High Energy from Space

Introduction and Executive Summary

Barely 25 years have elapsed since the first space-borne observations of high energy phenomena in the Universe. In that time interval, an astonishing amount of progress has been made. At the beginning of the era, we had only brief, indirect glimpses of sites of violent astrophysical activity, *e.g.*, cosmic rays, or optical observations of supernovae and radio galaxies, and virtually no understanding of the underlying physical processes. Now high energy astrophysics is a fully established discipline: both our catalogs of observations and the maturity of our theoretical interpretations of these data are fully comparable to those in most other areas of astrophysics.

Despite these encouraging beginnings, the discipline of high energy astrophysics has weathered a 10 year hiatus in flight opportunities, a hiatus that has drastically slowed the rapid progress of scientific discoveries of the 1970s, and that has had a chilling effect on the entry of young researchers into the field. During this time, the main centers of activity in high energy astrophysics have shifted to Europe and Japan. Nevertheless, the discipline has remained intact in the US, and workers are eager and ready to take full advantage of the remarkable opportunities of the 1990s to regain this lost ground.

It is a safe prediction that the decade of the 1990s will see an amount of forward progress equal to that of the entire past 25-year history of our discipline. Although some of this stimulus will come from areas common to many fields of astronomy, *e.g.*, the explosive increase in available computing power, a dominant source of progress will be NASA's Great Observatories. The *Gamma Ray Observatory* (GRO) and the *Advanced X-ray Astrophysics Facility* (AXAF) will each represent jumps of orders of magnitude in observational sensitivity over past experiments, and inject not only large amounts of new data on high energy phenomena, but also tremendous intellectual challenges in interpretation of these data. Correlative observations from the *Hubble Space Telescope* (HST) and eventually the *Space Infrared Telescope Facility* (SIRTF) can only make these problems more interesting and exciting.

It is impossible to forget, however, that both AXAF and GRO were recommended in the Field report: in effect, they are the long delayed program of the 1980s. There is no doubt that they will revolutionize the field, but they alone do not constitute a complete program. First, there are scientific areas of major importance that are not addressed by these missions. Second, like all disciplines, high energy astrophysics needs a mix of small, medium and large programs to provide vitality and scientific flexibility. Third, we must begin now to plan for the the first decade of the next millenium – if nothing else, surely the past ten years have taught us that.

In addition to AXAF and GRO, there are several less ambitious missions now approved or under study. The only approved moderate mission is the *X-ray Timing Explorer* (XTE). We have identified two further moderate missions that address science of the highest priority, both of which should be started and launched in this decade. Also approved are a variety of much smaller opportunities on foreign missions. These are of great importance to US investigators, and provide a particularly cost-effective way of doing science. Such collaborations must be encouraged and expanded during the 1990s. Several other American payloads now under study include exciting prototypes of a new class of small, inexpensive mission. We have identified a surprisingly large number of important problems in high energy astrophysics which can be addressed by such small missions in the coming decade.

A complete program also requires development of the technology and infrastructure to pave the way for the new discoveries of the 21st century. Therefore, we discuss a program of technology and instrumentation development to prepare for the next generation of X-ray and γ-ray astronomy missions. Lastly, we discuss certain programmatic issues and stress the need for continued support of the research base of the discipline through funding of theory, research, and analysis programs.

In summary, our vision of a vigorous program for high energy astrophysics in the next decade includes

- Launch and extended flight operations of AXAF and GRO, and wide community involvement in their observing programs
- At least two new Explorer-class missions in addition to XTE, to address particularly exciting opportunities in X- and γ-ray astronomy
- Exploitation of smaller, less expensive space missions for important specialized problems
- An ambitious program of technology development in optics, detectors, and related hardware
- Changes in selected programmatic approaches that affect the research base in our field

This program can in the coming decade challenge scientific issues as diverse as stellar chromospheres, relativistic stars, the intergalactic medium, dark matter, the energetics of active galactic nuclei, and the large scale structure of the Universe. Observations of high energy phenomena from space will be fundamental to the astrophysics of the 1990s.

Important Scientific Problems for High Energy Astrophysics

Stellar Activity

One of the most important discoveries by the *Einstein Observatory* was that normal stars of nearly all spectral types are unexpectedly strong X-ray emitters. The mechanisms that produce these X-rays are poorly understood and indicate fundamental problems in present theories of stellar evolution, the interiors and atmospheres of stars, and the mechanisms of coronal heating. The observations required to solve these problems are unique to high energy astrophysics. Future X-ray missions will provide valuable classification data, and spectroscopy will bring the power of plasma diagnostics that has been so fruitful in solar studies. Further observations that combine high sensitivity with spectroscopy and broad field imaging are necessary to provide sufficient X-ray data to correlate observables with stellar properties such as spectral type, rotation rates, optical luminosity, age and multiplicity, and to monitor variability and study activity cycles.

The Interstellar Medium In Our Own and Other Galaxies

As the repository of material from which stars form and to which they return nucleosynthetically-enriched material at the end of their lives, the interstellar medium (ISM) plays a dominant role in governing the evolution of galaxies. Over the past 15 years we have come to recognize the ISM as a violent, dynamic environment whose structure is determined by the mechanical energy ejected from stellar winds and supernovae and whose energy density is divided roughly equally among several components with a large scale magnetic field, high energy cosmic rays and turbulent motion of massive clouds each containing as much as energy as the light generated by the Galaxy's hundred billion stars.

We have discovered several distinct phases of interstellar matter with temperatures varying between 10 K in the cores of molecular clouds to $>10^6$ K in a coronal component which is dominant in the solar neighborhood, but we do not know the distribution of matter among these various phases and have no self-consistent global model for the medium's evolution. Likewise, we have only the most rudimentary understanding of the variations of interstellar environments in different galaxy types and of the interaction of interstellar gas with the intergalactic environment.

With our recognition over the past decade of the central role of hot coronal gas and our ability to map this component in the nearest external galaxies, along with the discovery of diffuse positron annihilation and ^{26}Al lines, it has become clear that high energy astrophysics has a central role to play in unravelling the structure and charting the evolution of the ISM. The coming decade will see dramatic progress. New soft X-ray spectroscopic experiments will determine the temperature structure and chemical composition of the local hot gas, while sensitive X-ray imaging will allow us for the first time to study this component in a range of galaxy types and orientations, to elucidate the role it plays in governing galaxy evolution. For the first time we will gather a complete census of the hot cavities created in the ISM by recent supernovae (both in our Galaxy and in other Local Group members). GRO and other γ-ray missions will open new windows on the ISM, mapping the cosmic ray distribution through their interaction with molecular clouds, and providing a global picture of recent element creation by charting the distribution of radioactive ^{26}Al.

A γ-ray spectroscopy mission could provide a completely new dynamical tracer of the Galaxy through observation of the positron annihilation line. Over 10^{43} positrons, resulting mostly from the decay of radionuclei produced in processes of nucleosynthesis, annihilate in the interstellar medium per second. The shape of the 511 keV annihilation line, observable with high resolution γ-ray spectrometers, can differentiate between annihilation in cold cloud cores and warmer interstellar gas. In the cold cores the line is broader because positrons form positronium in flight, which annihilates while still moving with relatively high velocity. Since positrons could be prevented by magnetic fields from penetrating into cold cloud cores, unique information on magnetic fields can be obtained by mapping the galaxy in annihilation radiation.

Supernovae and Endpoints of Stellar Evolution

The study of compact objects and supernovae is fundamental to astrophysics, being essential to the understanding of the life-cycle of massive stellar systems. Compact objects are the degenerate end-points of stellar evolution, and include white dwarfs, neutron stars, and stellar-mass black holes. Supernovae are the most visible and violent manifestation of the death of stars, and include both the gravitational collapse of massive stars and the nuclear detonation of white dwarf stars.

The bolometric luminosity of both Type I and Type II events is powered largely by the input of energy from positrons and γ-rays resulting from the decay of radioactive elements. Thus, X-ray and γ-ray observations can provide unique information on some of the most basic questions related to the understanding of supernovae: How much and what type of explosive nucleosynthesis occurs in Type I and Type II events? What is the mechanism for the explosion? What are the key characteristics of the explosion, for instance, total energetics and mass ejection? What was the nature of the progenitor system? That is, what was the mass, composition, and structure of the progenitor, and was it in a binary system?

SN 1987A has surely left behind a relativistic remnant. X- and γ-ray observations over the next decade will play a fundamental role in identifying and studying this unique object, the youngest collapsed star known.

Because production of high-energy particles (and therefore X-rays and γ-rays) is common in the vicinity of compact objects, either via accretion processes, or acceleration in strong magnetic fields, the study of X-ray and γ-ray emission addresses numerous fundamental questions, including: What is the inventory of compact objects in the Galaxy? That is, what is the number density and age distribution of white dwarf, neutron star, and black hole systems? What are the masses of compact objects? What magnetic fields are characteristic for compact objects and what is the time-evolution of these fields? How do systems involving compact objects evolve, in particular, interacting binary systems and globular cluster systems? What is the basic physics of accretion disks?

