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HORIZON
2000 Plus

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European Space Science in the 21st Century

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The nucleus of comet Halley as observed by ESA's Giotto spacecraft. This is a composite of 68 images taken by the Halley Multicolour Camera (Image courtesy of MPAe, Lindau)

Foreword

European Space Science in the 21st Century

This report describes ESA's future Long-Term Plan in Space Science, known as 'Horizon 2000 Plus', as submitted to ESA's Director General by the Survey Committee chaired by Prof. L. Woltjer. This Plan has been drawn up by the Agency in direct response to the request formulated by the ESA Council at its Meeting at Ministerial Level, in Granada in 1992.

Horizon 2000 Plus is a plan based on the ideas and aspirations of the scientific community of the ESA Member States (with some contributions from US scientists). It has been analysed and reviewed both by the scientists who advise ESA, and by those in special 'Topical Teams', formed especially to ensure the broadest coverage in terms of competence in the scientific disciplines that the ESA Space Science Programme usually serves, as well as the newly emerging discipline of fundamental physics. It can therefore be said without reservation that Horizon 2000 Plus is a plan drawn up by the scientific community for the scientific community, in accordance with the long tradition that has led to the formulation of earlier ESA plans, and 'Horizon 2000' in particular.

Horizon 2000 Plus has been formulated ten years after Horizon 2000 and at the mid-point of the latter's implementation, reflecting both the evolutionary concept being followed, and the need to maintain balance and continuity whilst opening the door to new fields of science. It is possible after ten years to assess the true value of the previous plan and to reinforce its most positive effects for the future, whilst at the same time attempting to correct some of the problems that have emerged.

Both Horizon 2000 and Horizon 2000 Plus are characterised by a combination of scientific ambitions and financial realism, of vision and wisdom, of stability and adaptation to the evolution of Space Science. Horizon 2000 has played an essential role in ensuring coordination between the ESA and national Space Science programmes. This is an absolute necessity in order to establish scientific complementarity and optimum cost efficiency for the global Space Science effort in Europe.

Horizon 2000 has made ESA one of the strongest agencies in the world, bearing in mind its budget, the number and size of its missions, and the level and size of the scientific community that it serves. In several areas of Space Science, ESA is in fact number one. This is undoubtedly the case in cometary science, astrometry, plasma physics, and solar and heliospheric physics, and will soon be the case in infrared, X-ray and gamma-ray astronomy also.

While Horizon 2000, and Horizon 2000 Plus, are intended to be undertaken by Europe alone, both programmes rely on international cooperation as a means of enriching the programme and increasing the scope of the missions. In the present post-cold war context, international cooperation is a necessity in Space Science as a means of attaining more ambitious goals whilst at the same time reinforcing stability and mutual confidence between the various space partners. Therefore, to foster early coordination of the world's Space Science programmes, ESA invited representatives of NASA, ISAS and RKA to attend the Survey Committee meetings as observers. With the comparative financial stability that the budget structure of its Science Programme provides, ESA is today's most reliable partner in Space Science, thereby attracting more and more offers of cooperation from renowned scientists outside its own Member States.

Horizon 2000 and Horizon 2000 Plus respect the independent spirit of the scientific community and its freedom to select the missions that best correspond to its ambitions, as well as embracing the necessary financial realism. European space scientists, and space scientists in general, are unwilling to be constrained by decisions to embark on large, politically-motivated programmes. They are, however, ready to take full advantage of the possibilities offered by the Space Station or a Moon programme when they provide unique facilities and an efficient means of conducting their research and testing new technologies. 'User friendliness' is the term that should characterise the future exploitation of such possibilities.

I am pleased to have this opportunity to thank all of those who have contributed so actively to the formulation of this new plan, not least all those who submitted so many excellent and often visionary mission concepts, through the Space Science Advisory Committee, the Working Groups, the Survey Committee and its Topical Teams. It is also a great pleasure to thank Prof. L. Woltjer for the invaluable work he has accomplished and the time he has unstintingly devoted to this effort.

May Horizon 2000 Plus achieve the same success as its forerunner!

R.M. Bonnet
Director of the ESA Science Programme

Introduction

This report presents the proposed update of ESA's long-term programme in Space Science, known as 'Horizon 2000 Plus'. It covers the period 1995–2016 and represents the forward roll-over of the twenty-year Horizon 2000 programme developed in 1984 and due to terminate in 2006.

Horizon 2000 has made a major contribution to giving European scientists a stable and predictable Space Science programme with, nevertheless, sufficient flexibility to adjust to developments in Europe and elsewhere.

Planetary research, the study of the Sun and the Solar System, and all branches of astronomy, would nowadays be unimaginable without access to space. With relatively small means, ESA has succeeded in giving European scientists a programme of sufficient breadth and depth to allow important and exciting knowledge to be gathered and integrated within the overall progress of science. Not surprisingly, other areas of science – like fundamental physics – are now also discovering the potential of space and are claiming their rightful place in the Space Science programme. Although this is a healthy development, it stretches the Agency's resources to, or even beyond their limits.

As a first step in the preparation of the new plan, ESA issued a 'Call for Mission Concepts' in June 1993. The scientific community – several thousand scientists in Europe alone – responded massively, proposing some 110 new ideas illustrating trends in Space Science for the next century and representing that community's main areas of interest.

Subsequently, a 'Survey Committee' was set up to draft the plan that would later be called 'Horizon 2000 Plus'. That Survey Committee included the eight members of the Space Science Advisory Committee and eleven additional scientists. It was a representative body, with its members being closely associated with numerous national and international organisations. Members of the ESA Executive participated in the meetings of the Survey Committee. In addition, five Topical Teams, each with seven members, were appointed to survey the main science areas, while the Astronomy Working Group and the Solar System Working Group (with 14 and 15 members, respectively) surveyed the international Space Science environment. In all, 75 scientists from outside ESA participated in the work of the Survey Committee.

The Survey Committee met five times between December 1993 and October 1994. The Topical Teams and the Working Groups presented their reports to the Survey Committee and the ESA Science Programme Committee during a three-day meeting in May 1994 in Capri (I). Thereafter, the Survey Committee met in Rome for three days at the end of September. Following a last meeting with the Chairmen of the Topical Teams on the first day (29 September), the Survey Committee formulated its final recommendations on 1 October 1994. It had thereby arrived – with a surprising degree of unanimity – at its conclusions concerning the main elements to be added to Horizon 2000 for the transition to the Horizon 2000 Plus programme, which is outlined in the following chapters.

I wish to express my personal satisfaction at the willingness of so many scientists in the Survey Committee, in the Working Groups, in the Topical Teams, and in the community at large, to devote so much of their time to the optimal formulation of a programme that will be executed in part by the next generation of space scientists. I also wish to express the indebtedness of the Survey Committee to the ESA scientists and engineers who have contributed so much to giving the programme the necessary realism, frequently by carrying out studies that had to be executed at very short notice.

Europe has a long tradition of research in the fundamental physical sciences and the overwhelming response of the Space Science community on this occasion again augurs well for the future.

L. Woltjer
Chairman of the Survey Committee

Why Space Science?

The Science Context

Planetary scientists and astronomers have, in the course of this century, revealed the true complexity of the world we live in. Instead of the static Universe of the middle ages, it appears to be full of change and evolution. Many unexpected phenomena and novel kinds of objects have been discovered. We are interested in learning more about the planets, stars and galaxies because they represent our larger environment, in the same way as earlier generations explored the Earth's geography. But there is more. It has become clear that external influences have been and are of importance to the atmosphere and the biosphere of the Earth, and the study of our environment helps in identifying these factors. Finally, while we have learned much about the laws of physics from laboratory studies, other aspects can only be identified from space. Here, we come to perhaps the deepest problems in the natural sciences, namely the nature and origin of matter on the one hand, and the origin and evolution of life on the other. In the elucidation of all of these problems, well-instrumented spacecraft will make an essential contribution.

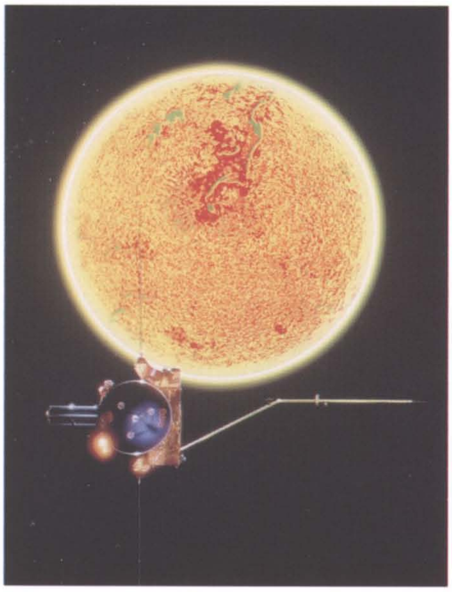
The nature of matter is being extensively studied in particle accelerators, as at CERN. Much has been learned about three of the four forces of nature – the strong, electromagnetic and weak interactions – and many particles have been discovered. The gravitational force, however, is so weak as to have eluded Earth-based physicists. In addition, very massive particles probably do exist which cannot be reproduced with the energy available in accelerators. Gravity is the dominant force in the Universe and becomes strong in situations of very large masses of matter confined in relatively small volumes. In addition, in the early phases of the expanding Universe, extremely energetic processes appear to have been created which may well have left various particles that cannot be reproduced in the laboratory.

Einstein developed a fundamental theory of gravity – his General Theory of Relativity – which has profoundly affected our notions about space and time and which accounts also for small observed discrepancies in planetary motions with respect to Kepler's Laws. But Einstein's theory cannot be the final truth because, in a fundamental way, it is incompatible with the quantum theory of matter that has been verified in many experiments. Einstein's theory has only been tested in very weak gravitational fields, where its most specific predictions have only minor importance. Strong gravitational fields may manifest themselves in two ways. When hot gas is confined in a strong gravitational field, X- and gamma-rays will be emitted, while when conditions are rapidly changing, such as during the merger of two massive black holes, gravitational waves should be generated. In the Horizon 2000 Programme, major X- and gamma-ray observatories – XMM and Integral, following up the pioneering work of Exosat and Cos-B – are already being implemented, whilst in Horizon 2000 Plus a space-based gravitational-wave observatory is being proposed. Gravitational waves have never yet been directly detected. The gravitational waves in question, as well as the X- and gamma-rays, can only be observed from space.

Matter in the Universe is not distributed uniformly, but is concentrated in a hierarchy of denser bodies, galaxies, stars and planets. Only a rudimentary understanding exists as to how this came about. To make further progress, we would like to know how our Milky Way Galaxy and its neighbours have formed. To find out, we should study the


Hipparcos


Giotto


Ulysses

motions of the stars in our Galaxy. Only space-based instruments can attain the accuracy needed to survey stellar motions and distances throughout our Galaxy. In Horizon 2000, a beginning has been made with the Hipparcos satellite, but a far greater precision is needed to explore the farther reaches of our Galaxy. In Horizon 2000 Plus, it is proposed to build an interferometric observatory capable of the task. This should also allow the indirect detection of planets around nearby stars, by measuring the reflex motion of these stars.

From the diffuse gas in our Galaxy new stars form. In the context of Horizon 2000, ISO and First are being readied to study this gas in detail. These studies will have a direct impact on our understanding of the origin of the Sun and the planets, which must also have formed from the interstellar gas. To further elucidate this origin, we can study the primitive material found in comets, and in Horizon 2000 Giotto and Rosetta have been foreseen. However, the study of the nearer, more Earth-like planets is of equal interest in this context, also from the point of view of comparative geology and climatology. In fact, an independent evaluation of the adequacy of the climate models for the Earth, so important in the context of global change, can only come from a comparison with other planetary atmospheres. Horizon 2000 Plus foresees missions to Mars and Mercury.

Without question, for mankind the Sun is the most important body in the sky. With the exception of nuclear energy, it is the source of all energy on Earth, from oil to hydroelectric energy. It is through the Sun's presence that the Earth came to life and we exist. Observation of the Sun remains of the highest scientific interest, because it directly influences conditions on Earth, through its radiation and through the solar wind which interacts with the terrestrial magnetosphere. Furthermore, the Sun is the only star which we can study in great detail and is of considerable importance in testing our ideas about stellar structure and evolution. The solar wind and the magnetosphere are also an important laboratory for plasma physics. Horizon 2000 included Ulysses, Soho and Cluster (the latter two to be launched in 1995), and further missions are to be foreseen in Horizon 2000 Plus.

The nature and origin of life is, of course, a problem studied extensively by molecular biologists. The question of the circumstances under which it arose and its possible uniqueness to Earth bring us again into the domain of space science. Two questions need to be answered:

- Are there other planets that could support life as we know it?
- If so, could we detect the presence of such life?

The answer to the first question requires an understanding of planetary systems and the conditions at planetary surfaces. Mars is directly relevant to the question of the ubiquity of life. If conditions on early Mars turn out to have been not too different from those on early Earth, the presence or absence of life at that time will have profound implications.

The direct observation of planets around other stars would provide important evidence. Technologically, however, it is a formidable, though soluble, problem. As a long-term goal for the Horizon 2000 Plus or thereafter, planetary detection and spectroscopic observation for the possible presence of oxygen – believed to be abundant only on planets with life – appears to be a promising aim.

Science and Society

Each generation leaves its legacy to the future. This is an unstructured but inevitable process, which has so far never been halted by selfish considerations. Plagues, wars, famines and all sorts of social problems have always dogged mankind, but have never stopped one generation from attempting to leave something worthwhile, however modest, for posterity. There is a legacy of ideas and ideologies which affect us deeply and can change our society dramatically, but these come and go. There are other legacies that, once established, last forever. Such are science, the exploration of the Universe and technological progress. All try to provide an answer to a fundamental question of man, that of finding his place in this world. It is fortuitous that space science is able to contribute, no matter how modestly, to all of these everlasting legacies. This document will make clear how we propose to proceed.

In the course of this century, man has completed the exploration of the Earth, has developed communications and informatics to a previously unimaginable degree, and has walked on the Moon. However, at this precise moment it seems that this enormous drive towards discovery has lost its momentum. We have entered the age of balanced budgets at all costs, and strictly monetary-based assessment of every human activity. Yet, the fundamental question has not been answered and, in fact, some solutions that thus far appeared to be acceptable have lost their appeal. To say that space science can solve all problems and answer all questions would be excessive, but at least it offers real progress in the right direction. Moreover, it is true to say that space science requires technology of the highest quality and stimulates technological progress, and thus can be of immediate value to our daily needs. But it also has an added educational value, because it offers progress that is not tied to our selfish interests of this precise moment. It gives scope and hope to our quest, and this is precisely what we need at this moment.

Can Europe afford Space Science?

The merits of the European space-science programme are clear, but is such a programme within Europe's financial means? A first point to be noted is that, as European or other programmes go, ESA's Space Science Programme is not particularly expensive. In 1995, it accounted for only 12.8% of the overall ESA budget. It costs only half as much as the European programme in particle physics, and four times less than the corresponding NASA space programme.

Space activities contribute significantly to the technological and industrial capabilities of Europe. The technological requirements of space science are particularly stringent. In this competitive international environment, scientists and engineers are highly motivated to push new technologies to their limits.

The impact of space activities on industry has been evaluated in various contexts on several occasions. A study carried out in 1987/88 by the Bureau d'Economie Théorique et Appliquée of the University of Strasbourg specifically addressed the economic impact of ESA's programmes. Four categories of effects were analysed, namely technological effects, work-related effects, commercial effects, and effects on organisation and methods. The conclusion reached was that, on average, every 100 units paid by ESA to European Industry produced indirect effects to a value of 320 units, for firms working on contracts for the Agency. The long-term, wide-range effects were not included in this estimate, which is therefore certainly a conservative one.

Viewing the costs and benefits, it would seem that a clear answer emerges:

Europe cannot afford to be without a significant, competitive Space Science Programme.

Chapter I

Coherence, Balance and Continuity with Horizon 2000

It is appropriate at this point to consider the Horizon 2000 programme's evolution since its inception in 1984, and to assess what lessons can be learned from the experiences of the past ten years for the planning of a follow-up programme.

The framework for the definition of Horizon 2000 was established by selecting a number of criteria that should be obeyed by a properly balanced long-term Space Science programme. The main criteria were:
- highest scientific standard (paramount criterion)
- a suitable mix of large and smaller projects
- flexibility and versatility to match scientific evolution
- continuity of effort in scientific institutes and industry
- high technological content
- realistic budgetary limits
- proper balance between purely European and cooperative projects with other agencies.

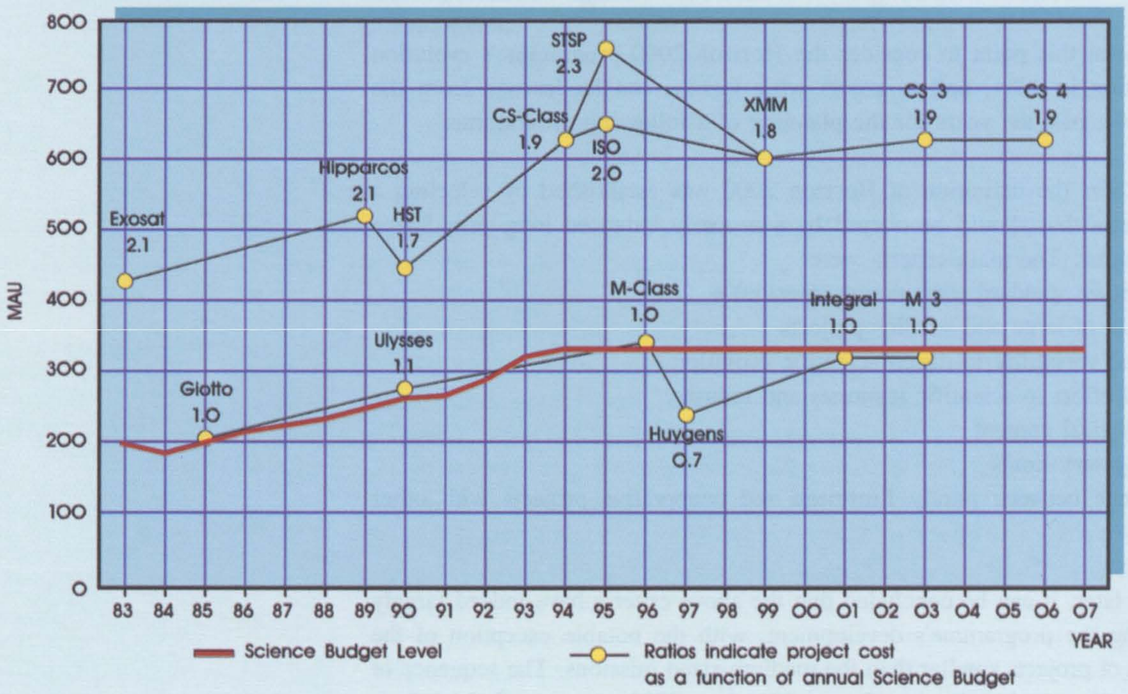
Now, ten years later, it can be concluded that the above criteria have indeed largely been met during the programme's development, with the notable exception of the implementation of projects smaller than the medium-sized missions. The sequence of missions already launched in the time frame of Horizon 2000 – many of which have already far exceeded their nominal lifetimes – and those still to be launched as part of that programme, are shown in the lower figure on the next page.

A 'Roll-Forward' Approach

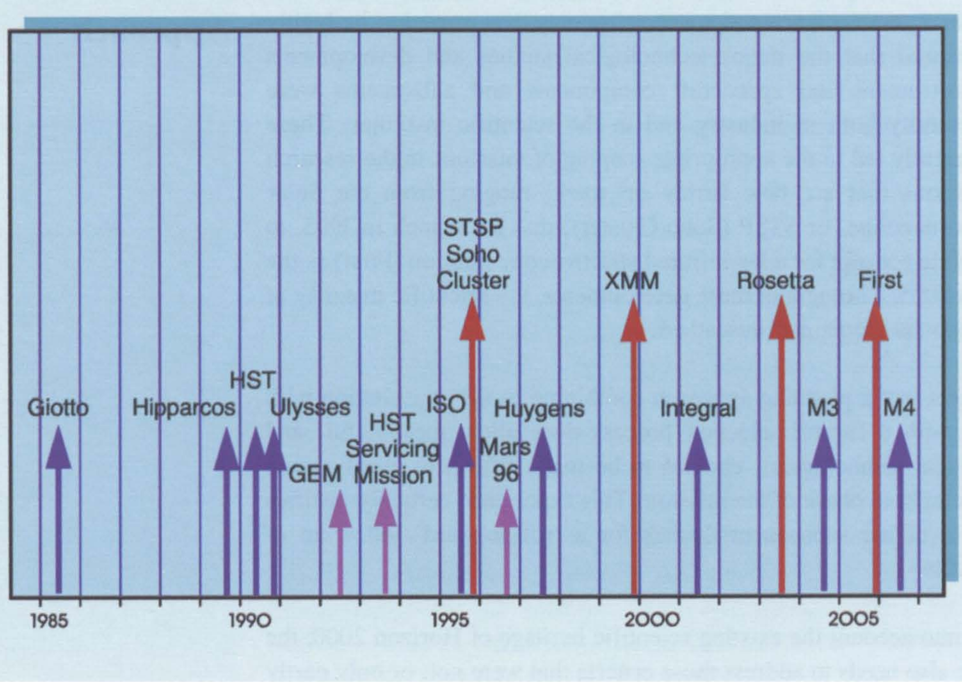
The timely identification of major scientific endeavours, designated 'Cornerstones' in Horizon 2000, in Solar System science and space astronomy, has proved to be highly successful. It has ensured that the major technological studies and developments related to critical instrument and spacecraft components and subsystems were accommodated successfully both in industry and in the scientific institutes. These activities have subsequently led to the appropriate scoping of missions in the research areas identified, missions that are now firmly on track: ranging from the Solar Terrestrial Physics Cornerstone, or STSP (Soho/Cluster), due for launch in 1995, to a technologically feasible concept for a far-infrared spectroscopy mission (First) as the fourth Cornerstone in 2005. Throughout these developments, the scientific integrity of the Cornerstone themes has remained unscathed.

This track record shows that a planning approach combining broad consultation with the user community with a careful selection process does allow meaningful, and indeed desirable (critical technologies), choices to be made fifteen to twenty years prior to the actual operational phase of the mission. This experience certainly justifies the current attempt to define subsequent themes for a 'roll-forward' follow-up of Horizon 2000 until 2016.

In addition to taking into account the existing scientific heritage of Horizon 2000, the follow-up programme also needs to address those criteria that were not, or only partly met thus far.



Medium and Cornerstone mission costs, as a function of the level of the Science Budget



Implementation of Horizon 2000 (1985 – 2006)

Scientific Heritage of Horizon 2000

Balance
Horizon 2000 established a certain balance between the various disciplines of Space Science. In the Solar System area, Cornerstones were established for research on the solid bodies (Rosetta) and on the Sun and plasmas (Soho/Cluster). In Astronomy, one Cornerstone dealt with high-energy phenomena (XMM) and another with the cool matter of the Universe (First). The medium missions were chosen so as to strengthen these central areas or to fill gaps between them.

