Department of Energy,
Division of High Energy Physics

Report on the DOE Review of the SNAP Experiment

Lawrence Berkeley National Laboratory
January 25-27, 2001

EXECUTIVE SUMMARY

On January 25-27, 2001, the Division of High Energy Physics of the Department of Energy (DOE) conducted an R&D review of the proposed SuperNova/Acceleration Probe (SNAP) experiment at Lawrence Berkeley National Laboratory (LBNL). The purpose of the review was to evaluate the R&D status and concept of SNAP in its current pre-conceptual planning phase. The Charge to the Committee is shown in Appendix A. The Agenda for the review is shown in Appendix C.

The review Committee was chaired by Kathleen Turner from DOE and included sixteen scientific and engineering experts from the fields of High Energy Physics, Astrophysics, and Astronomy. The Committee membership is shown in Appendix B. Observers were in attendance from three funding agencies: DOE, NASA and NSF. The expert reviewers on the Committee provided comments during the review which form the basis for this report.

Recent data by two scientific groups (the Supernova Cosmology Project and the High-z Supernova Search) using measurements of Type Ia supernovae (SNe Ia) produced significant evidence that there is an acceleration in the expansion rate of the universe. This was the first direct experimental evidence for an accelerating universe, driven by an unknown energy (termed "dark energy") that permeates all of space. To explain this acceleration, no current model including Einstein's cosmological constant fits naturally with the field of High Energy Physics' current understanding of the behavior of matter and energy at the most fundamental level. As the dominating energy in the universe, this dark energy would be of utmost importance to our understanding of the physical laws of the universe.

Members of the Supernova Cosmology Project (SCP), centered at LBNL, have initiated a collaboration which has proposed SNAP as a dedicated follow-on experiment to verify and further explore these measurements. The SNAP satellite is designed to discover and precisely measure properties of thousands of SNe Ia per year. From the data collected, it will be possible to measure accurately the history of the universe, including any accelerations or decelerations. This history is the only known indicator of the nature of the dark energy. The instrument consists of a 2m wide-field telescope designed to be launched into high-earth orbit. Features include a billion pixel charge-coupled device (CCD) optical camera (Gigacam), an infrared (IR) camera using HgCdTe technology, and a spectroscope which will provide follow-on measurements of the discovered SNe Ia.

The focus of the Review was on the R&D progress to date and that planned and required for the future. Special concentration was given to the technically challenging parts of the instrument. The scientific goals of the experiment were deemed extremely important by the SAGENAP panel in February 2000. The case for project justification in terms of techniques and concept is still under review. The

Committee was asked to review progress in preparation for establishing the CD-0 ("Mission Need") level of project approval, given by DOE at the end of the pre-conceptual planning phase.

This Review marked the first time that a broad, diversified group of physicists, astronomers and astrophysicists were assembled to review SNAP, including its concept and R&D that would lead to a Conceptual Design Report and Review. Despite a difference in approach and scientific culture, this review group functioned very well together. It is significant that observers from three agencies were in attendance. There was intense interest shown by committee members in the proceedings.

The Committee agreed and emphasized that SNAP is a science-driven project with compelling scientific goals. In addition to verification of the acceleration of the universe by measuring the dark energy and equation of state of the universe, SNAP will have the unique ability to measure its variation with redshift thereby allowing exploration of inhomogeneities in the dark energy density. The nature of the acceleration of the universe will have fundamental implications for High Energy Physics. SNAP measurements, including detailed studies of SNe Ia, will also be of fundamental importance to Astronomy. The Committee noted that it is likely that cosmology experiments such as SNAP will continue to grow to have a centrally important impact on the nature of the behavior of matter and energy at its most fundamental level, which is traditionally studied by the field of High Energy Physics.

Much preliminary work regarding estimation and requirements of systematic uncertainties and errors was presented by the SNAP team during the review. The systematic errors attainable by SNAP were of concern to the Committee. The current preliminary assessments are based on assumed isotropy and homogeneity of the Universe. In obtaining the estimates, nearby SNe were assumed to map all of the necessary parameter space in terms of age and metallicity. There are systematic uncertainties due to fundamental calibrations as well as those due to the experiment’s internal instruments and sampling.

The planned instruments on SNAP require leading-edge technology. The instruments used for IR and optical photometry measurements and spectroscopic measurements using these technologies were described in detail during the review. The Committee was concerned with the relative emphasis of IR versus optical photometry and spectroscopy planned by SNAP and whether these were distributed optimally in order to attain the desired measurements.

The Committee felt that further simulations and studies at this stage of the project are needed in order to verify and justify the observational and systematic requirements of the experiment. These studies should include the generation of an “error budget” which describes how each measurement affects another. Recommendations for specific studies and simulations are made in the report.

The SNAP team described the necessity of a dedicated space-based instrument in order to obtain all the SNe Ia measurements to the necessary accuracy and redshift (z) range. It would take a dedicated 8-m class ground telescope several years to track all the SNe Ia and do the follow-on spectroscopic measurements. This would only work for the low-z region. Matching low-z and high-z measurements obtained with different instruments would degrade the systematic error accuracy that SNAP plans to attain.

Some members of the Committee were not convinced that a space-based facility was needed for all of these dedicated measurements. However, most members agreed that some parts of the planned measurements (the SNe Ia measurements at high-z, i.e. at z greater than 1) could NOT be done from the ground. The Committee recommended that the SNAP team look into the possibility of coordination between existing and planned ground-based telescopes, planned future spaced-based telescopes such as the Next Generation Space Telescope (NGST), and a mission such as SNAP. Perhaps existing or future ground-based facilities could provide the SNe Ia discoveries in the lower-z region as well as initial photometric measurements. Follow-on measurements and spectroscopy of these SNe Ia could then be done with other dedicated telescopes.

The SNAP team has presented the experimental plans and received enthusiastic support by much of the High Energy Physics community. However, the relationship with the astrophysics and astronomy communities is less well developed. The Committee recommended that SNAP strengthen their collaboration by actively pursuing coordination and collaboration with these communities. SNAP was not included in the National Academy of Sciences Decadal Survey whose plan is followed closely by NASA and NSF. These relationships need to be explored and developed. The possible scenarios for funding the instruments, spacecraft and launch needs to be worked out. The Committee also recommended that the coordination by the funding agencies should begin to be developed as soon as possible at the agency and scientist levels.

The conclusion of the full Committee is that more work is needed before recommending that SNAP proceed to the establishment of Mission Need (CD-0), which marks the start of work on Conceptual Design. Specific recommendations were made by the Committee regarding studies that should be done before the next review, scheduled to take place before FY 2002, and are detailed in this report. The Committee recommends that DOE encourage and support substantial simulation and trade studies in this period. The full Committee believes that the SNAP science goals are excellent and address fundamental questions in particle physics and cosmology, confirming the conclusions of earlier reviews. The goals of the SNAP experiment justify significant costs and efforts associated with the project.

1 Introduction

On January 25-27, 2001, the Division of High Energy Physics of the Department of Energy (DOE) conducted an R&D review of the proposed SuperNova/Acceleration Probe (SNAP) experiment at Lawrence Berkeley National Laboratory (LBNL). The experiment is currently in a pre-conceptual planning phase. The focus of the Review was on the R&D progress to date and that planned and required for the future. The case for project justification in terms of techniques and concept was also a large part of the review.

