

## PARTICLE ASTROPHYSICS

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## EXECUTIVE SUMMARY

In the last few years, particle astrophysics has emerged as a new field at the frontier between high energy astrophysics, cosmology, and particle physics. A spectacular achievement of this new field in the last decade has been the establishment of neutrino astronomy with the detection of solar neutrinos by two independent experiments and the spectacular observation of the neutrinos from the supernova SN1987A. In addition, the field has produced tantalizing hints of new physics beyond the standard models of astrophysics and particle physics, generating enthusiastic attempts to confirm these potential effects.

The next decade promises to be even more productive. Extrapolating within the present conceptual framework, we expect the next ten years to bring fascinating results on the following issues:

- The elucidation of the nature of dark matter, especially if it is made of nonbaryonic particles.
- The explanation of the solar neutrino puzzle and if a supernova explodes in our own galaxy, a detailed account of the explosion mechanism by the analysis of the emerging neutrinos.
- The possible confirmation of the existence of point sources of energetic particles leading to the production of gamma rays, neutrinos and maybe new particles at energies as high as  $10^{14}$  eV.
- The understanding of the origin of cosmic rays, including the physical processes responsible for their synthesis and acceleration on a wide variety of scales.

It is even more likely that as yet unsuspected phenomena will be discovered, that entirely new concepts will be tested and that the deep link between particle physics, the early universe and the high energy astrophysical processes will be extended beyond what we can imagine today!

In contrast with more mature observational fields, particle astrophysics is likely in the next decade to still be based on the succession of experiments which will increasingly sharpen their scientific focus. We see, therefore, the evolution of the field as governed by a number of decision points occurring in the next decade when the information from previous experiments or technological development becomes available:

- The highest priority at the moment is the rapid implementation of the presently approved program of observations on the ground and in space. Within this program, existing neutrino detectors should be coordinated and maintained to provide an efficient supernova watch.
- The technology is now available to tackle the question of the extragalactic origin of cosmic rays with the study of their spectrum at  $10^{20}$  eV. We recommend that the high resolution Fly's Eye be supported, contingent on a favorable detailed technical review.
- We foresee important funding decisions to be made in a few years when the required information will be available. If the feasibility of the cryogenic technologies can be demonstrated, it is clear that a full scale search for nonbaryonic dark matter will have very high priority. If the large extensive air shower detectors currently being deployed confirm the claims for localized gamma ray sources above  $10^{14}$  eV, understanding these unsuspected acceleration mechanisms (and the new particle physics if the claimed anomalous muon content is substantiated) would require more powerful detectors. The results of current solar neutrino detectors may steer the field into new observational directions. Finally, if the early promise of gamma ray astronomy at  $10^{12}$  eV is fulfilled, then larger installations will surely be required.

The preparation for these decisions appearing on the horizon demands a strong development of new detection techniques: we emphasize in particular the cryogenic particle detectors for dark matter searches, the new solar neutrino schemes, and the test of the water Cerenkov technique for the detection of extensive air showers.

Such a complex and fundamentally multidisciplinary enterprise requires a strong theoretical activity and a variety of coordination mechanisms. Our main recommendation here is the establishment of a particle astrophysics

Advisory Panel which could advise the federal agencies involved in particle astrophysics (NSF, DOE and NASA) on the relative scientific priorities and help them set up long term policies.

### The emergence of a new scientific field

Particle detection techniques have been essential to the development of high energy astrophysics since the first cosmic ray investigations in the early years of this century. Their role has been greatly expanded during the past decade as a variety of large experiments have begun to address fundamental astrophysical questions.

Two of the most recent and important advances in astronomy have been made by the direct detection of neutrinos. Two independent experiments measured the solar neutrino flux using totally different techniques, and neutrinos from the supernova 1987A have been directly detected by experiments designed to observe proton decay. Moreover, observation of the electromagnetic spectrum from astrophysical objects has been extended above  $10^{11}$  eV using ground based Cerenkov detectors. Evidence obtained with air shower arrays suggests that gamma radiation in excess of  $10^{14}$  eV may also have been observed from astrophysical objects. Cosmic rays have been detected up to  $10^{20}$  eV but their nature and origin at these energies remains a mystery, as does the means of their acceleration.

In parallel, progress in the understanding of particle physics has suggested that the missing matter in the universe may consist of as yet undiscovered elementary particles, which are relics of the very earliest phases of the formation of the universe. It also appears that quantum fluctuations and topological singularities generated in phase transitions occurring at very high temperature in the early universe could have played a fundamental role in the formation of large scale structure. In addition, it is now well understood how the properties of neutrinos could be responsible for the solar neutrino puzzle, and the powerful acceleration mechanisms evidenced by the highest energy cosmic rays may require new particle physics.

The discoveries and activities described above have been mostly carried out by an unconventional breed of "astronomers" whose backgrounds have been in particle or nuclear physics. The nature of the research ranges from solid experiments with well defined systematic goals to investigations which test speculative ideas or follow on experimental hints. Therefore, such a field is a vital and exciting one, with new ideas, new practitioners, and the certainty of scientific progress. It may well be that the cosmos is providing us with the first evidences for physics beyond the standard models of astrophysics and particle physics.

In this new field, which we may call particle astrophysics, we can distinguish four interrelated areas dealing respectively with cosmology and particle physics, stellar physics and particles, high energy gamma and neutrino astrophysics ( $>10^{12}$  eV) and cosmic ray astrophysics.

We review first the essential scientific questions being tackled and the present experimental program, before turning to priorities and institutional questions.

### *Cosmology and Particle Physics*

The physics of the early universe is intimately related to particle physics at the very highest energies and it is not possible to distinguish them in the quest for the answer to the fundamental questions of cosmology: What is the nature of the ubiquitous dark matter? What is the origin of the predominance of matter over antimatter? What is the explanation for the smoothness, flatness, and old age of the universe? What is the origin of the primeval inhomogeneities that triggered the formation of structure and eventually galaxies in the universe? Conversely, the cosmological observations provide essential constraints in the construction of unified theories of particle interactions and may be the only source of information on physics at the very highest energies (up to the Planck scale -  $10^{19}$  GeV). Physics at these energies is difficult to probe in terrestrial laboratories and so, at the same time the early universe provides a natural laboratory in which physics at the most fundamental level can be studied.

### *Particle Physics and the Early Universe*

The past decade has seen the consolidation of two standard models: the  $SU(3) \times SU(2) \times U(1)$  gauge theory of particle interactions and the Hot Big Bang model. The former provides a fundamental theory of the elementary particles and their interactions at distances down to  $10^{-17}$  cm (energies up to  $10^3$  GeV), while the latter provides an accurate accounting of the history of the Universe from about  $10^{-2}$  sec after the origin of the universe. Encouraged by these impressive successes, particle physicists and cosmologists have begun to extrapolate to earlier times and attempted to answer the fundamental questions outlined above. The origin of the matter-antimatter asymmetry seems

to involve forces that violate both CP and baryon number conservation. At very early times ( $10^{-34}$  sec?) phase transitions may have played an important role. For instance, inflation provides a very attractive explanation for the flatness and old age of the universe and of the primeval density inhomogeneities. In addition, many theories that go beyond the particle physics standard model, such as supersymmetry, predict the existence of stable relic elementary particles that are left over from the early moments after the Big Bang and that may constitute dark matter. Combining such cold dark matter (which could also be made of condensed astrophysical objects or primordial black holes) with the Harrison-Zel'dovich spectrum of adiabatic density fluctuations predicted by the simplest models of inflation, it has been possible to obtain a fair first approximation to a theory of the formation of galaxies and clusters of galaxies. Alternatively, it is possible that cosmic strings or other topological defects associated with an early phase transition or with inflation, may be at least partly responsible for structure formation; in this case, there are indications that hot dark matter (e.g. relativistic particles) rather than cold dark matter may produce structures like the ones we see.

After more than a decade of intense theoretical work which has produced these fascinating ideas, the time is ripe for strengthened experimentation and observation. The main problems are easily identified:

- *Determination of the basic cosmological parameters.* We are still lacking a definitive determination of the Hubble parameter which enters in most cosmology calculations. A reliable measurement of the ratio  $\Omega$  of the average universe density to the critical density, for instance by probing the geometry of the universe on large scale, is central for solving the problem of dark matter. The age of the universe is still uncertain. The combination of these three parameters would allow us to determine the spatial curvature of the universe (testing therefore the inflation paradigm) and the value of the cosmological constant, the small value of which remains a mystery.

- *Measurement of primordial abundances:* This will allow us to test in more detail the standard Hot Big Bang model of primordial nucleosynthesis and determine the average density of ordinary (baryonic) matter.

- *Study of diffuse backgrounds:* The primordial fluctuations that presumably lead to the formation of structure in the universe must be reflected in small anisotropies of the 2.7 K cosmic microwave background. When these are finally detected, they will provide a firm foundation for theories of structure formation and will point back to the processes that gave rise to these fluctuations in the early universe. The X ray and  $\gamma$  ray diffuse backgrounds are not yet fully understood but are potentially of cosmological origin. Timing measurements of the millisecond pulsars place the most stringent bounds on the density of the stochastic gravitational wave background and help to constrain models such as cosmic strings.

- *Mapping of the universe.* The systematic measurement of the galaxy density and velocity fields on increasingly larger scales will help to reveal the initial conditions and basic evolutionary processes in the universe.

- *Search for dark matter.* The nature of dark matter remains a mystery but begins to be accessible to observations: condensed astrophysical baryonic objects may be detectable by micro-lensing, relic particles by direct detection via elastic scattering in the laboratory and primordial black holes by the gamma ray bursts they should generate.

Although the needed observational evidence involves a wide variety of observational fields of astrophysics, most of which are not covered by this panel, it is important to stress the importance of all these observations in order to understand the role of particle physics in the early universe.

### *Dark Matter*

Dark matter currently is probably the best example of the interpenetration of particle physics and cosmology. Based upon decades of astronomical and cosmological observations we are certain that most of the matter in the Universe is nonluminous and transparent. Various cosmological and astrophysical arguments suggest that the dark matter may not be ordinary matter (baryons), and that the most likely candidate is a relic elementary particle. The abundance of the candidate relics, their properties, and means of detecting them have been studied, and we are now ready to undertake the most important step: the experimental testing of the particle dark matter hypothesis.

