May 3 2010

FCPA/PPD/CD/AD review of the 21 cm R&D Initiative at Fermilab

The review was held on April 26, 2010, with a panel consisting of Rich Kron (chair), Wayne Hu, Huan Lin, and Gustavo Cancelo. The panel was charged with "reviewing the progress of the R&D effort to develop a radio telescope experiment to make a 3-D map of 21 cm radiation from large red shifts in order to measure baryon acoustic oscillations and thus study dark energy."

Presentations were given by Albert Stebbins, Nick Gnedin, John Marriner, Hee-Jong Seo, and Dave McGinnis.

Our report follows the charge with three sections: science, technical, and collaboration & funding. In each section, we first address each element of the charge, and then include some findings, comments, and recommendations. The recommendations concern both the project as a whole, and more specifically how subsequent presentations can be enhanced.

The appeal of the project at this time is at least partly based on the apparent low cost for the science reach. The science reach appears to be impressive, but the cost is not yet known since there is not yet a conceptual design. Since the components of the cylindrical telescope can be costed with good accuracy, and since the project has a relatively low level of technical risk (amplifiers and processors already exist that can do the job), a good cost basis should not be difficult to achieve. Prior to that time, it should be made clear that the $20M-$30M numbers refer to a project scope for M&S towards which the project is working, and is not yet an actual cost estimate that would include labor.

The project has a number of other appealing features: strong support from the theoretical cosmology group, significant involvement by experimentalists, a natural match to things that Fermilab can provide (e.g. expertise in RF technology and project management practices). Moreover, since the project is still at an early stage, it is a path for Fermilab to take a leadership role in a new major Dark Energy experiment.

The biggest hurdle may be finding the necessary funding. Our committee recommends the project continue forward with the scheduled external review and subsequent reviews.

1. Science reach

a) Is the science case strong and aligned with Fermilab goals? Is it likely to achieve 'mission need' at DOE?

The science case presented for the 21cm BAO project is strong and well aligned with the Fermilab and DOE interest in dark energy science. In particular, the statistical errors on the DETF figure of merit that may be achieved by 21cm BAO was shown to be competitive with the largest proposed BAO surveys, i.e., BigBOSS and JDEM, using "traditional" optical spectroscopy techniques. Moreover, 21cm BAO offers complementarity with optical surveys by providing independent measurements which will use different galaxy samples, as well as be subject to different sources of systematic errors.

b) Does this effort have a reasonable chance to achieve these science goals? Specifically, is there evidence that the problem of foreground subtraction can be surmounted?

A sophisticated simulation and analysis package was presented, including simulations of BAO and foreground sky maps, instrument modeling, and foreground removal algorithms. Detection of the simulated input BAO signal was demonstrated, at a qualitative level, for a realistic foreground sky. However, further algorithm development is needed (and is in progress) to quantitatively check the accuracy of the recovered BAO signal, as well as to incorporate the effects of noise, polarization leakage, sidelobes, and calibration errors in the simulations.

In addition, results were presented from recently published work from HIPASS and GBT surveys that demonstrated a positive cross-correlation signal between 21cm radio intensity maps and optical galaxy surveys, in particular at the higher redshifts $z \sim 1$ that would be probed by 21cm BAO. This is very encouraging, but additional observational work is needed to demonstrate detection of an auto-correlation signal as would be required for dark energy science via 21cm BAO.

c) What role will Fermilab have in extracting the science? Are personnel identified that will play a major role in the science?

A strong interest in extracting the 21cm BAO science, by both Fermilab theorists and experimentalists, was clearly demonstrated from the review presentations. In particular, Fermilab scientists were shown to be taking leading roles within the collaboration in terms of simulations, foreground subtraction, BAO measurement techniques, and dark energy figure of merit forecasts. However, there are still a relatively small number of Fermilab scientists involved at present, and participation of additional Fermilab scientists will benefit and further enhance Fermilab's scientific role within the project.

Findings

Dwarf galaxies with low rate of star formation cluster in a different way than the massive galaxies used for optical BAO surveys. This feature makes 21 cm and optical/IR surveys complementary in their tracers of LSS clustering.

The 21 cm approach enables detection of high-redshift neutral hydrogen. This provides a large number of modes to be sampled over 3 pi steradians (essentially limited by cosmic variance), and Dark Energy can be measured in a redshift range that complements other surveys.

The 21 approach measures a single strong line with good velocity resolution, yielding also the hydrogen surface density and its clustering amplitude.

The clustering amplitude of neutral hydrogen, and its evolution with redshift, is known theoretically to within a factor of 2.

Comments

An involvement in the 21 cm BAO project would allow Fermilab to participate in this Dark Energy experiment beginning at the ground level.