The project began its pre-conceptual planning phase in 1999. Since then, it has been supported significantly both by R&D funds from DOE and LDRD (Laboratory Directed Research and Development) funds from LBNL.

An initial review of SNAP was held by the SAGENAP panel in February 2000. At this review, the scientific goals of the experiment were deemed extremely important. A further, in-depth review was called for by the panel to focus on the R&D of the experiment and the science issues and concept. This recommendation formed the basis for the current review.

The Committee was asked to review progress in preparation for establishing CD-0 ("Mission Need") level of project approval, given by DOE at the end of the pre-conceptual planning phase. The review Committee was chaired by Kathleen Turner (DOE) and included sixteen scientific and engineering experts from the fields of High Energy Physics, Astrophysics and Astronomy:

Mr. William Althouse	SLAC
Prof. Charles Baltay	Yale
Dr. Marty Breidenbach	SLAC
Dr. Marcel Demarteau	FNAL
Prof. Sandra Faber	UC Santa Cruz
Dr. Tom Greene	Nasa - AMES
Dr. Matt Greenhouse	Nasa - GSFC
Prof. John Huchra	Harvard
Prof. Robert Johnson	UC-Santa Cruz
Dr. Steve Kent	FNAL
Prof. Gerry Luppino	U. Hawaii
Prof. Joel Primack	UC Santa Cruz
Dr. Abhi Saha	NOAO
Prof. Glenn Starkman	Case Western
Prof. Alex Szalay	Johns Hopkins
Prof. J. Craig Wheeler	UT-Austin

The Committee reviewed the detailed presentations made by the collaboration members on the science and technical aspects of the experiment. They provided recommendations to the SNAP collaboration and to the agencies during the closeout of the review. Their evaluations in terms of findings, comments and recommendations are contained in this report. Invaluable local support in organizing and running the review and for assistance to the Committee and agencies was provided by Brionna Johnson (LBNL).

Observers were in attendance from three funding agencies: DOE (Richard Nolan, Steve Tkaczyk, Timothy Toohig, P.K. Williams), NASA (Guy Stringfellow) and NSF (Gene Loh). They participated in the discussions and executive sessions and provided considerable assistance.

This report begins with an overview of the SNAP project. The sections following this consist of the written contributions from the outside Committee members. In the first sections, the scientific issues from the perspective of theory, particle physics and astronomy are discussed. These are followed by sections on each major instrument or subsystem in the experiment (Gigacam, spectrograph and near-IR camera, telescope, spacecraft and computing and data handling). Then come sections on each type of observation planned by the experiment (optical, infrared, and spectroscopic) which are followed by a section on cost, schedule, and funding. Lastly, the overall views of the Committee and action items proposed are given in the project management section. There are three appendices at the end of the report, which contain the Charge to the Committee, the agenda for the review, and the membership of the Committee.

2 Project Overview

The Supernova Cosmology Project (SCP), centered at LBNL and led by Saul Perlmutter, was one of two scientific collaborations (the SCP and the High-z Supernova Search) that recently published data using measurements of Type 1a supernovae (SNe Ia). Supernovae provide a direct and the least model dependent approach of producing a redshift versus magnitude plot (Hubble diagram) indicating the history of the expansion of the universe. These data were the first direct experimental evidence for an accelerating universe, driven by an unknown energy (termed “dark energy”) that permeates all of space. This conclusion is greatly substantiated by the current measurements of the mass density of the universe when taken together with the recently measured approximately 1° scale fluctuations of the CMB (cosmic microwave background). To explain this acceleration, no current model including Einstein’s cosmological constant fits naturally with the field of High Energy Physics’ current understanding of the behavior of matter and energy at the most fundamental level. As the dominating energy in the universe, this dark energy would be of utmost importance to our understanding of the physical laws of the universe.

In order to investigate the nature of the dark energy and definitively rule out other explanations such as evolution or grey dust for the recent supernovae results, the space-based SNAP experiment has been proposed by members of a collaboration initiated by the SCP team. The SNAP satellite is designed to discover and measure precisely the properties of thousands of SNe Ia per year with a redshift (z) range from 0.1 to 1.7. These data would increase the current published sample of SNe Ia by about two orders of magnitude and extend the sample much farther in redshift. Of key importance to the results, tight constraints would be placed on the systematic errors.

From the data collected, it will be possible to determine accurately the history of the universe, including any accelerations or decelerations. This history is the only known indicator of the nature of the dark energy. The data will provide experimental measurements of fundamental cosmological parameters. The measurements will be the first constraints available on possible particle physics models of the dark energy, allowing elimination of some models and providing an allowed range on others that are currently unconstrained. A strong constraint can be placed on the models by studying the pressure to density ratio (the equation of state, w) in the universe, and its evolution with redshift, $w_1 = dw/dz$, both of which are goals of the SNAP experiment.

While providing the first example of precision cosmology measurements by directly addressing the nature of the dark energy, the SNAP satellite would also complement the orthogonal results of the proposed CMB experiments to improve measurements of the cosmic microwave background.

The proposed instrumentation consists of a 2m wide-field telescope designed to be launched into high-earth orbit. Features include a billion pixel charge-coupled device (CCD) optical camera (Gigacam), an infrared (IR) camera using HgCdTe technology, and a spectroscope to provide follow-on measurements of the discovered SNe Ia.

3 Theory and Scientific Issues

Findings

1. SNAP is a science-driven project with compelling scientific goals.

2. There is a persuasive case for the existence of the dark energy that SNAP is trying to probe.

3. The preliminary assessment of SNAP's systematic sensitivities is based on current state-of-the-art spherically symmetric supernova simulations.

4. Thorough sampling of supernova properties, photometry and spectroscopy over the complete range of target redshifts is justified.

5. The current assumption is that the nearby supernovae map all of the necessary parameter space of, e.g., progenitor age and metallicity.

6. The SNAP proposal sets a goal of 2% error over $0.1 < z < 1.7$.

7. While SNAP may be able to obtain a measurement of Ω_{DE} (the dark energy density) and *w* that is independent of and better than those obtained from a combination of other experiments, nevertheless measuring the value of *w* at low redshift should not be viewed as the mission's primary science goal.

8. SNAP will have a unique ability to measure the variation in *w* with redshift, in particular the parameter w_1, where $w = w_0 + w_1 z + O(z^2)$

9. Inhomogeneities in the dark energy density could have arisen in many ways and those alternatives could be explored by SNAP.

10. SNAP wide field optical data could be a unique data set for weak lensing measurements, which could significantly extend SNAP's ability to constrain the cosmological parameters.