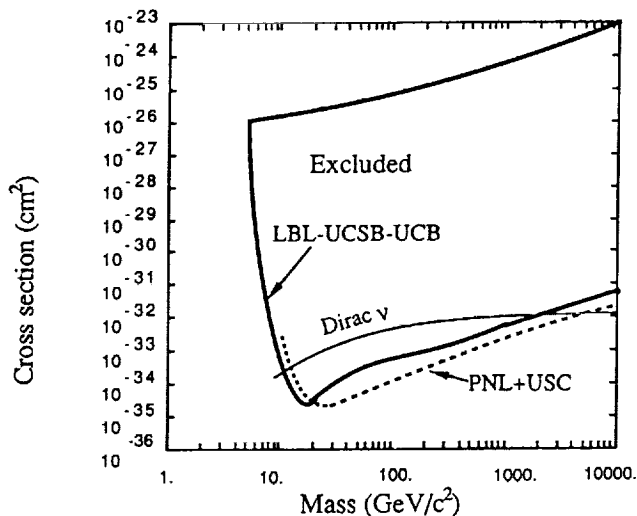
Three types of dark matter particles are particularly well motivated: light neutrinos, axions and weakly interacting massive particles .

*Light neutrinos* of mass of about 30 eV would solve the dark matter problem, although they may complicate the explanation of the formation of large scale structure of the universe. Unfortunately, no experimentally viable method has yet been proposed to detect the cosmological neutrinos directly and we will require laboratory

measurements of the neutrino mass for the three neutrino generations to fully test this hypothesis. The detection of a large number of neutrinos from distant supernovae could eventually provide interesting direct mass limits.

*The axion*, a very light (mass of about  $10^{-3}$  eV to  $10^{-6}$  eV) pseudoscalar particle still represents the best solution to a fundamental problem of the standard Particle Physics model (the strong CP problem of QCD). Unfortunately the axion interactions are expected to be very weak, not much stronger than gravitational, and it is a testimony to the talents of the experimentalist involved that the sensitivities of the first generation experiments employing resonant microwave cavities were within about a factor of 300 of that required. It may be possible to improve the sensitivity by this factor with large cavities in the lower mass region. The fundamental experimental problem remains that present detection schemes are narrow band, and, since the axion mass which leads to closure density is only known to a factor of about 100, scanning the entire region requires a long time and a large effort. Recent theoretical work suggests that the mass uncertainty may even be larger as radiation from global strings may be the dominant source of axions.

*Weakly interacting massive particles (WIMPs)* are another general class of candidates which correspond to the case where heavy dark matter particles were in thermal equilibrium with the rest of matter in the early universe. Their interaction cross sections can then be estimated from their current density and turn out to be of the order expected for "Weak Interactions" (in the technical sense). This coincidence may be purely accidental or may be a very precious hint that physics at the W and  $Z^0$  scale (e.g., Supersymmetry) may also be responsible for dark matter! For instance the lightest supersymmetric particle may constitute dark matter: this particle is the neutralino, sometimes referred to as the photino or higgsino, which are special cases. In order to test this fairly general hypothesis, we could attempt to detect directly the interaction of halo particles with a laboratory target, in a way complementary to the new particle searches at accelerators (LEP, Tevatron and the SSC). This requires, unfortunately, very sensitive detectors with a good rejection of the radioactive background. Current detectors using ionization techniques have set interesting limits, for instance excluding the possibility that dark matter is made of heavy Dirac neutrinos (Fig. 1) and severely limiting the existence of cosmions which could simultaneously explain the deficit of  $^8\text{B}$  solar neutrinos. But before the neutralino model can be probed, the rejection of backgrounds must be improved by two or three orders of magnitude. This factor could eventually be reached with emerging technologies based on the detection of phonons or quasiparticles in superconductors which should allow better energy sensitivity and much higher redundancy. Although not sufficient to establish the feasibility of a definitive experiment, the results obtained currently with cryogenic detectors of a few tens of grams are encouraging. It may also be possible to detect indirectly the presence of these dark matter particles in our galaxy, by their annihilation products: positrons, antiprotons,  $\gamma$  rays and neutrinos. Recent balloon measurements indeed indicate an excess of antiprotons and positrons but the interpretation in terms of dark matter annihilation products now seems less likely than some other possible explanations. Of these indirect methods, neutrinos originating from the sun may be the least model dependent.



*Constraints on the mass and the cross section of Dark Matter particles, obtained by the LBL-UCSB-UCB and the PNL-USC collaborations (as of December 1989).*

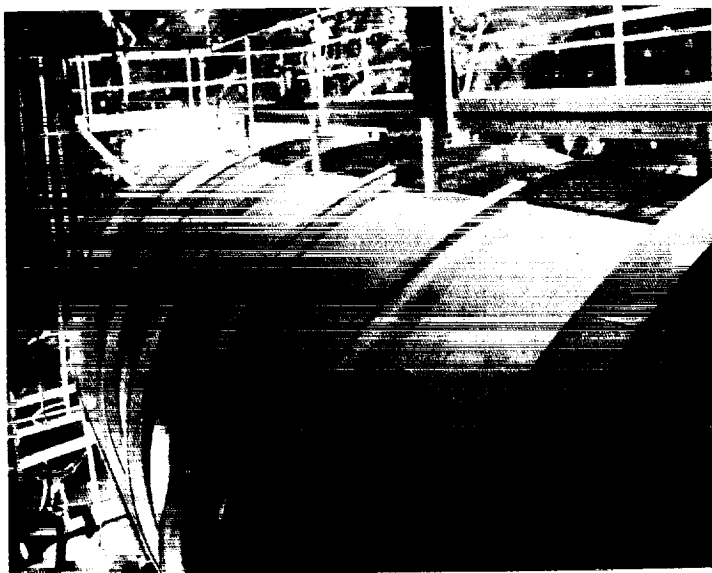
Figure 1

### *Other Relics*

Other relic particles may be significant in cosmology even if they do not constitute dark matter (recall that the cosmic microwave background contributes less than  $10^{-4}$  of the critical density). The observation of relic cosmological neutrinos would of course be of exceptional interest but no good ideas for their detection have been proposed. Significant relics include superheavy magnetic monopoles, decaying neutrinos, a neutralino species, or decaying axions. Monopoles are one of the two fundamental new predictions of grand unified theories (the other being proton decay). They may be detectable by a large, football field-sized detector, MACRO, which is well on its way toward completion. It will, for the first time, probe the flux range below the "Parker bound",  $10^{-15}$  /cm<sup>2</sup>/sec/ster. Should relic monopoles be discovered they would both confirm the ideas of grand unification and probe the universe at an age of about  $10^{-34}$  sec.

### *Stellar Physics and Particles*

Neutrinos play an important role in the physics of stars and there again particle physics and astrophysics are intricately intertwined. Neutrinos are elementary particles with very small interaction cross sections which, along with photons and positrons, are emitted in the nuclear processes which power the stars. Photons diffuse out of the stars with time scales of the order of millions of years, losing all information about the reactions which created them. Most of the neutrinos traverse the interior of the star without interaction and thus in times measured in seconds. Therefore, they carry direct information about the processes which created them in the stellar interior. To date, neutrinos have been detected from a middle-aged star, namely the Sun, and from one star at the end of its life, supernova SN1987A.



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*The solar neutrino Chlorine experiment in the Homestake mine.*

*Figure 2*

### *Solar neutrinos*

It is believed that the processes which produce energy in the solar interior are sufficiently well understood that the rates of the different nuclear reactions contributing to this energy release can be calculated, provided the empirical inputs to the calculation are known sufficiently well. Thus, measurement of the flux and spectra of solar neutrinos should confirm in detail our understanding of processes in stellar interiors. The well documented and recently confirmed disagreement between the calculated solar neutrino flux and the measured flux indicates that processes in the solar interior may be different from our expectations, or that the propagation of the neutrinos from

the interior of the sun to a terrestrial detector may be influenced by fundamental properties of the neutrinos themselves. If neutrinos have a mass, the observed deficit of high energy neutrinos may be due to the transformation of electron type neutrinos into another neutrino type either in the sun (matter oscillations), or during propagation to the earth (vacuum oscillations). Whatever the solution of the puzzle, an imperfect modeling of the sun or fundamental properties of the neutrinos, it will be important for astrophysics, cosmology and particle physics.

The original solar neutrino experiment (Fig 2) uses a subterranean radiochemical detector in which neutrinos above a threshold of 814 keV transform  $^{37}\text{Cl}$  nuclei into  $^{37}\text{Ar}$  nuclei through inverse beta decay. It is mainly sensitive to the neutrinos resulting from the decay of  $^8\text{B}$ , an end product of one of the reaction chains in the sun. Over a twenty year exposure, the average response of this detector is less than one half of the best calculated lower limit of the flux. This deficit of high energy solar neutrinos has recently been confirmed by the Kamiokande II experiment operated by a Japanese - U.S. collaboration. Their technique is completely different and is based on the detection of the recoil electrons scattered elastically by the  $^8\text{B}$  neutrinos in a large water Cerenkov detector. The electron direction is kinematically correlated with the neutrino direction, so that the angular distribution of electrons with respect to the direction from the sun shows, for the first time, that the sun is the source of the neutrinos (Fig 3).

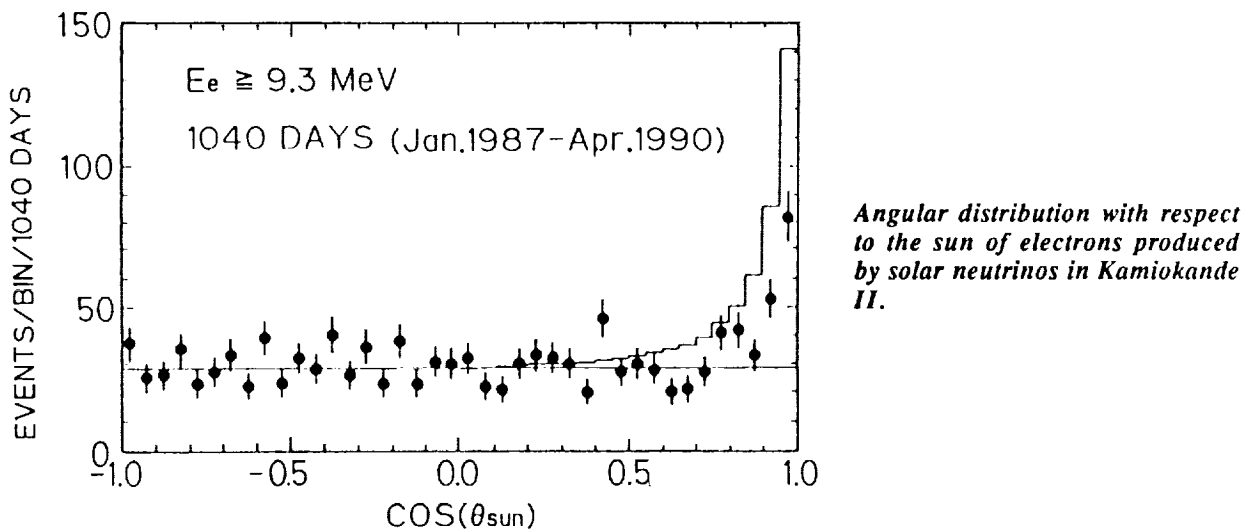


Figure 3

As 1990 begins, the  $^{37}\text{Cl}$  detector has observed an apparent time dependence in the detection rate which appears to be anticorrelated with sunspot number, or solar activity. Since solar activity is a surface phenomenon, and the neutrinos are born in fusion reactions deep in the solar interior, a connection between the two phenomena would be surprising. Such a flux variation is not confirmed by the Kamiokande II experiment but the small counting rates in both experiments precludes any definitive conclusion.