Nucleosynthesis

Second only in intrinsic interest to the origin and structure of the Universe itself, the origin and evolution of the chemical elements has long been a central theme in astronomy. It is now generally agreed that the elements other than hydrogen and helium, which come from the Big Bang, are byproducts of stellar evolution. The 1990s will witness the continued development of the theory and measurement of nucleosynthesis as a *quantitative* science. The key observational tool is emission-line spectroscopy. Gamma-ray lines from supernovae give direct evidence of newly synthesized elements, from which the yields of supernovae of various masses and types can be deduced. Examples of the problems amenable to study with the technology of the 1990s are γ-ray lines from ^{56}Co (the progenitor of iron) made in Type Ia supernovae; ^{26}Al, made, at least partly, in type II supernovae; and possibly ^{22}Na made in novae and ^{44}Ti in young supernova remnants, including perhaps SN 1987A. In addition to information on abundance, the profiles of γ-ray lines can reveal the mechanism, velocity distribution, mixing, and asymmetry of supernova explosion. High resolution X-ray spectroscopy of young supernova remnants can yield the abundances of the ejecta of all types of supernovae, providing information on the enrichment of all the material returned to the interstellar medium. Spatially resolved X-ray spectroscopy will show the degree of spatial segregation of the ejecta, for comparison with models of stellar evolution and of supernova mixing processes.

Relativistic Plasmas and Matter Under Extreme Conditions

A relativistic plasma is matter dominated by $e^+ - e^-$ pairs. Such a plasma is thought to be present around compact objects when the emission, consisting of radiation above the pair production threshold (511 keV), is large and the size of the emitting region is small. In this case the $\gamma\gamma$ opacity is large enough to lead to copious pair production. The hallmark of pair production is pair annihilation leading to 511 keV line emission. There is evidence for such emission from a compact object at or near the Galactic Center and possibly also from Cygnus X-1. Relativistic plasmas are also expected near the massive black holes that are thought to power AGNs. Our own Galactic Center may prove to be the Rosetta Stone of the physics of AGNs, with high resolution γ-ray observations playing a decisive role.

Degenerate dwarfs and neutron stars provide relatively nearby cosmic sites where the properties of matter under extreme conditions can be studied. For example, neutron stars commonly have surface magnetic field strengths $\sim 10^{12}$ G, 10^6 times stronger than the strongest magnetic fields that have been produced on Earth. They have central densities $\sim 10^{14} - 10^{15}$ gm cm^{-3}, equal to or exceeding the densities of atomic nuclei, and produce radiation fluxes $\sim 10^{28}$ erg cm^{-2}, $\sim 10^{17}$ times the radiation flux at the surface of the Sun. The proton fluid in neutron stars is thought to be the highest temperature superconductor in the Universe, with a critical temperature $\sim 10^9$ K.

Despite the impressive progress in understanding these objects that has been made during the past two decades, many fundamental questions remain unanswered. How are neutron stars formed? What is at the center of a neutron star? How hot are they, and how rapidly do they spin when they are formed? What are the most important processes that cool them? What are their life histories? What are the electrical, thermal, and magnetic properties of the incredibly dense neutron and proton fluids in their interiors? Where do neutron star magnetic fields come from? How do they survive?

Study of the relativistic pair plasmas that are thought to surround some black holes will require space-based instruments that can measure the radiation they emit over the energy range from about 10 keV to several MeV. One of the most intriguing aspects of neutron stars and black holes is that their properties can change within a microsecond, or perhaps even less. Thus, X- and γ-ray photometric and spectroscopic instruments with high time resolution will be required to advance our understanding of these objects. Space-based instruments that can observe targets uninterrupted for hours or days, and revisit targets frequently, will also be important.

Nature of γ-Bursts

The 1990s may well be the decade when one of the longest-standing mysteries in high energy astrophysics is solved — the nature of the mysterious cosmic γ-ray bursts. Flashes of γ-radiation that appear suddenly in unpredictable locations, γ-ray bursts may last from less than a tenth of a second to several minutes and then disappear, usually forever. Though over 400 bursts have been recorded, none has been unambiguously identified with any persistent source at any wavelength. Most theorists believe that these events are associated with magnetic neutron stars in our Galaxy (which would make them the only detectable signal from a very large number of neutron stars), but debate continues as to the roles of accretion, thermonuclear explosion, starquake, and other more exotic sources of energy. If in our galaxy, the luminosity of a typical γ-ray burst, coming from a region only 10 km in radius, is 10^5 times that of the Sun. Some theorists believe, however, that γ-ray bursts are at cosmic distances, more than 200 Mpc to display the observed isotropy, in which case the energy release is more like that of a supernova. The 1990s are a time when major progress is expected toward solving this problem, because GRO will determine the source distribution (to 1° for each burst observed) with sufficient accuracy to prove or disprove an association with our own Galaxy. The *High Energy Transient Experiment*, which will view γ-ray bursts in several wavebands (γ-ray, X-ray, and perhaps, ultraviolet), will pin down the burst sites even more accurately, hopefully restrictive enough that larger instruments can be brought to bear to isolate the quiescent counterpart. Interesting constraints on the source and physics of γ-ray bursts will also be given by studies of their spectra, time history, and frequency.

Identification of Black Holes

Stellar mass black holes are inferred to exist in a few X-ray binary systems in our Galaxy. However, no totally convincing proof of the existence of black holes, or method of unambiguously differentiating them from neutron stars, is available. Progress thus far has been made primarily through a combination of soft X-ray observations, and optical observations of the source counterparts. The hard X-ray signatures of accreting black hole binary systems may provide important future constraints on the mass and radius of the compact star. Hard X-ray continuum spectra can measure apparent temperatures and thus constrain M/R. Positron and iron line features, expected to be broadened if from near the hole, can measure velocities, and thus central mass. Timing measurements can constrain minimum hole size and thus mass.

A key to unlocking this fundamental problem is a detection capability over a very broad hard X-ray band, with very high sensitivity, at least modest spectral resolution, and possibly polarimetric capability. With high angular resolution as well, studies of black holes can be extended from our Galaxy to nearby galaxies, and the unknown total number of black holes in galaxies then investigated for the first time.

Active Nuclei, Including Our Own

The most energetic objects known in the Universe are the nuclei of active galaxies (AGNs). Their luminosities are observed to exceed by a large fraction the integrated stellar luminosity of all 10^{11} stars in a normal galaxy, yet observations strongly suggest that this energy is generated in a volume of radius $\lesssim 10^2$ AU. Chiefly by a process of elimination, accretion onto massive black holes is most often invoked as the energy source, with masses of $10^6 - 10^9$ $M_\odot$ normally assumed. Precisely how this process might work is unknown, and remains a central mystery in astrophysics, to be addressed in the next decade.

High energy observations in X- and γ-rays have a unique role to play in the understanding of the physics of AGN, because much of the energy radiated from the sources is at these wavelengths. Also, the great penetrating power of these photons allows us to study processes occurring close to the central object. The expectation of much of the community is that some unique fingerprint of massive black hole activity may emerge from high energy observations, allowing us to firmly establish their presence and ultimately understand the details of the accretion process.

The overall photon spectrum from a significant sample of these objects has yet to be measured, particularly at higher energies. High quality spectra may reveal the presence of $e^{\pm}$ annihilation features, or other gravitationally redshifted emission lines which will provide diagnostics of the potential well of the collapsed object; accurate continuum spectra may constrain the detailed physical processes at work. Many workers believe that more than one mechanism is involved, and that characteristic spectral breaks or kinks will become evident. Indeed, it is likely that different parts of the central region of AGNs are being studied at different wavelengths.

A study of the time variability and total luminosity of a large sample of AGNs is also crucial in constraining the size scale and energetics of the central object.

In recent years, a growing number of workers have come to believe that some small, quiescent version of an active nucleus might commonly reside at the center of ordinary galaxies, including our own, largely unnoticed because of its low luminosity. Tantalizing but not yet conclusive evidence that the center of our own Galaxy might harbor a black hole has been produced by infrared and radio observations, and the issue remains controversial. It is interesting to note that for 20 years, γ-ray astronomers have reported an intense and variable source of antiparticles somewhere near our own galactic center. A concerted effort to precisely position this source is now underway, and it may well be that our own galactic center will play a key role in our eventual understanding of AGN.

Accretion Physics

Within the past ten years, accretion onto compact objects has come to be recognized as one of the most important energy generation mechanisms in the Universe. The process is ubiquitous and operates on many different astronomical scales. Within our own Galaxy, accretion fuels the most powerful high energy stellar sources, the X-ray binary systems. As noted above, on a much larger scale, accretion onto massive black holes is believed to provide the energy source for nearly all varieties of active galactic nuclei, the most intrinsically luminous cosmic sources currently known.

