The Fermilab Holometer

A Proposal Review for the Fermilab Particle Astrophysics Center

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EXECUTIVE SUMMARY

The Fermilab Holometer is an experimental proposal to look for quantum mechanical fluctuations in the structure of space-time. The experiment is motivated by theoretical speculations of Craig Hogan, the experimental spokesman, and hints of a signal in the GEO600 gravity wave detector. Our conclusion is that the experiment is well designed to see a holographic noise signal, if it exists, and to otherwise achieve a definitive null result.

Specific responses to the charge follow.

a) Is the science case strong and aligned with Fermilab and DOE OHEP goals?

The science objectives are specific and clear. A detection of a signal would be a unique window to the Planck scale although the absence of a signal would probably not drive either theoretical or experimental effort in new directions. The science objective falls squarely within the mission of Fermilab and the DOE OHEP.

b) Does the project have a reasonable chance to reach its science goals?

The experiment is well designed and has excellent prospects to reach its science goal. The committee has a number of concerns and suggestions that are outlined in the detailed commentary, but none of the concerns rises to the level that makes us think that the experiment is not feasible.

c) What resources (FTEs, M&S) will be needed for this project, and is there a high probability that these resources will be available from within Fermilab?

The scope and duration of the review was not adequate for us to make a comprehensive analysis. Nonetheless, it is the sense of the committee that both the integrated scientific effort and the level of technical support required may be greater than currently anticipated. The M&S costs are dominated by the cost of renting space for the apparatus and purchases from commercial vendors and the estimates appear to be robust.

d) What synergies or conflicts are there with the rest of the Fermilab program?

There is a large synergy with the effort to pursue axion search experiments like GammeV at Fermilab. Many of the team members, and some of the equipment, are common to those two efforts. Expertise with lasers and optics is potentially useful for other applications (like particle sources), but the connection is not as direct.

e) What resources will be available from outside Fermilab?

It appears that the only significant funding from outside Fermilab is from U. Chicago, via an FRA grant to Stephen Meyer and Aaron Chou. However, collaboration from experienced scientists at MIT (Weiss, Waldman), Michigan (Gustafson), Caltech (Whitcomb) and Chicago (Meyer) is a vital asset to the project.

f) Are there significant technical, management, or schedule risks? What is the plan for mitigating those risks?

The main technical risk seems to be that the team may find cross-correlations in the MHz range that they cannot understand or reduce, and thus might not be sensitive enough to the holographic noise they seek. Most of the other technical risks are really cost and schedule risks. It may take far longer than estimated to understand the apparatus at the level of detail needed, and they may find they need to add significant upgrades (like mirror suspension) to achieve the desired sensitivity.

SCOPE OF THE REVIEW

A review of the project was held on November 3, 2009. The review materials consisted of the Holometer experimental proposal and presentations by Craig Hogan, Rainer Weiss, and Chris Stoughton. The duration of the review was approximately 3 hours. Committee member Stebbins is an expert in theoretical cosmology and Grote and Gundlach are experts in the use of interferometers to make precise measurements of distances. Bauer is Deputy Head of the Particle Astrophysics Center and an expert in scientific management. The committee concentrated on the science case and the experimental design. The review of the schedule and budget was based on overall impressions and did not include a detailed line-by-line review.

COMMENTS ON THE THEORY

The Holometer Project proposes to look for a phenomenon that is a manifestation of the interplay of gravity and quantum mechanics on the structure of space-time. The apparent incompatibility of gravity and quantum mechanics is one the most important problems of theoretical physics and any experimental evidence for how the two coexist would be of profound importance; and certainly would fit into Fermilab's mission of the study of fundamental physics. It would in some sense be a probe of physics at the Planck energy scale!

There is a common belief that due to quantum mechanical gravity, whatever that is, space-time must be "noisy" not smooth, on small scales. There is no generally accepted formulation of how this works. The Holometer proposes to measure a macroscopic manifestation of this space-time noise, and if successful this would be a transformational discovery in physics. The sort of noise to which the Holometer is sensitive has not been excluded by other measurements. The holographic principle is a conjecture which hypothesizes that the number of degrees of freedom of space-time (along with all the fields it contains) increases like the area and not like the volume, and there are a variety theoretical reasons why this might be the case. This limitation on the number of degrees of freedom results in space-time being noisier than one might naively expect from Planck scale fluctuations. The major proponents of this holographic conjecture (*e.g.*, Susskind, t'Hooft) have not brought their theory to the level where they can make predictions for macroscopic experiments like the Holometer. Hogan has developed a phenomenological theory for holographic noise, which can be applied to human scale interferometers such as the Holometer. While the phenomenological theory supposedly has no free parameters, it is clearly in a state of development (for example the dependence of holographic noise amplitude with the geometrical configuration of the interferometers is not yet known). The phenomenological model is not really based on any fundamental theory, although it is normalized by theoretical calculations of black hole entropy. It is not clear how accurate this normalization might be. Given the uncertain basis for the phenomenological model we feel it important that the Holometer not only be able to detect the holographic noise with high significance but also be able characterize its amplitude and spectral shape. Generally we feel the volume in the space of all possible models of "space-time noise" which would be tested by this experiment is small (a subset of holographic models which are themselves a subset of all models), although a positive detection would be ground-breaking. Given the relatively low cost of the Holometer the scientific case for doing this experiment is fairly good—low cost for a relatively uninteresting null result—but huge benefit for a positive detection.