Because of their relatively high threshold, neither the  $^{37}\text{Cl}$  experiment nor the Kamiokande II experiment are sensitive to the copious neutrinos from the main pp reaction chain which is responsible for most of the energy production of the sun. It is clear that it is advantageous to detect those lower energy neutrinos since the uncertainties associated with the calculation of their flux are much smaller than for the  $^8\text{B}$  neutrinos. The necessary technology involving a radiochemical method using inverse beta decay on a  $^{71}\text{Ga}$  target was developed in the United States in the early 1970's. Unfortunately, partially because the funding structure was not flexible (we will come back to the problem of particle astrophysics experiments falling between the cracks in section 4), funding could not be secured for such an experiment in the United States. Gallium experiments have been implemented by the Soviet Union in the Baksan Laboratory and by Europe at the Gran Sasso Laboratory in Italy. Both experiments, which will produce results during the early 1990's, are international collaborations, with a significant participation by United States scientists, especially in the Baksan experiment. The latter experiment recently presented tantalizing preliminary results, possibly indicating an even larger neutrino deficit at low energy. It is too early to draw any firm conclusion

but it is clear that these gallium experiments with their sensitivity to the main energy generation mechanism in the sun are a critical component of the total solar neutrino program.

A second generation experiment dedicated to the measurement of the  $^8\text{B}$  neutrino flux has just been approved. The Sudbury Neutrino Observatory collaboration (Canada, U.K., and U.S.) plans to use for this purpose a kiloton of heavy water,  $\text{D}_2\text{O}$ , as a solar neutrino target in a Cerenkov detector. In addition to measuring the elastic scattering of neutrinos by atomic electrons as in the Kamiokande II experiment, this experiment will measure the energy spectrum of electron type neutrinos through the inverse beta decay of deuterium, and the total flux of neutrinos, independent of type, through the neutral current disintegration of deuterium by neutrinos above the 2.2 MeV reaction threshold. This neutral current capability will be essential if it turns out that the deficit of solar neutrinos is due to oscillation phenomena. While the electron neutrino component may be depressed, the total flux of neutrinos should be constant. Furthermore, the counting rate in this experiment will be an order of magnitude higher than in the  $^{37}\text{Cl}$  experiment, an important advantage when trying to calibrate out systematics or to understand the time dependence in the signal.

The solar neutrino program appears therefore to be well directed. The results of the  $^{37}\text{Cl}$  and  $^{71}\text{Ga}$  experiments, and the findings of the Sudbury Neutrino Observatory will very likely clarify the overall situation within a few years. Follow up experiments may then become necessary to confirm the insight thus gained. If the anti-correlation with the solar activity is confirmed, high rate experiments based for instance on  $^{127}\text{I}$  may become necessary. The observation of a low rate in the gallium experiments would focus the attention on the low (pp) and intermediate ( $^7\text{Be}$ ) energy region, and the need for a spectrum measurement, for instance with ultra-low radioactive background scintillators, possibly loaded with  $^{115}\text{In}$  or  $^{11}\text{B}$ , or cryogenic methods in  $^4\text{He}$ .

### *Supernovae*

The most significant event in establishing neutrino astronomy was the observation of a burst of neutrinos from supernova SN1987A in coincidence in the Kamiokande II and IMB water Cerenkov detectors in February 1987. Because the supernova was in the Large Magellanic Cloud at a distance of 52 kpc, smaller scintillation detectors in the U.S., Europe, and the Soviet Union did not see it. The observation of a flux compatible with the expected energy release provided an impressive confirmation of our understanding of the basic supernova mechanisms.

Should a supernova occur in our galaxy, the existing detectors and detectors under construction will be able to detect orders of magnitude more events. This will allow both important studies of the emission of neutrinos from the collapsing core including the time development and energy spectrum of electron, mu and tau type neutrinos and antineutrinos and detailed measurements of properties of neutrinos. It may be, for instance, possible to achieve a mass limit of 100 eV for the mu and tau type neutrinos. The array of detectors capable of detecting signals from a galactic supernova will increase during the 1990's. These detectors include solar neutrino detectors, dedicated supernova search detectors, and deep underground scintillation detectors searching for magnetic monopoles and other "cosmic ray" phenomena. Given the importance of this information for refining current understanding of the mechanism of stellar core collapse and of the neutrino sector, all such detectors should be instrumented to maximize supernova detection capability including burst handling and absolute timing.

### *Unconventional Particle Physics and Stellar Physics*

In addition to the neutrinos, there may be other weakly interacting particles (e.g. axions or neutralinos). However these additional hypothetical particles may perturb supernova explosions and the stellar energy generation mechanisms. Their properties have recently been severely constrained by the SN1987A observations and by "conventional" stellar physics (e.g. population of the Hertzsprung-Russel diagram and lifetimes of stars). We expect these cross-disciplinary analyses to expand in the coming decade. More generally, many particle physics theories have implications for astrophysics. For example, if strange matter is the ground state of matter at high density and/or atomic number, then some (or all) neutron stars may be strange stars: This would have important implications for astrophysics which in turn will provide one of the few probes of the strange matter hypothesis.

### *High Energy Gamma Ray and Neutrino Astronomy*

When astronomers developed techniques which extended observations of the electromagnetic spectrum beyond the optical range, many new discoveries were made. It is natural to want to extend observations of

astrophysical objects in as broad a wavelength range as possible. Thus, at the upper end of the electromagnetic spectrum, space-borne instruments have been used to observe gamma rays up to about  $10^9$  eV. Higher energy observations are difficult because of rapidly falling fluxes. The Gamma Ray Observatory, GRO, which will soon be launched, will extend space-borne observations to their practical limit, a few  $10^{10}$  eV.

At about  $10^{11}$  eV, gamma rays begin to produce showers in the earth's atmosphere which can be detected by ground-based detectors. In addition, it is possible to build detectors on earth that have large collection areas and good sensitivity for very low fluxes. Attempts to observe astrophysical objects by detection of air showers have been made for more than twenty years. For the most part, these efforts have been an offshoot of cosmic ray studies. In the energy range  $10^{11}$  -  $10^{13}$  eV, ground-based optical detectors can observe the Cerenkov light produced by the shower in the upper atmosphere. In the U.S., such detectors have been developed at the Whipple observatory, Haleakala and Albuquerque. For energies greater than  $5 \times 10^{12}$  eV, a sufficient number of shower particles reaches the ground so that they can also be directly observed by particle detectors. Sparse arrays of such detectors at moderate altitude are typically sensitive to shower above  $5 \times 10^{13}$  eV.

However, both techniques, the atmospheric Cerenkov technique, and the air shower technique, have a problem with severe backgrounds from ordinary cosmic rays, which arrive isotropically. The ordinary extensive air showers are initiated in the upper atmosphere by protons and heavier nuclei, but the air showers they produce are not easily distinguishable from showers produced by the gamma rays being sought. The technique which can separate the signal from the background has primarily been the identification of an excess of showers from a given source direction. If the source is identified to have a well-established periodic modulation in another wavelength region, one can search for the same periodicity in the gamma ray signal. Other methods to suppress cosmic ray background depend on the technique. For the atmospheric Cerenkov technique, the Cerenkov light is imaged on the focal plane of a mirror which follows the source. The Cerenkov image is quite different for a gamma ray shower from that of a hadronic shower. For the air shower technique, one exploits the fact that a gamma ray shower is expected to have at least twenty times fewer muons accompanying it than a proton-induced shower of similar size. The separation techniques are statistical in nature and encounter all the dangers of statistical fluctuations when the signals are weak. Only recently has a steady flow of gamma rays in the energy range  $(0.3 - 2.0) \times 10^{12}$  eV been convincingly observed from the Crab nebula with the 10 meter telescope at the Whipple Observatory. Figure 4 shows a view of the telescope. The imaging algorithms used improved the signal to background ratio by a factor of 50.

