Report on the PhD thesis by Nicola Burianová

"Use of reactors and quasi-nonoenergetic neutron sources in the study of reaction cross-sections important for advance nuclear systems"

The thesis reports several different cross section measurements from neutron-induced reactions obtained using the standard activation technique. Specifically, the author followed up her studies of ^{55}Mn(n,2n), ^{90}Zr(n,2n) and ^{127}I(n,2n) reactions at LR-0 reactor performed during her master study. Namely, she first performed activation measurements at the VR-1 reactor and determined the spectral averaged cross section of ^{89}Y(n,2n), ^{46}Ti(n,p), ^{47}Ti(n,p), ^{48}Ti(n,p), ^{54}Fe(n,p), ^{63}Cu(n,α), ^{93}Nb(n,2n), ^{58}Ni(n,x)^{57}Co reactions there. One of the main reasons for these measurements was a check of the contribution of neutrons originating from 238U fission. These neutrons are almost absent in LR-0 reactor but their presence (on the level of about 5% in the fast neutron region) in the VR-1 reactor could play some role (although the Figure 4.5 of the thesis seems to indicate that neutron spectra are very similar for relevant energies in both these reactors). In general, spectral-average cross section measurements are valuable as their results – when compared to predictions based on theory or other experimental data – often point out to some problems in our understanding of physical processes. These cross sections were published in a peer-reviewed journal and are available in the EXFOR database.

Second, she also measured differential cross sections of ^{63}Cu(n,α), ^{63}Cu(n,2n), ^{89}Y(n,2n), ^{89}Y(n,3n) reactions at two neutron energies, 14 and 23.3 MeV, using a neutron source installed at the U-120M cyclotron at Řež. This cyclotron has been used for these types of experiments for the first time. The cross section for ^{63}Cu(n,α) reaction has never been measured for neutron energies higher than 20 MeV and the lowest energy experimental point for ^{89}Y(n,3n) reaction is at about 24.5 MeV. The cross-section obtained with 23 MeV neutrons thus extends the measured neutron energy range. All other measurements were then compared to existing data. The results from these cyclotron-based experiments have not been published in a peer-review journal yet.

Overall, presented results are valuable and from a scientific perspective the submitted thesis is of a sufficient quality to be accepted as a PhD dissertation.

Nevertheless, I have some reservations against the quality of the text, statements appearing there, as well as the form of results presentation.

One of the problems seems to be the English of the candidate whose quality is very problematic. Probably the most important issue is the structure of individual sentences. In a relatively large number of them, the verb appears in front of the subject. However, the problems with the structure of individual sentences are more general. Also, inappropriate words are used in some cases.

The formal aspect of this work then suffers also from other issues. Some figures (sometimes reprinted) have insufficient description, see for example Fig. 4.9. Further, in several cases the text does not correspond to the figures - for example the text just below Figure 2.11 talks about presence of "characteristic peaks of X-ray and summing peaks." However, no peaks of this origin are visible in the corresponding figures. Another example of issues related to figures can be found in Fig. 4.20, where the legend says that ENDF is shown in blue, but there is no blue line there. Similarly, in Fig. 4.13 the legend lists some abbreviations that are not touched anywhere in the text.

The text might also mention that data in TENDL library are available only up to 30 MeV. So, the drop of the cross section at this energy shown in several figures is artificial and has nothing to do with reality. I would also expect that all the measured points will be given with their uncertainties. However, this seems not to be the case of decay data in Figs. 4.18 and 4.19. It would be also reasonable to label different data sets from EXFOR (with different colors or symbols). For instance, in Fig. 4.21, there are surely at least two data EXFOR sets, but they cannot be easily distinguished; more datasets are probably present in all figures showing data from EXFOR.

Further, it seems to me that different symbols are used for the same quantity at different sections. For example, the symbol A_0 in Eq. (2.11) seems to be equivalent to the symbol N in Eq. (2.16). I would also say that the labeling in Sec. 4 is not fully consistent with that used in Sec. 2. Very likely the symbol N_{yield} in Eq. (4.1) corresponds to N or NPA in Sec. 2. It is also interesting that the value of $N_{0,g}$ in Fig. 4.18 and Table 4.12 are not the same (in practice, there is a wrong label in the Figure and the value there is given with precision that does not make any sense).

The author gives a relatively thorough description of some technical features of reactors, which seem to be not quite relevant to the actual measurements. It is especially interesting that there are sections related to description of LR-0 and LVR-15 reactors although there were no experiments performed at these reactors for the purpose of this thesis. Instead of description of technical details of the experimental facilities it would help, for instance, to mention the energies of γ rays used in the analysis and the source of their origin.

In general, I have had problems to follow the text. It seems to me that the author jumps from one topic to another in some cases. For instance, vast majority of corrections required to obtain actual intensity of a gamma-ray transition is described in Sec. 2. However, when presenting the results from reactor-based experiments, a correction not mentioned in Sec. 2 is described. Similarly, a general text about decays of metastable states is embedded into description and discussion of results of reactions on Y target in Sec. 4.2.7. As decay of metastable states is a general feature, I would expect a different placement in the text. When presenting her results, the candidate also mixes those obtained during her master and PhD studies. Thus, it becomes sometimes very difficult to disentangle what she actually did during her master and PhD study. On the other hand, she tried to summarize the work done during PhD later, in the section 4.3 “Results summary”. This section sheds some light into the work really performed during here PhD.