In view of the importance of this process, understanding the physics of accretion has become one of the greater challenges for modern astrophysics. Despite over two decades of intense effort, however, a number of fundamental uncertainties still remain: (a) How are accretion disks formed? (b) What process is responsible for the dissipation of angular momentum in disks? (c) Is accretion unstable? If so, how and in what parameter regimes? (d) What accounts for the extreme phenomenological diversity of known accreting sources? (e) What happens at the disk boundary layer with the compact object? (f) How does the accreting flow interact with the intense magnetic fields of neutron stars and white dwarfs? (g) What happens when the mass accretion rate exceeds the Eddington limit? (h) Can disks form and collimate jets around compact objects? (i) What is the role of internal fields in accreting flows?

High energy observations will play a crucial role in answering many of these questions. Since most of the energy release for accreting sources is in the X- and γ-ray regions of the spectrum, observations at these wavelengths provide the most direct and unambiguous signature of the accretion process. For example:

- X-ray timing observations on all timescales ranging from μsecs to years provide detailed information on instabilities in the accreting flow. For the galactic sources, improved phase coverage for binary systems will determine the geometric structure of accretion disks.
- Moderate-to-high resolution X-ray spectroscopic observations provide extremely detailed information on the interaction of the intense radiation with the circumsource environment. This in turn yields direct constraints of the physical structure of the accreting matter, including the density, temperature, and ionization dependences on position with respect to the central source. Elemental abundances can also be determined in this way.
- Improved hard X-ray spectroscopy yields detections of cyclotron absorption and emission features originating in the magnetospheres of accreting neutron stars. The rotational phase dependence of these features and of the continuum leads to determinations of how the accreting material penetrates the magnetosphere.
- γ-ray spectroscopy should provide the first detections of electron-positron annihilation features for most accreting sources. Many recent models suggests that the formation of $e^+ e^-$ pairs may be a crucial "regulating" process in the most energetic regions of the accreting flow. New γ-ray observations can test and constrain such models.
- X-ray polarimetric measurements would provide direct and unambiguous constraints on the geometry and structure of neutron star magnetospheres. Polarization measurements can also constrain the orientations of accretion disks with respect to our line-of-sight. These can be compared with the orientations of "jets" associated with the source to provide constraints on jet-formation models.

Large-Scale Structure

Study of clusters of galaxies at large redshifts is potentially one of the most interesting contributions of X-ray astronomy to cosmological studies. The sensitivity of X-ray surveys can be such as to permit identification and study of clusters at much larger distance than is possible in visible light. The X-ray luminosity of a cluster is a function of its mass, the amplitude of the density fluctuations at the epoch of formation, and its chemical and dynamical evolution. Study of large samples at different redshifts is required to disentangle these effects.

Study of the X-ray spectrum directly yields the redshift, due to the prominent iron emission lines, and the temperature, from the continuum shape. The virial mass can be derived from temperature and surface brightness distribution. Correlation of cluster distributions over contiguous large area of the sky ($10^\circ \times 10^\circ$) can reveal the presence of very large structures at remote epochs, and elucidate the process of their formation and evolution. Deep surveys with wide field optics will be required to supplement the AXAF capabilities for this type of scientific investigation.

Intracluster Medium

The discovery and study of an intergalactic medium in galaxy clusters was a major triumph of X-ray astronomy in the 1970s and early 1980s: This diffuse, hot gas (typically 10^{-3} atoms cm^{-3} at 10^8 K) contains as much or more matter than all the stars in all the galaxies in the cluster, and yet it reveals direct evidence of its presence and its properties only in the X-ray band. Past investigations show that the morphology of the gas in several dozen clusters takes various forms and that the chemical enrichment of a handful of systems is roughly half the the solar value. In some galaxy clusters or groups the gas is seen to be cooling and presumably condensing into stars at rates as high as 1000 $M_\odot$ yr^{-1}. In the 1990s, the study of the spatially extended and emission line rich thermal X-rays of the intracluster medium (ICM) through both imaging and spectroscopy, will make major contributions to our understanding of the structure and evolution of galaxies and clusters. One example is the use of X-ray studies to map the gravitational potential in systems ranging from well relaxed older clusters to multiple and merging subclusters (see also *Dark Matter*, below). Another is the mapping of ICM distribution and composition with redshift to trace the evolution of cluster potentials over cosmological timescales and to record the history of global nucleosynthesis and the ejection of stellar material from the member galaxies. Studies of cooling flows show a process of galaxy formation similar to that which must have occurred in the early Universe. And information on the properties of the ICM can be combined with radio observations of its effect on cosmic microwave radiation to give an independent measure of the Hubble constant (via the Sunyaev-Zeldovich effect).

Nature of Dark Matter

The existence of dark matter in many and probably all galaxies and galaxy clusters is now well established. It is highly popular but more speculative to conclude that this unseen matter constitutes 90–99% of the stuff of the Universe. However, the distribution of dark matter is poorly known and its nature is a total mystery; elucidation of these properties is a central problem of contemporary astronomy and astrophysics.

Pioneering studies with the *Einstein Observatory* demonstrate that X-ray observations can make a unique and important contribution to this question. The reason is that observations of hot gas in many galaxies and in most or all clusters and groups of galaxies traces the gravitational potential of the dark matter with a precision that far exceeds that of any other method. What are needed are high quality X-ray images and spatially resolved spectra from which one can deduce the distribution of gas pressure and hence, the gravitational pull that opposes it. In clusters the possible role of gas turbulence can also be discerned by X-ray spectra of sufficient resolution. In the 1990s X-ray studies can therefore provide for the first time detailed mapping of the dark matter in many parts of the Universe. Furthermore, these maps could help reveal the nature of the matter as well, because its distribution gives clues about the masses of its constituent particles and how strongly the particles interact with one another (dissipational versus dissipationless).

The X- and γ-ray Background

Almost 30 years ago, X-ray detectors on board a spinning rocket discovered both the brightest non-solar X-ray source, and an apparently diffuse X-ray background (CXRB). The source was ultimately demonstrated to be associated with a low-mass stellar binary system containing a neutron star; to date, the CXRB has defied unambiguous identification. Its spectrum mimics that of a 40 keV optically thin plasma, which is problematic for the determination of its origin in at least three respects:

1. A truly diffuse 40 keV origin is theoretically awkward;
2. No candidate populations of sources (including AGN, some type of which is still the odds-on favorite) seem to exhibit spectra that are as hard;
3. The only sensitive searches for CXRB point-source candidates have bee n made at energies near 1 keV, well below the energy at which the CXRB has its maximum energy density (this energy density is second only to that of the 3 K microwave background, at a level about two orders of magnitude lower).

Clearly, any experimental effort to identify the origin of the CXRB must be able to measure similar spectra from candidate sources near the energies at which their outputs peak. Another "bump" in the isotropic cosmic radiation spectrum may exist near 1 MeV, presenting an analogous identification problem in the γ-ray region that may be confirmed, but not solved, with the data available from GRO.

There is also a diffuse γ-radiation whose origin is unknown. Theoretical models for this emission included: redshifted π^0 decay γ-rays from matter-antimatter annihilation in the early Universe, Compton scattering of relativistic intergalactic electrons by the 3 K background radiation, and the superposition of emission from many unresolved active galactic nuclei. Some contribution from the latter is certainly present, but their contribution cannot be reliably determined until a larger number of active galactic nuclei have been studied in the γ-ray regime. A careful study of the uniformity and energy spectrum should also provide a major step towards determining which theoretical model accounts for most of the diffuse radiation.

The Existing Experimental Program

Advanced X-Ray Astrophysics Facility (AXAF)

Our panel unanimously endorses the paramount scientific importance and necessity of AXAF, which must be completed and launched in this decade.

The capabilities of the *Advanced X-Ray Astrophysics Facility* (AXAF) will address the major outstanding astronomical and astrophysical problems discussed above to an astonishing degree. The AXAF is a remarkable and unique scientific endeavor and is the centerpiece of the X-ray astronomy program presented here. The ultimate fate of the Universe could be inferred from accurate AXAF measurements of the rate of expansion (the Hubble constant) and the change in rate (the deceleration parameter). The existence of exotic particles predicted by supersymmetric theories might be confirmed (or ruled out!) by observing their effects on the hot, X-ray emitting gas found in both galaxies and clusters of galaxies. The equation of state of bulk matter at nuclear densities, totally inaccessible to experiments on Earth, can be tested by studying the X-radiation from neutron stars.

These are but a few examples of what may be accomplished with AXAF, one of the Great Observatories which also include the *Hubble Space Telescope*, the *Gamma Ray Observatory* and the *Space Infrared Telescope Facility*. Operating in concert, and complementing each other in their respective wavelength capabilities, the Great Observatories should place mankind in a unique and historical position to understand the Universe.

Plans are now advancing for an AXAF Science Center (ASC) to act as the scientific interface between the AXAF project and the community. Experience with institutional arrangements for previous large programs has shown that establishing such a center at the earliest possible opportunity in the lifetime of a project not only ensures that the scientific objectives are met and maximizes the scientific return of the program, but may also provide significant opportunities for minimizing the run-out costs of the entire undertaking. We therefore urge that the ASC be placed in operation at the earliest possible opportunity.