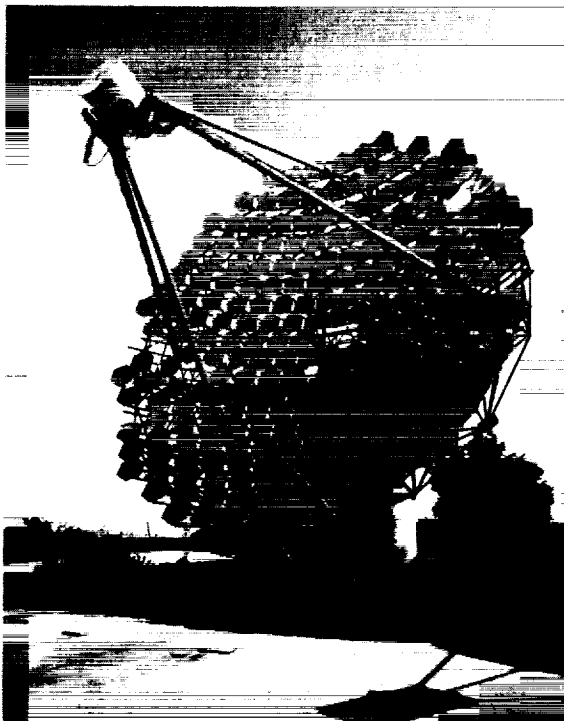


Figure 4

*The 10 meter Air Cerenkov detector of the Whipple Observatory.*



Air shower arrays have reported point sources of showers at energies  $>10^{14}$  eV from X-ray binaries such as Cygnus X-3 and Hercules X-1. A statistically significant observation of Cygnus X-3 over a four-year period was reported by the University of Kiel in 1983 and confirmed by Haverah Park, and the CYGNUS experiment at Los Alamos has reported a short burst from Hercules X-1 in 1987. Both results showed an excess of showers from the source as well as a periodicity that was characteristic of the source. In the case of Hercules X-1, the period is slightly different from that of X-rays. The most curious aspect of both observations was the fact that the muons accompanying the shower were not reduced as would be expected for a gamma-induced shower. Imaging of the Cerenkov light from lower energy showers coming apparently from Hercules X-1 and showing the same slightly displaced period, leads to the same puzzling conclusion that showers seem hadronic. Such observations are inconsistent with the expectations of particle physics for  $\gamma$  rays and, if confirmed, would require the existence of new, light, and strongly interacting particles or of dramatic new interaction thresholds. Furthermore, these results imply new compact acceleration mechanisms at the source. While radiation at  $10^{12}$  eV from the Crab nebula can be understood with conventional astrophysical mechanisms, radiation at  $10^{14}$  eV from compact objects, if it is gamma radiation, must come from neutral pions produced at the source. Given the exciting consequences which follow from the air shower observations, there are many groups throughout the world which are engaged in the search for point sources emitting radiation at energies greater than  $10^{14}$  eV. So far there have been no compelling confirmations of the Cygnus X-3 or Hercules X-1 results, a consequence, perhaps, of source variability. In the United States, two major installations at Los Alamos and Dugway, Utah are now starting operation. The expanded CYGNUS array is operating with 200 detectors covering an area of  $0.8 \times 10^5$  m<sup>2</sup>. By the end of 1990 an array built by the Universities of Utah, Michigan, and Chicago will be operating with 1089 detectors covering  $2.3 \times 10^5$  m<sup>2</sup>. These two installations are located at similar longitudes so that even short bursts of an object will be observable by both detectors. Figure 5 shows a view of the CYGNUS array. Whether a genuine astronomy will develop at energies greater than  $10^{14}$  eV depends on confirmation of the results discussed above. Detectors of sufficient sensitivity exist and it is a matter of time and patience before it will be known whether new instruments are required.

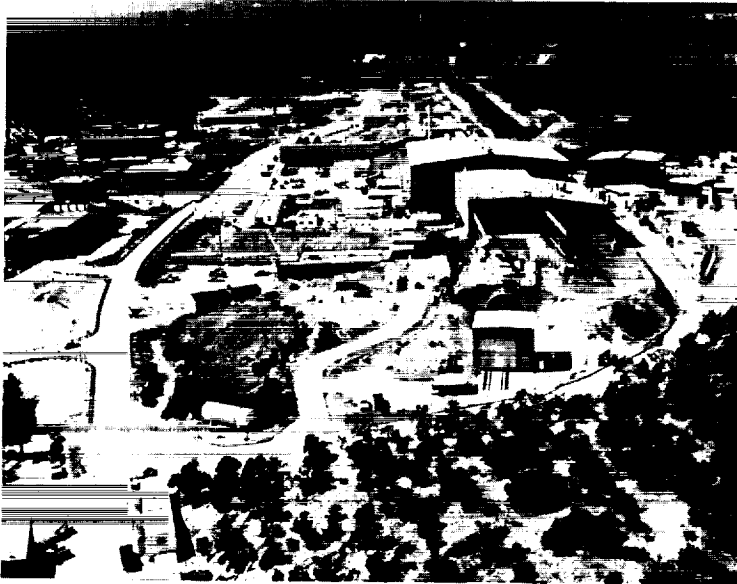


Figure 5

*The Cygnus extensive air shower detectors. The photo shows the LAMPF accelerator in the background with the beam stop area in the middle right. The CYGNUS counters can be seen on the roof of the beam-stop building and deployed in the surrounding area. Several counters of the expanded array can be seen in the immediate foreground.*

On the other hand, astronomy at  $10^{12}$  eV is truly beginning. At present, a second 10m telescope (GRANITE) is being constructed at the Whipple Observatory, which will greatly improve the sensitivity so that sources other than the Crab are likely to be seen. There are new technologies using large coverage or tracking detectors whereby the threshold of the air shower technique may be lowered to nearly overlap the atmospheric Cerenkov technique, especially if the detector is located at high altitude. One of these techniques involves the instrumentation of more than  $10^4$  m<sup>2</sup> of a clear lake with photomultipliers so that the observation of an entire air shower by Cerenkov light in the water is possible. The advantage of such a method is that its operation is not restricted to dark moonless nights.

It should be remarked that the American teams are currently mostly involved in arrays located in the northern hemisphere. Since most of the galaxy is visible only at southern latitude, it is likely that the need for southern instruments will arise. Because of its altitude and the fact that the same patch of the sky is always visible, the South pole may be particularly interesting. Currently a modest American-British array, SPASE, is pioneering this approach.

### *High Energy Neutrinos*

High energy neutrinos may provide another window onto acceleration mechanisms in the astrophysical environment, and if observed from point sources, would give a clear proof that hadronic processes play an important role. Such neutrinos are best detected by their production of upward-going muons. The neutrinos that pass through the earth may produce muons in the ground just below a large detector. One possibility is a water Cerenkov detector installed at the bottom of the ocean. Such experiments will have negligible background but the signal may be very small. The Dumand experiment recently approved by DOE represents a first attempt at exploring this virgin territory at a sensitivity sufficient to observe a few neutrinos from Cygnus X-3 if the continuous gamma fluxes are at the level reported by Kiel. Alternative techniques using the lake Cerenkov concept or the polar ice (as a Cerenkov light or microwave radiating medium) have been proposed.

If the existence of point sources radiating  $10^{14}$  eV gamma rays is confirmed, then this effort will become even more interesting. If a compact source produces neutral pions, it must produce charged pions as well. Decays of these pions will produce a powerful source of neutrinos. Depending on the details of the source, the flux of these neutrinos can greatly exceed the gamma ray flux since the neutrino absorption cross section is so small. Observation of both neutrinos and gamma rays from a source would provide important information about the mechanism of the compact accelerator.

It would seem prudent, however, to await either observation by DUMAND (or possibly MACRO or the LVD) of neutrinos from point sources or results from the Dugway and Los Alamos gamma ray detectors before proceeding with neutrino detectors of larger scale.

### *Cosmic Rays*

Cosmic rays which can be observed near Earth cover a large range of energies, from the MeV region to  $10^{20}$  eV, and comprise the nuclei of all known elements, as well as electrons, positrons, and antiprotons. The study of this tenuous plasma of relativistic particles addresses scientific questions closely related to the themes discussed above: high energy particle acceleration, stellar and galactic astrophysics, and cosmology and particle physics. However, contrary to the observational techniques reviewed thus far, cosmic ray measurements up to energies around  $10^{14}$  eV should be conducted in space or on high altitude balloons. It is for that reason that cosmic ray research in space is summarized separately in the following. We conclude the discussion with an account of the present status of air shower measurements from the ground which cover the range  $10^{14}$ - $10^{20}$ eV.

#### *Cosmic Rays from Space*

*Particle acceleration* is perhaps the most immediate scientific theme of cosmic ray studies. It is ubiquitous in nature, occurring in solar and stellar flares, interplanetary and interstellar shocks, pulsars, supernova explosions, and perhaps in shocks on the scale of entire galaxies. A major goal is the understanding of these cosmic accelerators and the determination of their energy sources and the physical mechanisms by which they are able to efficiently extract a small number of highly energetic particles from a nearly thermal distribution. The main observational tools available are studies of the energy spectra of the individual cosmic ray components over a wide energy range. Important progress was made during the 1980's by extending the energy spectra of major nuclei with measurements on balloons and from the Space Shuttle up to energies around  $10^{13}$ - $10^{14}$ eV/particle. The persistence of the power-law spectra of cosmic ray primaries over a large energy range lends support to the assumption that first-order Fermi acceleration in supernova-driven shocks acts as the prevailing acceleration mechanism. The spectral measurements also provided indications that during their propagation through the galaxy, the cosmic rays become enriched in heavy elements relative to protons at energies above approximately 10 TeV. Significant progress in understanding the mechanism by which particles are selected for acceleration resulted from the discovery that cosmic rays undergo fractionation which is strongly correlated with the particle first ionization potentials. The great

similarity of this fractionation to that occurring during the separation of the solar corona from the photosphere (Fig. 6) suggests stellar coronae may provide the pool of hot matter from which cosmic rays are extracted.

*The origin and evolution of matter* in the Galaxy is the major theme addressed by studies of the elemental and isotopic composition of cosmic rays. Since cosmic rays are much younger ( $\sim 10^7$  yr, as deduced from the abundances of radioactive nuclei such as  $^{10}\text{Be}$  and  $^{26}\text{Al}$ ) than the galaxy and the solar system ( $>10^9$  yr), the comparison between cosmic ray and solar system abundances elucidates the chemical evolution of the galaxy due to ongoing stellar nucleosynthesis processes. Measurements on balloons, followed by more precise data from spacecraft, have led to the first high resolution observations of the isotopic composition of heavy primary cosmic ray nuclei, and to the discovery of excesses of the neutron rich isotopes of Ne, Mg, and possibly Si (relative to solar system composition), indicating differences in the nucleosynthesis history of these two samples of matter. The first reliable measurements of abundances of ultraheavy nuclei above the iron group ( $Z \geq 30$ ) demonstrated that both slow and rapid neutron capture nucleosynthesis contribute to these elements, and that the heaviest nuclei are not produced solely by recent explosive nucleosynthesis as might occur if cosmic rays were both synthesized and accelerated in a supernova.