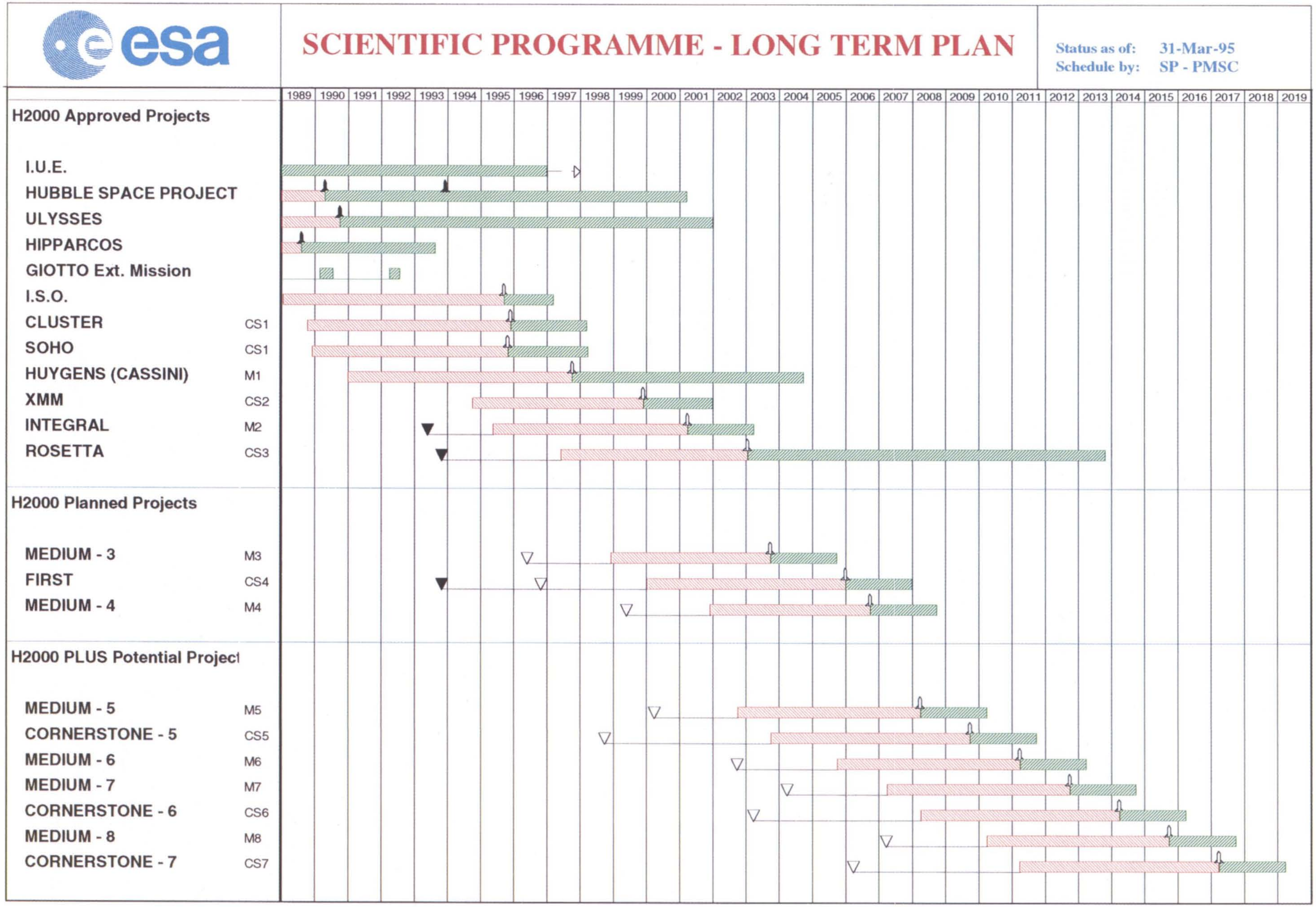
There is, of course, no fundamental reason why exactly the same balance should be maintained in the future. However, it is important to note that science progresses over a broad front and that progress in one area generally requires progress in other areas as well. We cannot sensibly discuss the origin of the Solar System without understanding the processes of star formation, and we cannot satisfactorily analyse X-ray sources without also having optical or infrared data. A very narrowly focused Space Science programme would therefore risk becoming sterile. A certain balance between the different areas is undoubtedly necessary.

The concept of Horizon 2000 Plus, as a roll-forward programme, needs to demonstrate clearly its consistency and coherence with the current Horizon 2000 activity. First of all, this requires a confirmation or a recalibration of the outstanding scientific problems as identified for each of the main research areas in Horizon 2000, and how these can best be further pursued, given the anticipated scientific return of the Horizon 2000 programme elements. In terms of continuity with Horizon 2000, the time period covered by Horizon 2000 Plus is too limited to be judged as a separate entity: one should rather regard the second part of Horizon 2000 and Horizon 2000 Plus as an integrated programme starting from 1995 onwards. Choices made in Horizon 2000 Plus will also feed back into the Horizon 2000 run-out in terms of timely accommodation of study and (technological) development work.

Continuity
Another important factor is the need for a certain continuity and predictability. The individual research communities cannot remain viable if the intervals between missions become too long. In each of the research areas, communities have been created over the last decades which are able to conduct research of the highest level. The ESA Member States have invested much in these communities – far more than just their contribution to the Agency. It would be wasteful if these highly successful research communities were to wither due to excessively long intervals between the influxes of new data. By the same token, a certain continuity in the technological and industrial developments is also necessary.

When one considers the continuity aspects in such an integrated programme, a number of observations can be made with respect to the scientific timeliness of the themes identified as green boxes in Horizon 2000 – the so-called 'green dreams'. These areas represent the '1984-wisdom' for post-Horizon 2000 compelling science.

In the area of solar and heliospheric physics, a solar probe is still regarded as a scientifically very fundamental and challenging mission. However, Europe does not yet have the full technological capability necessary to carry out a multi-pass mission, and so the solar probe cannot be considered as a suitable candidate for an early


SCIENTIFIC PROGRAMME - LONG TERM PLAN
Status as of: 31-Mar-95
Schedule by: SP - PMSC
1989 1990 1991 1992 1993 1994 1995 1996 1997 1998 1999 2000 2001 2002 2003 2004 2005 2006 2007 2008 2009 2010 2011 2012 2013 2014 2015 2016 2017 2018 2019
H2000 Approved Projects
I.U.E.
HUBBLE SPACE PROJECT
ULYSSES
HIPPARCOS
GIOTTO Ext. Mission
I.S.O.
CLUSTER CS1
SOHO CS1
HUYGENS (CASSINI) M1
XMM CS2
INTEGRAL M2
ROSETTA CS3
H2000 Planned Projects
MEDIUM - 3 M3
FIRST CS4
MEDIUM - 4 M4
H2000 PLUS Potential Project
MEDIUM - 5 M5
CORNERSTONE - 5 CS5
MEDIUM - 6 M6
MEDIUM - 7 M7
CORNERSTONE - 6 CS6
MEDIUM - 8 M8
CORNERSTONE - 7 CS7

Horizon 2000 Plus Cornerstone. On the other hand, given the high-quality research opportunities in solar physics offered by the STSP Cornerstone and other future missions, the solar probe should still be regarded as a prime candidate for a near-future cooperative project with other agencies.

In the area of planetary science, the Mars rover was identified as a prime candidate for the post-Horizon 2000 period. Given the great international interest in the study of Mars, an explicit effort by ESA, although not necessarily in the form of a rover, to become actively involved as a visible partner in the framework of the current Mars exploration projects should certainly be pursued. Since the magnitude and the timing of Mars exploration projects will be dictated in a global framework for collaboration, this activity does not fit into the Cornerstone concept. ESA participation in Mars studies does not impair the continuity of cometary science, in which Europe has achieved internationally recognised leadership through Giotto and the Rosetta Cornerstone, since cometary science is well covered until 2013 (Rosetta rendezvous) and does not require follow-up commitments yet.

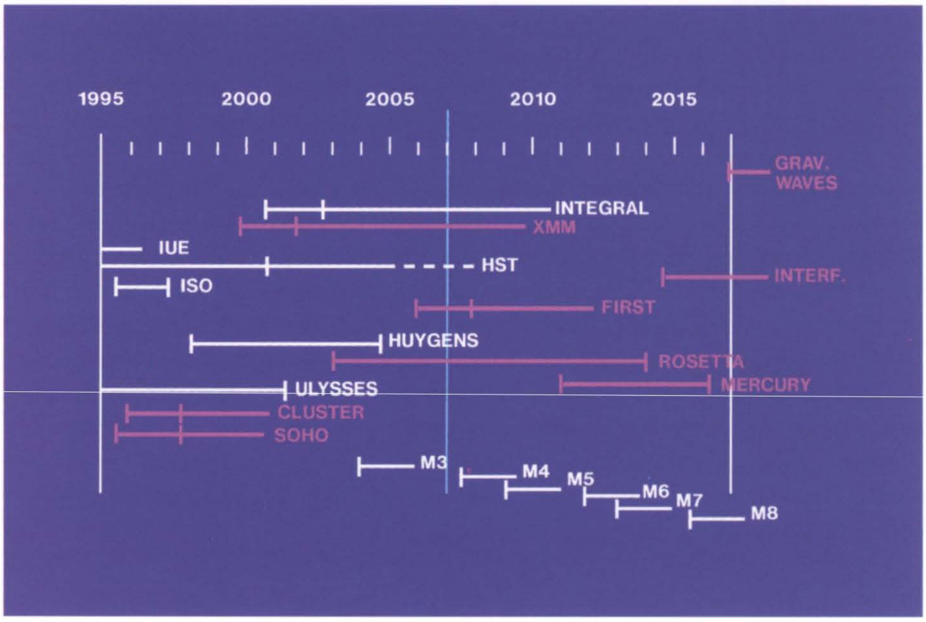
In the area of astronomy, two-dimensional interferometry in the optical, infrared or millimetre wavebands was considered the next major step for space astronomy in the post-Horizon 2000 era. Today, this is indeed considered to be a very challenging prospect in several areas. In particular, absolute astrometry at the 10 microarcsec level would constitute a very powerful scientific tool allowing investigation of the dynamics of the Milky Way (including distances, motions, and hidden companions of stars), as well as of nearby galaxies (global optical astrometry). Space interferometry would also open the way to the search for planets outside the Solar System (infrared interferometry). Although in-depth ground verification is still required to demonstrate the feasibility of these techniques in space, interferometry does constitute a scientifically very promising candidate for consideration as a next astronomy Cornerstone mission on the Horizon 2000 Plus time scale.

In the area of fundamental physics, no 'green box' was identified in Horizon 2000, but the far-reaching impact of gravitational-wave detection and experiments in General Relativity was explicitly highlighted in the Horizon 2000 report. Over the past years, several concrete proposals have indeed been submitted, e.g. testing of the equivalence principle (STEP) and low-frequency gravitational wave detection (LISA). This shows that the interest in space-borne research in fundamental physics is growing and should now be seriously considered for inclusion in a long-term programme.

The Case for Smaller Missions, Eureca and Space Station Utilisation

An important element within the flexible part of the Horizon 2000 programme that has not yet 'emerged' is the implementation of smaller size projects and the programmatic utilisation of space-infrastructure elements such as Eureca and the Space Station. New and cheaper science opportunities have, however, arisen through the extension of the IUE, Giotto and Ulysses missions. In fact, a fair amount of confusion has dominated the discussions on small missions, mainly due to the lack of a clear definition of their (scientific) scope and potential role in the Science Programme. This problem is addressed in Chapter II.

Timeline for the Horizon 2000 and Horizon 2000 Plus programmes



Although the potential use of the Space Station was generically alluded to in Horizon 2000, at the time of Horizon 2000's inception the Station was still ill-defined and its fate utterly unclear. However, given the present definition and the international commitment to the international Space Station, Horizon 2000 Plus should take a much more explicit and quantitative position with respect to the Station's capability and compatibility for Space Science by selecting specific candidate missions for in-depth implementation studies. This topic is discussed in Chapter IV in relation to the necessary development of new technologies and the need to ensure continuity in several disciplines.

Chapter II

The Pillars of Horizon 2000 Plus

Scientific Excellence

Excellence of science has been and still is the watchword of the ESA Science Programme. It could easily have been otherwise, but the use of excellence as the primary criterion for choosing the programme is critical. It alone is the yardstick that meets complete approval across the entire scientific community. It was scientific excellence that unified the scientific community behind the original Horizon 2000 plan.

Excellence is safeguarded as the ultimate selection criterion for the programme by the integration of peer-review groups of scientists from the community into the operation of the Science Directorate. These groups oversee the programme content and have consistently endorsed the use of scientific excellence as the ultimate criterion for choosing any element of the programme. As such, it is the unifying element of the programme.

Excellence as a criterion is not arbitrarily chosen. ESA's Science Programme is unique in that it must represent the aspirations of scientists who come from the enormously diverse cultural and technical backgrounds of the fourteen Member States.

Excellence, although primary, is not the only criterion for the structuring of the programme. European industry must be challenged by the programme. European technological skills must be exploited and also challenged. The programme must play to the strengths of the European science community and yet, operating on the time scales it does, it has also to provide underpinning for change and the development of science.

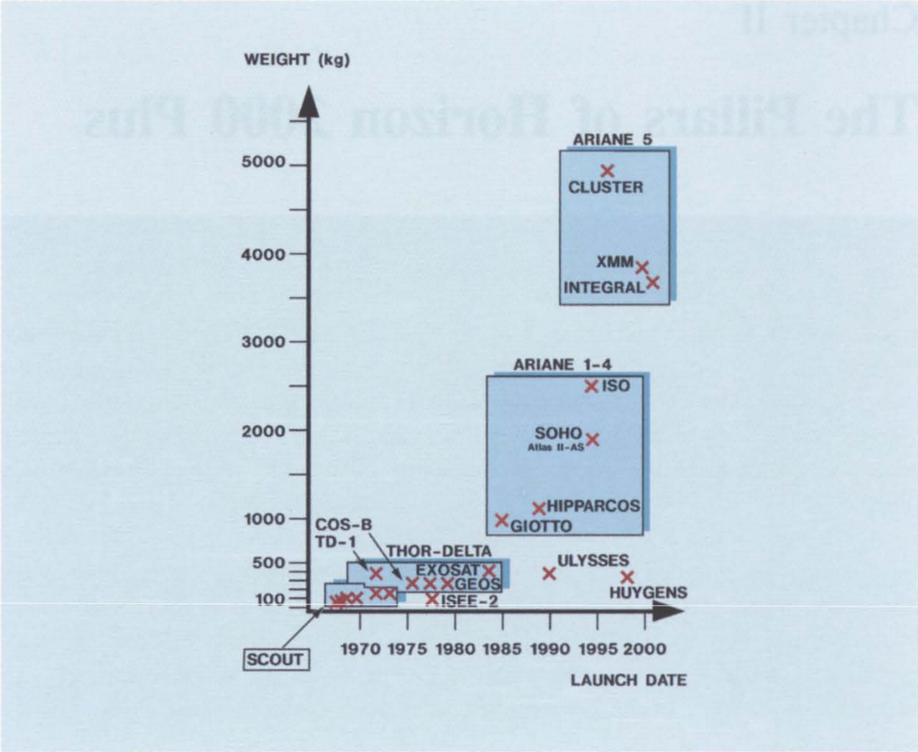
Realism

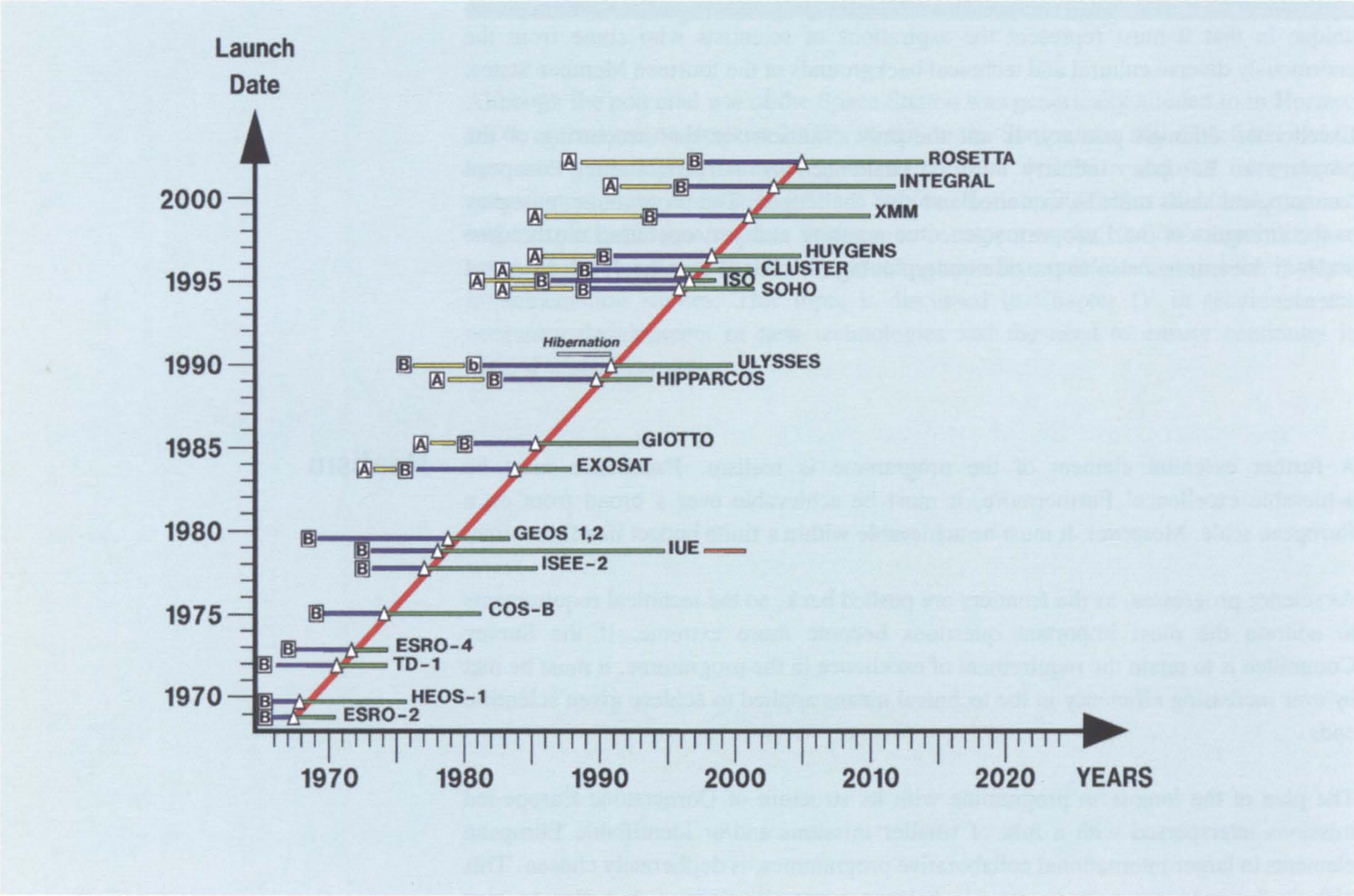
A further essential element of the programme is realism. Excellence must be achievable excellence! Furthermore, it must be achievable over a broad front on a European scale. Moreover, it must be achievable within a finite budget in a finite time.

As science progresses, as the frontiers are pushed back, so the technical requirements to address the most important questions become more extreme. If the Survey Committee is to retain the requirement of excellence in the programme, it must be met by ever increasing efficiency in the technical means applied to achieve given scientific ends.

The plan of the long-term programme with its structure of Cornerstone Europe-led missions interspersed with a mix of smaller missions and/or identifiable European elements in larger international collaborative programmes, is deliberately chosen. This plan is designed not only to provide balance across disciplines, but also to give European scientists an achievable leading world position in those disciplines within realistic financial boundaries.

Weight of ESA science spacecraft as a function of their launch date. The rectangles indicate roughly the weight capabilities of the various launchers. Ulysses was launched by the US Space Shuttle and the weight shown corresponds to the dry mass of the satellite. Huygens is also a special case, being carried aboard the US Cassini satellite to Saturn and Titan. The Hubble Space Telescope would be off-scale in this illustration.





The durations of the various phases of ESA science projects, indicated by the horizontal bars. The A's and white bars indicate the Phase-A starts and durations respectively. The B's represent the start of Phase B in industry and the grey bars represent the duration of Phase B and C/D. The launches are represented by white triangles, while the black bars indicate the durations (actual or foreseen) of in-orbit operations. Note the difference in the durations of Phases B/C/D for the Delta and Ariane families, and the near constancy of these Phases for each individual family.

Balance

It is clear that, given the limited Space Science budget, a delicate balance must be struck between the diversity of fields and the frequency of missions in each broad field. The Survey Committee has concluded that an acceptable balance is possible – but only just – with the disciplines included in Horizon 2000.

The foregoing does not imply that the Space Science programme should be closed to the addition of innovative fields. A case in point is the area of fundamental physics, where exciting prospects abound. However, the inclusion of such new areas requires corresponding budget adjustments if serious damage to the continuity and the balance of disciplines included in Horizon 2000 is to be avoided over the duration of the programme.

Need for Advanced Technologies

The ESA Science Programme has long been considered both a user and a driver of advanced technologies. Long-term technological preparation was the primary justification for the definition of the Cornerstones, sometimes, as in the case of First and Rosetta, 20 years ahead of their actual launches. It has proved to be a very efficient way of bringing the objectives of these missions within the bounds of realism and lowering their cost.

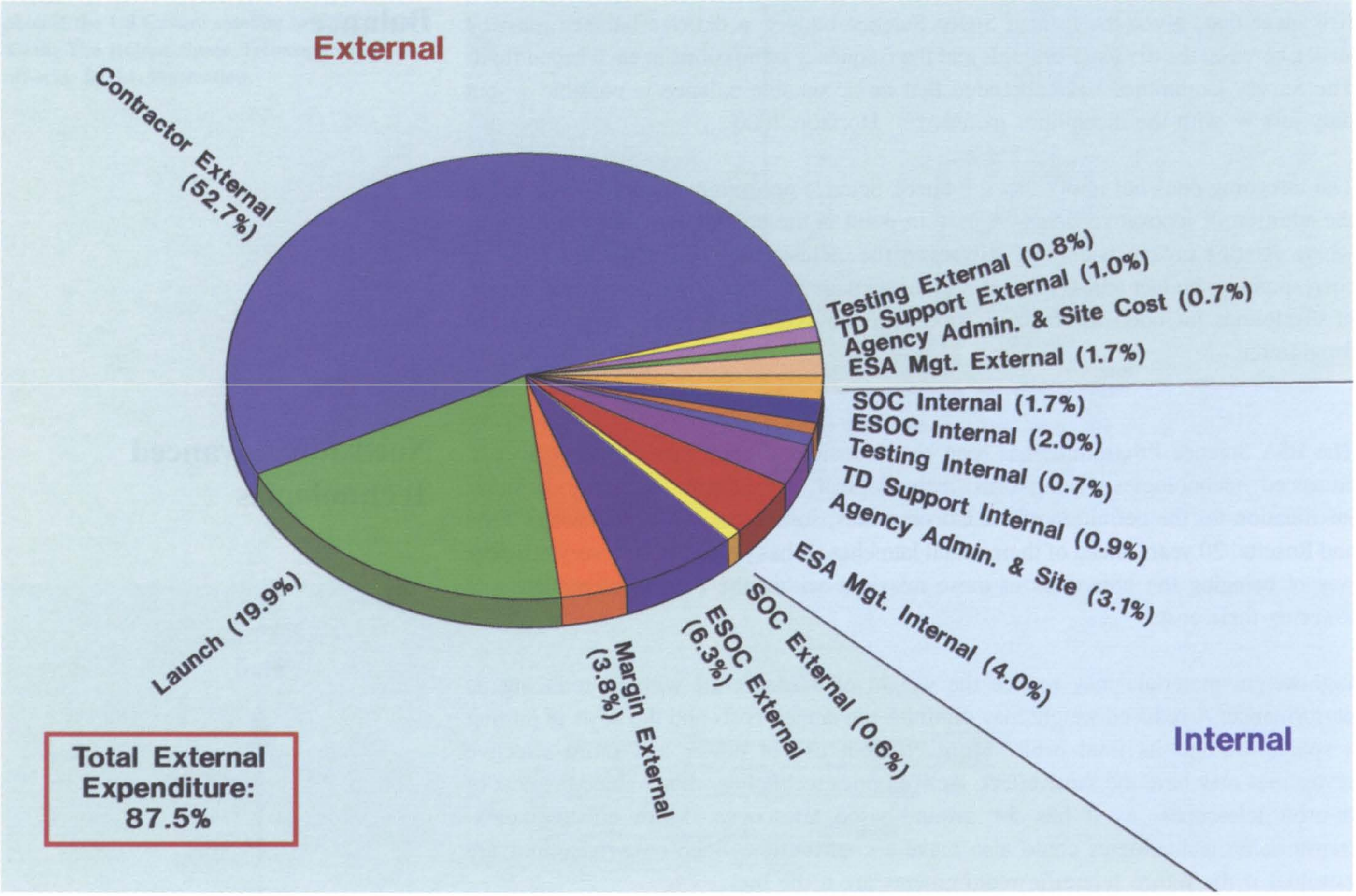
Lightweight materials may reduce the weight of a spacecraft without reducing its performance. A reduced weight may diminish the launch costs and the costs of putting a spacecraft into its final orbit. More efficient use of power and more effective cryogenics may have the same effect. Active optics technology may reduce the cost of in-orbit telescopes, as it has for ground-based telescopes. More effective data-transmission technologies could also make a contribution. Such cost reductions are essential if the future scientific requirements are to be met.

