hand, the Shape Method, when applied on the β decay of $^{70}\text{Cu}^{gs}$, predominantly selects E1 transitions. While the Shape Method is not reliable at low excitation energies (in the LEE region), its selectivity is an independent experimental confirmation that our interpretation is valid.

The referee also states: “Of course, as also pointed out by the other referees, most theories do expect the LEE to be of M1 character (mostly shell-model calculations, which cannot correctly describe E1 strengths because of restricted model spaces) and to result from low-energy transitions between states in the quasicontinuum. I am concerned that the authors interpretation of their data and resulting normalizations between the gSF from the Cu-70 g.s. and Cu-70 isomer are biased by expectations for the LEE and theory predictions.”

We agree with the referee that an interpretation of our results and normalization based on previous expectations for the LEE and theory predictions would be a point of concern. Therefore, we would like to take this opportunity to make it clear that we do not base our results on theoretical expectations for the LEE and this is not what we demonstrate in this manuscript.

We base our interpretation of the M1 nature of the LEE solely on experimental data and the availability of certain parity states in the nucleus. Our argument for the M1 nature of the LEE is based on the spins and parities of available states in ^{70}Zn . From the β decay of $^{70}\text{Cu}^{m2}$, we populate states with $J^\pi=0^+, 1^+, 2^+$. From the structure of ^{70}Zn (confirmed by both the experimentally known level scheme and theoretical calculations) the available states in ^{70}Zn accessible via dipole transitions are mostly states with $J^\pi=0^+, 1^+, 2^+$, and 3^+ (associated with M1 transitions); states with $J^\pi=0^-, 1^-, 2^-,$ and 3^- (associated with E1 transitions) are far fewer in number. The ^{70}Zn γ SF extracted from the matrix populated with $^{70}\text{Cu}^{m2}$ shows an incredibly strong LEE. This leads us to the conclusion that the LEE is due to M1 transitions. In this line of logic, we do not use any γ SF theory or previous expectation. Further, in our prior response we showed what would happen if we assumed the LEE was E1 in nature and the resulting gSF did not reproduce the experimental data.

Regarding the normalization of the two ^{70}Zn γ SFs, we also do not use any γ SF theory or previous expectations to inform the way we normalize the two γ SFs in magnitude. The ^{70}Zn γ SF extracted from the β decay of $^{70}\text{Cu}^{gs}$ should result from both E1 and M1 transitions, as is expected from the populated spins-parity of the available levels in ^{70}Zn . As this γ SF includes both E1 and M1 components (and agrees with the $^{70}\text{Zn}(p,p'\gamma)$ from the M.S. thesis from the University of Oslo, mentioned in the previous referee response) we consider this to be the full ^{70}Zn γ SF. For the ^{70}Zn γ SF extracted from the β decay of $^{70}\text{Cu}^{m2}$, we see fewer E1 transitions than we do from $^{70}\text{Cu}^{gs}$ β decay, leading to our statement that the E1 component of the γ SF is “suppressed”, all other

components of the γ SF should stay the same. This interpretation is what influenced the normalization of the two ^{70}Zn γ SFs, not previous theoretical expectations.

We give more detailed replies to the rest of the referee's response letter below, which we hope will mitigate their concerns.

A few more comments.

a) Response letter (not in order).

1. - "... we do present the comparison of a microscopic gSF model ...", as also pointed out above, this model is very restricted. The QRPA only includes 1p-1h excitations. Furthermore, I am certain that the QRPA does not predict the LEE. Typically, the QRPA also only predicts the gSF built on the ground state and cannot model decays. Extended models are very recent and it is unlikely that these were already added to TALYS or used in this work. I am also sure that this model would not predict the suppression of the E1 transitions. Do the mentioned shell-model calculations actually get that?

We would like to thank the referee for bringing up this point. Our language in our original response to the referee should have been clarified. We present a comparison of a microscopic γ SF model as an exercise to demonstrate the impact of suppressed E1 transitions on the total γ SF. We do not present these QRPA calculations as a quantitative model of the γ SF in ^{70}Zn . We state in the manuscript that we use this model to "qualitatively demonstrate the suppression of the E1 strength function". We explain more in detail below and additionally include a discussion about shell model calculations.