Gamma Ray Observatory (GRO)

The *Gamma Ray Observatory* (GRO), scheduled for launch in late 1990, is a major new mission for γ-ray astronomy, covering six decades of energy, from 30 keV to 30 GeV, with a suite of four instruments. The range of astrophysics objectives targeted by GRO is extremely diverse, and includes high-energy phenomena in the vicinity of neutron stars and stellar-mass black holes, such as accretion processes, pair-plasmas, magnetic fields, and particle acceleration; determination of the origin of γ-ray bursts; the energetics and emission mechanisms of AGN; nucleosynthesis in massive stars, novae, and supernovae; energetic particle interactions in molecular clouds and the ISM; γ-ray line and continuum emission from solar flares; and the diffuse cosmic γ-ray background.

It is essential for the scientific success of GRO and for the future health γ-ray astronomy that this mission, as part of the Great Observatory program, enjoy widespread participation from the astronomical community. To this end, a particularly strong GRO Guest Investigator Program must be encouraged, supported by the

GRO Science Support Center and the four Principal Investigator instrument teams. NASA must ensure that the requisite resources are in place to support broad use of GRO by the astronomical community from the start of the mission through its completion. Access to both high-level and low-level data products on a reasonable time scale will be essential for Guest Investigators, as will be the availability of suitable documentation and calibration data sets. Support for correlative and theoretical studies related to the GRO mission science is also necessary to obtain the maximum scientific benefit from the mission. GRO may well be capable of an extended mission significantly beyond the baseline duration; continued support for such operations is essential.

XTE

The *X-Ray Timing Explorer* (XTE) is an important mission, having unique capabilities for moderate resolution spectroscopy and photometry over the entire energy range from 1 keV to 100 keV, with microsecond time resolution. It will substantially advance our understanding of the physics of accretion flows, the properties of relativistic plasmas and matter under extreme conditions, and the central engines of active galactic nuclei. The XTE program has been started, and should be completed in a timely fashion.

There are good scientific reasons to expect that a full understanding of processes occurring near the event horizons of black holes and the surfaces of neutron stars will require instruments with even better sensitivity and higher time resolution than XTE. Data collected with XTE will help to guide the planning of missions carrying such instruments.

HETE

The *High Energy Transient Experiment* (HETE) is a mission currently scheduled for launch in 1994 as a "Gas Can" by shuttle. It is composed of three instruments offering continuous, nearly full hemisphere coverage in the ultraviolet (4 eV to 7 eV), X-ray (2–25 kev), and γ-ray (6 kev – several MeV, provided by the French). The total weight of the experiment and free-flying spacecraft is $\sim$100 kg, and the estimated cost is \$13 M. The chief scientific goal is the panchromatic detection and monitoring of γ-ray bursts and bright X-ray bursts, with an eye towards obtaining accurate source locations for the former. The angular position accuracy is $6'$ in X-rays and $3''$ in UV, sufficient to offer good prospects for optical and/or radio identifications.

American Participation in Foreign Missions

There are several currently approved opportunities for US participation in foreign missions. In many ways, these *are* the low-cost, "small" Explorers of the 1990s, and this is a definite benefit of this type of collaboration. On the other hand, such activities cannot take the place of US-lead missions for maintenance of our technology base and personnel expertise, and data return to American investigators is often very limited.

NASA participation in the recently-launched German ROSAT mission will provide unique opportunities for imaging X-ray sources that have not been available to US astronomers since the end of the *Einstein Observatory* in 1981. The five-fold oversubscription to the first proposal solicitation for ROSAT, and the wide scope of scientific questions addressed in the first round of selected proposals, has demonstrated the value this mission will have in complementing the US program. The all-sky, high spatial resolution survey to be made by ROSAT should provide a unique resource for a variety of future investigations.

ASTRO-D is a cooperative program between Japan and the USA, scheduled for launch in February 1993 on board a Japanese-furnished satellite launched by a Japanese-furnished launch vehicle. It contains four BBXRT-type foil telescopes with a total geometric area that exceeds that of AXAF (and is some four times greater at 7 keV). Two of these telescopes have GSPCs furnished by the Japanese at their foci, while the other two have X-ray sensitive CCDs furnished by the USA. The mission has great generality, devoted to pointed observations of X-ray sources with all four imaging spectrometers coaligned; the angular resolution of the telescopes is $\sim 2'$ FWHM, which is constant over the entire $30'$ FOV, with energy resolution $\sim$ 100–200 eV over the range 0.1–12 keV. After an initial proprietary season of about nine months for the PI teams, US guest investigators will be entitled to 15% exclusive use of the facility, and another 25% in collaboration with Japanese investigators.

Spectrum X-Gamma is a Soviet mission with extensive European, and some American, participation, due for launch in 1993. As presently configured, it will carry several large X-ray telescopes and an array of complementary instrumentation. US groups are supplying hardware in support of an X-ray polarimetry experiment and an X-ray all sky monitor. The mission offers American investigators a chance to conduct some significant high sensitivity X-ray observations well in advance of AXAF. In addition, the US hardware participation on Spectrum X-Gamma provides an important precedent for increased cooperation in this field between the world's two major space powers.

The *X-ray Multi-Mirror Mission* (XMM) is a "facility-class" X-ray observatory under development by the European Space Agency for launch near the end of the 1990s. XMM is a "high-throughput" X-ray spectroscopy mission which provides a very significant complementary capability to that provided by AXAF. The scientific instruments selected for XMM involve substantial US hardware participation. This will ensure access to this facility for US Guest Observers, and, therefore, provides an extremely cost-effective way for Americans to conduct important X-ray astronomical observations.

We strongly endorse the US participation in these projects, and encourage the inclusion of an International Programs line item in the NASA budget to regularize the funding of such efforts.

Attached Shuttle and Space Station Freedom *Payloads*

The *Diffuse X-ray Spectrometer* (DXS) is a relatively simple, low-cost attached-Shuttle experiment which will directly address an important and poorly explored question: the physics of the low-energy component of the diffuse X-ray background. An understanding of the physical conditions in the hot component of the ISM responsible for this emission will impact many areas of astrophysics.

The Shuttle mission ASTRO with its X-ray spectroscopy component *Broad Band X-ray Telescope* (BBXRT) offers an important and uniquely new capability to high energy astrophysics. Reflight of this payload involving guest observer participation would be highly desirable in the early years of the decade, but should probably not be pursued beyond mid-1993 when the free-flying ASTRO-D will begin to provide an enhanced capability for moderate resolution spectroscopy and imaging in the 1–10 keV band.

The *X-ray Background Survey Spectrometer* (XBSS) is an attached Space Station *Freedom* payload which will greatly complement and expand DXS results by determining the spectrum of the soft diffuse X-ray background over 100% of the celestial sphere (as well as with increased resolution and superior wavelength coverage than DXS). This experiment is particularly well-matched to the Space Station, and should provide results fundamental to our understanding of the ISM.

A second Station payload is the *Large Area Modular Array of Reflectors* (LAMAR). The high throughput imaging and spectroscopic observations to be performed by LAMAR are of the highest scientific priority, and the successful execution of this science requires the full effective area of the current LAMAR concept. In addition, the possibility of using the foci of LAMAR optics modules as readily accessible test sites for novel detectors is attractive. However, the Panel is concerned that complexities of the Space Station, and the interface of LAMAR to the Station, may greatly delay the project and increase the cost very substantially. Thus, we urge that alternative missions to accomplish the important LAMAR scientific goals remain under consideration, and that particularly close attention be paid to LAMAR cost and schedule, to retain the