In the USA, new technologies are being developed with the powerful support of the Department of Defense. Such developments based on billions of dollars of investment have been at the origin of 'Clementine', a small lunar orbiter whose success is a testament to the power of new technologies. Since Europe cannot benefit from a similar approach and similar investments, it and ESA have no choice but to devote part of their funds to advanced technological development. Without such an effort, future European spacecraft might become 'technological dinosaurs', lacking both cost and technical efficiency.

It is therefore essential that in the Horizon 2000 Plus programme, in addition to the normal source of funds provided by the Agency's Technology Research Programme (TRP) and General Support Technology Programme (GSTP), a significant percentage of the funds be devoted to technologies in those areas most likely to benefit the future missions of the programme.

Cost and Management Efficiency

On several occasions in the past, ESA's Science Programme has been reviewed by groups of independent experts with the aim of increasing its efficiency and getting more science from its budget. Document ESA/SPC(94)44 discusses these matters and shows that over the years the efficiency has undoubtedly increased. Although the frequency of ESA's science missions has remained more or less constant at some six



Typical spacecraft costs (excluding the payload) and the distribution between external (87.5%) and internal (12.5%) expenditures

missions per decade, the scope and size of these missions have increased dramatically, sometimes by more than one order of magnitude over a 15 year period. In addition, ESA serves a larger number of Principal Investigators and Co-Investigators and an increasing number of users of its missions whose data are now systematically archived and whose operations are systematically extended, as in the case of IUE, HST, Giotto (GEM) and Ulysses, providing more science than ever before.

This increased efficiency stems from improved management methods both in ESA and in industry, despite the constraints of a continuously more restrictive policy of *juste retour*, and a closer dialogue between them, as illustrated by the teamwork of the Industrial Workshops that have been organised by the Agency following the report in 1989 of the Science Programme Review Team, chaired by Prof. K. Pinkau.

While there are, therefore, definite possibilities for increasing the performance-to-cost ratio which should be actively pursued at ESA and in European Industry, one should also avoid excessive optimism. In particular, cost reductions claimed to have been achieved elsewhere should be regarded with caution. It is true that the costs of other space agencies' missions have sometimes been reduced by a substantial factor, but this has often been achieved by a severe descoping. Also, in making comparisons with the costs of missions at other agencies, the different accounting procedures need to be fully taken into account.

Other savings could be achieved by changes in the overall management of the Science Programme. Within the Horizon 2000 programme, the emphasis has been on the optimisation of each individual mission, though there has of course been some technology transfer between missions. It would seem that the creation of a more general framework into which individual missions would have to fit could reduce costs substantially. The requirements for many of the spacecraft are not all that different: power, communications, perhaps three-axis stabilisation, to name just a few. It might, therefore, be attractive to standardise the basic characteristics of a set of spacecraft and to impose these as boundary conditions for the payloads of individual missions.

It is well known that if one tenders for a number of identical units, the unit cost falls rapidly. This is particularly true if the units are tendered for at the same time, but even if not considerable savings remain if each unit is built by the same combination of industrial firms according to the same design. While the favourable cost aspects of such an approach are generally agreed, two objections are raised.

Firstly, the scientists developing a mission wish to optimise their payload and spacecraft with a minimum of constraints, the more so because science mission opportunities are not very frequent. However, if more missions can be fitted within a given budget, the advantages of accepting the constraints may well outweigh the disadvantages and lead to an increased scientific return. This, of course, does not apply to every mission; there are certainly exceptions.

It is perhaps useful to note that there is nothing particularly novel in these considerations. The same proposal was already made in the Atkinson Report (ESA/SPC(77)17, Recommendation XVII); the same philosophy was expressed in a different form in the 'Promise' proposal, submitted as one of the 'mission ideas' for Horizon 2000 Plus. Within the current constraints, ESA has already attempted to implement some of this philosophy, in the case of Integral and in the Phase-A study for Stars (under consideration for M3). However, much more could probably be done, and the Survey Committee recommends that the matter be further analysed by ESA.

The second problem is one of ESA procedures, particularly in the area of industrial return. For such an approach to be possible, there is certainly no need to abandon the principle of *juste retour*. However, it would probably be necessary to average the return in a more flexible manner. In particular, lowering the minimum percentage ratio of *juste retour* may have positive effects in increasing the competition and lowering the cost of subsystems.

Recent experience with Rosetta, First and proposed medium-size missions has shown that there are severe difficulties in maintaining the cost envelopes for Cornerstones and blue missions that were set in 1984. However, increasing these envelopes would unavoidably lead to increased intervals between missions. Because of the possibilities for future cost reductions outlined in this section, it would seem desirable that the general cost envelopes should not be increased. On the other hand, it would seem to be imprudent to assume that very large cost reductions will be achieved. *It is therefore recommended that in Horizon 2000 Plus the cost envelopes of 625 MAU for Cornerstones and 345 MAU for medium missions (at 1993 economic conditions) be maintained unchanged*. It should be stressed that these are maximum values and that mission proposals at lower cost should be regarded with particular favour. Should further cost reductions turn out to be possible, they should mainly be used to increase the frequency of missions rather than to simply make them bigger.

It has often been said that collaborative missions with other space agencies are difficult to organise because of the different time scales in planning. The Survey Committee recommends that when particularly attractive opportunities arise for joint missions, deviations from a standard schedule for blue missions be accepted and the necessary re-arrangements in the overall schedule be made if this is possible without incurring extra cost.

Quickness and Cheapness

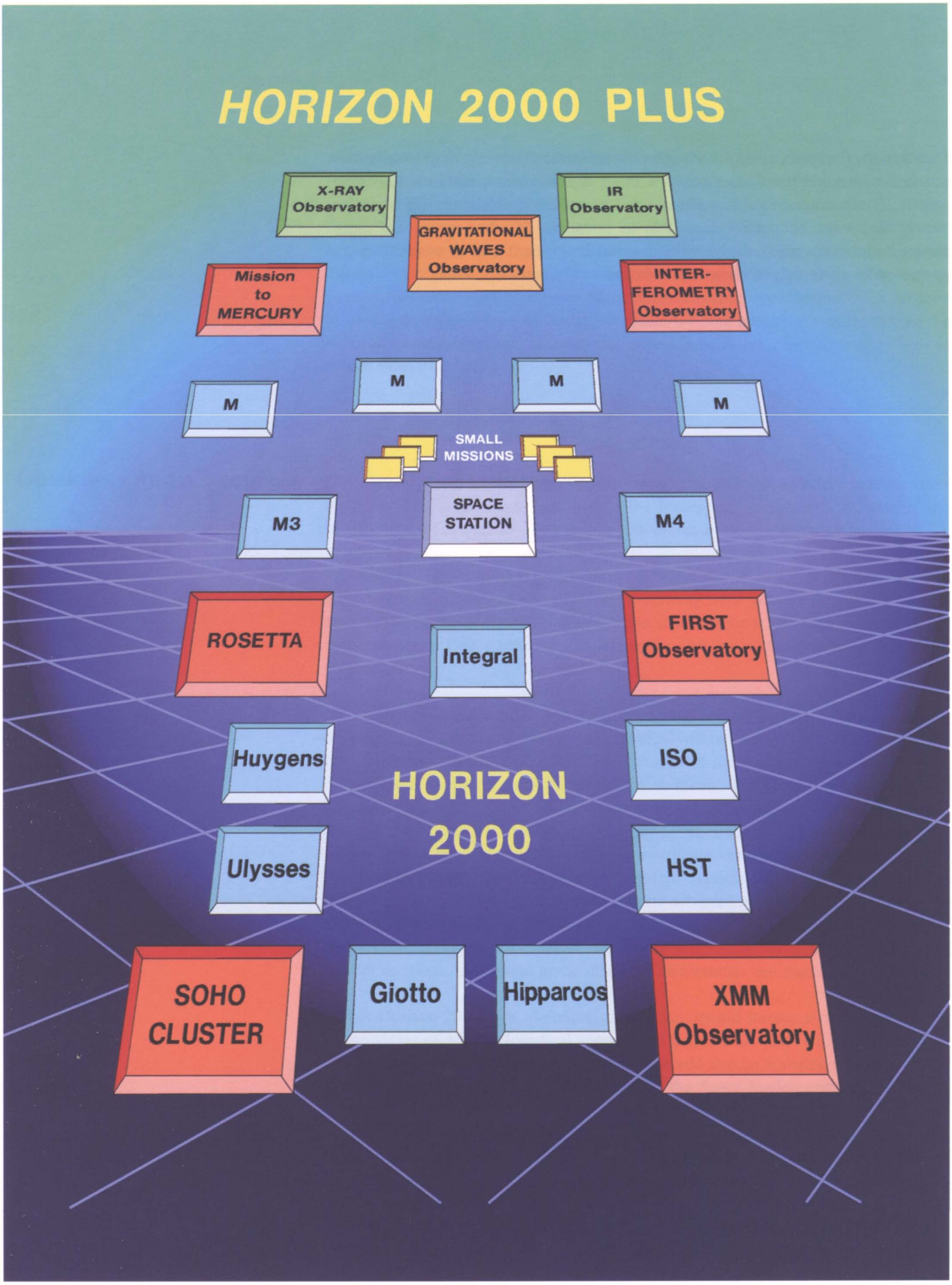
It is certainly true that making missions faster (and therefore smaller) should reduce their cost, if for no other reason than the leaner size and shorter lifetime of the industrial and ESA teams. This would also increase the frequency of missions.

The original Horizon 2000 programme contained provision for small missions, but there has been a considerable problem in defining what constitutes a small mission and, furthermore, in establishing that those small missions that have been proposed meet the underlying requirement of scientific excellence that underpins the whole programme.

Various options have been proposed for a small mission programme within the context of Horizon 2000, but none can be really satisfied without resolving the definition issue. In formulating Horizon 2000 Plus, the question of the possible usefulness of small missions was explicitly put to the five Topical Teams. The general response was not very encouraging: for most astronomical missions, a three-axis-stabilised spacecraft capable of pointing with arcsecond accuracy is required, planetary missions tend to require much manoeuvring to arrive at a suitable orbit around their target, and fundamental-physics missions require drag-free systems. With the present state of technology, within ESA, none of these can be achieved at a cost substantially below that of a medium mission. This does not mean that the Agency should abandon all its responsibilities in the fostering of a possible programme of quicker, cheaper and smaller missions, in addition to the menu of Cornerstones and medium-size missions.

Within Horizon 2000 Plus, therefore, the Agency should foresee a moderate sum for the provision of advice, logistical support and use of ESA test facilities for national and multilateral small missions. This support should also be available for the purposes of demonstration and verification missions in the Space Station context. One could foresee a Call for Proposals and the selection of one small mission on a competitive basis approximately every two years, involving more substantial ESA funds. Managerial responsibility would be devolved to a national agency or agencies once the selection had been made. During the hardware phase, ESA might be involved only as an advisor or in providing test facilities. Such a small-missions programme could potentially also be implemented via an optional programme similar to PRODEX.

In conclusion, European scientists who propose scientifically excellent small missions need to be sure that the means exist for their implementation within a European context. If infrastructure items such as the Space Station are to be exploited for science, they must be readily accessible to scientists. If other means are more appropriate, the Agency must be willing to facilitate them and to ensure that its approach does not preclude cheap and efficient small-mission science.


HORIZON 2000 PLUS
X-RAY Observatory
IR Observatory
GRAVITATIONAL WAVES Observatory
Mission to MERCURY
INTER-FEROMETRY Observatory
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M
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SMALL MISSIONS
SPACE STATION
M3
M4
ROSETTA
Integral
FIRST Observatory
Huygens
ISO
HORIZON 2000
Ulysses
HST
SOHO CLUSTER
Giotto
Hipparcos
XMM Observatory

Chapter III

Horizon 2000 Plus in Outline

Introduction

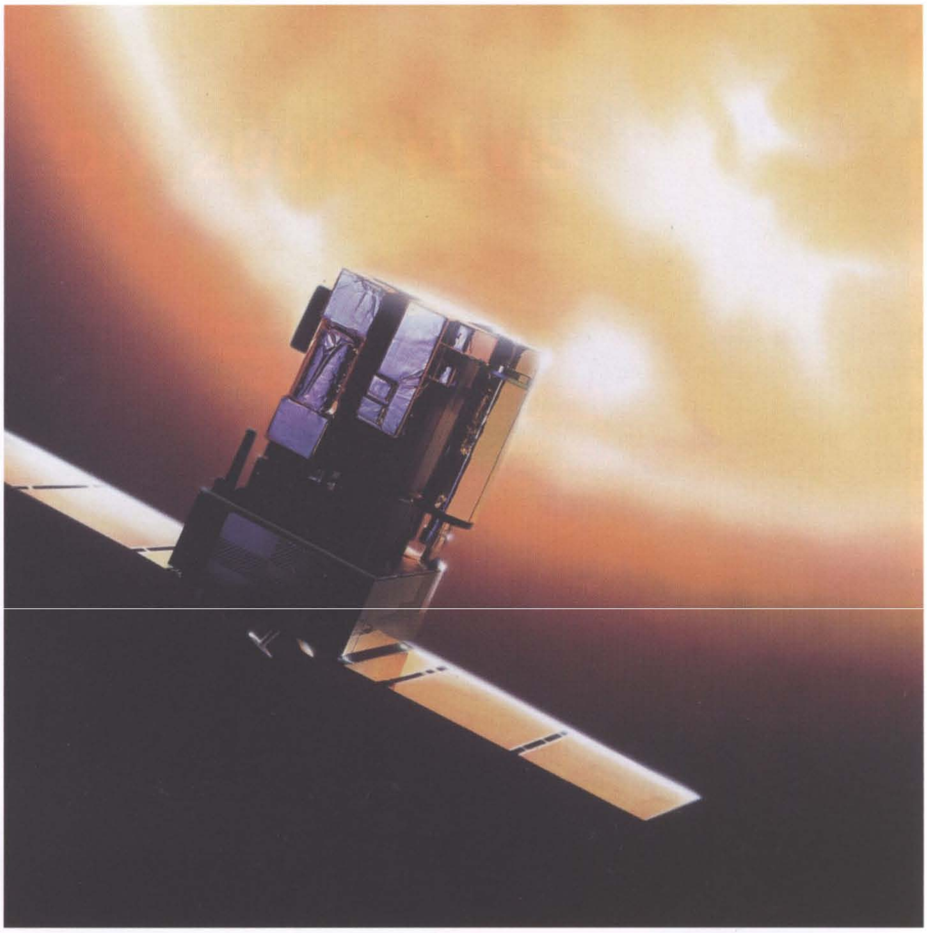
Horizon 2000 (described in detail in ESA Special Publication SP-1070, December 1984) was built around four 'Cornerstones', namely large missions in well-defined strategic areas of Space Science: Solar Terrestrial Programme; Mission to Primordial Bodies in the Solar System; X-ray Spectroscopy and Sub-millimetre Observatory. These programmes needed long technological preparation/studies to bring them within the realms of feasibility and financial acceptability. While their precise implementation may have undergone modifications and sometimes down-sizing as the technological studies progressed, the basic aims of each mission have, in most cases, remained unchanged. As a consequence, the Cornerstones have given much-needed stability to the overall programme and have allowed the respective communities to structure and prepare themselves for optimal contributions to the instrumentation and optimal use of the facilities after launch.

Scientific aims evolve, and the activities of other agencies may also lead to changed priorities. The medium missions (so-called 'blue missions') in the Horizon 2000 programme were foreseen to give the programme flexibility. These missions were not specified at the time, but left to later selection. In fact, Huygens (Titan Probe) and Integral were selected in 1988 and 1993, respectively, as M1 and M2. Phase-A studies are currently taking place for the selection of M3, which is scheduled for 1996, while the selection of M4 is foreseen around 1999. The blue missions are not necessarily medium-sized: the cost to ESA is 'medium' in each case but, due to the contributions from other partners, the missions themselves may be 'large'. Hence, Ulysses, Huygens (ESA's contribution to the NASA Cassini mission to Saturn), and Integral are blue missions, but overall these are very large projects.

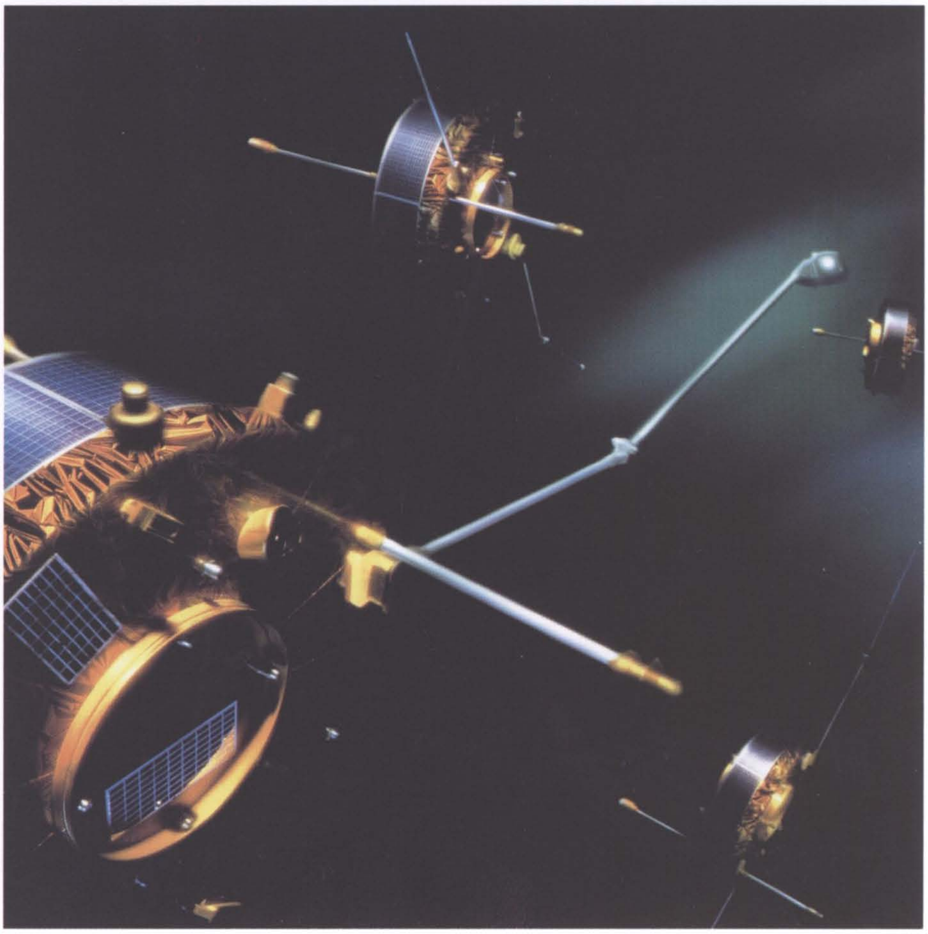
Since the extension of Horizon 2000 covers a limited period, it is clear that only a limited number of Cornerstone missions can be envisaged. Priorities will have to be set on the basis of scientific considerations and also as a function of programmatic aspects. It would hardly be feasible to select new Cornerstones in fields where the operations of the Horizon 2000 Cornerstones extend well into the additional period covered in Horizon 2000 Plus. The most relevant scientific questions can only be formulated fully when these missions are approaching completion. What should be done in these fields, however, is to conduct the necessary technology development and system studies so that suitable missions become possible in the post-Horizon 2000 Plus time frame.

In formulating Horizon 2000 Plus, the Survey Committee has therefore considered the parts of Horizon 2000 beyond 1995 to be part of Horizon 2000 Plus. The latter covers the period 1995–2016, taking it a decade beyond the present nominal end of Horizon 2000. Depending on the detailed funding situation and on the evolution of cost factors, it would then be possible to include some two or three new Cornerstones and four medium-sized missions. The financial aspects are discussed later.

The Cornerstones are missions with very long lead times. International cooperation over such long time scales entails much uncertainty, as several recent events have shown. It therefore seemed desirable to the Survey Committee that the Cornerstones



The Solar and Heliospheric Observatory (Soho) will provide the most comprehensive investigation of the Sun, our closest star, ever attempted from space



The fleet of four identical Cluster spacecraft will study the structure of plasma in near-Earth space in three dimensions

be, in principle, self-standing ESA missions. This does not mean, of course, that international cooperation would not be desirable and would not give added value. However, it was felt that to give the Horizon 2000 Plus planning sufficient stability, it is essential that the Cornerstones not be contingent on decisions taken outside ESA by other organisations.

Solar-System Science

European space research relating to the Solar System has proceeded in two main areas:

(i) The plasma in the Sun and in the Solar System

This area has been the subject of a number of missions. Currently, Ulysses is observing the solar wind above the poles of the Sun, while Cluster and Soho will soon begin to measure the Earth's magnetosphere and the solar wind. The Sun itself will also be observed by Soho. Spectroscopy in the ultraviolet and extreme-ultraviolet parts of the spectrum, as well as solar seismology, are also included.

While the results of the current and planned missions cannot be foreseen at the present moment, it is clear that future missions will be required in these broad areas if further progress is to be made in our understanding.

(ii) The solid bodies of the Solar System

The Giotto mission made it possible to observe Comet Halley at close range. The Rosetta mission, including a surface lander, should set out in 2003 for a rendezvous with another comet and will probably also fly-by one or two asteroids.

The Titan atmospheric probe Huygens forms part of the joint NASA/ESA Cassini/Huygens mission to Saturn and Titan.

The Sun and Heliosphere

Studies of the solar wind near Earth will shortly be undertaken by Soho, whilst Ulysses is currently exploring the solar wind high above the Sun's poles. Both of these missions will study the solar wind far from its origin. Even in Horizon 2000, a Solar Coronal Probe was mentioned as a 'green dream'; this Probe would study the corona at perhaps 4 solar radii and provide detailed information about conditions there. The mechanisms of coronal heating would be elucidated and the dynamics of the beginnings of the solar wind explored.

The Solar Coronal Probe would have to follow a trajectory with a Jupiter gravity assist, if it were relying on chemical propulsion. Effective functioning over such a range of distances would presumably exclude the use of solar panels for energy supply. Even a trajectory with only one pass through the corona would be a formidable undertaking. It is doubtful, however, if a single quick pass through the corona would give the solid information needed to obtain a better physical understanding of the region. An orbit with multiple passes would further add to the difficulty. However, interest in the Probe is so great that the Survey Committee recommends that ESA consider participating with other space agencies at the level of a medium mission if an attractive opportunity arises, also making use of new technologies such as electric propulsion.