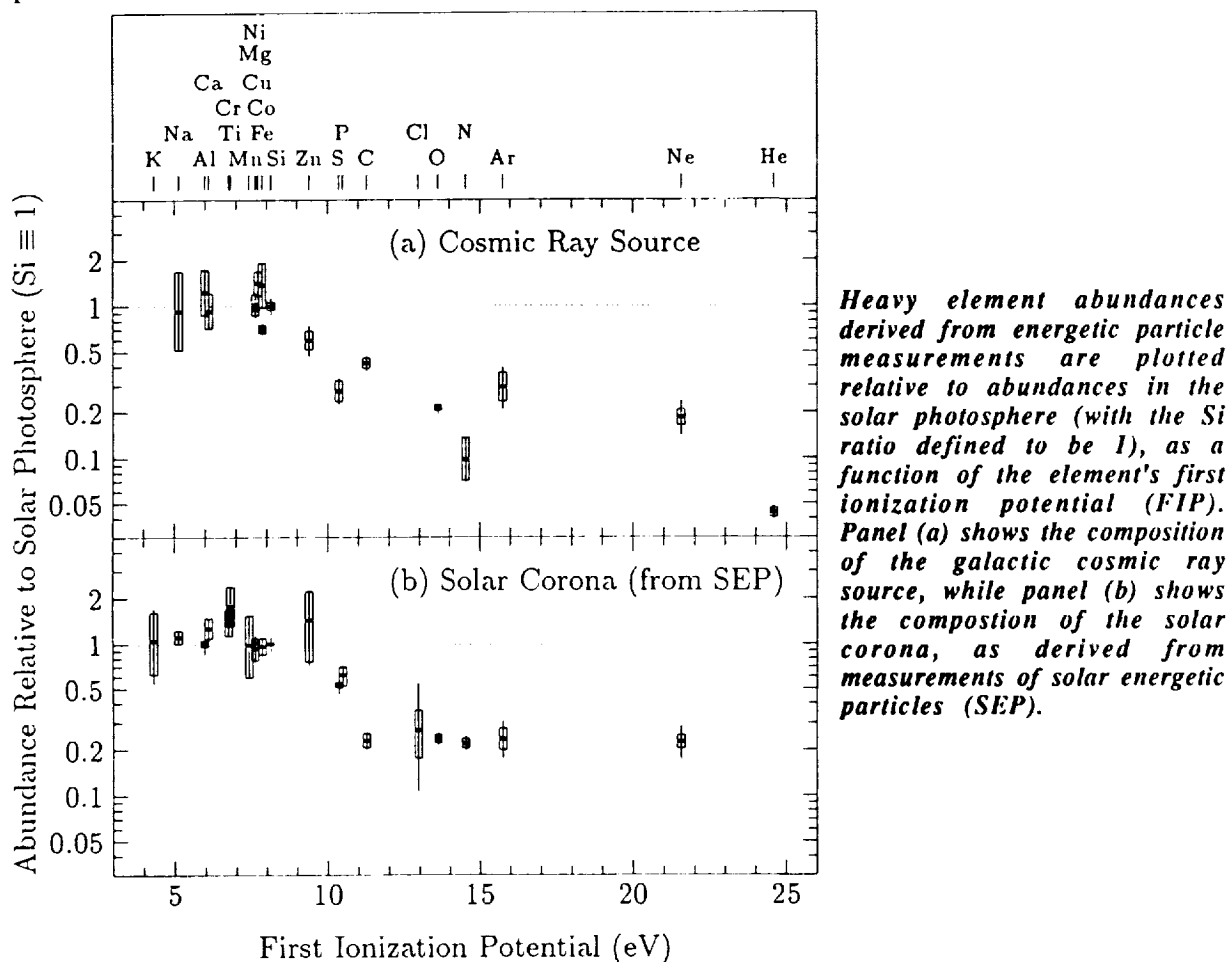


Figure 6

*Observational tests of cosmology* constitute another active area for cosmic ray investigations. For instance, the determination of the contribution of cosmic ray-induced spallation reactions to the production of light isotopes, particularly  $^7\text{Li}$ , is essential for deriving the cosmogenic yields of these species and establishing the baryonic density in the universe. In the search for antiparticles in the cosmic rays, recent balloon measurements have placed stringent limits on possible contributions of annihilations of supersymmetric dark matter particles to cosmic ray antiprotons at low energies. However, in the GeV region, there are indications of fluxes of  $\bar{p}$ 's and positrons that are larger than those expected to arise from interstellar nuclear interactions of cosmic rays. It remains

to be determined whether these excesses are indicative of a cosmological source, or are due to peculiar phenomena in the galaxy, for instance  $e^\pm$  pair production in the magnetospheres of pulsars.

*The interactions of cosmic rays in interstellar and interplanetary space* are probed by particles of the lowest energies, below a GeV/nucleon. Particle acceleration can be studied in the heliosphere in much more detail than in more remote regions of the galaxy, and will provide invaluable tests for theoretical models that are applicable at larger scales and higher energies. In order to establish the role of low energy cosmic rays in the heating and ionization of the interstellar medium, *in situ* measurement of these particles will be required in local interstellar space, as pointed out in the 1980 Astronomy and Astrophysics Survey Report (Field Report). An "Interstellar Probe" mission to carry out such measurements remains a high priority objective in this field, but will not be discussed in detail here since it falls outside the scope of the present study.

This brief discussion illustrates the importance of cosmic ray observations in space for the understanding of a variety of astrophysical phenomena relevant to fields ranging from cosmology to radio and gamma ray astronomy. In the coming decade, major advances are expected in a number of areas:

*Antimatter studies.* High precision observations of antiprotons and positrons over a wide range of energies with magnetic spectrometers on balloons and in Space ("Astromag") should make it possible to determine conclusively whether there is a significant contribution of these particles from a source other than interstellar interactions of cosmic ray protons. This will lead to improved limits on the production of  $\bar{p}$  and  $e^+$ 's by the annihilations of candidate dark matter particles. In addition, the sensitivity of searches for heavy antinuclei should be improved by 2 to 3 orders of magnitude.

*Isotopic composition studies.* The abundances of essentially all stable and long lived isotopes of elements with  $Z \leq 30$  should be measured, and exploratory isotope observations should be extended up to  $Z \approx 40$ . These data should clarify the pattern of isotopic anomalies, making it possible to determine the dominant nucleosynthesis process contributing to the production of cosmic ray source material. The observations made in particular with the Advanced Composition Explorer should establish the time between nucleosynthesis and acceleration, using the abundances of primary electron capture nuclides such as  $^{57}\text{Co}$ . With the Astromag magnetic spectrometer, isotopic separation will be extended to GeV energies, making it possible, for instance, to use radioactive "clock" isotopes in a regime where their half lives are increased by relativistic time dilation.

*Ultraheavy element studies.* Abundances of individual elements with  $Z > 50$  should be measured with passive track detectors (by the Heavy Nucleus Collector) with good statistical accuracy and resolution sufficient to separate adjacent elements. These will make possible studies of neutron capture nucleosynthesis and determination of the relative importance of steady state and explosive processes in the production of cosmic ray source material.

*Studies of high energy spectra and composition.* Direct measurements of the spectra of major elements at high energies can be achieved through exposure of large instruments in space. By exposing detectors of several  $\text{m}^2$  ster in space for at least a year, direct composition measurements can be extended to  $10^{14}$ - $10^{15}$  eV/particle. Such measurements will not only test models for particle acceleration at high energies, but will also provide an essential overlap and calibration for the indirect measurements by ground based air shower arrays that provide information up to the highest particle energies known in nature. This could be accomplished by a one-year flight of the transition radiation detector system that was successfully tested on the shuttle (Spacelab-2) in 1985, or by new instrumentation to be developed for attachment to the Space Station.

*Studies of interaction with the solar system.* A number of space missions selected for flight in the 1990's will measure the nuclear composition and atomic charge states of solar energetic particles and anomalous cosmic rays. These should significantly improve our knowledge of the composition of the solar surface, and should conclusively establish the origin of the anomalous component. Information on the three dimensional structure of the heliosphere and its role in the modulation of galactic cosmic rays will be greatly improved through particle observations at high heliographic latitude by the Ulysses mission, and by ongoing studies using the Pioneer and Voyager space probes in the outer heliosphere. Investigation of differences in the modulation of particles of opposite charge sign, particularly electrons and positrons, will also contribute to understanding the physical processes responsible for the solar modulation of cosmic rays.

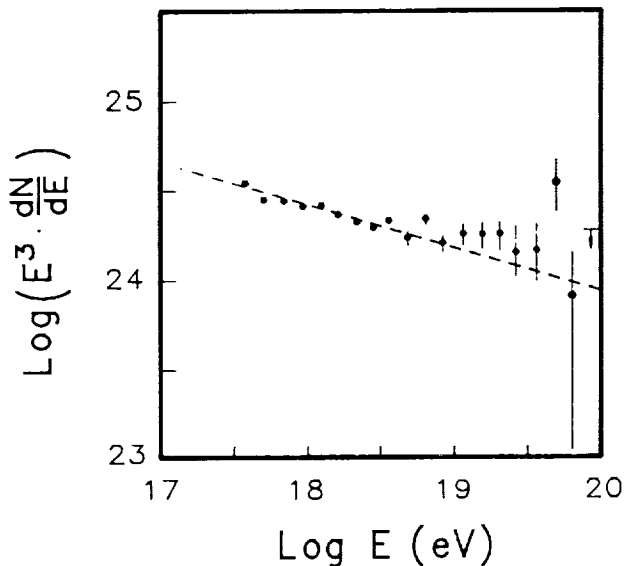
### *Cosmic Rays - Ground Observations*

While cosmic rays with energies below about  $10^{14}$  eV/particle may be best studied by experiments in space, the low flux at greater energies limits studies to the observation of the showers they produce in the atmosphere.

Their energy spectrum is rather well known, falling from  $10^{-4}/\text{m}^2/\text{sec}$  for energies greater than  $10^{14}$  eV to  $\sim 1/\text{Km}^2/\text{year}$  for energies above  $10^{19}$  eV.

The highest energy region is particularly interesting. The mere existence of cosmic rays up to  $10^{20}$  eV is rather surprising and there is at present no understanding of a process which could accelerate or produce directly particles of such energy. Moreover, at energies greater than  $10^{19}$  eV there are sufficiently distinctive phenomena which should permit the identification of the source of the radiation (galactic or extragalactic) and the identity of the radiation, protons, heavy nuclei, or gamma rays. The radius of curvature of a proton of energy greater than  $10^{19}$  eV in the galactic magnetic field is larger than 10 Kpc, which is comparable to the size of galactic disk. Heavier nuclei will have correspondingly smaller radii of curvature. In addition, if the cosmic rays at this energy consist of extragalactic protons, their spectrum should have a sharp cutoff at  $10^{20}$  eV because of photon-pion production on the 2.7 K background radiation (Greisen cutoff). Below this cutoff, the spectrum is expected to flatten because of the pile-up of particles produced in these interactions.

The Fly's Eye experiment of the University of Utah has collected the largest number of events (200) with energy greater than  $10^{19}$  eV. Their technique of observing the cosmic rays by their fluorescence in the atmosphere allows many details of the induced shower to be observed. The direction, energy, and longitudinal development of the shower can be measured. The longitudinal development permits incident gamma rays, protons, and heavier nuclei to be distinguished on a statistical basis. The energy spectrum of the upper end of the spectrum is shown in Fig. 7. It suggests that there is a flattening of the spectrum above  $10^{19}$  eV and a possible cutoff based on the absence of events at energy higher than  $10^{20}$  eV. These cosmic rays arrive isotropically and the longitudinal development suggests that they are protons. Thus, there is some evidence that these cosmic rays come from outside our galaxy. Other groups using conventional extensive air shower techniques dispute these findings but their sample is also small and their energy resolution worse.