Comments

1. One line of argument for the existence of dark energy is based on the existing SNe Ia data, as summarized in the SNAP proposal. The other combines the evidence that $\Omega_m + \Omega_\Lambda = 1$ (where Ω is the density), primarily from the location of the first acoustic peak in the CMB anisotropy, with the very strong evidence that $\Omega_m \approx 0.3$ from several different directions, for example (a) clusters and (b) the power spectrum P(k). Two different sorts of cluster data each provide clear evidence that $\Omega_m \approx 0.3$: (a.1) the falloff of the cluster abundance with increasing redshift is consistent with this (and much slower than predicted in a universe with $\Omega_m = 1$); and (a.2) $\Omega_m = \Omega_b/f_b$, where the baryon density Ω_b is determined from the deuterium abundance, and the same value of the baryon fraction in clusters f_b is determined independently by different methods. Two different sorts of data on the power spectrum P(k) also independently imply that $\Omega_m \approx 0.3$: (b.1) the shape of P(k) at low redshift; and (b.2) the evolution of its amplitude up to redshifts of about 3, where it is measured from Lyman alpha forest data. EACH of these four separate arguments imply that the matter density is less than unity by at least 3σ. It follows that there must be a large amount of dark energy.

2. Very little supernova theory has been required for current results of the Supernovae Cosmology Project. However, investment in theory and ancillary supernova observations is necessary in going forward to define and refine expected systematics. For example, the state of the art of the study of the progenitor evolution and three dimensional radiation hydrodynamic models will continue to advance. Accelerating the rate of advancement as part of the SNAP proposal, will improve pre-launch understanding of systematics.

3. The specific choice of the redshift range has not been adequately studied and justified.

4. The DEEP survey may be able to see the first indications of the z-dependence of *w* if it is strong, but the only observational program currently being considered which is likely to yield a value for w_1 or constrain it meaningfully is SNAP. With the values of Ω_m and Ω_{DE} determined from combinations of other experiments (SDSS, MAP, …), SNAP seems likely to fit w_1 to better than 0.1, and begin to offer some constraints on higher derivatives of *w*. Unless future measurements show that the current case for dark energy of some sort is incorrect, then, no matter what values of Ω_{DE} and *w* may be extracted from CMB, large scale structure, and weak lensing measurements, if SNAP does not proceed, its determination of w_1 will be sorely missed.

5. Current planning is based on assumed isotropy and homogeneity of the Universe. SNAP has another scientific opportunity, which its current observing strategy short changes. While inflation may cause the homogenization of the universe at high redshift, if the dark energy is due to the potential energy of some field which is not at its minimum, then the homogeneity of that field, and hence of the dark energy density, is not assured. By distributing the target fields more widely on the sky, SNAP could readily test the isotropy, and by implication the homogeneity of the dark energy.

<u>Recommendations</u>

1. Research and development of SNAP should continue.

2. Future work must involve a close coupling of ground-based supernovae observations (e.g.. the Supernova Factory) and theoretical modeling to refine the systematics.

3. The optimum strategy versus cost of the redshift range to be studied requires further study.

4. The committee recommends that the SNAP collaboration perform a trade study which compares the cost and complexity of a mission which can access supernovae over more of the sky (over the duration of the mission) than the current baseline mission, which searches only near the ecliptic poles. It is also the sense of this committee that a more isotropic observing strategy would have considerable benefits for the ancillary science.

5. A cost/benefit analysis of weak lensing is needed, as well as comparisons to other possible ancillary science.

4 High Energy Physics Perspective

Findings

Results from type Ia Supernovae (SNe Ia) have provided persuasive indications that the expansion rate of the universe is accelerating, rather than slowing down as might be expected from a matter dominated universe with attractive gravity. One of the goals of the SNAP project is to collect a large sample of SNe Ia's with carefully controlled systematics so that, combined with other cosmological probes, such as CMB measurements, gravitational lensing, galaxy cluster measurements, etc, the confidence that the universe is accelerating can be increased sufficiently to reach a compelling conclusion. A fairly precise measurement of Ω_m and Ω_k will be a likely result of the non-SNAP measurements. In that case SNAP will be in a position to make the most sensitive measurement of the equation of state parameter w and its evolution with redshift $(w = w_0 + w_1 z)$.

Comments

The results on the nature of the increase in acceleration of the universe will have very important implications for Particle Physics. We believe that it is not an overstatement to say that the SNe Ia measurements will uniquely address issues at the very heart of the field in a number of ways. In an accelerating universe, w has to be less than –1/3. If $w = -1$ exactly, the acceleration can be said to be due to a small but non-zero value of the cosmological constant, Ω, or a homogenous false vacuum energy. For other values of w, we are faced with a completely new form of energy density, which has been dubbed "Dark Energy". A measurement of w by SNAP (and possibly others) will distinguish between these two possibilities.

1. If the acceleration is caused by Ω, then particle physics is faced with a severe dilemma: naïve expectations regarding the vacuum quantum fluctuations imply a cosmological constant term in Einstein's equations of general relativity with a value ~10^{120} times larger than the value of Ω implied from the cosmological measurements. This disagreement has profound implications for particle physics – some have called it the most significant problem known. The understanding of this problem is at the heart of any attempts at unified theories which involve a quantum theory of gravity. There is some feeling that an exactly zero value of Ω could be explained by some as yet undiscovered symmetry law in nature. A small but non-zero value is very difficult to reconcile with any attempts at a fundamental theory such as string theory.

2. If w turns out to be negative but not –1, then particle physics is faced with a completely new form of energy density, the Dark Energy, with negative pressure and therefore large scale repulsion. There is nothing in the Particle Data Book that would do this. Neither can Dark Energy likely be explained by

other fashionable hypothesized, but in this context garden variety, forms of matter, like the Higgs boson, Supersymmetric or Technicolor particles, or the contemplated candidates for cold or hot Dark Matter. Thus, the existence of this new Dark Energy would have immense implications for Particle Physics.

3. The measurement of the value of w would clearly be of fundamental importance in starting to understand what this new form of energy is. Models have already been proposed (dynamical scalar fields, including “quintessence”, tracker fields, etc.) which can be distinguished by the redshift evolution of the equation of state parameter, $w = w_0 + w_1 z$. SNAP promises to have a special capability of measuring w_0 and w_1, given a value of Ω_m and Ω_k. This will clearly be of fundamental importance for particle physics, especially since there are no ideas at this time to investigate Dark Energy employing an accelerator based approach. It is thus very likely that experimental cosmology will have a centrally important impact on progress in these fundamental issues in particle physics.

Recommendations

It is appropriate for particle physics in DOE and NSF to help initiate and realize the SNAP project.

5 Astronomy and General Design Issues

Findings

1. The problem of verifying the acceleration of the universe and quantifying the dark energy content and its equation of state is an important one for astronomy. The high redshift SNe Hubble diagram is currently the most promising technique for addressing this problem. The greatest leverage on the problem comes from the high redshift ($z > 0.5$) supernovae.

2. Systematics dominate the uncertainty. These include, but are not limited to, photometric calibration and sample selection, and possible variations in the extinction law as a function of redshift.

3. A key issue in determining the justification for SNAP is the comparison of the capabilities of SNAP as proposed with the combined capabilities of other existing or planned facilities. This bears directly on the instrumentation required to accomplish the scientific goals, i.e. the global design of the experiment, and the necessity of the satellite itself. Part of this issue is to convicingly demonstrate that ground-based calibrated samples of supernovae cannot be obtained and transferred over the wavelength range required.