The Ulysses spacecraft, currently surveying the unexplored regions of space above the poles of the Sun

The corona is the region where much of the solar activity that later influences conditions in the Earth's upper atmosphere takes place. All images of the Sun obtained from Earth represent an integration along the lines of sight through the corona, which makes it impossible to ascertain the three-dimensional structure of the observed features. Long-lived structures may be disentangled to some extent by making use of the solar rotation, but few are stable enough for this to be done reliably; for the most interesting, rapid events like coronal mass ejections, this is totally impossible. An attractive possibility would be to observe the Sun stereoscopically, simultaneously from several directions. The most appropriate solution would be to place two or more spacecraft at, for example, L1 (near Earth) and at L4 or L5 ($\pm 60°$ from Earth). Simulations show that four to six strategically located spacecraft would be needed to obtain a fully satisfactory three-dimensional image reconstruction. By its nature, such a project could also lend itself very well to a cooperative venture.

These two examples illustrate the many possibilities for cooperative missions devoted to solar research. An important solar research community exists in Europe. On the ground, new facilities are being constructed for solar observations in the optical part of the spectrum. While it may be premature to select a specific mission now, the Survey Committee is convinced that within the time frame of Horizon 2000 Plus at least one mission should be devoted to solar research.

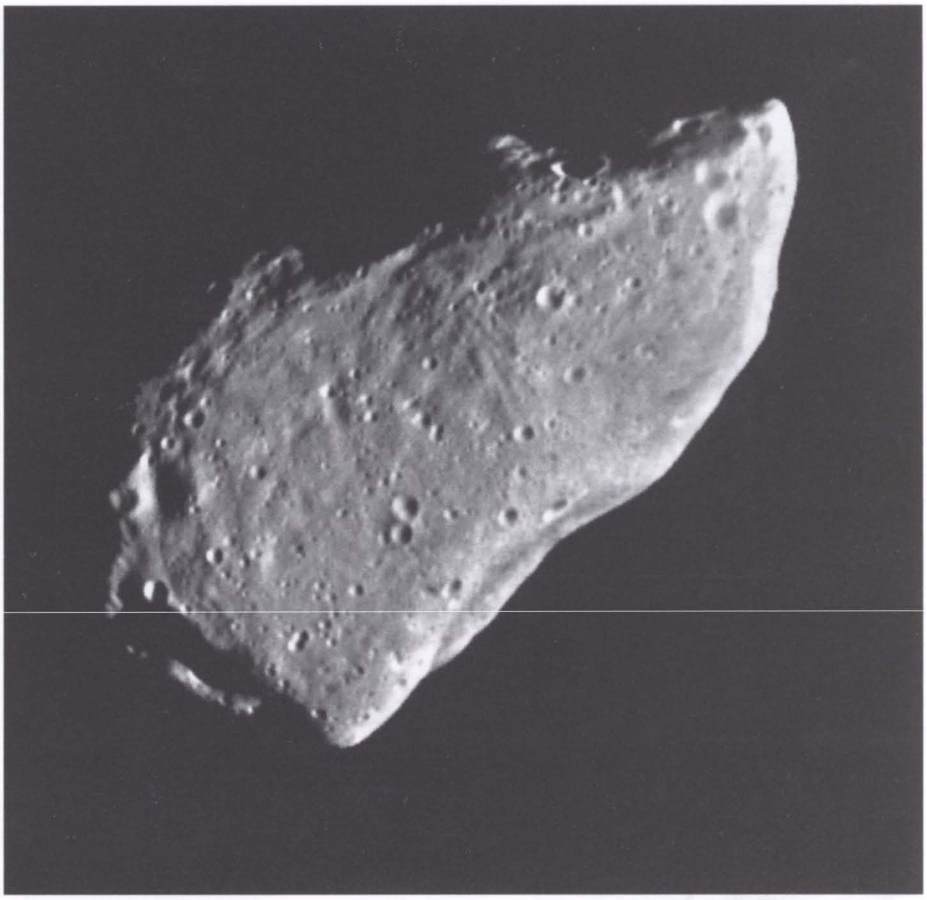
The terrestrial magnetosphere will remain a subject of much interest after Cluster. Proposed missions include an auroral polar orbiter and multi-spacecraft studies of plasmas in the magnetosphere. The requisite missions should be considered in the framework of the ESA medium or small missions; alternatively, some could be executed as national projects.

The magnetosphere is a region where very detailed studies of plasmas may be conducted. Rapid changes occur caused by variations in the solar wind, by the rotation of the Earth, and by various instabilities. Moreover, important couplings exist between the magnetosphere, the ionosphere and the mesosphere. Cluster, with its four spacecraft, will provide multipoint observations of electric and magnetic fields in the magnetosphere. More global imaging of the magnetosphere will be needed to view changes over the whole auroral zone simultaneously. Such imaging would contribute to a fuller determination of the topology of the magnetic fields, and of the energy transport in the auroral region. Several European countries have created joint ground-based facilities, like EISCAT, to survey the polar ionosphere, and the combination with space-based instruments appears to be particularly useful. However, before the results of Cluster and Soho are in, it is too early to specify priorities for possible missions.

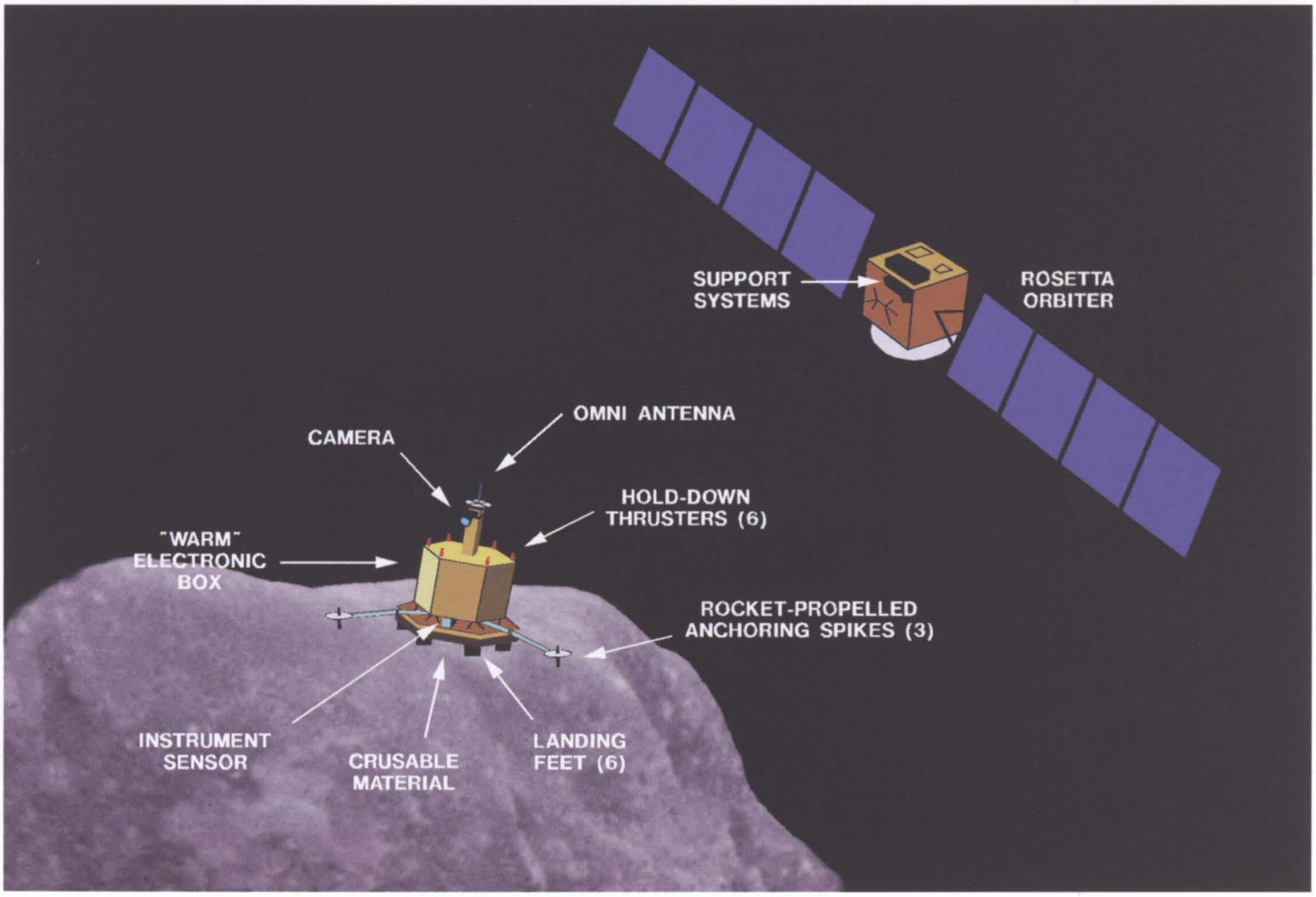
The Planetary System

The Horizon 2000 programme put much emphasis on the study of comets, which represent the most primitive material of the Solar System. A sample-return mission as originally planned for Rosetta would still be of great interest. However, the same financial and technological circumstances that made such an objective overly ambitious for Rosetta would make it difficult to envisage this kind of mission now. The early emphasis on comets was also due to the fact that a special niche was

Asteroid Gaspra, as seen by the Galileo spacecraft in October 1991. It is about 18 km long from lower left to upper right. (Image courtesy of NASA/JPL)



The Rosetta mission



available for Europe to make a meaningful contribution to the exploration of the solid bodies in the Solar System: other space agencies already had an active and extensive programme of planetary exploration, and it seemed difficult for a newcomer to make a substantial impact.

Times have changed: planetary exploration by the other space agencies has proceeded much more slowly than envisaged, and at the same time Europe has gained considerable experience in Earth observation and also in conducting missions at great distances from Earth – including the fly-bys of Comets Halley and Grigg-Skjellerup (Giotto and GEM). As a result, there is now every reason for ESA to engage in an active programme of planetary exploration. The terrestrial planets offer a particularly appealing target for a number of reasons.

There is a large community of geologists, geochemists, geophysicists and atmospheric physicists in Europe who would very much like to study these planets, which offer many opportunities for comparison with Earth. In addition, ESA-only missions to the outer planets would involve specific difficulties, including the need to develop a deep-space communications network and non-solar energy sources. Since other agencies have developed these, it would seem best for the moment to follow the Huygens model and to conduct such projects as medium missions in a cooperative framework, selected via the usual ESA procedures.

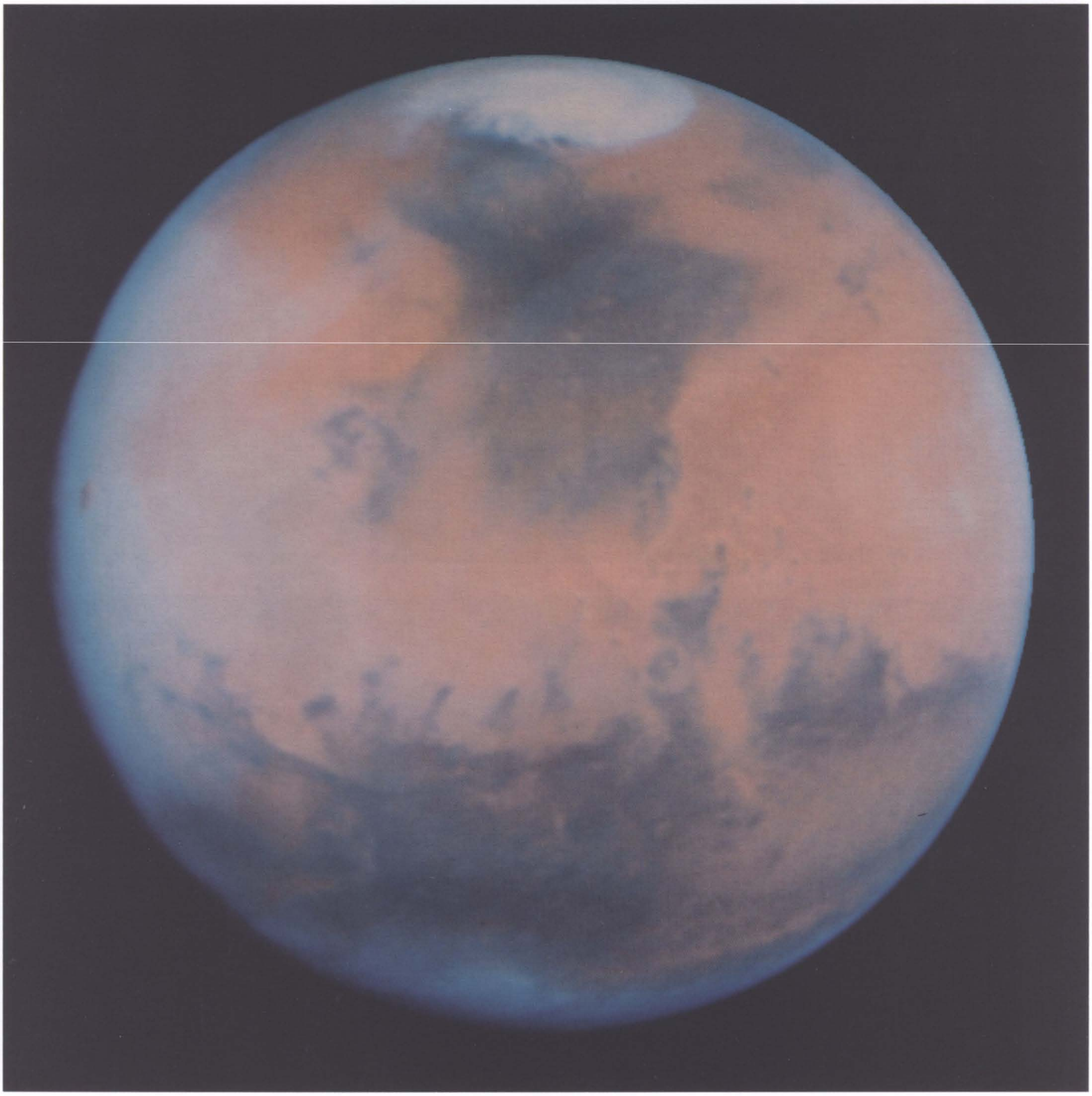
Of the inner planets – Mercury, Venus, the Earth and Mars – Venus has just been extensively studied by NASA's Magellan mission, although much remains to be done, especially in terms of detailed study of the Venusian atmosphere and interior. The high temperature and pressure in a corrosive, not very transparent atmosphere make this a substantial undertaking. Perhaps more promising targets would be Mercury and Mars: the former because so little is known about it, and the latter because of its broad scientific appeal to a very wide community in Europe.

Mars

There is a worldwide consensus that gives Mars the highest scientific priority among the inner planets, due to its outstanding interest for comparative planetology. The planet is smaller than the Earth, but nevertheless shows enough similarity for many geological and atmospheric processes to provide interesting comparisons and tests for theories regarding terrestrial processes.

In the broad context of planetary science, Mars represents an important transition between the outer volatile-rich, more-oxidised regions of the accretion zone of the terrestrial bodies (asteroid belt) and the inner, more refractory and less-oxidised regions from which Earth, Venus and Mercury accreted. This special position of Mars and its transitional character is also manifested by its size, the degree of internal activity, the age of its surface features, and the density of its atmosphere – properties that are intermediate between those of the large terrestrial planets (Earth, Venus) and the smaller planetary bodies (Mercury, Moon, asteroids).

Although geologically less evolved, Mars is more Earth-like than the other terrestrial planets. Its internal evolution, and the exogenic processing of its surface, have extended over several billion years. Aside from the Earth, Mars is the only other planet in the



Mars, as seen by the Hubble Space Telescope (WFPC2) in February 1995 (courtesy of NASA/Univ. of Toledo)

Solar System that has a transparent atmosphere, with surface-temperature conditions in the range of the stability of complex organic compounds. It is therefore an obvious target in the search for present or extinct life forms.

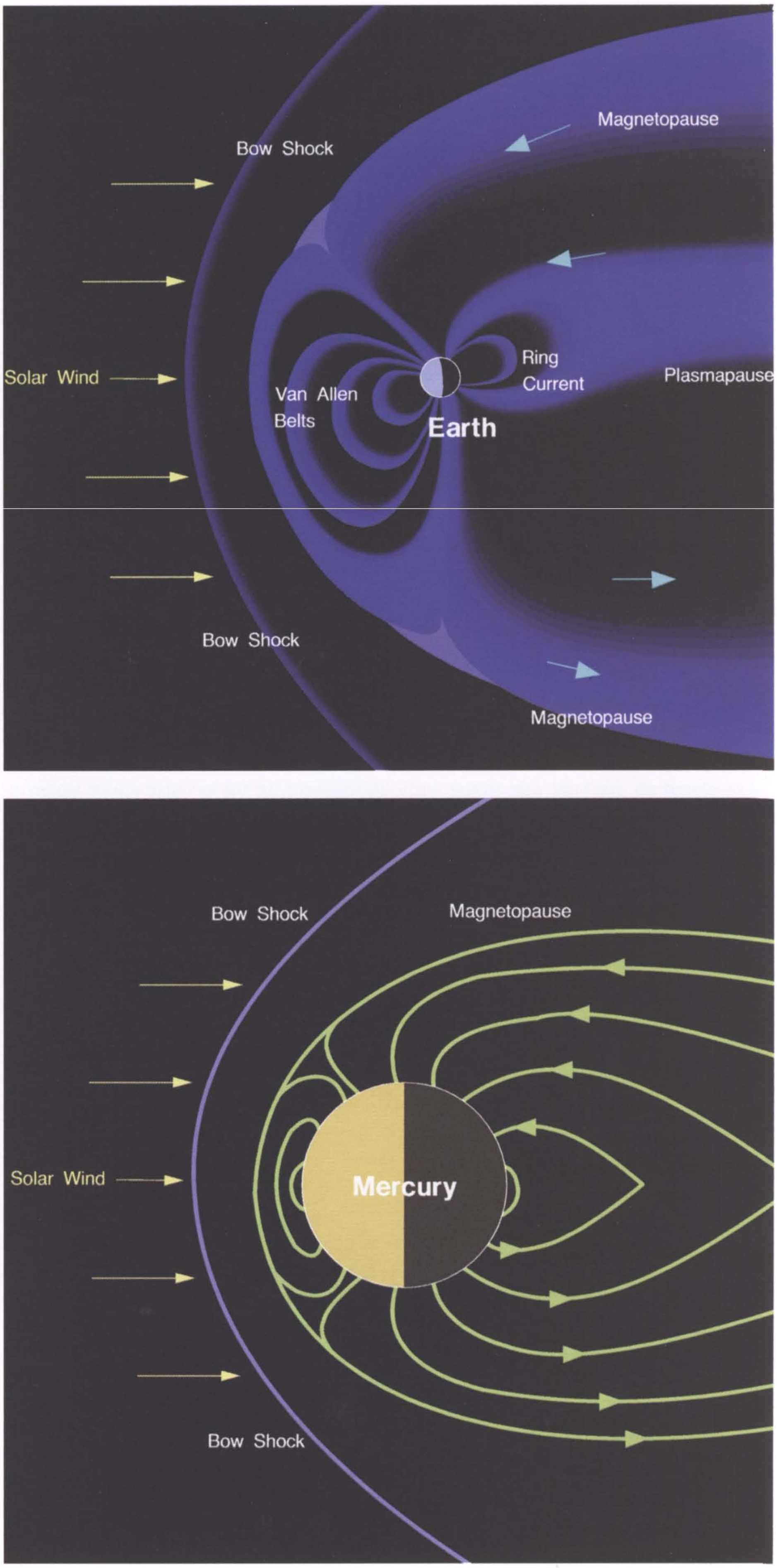
Mars displays a wide variety of surface features formed by exogenic processes whose controlling factors – composition and density of the atmosphere and its seasonal and diurnal variation, surface temperature, type and abundance of surface and subsurface volatiles, oxygen fugacity, rate of impact cratering – are distinctly different from the corresponding factors on Earth. Also, the morphology and scale of tectonic and volcanic landforms resulting from endogenic processes (controlled by the composition, structure and activity of the interior of the planet) are particularly different from the equivalent features on Earth. All of this provides an as yet unexplored set of boundary conditions for most of the fundamental geological processes that we also encounter on our own planet. Consequently, Mars exploration is crucial for a better understanding of the Earth from the perspective of comparative planetology.

Mars is believed to have a core, but different models predict very different radii for and conditions in that core. Also, the structure of the mantle of Mars is unknown. Appropriately instrumented surface stations with seismographic capability could provide answers. The chemical composition of the Martian surface is largely unknown, as too are the possible subsurface stores of water and carbon dioxide, which would have played an essential role in the early atmosphere, in the occurrence of water on its surface, and in the possible evolution of life on the planet. Even though such life would now be extinct, its early presence or absence would be a major discovery in terms of its implications.

There is abundant evidence in the record on the surface of present-day Mars to show that conditions on the planet used to be very different. In particular, the presence of free surface water, in amounts large enough to form rivers and seas, implies that the atmosphere must have been much thicker about three billion years ago. Modelling studies suggest that a surface pressure of at least ten times the present mean value, and perhaps much more, was probably the norm on early Mars. At a time when there is great concern over the stability of the Earth's climate, there is particular interest in understanding the much grosser changes that have taken place on Mars.

The Martian atmosphere is of unusual interest for comparative climatology and meteorology. Daily, seasonal and latitudinal variations in the insolation profoundly affect the Martian atmosphere, in which violent dust storms may occur. Detailed observations of the atmospheric chemistry, temperatures, pressures and motions will provide interesting tests for atmospheric models and thereby also increase our understanding of the physics of our own Earth's atmosphere. Long-term monitoring of the Martian atmosphere by surface stations placed on the planet would appear to be the most effective approach for such studies.

No good measurements of a possible Martian dipolar magnetic field have been made. Consequently, it is not known whether dynamo processes take place in the core as on Earth. Moreover, very little is known about the planet's interaction with the solar wind; the possible presence of a small magnetosphere cannot be ruled out. Magnetometers on some surface stations and on the orbiter could tell us much about the nature of the Martian magnetic fields.



Comparison of the magnetospheres of Earth and Mercury (not to scale). Mercury's magnetic field is dipolar like the terrestrial one, but is much weaker (350 gammas). As on Earth, the hermean magnetic axis has an inclination of 11° with respect to spin axis and polarity is such that a compass needle would point north. (from The Elusive Planet, by R.G. Strom, Cambridge University Press)

Despite the general agreement about its very high scientific priority, the future of Mars exploration is now quite uncertain. The loss of NASA's Mars Observer in 1993 and the programmatic uncertainties both at NASA and in the Russian programme, cast much doubt on the realisation of the plans made some years ago. New scenarios for Mars exploration have been proposed by the International Mars Exploration Working Group, which includes the world's main space agencies.

In such a fluid situation, it is impossible to specify a well-defined ESA Mars mission; in any case, international cooperation will be required. All that can be done at this time is to specify a priority for a meaningful substantial participation when the opportunity arises. This priority should be such that other items in the ESA planetary-science programme may have to be rearranged.

Nevertheless, Europe clearly cannot allow itself to be absent when serious exploration of Mars starts up again. Not only will the science be of the greatest interest, but there is a large community in Europe not only of planetary scientists, but also meteorologists, climatologists, geologists and others, who would have a vital interest in the results.

Mercury

While Mars is perhaps the most interesting planet for comparison with the Earth, Mercury is by far the least known of the four inner planets and also the most different from Earth. The mission to Mercury can be considered as a counterpart to Rosetta. While comets contain the Solar System's most primitive matter, Mercury was built up from highly fractionated matter. The planet's most noteworthy characteristic is its high density, generally explained as due to a core extending to three-quarters of the planet's radius. If the core were to consist mainly of metallic iron, a five-fold increase in the Fe/Si ratio of Mercury compared to Sun would be required.

Only 40% of Mercury's surface was observed by Mariner 10 in 1974, with a modest spatial resolution. It has a lunar-like surface, shaped partly by ancient lava flows and extensively reworked by meteoritic impacts. A Mercury orbiter could furnish much information about the core of the planet. It could also study the magnetic field in detail and map the planet's complete surface with good resolution, thereby providing information on its tectonic and volcanic history. With appropriate multispectral imagers and X- and gamma-ray spectrometers, much could be learned about the composition of the surface.