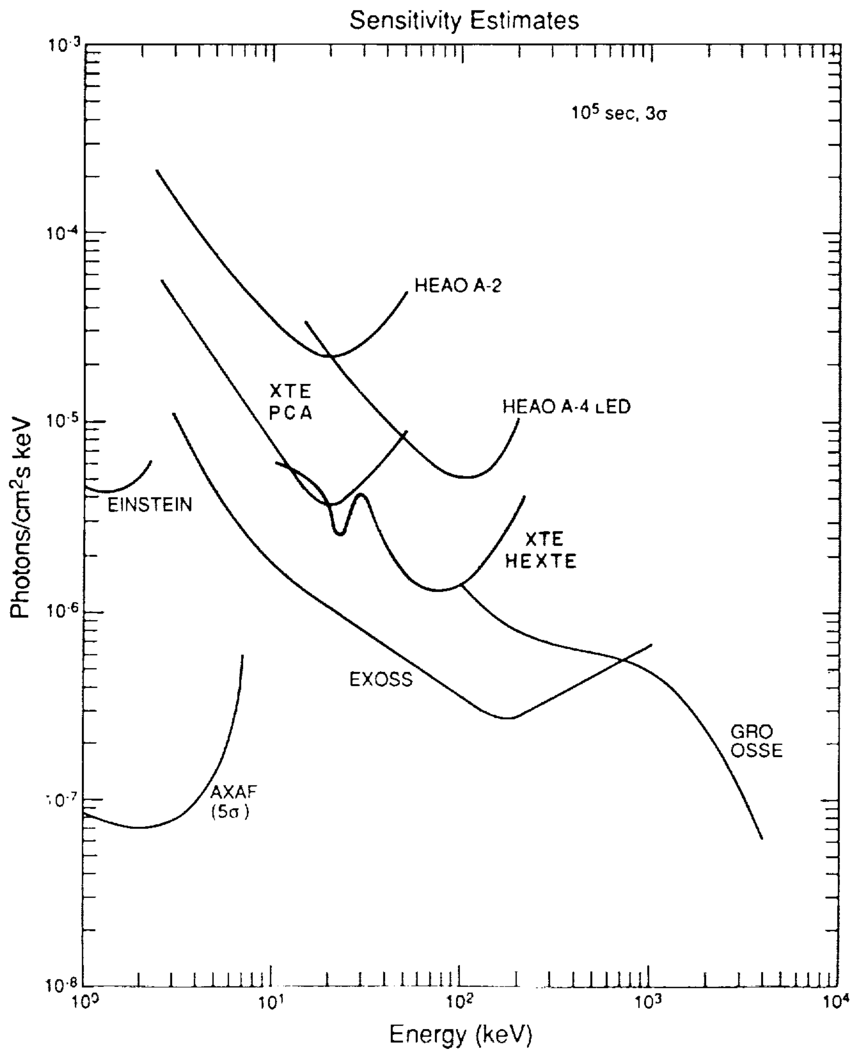

FIGURE 1 Sensitivity of selected X- and γ-ray astronomy missions for typical 10^5 s observations. EXOSS is an example of a concept for a future $\sim$1 m^2 collecting area hard X-ray imaging experiment.

possibility of switching to a high throughput concept unconnected to the Station, should this become necessary to accomplish the science in a timely and economic manner.

Major Missions for the 1990s

The highest priority major programs in high energy astrophysics from space are the rapid completion and flight of the *Advanced X-ray Astrophysics Facility*, together with integration of the AXAF Science Center into the worldwide astronomical community as a smoothly functioning entity; and the successful flight and reduction of data from the *Gamma Ray Observatory*. We cite these highest priority items succinctly simply because the case is so clear-cut. Figure 1 compares the sensitivity of selected instruments on these two missions with past experiments. These two components of NASA's Great Observatory program will without question transform our field, and shape the course of research in this area for one or more future decades.

We can already be virtually certain that technology development will raise attractive possibilities for second generation AXAF instruments, probably on the timescale of the years 2000–2005. These developments may permit use of AXAF in certain exciting scientific areas not currently addressed by the payload, *e.g.*, polarimetry, and also greatly expand the capabilities of the Observatory in imaging and spectroscopy.

It is also already clear that beyond the year 2000, there will be a requirement for at least one major new mission in both the X- and γ-ray regimes. We can make this statement with confidence even before the flights of AXAF and GRO, because examples of the sources to be studied, and thus their fluxes, are already known, and basic physical questions already evident define certain instrumental parameters. For example, numerous QSOs are known from *Einstein Observatory* observations to have fluxes of 10^{-13} ergs cm^{-2} s^{-1} in the soft X-ray band. If we are to study the X-ray spectra of these objects with sophistication comparable to the optical spectroscopy done routinely today, and address such fundamental issues as emission line profiles, we will require spectral resolution $E/\Delta E \sim 1000$. Such resolution at these flux levels will require X-ray optics with $> 2\,\mathrm{m}^2$ collecting area, far beyond the capabilities of AXAF. Technology development, described in more detail later in this report, will be required in several areas of optics and detectors for projects such as these.

Similarly, clear instrumental improvements are needed in the γ-ray regime. GRO instruments all have a basic angular resolution of one to a few degrees. While this may be sufficient, for example, for currently achievable sensitivities of 10^{-7} photons cm^{-2} s^{-1} keV^{-1} at 1 MeV, with the order-of-magnitude improvement in sensitivity expected for post-GRO instruments, angular resolution of considerably better than 1° is needed to avoid severe source confusion. Similarly, while 1° resolution is sufficient for determining the spectra of bright γ-ray emitting objects, detection and identification of fainter objects will require arc minute resolution or better. Order-of-magnitude improvements in both sensitivity and angular resolution are expected given technology development in areas such as position-sensitive detectors and large-scale structures.

What is as yet unclear is precisely which technologies will prove most successful and cost effective, and their timescales for flight readiness for these very large missions or new instruments. It is thus premature to define such missions in detail at this point. Instead, we recommend a vigorous, major technology development program in certain key areas discussed later in this report.

A New Program of Moderate Missions

Highest Priority

Our panel believes strongly that specific moderate/Explorer missions should be selected competitively through the established peer review process. We have identified two particularly important scientific problems which can be well addressed by moderate class missions in the coming decade. These areas, which we regard of equal priority, are hard X-ray imaging and γ-ray spectroscopy.

Hard X-ray Imaging A serious programmatic gap is developing in American high energy astrophysics in the 10–250 keV range. During the coming decade, AXAF and GRO will almost surely revolutionize our understanding of high energy phenomena above and below these boundaries. Yet this intermediate energy range remains poorly explored. There are indications that it may be particularly fruitful. This range is high enough that the emission is dominated by non-thermal processes, thereby promising results quite different from those obtained by AXAF and its predecessors; yet it is sufficiently low that the expected photon fluxes from a large number of both galactic and extragalactic objects are respectably high, and these objects are thereby amenable to study in great detail.

Among the key scientific objectives of a hard X-ray imaging mission are: determination of the log N–log S relationship for hard X-ray emitting AGN out to $\sim$ 100 keV; determination of the accretion processes and particle energy sources occurring in the immediate environment of the central engines of AGN; determination of the physical parameters of accreting neutron star systems, including magnetic field strength and geometry; study of possible signatures of accretion onto stellar-mass black holes; and study of the physics of the soft γ-ray repeaters. To study the high energy processes associated with galactic compact objects and AGN, a mission with broad-band energy response, extending over two decades of energy, is required: from the Fe K-shell line at low energies ($\sim$5 keV) to the positron annihilation line at high energies ($\gtrsim$ 500 keV). Such a mission would probe a very wide range of accretion physics over a wide range of central object mass scales. Many of the same physical emission processes expected to be operative in accretion onto stellar mass systems are also expected to occur in massive AGN. Study of Fe K-shell emission and comptonized photon spectra above 15 keV will constrain the physical parameters of accretion disks, while non-thermal power-law spectra in the range 50–500 keV and pair-plasma signatures above 500 keV will help determine the key characteristics of the physical environment in the immediate vicinity of the central compact object. In the case of accretion onto magnetic neutron stars, phase-resolved spectroscopy of cyclotron line emission will be critical in determining the magnetic field and plasma characteristics of the accretion column.

No high sensitivity survey of hard X-ray and soft γ-ray emitting objects currently exists. Detailed studies such as those described above should take place within the context of a proper understanding of the classes and number densities of galactic and extragalactic hard X-ray and soft γ-ray emitting objects. For this reason, survey objectives are a critical part of a new hard X-ray mission, and absolutely require imaging capability for the identification of objects and the elimination of source confusion. The technology of coded aperture masks is sufficiently mature that experiments with angular resolutions of one to several arc minutes seem achievable. Experience with soft X-ray astronomy indicates that this is the spatial resolution threshold where optical identifications suddenly become feasible for a large fraction of detected sources, thereby vastly increasing our understanding of each class of source detected. Spectral resolutions of 5–10% FWHM also seem straightforward to achieve, assuring accurate measurement of continuum slopes, and excellent data on the cyclotron emission features expected to dominate at these wavelengths for many objects.

A hard X-ray Explorer-class imager with 1 m^2 of effective area will be two orders of magnitude more sensitive than the HEAO-1 A-4 experiment, the only recent sky survey, at 100 keV, and 10$\times$ more sensitive than XTE at this energy. In a long (10^6 s) pointing, a 100 keV limiting sensitivity of 2×10^{-7} ph cm^{-2} s^{-1} keV^{-1} will be achieved, which will easily detect Her X-1 (or a source 10$\times$ fainter than Cygnus X-1) in the LMC. Although Cygnus X-1 is not quite detectable in M 31, an Eddington-limited 1 $M_\odot$ source would be. For extragalactic work, this long pointing could detect 3C 273 at 100 keV at 15$\times$ its true distance. A survey with 3×10^4 s per pointing can cover the entire galactic plane in 6 months, yielding 10^3 sources, or cover the entire celestial sphere in 2 yr, yielding 10^3 AGN to a limiting flux of 2% of 3C 273. In all the above cases, source confusion is never approached, and positions sufficiently accurate for optical identifications are obtained, except at very low latitude. Figure 1 shows the sensitivity of a typical 1 m^2 hard X-ray imager mission concept (EXOSS).