*Differential energy distribution multiplied by the cube of the energy, for the highest energy cosmic rays as observed by the Fly's Eye experiment.*

Figure 7

A new proposal to build a Fly's Eye with ten times the sensitivity has been made to the NSF. This instrument would have much finer resolution for the measurement of the longitudinal development of the shower. Measurement of the depth of maximum of the shower and its longitudinal extent would permit a clear separation between incident protons, heavy nuclei such as iron, and gamma rays. Gamma rays with energy greater than  $5 \times 10^{19}$  eV could be observed by the Landau, Migdal, Pomeranchuk effect whereby at these high energies the mean free path for pair production and Bremsstrahlung is greatly increased. Thus, by a combination of the measurement of the spectrum, composition and anisotropy it would be possible to establish the galactic or extra-galactic nature of these cosmic rays. The ability to collect over several years 2000 events with energy greater than  $10^{19}$  eV would make this determination possible.

Cosmic rays in the  $10^{15}$ - $10^{19}$  eV region also present mysteries. They may be galactic in origin but there is no established galactic acceleration mechanisms that can produce protons with energies greater than about  $10^{15}$

eV. The existing air shower arrays referred to in the gamma ray section, or the combination of surface arrays with underground detectors should be able to make crude composition measurements which will help answer these questions. As mentioned above, cross calibration with space instruments will be essential. One would also like to know whether the galactic cosmic rays are produced in localized compact sources, or are accelerated slowly over much larger regions in the galaxy. The observation of discrete gamma ray sources at energy greater than  $10^{14}$  eV may resolve this question, since a discrete source implies the production of neutral pions by considerably higher energy primary particles.

### Highest Scientific Priorities

From the discussion above, we can extract the most likely themes in the next decade:

- The nature of dark matter and the possibility that it is made of nonbaryonic particles produced in the early universe. More generally, the imprints left by physics at ultrahigh energy on the universe are fundamental to the understanding of both cosmology and particle physics.
- The solar neutrino problem and the use of neutrinos to understand supernovae. In the process, stellar models may be refined and the intrinsic properties of neutrinos will also be better delineated.
- The nature of particle acceleration mechanisms, especially if gamma rays are indeed observed as high as  $10^{14}$  eV. Confirmation of a muon anomaly would require major revisions of particle physics. In any case, detection of gamma rays in the  $10^{12}$  eV region will complement observations at longer wave length.
- The origin of cosmic rays, their composition, the nature of their acceleration mechanisms and the possibility that they may partially be extragalactic.

We can be assured that other concepts and fascinating questions will be generated along the way, but these problems are useful tools to define the highest scientific priorities.

### Implementation of the current program

The highest priority is to implement and strengthen the current program. As explained above, very important experiments are coming on line or have just been approved which will shed critical light on the scientific problems tackled, while small developments explore the feasibility of new techniques. It is to the credit of the funding agencies and to the community that this represents a good first approximation for a balanced program and *it is critical that these experiments and developments be vigorously supported at the fastest rate feasible.* Concurrently a *strong theoretical effort* should be continued. In order to expand on these points, we reiterate the fields considered above:

#### Particles and Cosmology

- Although they may not yet have the required sensitivity to probe the entire available parameter space, first generation experiments attempting to detect dark matter particles (axions and weakly interacting massive particles) are essential to explore an already large class of models, develop the technology and master the experimental problems. The support of the present low cost efforts both by NSF and DOE should be continued and expanded. For axions, the current technological development should be aggressively pursued and a very interesting next step would be to extend the sensitivity of the search to the low mass region. It is also important to continue the weakly interacting massive particle searches with improved ionization detectors (possibly with isotopically enriched materials to improve the sensitivity to Majorana particles) and eventually with the transformation of these set ups into pilot experiments using cryogenic detectors of a few tens of grams. Indirect searches for dark matter particles with existing neutrino detectors [mostly DOE] and the antiproton, positron and gamma ray observations [NASA] should also be actively pursued.

- MACRO [supported by DOE] is expected to reach an important milestone in the search for monopoles with a sensitivity below the Parker bound and to improve significantly our searches for high energy neutrinos potentially produced by annihilation of dark matter particles in the sun or the earth. More generally, its large volume and area allows this instrument to detect supernova neutrinos and high energy muons from cosmic rays. Similarly the LVD [supported by NSF and DOE] will begin in late 1991 to contribute to these studies.

- In the past ten years, theory has led the way in establishing the bridge between particle physics and cosmology. It is critical for this effort to be expanded at the time when experiments begin to be implemented.

*Particles and Stellar Physics*

- The highest priority for the United States solar neutrino program [DOE] is the participation in the Sudbury Neutrino Observatory (SNO). This experiment will produce a high counting rate study of  $^8\text{B}$  solar neutrinos, including the energy spectrum of the electron type neutrinos and a measurement of the total flux of neutrinos independent of their type.
- Participation of the US teams in the gallium experiments [DOE] is important, since their results will determine a portion of the overall picture of the solar neutrino problem.
- It is important to keep the  $^{37}\text{Cl}$  experiment running over the long term, until a larger  $^{37}\text{Cl}$  detector planned by the Soviet Union becomes operational.
- It is critical to organize a well coordinated supernova watch. The detectors currently running or being constructed [Kamiokande, IMB, MACRO, and SNO all funded by DOE, and LVD funded by NSF and DOE] have a mass well suited for the detection of neutrinos from supernovae in our own galaxy. It is important to maintain IMB in operation at least until MACRO fully comes on line.
- Support of the theoretical efforts in understanding the sun and the supernova explosions are obviously an important complement of this experimental program.

*High Energy Gamma and Neutrino Astrophysics*

- The installation at the Whipple Observation of a second atmospheric Cerenkov detector [GRANITE supported by DOE] is an important step in the observation of gamma rays at  $10^{12}$  eV. This region is particularly interesting, with the only unambiguous observation of a steady flux from a high energy source, the Crab nebula, and the expectation that many other sources may be observable in this energy region where the flux is expected to be larger than at higher energies. Moreover such studies will complement nicely the observations in space made by the Gamma Ray Observatory, which will be sensitive up to  $10^{10}$  eV.
- Two large extensive air shower arrays are currently being put in operation: the Chicago-Michigan-Utah array [NSF-DOE] and the expanded Cygnus array in Los Alamos [NSF and Los Alamos (DOE) discretionary funds]. They will be critical in clarifying the observational situation in the  $10^{14}$  eV region: existence of pulsed sources and muon content anomaly. Since they are located at approximately the same longitude, the two experiments will be able to cross check each other.
- DUMAND II is a useful first exploration of the virgin field of high energy neutrino astrophysics and an important pilot implementation of a technology which, if successful, could be extended to a larger size, if this is required for the study of neutrino sources.

*Cosmic Rays*

High priority should be given to carry out expeditiously the space missions recently selected for cosmic ray investigations:

- The Advanced Composition Explorer (ACE) will measure isotopic, elemental and charge state composition over six decades of energy (0.5 keV/nucleon to 0.5 GeV/nucleon).
- The Astromag facility selected for the Space Station Freedom, will be a unique facility with a large magnetic spectrometer which should allow major advances in the study of cosmic antimatter components and isotopic composition over a wide range of energies.
- The Heavy Nucleus Collector (HNC), another Space Station payload, will make the first high resolution determinations of the abundances of individual ultra-heavy elements with  $Z \geq 50$ .
- The POEMs experiment on the Earth Observation System, will measure spectra of electrons and positrons below 1 GeV, and determine the relative contributions of primary and secondary production.

These space observations have to be complemented at the highest energy by observations from the ground:

- Composition studies in the  $10^{14}$  -  $10^{16}$  eV region are natural by-products of the gamma ray experiments mentioned above and of the MACRO [DOE], LVD [NSF-DOE], Homestake and Soudan 2 [DOE] underground experiments which are operated in connection with surface arrays.

• In addition, important new information is being collected at very high energy by the Fly's Eye detector [NSF] which has been a recognized experimental success and should continue to be supported.

*New Initiatives in the coming decade*

The program described above is well balanced with a number of new experiments. However, the scientifically essential *supernova watch* requires a better coordination, and the proper exploration of the *highest energy cosmic rays* and the elucidation of the old puzzle of their origin requires a new instrument. In addition it is clear that the experiments currently implemented and the present technological developments will lead to major new initiatives, the character of which will greatly depend on the results obtained in the intervening time. In charting the future of particle astrophysics it is then essential to take into account *decision points* that we see occurring naturally in a few years when the following information is obtained: feasibility of cryogenic detection of dark matter particles, nature of the solar neutrino problem, existence of sources at  $10^{14}$  eV. This is summarized in table I.

Table I

**Potential New Initiatives**

Potential New Initiative	When	Cost (\$M)	Information Necessary	Technological Developments
Supernova Neutrino Watch	Now	1/yr		Long term maintenance
$10^{20}$ eV cosmic ray facility	Now	15		High resolution Fly's Eye
Full Size Dark Matter Experiment	1993	5-15	Feasibility of cryogenic detection techniques	Cryogenic Detectors
New Generation of Solar Neutrino Detector	1993	10-30	Results of Gallium experiments	Cryogenic and Scintillator Techn. High Rate
Gamma ray facility in the $10^{11}$ - $10^{13}$ eV region	1993	10	Abundance of sources	Stereo operation of air Cerenkov
Gamma ray facility in the $10^{12}$ - $10^{16}$ eV region	1994	30	Finding $10^{14}$ eV sources (with present large arrays)	Lake technology

**Essential Technological Developments**

Technology	Recommended funding levels (\$M)
Cryogenic Detectors for Dark Matter Searches	1/yr in addition to present ~ 1/yr
Cryogenic and Scintillator Techniques for Solar Neutrinos	1-1.5/yr
Lake Cerenkov Technology for Extensive Air Shower Detectors	2.5-5 (1 prototype)

*Immediate recommendations*

The two projects that we recommend for immediate action are the systematic organization of a *supernova watch* and the construction of the *High Resolution Fly's Eye*, currently proposed to the NSF, provided the technical review of the instrumentation and the institutional arrangements is favorable. The first project involves mainly the continuation of the present support to existing instruments at least until new ones of similar supernova detecting capabilities come on line. The cost involved in this transfer period will be minimal. In the long run,



however, the support for this activity may have to increase since the low rate of galactic supernovae may require the long term maintenance and regular upgrading of detectors not designed initially to be operated for more than a few years. We will come back to the institutional problems involved. The second project is technically ready to go, has a capability of collecting ten times more events than the present instrument, and a multi-university collaboration has been established. Prototype mirrors and detectors have been built. The cost of the instrument is about \$15M. It has the capability with several years of operation to observe a Greisen cut off, to determine the extragalactic origin of the particles, and to establish whether they are protons or heavier nuclei. The instrument will be capable of observing cosmic rays down to  $10^{16}$  eV, and it is likely that at a later date ancillary detectors will be added to enhance the capability of the instrument.