Up to now, essentially nothing is known about the composition of Mercury's surface, not to mention that of its interior. Scientists want to know if there is indeed, as several models predict, a compositional gradient from Mercury to Mars, in terms of the depletion of chemical elements according to their volatility as well as their state of oxidation. Measuring the potassium-to-uranium ratio and the abundance of FeO in the Martian crust by gamma-ray spectroscopy will provide important information on these issues. Closely related to the degree of oxidation of the planet and the abundance of SiO_2 in the crust is the question of whether the core's sulphur or silicon content is high enough to substantially lower its melting point. Is a liquid core really required to explain Mercury's dipolar magnetic field?

While the planet does not have much of an atmosphere, some gas is present above its surface, such as the sodium ions already observed from Earth; the origin and characteristics of this exosphere would be of interest. The recent radar observations suggesting the presence of water-ice in the craters at the Mercurian poles is another topic that can be studied by the Mercury orbiter's gamma-ray instruments.

Of particular interest is Mercury's magnetosphere. Because Mercury does not have a conducting surface or an ionosphere, the currents that flow in the magnetosphere cannot be closed in the non-existent atmosphere. Consequently, it should be much more unstable than the Earth's magnetosphere. Comparison of the two should teach us much about geophysical plasma processes.

The Moon

The Moon shares many of the characteristics of the terrestrial planets. It provides much information about the early history of the Solar System, and its close connection to the Earth gives it particular interest. The focus on manned lunar missions has resulted in very incomplete coverage and major questions are as yet unanswered.

A lunar orbiter such as Moro, currently under Phase-A study by ESA, could make an important contribution. There is, of course, an obvious relationship with the Moon programmes currently under study by ESA (ESA Brochure BR-101, May 1994) and other agencies, which identify a phased approach with small or medium-size missions capable of providing major progress in lunar science.

Other planets

Many other planetary exploration missions have been proposed. In the near parts of the Solar System, the atmosphere of Venus remains largely unexplored. Further out, a mission to Jupiter, focusing on its internal structure, the Galilean satellites, particularly Io and Europa and the Io plasma torus, and a mission to Neptune to study its rings, Triton and the icy satellites, would be particularly interesting. In between, a reconnaissance-type mission to a number of asteroids could also be considered. Such missions could be treated as medium-class missions in a cooperative framework.

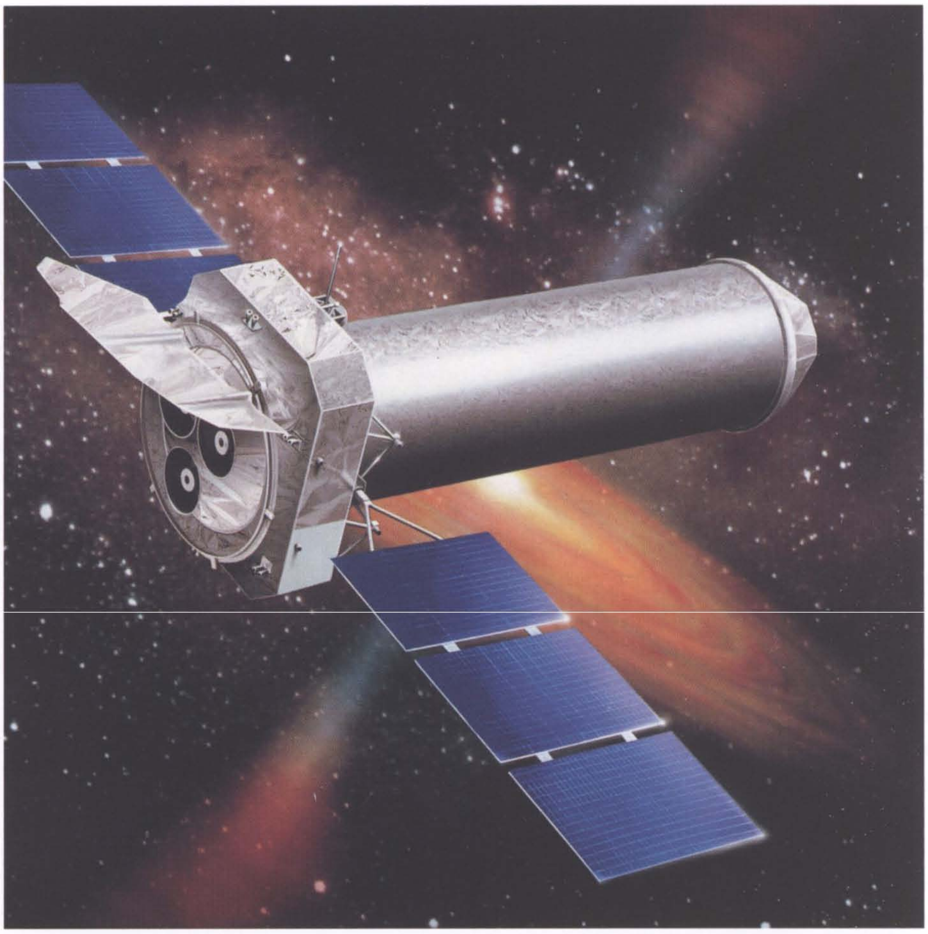
Conclusion

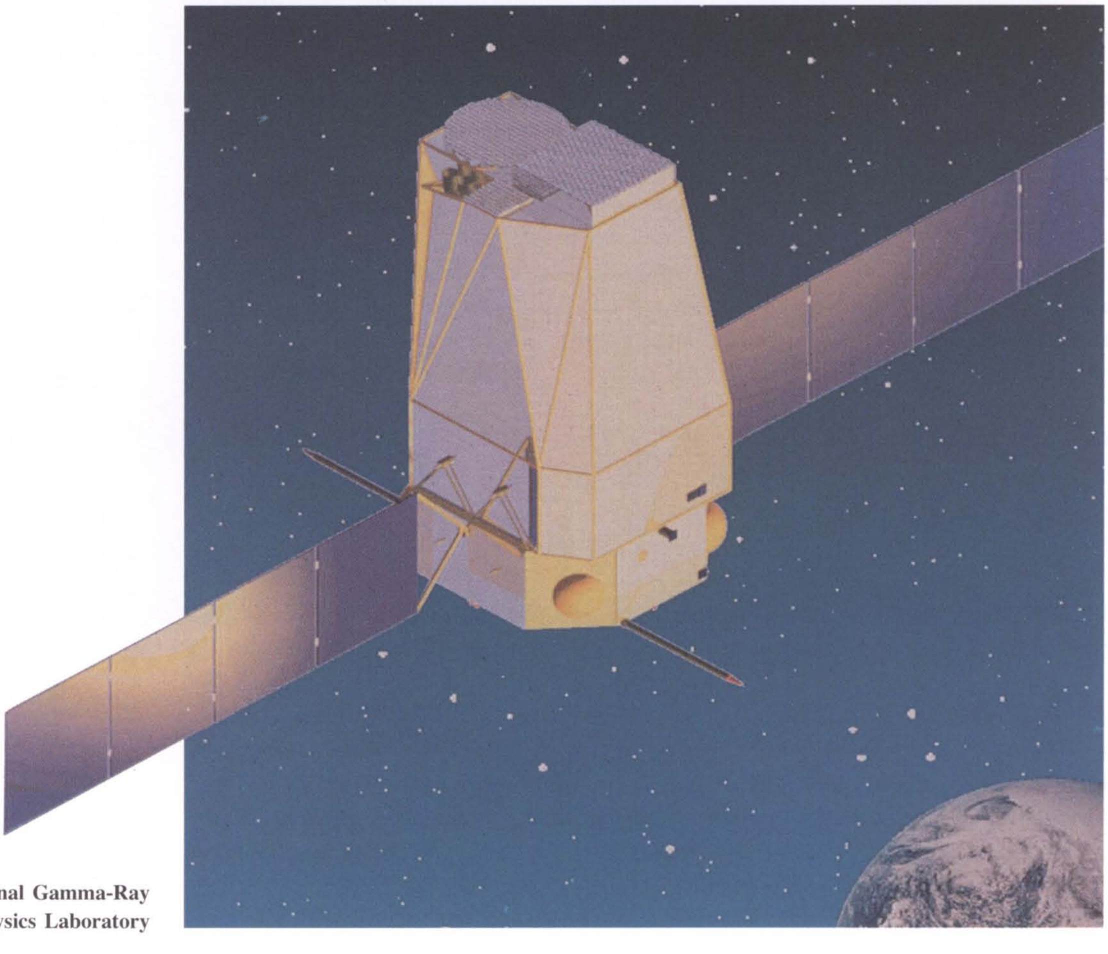
The Survey Committee recommends a Cornerstone-level mission to Mercury, the planet nearest to the Sun, which is still largely unexplored. Both planetary and magnetospheric aspects should be addressed by this mission.

In view of the great international interest in the study of Mars, the Survey Committee recommends that ESA participate at the level of a medium-class mission in opportunities that may arise in the international context of Mars exploration.

In view of the great interest expressed by the solar-physics community, the Survey Committee recommends that ESA should, at an appropriate moment within the time frame of Horizon 2000 Plus, participate in an international solar mission or take advantage of opportunities provided by the Space Station or the small and medium-class missions of Horizon 2000 Plus.

The XMM spacecraft





Integral — The International Gamma-Ray Astrophysics Laboratory

Astronomy

Space astronomy in Europe has developed in four principal directions:

(i) X-and gamma-ray astronomy,
with XMM and Integral under development for launch in 1999 and 2001, respectively. These missions build on the early success of Exosat (1983–1986) and Cos-B (1975–1982). In addition, some national missions (in particular Rosat), as well as experiments placed on Japanese, Russian and US satellites, have contributed much to ensuring an active and healthy programme in this area in Europe.

(ii) Optical/ultraviolet astronomy,
with IUE and the Hubble Space Telescope continuing to produce abundant high-quality data. A substantial community of astronomers in Europe depends on data from these instruments. While this community appears to be well-satisfied for the moment, the situation would change if the Hubble Space Telescope mission were to terminate.

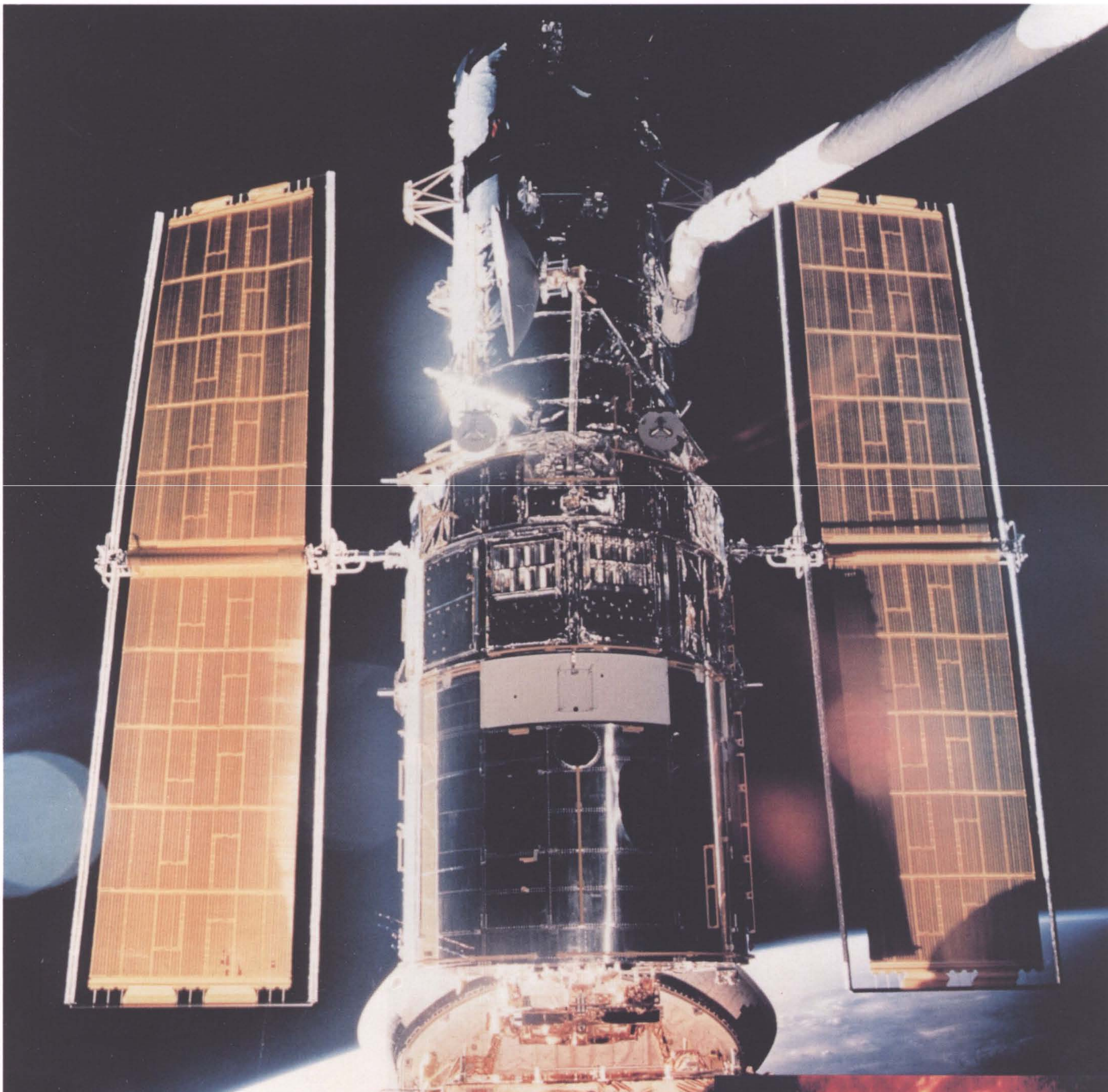
(iii) Infrared/sub-millimetre astronomy,
with ISO to be launched soon and First in 2005. The interest in the infrared part of the spectrum is evidenced by the 1000 requests for observing time on ISO.

(iv) Astrometry,
with Hipparcos having completed its mission in 1993 and the data currently being analysed.

Overall, some 3000 astronomers in Europe are dependent on data from one or more of the missions mentioned above. While the period covered by Horizon 2000 Plus stretches far into the future, and while the final scientific harvest from the missions currently under development cannot yet be totally foreseen, it is clear that additional missions in each of the areas mentioned will be required.

X- and gamma-rays
During the period 2000–2010, XMM will obtain spectroscopic data of unprecedented spatial and spectral resolution for bright and moderately faint sources. However, spatially resolved, high-resolution X-ray spectroscopy of extended sources, like hot gas in supernova remnants, galaxies, groups and clusters will be severely limited due to the dispersive nature of XMM's high-resolution spectrographs. This means that post-XMM, high-resolution spectral studies of both bright, relatively nearby, extended sources and of faint, distant cosmological sources will constitute a main observational priority. Adequate spatial resolution is required to disentangle the spectral resolution of the salient emission features in extended sources and to maximise the signals of faint sources against the noise of background and confusing sources. This spatial resolution needs to be carefully traded against collecting power to ensure adequate photon statistics in the resolved spectral features. As a first estimate, an effective collecting area of approx. 1 m^2 coupled with a sub-arcminute resolution of say 20 arcsec half-power beamwidth would seem appropriate.

In the 1–10 keV range, double-reflection telescopes are effective. To achieve the above combination of throughput and angular resolution, two technologies are available:



The Hubble Space Telescope prior to its release from the Space Shuttle 'Endeavour' at the end of the HST servicing mission, in December 1993. Inset: Core region of Galaxy NGC 1068 imaged by ESA's Faint Object Camera, after HST's refurbishment gold- or iridium-plated nickel as used on ESA's XMM, and metal foils as on Japan's ASCA. The former gives stable optical surfaces and thereby adequate angular resolution, the latter much less so but with the advantage of much lower weight. The technology developed for AXAF is not relevant for this purpose. To have a chance of achieving a large X-ray collector at affordable cost, it will be necessary to develop an intermediate technology providing low weight and high-accuracy surfaces (XMM).

The Survey Committee therefore recommends that ESA perform technological studies on lightweight X-ray reflectors with adequate image quality. Also, work on high-spectral-resolution detectors should be vigorously pursued, in particular at ESTEC.

While an X-ray telescope with a 1 m^2 effective area therefore constitutes an important goal, other missions could make very substantial contributions. To explore the higher energy X- and gamma-rays, different techniques are needed. At this moment, it is not entirely obvious how a mission with, say, 10 times the sensitivity of Integral is to be brought about within an acceptable cost envelope. Medium-type missions may also be envisaged, for example to study the X-ray spectra of gamma-ray bursts, to monitor relatively bright variable sources, etc. Such missions may be (and have been) proposed via the usual ESA procedures for medium-size missions. Another area in high-energy astrophysics – e.g. cosmic-ray observations – could also be considered.

The Survey Committee has also considered the possibilities offered by the Space Station for testing, and possibly implementing, a major high-energy astrophysics facility. Now that the layout of the Space Station is becoming clearer, a more detailed study of the options should be undertaken.

Optical/ultraviolet

The Hubble Space Telescope (HST) programme has been foreseen as being of 15 years' duration. New, more powerful focal-plane instruments will be added some time towards the end of this decade. What will happen thereafter will be decided by NASA as a function of the prevailing technical and financial aspects. At present, there does not seem to be any technical reason why HST could not continue to operate for a long time to come, but the necessary refurbishments are costly.

HST fulfils a vital role in contemporary astronomical research. For European astronomy to remain competitive, it is essential that ESA see to it that European access to HST continues. Should IUE and HST fail, access to successor missions would be equally important.

The Survey Committee therefore recommends that ESA assure its continuing participation in the HST programme and in possible successor programmes. The traditional procedures for medium-size missions might provide an adequate basis for this.

Again, other medium-size missions could make significant contributions. Included in these are telescopes for the spectral region below 1200 Å and missions to study stellar oscillations (astroseismology).

ISO

An infrared image of starmaking regions in the Orion constellation, from the Iras satellite, gives an impression of the sprawling clouds that First (inset) will explore in molecular detail (Image courtesy of Iras/NASA/JPL)

Infrared

ISO will be the first observatory-type mission in the infrared. The cryogenic requirements have resulted in a small telescope (60 cm) with a short lifetime (18 months) and a high cost, making ISO equivalent to a Cornerstone mission. Future missions will need larger, cooled telescopes to achieve better angular resolution. The only way to achieve such missions at an acceptable cost is to make use of high-accuracy, lightweight, passively cooled (by radiation into space) mirrors. Longer missions would also require closed-cycle coolers for the detectors, as currently under development for First. Infrared detector development is proceeding at a very rapid pace, in particular in the USA and largely for non-astronomical applications. It is important that European programmes in this area be intensified. ESA's role probably should consist mainly of ensuring that adequate information exchange takes place between the industrial and the scientific teams involved.

The Survey Committee therefore recommends that ESA perform technological studies on lightweight, passively cooled, high-optical-quality mirrors for use in the 2–100 micron part of the spectrum, and that ESA monitor the development of infrared detectors.

First, to be launched in 2005, is expected to be the first large space telescope operating in the far-infrared, with a 3 m mirror. Very much improved angular resolution will have to await the development of interferometric techniques. It would probably be wise to first see what progress will be made in far-infrared interferometry on the ground – particularly on the high Antarctic plateau – before considering major missions in space.

Interferometry/astrometry

For most astronomical studies, one of the principal obstacles to adequate quantitative analysis is insufficient angular resolution. While HST has achieved a resolution of better than 100 milliarcsec, stars and galactic nuclei are substantially smaller than this. In principle, interferometric techniques allow better resolution to be obtained and, in practice, intercontinental radio interferometers are now reaching milliarcsecond resolutions. Since the terrestrial atmosphere makes interferometry in the ultraviolet and most of the infrared impossible, while also severely limiting the possibilities at optical wavelengths, space-based interferometers appear to be more promising. The very tight tolerances in the relative positioning of the elements of an interferometer make their realisation rather challenging. In Horizon 2000, interferometry was therefore considered to be a 'green dream'. In the meantime, technology has been making progress, and on the time scale of Horizon 2000 Plus it would seem extremely desirable to make a start with the development of the field, which has been the subject of several studies at ESA (most recently 'A Proposed Medium-Term Strategy for Optical Interferometry in Space', ESA Special Publication SP-1135, August 1990; see also 'ESA Lunar Study – Phase 2: Report of the Interferometry Review Panel' (1994), which deals with interferometry from the Moon and necessary precursor missions).

Imaging interferometry attempts to obtain images with a resolution equal to that achieved with a filled aperture of the same dimensions as the interferometer (angular resolution approximately equal to wavelength/linear size). A 100 m interferometer operating at 5000 Å would have an angular resolution close to 1 mas. An interferometer with two fixed apertures obtains only a fraction of the necessary information: for good image reconstruction, movable apertures or a larger number of fixed apertures are needed. The accuracy of the positioning of an object (astrometry) is better than the angular resolution by a factor related to the signal-to-noise ratio. In this sense, it is simpler to achieve good astrometric accuracy than to obtain complete images.

Hipparcos has achieved an all-sky astrometric accuracy of the order of 1 mas. This has also allowed the distances and luminosities of many stars to be obtained from their parallaxes. In order to do very much better than that, interferometric methods have been proposed. One possibility would be to have an interferometer which measures angles all over the sky with 0.01 mas accuracy or better. This would yield a fundamental reference frame and parallaxes and proper motions of unprecedented accuracy. Such an instrument would scan the whole sky and therefore have limited signal-to-noise at any particular place: as a consequence, accurate results for faint objects could not be obtained. Another possibility would be to do relative astrometry with respect to the Hipparcos stars. Fainter objects, frequently of greater astrophysical interest, would be measured, but the overall reference frame would not be improved as much. An astrometric interferometer can only be placed in space. Moreover, it is the simplest kind of interferometer. It would therefore seem attractive to initiate the interferometric programme with this aim in mind.

Imaging interferometry is currently being developed on the ground for the optical part of the spectrum. The atmosphere poses serious problems, but adaptive-optics technology may reduce these. Only time will tell how far ground-based optical interferometry can be pushed. In the infrared, the atmospheric difficulties become more severe and space-based observations a necessity. The infrared is of particular interest because galaxies at large redshifts would radiate there most intensely, and because it is the wavelength region in which the direct detection of planetary systems around stars would be most probable. Development of an infrared imaging interferometer should therefore be initiated in the very near future.

Obviously, the programme should be closely associated with the programme of technology development directed towards interferometry from the Moon, if that programme were adopted. Since the Moon is of interest only for the construction of large interferometers (greater than 100 m), the smaller instruments discussed here should in any case be implemented first.

Radio Astronomy

A number of radio sources have not been resolved by the most powerful intercontinental Very Long Baseline interferometry techniques, which have allowed resolutions of less than a milliarcsecond to be achieved. The only way to improve the resolution is to increase the baseline and this requires the addition to the worldwide VLBI network of a space-based antenna in an eccentric orbit. Equally importantly, such a space-based antenna would allow much better quality images to be obtained for resolved sources. In the next five years, a Japanese and a Russian project foresee the launch of spacecraft with a modest antenna. Based on the result of these missions, a more ambitious project may well turn out to be justified and could be suitable for a medium-sized mission in a cooperative framework. There also have been suggestions for observations of very-low-frequency radio waves (1 MHz), possibly with low-cost 'piggy-back' antennas on other spacecraft.

Search for planetary systems around stars

Several proposals have been made to search for planets around stars other than the Sun. Most such searches would be based on interferometric techniques. The Sun describes an orbit around the common centre of gravity of the Solar System, a centre mainly determined by the position of the Sun itself and that of Jupiter, the most massive planet. If our Solar System were viewed from a distance of 10 light years – characteristic for the distances to the nearer stars – the amplitude of the solar orbit would be about 1 milliarcsec. Consequently, a 10 microarcsec-level astrometric mission of sufficient duration (5 – 10 years) could detect Jupiter-like planets to a substantial distance, and also 10 – 100 times less massive planets around the nearest stars.