Recommendations

The nature and identity of the other potentially competing experiments in this field (including epoch of reionization) should be brought forward. The alignment of technologies suggests important things could be learned from these other efforts.

Opportunities for discovery of pulsars, especially millisecond pulsars, is potentially an important feature of the project, and thus should be developed further. What discovery space is opened with respect to, say, the PALFA survey at Arecibo?

The actual values for the adopted science requirements (minimum and maximum redshift, angular and velocity resolution, sensitivity, area of sky) should be presented in one place (not just the range of parameters studied), with justifications for the choices made. This will help define the project and better allow a comparison to othe projects.

2. Technical approach

a) Is the 21cm survey competitive in reach, cost and schedule
with other techniques proposed to study baryon acoustic

oscillations? Specifically, how does it compare with other BAO efforts FNAL might consider (JDEM, BigBoss, LSST)?

The CRT seems to be a single-purpose experiment; other astrophysics capabilities need to be better developed. As a consequence the CRT may not have the broader reach of other proposals. The CRT proponents think that 21 cm BAO is superior to photometric LSST BAO and comparable to BigBOSS and JDEM. It seems possible that 21 cm BAO measurement may provide a complementary measurement, given that 21 cm BAO uses a different technology with different systematic errors, calibration, etc.

The costing of the CRT is at a very early stage. Even when the detector architectural design is not complete, the proponents should choose an architectural configuration and supply their best understanding of the M&S and SWF cost. Having many alternative designs and costs will only confuse reviewers and delay the advance of the project. For the claimed \$20M-\$30M range the M&S cost seems to be driven by the electronics. A better presentation of the electronic design is required. A better understanding of the cost and schedule will come from filling the gap between the engineering requirement numbers and the engineering implementation of those requirements.

b) Is the specific technique explored by the R&D effort at FNAL (cylindrical radio telescope array) the best approach to a 21 cm survey?

The technique seems adequate and the collaboration has made progress in understanding the signals and background subtraction. It is not easy to evaluate if this is the best approach. The current simulations show good work in progress. The simulation work should advance to include noise and systematic errors in order to obtain better estimates. Building a prototype telescope will help answer questions, but the R&D goals for the prototype should be defined first.

A point of concern is the proposed site for the CRT. A remote location in Morocco will make the logistics complicated and expensive. The proposed site should be evaluated in comparison to other candidate sites. The evaluation should cover technical advantages versus a cost estimate for building, commissioning, and operating a detector.

c) Assess the technical progress made to date. What resources were used and is the current technical status promising?

The proponents should be congratulated for the technical progress. The CRT proposal seems to be designed on a solid background of theory, engineering requirements and simulations. However, the proposal is still in an early stage. There is substantial work ahead to organize the proposal as a project that can be presented in front of DOE reviewers. Although the next step is an external review with probably more focus on the science and technical aspects, the collaboration would benefit from looking at the way documentation of other DOE projects more advanced in the DOE approval chain is organized and presented. Not all the material presented at the April 26 review seemed relevant for a technical review.

d) What is the expected technical role at FNAL? Does the lab have the required facilities and personnel to fulfill this role, or would we have to import radio astronomy expertise?

The following only refers to the engineering design of the CRT. Fermilab has strong teams of RF engineers mainly in AD but also in CD and TD. Fermilab has also strong teams in digital signal processing, detector electronics and data acquisition. At a first glance the engineering specifications do not seem a major challenge. However, the engineering specifications need to be put in terms of electronic and mechanical requirements. It is relevant to understand noise and signal values, number of ADC bits needed, mixer S/N, linearity, DNL, LO leakage, phase stability, channel-to-channel uniformity and many other parameters. Competing for Fermilab engineering resources may not be easy, in particular for RF engineers. RF engineers are needed for every accelerator project. Upcoming reviews of this proposal will likely ask the managers for a plan to access engineering resources.

Findings

The project is largely enabled by low-cost amplifiers and digital processors.

R&D funds would be used to explore increasing the bandwidth and the speed of data transport and parallel processing.

The cylindrical telescope concept (drift scanning the sky) is efficient, and it allows a number of kinds of calibration drifts to be controlled.

The simulations of the foreground, and subtraction of the foreground to reveal the BAO signal, are impressive. While more work needs to be done to include various kinds of spurious signals, so far the studies of the efficacy of foreground subtraction are very positive.

Comments

The design of the production telescope needs to consider the reliability of operation in a remote environment.

The simulations need to be enhanced to include, for example, drifts in the calibration due to a number of possible effects.

The project has developed an appropriate and powerful set of tools to explore and compare the cost versus science reach of different design concepts.

