Review of the project "Development of a wide-aperture backscattering detector (BSD) for the HRFD diffractometer"

Executive Summary:

Within the presented proposal for the project, the upgrade is well founded and well justified. The detector upgrade is the most cost effective way on achieving competitiveness for the HRFD instrument with present day state of the art instruments. The project itself looks feasible and realistic. The request for funding is modest given the scale of the project.

This project addresses an aspect where there is a shrinking capacity of expertise globally. There is therefore potential for the impact, not only to have an impact within the institute, but also an impact and interest wider, both regionally and internationally.

Therefore the referee ***strongly recommends*** that this project is accepted and proceeds as planned.

Below are presented some comments on the specific questions raised for this review as well as some general suggestions to be considered during the execution of this project.

– scientific merits and intellectual contribution;

The HFRD is already a productive instrument. Its strengths are its high resolution, and the fact that it is one of the few instruments worldwide in operation with a Fourier chopper. This makes the instrument a good choice for a flagship upgrade.

There is considerable discussion of the gain factor from the detector upgrade in this proposal, and it looks like it is in the region of 12 from consideration of solid angle coverage. The comparative gain factors from the recent other upgrades (chopper, guide, DAQ, etc) are not mentioned quantitatively, so cannot be directly compared. The argument on the gain from the increase in solid angle coverage is a good one and a gain factor of 10 is appropriate for a significant upgrade on the instrument.

The ambition of the proposal is high: to have a solid angle detector coverage greater than that from POWGEN. The detector area is impressive to see on POWGEN. This would be an upgrade which makes the coverage world-competitive. With a 2m diameter detector, as indicated in the figure in the proposal, this will look impressive!

However, it should be remembered that it is not just the quality and competitiveness of the diffractometer is not just a function of efficiency and coverage; for example for POWGEN the first years of commissioning were held back by "spurions"- i.e. data that is spurious. Care should be taken to make sure that quality is ensured throughout the build process. This is important to consider during detailed design, as the compression chosen for readout channels, to reduce total build cost, may accidentally introduce such effects. Testing of prototypes during the project is important to identify such problems before installation.

The choice to build the backscattering detector is important. Thus is the area where the highest resolution is accessible, and therefore maximise the impact of the gain factor for the detector upgrade. This upgrade will create a very performant instrument with a clear science application in mind to focus the design.

The exact requirements for the detector need to be expounded in detail at the beginning of the project. This is dealt with in the section below on technical feasibility.

On p5, the gamma sensitivity is mentioned as something needing improving. This is not enumerated further - and is of great interest to understand the impact and extent of this. Recent work reported on statistics from single photon counting from ISIS and other groups have made a significant improvement for the gamma sensitivity for such wavelength shifting fibre scintillator detectors. The referee recommends that the ambition during the project should be to determine the reduce the sensitivity by 1 order of magnitude. The referee expects that the present detector prototypes are probably achieving something in the region of 10ˆ-3 - 10ˆ-4.

The use of the glancing angle for the scintillator (used already on IBR2) is a very topical idea. This configuration is growing increasing interest, both for scintillators from SNS and ISIS, and for Boron-10 detectors. For scintillators, this is a natural way to overcome the limitations from the fact that ZnS is opaque, and the light needs to be extracted from the scintillator.

Both this development as a concept phase, and later the testing, realisation and results are (and should be) publishable as high quality journal articles.

– technical feasibility of the project within the proposed timescale;

The technical design of the project builds upon existing detectors installed at IBR2. As they form the basis on which the design is built on, the technical capability is demonstrated from the existing detector arrays. This means that the project should be technically feasible.

The overview plan for the project as outlined on p12, is sound. What is missing in the milestones of demonstration with neutrons of the performance of prototypes and mockups of mechanical items before series production. This is important to the engineering process, to ensure that the requirements specified are met within the project.

The schedule of the project roughly corresponds to the schedule that the reviewer would map out for such a project. It assumes a running start, i.e. the ability to start effectively and quickly right at the beginning of the project. It also doesn't contain much schedule contingency in case of procurement delays.

Care needs to be taken throughout the project is to regularly align the detector design to the scientific requirements and performance. It is important to think about the data needed from the final detector and what it will be used for. A demonstrator of modules on the instrument would be helpful, once a final design is arrived at, before series production starts.