First, we agree with the referee's statement that the QRPA model referenced here is very restricted. As the referee themselves mentions, it is very difficult to theoretically model the γ SF and the LEE, as HFB+QRPA calculations do not include complex configurations and shell model calculations cannot correctly describe E1 strengths based on restricted model spaces. Therefore, we base our arguments for the nature of the LEE on our experimental data and the availability of nuclear levels, and include the γ SF model as an additional exercise to demonstrate our argument.

The QRPA calculations we present here are available publicly in the reaction code TALYS (we used version 1.96, which was released in 2022 and does include the LEE). We state in the manuscript: Here, the shape of the E1 strength was based on the Gogny+HFB with QRPA model (using the D1M version of the Gogny force) by Goriely *et al.*⁵¹ and the shape of the M1 strength is modeled by a generalized Lorentzian⁵². An additional exponential component was added to account for the M1 LEE, as the LEE is expected to have an energy dependence that can be modeled by an exponential function^{20,28-34}." To produce the "E1 + M1 + LEE" solid-black line seen in Figure 3, we take the E1 strength from the Gogny+HFB with QRPA model, the M1 strength from a generalized Lorentzian (shape taken from TALYS but scaled such that it is 1 order of magnitude lower than the E1 strength, following trends in the region, see Ref. 10), and the LEE in the form of an exponential function (also taken from TALYS). These portions of the γ SF are added together to get the full γ SF. The resulting γ SF, was then scaled in magnitude to match the experimental ^{70}Zn γ SF. This combination of E1 + M1 + LEE γ SF components describe the shape of the ^{70}Zn γ SF from $^{70}\text{Cu}^{\text{gs}}$ relatively well.

To demonstrate the impact of the parity asymmetry in the ^{70}Zn γ SF from $^{70}\text{Cu}^{\text{m2}}$, we then apply the reduction factor (derived from experimental data) to the E1 strength. We do not change the LEE or the M1 component. The result of the sum of these components is the "reduced E1 + M1 + LEE" ^{70}Zn γ SF presented as the dashed-black line in Figure 3. This line agrees very well with the shape of the ^{70}Zn γ SF from $^{70}\text{Cu}^{\text{m2}}$. This exercise qualitatively confirms our argument for the origin of the LEE

from our experimental data. The referee is correct that QRPA calculations cannot quantitatively predict this behavior, but we do not claim in the manuscript that this is the case.

Second, we answer the referee's question: "Do the mentioned shell-model calculations actually get that?". The short answer is yes. Shell model calculations for both the E1 and M1 components of the ^{70}Zn γSF are computationally very intensive and difficult to perform. As a result, we will also demonstrate this qualitatively and show a combination of KSHELL calculations, where the M1 component of the ^{70}Zn γSF modeled with the jun45 SM Hamiltonian and the E1 component comes from another Zn nucleus with a documented significant LEE, ^{64}Zn . The low-lying structure of ^{64}Zn is very similar to ^{70}Zn ; the ground state is 0^+ and there are only positive parity states present in the nucleus until about 3 MeV (at which point more negative parity states are present). Based on our arguments from before, this should result in a very similar phenomenon where the E1 strength function is suppressed and the region of the γSF where the LEE occurs should be primarily dominated by M1 transitions. Please note that we have not done any gating on the spins for these calculations; these results include all the spins included in the shell-model calculations: $J=0-12$ for ^{70}Zn and $J=0-16$ for ^{64}Zn . These calculations are shown below in Figure 1.

[Figure redacted]

[Text redacted]

In Figure 1 the E1 γSF from ^{64}Zn is quite suppressed until about 3.5 MeV, at which point it begins to grow and dominate the total γSF . Below this the M1 γSF (which includes the LEE) dominates the total γSF . This is consistent with the argument that we present in the manuscript regarding suppressed E1 transitions, and the behavior of the γSF here is very similar to the ^{70}Zn γSF from $^{70}\text{Cu}^{\text{gs}}$ β decay.

We would also like to make note that both the jj44b and jun45 shell model Hamiltonians predict the parity asymmetry in ^{70}Zn that would lead to the suppressed E1 γSF .