Such planets would be difficult to observe directly at optical wavelengths because they are so much fainter than the star they orbit. In the infrared, the stars are less bright and the planets brighter, although the intensity ratio is still of the order of $10^6 - 10^7$. Interferometric techniques should, however, permit the detection of such planets and a crude spectral analysis of their light. Of particular interest is the possibility to detect the spectral signature of O_3 at 10 microns. Since free oxygen is believed to be present on planets only if life occurs, its detection would be a most significant event.

It is clear, therefore, that the two interferometers discussed earlier would be eminently suited for the search for planetary systems beyond our own.

Cosmology

Cosmological studies are numerous, both from the ground and from space: the observations are presently being performed with large ground-based optical and radio telescopes, as well as with satellites, in particular the Hubble Space Telescope (HST). The debate on the determination of the Hubble constant, and thus on the size and age of the Universe, remains very lively, as witnessed by recently published data.

Further opportunities for cosmological studies will come from observations first with ISO and later with First. The expansion of the Universe shifts the spectral distribution of galaxies and quasars redwards; in addition, many of the most luminous galaxies are enveloped by dust, which absorbs the optical light and re-radiates it in the infrared. Infrared observations are, therefore, uniquely suited for studies of the early Universe.

Very accurate interferometric determinations of the distances and motions of celestial objects will lead to better values for the distance scale and the dark-matter content in our Galaxy, with a major impact on cosmology.

Detailed study of the cosmic background radiation generated in the very early phases of the Universe will remain of fundamental importance. Possible medium-class missions have already been proposed in the framework of Horizon 2000. The results should provide unique information about the formation of galaxies. The prospects offered by space systems may allow the detection of the cosmic background of gravitational waves which, because of their very weak interaction with matter, can still reach us unaffected since the time they were emitted at the very birth of the Universe.

Conclusion

1. *The Survey Committee recommends that ESA initiate a Cornerstone-level programme in interferometry for use as an observatory open to the wide community. The first aim is to perform astrometric observations at the 10 microarcsec level. In addition, the Survey Committee recommends that ESA undertake studies of infrared interferometry, in particular with the aim of detecting planets around other stars.*

   After some years, the relative performances of the options should be reviewed.

2. *In view of the great advances achieved by Europe in X-ray, gamma-ray and infrared astronomy, and also taking into account that the currently planned missions in these areas may extend until about 2010, the Survey Committee recommends that appropriate studies be undertaken in the future, aimed at the development of Cornerstone-level missions soon after the conclusion of Horizon 2000 Plus.*

   In the meantime, the potential of a major high-energy astrophysics facility in the context of the Space Station should be analysed.

   Also, access to small and medium-class missions should be fully exploited.

Fundamental Physics

Astronomy and Solar-System exploration take the laws of nature for granted and apply them to study and explore celestial objects and events. By contrast, the field of fundamental physics includes those research activities in gravitational and particle physics that aim at finding new, more comprehensive concepts and laws, the testing of existing ones, and the resolution of some very basic inconsistencies. This includes, in particular, the direct detection and detailed analysis of gravitational waves, the investigation of possible violations of the Equivalence Principle, the study of new hypothetical long-range forces, the testing of General Relativity and its alternative theories, and the unification of the fundamental interactions. Puzzling inconsistencies like the too-low neutrino-flux from the Sun and the problem of Dark Matter in the Universe are even situated at the present border-line between fundamental particle physics and astrophysics.

Together with the further increasing importance of non-accelerator particle-physics experiments on the ground, space experiments are now also becoming crucial in investigating these questions. In fact, recent years have seen a marked convergence between our theoretical understanding of gravity, cosmology and particle physics which, in some countries, in a very mundane manner, is already reflected in the restructuring of comprehensive funding agencies for studies of fundamental interactions in physics, astrophysics and astronomy.

Even though the possibility of missions aimed at a better test of gravitational theories and their foundations was considered in Horizon 2000, no specific missions have been undertaken as yet. However, assessment studies in the M3 cycle were carried out for STEP (Satellite Test of Equivalence Principle) and LISA (Large Interferometer Space Antenna), a project aimed at the detection of gravitational waves. Of those two, STEP was selected for a study at Phase-A level; LISA, while scientifically at least as important as STEP, could not be selected as the required funding far exceeded the budget for a medium-size project.

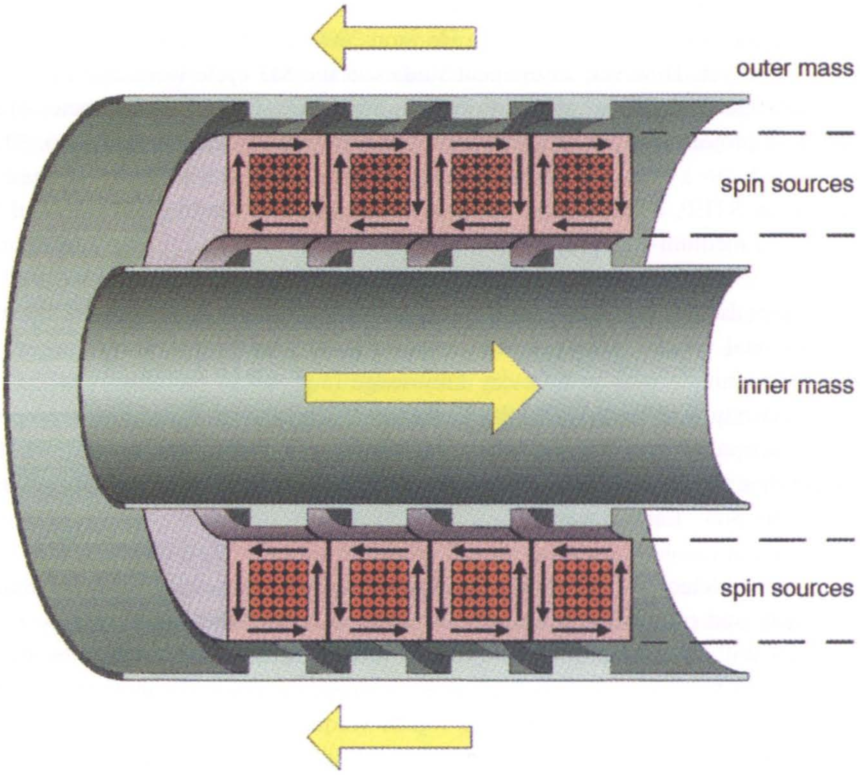
Many proposals were submitted for Horizon 2000 Plus for future missions in the area of fundamental physics. These missions typically provide improvements of several orders of magnitude over present-day knowledge (e.g. 10^5 in the case of STEP) or open up entirely new windows to the Universe. By far the most important proposal is the detection of gravitational waves of relatively low frequencies (10^{-4}–1 Hz), corresponding to wavelengths in the range 3×10^{10} – 3×10^{14} cm (the latter value being 20 times the Sun–Earth distance).

Ground-based detectors will look for signals from supernovae, compact binary coalescence, and pulsars, at frequencies well above 1 Hz. The low-frequency range below 1 Hz will never be accessible from the ground because it is masked by Earth-based gravitational noise. Moreover, there are intrinsic uncertainties about the strength and distribution of all sources emitting higher frequencies. By contrast, the space-based detector has assured sources: local, known binaries. Moreover, any black holes that may be seen from the ground will be of only stellar mass, and the signals will be weak. It is most unlikely that such detections will allow detailed comparison with predictions of strong-field General Relativity theory. The supermassive black holes that the space-based detector is designed to detect do not radiate above 10^{-2} Hz. They can be seen from space, and with amplitude signal-to-noise ratios of 5000 or more the space-based detector will be able to extract detailed information about gravitation theory from them. Furthermore, that detector extends the range of gravitational-wave detection to most of the observable Universe.

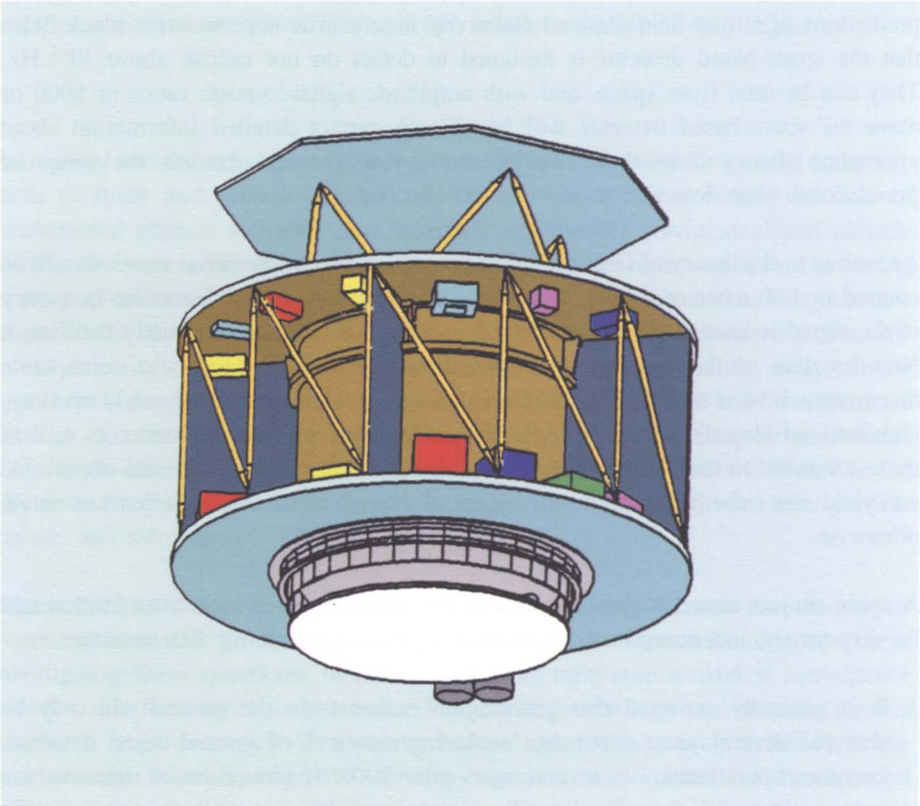
According to the theory of General Relativity, detectable gravitational waves should be emitted by a number of nearby, very close double-star systems. Since the frequency of the signal is known, detection of the periodic signal should be greatly facilitated. Non-detection of the expected gravitational waves would indeed be a remarkable discovery, whilst if they were to be found a whole new research area would open up. Gravitational waves resulting from the very early phases of the Universe, as well as those produced in the coalescence of massive black holes, would become observable and yield new information about the nature of gravity which would be hard to obtain otherwise.

A space project aimed at the detection of gravitational waves soon after 2010 would be very timely, independent of any detection on the ground, for four reasons:

1. It is generally expected that gravitational waves from the ground will only be detected several years after the 'second generation' of ground-based detectors becomes operational, i.e. several years after 2003. If ground-based detectors are indeed successful, their results will generate enormous interest, and the scientific



The mass spin coupling experiment on the STEP spacecraft



The latest concept for the STEP spacecraft community at large will need a space mission as soon as possible to observe gravitational waves in the rich low-frequency window. If ground-based detectors still have not found anything by 2010, there will be an even greater need for a space mission in the more reliable low-frequency region. Only then can it be decided whether the problem has resided in source estimates or is related to the very foundations of General Relativity.

2. The most promising means for detecting such gravitational waves would be to place in heliocentric orbit a triangular array of three pairs of spacecraft, each pair being 5 million km from the other two pairs. Laser interferometry could be used to determine the relative distances with sufficient accuracy. Although certain elements of this system are already the subject of very promising work, a project of this nature requires a number of technology developments in the area of lasers, drag-free systems, etc., and a full system study. A technology-development programme of several years' duration should be envisaged to bring the mission to the level of maturity required before such a demanding project could be approved.
3. Although a gravitational-wave project can, in principle, be flown at any time, there may be certain advantages to scheduling this mission during the years near solar minimum (around 2016), when the probability of large solar flares producing copious amounts of energetic charged particles is reduced.
4. At present, the estimated costs for a gravitational-wave project involving six spacecraft are clearly above the financial limit set for a Cornerstone. A strong effort has to be made to reduce that cost, but not by reducing the number of spacecraft as all six are needed for added science and redundancy.

The further course of action regarding the gravitational-wave observatory should only be decided a few years from now. In the meantime, work should continue on the further definition of the project and the reduction of the cost.

Various other ideas proposed for Horizon 2000 Plus aim at testing the Equivalence Principle, searching for spin-dependent interactions, testing Einstein's theory, testing Newton's inverse square law of gravity, and carrying out particle-physics studies in space. The most important ones address the universality of free fall (also called the 'Weak Equivalence Principle'), which is fundamental to Einstein's theory, and the relation between spacetime curvature and matter, usually quantified by the post-Newtonian parameter 'gamma'. In Einstein's theory of gravitation gamma, by definition, takes the value 1. Proposals for space missions are typically based on measurements of angles (deflection of light in a gravitational field) and/or time delays. Large improvements in accuracy over the currently determined values would be possible (by factors of up to 10^4). Such experiments could well be considered as medium-size missions. A 10 microarcsec astrometry mission could also make a significant contribution in this area.

In conclusion, it appears that the European fundamental-physics community, which has so far been confined to the ground, is now discovering the enormous improvements (orders of magnitude) of experiments in space in certain crucial areas. However, taking this new community on board will lead to serious financial implications for the existing more 'traditional' Space Science communities in Solar System Exploration and Astronomy/Astrophysics. The mission frequency in the 'traditional' Space Science areas is already so low that further increases in the interval between missions would lead to severe programmatic difficulties. It is clear, therefore, that the inclusion of entirely new disciplines requires some increase in the funding of ESA's Space Science Programme in the post-2000 period.

Conclusion

The Survey Committee recommends that ESA engage in the necessary technology and system studies with a view to implementing a Cornerstone-level programme on the observation of gravitational waves in particular at low frequencies (less than 1 Hz). A proper technological and financial framework should be provided so as to make such a mission possible towards the end of Horizon 2000 Plus. System studies should start when feasible.

Technological Innovation

Building on the experience gained with the Rosetta and First missions of the Horizon 2000 Programme, Horizon 2000 Plus will continue to be a focus for the development of new and advanced technologies.

From the starting point of mission-definition studies, the system design of each Cornerstone will be progressively developed, the required technology identified, and the hardware defined and tested to a level at which confidence in the success of the final mission implementation is achieved.

Horizon 2000 Plus, with clear requirements, thus provides a well-structured approach to the development of the critical technologies. The overriding aim will be to provide new equipment to improve the efficiency of project implementation, lightness and low-power operation being particular prerequisites.

Specific Technology

Mission to Mercury

The mission to Mercury represents a particular challenge in that the environment is extremely harsh, with high thermal input flux from both the Sun and Mercury itself. The spacecraft has to operate in this environment and so special emphasis is needed on the development of high-temperature system elements. Items already identified are:

- thermal subsystem components capable of protecting spacecraft equipment under high-heat-flux conditions
- high-gain mesh antenna: materials, mechanical properties, radio-frequency performance
- possible high-temperature de-spin mechanisms, associated feeds, subreflector assemblies, and power/RF transfer through rotary joints
- GaAs solar cells with special coating, and high-temperature bonding agents
- lightweight, low-power Stirling coolers
- high-temperature, low-power and low-mass sensor technology
- autonomy (spacecraft operations; guidance, navigation and control recovery modes)
- non-magnetic, high-temperature, re-chargeable battery technology
- lightweight, low-power mass storage technology.

Interferometry

Future interferometry missions require many new technology developments associated with the precision of the measurement to be made and with the need for lighter components. Some examples are:

Infrared detectors & instrumentation

Top-level technical requirements for future detectors will include:

- high quantum efficiency (order 1)
- very low dark current and read noise (necessary for planet detection)
- large array sizes, especially at longer wavelengths
- improved surface/inter-surface stability
- alternative technologies and reduced costs.

Precision ultra-lightweight optics

- lightweight optics to reduce overall mass and hence new manufacturing methods need to be explored, using new lightweight materials such as SiC.

Advanced mirror coatings & ultra-smooth polishing

- higher efficiency mirrors, so that mirrors can be kept relatively small.

Optical-system alignment techniques

- methods capable of achieving and maintaining microarcsec angular, and picometre linear, alignment between optical components.

Thermal-control systems

- passive cooling techniques, advanced heat-pipe and louvre systems, gas cooling loops, and improved cryo-coolers.

Low-cost pointing systems

- pointing platforms and image-stabilisation techniques
- smaller, cheaper star trackers.

System modelling

- sophisticated system-modelling techniques need to be developed in parallel with the system design as part of the end-to-end data processing required with such missions.

Detection of Gravitational Waves

In addition to the technology mentioned above for space interferometry, the instrument techniques used in ground gravitational-wave laboratories need to be adapted and developed for space applications, where low mass, low power and long unattended lifetimes are the norm. Areas requiring special attention are:

- lasers: increased reliability, improved frequency noise
- accelerometers: increased sensitivity and reduction of spurious accelerations
- low-thrust systems for drag-free control, such as the Field-Emission Electric Propulsion (FEEP) system, which require further development and flight qualification
- non-mechanical antenna-beam steering systems
- ultra-stable oscillators
- high-stability structures
- testing and verification techniques.

A Tool for Future Generations of Scientists

The powerful research facilities envisaged in Horizon 2000 Plus require developments that go beyond the active careers of many of those who developed the plan. Future generations of scientists will feel its impact without having been able to influence the decisions taken.

Of course, this situation is not peculiar to space science, but is inherent in all large projects with long technological lead times. Few, if any, of the current users of the Palomar 5-m telescope participated in its planning, and the situation with regard to the large particle accelerators, though less extreme, has similar aspects. This cannot be a reason to construct smaller facilities when there are no sound scientific reasons for doing so. What it does show, however, is that it is important to retain as much flexibility as possible. It should be recalled that Horizon 2000 Plus is not a plan for specific missions. It is a programme of technological and scientific developments in certain areas of science that should make it possible later to formulate detailed mission profiles. It will therefore offer ample challenges to young scientists and engineers: that of using their imagination and ingenuity to bring missions now only dimly perceived into the realm of the realisable through the development and application of advanced technology.

A related issue is that it is hardly possible for young scientists to design, construct and exploit space instrumentation on the time scale of a thesis or post-doctoral project. Again this is not unique to space-science projects. However, it should be entirely possible in a thesis to combine the analysis of data of a preceding mission with the development of new and superior instrumentation. It is particularly important to foster a culture in which young scientists remain fully involved in instrument building and technology development. In the award of ESA Fellowships also, the future need for creative instrumentalists should be kept in mind.

The most advanced science frequently comes from the combination of data obtained with different instruments. Investigating the solar wind, one would also like to know what goes on at the solar surface. In addition, one would also wish to have data on what happened one or two solar cycles earlier, so as to be able to separate the ephemeral from the more fundamental. For future generations of scientists to be able to make such studies, it is essential that the data from the Horizon 2000 Plus missions be appropriately archived. Such archives should be user-friendly and constructed in such a way that they remain accessible when technology changes. The recently implemented policy of including appropriate archiving of data obtained by ESA spacecraft in the mission planning and cost projections is therefore to be applauded.

Chapter IV

Relationship with Other Programmes

The Horizon 2000 Plus programme cannot be seen in isolation from other programmes presently being developed in Europe and elsewhere and it is essential to analyse how its value can be enhanced in the presence of the international Space Station or a future Moon programme, and international cooperation in general.

Space Station Utilisation

Space Science experiments primarily require accommodation outside the Space Station, by the inclusion of an external viewing platform at the end cone of the European Columbus module, as well by ensuring access to external mounting capabilities on the Station's structure.

Opportunities offered by these means will complement the free-flying missions in the Horizon 2000 Plus programme and will take advantage of the unique capabilities of these external mounting platforms. In addition, the Space Station is expected to provide test beds with quick turnaround capabilities to support technological development, e.g. new detector systems or new experimental devices.

The operating environment of the Space Station also looks well-suited for high-energy astrophysics instrumentation. Moreover, with the XMM and Integral missions, there will be a leading high-energy astrophysics community in Europe which will also need continuity in the development of enabling technologies for next-generation instrumentation. There is also a need for long-term monitoring of known astronomical sources over a wide spectral range (UV/X-rays/gamma-ray burst sources). Such observations could be carried out with small multi-purpose telescopes, which could also be used to observe targets of opportunity.

To take advantage of the capabilities for large experiments, the Survey Committee recommends analysing the potential offered by a major high-energy astrophysics facility within the Space Station Utilisation Programme.

A need has been identified for continuous space-based measurements of the solar total and spectral irradiance from the ultraviolet to the infrared. The International Astronomical Union (IAU) is strongly encouraging such an activity, which could be carried out by an international set of instruments dedicated to these measurements in the various wavelength bands. There is also interest in continuous monitoring of the Space-Station environment with the aim of studying the distribution of natural particles and artificial space debris. The use of a tether would allow active plasma experiments.

The Columbus laboratory may well be very suitable for some fundamental-physics experiments specifically addressing the following areas:
- atomic clocks
- accelerometers
- picogravity boxes using gravity isolation mounts.

Such experiments will be considered in close collaboration with the microgravity disciplines.

The Survey Committee judges it important that the concept of the Station be user-friendly. It is important also that a period of promotion for its further utilisation be included in the programme before and after the Station is fully assembled in space, so that the scientific community can assess its capabilities. The Station will more easily be used by the scientific community if it provides unique opportunities that are cheaper, quicker and more efficient than other space capabilities. New technologies should be tested, assuming that risks can be taken. A general relaxation of the constraints associated with the presence of man on board seems unavoidable if one wants to achieve these goals.

The Moon Programme

The lunar surface imaged by Clementine (courtesy of BMDO)

The Moon, our closest natural neighbour, is a unique space laboratory offering considerable scientific potential. Its scientific assets have been considered in a recent ESA study (see 'Mission to the Moon', ESA Special Publication SP-1150, June 1992) which highlights the Moon as a unique location for investigating the early evolution of the inner Solar System and as a laboratory for geology, geophysics and geochemistry (so-called 'Science of the Moon'). The Moon may also be a very attractive site for astronomy ('Science from the Moon'), particularly suited for the development of high-resolution imaging interferometry from ultraviolet to submillimetre wavelengths, and very-low-frequency (VLF) radioastronomy from the 'clean' environment of the lunar far-side.

The new Moon Programme presently being studied by ESA (outlined in ESA Brochure BR-101, May 1994 and ESA/SPC(94)43) as Europe's future strategy for lunar exploration and utilisation, is based on a progressive, phased approach with an initial phase devoted to the exploration of the lunar environment and proceeding, in the subsequent phases, to the establishment of a lunar outpost. This is foreseen as an optional programme and is therefore clearly independent from ESA's existing mandatory Scientific Programme. The initial exploratory phase of the Moon Programme would, however, benefit from the Moro lunar-orbiter mission currently under Phase-A study as a candidate for selection as the third medium mission (M3) of Horizon 2000.

In parallel, ESA is also studying a technology-demonstration mission, LEDA (Lunar European Demonstration Approach), with the aim of landing a payload to carry out investigations relevant to the subsequent phases of the Moon Programme. Such investigations would include study of the thermal properties of the lunar surface, soil mechanics and soil-sample characterisation, imaging of the surface, measurements of the micrometeorite flux and exospheric gas species, as well as of suspended dust particles and sky background, all of which would be essential to assess the quality of the lunar environment for future astronomical research.

Like the Space Station, the Moon should be used whenever it provides a unique capability to conduct Space Science experiments, or offers better and more efficient working than other space facilities. The Survey Committee fully endorses the fundamental principles outlined in the ESA study in that future use of the Moon, either as a scientific base or as a reservoir of natural resources, should preserve the lunar environment.