We strongly endorse an Explorer-class satellite to achieve these imaging and spectroscopic goals.

γ-ray Spectroscopy GRO will be exceptionally effective at mapping the sky at a variety γ-ray wavelengths, and providing initial glimpses at spectral features in a large number of sources. It is clear that substantially higher resolution γ-ray spectroscopy, at flux levels far beneath those attainable by the GRO instrument complement, will be the next step to build on GRO results. Sensitivity of at least 10^{-6} ph cm^{-2} s^{-1} (100$\times$ superior to HEAO C-1, and 10$\times$ superior to GRO OSSE) is needed to address the next set of interesting problems. To measure line profiles and Doppler shifts, spectral resolution of $E/\Delta E \sim 1000$ is necessary. Both of these goals appear feasible with technology available during the coming decade. Similarly, angular resolution of a few degrees or less will be needed, again an achievable goal.

Among the key scientific objectives of such a high resolution γ-ray spectroscopy mission are determination of the sites and rates of recent nucleosynthetic activity in the Galaxy; exploration of the physics of Type I and Type II supernovae, including nucleosynthetic processes and the characteristics of the explosion; determination of the characteristics of low energy cosmic rays and the interstellar medium; study of the physical environment of collapsed objects, in particular the relativistic pair-plasmas; and measurement of the magnetic fields of neutron stars, including studies of cyclotron line emission and positron production.

Maps of the Galaxy in the light of several nuclear lines (*e.g.*, $e^{\pm}$, ^{44}Ti, ^{26}Al, ^{22}Na, ^{60}Fe) will identify the nature and general sites of nucleosynthetic activity over the past 10^6 yr, including possibly Type I and Type II supernovae, novae, and Wolf-Rayet stars. The rates of these processes and the mixing of nucleosynthetic material into the interstellar medium will be measured. It is likely that several unknown supernova remnants less than 500 years old will be discovered. The profile of the $e^{\pm}$ line will be mapped to obtain a determination of the temperature and density phase where annihilation occurs in the interstellar medium. Nuclear lines from the interactions of low energy cosmic rays (E<100 MeV) with the interstellar gas and dust may allow a determination of the intensity and distribution of these cosmic rays, as well as the dust fraction and size, and the elemental abundances in the interstellar medium.

Discrete source observations will allow a number of Type I supernovae to be studied at distances approaching the Virgo cluster and their nucleosynthetic yield, energetics and expansion dynamics to be determined through measurements of their ^{56}Ni and ^{56}Co decay γ-rays. Detailed study of $e^{\pm}$ annihilation radiation from relativistic pair-plasmas in the vicinity of AGN will determine important physical parameters of the environment of the central engine and provide clues to its nature. The detection of nuclear lines, if accomplished, would provide significant new information on the energetic particle environment in AGN. Similar studies of galactic compact objects, in particular neutron stars and stellar-mass black holes, would yield correspondingly important measurements of the plasma and energetic particle environment around these objects. Observations of cyclotron, $e^{\pm}$ annihilation, and possibly nuclear lines can give new insights into the nature of γ-ray bursts, and the detection of gravitationally redshifted lines from the surface of neutron stars can provide direct information on the neutron star equation of state.

A spectroscopic mission of the this class would profoundly impact virtually every problem in γ-ray astronomy discussed earlier in this report. Recent advances in detector and cryogenic technology imply that a very powerful experiment of this type can still fit within the Explorer envelope; the sensitivity of such a mission (NAE) is shown in Figure 2. Many of the relevant technologies are already under test in balloon-borne spectrometers, although such experiments of course fall orders of magnitude short in the desired integrated exposure times. We thus enthusiastically endorse an Explorer-class high resolution γ-ray spectroscopy mission.

Programmatic Considerations There are moderate-mission concepts in relatively advanced design stages for both the hard X-ray imaging and the γ-ray spectroscopy missions discussed above, namely EXOSS and NAE, respectively. These missions address the scientific goals described here, goals which our Panel strongly supports. Other innovative technical approaches to these goals may also be possible.

Additional Mission Concepts

In X-ray astronomy, there are several additional important scientific areas which are also amenable to Explorer-class missions. One example is high resolution ($E/\Delta E \sim 10^4$) spectroscopy, to obtain for the first time data on line profiles and Doppler shifts of comparable sophistication to that of optical spectroscopy, albeit on a relatively small number of bright sources. There is also interest in an X-ray component to a "panchromatic" facility, which obtains simultaneous observations at UV and visible wavelengths as well.

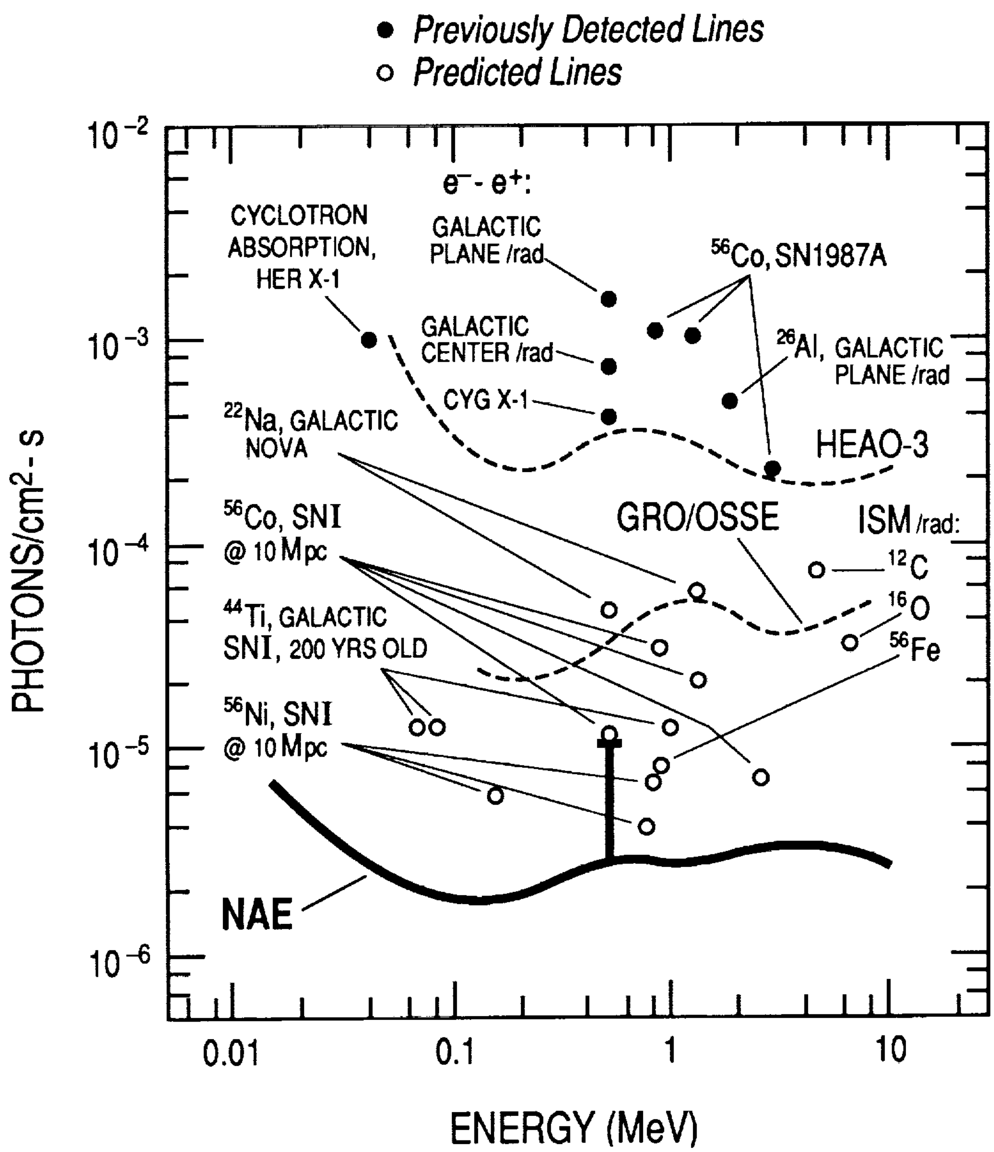

FIGURE 2 The line flux from a variety of known and predicted galactic and extragalactic γ-ray sources, together with the sensitivity (10^6 s exposure, 3σ significance) of several past/approved (HEAO-3, GRO/OSSE) missions and mission concepts (NAE).

Following the GRO mission, there will be a need for high-energy γ-ray observations with sufficient sensitivity and angular resolution to accurately locate sources, and to define detailed spatial features of emission regions such as molecular clouds, Galactic arms, and nearby galaxies, as well as to measure variations of compact sources. From a threshold of approximately 3×10^7 eV, the possibility of extending the energy range to approach 10^{11} eV is a desirable goal, requiring a sensitivity about five times that of the GRO high energy γ-ray telescope. Source location approaching 1′ should be attempted, in part by detector improvements and in part by greater sensitivity for the high energy γ-rays whose character permits inherently better individual angular resolution. The possibility of measuring polarization may be considered. Additional areas of interest include the spectra of γ-burst sources at very high resolution (presumably with Ge detectors), and extrasolar applications of pinhole subarcsecond resolution imagers designed primarily for solar work.