4. Various systems appear not to have been studied in sufficient depth, even for this preliminary review, such as the data processing pipeline, parts of the instrument design such as shutters and filters, the actual capabilities of the IR spectrograph on such a small telescope, and the IR camera configuration.

Comments

1. A significant systematic issue that was not addressed is the “absolute relative” problem, that is the fact that the existing calibration of the fundamental photometric and spectrophotometric system may only be good at the 3-5% level over the wavelength range of interest --- that is, the current fundamental calibration in the IR is based on model atmospheres and extrapolation from the fundamental optical calibration. This means that no matter how well the project can internally calibrate their instrument(s), the limiting accuracy of the project may be determined by an effect not directly addressed by the project in the current preliminary proposal.

2. Many of the comparisons with other facilities used parameters that do not represent the current or soon to be state of the art, examples being the wide field near-IR camera on the 8-m Subaru telescope which is achieving 0.2 to 0.3 degree FWHM images in the near-IR.

3. The committee felt that many of the system and observational requirements were not sufficiently justified. There was no overall error budget. The difficulty of obtaining high precision wide-field photometry from undersampled data was not fully appreciated. Contamination of both photometry and spectroscopy by the underlying galaxy light has not been considered. Are 1% photometric and systematic errors required and, if so, does the system error budget provide that level?

4. Given the possible unique capabilities of SNAP for the near-IR imaging necessary to study the higher redshift SNe, it was unclear why so much of the planned near-term development effort was concentrated on the optical camera. Conversely, the multiplicity of possible IR imaging alternatives was confusing.

Recommendations

1. The committee feels that considerably greater effort needs to be put into simulations and trade studies. Issues that need to be studied in more detail include the need for an optical spectrograph on SNAP given the availability of large ground-based telescopes and spectrographs, and the possibilty of ground-based discovery work using wide-field near-IR cameras on modern telescopes with excellent imaging (e.g. Subaru or the MMT operating near 1 micron) and even the use of the IR channel on WFPC3 to follow up a few hundred SNe at high redshift. Scientifically, it is necessary to address the question of how much of the proposed program will or could be carried out using existing or soon to exist

facilities such as Keck, Subaru, Gemini, HST+ACS or WFPC3, and PRIME before the probable launch of SNAP. We note that convenience is generally not a sufficient justification for a space mission or an instrument on a space mission.

2. The proposed suite of instruments should be prioritized with respect to the main scientific goals of the program. The committee's rough ranking based on information provided, especially the need to study well high-redshift SNe, placed the wide-field IR imaging first, near-IR spectroscopy second, with the caveat that it was unclear that with real assumptions about detector and spectrograph throughputs a 2-m telescope was large enough to provide adequate signal-to-noise (S/N) for such spectroscopy, the wide-field optical imager third and optical spectroscopy last. Will it be necessary to wait for NGST to obtain sufficiently high S/N spectroscopy of the SNe at $z \geq 1.5$? Should SNAP be phased with NGST?

3. The project should examine the feasibility of achieving the majority of the scientific goals with a descoped instrument complement, for example, just a wide-field IR camera using the HgCdTe technology with the detector response stretched down to 6000Å on SNAP combined with optical groundbased imaging and spectroscopy of the lower redshift SNe and NGST NIR spectroscopy of the higher redshift SNe. Note that such extended range HgCdTe detectors would still allow SNAP SNe studies in rest B-band over the redshift range 0.5-2.0.

4. By shifting the bulk of the lower redshift studies off to ground based telescopes, the mission would allow for increased attention to the higher redshift sample.

5. Simulations and trade studies should also be expanded to study the effects of increasing the spatial resolution both for the imaging (to perhaps improve photometric precision by properly sampling the point-spread function) and for spectroscopy (to study improved background suppression, including that due to the host galaxy).

6. The issue of "absolute relative" calibration should be addressed either by showing more convincingly that it is not a problem or by developing a calibration plan to deal with it.

7. It was clear to the committee that the basic SNAP concept, wide-field imaging to provide sufficient statistics as well as data of sufficient quality on high-redshift SNe, is a good idea. This is especially true for a space-based near-IR imager where substantial gains are to be achieved due to the considerably lower backgrounds.

8. The SNAP dataset will be useful for a host of other astrophysical studies. A list of these should be assembled to aid in justifying the project and to examine any special data taking, processing or archiving requirements that might also minimally drive the project design.

Issues 1-3 and 6 above need to be addressed before we can recommend that the project proceed to CD-0.

As an example of addressing issues 1 and 2, the team might construct a matrix of instruments (both different parts of SNAP and other current or planned telescopes and instruments such as Keck, Subaru, the IR channel of WFPC3 on HST, NGST, LSST, or combinations of SNAP and these instruments) versus measurements needed by SNAP (mid-z and high-z discovery and followup) and describe the relative merits of each. In such a matrix, for example, optical spectroscopy with 8m class telescopes would have high weight or import for the science goals of classifying SNe suspects and determining their physical properties. The time it would take to obtain the measurements should be folded in. This "budget" table could take the form shown below as an example. The uniqueness at each x,y point could be given as "high", "moderate", or "low" or could be made quantitative which has a metric folded in of how fast each instrument could make the measurement.

	High-z SNe		mid-Z SNe	
	Discovery	Followup	Discovery	Followup
SNAP				
IR Camera				
IR Spectrograph				
Gigacam				
Optical spectrograph				
NGST IR spec				
KECK opt spec				
HST WF3				

6 GIGACAM

The large Gigacam optical CCD camera envisioned for SNAP is an ambitious but straightforward instrument. In the section below, the Gigacam instrument is broken into 3 sections: a) the CCDs and focalplane, b) the control and readout electronics, and c) the filter and shutter mechanisms.

6a CCDs and Mosaic Focal Plane

Findings

1. LBNL is commended for developing the p-channel, deep-depletion CCDs with high QE, high radiation hardness and minimal NIR fringing. These devices offer important advantages for this project as compared to other conventional CCDs.

2. The CCD technology appears viable and reasonably mature. Major issues involve the technology transfer to a commercial foundry. This is underway.

3. 4-edge buttable packaging design presented looks very good. No particularly difficult problems have been identified.

4. Focalplane design is not considerably different from other existing mosaic designs. This technology has been demonstrated elsewhere and merely needs to be shown to be flight qualified.

Comments

1. The LBNL group made such a good case for the quality of the detectors that it appears a significant technology demonstration effort at the level proposed is not required at conceptual design.

2. The project must continue technology transfer to the commercial foundry, and must also identify a second source for the flight detectors should the foundry efforts fail. If the lab is considered to be the second source, it should be shown how the laboratory plans to produce the required number of devices.

3. Crosstalk is an important consideration. It will be impossible to calibrate channel-to-channel crosstalk if a saturated star on one part of an imager produces a signature on another channel. This will profoundly affect their ability to carry out the precision photometry they need.

Recommendations

1. The technology of the CCD and focal plane development appears to be sufficiently mature that a significant technology demonstration effort is not considered to be of the highest priority (as compared to other technical areas addressed in other sections) at this conceptual design phase. Instead, the panel felt that other aspects of the instrument need more careful attention than they have received so far.