2. - " ... this work since we were able to provide an interpretation based on experimental observations.", as mentioned above, the authors normalized their data based on expectations from a theoretical model, their data are, thus, not a clear experimental observation. The data was "manipulated".

We would like to reiterate that we do not normalize our data based on expectations from a theoretical γ SF model. The two ^{70}Zn γ SFs were normalized based on the conclusion that the E1 γ SF from $^{70}\text{Cu}^{m2}$ β decay is suppressed, resulting from the availability of nuclear states with the necessary spin and parity for E1 transitions to occur. This interpretation relies on the low energy level scheme of ^{70}Zn which is well known in the literature. We support this conclusion with the experimental high energy level scheme of ^{70}Zn , shell model calculations from the jj44b and jun45 Hamiltonians, and Gogny+HFB microscopic nuclear level density calculations. We do not use any theoretical γ SF model to normalize the data. If, by “manipulated” the referee is referring to the fact that we must normalize our γ SF in magnitude, then this is correct. However, this is a standard and necessary step of any-Oslo type method.

3. - “This gSF has a suppressed E1 gSF and is not a “true” gSF (containing the full E1 and M1 strengths), thus it is unphysical to extrapolate it to the GDR.”, while I appreciate that the authors deleted this extrapolation, the reduced E1 component is still derived from the tail of the GDR. So, technically, while not plotted in Fig. 1 of the manuscript, a part of the tail of the GDR is still part of the fit in Fig. 3 and the strength of the part is determined by anchoring the data to the (γ, n) data and scaling it by the parity asymmetry reduction factor shown in the response letter. This is an additional bias and the outcome would also be different if there were another low-energy E1 “bump” from, e.g., a mode like the PDR.

Our answer to this point is informed by our response to point 1, where we describe how Figure 3 was created. We provide an answer to the referee’s concerns here in two parts.

First, the reduced E1 component we present was not explicitly derived from the tail of the GDR. As described in our response to point 1, the E1 component of the γ SF was obtained from TALYS, as were the M1 component γ SF and the LEE component of the γ SF. These three components were added together, and the sum of the three was scaled in magnitude to fit our experimental data. We reiterate here that the data were not fit to these theoretical models; the models were just used to compare to the data and estimate the E1 reduction. Our experimental data were normalized to the GDR, so in this way the E1 component of the γ SF is “derived” from the GDR, but only in magnitude. The shape of the E1 component is strictly determined by the pre-tabulated Gogny+HFB with QRPA calculations from TALYS. We direct the referee to Figs A, B, and C of our previous referee response to see the comparison between the tail of the GDR and this E1 component of the γ SF. The reduced E1 component was determined by taking an experimentally derived factor and applying it to the full E1 component. Thus, the only way the reduced E1 component is “derived” from the GDR is in magnitude, not in shape.

Second, we would like to address bias from additional low-energy “bumps” from modes like the PDR. Our nucleus ^{70}Zn ($N=40$, $Z=30$) is not particularly neutron-rich and lies along the $N=40$ subshell closure. For this reason, we do not expect a PDR to be present in the γ SF. This expectation is confirmed by the $^{70}\text{Zn}(p, p'\gamma)$ measurement from the M.S. thesis from the University of Oslo, where the γ SF of ^{70}Zn was measured up until approximately 9 MeV and there is no indication of the PDR.

4. - “The referee asks, “how was the width of the M1 component determined?” ...”, the LEE component does not have a width. So, this question was asking about the width of the M1 component at higher energies, not the M1 LEE component.

We thank the referee for this clarification. We answer this question in combination with our response to question 5, please see below.

5. - “[...] In Fig. 1, we do not attempt to disentangle the contributions of E1 and M1 fit but assume

GLOs are some combination of the two components.” What does that mean? How was the extrapolation to the GDR region done? Which GLOs were fitted? Obviously, the components are shown individually in Fig. 3 (HFB “predictions”) and were “scaled” such (?) to describe the “data”. Does the black curve in Fig. 3 also describe the GDR data? I would like the authors to show the plot expanded to the GDR region and to add the comparison to the manuscript.