Recommendations

For the external review, the calibration plan should be presented, as well as a discussion of RFI and other environmental and operational aspects that drive site selection.

Up to this point the M&S costs are in units that enable trades to be evaluated. Once a conceptual design is produced, the basis of the cost estimate should be presented more clearly, showing not just the M&S costs (and scaling) separately for the reflector, electronics, and feed lines, but also the other costs that DOE would normally expect to be listed.

Once a conceptual design exists, it will be natural to consider building a prototype of the telescope that would be capable of detecting the 21 cm signal (not the BAO signal) in the intensity map with the proposed foreground subtraction techniques.

The presentation of the simulations (e.g. for the external review) can be improved by removing slides with excessive numbers of formulae and excessive numbers of numbers. All plots should have readable labels on the axes. It would also help to present more information about what is known about the spectrum of the foreground, e.g. to justify the smooth (polynomial) sky model.

3. Collaboration and funding

a) Has a strong collaboration emerged, capable of mounting an experiment? What is the role of FNAL in this collaboration? Is there a project-oriented management structure being formed?

The collaboration has scientific and technical depth that is adequate to create a Conceptual Design Report (the current goal of the collaboration). The collaboration needs to grow in order to mount an experiment. FNAL is capable of contributing to any aspect of the project, but up to now has contributed mostly in the areas of site characterization, project management, and creating simulation tools. A management structure has been recently created that can evolve, but we were not able to assess this new structure in any depth.

b) Has sufficient progress been made towards a conceptual design that a target date can be identified? Is the proposed schedule and budget reasonable for completing R&D and moving forward with a project?

Very good progress has been made in addressing the critical question of whether the 21 cm BAO signal can be extracted from the much larger foreground. The simulations need to be extended in a number of directions, after which the optimization of cost and science reach (specifically Dark Energy figure of merit) can be explicitly laid out. Once this is done, the collaboration can decide on a conceptual design and proceed with a more detailed and credible cost estimate. These products are expected to be ready in time for the Fermilab review in September. At that point, a specific plan for the R&D can be derived.

c) What is the cost of such a project, and what are the planned funding sources? Is the cost estimate credible at this stage?

The 21 cm BAO CRT requires a very large reflector and array of detectors, but the basic technology is well understood, and parts can be manufactured economically. So far the collaboration has been studying the trade-offs between different possibilities in a comparative sense, where the cost of acquiring and assembling the reflector + electronics + feed lines is approximately $20M. Labor and operating costs are extra, as are the costs of site development, R&D for a prototype experiment, travel, etc. In summary, a cost estimate does not yet exist, but there is a plausible path to get there.

The funding plan is similarly in a very preliminary stage. Different scenarios were presented, but only to frame some ideas of the mix of federal agencies, private funds, and foreign investment. The level of FNAL participation would be proportional to the DOE contribution.

d) Has sufficient progress been made that this effort can go forward towards external review?

Yes, an external review will be helpful at this stage.

Findings

The collaboration currently consists of eight institutions in four countries.

There are no more than 4 FTE currently working on the project, and most of that effort is at Fermilab.

The focus of the collaboration now is on creating a Conceptual Design Report by the end of June 2010. A plan for R&D will evolve from that.

The project has recently organized itself to include a Chief Scientist, a Spokesman, an Instrument Scientist, and a Management Committee.

Spending to date has included travel for site testing and collaboration meetings. Scientist time is being invested at Fermilab, but there is no place to charge design effort by technical staff, or M&S.

The project does not yet have a funding plan. The strategy is first to create the Conceptual Design Report, have it reviewed internally and externally, present the project to the Fermilab PAC, and then develop a funding plan.

Comments

The collaboration will need to grow substantially before it is ready to build the telescope, in terms of number of involved individuals, their commitment to the project, and number of invested institutions.

To the extent that the postings to the docDB for 21 cm represent activity in the collaboration, we note a good level of activity, but apparently dominated by a small number of individuals.

At Fermilab there is strong interest by theorists and by experimentalists with background in relevant technologies.

Recommendations

Non-Fermilab members of the collaboration should be present at the upcoming external review.

It would be helpful to seek input from people with deep experience in radio astronomy. This could take the form of visits to Fermilab by radio astronomers thinking about epoch of reionization experiments, for example.

The project has put itself forward as an inexpensive approach to Dark Energy in terms of FoM gain per dollar. At the same time, the costs are not yet known (but we are confident that costs can be determined once there is a conceptual design). For the time being, it may be better to characterize the 21 cm CRT approach to Dark Energy as being independent of others and using well understood technologies.

Radio astronomy in the US has been funded almost exclusively by the NSF. This suggests that this part of the funding plan be thoroughly explored as soon as possible, and may be another good reason to involve senior radio astronomers.