For space-borne investigations, the frontiers will remain in the areas of the rare ultraheavy cosmic ray nuclei (up through the actinides), and of direct composition measurements well above TeV energies. The former are essential for understanding the roles of steady state and explosive nucleosynthesis processes in the galaxy, and the latter are necessary to unravel finally the mystery of cosmic ray acceleration. It will be important that NASA develop both an Ultraheavy Nuclei Explorer Mission and a Trans-TeV Particle Composition Mission.

### *Future Initiatives*

The other initiatives which are presently being considered in order to tackle some fundamental scientific issues of particle astrophysics require more scientific or technical information before a responsible decision can be made. We can see major regions where such initiatives are likely.

- The nature of dark matter is one of the most pressing scientific problems today, and the discovery that it is made of nonbaryonic particles would be of extremely fundamental significance. Unfortunately the level of the present technology for the *direct detection of dark matter particles* prevents us from launching the definitive experiments now which would fully test the particle hypothesis. We have to rely on a step-by-step strategy based on the use of existing technologies to probe part of the parameter space while developing the new techniques necessary for a full exploration. This applies both for the axion search where new methods are needed to explore the high mass region, for instance using ferrite loaded detectors, and for the weakly interacting massive particles. In this case, the feasibility of building phonon or quasi-particle detectors with sufficient energy sensitivity and powerful background rejection and the practicalities of reaching very low radioactive background in low temperature refrigerators must be proved by the pilot experiments mentioned above before a full size experiment could be implemented. However, if and when the technology can be firmly demonstrated, it will be important to move rapidly to the implementation of such experiments. The cost may be in the 3 to 5 million dollars range per experiment. In addition, if present underground laboratories appeared unsuitable, a modest, dedicated ultra-low background facility might have to be built at a cost not expected to exceed \$10M.

- The gallium experiments and the measurement of the neutrino flux as we enter a period of enhanced solar activity are likely to tell us a great deal about the *nature of the solar neutrino problem*. It is quite natural that a few years from now new proposals will be made which attempt to check the insights thus gained. There again, it will be important to answer rapidly the new round of questions, whether they concern particle physics or astrophysics. We could imagine detectors in the range of 10 to 30 million dollars but their precise type will depend on whether high rate, energy resolution or threshold will be the most important required feature. We estimate that a design for the next generation of detectors may be ready by 1993.

- The third area where a major initiative is likely is that of *gamma ray observations in the  $10^{11}$ - $10^{13}$  eV region*. The scientific interest of this region has already been pointed out. The atmospheric Cerenkov technique has demonstrated the necessary background rejection capability at  $10^{12}$  eV, but it remains to be seen with the second telescope being installed at Whipple Observatory how a multiple mirror system behaves and how rich the sky is at TeV energies. A decision for a large array of Cerenkov mirrors can be taken as early as 1993. The cost may be of the order of 10 million dollars.

- If the current investigations at  $10^{14}$  eV show that new phenomena are indeed occurring at this energy, a *careful study of the region  $10^{13}$ - $10^{16}$  eV with more powerful instruments will become essential*. For that purpose a major facility using the lake water Cerenkov technique may be particularly interesting as it would allow full analysis of the extensive air shower. It may be advantageous to locate it at high altitude in order to extend its sensitivity down to the lowest energy possible, to overlap with the Cerenkov technique and map in detail a potential transition region. However, *this panel believes that a decision to construct such a facility will have to wait for the confirmation of the existence of sources in the  $10^{14}$  eV region and the demonstration at the prototype scale*

of the water Cerenkov technology applied to extensive air shower measurements. In any case, it is very likely that a powerful ultra-high energy gamma ray facility will become essential. A decision may be made in 1994. Depending on its exact scope and on the technology or combination of technologies used, the cost may be of the order of 30 million dollars.

- With the approval of DUMAND II, there is no immediate need for an additional high energy neutrino instrument. However another type of detector (such as a deep lake detector) may have to be considered in a few years in the case the deployment of a few phototubes strings by the DUMAND II team fails to confirm the practicality of operating a detector in the deep ocean. By that time, a confirmation of  $10^{14}$  eV point sources showing that hadronic processes are important may have been obtained and would provide a better estimate of the size needed for a neutrino observatory.

- We should acknowledge that the last two points are not accepted by the team which is proposing the GRANDE detector, including several of its members who are serving on this Panel. The proposed instrument is a deep lake water Cerenkov detector, located at low altitude in Arkansas. It would simultaneously study  $\gamma$  rays between a few TeV and  $10^{15}$  eV with excellent muon coverage, angular resolution and background rejection and neutrinos above 6 GeV in a more hospitable environment than the deep ocean. The proposers argue that whatever the results of the large arrays currently being set up, an instrument like GRANDE would have to be built, and that in the general interest of the field, no time should be lost. After thorough discussions and in spite of the power of the proposed detector and the breadth of the physics tackled, this panel concluded that it is preferable to wait for the results of the current large arrays before starting the construction of a new major instrument, which could then be optimized to follow up on the findings of the previous generation detectors.

#### *Longer Term*

In the longer term (beyond this decade), it is of course more difficult to predict the evolution of the field. The future depends so much on what happens during the next ten years that it is impossible to guess the discoveries and what will be the most exciting in the long term. These are some of the items discussed:

- Cosmology is likely to remain an extremely active field of study. But ideas are evolving rapidly, and radically new windows may be opened onto the early universe. It is therefore not very useful to attempt to guess in which direction we will be going. However, the existence of a solid theoretical effort and a strong experimental community with powerful techniques at its disposal, spanning the various observational fields from radioastronomy and optical methods to particle physics techniques, is the best guarantee that any breakthrough will be exploited rapidly.

- In the field of particles and stellar physics, the detailed study with neutrinos from the interior of the closest star will certainly continue. In 10 years we may not yet have seen a supernova from our own galaxy but it will still be vital that observations continue so that such a rare and important event would not be missed. The greatest challenge that we can presently conceive in this line of research is probably the development of techniques capable of reaching the tens of megatons necessary for supernova neutrino detectors to be able to detect supernovae in the Virgo cluster. Such instruments would permit us both to study many collapses and to put precise limits on the mass of all types of neutrinos, an enterprise extremely important for cosmology and particle physics.

- The future of high energy gamma ray and neutrino astronomy depends to a large extent on the confirmation in the present decade of the existence of very high energy sources. If they do indeed exist, we can envision a long program of powerful gamma ray and neutrino observatories, probably with some installations in the southern hemisphere and the support of large international collaborations.

- A dedicated Interstellar Probe mission that would rapidly leave the heliosphere would provide invaluable *in situ* measurements of the nearby galactic environment.

- The deployment of very large orbiting detectors, 100-1000 m<sup>2</sup> ster, with exposure of several years, may allow us to observe directly very high energy primaries (charged and neutral) and allow in particular a direct study of the composition of cosmic rays near the spectral knee ( $10^{16}$  eV). Measurement at these very high energies may be done with massive calorimeters recording the cascade generated by interactions. One promising possibility is to develop such an instrument at a lunar base, where most of the required mass would come from the moon itself.

### Essential Technological Developments

With the results of present developments and experiments, major branch points in the scientific strategy of particle astrophysics will present themselves in the coming three to five years. It is essential to prepare the technologies likely to be needed and to study now the necessary facilities so that we can proceed with the next generation of detectors as soon as the scientific questions are clarified. Following the discussion of the last section, the items of highest priorities are:

#### *Cryogenic Detectors of Particles.*

As argued above, answering the central question of the nature of dark matter requires the development of new detection methods. For the specific case of weakly interacting massive particles, the technique would have presumably to be based on the breaking of Cooper pairs in superconductors or the production of phonons in materials at very low temperature. Reviewing the level of support of this activity in this country (around \$1M for massive detectors shared nearly equally between DOE and NSF), this Panel is struck by the disparity with the European development funded at a level approximately 4 times higher. Moreover some development areas such as that of tunnel junctions are nearly absent in the US, while they are fairly successful in Europe. *We recommend multiplying the US funding level by at least a factor of two and involving more institutions* so that within 2 to 3 years, pilot experiments could effectively attempt to demonstrate the radioactive background rejection and the redundancy of these methods and compare various technologies. This is an essential element if we are to install full size experiments featuring the 5 to 10 kg detectors necessary to probe the existence of supersymmetric particles as a significant dark matter component.

#### *New Solar Neutrino Techniques*

As the nature of the solar neutrino problem is being unravelled by the present generation of detectors, it will be important to have at our disposal technologies able to tackle rapidly whichever aspect emerges as the most important then: high rate, energy spectrum, low or high energy region.

Two directions need to be actively pursued:

- *Cryogenic techniques* similar to those developed for dark matter. An interesting possibility, given the large mass of detector necessary, is the use of  $^4\text{He}$ , presently being developed by Brown University, in particular if it is important to measure the spectrum of pp neutrinos. This effort must be sustained.

- *Scintillator techniques* utilizing as target electrons  $^{11}\text{B}$  or possibly  $^{115}\text{In}$  in the scintillating material. The main problem appears to be the control of their radioactive background and a strong development effort should be directed to that question.

#### *New Extensive Air Shower Detectors*

The lake water Cerenkov detector technology is a radically new technique which exploits the experience gained with water Cerenkov proton decay detectors and potentially offers several advantages with: full calorimetry which may allow excellent hadronic shower rejection, or if necessary the study of the muon anomaly; excellent angular resolution; and low threshold especially at high altitude. This last feature could make this technology competitive with the atmospheric Cerenkov technique, with the additional advantages of an "all" sky sensitivity, continuous operation and the possibility of detecting sources with short duty cycles (if such exist).

We believe, however, that this very interesting technique initially proposed by the GRANDE collaboration should be tested at a small scale before being fully deployed. We therefore recommend *operation of at least one prototype instrument (if possible at high altitude), at a modest funding level* in order to evaluate the potential of the technique. This technology may become critical at the decision point which is likely to appear within 3 to 4 years, when the situation with  $10^{14}$  eV gamma ray sources is clarified by instruments that are currently being constructed. The experience gained with smaller scale water detectors may be invaluable for the optimization of a full size lake facility. In addition, modest developments of other potentially useful technologies such as tracking detectors should continue.