International Cooperation and Coordination with Other Programmes

As was the case for Horizon 2000, ideally Horizon 2000 Plus should be an autonomous programme that the Europeans can undertake with their own means. However, this rule is only dictated by the need for the Programme not to be dependent on decisions taken by outside, non-European organisations, and is applied only to the Cornerstone missions. International collaboration for the Cornerstones should take the form of add-on elements which, albeit important, if eliminated would still leave a first-class scientific mission. In the case of the so-called 'blue missions', however, more extensive partnerships can be envisaged. In fact, international cooperation is an essential element for all Space Science programmes in the world today. It is only through international cooperation and early coordination of the various programmes that the global Space Science effort can be conducted efficiently, without wasting precious resources through competition or duplication of effort. This is the reason why representatives from the four delegations of the Inter-Agency Consultative Group (IACG) were invited to participate in the Survey Committee's meetings as observers. Hence, Horizon 2000 Plus should benefit from cooperative agreements with all of the world's main space agencies, which will add more capabilities to the missions and enhance their intrinsic scientific value.

It is now clear that only through international cooperation can Europe participate in a Mars exploration programme or in a Solar Probe which could be initiated even earlier than the first Cornerstone of Horizon 2000 Plus. Hence, the Survey Committee strongly supports the continuation of the discussions and negotiations in the framework of the International Mars Exploration Working Group (IMEWG), or of any similar international initiative that might be set up in another discipline.

It is also essential for the Member States that the coordination with the national programmes which was initiated through Horizon 2000 be pursued and even enhanced with the aim, again, of achieving even greater efficiency in the conduct of the overall European Space Science activities, whilst ensuring the necessary complementarity between the ESA and national programmes. This sharing between both efforts should be an important element of the future European Space Science policy; it is probably the only policy by which the necessary continuity of work in the various science institutes can be secured.

The accompanying tables (overleaf) list the international space missions in the areas of Astronomy and Solar System Science which are either in process, or approved but not yet launched. Missions that are still in the study phase have been deliberately omitted because their number is appreciably larger than the technical and financial capabilities of the space agencies involved can cope with.

The tables show the involvement of the ESA Member States in the missions in terms of hardware (i.e. participation in the construction of the spacecraft) and of science (i.e. participation in the development of payloads). The column marked 'ESA' represents the involvement of ESA personnel in payload development.

Astronomy Missions: European Hardware and/or Scientific Participation

Missions	Member States														
	A	B	CH	D	DK	E	EIR	F	I	NL	N	SF	S	UK	ESA
Radio Waves															
VSOP* (J)															
Radioastron* (Rus)				X						X		X		X	
Sub-mm & mm															
Relict-2 (Rus)															
Swas (USA)				X											
Odin (S)			X					X		X		X	X		
First (ESA)															
Infrared															
ISO (ESA)		X		X	X	X	X	X	X	X		X	X	X	
IRTS (J)															
Visible — UV															
IUE (USA/ESA)														X	
HST (USA/ESA)		X													X
IEH-UVSTAR (USA)															
Spectrum UV (Rus)									X			X			
Mars 96 (Rus)	X							X							
EUV & X-ray															
Rosat (D/USA)				X										X	
EUVE (USA)															
ASCA (J/USA)															
Alexis (USA)															
SAX (I)				X					X	X					X
Spectrum X (Rus)			X	X	X	X		X	X			X		X	
XTE (USA)															
SAC B (USA/Argentina)															
Minisat-1 (E)						X								X	
HETE (USA)								X							
AXAF (USA)				X					X	X				X	
XMM (ESA)		X	X	X				X	X	X				X	
Hard X-ray & Gamma-ray															
Mir/Kvant (Rus)															
Granat (Rus/F)					X			X							
Ulysses (ESA/USA)		X						X							X
CGRO (USA)				X						X					X
GGS/Wind (USA)															
Mars 96 (Rus)								X							
Integral (ESA/Rus)**															

* With involvement of the European VLBI Network (EVN) **Payload not yet selected

Solar System Missions: European Hardware and/or Scientific Participation

	Member States														
Missions	A	B	CH	D	DK	E	EIR	F	I	NL	N	SF	S	UK	ESA
Minor Bodies															
Rosetta (ESA)*															
Near (USA)*															
Plasma/Fields/Particles															
Cluster (ESA)	X		X	X	X	X	X	X	X		X	X	X	X	X
Astrid (S)												X	X		
Freja (S/D)				X	X						X	X	X		
Wind (USA)			X	X				X							X
Polar (USA)			X					X			X	X	X	X	X
Oersted (DK)					X			X							
Interbal (Rus/S)	X			X				X	X			X	X	X	
Tiros (N/USA)											X				
Wisp (USA)													X		
Geotail (J/USA)				X									X		
TSS-1R (USA/I)				X				X	X						X
FAST (USA)															
Equator-S (D)				X									X		
Planets															
Cassini (US/ESA)	X		X	X		X		X	X	X	X	X	X	X	X
Galileo (USA/D)				X				X	X					X	
Mars-Surveyor (USA)															
Mars-Pathfinder (USA)				X											
Mars 96 (Rus)	X	X		X	X	X	X	X	X			X	X	X	X
Planet-B (J)													X		
Sun															
Soho (ESA/USA)		X	X	X	X	X	X	X	X		X	X	X	X	X
Ulysses (ESA/USA)		X	X	X	X			X	X	X			X	X	X
Yohkoh (J)														X	
Koronas (Rus)				X				X			X	X		X	
Atlas (USA)		X						X							X

* Payload not yet selected

Comparison of Responses to Calls for Mission Concepts

	Horizon 2000 2/11/83-31/12/83	Post Horizon 2000 29/6/93-15/10/93
Total proposals (excluding comments)	68	108
Astronomy	30	32
Solar	34	41
Fundamental	—	29
Interdisciplinary	—	4
Miscellaneous proposals	4	2
US proposals:	0	14 (13 in fund. phys.)

Chapter V

Financial Aspects and Schedule

The new missions in the Horizon 2000 Plus programme are to be realised in the ten-year period following the nominal end of Horizon 2000, namely between 2007 and 2016.

At the present budget level, about two Cornerstones and four Medium missions could be launched over that period. Inclusion of a third Cornerstone and an increased level of technology development require an augmentation of the resources: the technological development should begin rather soon, so as to bear fruit during the Horizon 2000 Plus period. The Survey Committee therefore suggests that ESA seek a budget increase of about 5% per year for the Science Programme for each of the years 2001–2005, after which the level could remain constant.

It is clear that not only should the spending on technology begin well before 2007, but also that some of the new missions should be in various stages of development by then. In order to achieve a smooth 'roll-forward' into the Horizon 2000 Plus programme, it will therefore be necessary for ESA to review the detailed schedule of the later missions of the existing Horizon 2000 programme so as to avoid potential cash-flow problems.

Chapter VI

Summary of Recommendations

The Survey Committee recommends the implementation of three additional Cornerstones within the context of Horizon 2000 Plus (before the end of 2016) as follows:

Cornerstone 5 (or 6) – Mission to Mercury

Cornerstone 6 (or 5) – Interferometric Observatory

Cornerstone 7 – Gravitational Wave Observatory

Moreover, four more medium-class missions are recommended, in addition to M3 and M4, which have not yet been specified within Horizon 2000. In the Solar System area, at least one of these should be devoted to participation in the International Mars Programme and another to Solar Physics. Also, effective use should be made of the European participation in the International Space Station; in particular, the possibilities of a major high-energy astrophysics facility should be explored.

Increased technological studies are recommended as an essential pre-requisite for more effective and less costly future missions. This should make it possible to maintain the upper limits on the cost of Cornerstones and Medium missions, despite the very much increased scientific requirements.

The inclusion of the Fundamental Physics discipline and the expanded Technological Activities will require a modest increase in the funding of the ESA Scientific Programme beginning in 2001. It is therefore proposed to augment the budget level by 5% each year for the years 2001–2005.

Chapter VII

Topical Team Reports

The recommendations made by the Survey Committee were based on the inputs provided by the five Topical Teams that were established in late 1993:

–relating to Solar-System Research

Team I: Moon, Planets and Smaller Bodies of the Solar System

Team II: Sun, Heliosphere and Plasma Physics

–relating to Astronomy

Team III: High-Energy Astrophysics (Cosmic Rays, Gamma- and X-Rays)

Team IV: Ultraviolet, Optical, Infrared, Submillimetre and Radio Astronomy (including Astrometry and Space Interferometry)

–relating to Fundamental Physics

Team V: Fundamental Physics (Cosmology, General Relativity and Gravitation, Particle Physics)

The Terms of Reference that were assigned to the Topical Teams included both general and specific tasks.

The general tasks consisted of performing an analysis of the expected trends, major scientific goals and expected technological developments, each Team focussing on its respective area of expertise. The Teams were invited to make use of the information contained in the submissions received by the Executive in answer to the Call for Mission Concepts issued on 29 June 1993 (see Annex 1), as well as any other input they might consider useful.

Mindful that Cornerstone missions were identified by the SSAC as flagships for European Space Science, as a conclusion of its work each Topical Team was expected to establish, in its particular area of expertise, those priority objectives that could become Cornerstones of the Horizon 2000 Plus Programme.

Each Topical Team was also requested to discuss the general scientific themes for medium-size missions and to assess the value of small missions in its particular field of science.

In addition to the above general tasks, the Topical Teams were asked to be ready to answer detailed questions that might be put by the Survey Committee whilst establishing the final programme.

In the few months of their existence, the Topical Teams did a vast amount of work, which is summarised in the following reports and, of course, reflected in the final recommendations by the Survey Committee, given in Chapter III.

Topical Team 1

Planetary Science

Y. Langevin (Chairman), *S. Bauer, M. Fulchignoni, J.C. Gerard, G. Neukum, F. Taylor & H. Wänke*

Introduction

ESA's planetology programme is now in a completely different situation from 1983, when Horizon 2000 was defined: at that time, ESA had approved Giotto, a fly-by mission to comet Halley, but had yet to conduct its first deep-space mission. The outstanding success of Giotto in 1986, the selection of Huygens in 1988, and finally the selection in November 1993 of Rosetta as the third Cornerstone of Horizon 2000, have resulted in a much more mature planetology programme for the Agency. It gives ESA a major role on the international scene, and now constitutes the main focus for contributed instruments from the Member States, which ten years ago were mainly destined for flight on American or Soviet missions. It is, of course, impossible to cover all of planetary science with just three missions, and the inner planets are not yet represented in a major way in any programme in which ESA participates. In order to achieve a properly balanced scientific programme, the inner planets should be assigned a very high priority in ESA's mid- and long-term planning.

An important input in this context was the set of answers to the Call for Ideas that ESA issued in preparation of the Horizon 2000 Plus definition procedure. Fifteen proposals were received in the planetary field:

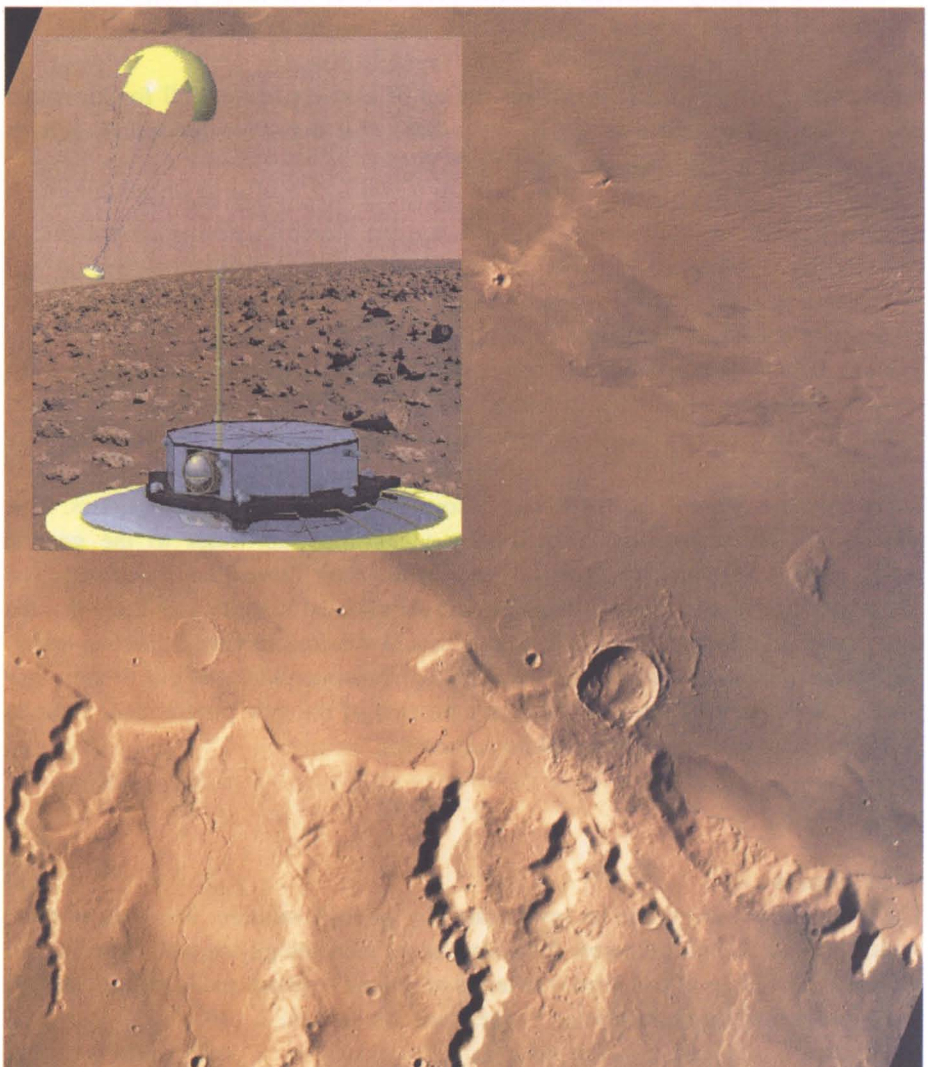
- inner planets: seven proposals (four missions to Mercury, a lunar programme, a Mars rover, a solar-sail mission to Venus and Apollo-Amors)
- outer planets: three proposals (a mission to the inner Jovian system, a mission to Neptune and Triton, an Earth-orbiting radio/UV observatory)
- small bodies: five proposals (a comet nucleus sample return, the Venus/Apollo-Amors mission, a multiple asteroid rendezvous, a mission to discover very distant small bodies, and an Earth-orbiting observatory for cometary research).

Three proposals were dedicated to the discovery of planets around other stars. This topic is of outstanding interest and may yield important clues to the generic characteristics of planetary systems, and hence their formation processes. The investigation techniques clearly belong to astronomy, and it appears unlikely that the early phases of this search would justify dedicated missions of very high complexity. The proper approach is to include the search for extrasolar planets as one of the major goals of any astrometry or interferometry mission of the new programme, so as to guarantee that these objectives will be addressed.

Scientific and Programmatic Considerations

Inner planets
The inner planets are yet to be investigated by an ESA mission. Missions to these bodies of high scientific interest for comparative planetology are well-suited to the technical capabilities of the Agency in terms of ground segment and launch capability, which are major requirements for Cornerstone candidates.

A large rock on the lunar surface



Tectonic features on the martian surface. Inset: One of the proposed InterMarsnet landers

The Moon

The Moon is unique among the set of inner planets as the least evolved of this family of bodies. It is a natural laboratory for the early accretional processes and early evolution of the inner Solar System. Indeed, most of our ideas about the first hundred million years of the evolution of terrestrial planets are based on our knowledge of the Moon's history. The Moon is also a unique body in terms of its relationship with the Earth, providing us with the opportunity to unravel the origin and early evolution of the Earth–Moon system. The Moon was extensively studied during the Apollo period, and it is the only planetary body from which documented samples have been retrieved. However, the focus on manned missions resulted in very incomplete coverage, and major questions remain unanswered, such as the existence and size of a lunar core, and the possible presence of ices in lunar polar craters, similar to that recently observed on Mercury. After more than 30 years, we can build on the existing database to define new science missions dedicated to the Moon, addressing major issues such as its global composition and internal structure, and the early evolution of a unique double-planet system. There is an obvious relationship with the 'Return to the Moon' programme presently being studied by ESA for submission to the next Ministerial Conference. However, the phases considered for the years 2010–2015 put strong emphasis on manned space activities and industrial uses, and far exceed the budgetary means of the Science Programme.

Venus

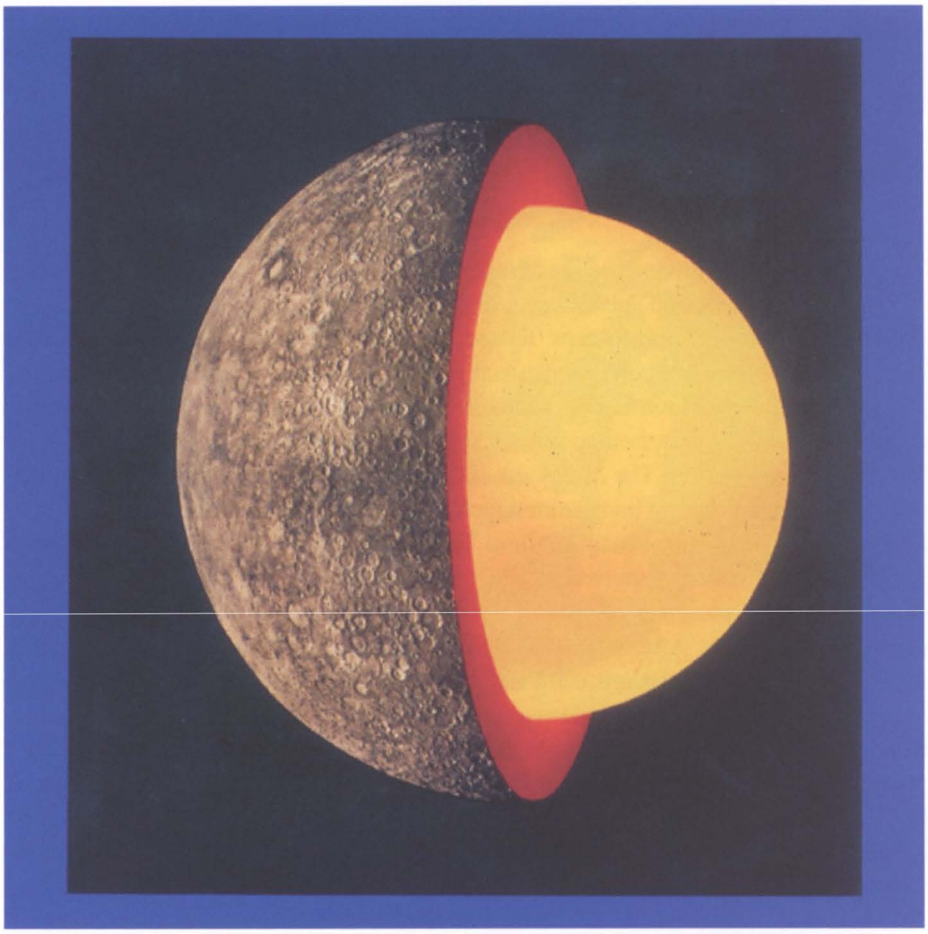
Although formed in the same part of the Solar System as the Earth, Venus differs in many important ways from our planet: the lack of plate tectonics, the depletion of water and other volatiles, and the dense CO_2 atmosphere, which results in very high surface temperatures and pressures. The global super-rotation and the recently discovered dynamical activity in the deep atmosphere and over the polar regions still defy interpretation in terms of terrestrial meteorology. After the highly successful Soviet and American missions, the next steps are an orbital mission with multispectral mapping and sounding instruments, a large number of small probes and larger entry probes to study the atmosphere, and eventually long-duration surface stations (operating for at least a few months). Given the obvious interest in Venus for comparative planetology, ESA participation in an international project of this type should be considered.

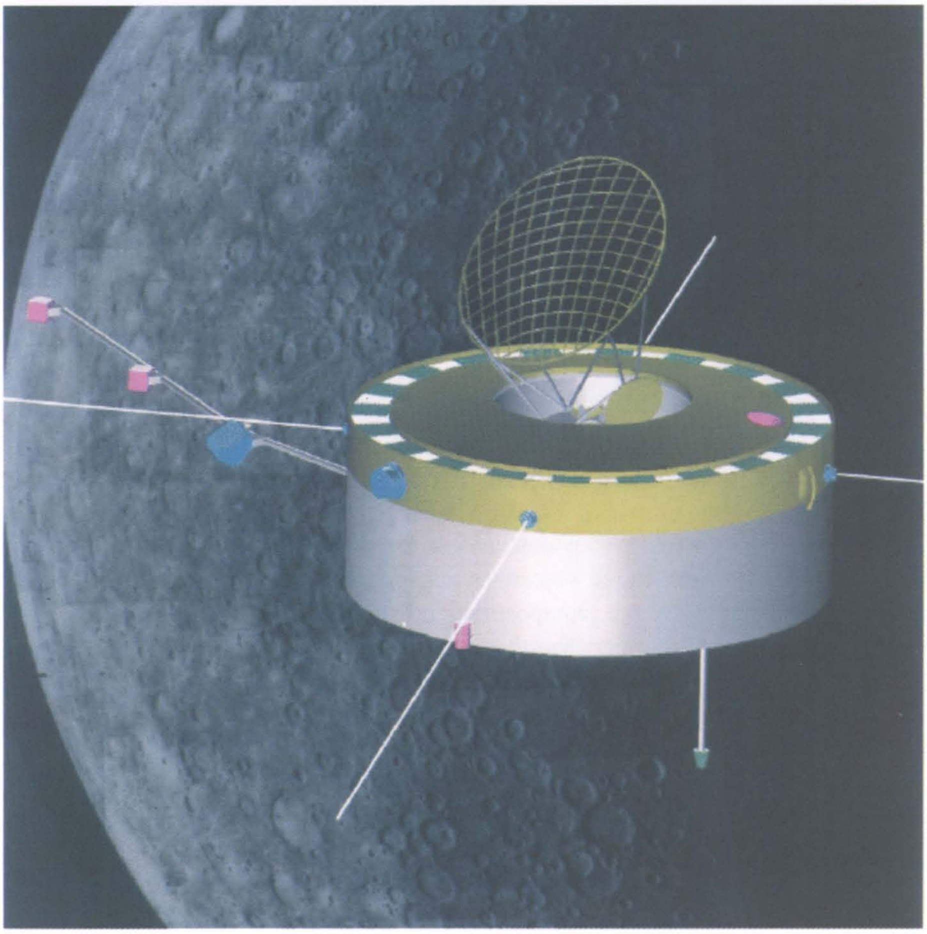
Mars

There is a worldwide consensus that Mars has a very high scientific priority among inner planets, due to its outstanding interest for comparative planetology. A non-restrictive list of major science objectives may be summarised as follows:

- the internal structure: size, composition and state of the core
- the origin of the hemispheric north-south asymmetry
- the history of volcanism and tectonic activity
- the chemical and mineralogical composition of the surface
- the meteorology and climatology of the planet
- the role of minor constituents and aerosols in atmospheric photochemistry
- the interaction of the planet with its environment, in particular escape processes for atmospheric gases
- the role of volatiles (water, CO_2) throughout the history of the planet
- the planet's potential as an abode for life.