New Opportunities for Small Missions

Our Panel has identified a number of exciting scientific problems that may be addressed in the next decade via small missions. In this context, we use "small" as a cost <\$30 M, a payload mass <500 kg, and a launch vehicle smaller than a Delta. We discuss here several examples of such missions, *not* with the goal of establishing scientific or schedule priorities, which we believe should be left to the Announcement of Opportunity/peer review process, but rather to illustrate the diversity of important problems in high energy astrophysics that are accessible by these relatively low-cost projects.

A panchromatic attack on γ-ray bursts is available through HETE, the mission described previously above. It is remarkable that for a total mass of 100 kg, including both instrumentation and platform, one can cover the UV, X-ray, and γ-ray bands simultaneously, with sensitivity and angular resolution almost surely sufficient to make very significant, and quite possibly definitive, progress on the issue of the nature of γ-ray burst sources.

As a specific example of future possibilities, we note that recent innovations in X-ray optical design raise the possibility of a Wide Field X-ray Telescope in this small mission category. With an estimated payload weight of <300 kg (half of which is the optics), such a mission could survey 10^3 deg^2 in the 0.4–3 keV band, yielding $>10^3$ clusters (to z$\sim$2) and $>10^4$ AGN to limiting fluxes of $\sim 3 \times 10^{-14}$ ergs cm^{-2} s^{-1}. Such an instrument, specifically optimized for surveys, could be a splendid complement to AXAF, which is not designed for such work. As noted earlier, X-ray observations of clusters at this level of sensitivity may open entire new avenues for research in the large scale structure of the Universe.

We have identified a number of other important scientific problems which we believe are potentially well-suited to these small missions. Examples include application of multi-layer optics technology to produce a wide-field soft X-ray telescope with quite small energy bandpass, but very large field of view and effective area; very high resolution X-ray nebular spectrometers; all sky X- and γ-ray monitors to routinely monitor transients; and certain approaches to X-ray polarimetry.

We see a bright future to experimental X- and γ-ray astronomy in the small mission category, and we urge vigorous exploitation of these flight opportunities in the coming decade.

Technology Development Issues

X-ray Astronomy

A vigorous flight program in X-ray astrophysics should be accompanied by an equally vigorous program in technology development which will enable the design and testing of new experimental tools to be incorporated in future missions. There has been a virtual revolution in this field over the past few years owing to the discovery of a number of promising technologies which have not previously been utilized in X-ray astronomical instrumentation. Below we discuss developments in several key areas which are especially deserving of further study:

Large Area Telescopes (0.1 - 10 keV) Recent discussions of scientific requirements for the next phase of X-ray astronomical facilities have pointed to the need for large ($\geq 2\,\mathrm{m}^2$) collecting area with moderate-to-high angular resolution. A reasonable goal is $\sim 2 - 3''$, which will avoid source confusion problems and still be adequate to resolve galaxy clusters and even galaxies at high redshift. Collecting areas should approach 10 m^2 at reasonable cost. The primary driver will be to achieve these parameters in the 0.1–10 keV range, but we should also continue to explore ways to push up to 20 or 30 keV with very low graze angle mirror arrays.

A number of techniques have been suggested to achieve these goals. Several approaches, including the use of thin metal foils, electroforming, epoxy replication onto thin carriers, and flat mirror plates with long focal length, appear especially promising. All of these concepts, and quite possibly others, are deserving of further development. Normal incidence optics may also play a role, especially if the multilayer technique can be pushed to shorter wavelengths. Much of this effort can be accomplished via small grants to universities and research institutions; however, some larger-scale coordination with industry may be warranted in this area.

Focal Plane Imaging Arrays The past few years have witnessed the rise o f the CCD as the "workhorse" imaging detector for X-ray astronomy. CCDs certainly present an attractive option for future missions because of the combination of very high spatial resolution coupled with moderate energy resolution that they provide. The use of CCDs for X-ray astronomy has consequently received much attention in this field, both in the US and abroad. Nevertheless, there is much yet to be achieved. In particular, large area telescopes will inevitably require larger focal plane detectors — the present technology is already being "pushed" in this regard for AXAF. Present CCDs are roughly the size of a flattened ping-pong ball, whereas detectors the size of ping-pong tables may be required in the future. Larger and/or smaller pixel sizes are also necessary for some applications. Deeper depletion regions are needed to enhance the high energy efficiency, and there is continuing concern about CCD susceptibility to long-term radiation damage.

At present, most CCD fabrication is performed in large industrial firms where the effort is driven primarily by commercial and defense interests. It is likely that the astronomical devices of the future will need to be "customized" in ways that are not necessarily consistent with the requirements of these other, more lucrative applications. NASA should take account of this problem in planning for future CCD development.

Non-Dispersive High-Resolution Spectrometers An extremely exciting recent development in X-ray instrumentation has been the introduction of non-dispersive, high resolution spectrometers which rely on cryogenic technology. These devices combine the high spectral resolution characteristically achieved with dispersive systems with the high quantum efficiency of conventional lower resolution detectors. A reasonable scientific goal for these detectors would be a spectral resolution of 0.5 eV, so that one could achieve resolving powers better than 1000 at the oxygen Kα line and the iron L lines. Ideally, these detectors should be position sensitive, at least over a limited field. Logistics of the space program in the foreseeable future require detector lifetimes of at least five years, which puts strong demands on the cryogenic systems.

There are several viable technological approaches under study in this category. These include resistive calorimetry, dielectric calorimetry, kinetic inductance read-out schemes, and superconducting tunnel junction arrays. The tunnel junction arrays, in particular, appear especially promising; however, all are deserving of further support. Fabrication of some of these kinds of devices may be outside the scope of what can be achieved by a university group with a typical SR&T grant, so NASA should consider some other funding options in this area. In addition, nearly all of these designs require cryogenic systems capable of holding stable temperatures at the sub-Kelvin level. NASA should assure additional funding for cryogenics technology development to guarantee that the special needs of these devices can be met with practical flight systems.

Other Basic Technology In identifying the major areas of basic technology, it is important that we also emphasize the importance of continuing a variety of other, more specific development activities. In effect, we must maintain a balance between large and small research programs just as we must maintain a balance between large and small missions. It is clear that remarkable returns have been achieved from modest investments in the SR&T program. During the long dry spell of the 1980s in terms of flight opportunities, the SR&T program has delivered an impressive number of successes in technology development. That effort has to be continued while we proceed with the missions of the 1990s. A partial list of areas for further investigation includes:

- high pressure gas counters and liquid noble gas detectors
- synthetic multilayers (the goal should be 2-D spacings of $\sim$ 10Å, both as Bragg diffractors and normal

incidence mirrors)
- ultra-thin windows
- improved reflection gratings
- improved transmission gratings
- various types of polarimeters

γ-ray Astronomy

It is likely that the scientific objectives of γ-ray astrophysics in the 21st century will be addressed by multiple missions, involving a mix of platforms for scientific observations. Examples include: a broad-band high-sensitivity, high-spectral resolution mission in the 5 keV to 10 MeV energy range with angular resolution of $1'$ or better, a high-sensitivity, high-energy mission in the energy range 30 MeV to 100 GeV, also with angular resolution better than $1'$, and a mission capable of high-sensitivity, high-spectral resolution observations of γ-ray bursts. Several technology thrusts are readily identifiable to enable such missions. Significant improvements in angular resolution, energy resolution, and sensitivity are both required by the scientific goals, and feasible given anticipations for improvements in technology during the next decade. Specifically, investment is needed in the development of advanced detector technologies.

A common characteristic of the most promising new detector technologies is detailed event visualization, *i.e.*, excellent spatial resolution for identifying the primary interaction of the γ-ray and reconstruction of the geometry of secondary photon and particle interactions. Such event visualization is critical over the entire hard X-ray and γ-ray energy range for determining the photon arrival direction accurately and for improved sensitivity due to enhanced background rejection. Typically, sensitivity is correlated with size of the detector systems, so development of cost-effective, large-area detector technologies are important. Candidates for future technology development include high-pressure gas detectors, liquid Xe and Ar detectors, imaging scintillation detectors, position sensitive Ge and Si detectors, superconducting transition detectors, and high-energy bolometer systems. These are typically the same types of detector systems under development in high-energy and nuclear particle physics. It is difficult to currently assess which of the emerging technologies will be most attractive for 21st-century space missions. A prudent approach for technology investment would be diversification, with support both for investigating the feasibility of new detector concepts, and bringing to maturity those concepts already demonstrated to be feasible for space mission application.

In addition to detector technology, investment is required in various support technologies. These are likely to include large structures for imaging approaches such as coded-aperture and Fourier transform imaging, cooling technology for several of the promising detector systems, and advanced electronic and optical readout technologies.