2. It would seem prudent, if not essential, that the project endeavour to obtain real astronomical data with the proposed CCDs. Experience has shown that many subtleties in detector performance may not be apparent in laboratory test data, but show up only when some poor graduate student or postdoc is beating his/her head against the wall trying to understand some faint spectrum or image. In the spirit of controlling systematics by understanding anything and everything about the detector performance, we would encourage the project to obtain ground based astronomical data with not only the existing LBL CCDs, but also (and this is crucial) with the new ones fabricated at the foundry. The efforts so far do not even approach what should be done. Certainly the device planned for Keck/ESI should be delivered asap, and the performance of this device should be monitored and scrutinized for subtle problems. If possible, devices should be

distributed to other observatories as well with the intention of understanding these devices.

Item 2 above needs to have its planning started before going to CD-0.

6b Control and Readout Electronics

Findings

1. The layout of the flight detector control electronics is at a very preliminary stage.

2. Although complex, the design will rely heavily on ASICs and ASIC expertise within the laboratory and collaboration.

Comments

1. System design must be developed early on so that ASICs can be specified and so that ASIC design and production does not drive the schedule.

2. Elimination or minimization of crosstalk MUST be considered at all levels of the electronics from the devices, to the ASICs, to the cabling, etc.

3. ASICs of the complexity needed for elements of the instrumentation electronics can be developed within the 2-year schedule described, as long as the precise requirements and interfaces can be derived sufficiently early in the system design. However, there are come concerns that should be addressed or watched carefully.

4. Engineering Manpower: It is difficult to hire and retain qualified IC designers at universities and laboratories, and often the available designers are (over) subscribed by other projects. The ASIC development effort described by the SNAP team will be involved with three different IC processes (SOI BiCMOS, high-voltage, and some other rad-hard CMOS process). The more specialized processes may not be familiar to many designers, and doing the design work in multiple processes puts additional demands on manpower and requires more work for space qualification. Therefore the manpower demands and availability should be carefully considered and watched, and the number of different processes used should be no larger than necessary.

5. ASIC Space Qualification: A plan should be made for space qualification of the ASICs. Total dose does not appear to be a problem for the processes of interest but should still be tested for the particular designs. However, single-event latchup testing (for the non-SOI processes) and single-event upset testing should also be planned and kept in mind during the design phase.

6. ASIC Process choice: There is some concern about the reliability, long turn-around, and potential longevity of the very specialized DMILL process currently being used by the development effort. The commercial Peregrine SOI process could make an excellent backup so long as the design is kept compatible with both processes.

Recommendations

1. The system design is at the level expected for a pre-CD-0 phase. The comments above should be considered to be warnings and guidelines.

2. Eventually, the project should endeavour to take astronomical data with the proposed CCDs in a readout configuration as close as possible to the final SNAP configuration to address issues with regard to crosstalk and other potential problems.

6c Filter and Shutter Mechanisms

Findings

1. The project showed little in the way of various mechanisms. A simple filter wheel design was shown but it was pointed out that various other schemes were under consideration.

2. The shutter was not considered.

Comments

1. The shutter and filter wheel are single point failure areas.

2. Can the instrument work shutterless?

3. The relationship of the shutter design and performance with regard to precision photometric standard star calibration needs to be carefully considered.

Recommendations

1. Any moving parts on a satellite need to be carefully thought out. While much of this work will clearly take place as part of CD0, the lack of concern by the project for the critical nature of these moving components was viewed as a serious issue.

Item 1 above needs to be carefully thought out before going to CD-0.

7 Spectrograph and near-IR camera

Findings

1. The (near) infrared (IR) camera is required for the mission; IR photometric capability is needed for identification and follow-up of $z \geq 1$ supernovae. However, the observational requirements of this instrument are confusing and were not clearly presented. The three IR camera concepts span over a factor of 50 in areal coverage, so it is difficult to understand how they can all meet the same observational requirements if these requirements are well-defined. Since the specific requirements of field of view (FOV) and sensitivity were not explicitly presented (and their derivations from the SNAP science goals were also absent), it has not been shown than these requirements are known or understood in detail.

2. The spectrograph requirements of wavelength coverage and spectral resolution were also conflicting and unclear. The spectrograph FOV requirement was better argued. The information presented in the SNAP materials and presentations (particularly spectral range and resolution, to a lesser extent) could result in anywhere from 1 to four spectrograph channels.

3. The spectrograph instrument concept and layout were reasonably complete and are at the right level for this stage of the project. However, the IR camera concepts were somewhat incomplete in that they lacked details on packaging and filters. No spot diagrams were shown for the complete instruments (after filters, gratings, and re-imaging optics), so optical performance at the detector focal planes could not be gauged.

4. Finally, the computed optical throughput of the spectrograph was given as 70%, which is very high. No budget was presented to support this estimate, and it appears that this value includes zero margin (i.e. for alignment etc.). Also, no laboratory prototype data were presented to support this high value.

Comments

1. The Rockwell 1.7 µm cutoff HgCdTe detector is a good choice for a 1024 x 1024 pixel device, and there is promise that 2048 x 2048 devices may be ready in time for the SNAP mission. It would be reasonable to plan on flying up to about a dozen of these devices; a design which requires more devices may present cost and schedule problems.

2. The HgCdTe technology needed for SNAP needs at least as much characterization, optimization, and refinement as the CCD detectors. In particular, the noise performance of the detectors will directly drive spectroscopic

observation times of high z SNe and the overall SNAP mission lifetime. Therefore it would be prudent for the SNAP team to gain experience in the operation of these devices soon so that their design and performance can be optimized in time for the mission. The detector design will also likely require iteration, so SNAP would benefit by establishing a relationship with the detector vendor soon.

3. The spectrograph team has good experience and presented a very solid concept. Their chosen integral field design is probably optimal for SNAP. However, the spectrograph performance may be worse than expected since very optimistic throughput, QE, and detector noise assumptions were made. The target signal-to-noise is already low, so the already long integration times of faint objects could significantly increase (by perhaps up to a factor of 2) if the optimistic assumed throughput cannot be achieved.

Recommendations

1. The observational requirements for the IR imager and the spectrographs should be defined better and prioritized before conducting further design trade studies or development. In particular, the SNAP team must analyze and constrain the exact wavelength ranges, areal coverages, signal-to-noise, and resolutions needed to complete the baseline mission as well as a minimum science mission.

2. SNAP should consider using thinned HgCdTe devices from visible wavelengths out to their 1.7 µm cutoffs. This would allow observing SNe from z=0.5 to z=1.7. The team should study whether the GigaCAM imager could be replaced with a HgCdTe mosaic.

3. The throughput of the spectrograph should be reinvestigated, with each component given a realistic term that includes likely losses due to misalignments, etc. The team should determine whether the spectrograph will still be useful if the resulting throughput turns out to be lower.

4. Finally, the SNAP team should plan to acquire one or more HgCdTe devices and multiplexors with designs which are as flight-like as possible. This will allow the team to gain experience in their operation as well as to start characterization, optimization, and refinement of the design (iterating with the vendor) during the CD0 phase. The planned scope and budget for this work is currently inadequate; it should be substantially increased to be closer to that for GigaCam in CD0.