We thank the referee for the opportunity clarify this point. The extrapolation to the GDR we present in Figure 1 of the manuscript. This extrapolation was done to illustrate that our experimental data were normalized in such a way that they connect to the GDR. We use this extrapolation simply as a tool to demonstrate how higher energy parts of γ SF can connect to the GDR, but for no other purpose. The blue band we show in Figure 1 was created with three components. The first is an exponential function that was used to describe the LEE, as is supported by the relevant literature. The second and third terms are two generalized Lorentzian functions, used to connect the experimental data to the first and second bumps of the GDR (it can be seen in Figure 1 that there is a bit of an enhancement in the GDR at around 10 MeV both in ^{70}Zn and ^{68}Zn). We do not attempt to break down these GLOs into E1 or M1 components, but rather simply use the general shape to extrapolate between the end of our experimental data and the GDR. These GLOs + the exponential to the LEE were fit to the upper and lower bounds of our data, and the result of this exercise is in the blue band that is presented in Figure 1.

This extrapolation is in no way connected to what is presented in Figure 3. However, the black curve in Figure 3 does connect to the GDR. We again direct the referee to Figures A, B, and C of our previous referee response to see proof that the results from Figure 3 agree with the blue band from Figure 1.

6. - “As a result, we currently do not consider our result to be a violation of the gBA hypothesis.” The gBA hypothesis states that “that the g-ray strength function depends only on the transition energy, not on the specific initial or final excitation energy, spin, or parity.” → In my opinion, the authors show that the gSF depends on initial and final spin and parity (both of which they select in their experiment), or alternatively, they show that the statistical model description breaks down for their case. So, is the “different availability of states” a violation of the gBA hypothesis?

We really appreciate the referee’s continued interest in discussions regarding the gBA hypothesis. We think the referee brings up some very interesting points, but we ultimately think this discussion is beyond the scope of this manuscript. The referee suggests, what is in our opinion, a philosophical discussion about the gBA hypothesis we think is better to be had by the broader community in context of our result. As we move forward and continue to disentangle the origins of the LEE, we may be able to more definitively answer the referee’s question: “So, is the ‘different availability of states’ a violation of the gBA hypothesis?” But for now, we leave this point open.

We also want to make note that the referee is correct that the gBA hypothesis states that “the gamma-ray strength function depends only on the transition energy, but not on the specific initial or final excitation energy, spin, or parity”. What the referee omits from this statement is that the gBA hypothesis includes an exception for the obvious dipole selection rules (M1 vs E1 decays), see Refs. 1 and 2 below. The differences in the γ SF that we see from $^{70}\text{Cu}^{\text{m}2}$ and $^{70}\text{Cu}^{\text{gs}}$ β decay are purely the result of dipole selection rules, not from the initial excitation energy, spin, or parity of states in the compound nucleus. If this assumption is removed from the gBA hypothesis all features in the γ SF that arise from strictly E1 or M1 transitions (ie. the scissors resonance) would also be considered as violations of the gBA, which they currently are not.

This is the reason we state in our manuscript that: “the difference in the two gSF from $^{70}\text{Cu}^{m2}$ and $^{70}\text{Cu}^{gs}$ β decay does not appear to violate the generalized Brink-Axel hypothesis^{45,46} but instead reflects a different availability of states”. We have modified this statement to reflect that in our interpretation of the gBA hypothesis, we do not see our result as a violation of the gBA: “the difference in the two gSF from $^{70}\text{Cu}^{m2}$ and $^{70}\text{Cu}^{gs}$ β decay does not appear, in our interpretation, to violate the generalized Brink-Axel hypothesis^{45,46} but instead reflects a different availability of states.”

We also state at the end of the manuscript: “With the nature of the LEE finally fully established for the case of ^{70}Zn , we can focus our efforts into addressing remaining questions regarding the γ SF, such as how the amplitude and shape of the LEE might depend on nuclear structure, how we can predict this behavior⁵³, and how it might be related to the generalized Brink-Axel hypothesis.” We think that this statement invites the sort of discussion the referee is interested in having about the gBA.

[1] L. Crespo Campo, et al. “Test of the generalized Brink-Axel hypothesis in $^{64,65}\text{Ni}$ ”. PRC **98**, 054303 (2018).

[2] M. Guttormsen, et al. “Validity of the Generalized Brink-Axel Hypothesis in ^{238}Np ”. PRL **116**, 012502 (2016).