In addition, I have also some comments to the physical issues mentioned in the “Introductory part” (Sections 1 and 2) of the thesis. The most important ones in my eyes are:

- In Section 1.3 the candidate talks about the general energy dependence of the neutron-induced cross section. However, this description is relevant (and illustrated) only for a fraction of these reactions (exothermic ones) and it is likely not relevant at all to reactions studied in the thesis.
- In Section 2.1.3 the candidate introduces some statistical issues related to the detection process. However, the section evidently mixed some considerations related to the number of detected events within time and energy windows. I have completely lost myself in this section.
- In Section 2.2 the author describes individual processes relevant for interaction of gamma rays with a matter. She says that there are three such processes. However, when she talks about the Compton scattering, she mentions also “elastic (Raleigh)” scattering (that would

then be the fourth process). The sentence about this process indicates that the elastic scattering is only a variant of the Compton one, but this incorrect statement might come from problems with the English. In addition, later the author shows figure 2.9 that contains five different processes. One of them (in fact a variant of one of the discussed processes) is not touched in the text at all.

Questions to the candidate:

1. There are measured and calculated efficiencies of used gamma-ray detector in Figures 2.13 and 2.14. The curves look very different. What is the reason for the difference? It is also a bit strange that the absolute efficiencies in the two figures are very different. Could the candidate comment on that?
2. In the beginning of Section 4 the text says: "There are many possible methods, how to experimentally obtain cross section", but only "off-line" activation method is mentioned. Could the candidate mention some of the other methods?
3. In Eq. (4.2) the factor in front of the efficiency for the 1332 keV transition reaches a value larger than one. Can the candidate explain how is it possible? I would expect that the factor can be at maximum equal to one.
4. On page 47 the text says: "Those result thus confirm the hypothesis that even 5% of 238U(n,f) does not have a significant influence on SACS uncertainties". I have no idea why the "uncertainties" are mentioned here. I would expect that the results are related to the SACS value itself but not to uncertainties. Is the text correct or not here?
5. I apologize but I have not found any remark on uncertainty of the absolute value of the flux in the text. However, Figure 4.13 seems to indicate that different "methods" for determination of the flux in "accelerator-based" experiments yield (slightly) different results. For instance, the absolute value in the "peak" seems to differ by at least 10% among the three shown curves. Has been any uncertainty in the flux considered during the analysis? Is the relative ratio of the flux corresponding to the "peak" vs to the "low-energy tail" known with a sufficient precision (the ratio for the black and green curves in the figure seems to be rather different)? The tail contribution enters one of the corrections.
6. Text in Section 4.2.6 mentions that the tabulated lifetime of ^{62}Cu is 9.67 min. However, the fit to data probably gives a value that differs by about 20% from the tabulated one. What was the typical uncertainty in the "fitted" lifetime? It is mentioned that the tabulated value is within the uncertainty of fitted value, but no uncertainty is given. In connection to this, as the lifetime of all studied nuclei is certainly known with a high precision, why was it not fix during the fitting? Or was it fix? Such a fixing should be fully justified and should give much smaller uncertainty in the remaining fitted parameters (i.e. those of the author interest).
7. It is mentioned in the text that the measurement with the Au target is used in cyclotron-based measurements as a monitor. However, the results shown in Fig. 4.20 indicate that the Au results are not fully consistent with the presented data. Is this correct or can this be the reason why most of measured points for other nuclei are also "below" the experimental data?
8. The text on page 60 says: "The metastable state ^{87m}Y is harder to determined and thus the measured value is not in such a good agreement with data library as the ^{87g}Y." This argument seems very strange to me. I would expect that difficulties in measurement will propagate

into larger uncertainties and not into the expectation value. Can the candidate make a comment also on this issue?

9. In Fig. 4.22, the TALYS prediction is not smooth? Is this a physical behavior or not?
10. Among “systematic uncertainties” in Sec. 4.4.1 the candidate mentions “current measurement”. What does this “quantity” mean?
11. The text on systematic uncertainties then also says: “The beam instability correction is within our precision negligible. The difference in neutron flux density between the first and the last irradiated sample can be up to 20%. Therefore, it is necessary to record the exact position of each sample.” I am confused with this part of the text. All these problems seem to be related from the text. It looks like that all the issues are connected. However, I have no idea what their relationship is, especially since an uncertainty in the irradiation position was mentioned to be of about 2% above. I would say that the flux should be connected to the beam instability. However, I have no idea how this is connected to the sample position. Could the candidate comment on this?
12. In the same section the text also says: “Those spectrometry uncertainties are the NPA uncertainty, which is less than 1%, …”. Personally, I would expect that the uncertainty in NPA (peak area) must correspond to the counting statistics uncertainty, which is certainly not of a systematic nature. Could this be commented on?
13. Further, the same section mentions how the “uncertainty” in the neutron energy of the beam was determined. I have a couple of questions to this fit. First, Figure 4.13 indicates that at least for neutron energy of 23 MeV the beam profile is not given by a Normal distribution. Does a fit with the Normal distribution make sense in this case? Second, the uncertainty in Eq. (4.13) corresponds to the width of the distribution. Can this quantity be considered as the “uncertainty of the neutron energy determination”? Finally, the text just below Eq. (4.13) indicates that the “peak” at lower energy (i.e. 14 MeV) has larger FWHM than that at 23 MeV. Based on Figures 4.13 and 4.14 this is likely not the case. However, such statement might be correct if one talks about “relative FWHM”. Is this the actual meaning of the sentence?
14. The section 4.4.2 then discusses “statistical uncertainty” of individual measurements and it contains Eq. (4.16). Should the individual uncertainties $\Delta q(i)$ in this equation account only for the statistical uncertainty or should a contribution of at least a part of the systematic uncertainty be included?

Despite all the comments and reservations, I have presented above, I believe that the submitted thesis fulfills all demands required for the PhD dissertation and after a successful defense I recommend awarding the candidate with the PhD degree.

Prague, October 2, 2024

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