Items 1-4 above all need to be addressed before going to CD-0.

8 Telescope

Findings

The SNAP team has developed an innovative low risk observatory architecture and OTA (Optical Telescope Assembly) design concept. A wide design trade space was explored leading to a TMA (Three mirror Anastigmat) reference design concept. A monolithic primary mirror of relatively high aerial density was adopted to facilitate low development cost and low risk ground verification. This design concept has been developed in substantial detail. Top-level technical challenge areas necessitating long lead technology development during the R&D phase are not apparent in the OTA subsystem.

Comments

The current OTA reference design does not present a high risk or technical feasibility challenge to the SNAP. However, allocation of the OTA pointing requirement between the spacecraft ACS (attitude control system) and OTA active optics (fast steering mirror) was not clearly described. Allocation of TMA field to science instruments and image quality over TMA field not discussed.

Recommendation

We recommend that the OTA system design permit a fast steering mirror to be included in the ACS trade space, and that this trade be fully explored in terms of risk and net mission cost. We support the OTA make/buy decision and development of a "biddable requirements document" as the primary deliverable of R&D phase work in the OTA area.

9 Spacecraft

Findings

SNAP places stringent ACS and data volume requirements on the spacecraft. No funding was requested to support spacecraft development work during the conceptual design phase.

Comments

The high pixel count and data volume associated with the reference design science instrument suite will present a technical challenge in the areas of command and data handling (C&DH) avionics and down link budget. R&D phase design work will be needed to establish feasibility and cost.

Recommendation

We recommend that at least 1 arc-sec rms pointing jitter be allocated to the spacecraft ACS at the onset of the R&D phase (see OTA recommendation above), and that a thorough systems level trade be conducted to determine an optimal allocation. We further recommend that C&DH system requirements definition and development of a system architecture (including selection of a flight processor) occur early in the R&D phase.

10 Computing and Data Handling

Findings

There is a lot of expertise in high performance computing at LBL, and the group is very well connected to these efforts. The Supernova Factory is a useful practice, and gave the group a lot of experience in running a production pipeline. The presentations indicated that most of data processing software development is seen as in-kind effort, largely thought to consist of incremental improvements of existing software. Archiving of data is low priority, and is not seen by the project as a difficult issue.

Comments

The panel felt, that software will have a much more central role in the project than the one apparent from the presentations. The data are considerably more complex than those of existing projects, like 2MASS and SDSS, which ended up with $20-$30M in software development/data processing budgets. The track record of existing projects show that development of large-scale scientific software products requires a full time commitment; it is not a part-time effort. In a distributed collaboration a large fraction of time is spent on communication between the developers. The development effort will need personnel at postdoctoral level or above: it cannot be done with graduate students.

Recommendations

1. The project should show and treat the data processing software as a top level deliverable.

2. The project needs to develop a plan for detailed simulations of the complete system data flow, well in advance of launch, driven by need of real-time processing.

3. Identify major modules, pipelines, and their integration mechanisms for all aspects of operations, both onboard and on the ground.

4. Address, identify and discuss algorithms in detail for the co-adding of images, and the calibrations, in order to fully capture the scope of the effort.

5. We endorse the Supernova Factory effort, and its importance for the preparatory phase. At the same time it is important to emphasize that it should have a finite lifetime, so that the personnel can fully focus on the SNAP development in the later phases of the project.

6. There will be a lot of community interest in the data. Archival access to the various components will be an important issue and should be addressed as such, including the identification of specific data products.

11 Observations - Survey Strategy

Findings

1. Notes on survey strategy have appeared in other sections of this report.

Comments

1. It was thought that the survey should be spread over more widely spaced fields.

2. More emphasis should be placed on the high-z objects and on near-IR observations.

Recommendations

1. Committee encourages the project to consider the optimal survey strategy that will get some minimal set of data should the satellite or some critical component fail before the scheduled end of the project. For example, is it prudent to spend the first period of time acquiring images and photometric redshifts? Could this be done in advance from the ground? Perhaps searching should begin immediately.

2. Project should consider trades of the viability of folding in ground-based and other space-based facilities to carry out parts of the project.

Item 1 above needs to be addressed before going to CD-0.

12 Observations - Photometry

Findings

To achieve its scientific goals, the project estimates that it requires containment of systematic errors in photometric and spectrophotometric calibrations to 1% over the full range of wavelengths (0.4 to 1.7microns). Fluxes and rest-frame B-V colors from supernovae at different redshifts must be transformed to a common rest wavelength.

Comments

Achieving this systematic accuracy is ambitious and challenging. It requires having external references in the form of photometric standard stars plus accurate internal calibration of all four instruments at a level that is not routinely accomplished in either ground or space based experiments.

1. The best existing absolute calibrations of bright stars do not have the required accuracy, with errors reaching 3% at infrared wavelengths.

2. The Hubble Space Telescope wide field camera II (WFPCII) data are notoriously difficult to calibrate at the 2% level.

3. The proposal recognizes the importance of this process, but gives only sketchy information on possible paths to explore to achieve it, and in our opinion underestimates the difficulty of the problem.

Recommendations

1. We recommend that the SNAP team develop a detailed error budget that properly propagates errors to the final science result in order to determine if photometric requirements can be relaxed.

2. We recommend that the SNAP team develop a detailed calibration plan tracing all steps in the calibration process for each instrument. Each step should include the procedures and the means to validate those procedures.

3. If external "experts" are needed to assist in setting up external reference stars, those people and the work that they will do should be identified by the end of CD-0.

4. Any impact of photometric requirements and calibration procedures on hardware should be identified early. A particular concern is that if bright stars

must be used for calibrations, the shutter should be able to execute short exposures and time them to high accuracy.

5. Aperture corrections are a function of wavelength and field position. The SNAP team should develop a plan to quantify such effects and, if necessary, calibrate them in orbit.

Item 1 above should be completed before going to CD-0.

13 Observations - Spectroscopy

Findings

1. At a minimum, redshifts need to be determined for the SNe and their host galaxies. This can be done spectroscopically or by photometric redshifts.

2. There will be some contamination of any initially photometrically selected sample of SNe Ia by SNe Ib, SNe Ic and possibly other variable objects.

3. The parameters of the near-IR spectrograph appeared to be marginal for obtaining spectra of sufficient S/N for the high redshift SNe. Realistic throughput and QE measures might make these observations impossible with SNAP. The IR spectrograph, even with optimistic assumptions, delivers low S/N spectra in very long integration times for high-Z SNe.

4. The contribution of the underlying galaxy background light (and structure in the galaxies) did not appear to have been included in the S/N and calibration estimates for spectroscopy.

5. We could not see any obvious advantage of optical spectroscopy with SNAP over ground-based spectroscopy.

Comments

1. Photometric redshifts can be determined in advance from multicolor photometry.

2. High precision spectroscopy and spectrophotometry may require obtaining a spectrum of the galaxy at the position of the SNe long after maximum.

Recommendations

1. Simulations should be done to see how to optimally eliminate contaminating SNe Ib and Ic, etc. without requiring spectroscopy.