7. - “However, as our interpretation of the LEE developed, it became clear that the gSF extracted from $^{70}\text{Cu}^{m2}$ was not the full gSF, but a gSF with suppressed E1 transitions. Therefore, this gSF was renormalized in magnitude so there was agreement at low energies.” ... yes, this is exactly the problem. The renormalization was a choice guided by theoretical expectations, some of which might not be correct. Without this renormalization and the connected suppression of the E1 strength, the result would be quite different as shown in the PhD thesis.

We direct the referee to our responses to point 2 regarding the normalization of the two γ SFs. We hope that our response is sufficient to convince the referee that we did not normalize based on theoretical expectations but rather based on experimental data.

b) Revised manuscript

- line 43: “... and how the nucleus is structured.” This is a funny formulation. Maybe “how nuclear structure emerges” or “impacts our understanding of [...] and nuclear structure.”

We have modified this line to now read: “significantly impacts our understanding of how elements are created in stars and the structure of the nucleus”.

- line 44: “... which show conclusively”, please avoid “conclusively” and choose softer claim. Any difference in the two extracted gSF is interesting enough in my opinion.

We have removed the word conclusively from this sentence (and the others below, as suggested). The sentence now reads: “In this work, we present results from an experiment on the γ SF of ^{70}Zn which show for the first time that the enhancement in the γ SF of this nucleus is of magnetic nature.”

- line 78f: “... to arise from the collective behavior of the nucleus, ...” This is very vague. Please be more specific here.

This sentence has not been modified to read: “Additionally, a resonance occurring from 5-10 MeV, known as the pygmy dipole resonance (PDR), **is thought to arise from collective oscillations of a neutron-skin around a proton-neutron core**, though the understanding of the origin and properties of the PDR is still a work in progress^{15,16,17}.”

- line 96: Please avoid “conclusively”.

The word **conclusively** has been removed. The sentence now reads: “With our measurement, we are able to show for the first time that the LEE is of magnetic nature, providing a solution to a decades-long unanswered question in the nuclear physics community.”

- line 113: “... or in previous publications.” The PhD thesis is not a publication and the manuscript submitted to PRC has not been published yet.

The manuscript submitted to PRC has now been published. We have updated the reference accordingly and removed the citation to the PhD thesis, as the referee is correct that it is not a publication. The sentence has been modified to read: “... or in **a previous publication**.⁴²”

Reference 42: Ronning, E. K. Total Absorption Spectroscopy of Two Isomers in ^{70}Cu Influencing Nucleosynthesis Signatures. *Phys. Rev. C*. **113** 055802 (2026).

- lines 136ff: “... does not appear to violate the gBA hypothesis, ...” Maybe have a more nuanced discussion here. See my comments above.

We have modified this line to read: “the difference in the two γ SF from $^{70}\text{Cu}^{\text{m}2}$ and $^{70}\text{Cu}^{\text{gs}}$ β decay does not appear, **in our interpretation**, to violate the generalized Brink-Axel hypothesis^{44,45} but instead reflects a different availability of states.”

Please see our response to point 6 from the response letter for additional discussion about the gBA.

- line 173: “... because those transitions are not allowed to decay to...” Transitions do not decay, states do. Please correct.

This line has been corrected to: “...because those **states** are not allowed to decay to...”

- line 177: “... we loop over ...” You loop where? TALYS, Shapelt, Sorting Code? Please be more precise.

The loop is present in the analysis code that creates the reduced E1 γ SF shown in Fig 3. We have modified the sentence to read: “For a given primary γ -ray energy we first identify all excitation energies (E_x) where it is energetically possible to emit a primary E_γ . For each E_x we then determine the reduction factor....”

In summary, I think that the authors performed a remarkable experiment which warrants publication. However, I want them to address my comments listed above, implement changes accordingly, and to avoid the use of words like “conclusively” in their manuscript since the renormalization of their data intimately depends on the interpretation of the data they are seeking for. Work out the model dependency of the interpretation more clearly and then I support the publication.

We would like to thank the referee again for taking the time to read our responses and their continued discussion of our results. We also thank the referee for their suggestion to remove “conclusively” from the manuscript. We hope we have convinced the referee that we have based our interpretation and normalization solely on experiential data and not on γ SF theory.