Policy Issues

Changes in NASA Management Style

NASA science management has been responsive to the astronomy community in formulating a program whose content reflects community scientific priorities. NASA has also been successful in providing a continually growing pool of monetary resources for science, which has been especially enhanced in the past few years in the areas of data analysis and theory. One area in which there has been a growing problem, however, is the decreasing frequency with which instrumentation can be placed in space. For major missions, the timescale from initial study to fruition is now approximately a generation. For moderate missions of the Delta-class Explorer variety, the timescale is only a factor of two smaller, at 10–15 years. It is not clear that even small missions can be mounted much faster than a decade with standard NASA management practices.

The unfortunate policy error that resulted in the suspension of expendable launch vehicles in favor of the Space Shuttle also resulted in a management style that stresses formal safety, reliability and accountability over innovation and sensible risk. The *Challenger* tragedy is at least partially responsible for this cautionary approach to the development of flight hardware. The administrative burden of properly documenting even the smallest space mission results in stretching out the development schedule and therefore increasing cost. Furthermore, the formal reporting procedures may not even be the most effective way of providing the level of reliability that NASA desires. The cost/benefit ratio of this approach is not at all clear, as instrument development costs per pound of payload typically exceed launch costs by a factor of 10 or more.

NASA has already recognized some aspects of this problem and has begun to address it. A "mixed fleet" policy is now in effect, so that expendable launch vehicles can be used for at least some missions. The SMEX (SMall EXplorer) Program is an attempt to alter the management style of the smallest missions by establishing an in-house project team to produce standard spacecraft systems.

We encourage NASA to continue these efforts and to guard against their inevitable tendency to slip back toward the more cumbersome and costly approach. We also urge investigation of other changes in management style for space missions, such as less concentration on formal documentation and management oversight, and more direct management responsibility for involved scientists. The very successful example of the Japanese X-ray astronomy program, which features a fixed budget on a fixed schedule, where scientists and their small management teams make all the tradeoff decisions, is an interesting paradigm. We would like to see NASA investigate the utilization of similar management practices, and we suggest experimenting with such innovative management approaches on the "low" end, with the missions that have smallest costs. NASA is already making a first step in this direction with the HETE project.

The important issue here is flight frequency. Timescales of more than a decade for most missions dissuade much of the community from participating in the development of space missions; most university scientists are now extensive users of NASA data, but only a few contribute to the development of new missions. The negative implications for the training of the next generation of graduate students is obvious and could be disastrous to the future health of space science.

Use of Expendable Launch Vehicles Versus Manned Missions

We support the current NASA policy that manned vehicles should be used only for those launches where the presence of a man is mandatory for completion of the experiment. We therefore strongly recommend that all future high energy astrophysics experiments be launched on Expendable Launch Vehicles (ELVs), except in those cases so far into development that a change in plans would cause severe financial disruption. (Those experiments well matched to Space Station *Freedom* obviously also require manned support). A corollary of this recommendation is that, lacking some new and compelling rationale to the contrary, the concept of recoverable/interchangeable buses should be abandoned.

Barriers to Mixing Ground-Based/Space-Based Funding

Multi-wavelength studies of high-energy phenomena in the Universe are becoming increasingly fruitful, and are crucial for solving a significant subset of problems. These studies are greatly facilitated when sponsoring agencies fund problem-oriented, as opposed to wavelength-oriented, research. We encourage continued examination of the structure and policies of funding agencies with these issues in mind.

Lunar Base

In the time interval considered by this report, we can see no compelling experiments which can be performed only from the lunar surface. On the other hand, the moon might on longer timescales provide an effective site for future high energy astrophysics missions that require large, stable structures (such as long focal-length telescopes, coded apertures, large detector arrays), or that can make good use of the lunar soil for shielding (such as γ-ray spectrometers). The technology developments described above for earth-orbiting experiments will be a prerequisite to exploit future lunar experiments in X- and γ-astronomy. At the same time, planners of lunar initiatives should keep several strawman high energy instruments in mind as they plan the capabilities of the future lunar bases.

Mission Operations and Data Analysis Funding

It is hardly novel to emphasize that Mission Operations and Data Analysis (MODA), rather than construction of spacecraft and instrumentation, are the true goals of scientific research from space. MODA funds are invariably a small fraction of the capital costs of most projects; unfortunately there has often been severe pressure on the NASA MODA line item during times of budget difficulties in the past decade. If the scientific program advocated in this report is to be implemented, MODA funds must not only be successfully protected from such cuts, but expanded in a manner commensurate with the expansion of flight opportunities. The survival of graduate students and postdoctoral fellows is closely tied to the health of MODA funding, making this issue central to the question of the next generation of space scientists as well. We hope that in the next decade, high energy astrophysicists can work together with their sponsoring agencies to heighten Congressional awareness of the critical importance of MODA funding.

Line Item for International Instrument Opportunities

The opportunity to develop and fly individual instruments on foreign spacecraft has emerged as an important and very cost effective way for US investigators to build hardware, and for the US community at large to obtain data. In the past, each of these initiatives has been handled as a special case, often with extraordinary effort required from NASA Headquarters and the investigators. The NASA Astrophysics Division has attempted unsuccessfully to obtain a regularly recurring budget line item specifically to fund US participation in foreign space missions. We strongly commend this approach to financing these missions, and urge its implementation immediately. Each such opportunity should be reviewed by the appropriate NASA scientific advisory committee. Instrumentation for these opportunities should be selected by open competition and peer review wherever possible, and the needs for incremental MODA funds created by each such opportunity must be kept in mind.

Smaller NASA Programs

Sounding rockets We perceive that a substantial fraction of the funds currently expended in NASA-supported sounding rocket programs goes towards detector development, rather than to acquisition of unique scientific data. We therefore believe that consideration should be given to more overtly redirecting a portion of funds from the sounding rocket program to detector/technology development, where they may be more efficiently expended and are likely to produce a greater return.

Balloons We note that the γ-ray balloon program has been greatly oversubscribed recently, and is producing exciting science as well as serving as an essential testbed for instrumentation development. We recommend doubling the current scope of this modest program.

Support of young scientists The key problems of training the next generation of space scientists are well-known and have been discussed above: even if funds are available to support the best graduate students, the time required to complete most exciting projects has grown so long that such projects are hopelessly incommensurate with the graduate education programs. As a result, few if any students can see a space research project through completely, from the initial intellectual formulation of the problem to publication of results. Shortening the relevant timescales is a complex policy issue, but providing funding for support of junior scientists is not. We laud NASA for its recently-initiated Graduate Student Researchers Program, which, although limited in scale, represents a gradual return to the more halcyon days of graduate support of the 1960s and 70s. Likewise, the Hubble Fellow program is a welcome acknowledgement of the problems of supporting the best young scientists in our field. Programs such as these must be nurtured, expanded, and seeded to other agencies that support research in high energy astrophysics. A prerequisite for adequate support of graduate students is the maintenance of a healthy research base at universities where these students are trained. A careful balance of research support must exist between universities and other centers.

Theoretical Programs Theoretical research in support of space missions has always been crucial for advances in high energy astrophysics, and there are firm grounds to believe it will become yet more vital to the success of the missions of the 90s. As both X- and γ-ray astronomy shift an increasing fraction of their observations from imaging to spectroscopy, far more sophisticated theoretical models will be required to interpret the data. The cost of these indispensable modeling efforts is very small compared with the capital costs of virtually any mission, and even with the operations costs of most projects. Again we commend NASA for the recently instituted Astrophysical Theory Program, and urge its maintenance and vigorous expansion.

Laboratory Astrophysics The preceding comments on the rapidly increasing importance of spectroscopy in X- and γ-ray astronomy also impact laboratory astrophysics. It would be regrettable if data from elegant space-borne experiments cannot be adequately interpreted due to lack of fundamental laboratory data, especially given the sophistication of current techniques in laboratory plasma physics. We recommend that NASA and NSF support experiments in laboratory astrophysics, especially where there is promise of close connections to analysis of AXAF and GRO observations, or significant impact on the design of future instrumentation.

Two moderate missions	\$300 M
Four SmEx	\$100 M
Technology development	\$50 M
International budget line	\$50 M
Theory, suborbital	\$40 M

Table 1. Incremental Costs of New Initiatives for the Decade

Conclusion

High energy observations from space by American investigators have undergone a relatively quiescent phase during the 1980s, a time when important scientific problems have been amply clear, but opportunities for presence in space have been all too rare. The launches of AXAF and GRO, as well as a variety of smaller currently approved projects described earlier, give the United States the opportunity to regain much of the momentum it built in X- and γ-ray astronomy in the 1970s. The new program for the 1990s which we have described here is of modest incremental cost, as can be seen from the estimates in Table 1, but we believe will lead the discipline into the new millenium with an exciting array of new data, a technology base needed for future experiments, and an infrastructure of both scientists and facilities essential for leadership in the field.

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