2. A realistic assessment of the capabilities of the near-IR spectrograph should be done.

3. Simulations including the effect of an underlying galaxy with structure should be done. These should include the effects of improving spatial resolution.

4. The project should consider the use of NGST for the IR spectroscopy and whether NGST will or might be required to achieve the scientific spectroscopic goals. Comparisons should be made with spectroscopy done with large ground-based telescopes and adaptive optics.

5. We agree with the project that every SNe that goes into the final analysis should have a spectrum.

Item 2 above needs to be addressed before going to CD-0.

14 Cost, Schedule, and Funding

Findings

1. The proposers estimate that the total cost to perform the proposed R&D, conceptual design studies and requirements development, leading to a Conceptual Design Report, is $15,886K over FY01 and FY02. Of this amount, $3,127K would be provided from existing laboratory funds, leaving a request for additional funding of $12,759K. This is proposed to be shared among several funding sources as follows:

	FY01	FY02	Total
DOE	1,424	4,990	6,414
NSF	1,151	4,024	5,175
Foreign sources	568	601	1,169
Total			$12,759K

   The DOE funding would be used to support activities predominately at LBNL, while the NSF funding would be used to support activities predominantly at SSL.

2. The cost estimate includes approximately 8% “contingency” (unallocated management reserve).

3. No cost estimate for the overall mission was provided. Preliminary estimates for several significant components (e.g., optical telescope assembly, instrumentation) were furnished, but other significant components were not estimated.

4. The schedule presented shows CD1 (roughly the System Requirements Review (SRR) in NASA parlance) in October 2002, CD2/NAR/PDR in summer 2003, and launch in July 2008.

Comments

1. The estimated cost is consistent with the level of effort proposed. The 8% contingency seems thin given the uncertainties at this stage and the Committee's other recommended studies, above. These additional studies might be accommodated by rearranging priorities within the planned scope of work. However, a larger reserve at this time could lower overall risk at CD1 and throughout the mission lifecycle, and seems prudent.

2. The proposed 20 months remaining until CD1 is reasonable for developing and documenting the conceptual design and performing the proposed and recommended trade studies. The five years allotted from PDR through launch is a reasonable estimate for planning purposes.

3. Failure of any of the US or foreign sponsors to provide the requested funding would significantly increase risk at CD1 and delay the overall schedule, thereby likely leading to increased costs.

Recommendations

1. If possible, the sponsors should provide the requested funding on the schedule requested, and should consider providing up to 50% contingency to support some of the studies recommended in other parts of this report and reduce risk at CD1. Many studies have shown that heavier investment at this stage of project development pays large dividends in the form of reduced risk and better cost/schedule/technical performance during the implementation phase.

2. The SNAP team should develop a cost estimate for all elements and phases of the mission, including both parametric and industrial rough order of magnitude estimates. The committee recommends that the cost estimates be developed to show the relationships between science requirements which flow down to primary design drivers, to permit mission scope re-optimization at CD-1 if necessary.

15 Project Management

Findings

1. The full committee believes that the SNAP science goals are excellent and address fundamental questions in particle physics and cosmology and justify significant costs and efforts associated with the project.

2. The relationship of SNAP to NASA is undeveloped. There does not seem to be a scenario for procurement of the spacecraft and launch.

3. The scope of required computing, particularly the data processing pipeline, appears to be underappreciated by management.

4. The manpower plan for the R&D phase has a significant number of people putting in small fractions of their time.

5. The plan for External Advisory Committees seems vague.

6. The collaboration seems small for the scale of the project.

7. The planning for utilization of SNAP appears to be largely within the SNAP collaboration.

8. LBNL has very strongly supported SNAP with their discretionary funds.

Comments

1. While SNAP has received enthusiastic endorsement by much of the HEP community, (e.g. SAGENAP), this review is probably the first time that SNAP has been scrutinized by a panel including a large number of astronomers. Both the High Energy physicists and the astrophysicists agree that the science is important and compelling. However, the committee is not convinced that the preconceptual design of the experiment, as presented, is adequate for CD-0, particularly with the IR capabilities discussed elsewhere in this report. There are crucial details in the design of the experiment, and even in the emphasis planned for the Conceptual Design studies, that need to be addressed and clarified before the panel can recommend CD-0 approval.

2. It is obvious that SNAP will need a launch vehicle, and possibilities include the US (NASA), the European Space Agency (ESA), and Russia. It is important that SNAP begin to develop credible scenarios for a spacecraft and launch. SNAP has enthusiastic French collaborators that might provide a link to their government. There is also a large community of expertise associated with NASA that could provide both technical support and scientific enthusiasm for

SNAP if suitably recruited. Such participation would require integration with the planning and prioritization processes of NASA.

3. An unknown factor is likely new management at DOE and NASA. There will be a need for coordination of all funding agencies that become involved with SNAP.

4. The committee is concerned that the scope of developing the data processing pipeline - handling data aboard the spacecraft, moving 250 Gbytes of data per day to the ground, and effectively processing that data to achieve the required photometric accuracy - will require very substantially more manpower than seems anticipated.

5. The manpower plan has many people at the 10 –20% level, although in some cases the same person shows up in several smaller assignments, which is ok. The committee is concerned that small manpower fractions often round off to zero. This may be a particular issue with ASIC designers.

6. The plans outlined for external advice seem vague. The group described in the Draft Management Plan sounds much like yet another review committee, and it seems unlikely that SNAP needs to be calling in more reviews than required. Other collaborations have used groups ranging from Machine Advisory Committees (usually for accelerator projects) that are drawn from the highest levels of expertise worldwide. While extremely wise and experienced, these groups typically meet once or perhaps twice a year, and tend towards advice on major strategic issues. They usually formally advise the laboratory management structure. At the other end, several HEP collaborations have used smaller collections of people to advise on one subsystem at a time (e.g. CDF's Godfathers, SLD Detailed System Reviews). These groups, distinct for each subsystem, meet every two to three months to stay well informed on technical progress and provide detailed advice.

7. In strengthening the collaboration, it would be wise to recruit from the astronomy and astrophysics community.

8. The planning for utilization of SNAP seems to be within the collaboration, as would be expected in a DOE project. This is in contrast to NASA style, in which the community plays a much larger role in determining science priorities.

9. The committee wishes to commend LBNL for its support and seed money to the forming SNAP collaboration. LBNL has an excellent history of seeding good science.

Recommendations

1. The full committee recommends that DOE encourage and support substantial simulation and trade studies for approximately the next six months, and reconsider CD-0 at that time.

2. Both DOE and SNAP should begin to develop a basic understanding of the NASA role with SNAP in the next year and forge the necessary agency links. This should be at both the project and agency level. SNAP should also develop other launch possibilities that they deem appropriate.

3. Try to understand the software experience of other recent space and ground experiments.

4. Try to ensure that the required manpower for the substantial R&D activities will be available and effective.

5. Develop a plan for external advice that will be most helpful to SNAP. Check on the experience of other collaborations.

6. Try to strengthen the collaboration, particularly with people from the astronomy and astrophysics communities.

7. The collaboration should consider soliciting community input for determining the best use of the instrument for science beyond the central SNe mission.