### *An Active Balloon Program*

The NASA balloon program produces important scientific results and supports the development of new instrumentation for future space flights. Balloon-borne experiments play a vital role in the education and training of students and young scientists. Recent improvements in the reliability of heavy-lift balloons and the initiation of efforts to provide long-duration flights around the globe or at the South Pole promise to further enhance the scientific output. An adequate level of support of the balloon program and its technical staff as well as enhanced funding for state of the art detector development are essential.

### **Critical Institutional Issues**

Particle Astrophysics attracts a growing number of physicists, reaching 200 experimentalists in the United States alone, while the cosmic ray astrophysicists working in space represent a well structured and organized community. Increasingly these two communities overlap. A certain number of institutional issues will become critical in the coming decade.

### *The Funding of Particle Astrophysics*

Particle Astrophysics happens to be in a very peculiar situation. If one excludes traditional cosmology observations funded mainly by NSF (Astronomy) and NASA, and the cosmic ray studies in space which have been a traditional element of the NASA scientific program, the other observational fields covered by this panel have been supported by other divisions of NSF (Physics, the Polar Program, and the Science and Technology Office--with the Center for Particle Astrophysics in Berkeley), and by two divisions of DOE (High Energy Physics mainly for high energy astrophysics, and Nuclear Sciences for solar neutrinos).

These new funding sources have helpfully substituted for the traditional channels which appeared saturated, and they have supported instruments which have made some of the most significant astrophysical discoveries of the decade (e.g., the direct measurement of neutrinos coming from the sun and the neutrino burst from SN1987A). They have facilitated naturally the evolution towards astrophysics of a different physics community (mainly particle physicists). Moreover agencies such as DOE are particularly well equipped to handle large facilities.

In spite of these strengths, this funding situation has significant weaknesses. There is a clear danger for multidisciplinary efforts such as particle astrophysics to "fall between the cracks". Part of the problem may come from ambiguities in the perceived mission of the funding agencies. In particular DOE has historically a clear responsibility in particle physics but its involvement in astrophysics does not appear to have been fully accepted. As the distinction between particle physics, cosmology or astrophysics becomes increasingly blurred, this Panel feels strongly that *too narrow an interpretation of particle physics would be detrimental to scientific productivity; we suggest that the responsibility of DOE in particle astrophysics should be clearly affirmed.* Funding decisions of proposals should then be based on their scientific merit, the techniques used and the scientific priorities recommended by the advisory bodies, without attempting to define too rigidly what is particle physics and what is astrophysics. For instance, it will not be productive to argue that a supernova watch is a purely astrophysics problem; most of the current neutrino detectors are DOE supported, and it is clear that the detection of a supernova in our galaxy would also provide crucial information on neutrino properties.

Coordination between the various funding decisions is also difficult in this new field. High Energy and Nuclear Sciences decisions at DOE have traditionally relied heavily on the accelerator laboratories which, with their respective program advisory committees, filter proposals and set up priorities and long term policies. Such an intermediate structure does not exist in particle astrophysics. Small to medium cost projects are judged through conventional mail review while larger ones are evaluated by ad-hoc committees. But in most cases, the reviewers do not have access to the broader picture, where it is necessary to maintain the balance between experiments of widely different costs but similarly fundamental interests or between short term endeavors and long range technical development.

### *Recommended Funding Mechanisms*

This analysis has led this Panel to make the following recommendations:

- *A Particle Astrophysics Advisory Structure.*

There is first a clear need for the establishment of a *Particle Astrophysics Advisory Panel* which could advise the federal agencies involved in Particle Astrophysics (NSF, DOE and maybe NASA) on the relative scientific priorities and help them set up long term policies. Such a body will certainly help proposals not to fall between the cracks, and will be critical in the decisions at the decision points that we identified above. Consultation with the community and discussions with some of the agency officials have shown a surprising level of consensus on the need for such a committee!

In order to guarantee coordination of priorities, this specific advisory structure should be organically linked to any general advisory structure for astrophysics. For example, its chair may be a member of a general Astronomy and Astrophysics Committee, if such a committee is formed. Similarly, official links to the High Energy Physics Advisory Panel (HEPAP) and to the Nuclear Science Advisory Committee (NSAC) and the relevant NSF and NASA advisory committees will have to be established. However, the specificity of the Particle Astrophysics community, the operational methods of the agencies involved, the multidisciplinary aspects of the program and the cost of large observational facilities are strong reasons for organizing a specific advisory channel.

- *A multidisciplinary approach to cosmology.*

Funding of multidisciplinary research is always difficult and particle astrophysics offers a typical example. The problem has been particularly acute for cosmology which appears to have been underfunded in the last decade both for experiments and theory. This suggests that the present funding institutions were unable to adapt to the emergence of a new discipline which cuts across many observational fields (including particle astrophysics). The scientific importance of the field, its philosophical implications and its appeal to the public at large require a much better coordination (among the various observational fields) and an improved general level of funding. This will require innovative institutional schemes bringing together astrophysicists of several observational fields and particle physicists, theorists and experimentalists alike. The theoretical astrophysics group at Fermilab is a recognized success and we encourage the creation of an experimental counterpart. It will be interesting to watch the evolution of the new NSF Center for Particle Astrophysics at Berkeley in its attempts to focus resources and expertise from many disciplines, institutions and funding origins on the problem of dark matter. It is conceivable that several such multidisciplinary center-like cosmology institutions nation-wide, complementary to individual PI grants rather than in competition with them, could significantly contribute to the implementation of the high scientific priority of cosmology and the coordination advocated above.

- *Balance between long range projects and short term scientific opportunities.*

In our above description of the future of the field we have focused on major facilities. It is important however to maintain a balance between a long range program, and the rapid exploitation of scientific opportunities when they emerge. This requires in particular a large enough financial "reserve" to be able to react rapidly to a promising idea or a breakthrough.

Along a similar line, the Panel recognizes the importance of frequent small to moderate mission opportunities within the NASA program. The rapid access to space that these could provide for new, innovative experiments is essential for the vitality of space science. Of particular importance in that respect is the Explorer program, including the new series of "Small Explorers" as well as balloon programs.

### *Recommended Facilities*

Experimental particle astrophysics has been developing so far mainly at universities, although the Lawrence Berkeley Laboratory and the Los Alamos National Laboratory have been among the pioneers of the field and the Fermi National Laboratory is seriously considering the possibility of getting involved in experimental aspects. The absence of national structures such as accelerators which naturally regroup the physicists and support their activity leads this panel to recommend the following actions.

*Establishing high energy astrophysics observational facilities* on nearby sites sharing common facilities is in many cases quite natural scientifically and has already been informally implemented at sites such as Dugway in Utah and institutionalized at Gran Sasso in Italy. But the Dugway experience may demonstrate the need for significant support facilities that only a formal structure can fully provide at such remote sites. Such regrouping of

facilities should not preclude experiments at other sites, when this is clearly preferable for the specific purpose or technique proposed. An obvious example is the use of the existing infrastructure of astronomical observatories.

This panel also studied in some detail the need for a national underground facility. It concluded that a national facility of the size of Gran Sasso was not justified, since large experiments could easily use this Italian facility. However the specific needs of searches for weakly interacting dark matter particles and the cryogenic detection of solar neutrinos (difficulty of transporting ultra low radioactivity detectors, low temperature refrigerators, need for ultra-quiet environment in terms of radioactivity, electric and mechanical noise) does warrant the *immediate study of the necessary ultra-low background facility or facilities*. Their size will probably be modest, they may well be installed in underground laboratories existing on the American continent (e.g., Soudan, Sudbury) or be built at shallower depths. In any case, a clear understanding of the requirements and of the possibilities has to be obtained in parallel with the technological developments, so that if and when the necessary detector technologies have been demonstrated, no unnecessary delay is encountered in implementing significant experiments.

*The necessity of a continuous supernova watch* over possibly several decades will require innovative institutional mechanisms. Although a straightforward coordination of the down periods of the various underground experiments will be sufficient in the short run, the operation and maintenance of instruments for tens of years cannot easily be accommodated in the present research structure. National laboratories may have to take the responsibility for this long endeavor.

More generally, this Panel strongly favors *the expansion of the involvement of National Laboratories* (including SLAC and SSC) in particle astrophysics. While helping this new field with their technical expertise, they may benefit from the intellectual stimulation and the diversification provided by a fundamentally multidisciplinary discipline.

#### *International Collaborations*

The field of particle astrophysics is already strongly benefiting from international collaborations, in part because of the tradition in Particle Physics. In addition to the continuation of the use of the deep underground facilities in Europe and Soviet Union (Gran Sasso, Frejus and Baksan), we see the potential for new collaborations around large observational facilities, in particular for supernova watch and for high energy gamma ray, neutrino and cosmic ray astrophysics. The probable need for a major extensive atmospheric Cerenkov telescope and air shower array in the southern hemisphere may provide an excellent opportunity for the involvement of developing countries.

#### *Education and Technology*

Particle astrophysics is in a unique position to help in the improvement of the scientific and technical education in the United States. Its scientific focus combines astrophysics which has always been a source of fascination and particle physics which enjoys a very prestigious position at the frontier of knowledge. It is therefore particularly easy to explain some of the problems addressed by the field to the public at large and the number of recent articles in the general press on dark matter or neutrinos, for instance, demonstrates a real interest. This certainly contributes in the long run to a better general understanding of science and technology.

For the same reasons, particle astrophysics attracts an increasing number of undergraduate and graduate students, and postdoctoral researchers. The experimentalists are in particular excited by the possibility of tackling very fundamental problems with relatively small instruments, modest sized teams, and reasonable time scales. Such a combination allows them to have a complete grasp of all aspects of the experiment and probably provides the best training for a scientist. Moreover, the diversity of the science involved offers experimentalists and theorists a marvellous multidisciplinary education.

Particle astrophysics also contributes directly to the technological base of this nation. Its progress depends critically on the sensitivity of its sensors, the power of its electronics, and the efficiency of its computer codes. In a way similar to particle physics, such performance is achieved by a combination of innovative in-house development, adoption of the most advanced technologies developed elsewhere and specific collaboration with industry to adapt manufacturing processes. Some of the developments specifically recommended in this report may be particularly beneficial for other fields. For instance, it is likely that the development of cryogenic detectors will have spin-offs not only in astrophysics (e.g., X and  $\gamma$  ray astrophysics) but in particle and nuclear physics, high resolution X and  $\gamma$  ray spectroscopy, and biomedical imaging. Ultra-low background techniques may also have interesting applications in biology (e.g., high sensitivity radioactive tracing) or in electronics (e.g., soft errors in large scale integrated circuits).

Therefore, far from being an esoteric abstract field, particle astrophysics can contribute at a modest but real level to the solution of some of the fundamental causes of recent economic difficulties.