Another motivation for the Holometer is that holographic noise already may have been detected by the GEO600 gravitational wave detector. The basis for this speculation is an unpublished noise spectrum that shows an excess noise compared to the sum of known sources. The excess noise is referred to as "mystery" noise and, at this point, it is pure speculation that the excess might be due to holographic noise.

During discussions between the committee members we learned that the GEO600 detector group is engaged in a program to increase the sensitivity of the detector over the next 3 years. GEO experimentalists consider it likely that GEO will have reached or surpassed the sensitivity to observe or rule out the predicted level holographic noise some time in 2010. A null result could be considered robust since the observed noise would be less than the predicted signal. A positive result would be less robust because it would be difficult to differentiate a signal for holographic noise from some conventional, but unidentified, "mystery" noise. The Fermilab Holometer proposal is much more powerful in its ability to differentiate holographic noise from conventional sources, but this advantage is important only if a signal is observed.

Clearly it will be necessary for the Fermilab Holometer experimental team to monitor progress at GEO600 and adjust its strategy to meet new developments.

EXPERIMENTAL TECHNIQUES

The team understands the known technical challenges quite well, with plenty of expertise stemming from LIGO and GEO. Besides this winning aspect of having assembled a highly qualified team, the instrument has two major intrinsic advantages over existing gravity detectors for detecting the holographic noise. It uses the fact that the noise in co-located detectors must be correlated between the two, while the correlation should disappear when the detectors are spatially separated by more than their size. Secondly the detector uses the white noise nature of the holographic noise and hence the fact that it is detectable at frequencies much above where seismic, acoustic or mechanical noises are dominant.

There has been a rather complete analysis of the noise levels in the interferometers by highly experienced individuals and the overall design seems very sound. However, we would comment

1) Alignment issues have only been taken into account very recently at a very elementary stage. At the requirement level of 1μrad (as proposed by Rai), an automatic alignment system is mandatory. All ambitious interferometer projects in the past have under-estimated the importance of alignment issues, mainly because it's a second-order effect on paper (meaning that small misalignments do not couple to sensitivity). However, it's essential to find out for a given experiment, what "small" means. Adding automatic alignment to the experiment needs simulation, and adds another layer of complexity to the experiment.
2) There is virtually no experience about any interferometer sensitivity in the MHz regime, with rigidly mounted optics. That's simply risky. Adding suspensions at a later stage is possible, but will be very time-consuming, and add another layer of complexity.
3) Higher order modes (at the anti-symmetric port) were dealt with only at the level of alignment of the beams. However, there are more higher order modes to expect at the anti-sym. port: if the radii of curvature of the end mirrors are not matched sufficiently, the (useless) light power at the anti-sym. port will increase. This can corrupt sensitivity and should be simulated to at least specify the mirrors properly. Having only a few mW of higher-order mode light at the anti-symm. port (as in the current proposal) seems to be an optimistic design, given that there is no consideration of thermal effects written up.
4) Pre-isolation tables are mentioned in the proposal, but not linked to seismic noise measurements. Both should be discussed together.

The proposed experiment is extremely powerful in it's use of a cross-correlation technique and operation at much higher (MHz) frequencies. These differences will put the operation of the interferometers into an unfamiliar regime. One particular concern is stochastic r.f. noise that would be picked up in both detectors and produce a correlated response. There are myriad possibilities for injecting common signals into the nominally independent interferometers: r.f. pickup, inductive coupling, currents in the grounding system, modulation of the electro-optical devices, and so on. Separating the detectors would help to characterize such common noise but the noise could have many different behaviors with the variation of interferometer separation.

There didn't appear to be any formal analysis of the expected level of correlated noise and the committee was not presented with any details of the design of the electronics and shielding so we are not in a position to judge whether there might be an issue in this area. However, we would suggest that more attention be given to analyzing the possibilities and to perform early tests on the electronics with an eye towards measuring the level of correlated noise.

Another component that should be tested early on is the piezo actuators of the end mirrors to verify that these move parallel to the optical beam.

MANAGEMENT AND SCHEDULE

The team is not organized like a project and seems to operate by consensus. They will need to adopt a somewhat stricter approach to cost/schedule before receiving DOE approval to proceed. However, since this is likely to remain below the $2M project threshold, it will likely never require the project apparatus needed for larger experiments.

The group appears to be woefully short of technical labor and seem to be seriously underestimating the need here. This will be a continuing problem for them, given the overall lab shortage of engineering and technicians. It will put the burden on the scientists to do much of the technical work, although the team actually seems to relish that. However, the lack of engineering support will significantly slow the schedule in areas of safety, documentation and operational readiness.

The team seems to have a reasonably good handle on the M&S costs to do the experiment within the 50% contingency level they are currently quoting.

The schedule for bringing the Holometer into operation is probably underestimated. The construction time is likely underestimated and the commissioning time will probably be longer since it must include debugging, i.e. unexpected problems may be encountered and must be understood and solved. The committee speculates that bringing the apparatus to the point of design sensitivity with the proposed level of scientific effort (4-6 FTE) may require a two-year period of commissioning.