Comment on Constraints on the low-energy E1 cross section of $^{12}\text{C}(\alpha, \gamma)^{16}\text{O}$ from the $\beta$-delayed $\alpha$ spectrum of $^{16}\text{N}$ *

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We dispute the alteration by Azuma et al. of the energy calibration of the Mainz('71) data on the beta-delayed alpha-particle emission of $^{16}$N as well as the very justification of the recalibration. We use the unaltered data to observe a pronounced disagreement between the TRIUMF('94) and Mainz('71) data sets on both the high and low energy sides of the primary peak (at 2.36 MeV) of the $^{16}$N alpha-spectrum. We cannot support the dismissal of the Mainz('71) spectrum by the TRIUMF collaboration and emphasize the need to include it in R-matrix fits. We discuss the need for new improved data to resolve this disagreement and its implication for the extracted p-wave astrophysical S-factor of the $^{12}$C($\alpha, \gamma$)$^{16}$O reaction.

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A measurement of the beta-delayed alpha-particle emission of $^{16}$N was performed at TRIUMF [2], which together with an R-matrix analysis of these and related data, was used to extract the p-wave astrophysical S-factor of the $^{12}$C($\alpha, \gamma$)$^{16}$O reaction. Such an analysis relies upon accurate knowledge of the line-shape of the spectrum of the beta-delayed alpha-particle emission of $^{16}$N. In the same paper [2] a comparison with the Mainz('71) data is shown, as communicated to Dr. F.C. Barker by Dr. H. Waffler [4] and published [5-7], and it is claimed [2] that the Mainz('71) spectrum "...is difficult to fit..." due to a broader line-shape. Hence the Mainz('71) data have been largely ignored by these and other authors. In this comment we discuss the above mentioned comparison and the validity of the Mainz('71) data and suggest that the Mainz('71) data should not be ignored in future R-matrix fits due to its high statistics (32 million counts as compared to 1 million of the TRIUMF data).

Recently, we submitted for publication a comment on Azuma et al. [2] which led to an ERRATUM [3] explaining the reason for the change in the original Mainz('71) data, thus compelling us to resubmit so as to comment on the ERRATUM as well. Azuma et al. [3] make the (implicit) claim that the energy calibration (10.6 keV/ch) contained in Waffler's communication to Barker [4] is wrong, and that the Mainz('71) spectrum can be self calibrated with high accuracy. They use Waffler's statement (in his letter) that "...channel 37 corresponds to 1281 keV..." [4] and claim that the centroid of the $2^+$ state is accurately extracted from the Mainz('71) spectrum [4]. Using the known energy of the $2^+$ state (only!) they derive a different energy dispersion (not quoted [3] but most likely 10.45 keV/ch [3]). This recalibration leads to a (very) different spectrum with energy shifts with "The difference ranges from 6.5 keV at the low end to 18 keV at the higher energies" [4].

At first we comment that it seems arbitrary that Azuma et al. [3] adopt a part of Waffler's calibration ("channel 37 corresponds to 1281 keV") [4], but reject the very dispersion (10.6 keV/ch) used to calibrate channel 37. This illogical argumentation in and of itself should cast doubt on Azuma et al. [3]; nonetheless, in this case their recalibration [3] hinges on the ability to extract the centroid of the $2^+$ state in the Mainz('71) spectrum [4]. In Fig. 1a we show the Mainz('71) data over the region of interest. The raw data show a very strong energy dependence, and in the vicinity of channel 106 one observes a minuscule excess of counts, most likely due to a contribution from the $2^+$ state. An accurate extraction of a centroid for this excess is very dependent on the choice of background and requires data with extremely good statistics. In addition a reanalysis of the data at this time requires a knowledge of experimental artifacts (e.g. differential non-linearity of the data recording etc.) which no longer exists.

The exact energy dependence of the background cannot be calculated ab initio as it is a convolution of the beta-decay phase space with contributions from the broad $1^-$ state plus non-calculable background state(s). In Fig. 1a we show a fourth order polynomial background fit ($\chi^2/\nu = 0.7$ for channels 92-100 and $\chi^2/\nu = 5.2$ for channels 111-114). The background subtracted data are shown in Fig. 1b together with a fit to a gaussian with a centroid fixed at the expected energy of the $2^+$ state ($E_\alpha = 2.0115$ MeV, channel 105.9). The resultant fit is not inconsistent with the expected shape for a $2^+$ state together with undulations in the background (more evident at the higher channels, e.g. $\chi^2/\nu = 5.2$ for channels 111-114). In addition, we emphasize that choices of background lead to a sizable (at least one channel) systematic uncertainty in the extracted centroid, which is different than the statistical uncertainty (derived from chi-square considerations). In contrast, Azuma et al. [3] imply an extraction of a centroid (with a sub-channel accuracy) that is approximately one channel higher than the expected location of the $2^+$ state [3]. We emphasize that this implied accuracy of Azuma et al. [3] is considerably better than claimed by the Mainz group itself using the reliable $^{10}$B($n, \alpha$)$^{7}$Li calibration procedure ($\pm 10$ keV) [4,5]. We conclude that the claim that the Mainz calibration is wrong cannot be substantiated, nor can the recalibration procedure of Azuma et al. [3] be justified.

In Fig. 2 we show the ratio of the original unaltered Mainz('71) data and the TRIUMF('94) data [4]. The two data sets were normalized to each other at the highest yield point at approximately $E_{cm} = 2.36$ MeV; we employed linear interpolations whenever necessary, and the error bars include the uncertainty of both data sets. This ratio deviates from unity by as much as 30-40%, with a chi-square per data point of 123. Pronounced deviations from unity are
observed on both the high and low energy sides of the highest yield point (at $E_{cm} = 2.36$ MeV), in contrast to the statements of Refs. [2,3]. The discrepancy on the high energy side of the $^{16}N$ data negates the assertion [2] that it is due to partial charge collection in the Mainz detector affecting their line-shape. This disagreement underlines the need for new reliable data. In this context we refer the reader to our recent (peer reviewed and soon to be published) preliminary report [10] that includes detailed figures comparing the TRIUMF data to new data measured by our group as well as a comparison with an unpublished (but extensively discussed) data communicated to us by the Seattle group [11]. As shown in Ref. [10], these new data disagree with the TRIUMF data but agree with the unaltered Mainz('71) singles data. 

It is most important to evaluate the effect of the various data sets on the extracted p-wave astrophysical S-factor of the $^{12}C(\alpha, \gamma)^{16}O$ reaction. This question is beyond the scope of this short comment, but we remark that the $^{16}N$ spectrum allows for extracting the reduced alpha-particle width of the bound $1^-$ state at 7.12 MeV, but it can not determine a priori whether the interference between the bound and quasi-bound $1^-$ states is constructive or destructive, and we doubt the validity of the rather strong statement of Azuma et al. [2] that their data rule out the small S-factor solution (i.e $S_{F1} < 20$ keV-b). Clearly a change in the line shape by as much as a factor of two at 1.4 MeV (the region of the interference minimum) [10] is expected to, for example, significantly alter the f-wave contribution and thus the extracted p-wave astrophysical S-factor of the $^{12}C(\alpha, \gamma)^{16}O$ reaction.

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Fig. 1: (a) The Mainz(‘71) data with a background fit, (b) and subtracted from the data with a gaussian fit, as discussed in the text.
Fig. 2: (a) Ratio of the TRIUMF('94) data to the Mainz('71) data, as discussed in the text.