8. LBNL should continue using its discretionary funds for seeding such projects!

Items 1 and 2 above should be addressed before going to CD-0.

Action Items

SNAP and DOE should schedule another CD-0 review before FY02.

Appendix A: Charge to the Committee

Charge to the Committee for the SNAP Research and Development Review Jan. 25-27, 2001 at LBNL

The Supernova/Acceleration Probe (SNAP) is an experiment designed to discover and precisely measure thousands Type Ia supernovae (see http://snap.lbl.gov for more information). From the data collected, it will be possible to investigate properties of the accelerating universe and study both the dark energy and dark matter in the universe. Features of the apparatus are a 2m wide-field telescope with a one-billion-pixel CCD detector launched into high earth orbit.

The subject of the Review is the SNAP experiment in its current pre-conceptual design phase. The scientific importance of the SNAP goals were established in the Feb. 2000 SAGENAP review. Although the scientific goals were deemed important, the case for project justification is still under review.

The focus of the Review will be on the R&D progress to date and that planned and required for the future. Special concentration will be given to the technically challenging parts of the instrument. The Committee is asked to review progress in preparation for establishing CD-0 ("Mission Need") level of project approval, given at the end of the pre-conceptual planning phase.

In addition to details of the R&D program, the Review will also cover other key issues. There will be a discussion of the context of the experiment including choice of space-based technique, science goals and how the instrumentation set was derived from those goals. The science case for SNAP and the goals necessitating the technology proposed will be described and discussed. The arguments for a space-based rather than ground-based apparatus will be described and discussed.

Specific charges directed to the Committee are:

1. Evaluate the R&D progress to date and the plans for achieving the Conceptual Design, including estimates of R&D baseline costs, schedules, the collaboration, and the management structure.

2. What are the issues associated with building an instrument of this complexity? Does it appear feasible to develop SNAP to meet the scientific goals in the next eight to ten years? Are there problem areas not being addressed by the SNAP collaboration?

3. Comment on the proposed project cost, schedule and management structure as presented.

A formal, written report is due to the Division of High Energy Physics of the Department of Energy by March 31, 2001. The committee members are asked to contribute draft sections of this report by the end of the Review, Jan. 27, 2001.

Appendix B: Committee Membership

Committee Participants:

Kathleen Turner (Chair)	DOE	kathy.turner@science.doe.gov
William Althouse	SLAC	wea@SLAC.Stanford.EDU
Charles Baltay	Yale	Charles.baltay@yale.edu
Marty Breidenbach	SLAC	Mib@slac.stanford.edu
Marcel Demarteau	FNAL	demarteau@fnal.gov
Sandra Faber	UC Santa Cruz	faber@ucolick.org
Tom Greene	AMES	tgreene@mail.arc.nasa.gov
Matt Greenhouse	GSFC	matt@stars.gsfc.nasa.gov
John Huchra	Harvard	Huchra@cfa.harvard.edu
Robert Johnson	UC-Santa Cruz	johnson@scipp.ucsc.edu
Steve Kent	FNAL	Skent@fnal.gov
Gerry Luppino	Univ. Hawaii	ger@hokupa.ifa.hawaii.edu
Joel Primack	UC Santa Cruz	joel@ucolick.org
Abhi Saha	NOAO	Saha@noao.edu
Glenn Starkman	Case Western	starkman@huxley.PHYS.CWRU.Edu
Alex Szalay	Johns Hopkins	szalay@tardis.pha.jhu.edu
J. Craig Wheeler	Univ. Texas	Wheel@astro.as.utexas.edu

Observers:

Gene Loh	NSF	ecloh@nsf.gov
Dick Nolan	DOE	dick.nolan@oak.doe.gov
Guy Stringfellow	NASA	guy.stringfellow@hq.nasa.gov
Steve Tkaczyk	DOE	steve.tkaczyk@science.doe.gov
Tim Toohig	DOE	timothy.toohig@science.doe.gov
P.K. Williams	DOE	pk.williams@science.doe.gov

AMES = Ames Research Center (NASA)
FNAL = Fermi National Accelerator Lab (DOE)
GSFC = Goddard Space Flight Center (NASA)
NOAO = National Optical Astronomy Observatories (NSF)
SLAC = Stanford Linear Accelerator Center (DOE)

Appendix C: Agenda for the Review

Thursday, January 25, 2001, Building 50A, Room 5132

8:00 am	Executive Session
9:00 am	Welcome …………………………………………………....Pier Oddone
9:05 am	Introduction ……………………………………………….Kathy Turner
9:10 am	SNAP science (40+10)…………………………………..Saul Perlmutter
10:00 am	Systematics and Requirements (35+10)…..…………………Greg Aldering
10:45 am	Break
11:00 am	Weak Lensing (15+5)……………………………… (telecon)..Richard Ellis
11:20 am	Project Overview (35+10)……………………………..……Michael Levi
12:05 pm	Working Lunch
12:35 pm	GigaCAM (40+10)…………………………………………. Chris Bebek
1:25 pm	Electronics architecture (25+5)……………………… Henrik von der Lippe
1:50 pm	ASIC Development (15+5)……………………..….. Jean Francois Genat
2:10 pm	NIRcam (15+5)....……………………………………………. Greg Tarle
2:30 pm	Break
2:45 pm	Spectrograph (20+5)…..…………………………….……..Olivier LeFevre
3:10 pm	HgCdTe Technology for SNAP(15+5)…………………….. James Graham
3:30 pm	Breakout Sessions #1 GigaCAM/Electronics 50A-5132 #2 NIRcam & Spectrograph 50B-6208
5:00	Executive Session

Friday, January 26, 2001, Building 50A, Room 5132

8:30 am	Spacecraft/SE (telemetry & pointing) (30+5)……………Henry Heetderks
9:05 am	Telescope (30+5)…..……………………………………Michael Lampton
9:40 am	Computing/NERSC…………………………….…………….. Stu Loken
10:00 am	Break
10:15 am	Theory (models, assumptions)

1. Cosmological Parameter Measurements
   M. White (Harvard-Smithsonian, CfA) – telecon

2. Cosmic acceleration and fundamental physics
   A. Albrecht (UC Davis)
3. Constraints on the Nature of the Dark Energy
   M.S. Turner (U. Chicago) – telecon

11:00 am	Cost and Schedule: R&D Costs/Schedule, "Cost" range as it appears now……….Bill Edwards
11:30 am	Project Management………………………………………..….... Peter Harvey
12:00 noon	Working Lunch
12:30 pm	Q&A session on general scientific and detector issues
1:30 pm	Breakout Session: #3 Spacecraft/Telescope 50A-5132 #4 Computing 50B-4205 #5 Project Management/Cost/Schedule 50B-6208
2:30 pm	MicroSystems Lab/CCD Technology……………………….Steve Holland
2:45 pm	Space Sciences Laboratory……………………….……..………Robert Lin
3:00 pm	Tours
4:00 pm	Executive Session

<u>Saturday, January 27, 2001, Building 50A, Room 5132</u>

8:00 am	Report Writing
9:30 am	Executive Sessions and Close-out Dry-Run
12:00 noon	Closeout Session with SNAP
1:00 pm	Adjourn