As mentioned above, the exact requirements for the detector need to be expound in detail. This should be agreed, fixed and documented early in the project. The efficiency and coverage of the detector are important details in the detector design, and these are detailed in the proposal. However, the real level of performance will be defined by the time and space resolution (both the fwhm local resolution and the point spread function) as well as the non-local resolution/scattering/spurious effects. This is important to define the requirements in detail - and verify that the design exactly meets them before a full upscaling. The minimum set of requirements to be documented is:

- spatial resolution (fwhm and psf)
- time resolution,
- efficiency,
- gamma rejection.
- noise
- scatted neutrons.
- effect/requirements on dead spaces
- stability and uniformity.

Gamma rejection in particular is important to think about, which has been recognised in the proposal.

The design of the configurable 2nd layer allows for future performance upgrades.

A risk in such projects is always the delivery schedule of suppliers. The project proposal has wisely identified suitable suppliers for key items. It is important to ensure early on that procurement is compatible with the schedules desired. It is also important to identify, realistically, by when key design decisions are needed, to enable procurement to happen. This aspect should be closely monitored. In this aspect, HAMAMATSU, the only realistic supplier of PMTS (unless very simple PMTS are needed and they can be purchased from Electron Tubes). HAMAMATSU have a reputation of delivering when they are ready. The reviewer also thinks that Bicron is now Scintacor, having undergone 2-3 name changes in the past years.

– compliance of the requested financial resources with the project objectives;

The cost is ca 800k$. At about 2mˆ2 detector area this leads to a cost price of 400k$/mˆ2. This is below equivalent costing from SNS, JPARC, Julich and competitive with the cost from ISIS. This is therefore a reasonable estimate.
This cost is a lot less than the cost of an equivalent He3 detector.

Currency fluctuations are a risk. Buy early when design decisions known, to retire currency risk. Also importation risk may be a factor.

The travel costs allocated are reasonable and justifiable.

In conclusion, the financial request looks reasonable. It represents good value, as it is on the lower side of equivalent detectors at other institutes.

In the analysis above, it is assumed that the financial resources are estimated, excluding any budget contingency.

In appendix 4, the reviewer notes that several cells in the table are blank and assumed to be zero.

– availability of adequate human resources at JINR and in the collaborating institutions.

The manpower, ca. 1 person year (PY) effort is marginal. This 1 PY is allocated from internal resources to this project. This is very skilled and involves artisan effort. The manpower allocation should be followed in detail, and there is the possibility of the need for contingency for this. This certainly implies a very efficient build process.

BSU, Minsk, Belarus as a collaborating institute is mentioned, however, their role is unclear from the proposal. It is important to have competent collaborative partners involved in the project, even if their role is mostly a supporting role, and therefore this is to be encouraged.

Similarly, VTT, Finland is also mentioned in the proposal. It is not clear what their role is.

From the reviewers prior knowledge, the appropriate people and high level of trained expertise exist internally at JINR for this project. Numbers-wise, they are just about adequate for this. As is nearly always the case for such projects (including at the reviewers institute), loss of key people presents perhaps the largest risk for the delivery of this project, and should be monitored carefully.

The schedule (3yrs) looks reasonable, if tight. There is little opportunity for delays, and little schedule contingency on the critical path. This needs to be tightly monitored during the project. In particular, watch out for procurement delays, and try to gain schedule contingency by making key decisions early when possible.

Another risk is in the delay of resource (equipment, money or people) being committed to the project at the beginning. This should be done in a timely fashion on approval of the project to allow the ambitious schedule to be met.

In the schedule that testing with beam needs to be integrated into the schedule.

Other comments:

Loss of key people should be monitored carefully. To partially mitigate against this, opportunities should be taken to train the new generation of expertise, so that capacity is enhanced and retained past the end of the project. Construction of the detector is ideal for this. This also partially mitigates the dangers to this project whilst it is ongoing. This is about keeping technical expertise in house for future upgrade of this and other projects.

In terms of opportunity, it should be noted that there is a global lack of institutes working on detector development and construction. A major project such as this allows JINR to demonstrate that they can deliver large scale, competitive neutron detectors. This also increases internally expertise, which leads to a capacity enhancement internally at the institute. This is likely to lead to greater external interest.

To maximise impact, advise involving external collaborating partners in discussions. Would also suggest that dissemination of the concept (and later the results) through journal publication will lend to higher institutional reputation and recognition from other neutron sources for detector construction. This is beneficial for the enhancement of the longer term impact of this project.

On p7, it should be emphasised that the DAQ integration is an important aspect that should not be neglected.

P7 doesn't actually give the wavelength at which the efficiency was calculated. The reviewer assumes thermal=1.8A. This is important to judge the design. It should be noted that these numbers should be verified with testbeam data.

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