Mercury, is believed to have the largest core, proportionally, of all of the planets (up to 75% of its radius). The planet's thin lithosphere (600 km) is shown in red. The surface is represented by a photomosaic of images taken by the Mariner-10 spacecraft (from: The Elusive Planet, by R.G. Strom, Cambridge University Press)





The proposed Mercury Orbiter spacecraft

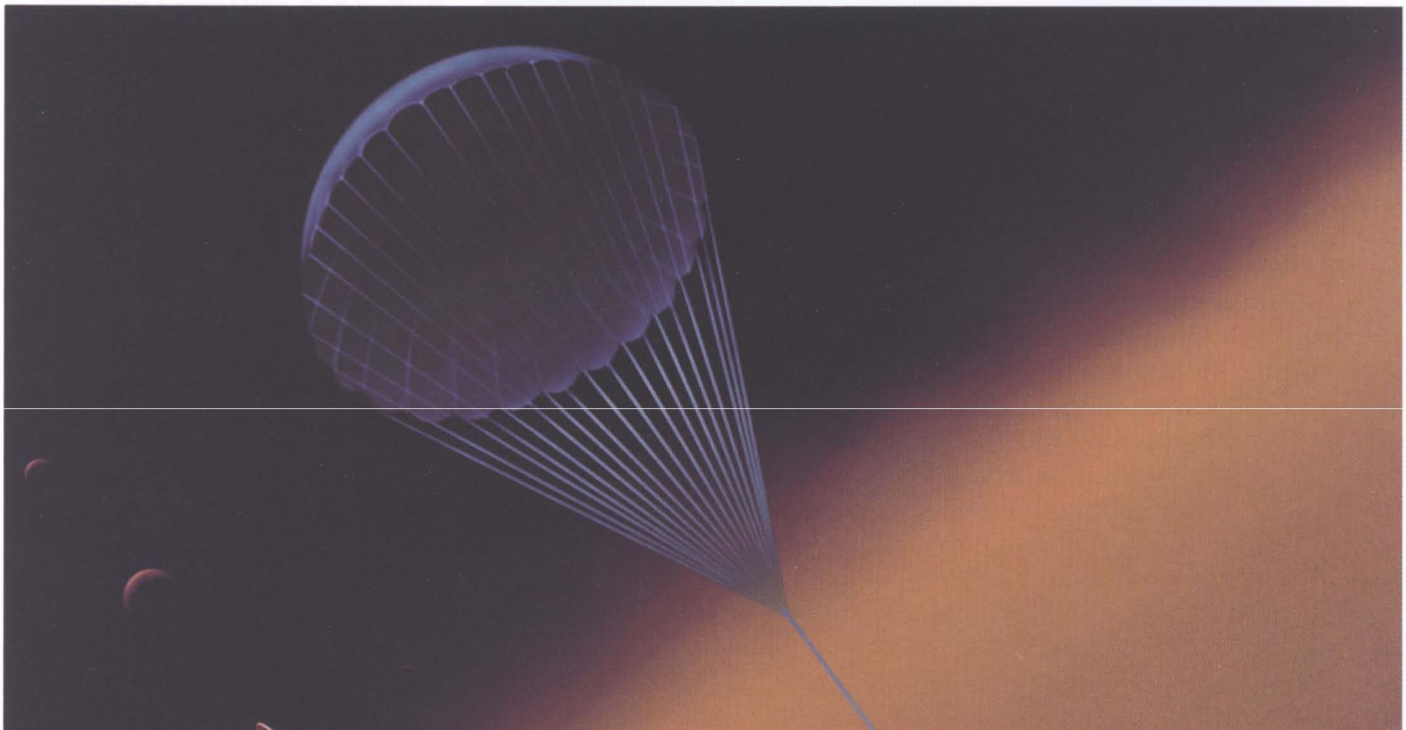
Despite this very high scientific priority, the future for Mars exploration is now quite uncertain. The programmes that were planned for 1993–1995 were to obtain comprehensive remote-sensing coverage in terms of the geomorphology, mineralogy and chemistry of the surface, as well as the composition, structure and time evolution of the atmosphere of this planet. The loss of Mars Observer in August 1993, and the present uncertainties regarding the content and schedule of the Russian Mars 1994 mission put these remote-sensing investigations in jeopardy. Russian plans beyond the Orbiter initially scheduled in 1994 can be considered tentative at best. The ambitious Mesur network of surface stations has been de-programmed by NASA. A four-station network is now being considered as a collaboration between ESA and NASA (InterMarsnet). An international programme focussed on Mars is presently being discussed in the International Mars Exploration Working Group, which brings together all of the major space agencies. This international effort and the present programmatic uncertainties make it difficult to define a stand-alone large mission which would provide the best scientific return for Europe 15 years from now. What should therefore be considered is a Cornerstone-level commitment to Mars exploration for the years 2010–2015. Such a commitment would give ESA a leading role in defining the international Mars exploration programme for the period considered. Scientific priority should be given to study of the subsurface structure, the geochemistry, the climatology, the geology and exobiology of Mars using surface modules.

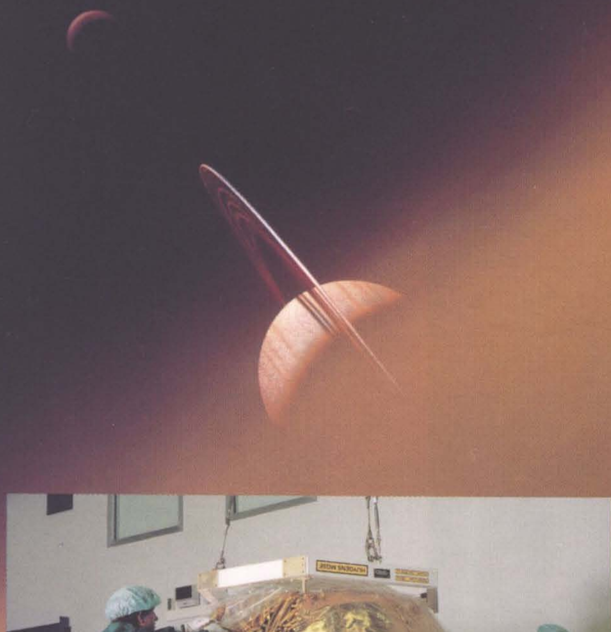
Mercury

Mercury is by far the least well known of the inner planets: less than 40% of its surface was observed, at a resolution of a few kilometres, by Mariner 10 in 1974. It is the innermost object of the Solar System, with a period of 88 days, and an orbit ranging from 0.31 to 0.46 AU. The rotation state of Mercury is unique: its rotation period of 58.7 days is exactly two thirds of its orbital period. As a result, Mariner 10, which was in a 3 : 1 resonance with Mercury, observed the same hemisphere three times. The next logical step is therefore an orbiter mission providing complete surface coverage with high resolution and exploring the planet's unique plasma environment (see Topical Team 2 report).

Mercury's diameter (4880 km) lies between those of the Moon (3480 km) and Mars (6800 km). Its density, at 5.4, is higher than that of any other planet, including the Earth (4.4) when the gravity-induced compression is corrected. This implies that Mercury has the largest core, in proportional terms, of all of the planets (up to 75% of the radius, depending on the models). With an orbiter mission, it would be possible to determine the principal moments of inertia, which provide strong constraints on the mass distribution inside the planet. Higher terms of the gravity field would be obtained, as well as a comprehensive altimetric coverage. These two sets of data should yield information on the rigidity and structure of the upper layers. A unique characteristic of Mercury is its dipolar field. The example of the Earth has shown the large potential of magnetic-field investigations for probing the core's evolution. The determination of remnant crustal fields would provide information about the long-term evolution of the dipolar field.

Mercury's surface shows significant similarities with that of the Moon: a large impact basin, Mare Caloris (1200 km in diameter), was observed by Mariner 10, and others may be present in the unobserved regions. The whole surface topography is dominated





The Huygens Probe

by impact craters. There are, however, profound differences between the surface of Mercury and that of the Moon: the albedo of Mercury (11%) is similar to that of lunar maria not only in Mare Caloris, but also in the inter-crater plains which constitute most of the observed areas. The basins are assimilated to lava flows by analogy with the lunar case, but the resolution of Mariner 10 is not adequate to resolve specific magmatic features. Similarly, our understanding of crater morphology and crustal relaxation processes is severly hampered by the kilometre-level resolution of Mariner 10. Craters can probe the upper 10 to 100 km of the crust, and yield clues as to the physical and mineralogical characteristics below the surface.

The chemistry and mineralogy of Mercury's surface is particularly important as this planet is an end member of the accretionary process. A major question is: what was the compositional gradient of condensates in the solar nebula from the formation zone of Mercury to that of Mars, and then to the main belt of asteroids? The evidence from meteorites suggests that there is a gradient in oxidation state. Contrary to main-belt asteroids and Mars, no meteorite from Mercury can reach the Earth. In-situ or remote-sensing investigations of Mercury are therefore the only method of obtaining information on the chemistry of the inner solar nebula. A detailed understanding of the chemistry and mineralogy of the major structural units is also essential in providing constraints on early differentiation processes. The chemical composition can be determined at a resolution of a few 100 km (depending on the orbit) with gamma-ray spectrometry. The tool of choice for investigating the mineralogical composition is reflectance spectrometry in the near-infrared and thermal-emission spectrometry. The latter is quite a promising investigation method for Mercury given the very high surface temperatures.

A Mercury Orbiter mission constitutes an extremely attractive Cornerstone candidate. The science objectives cover a broad range of goals in comparative planetology, from the origin of dipolar fields to the chemical and geological evolution of the innermost planet of the Solar System. Such a mission would explore a fascinating plasma environment and make possible long-term investigations of the inner heliosphere (see the Topical Team 2 report). The two Venus flybys required would provide an opportunity to study this planet.

Finally, while technologically challenging, this mission can be conducted by ESA alone (not excluding non-critical participations by other agencies).

Outer planets

The next major steps in the study of giant planets will be taken by Galileo (despite its telemetry-rate problems), which will reach the Jupiter system in 1995, and Cassini/Huygens, which will study Saturn, the rings, Titan, the icy satellites and the magnetosphere in the years 2004 to 2008. In the 2010–2015 time frame, two objectives are considered high priorities: a third-generation mission to the Jupiter system, focussing on the planet's internal structure, the Galilean satellites, particularly Io and Europa, and the Io plasma torus, and a Cassini-type mission to study Neptune, its rings, Triton and the icy satellites.

Jupiter is just beyond the reach of missions powered by solar panels, the survival of which in the intense radiation belts of the inner regions is anyway questionable. This means that RTGs are required for both the Jupiter and Neptune missions. Also, the presently available ESA 15-m ground segment is limited to a range of 3.25 AU. These technical limitations and the large budget required make it difficult to propose a Cornerstone in this area. Both proposed missions would, however, have outstanding scientific returns and should be highly considered in the competitive framework if a collaboration with NASA appears feasible.

Small bodies

The main goal of the Rosetta mission, which has been selected as the third Cornerstone of the Space Science Horizon 2000 Programme, is to analyse cometary material on or close to a cometary nucleus. The next step in the study of comets is a sample return such as that originally proposed for Rosetta. The main specific assets of a sample return are to provide detailed information on ages, the isotopic composition of noble gases and other heavy elements, and the organic chemistry of cometary material. Such a mission is technically very ambitious, requiring a major involvement by another agency.

By 2010, one can expect the two flybys of small S-type asteroids in the main belt already performed by Galileo to have been complemented by several additional flybys in the main belt (Rosetta, possibly Cassini, or a multiple flyby Discovery mission) and one rendezvous with Eros, the largest Apollo–Amor asteroid (NEAR). The next step is a multiple rendezvous mission to main-belt asteroids. This mission requires an advanced Solar Electric Propulsion system, which with the associated development costs, means that it probably exceeds the budget available for an ESA-only Cornerstone mission. There is increasing worldwide interest in missions of this type, and it would be a very good candidate for an ESA collaborative mission with another partner for competitive selection in the 2010–2015 time frame.

Proposals for Planetary Missions

Cornerstones

Two candidates have been considered to be of equal scientific priority and would strongly mobilise the European planetary community:

- A mission to the inner Solar System focussed on Mercury. The Mercury component could be a spinning polar orbiter with a despun platform, so as to provide the best configuration for remote-sensing observations of the planet, investigations of its plasma environment, and possibly close-range observations of the Sun. The scientific opportunity constituted by the two Venus flybys should be investigated during the study phase.

- An ESA Mars programme emphasising surface modules. Two candidates have been identified: a medium-sized rover and an advanced geochemical laboratory. Both would require some level of international collaboration. What is therefore needed is a Cornerstone-level commitment to Mars which would give ESA a leading role in shaping the future of the international Mars exploration programme.

Both proposed Cornerstone programmes fully qualify as technology drivers:

- For the Mercury orbiter mission, the main fields to be developed are the miniaturisation of subsystems and of the payload (so as to maximise the science return within the launch capability of Ariane-5), high-temperature materials and thermal protection and communications at small angular distances from the Sun, as well as delivery systems for Venus and entry-probe design if such a component were implemented.
- For the Mars programme, a non-exhaustive list of useful technology developments includes entry modules, passive and active descent systems, direct communication devices, autonomy on the planet's surface, miniaturisation, low-temperature energy sources and storage, sample acquisition and distribution devices, and a vacuum/cold resource for science experiments.

These two projects are highly complementary in terms of the role of ESA in the international space-science framework. The Mercury mission would fulfil very valuable science objectives which are not presently included in the long-term programmes of any other agency. In contrast, a commitment to Mars would give ESA a major role in a worldwide international programme. Both proposed programmes have such high priorities within the European planetary-science community that programmatic means should be considered to fulfil the goals of the programme not selected as a Cornerstone.

Missions other than Cornerstones

Among large programmes, three candidates have been given a very high scientific rating: a Multiple Asteroid Rendezvous mission, a Jupiter Orbiter dedicated to Europe, Io and the Io torus, and a Neptune/Triton Orbiter mission. All three require an international partner, most likely NASA, due to the shortage of critical technology in Europe for outer-planet missions (RTGs, DSN), or the very high cost of Solar Electric Propulsion development (asteroid tour). In the medium-mission range, a Venus Orbiter mission dedicated to the atmosphere (possibly in combination with a NASA 'Discovery' mission) is considered very attractive.

ESA's mid- and long-term programme as defined by the original Horizon 2000 Programme has proven remarkably stable. This is recognised as an important asset by the European planetary community, when contrasted with the uncertain future of approved NASA or Russian programmes. Several problems have been identified by Topical Team 1 as regards the competitive selection process:

- With the existing accounting rules, any planetary mission in the 2010–2015 time frame with a significant scientific return is likely to exceed the present budgetary limit. Either the rules or the limit may have to be questioned, otherwise ESA's contribution may become restricted to minor contributions to international programmes.
- The mission process requires 10 years from the Call for Ideas to the launch (1993 to 2003 for the M3 mission). This raises severe problems for the collaborative missions (in particular with NASA). Furthermore, there is no possibility of seizing interesting collaboration opportunities at modest levels. Some flexibility should therefore be considered for the Horizon 2000 Plus Programme.

Topical Team 2

Solar, Heliospheric and Space Plasma Physics

B. Hultqvist (Chairman), *C. Chiuderi, C. Fröhlich, G. Haerendel, P. Hoyng, P. Lemaire & L.J.C. Woolliscroft*

Introduction

The disciplines of solar, heliospheric and space plasma physics are closely associated in the ESA Science Programme tradition, with common elements of physics providing a strong link between them. There is also an overlapping of the scientific communities involved. There have been major initiatives by European national agencies in these fields and there is a common historical pattern of development. The earliest missions were carried out by NASA, then came early European access to NASA missions, and finally ESA's own initiatives, which have brought Europe the recognised leadership in many major areas. All three fields are well supported by an active scientific community.

The ideas submitted in response to the Call for Mission Concepts for Horizon 2000 Plus has led Topical Team 2 to the following conclusions:

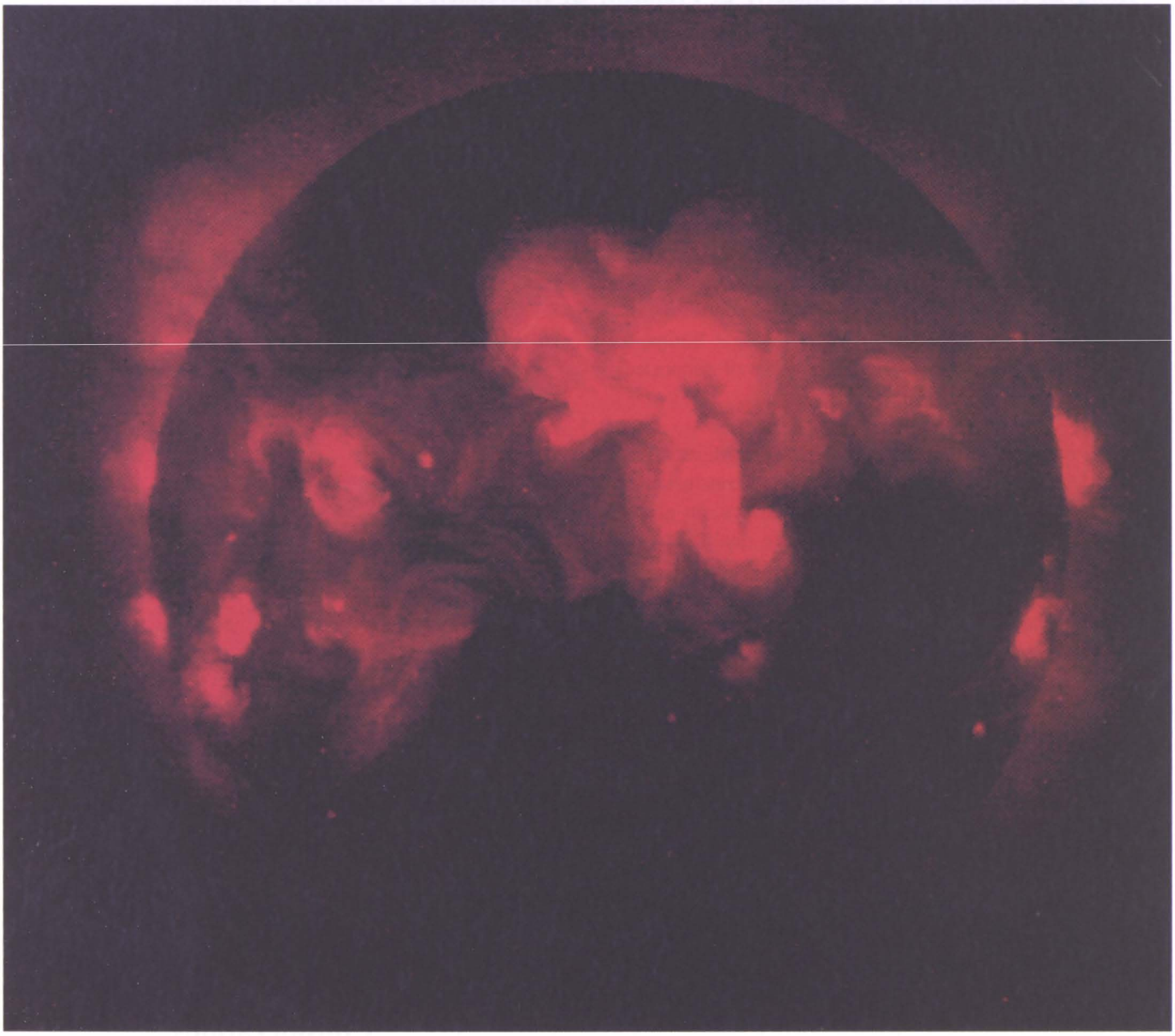
- In the area of solar and heliospheric physics, an array-type mission for stereo viewing of the Sun and the heliosphere qualifies well as a solar-heliospheric Cornerstone.
- In the area of space plasma physics, a Mercury mission, with a spacecraft orbiting the planet, has the full potential of a Cornerstone mission.
- A Solar Probe, which for technical reasons cannot be a Cornerstone mission, is extremely interesting to the European community as a multi-agency project.
- Many proposed missions may be well suited as future medium-size missions in ESA's Scientific Programme (to be determined by future competitive selection procedures).
- There is a strong need for satellite monitoring missions in the future. These should take the form of joint projects by the various space agencies.

Scientific and Programmatic Considerations

The strongly related plasma, solar and heliospheric disciplines share a scientific maturity that allows them to plan problem-oriented missions aimed at solving key scientific issues. This does not, however, preclude the possibility of unexpected discoveries, but distinguishes these disciplines from other more exploratory fields.

Solar and heliospheric physics has a number of growth areas:

- *Solar oscillations,* first detected at the end of the seventies as global modes of the Sun, opened up the new field of helioseismology, the sounding of the solar interior through detailed analysis of the frequencies of oscillations. As this research is based on time-series analysis, the data continuity is of paramount importance, as exemplified by the results of the Phobos IPHIR experiment. Helioseismology throws new light on the structure of the Sun (helium abundance, depth of the convection zone, internal differential rotation, equations of state, opacities) and will improve our understanding of solar/stellar evolution.



The solar corona as seen by the soft X-ray telescope flown on the Japanese Yohkoh satellite, providing insight into the evolution of magnetic activity and the range of dynamic phenomena. Detailed spectroscopic observations of these phenomena by Soho should reveal how energy is supplied to the corona.

- The structure of the *solar atmosphere* in space and time has always been a major field of interest to solar physicists. The understanding of the generation, dynamical evolution and dissipation of magnetic fields is an open problem of solar physics and represents a strong link with the discipline of space plasmas. The solar corona is the seat of many energetic phenomena with widely differing space and time scales and the solar-flare phenomenon could be a unifying scheme to understanding most high-temperature processes taking place in the solar atmosphere.
- *The solar wind,* and in particular its origin and acceleration, is another key problem of solar physics, strongly coupled with our understanding of the behaviour of magnetised plasma flows.

Space plasma physics, the first discipline to exploit the new space-transportation techniques in the 1960s, has long since left the immediate exploration stage, and yet its evolution is punctuated by 'surprises', which in turn have opened new fields of research.

A common element linking the large and active solar heliospheric and space plasma communities in Europe is the fact that ESA, besides playing the primary role in the Ulysses mission, is about to launch two major missions, Soho and Cluster, that together compose its first Cornerstone. These three missions will put the scientific leadership in this field firmly in European hands by the end of this century.

The development of the space plasma physics field in the period after Ulysses, Soho and Cluster should involve a combination of several different lines:
- Dedicated plasma-physics missions in the Earth's magnetosphere, aimed at a deeper understanding of specific processes – for which fairly small spinning spacecraft can be used – are best conceived and executed within national programmes.
- Missions requiring multi-point measurements in the Earth's magnetosphere are expected to play an increasingly important role in future magnetospheric-physics research. Only ESA is likely to be able to carry out multi-spacecraft missions in Europe, at least for some decades.
- Comparison of the physics in different magnetospheres has contributed, and will contribute further, in important ways to the understanding of the basic processes. Many physical processes reveal their very nature by comparison of realisations in different parts of parameter space. Well-defined planetary missions should therefore be a major part of future European space plasma physics research.

There is, and will continue to be, a fast development of basic measurement techniques in space plasma physics, which is expected to open up new phenomena for investigation in the future.

Proposals for Solar Heliospheric and Space Plasma Physics Missions

Cornerstone missions

Solar Stereoscopic and Heliospheric Mission

Observations of the Sun and the heliosphere are hampered by the fact that they integrate over the line of sight, which makes it difficult to determine the three-dimensional structure of the observed features. Long-lived structures may be disentangled by exploiting the solar rotation, but as a rule it has not been possible to determine the geometry of solar magnetic fields and coronal structures. Stereoscopic observations would be a real breakthrough in this respect.

A typical minimal mission, with no redundancy and limited three-dimensional imaging capacity, would consist of two spacecraft with identical payloads, one placed, for example, at L1 (0.01 AU from the Earth) and one at L4 or L5 (60° and 1 AU from the Earth). The optimal location needs to be studied, and this might entail among other things a trade-off between three-dimensional imaging capacity (desired stereo angle) and data rate. Ariane-5 is able to put two such spacecraft, each weighing 1500 kg, into position.

Problems that could be tackled with two suitably equipped spacecraft include:
- dynamic processes, such as coronal mass ejections
- the dynamics and evolution of magnetic fields from the photosphere to the transition zone and lower corona
- in helioseismology, the identification of different m-modes (phase shift proportional to $m\alpha$, with α the phase angle between the two spacecraft) and the rotation in the solar interior
- how the solar activity and its changing magnetic fields influence irradiance (the solar energy seen at the Earth) and luminosity.

From the widely separated spacecraft, one could also get information about the large-scale structure and evolution of phenomena like coronal mass ejections, fast solar-wind streams, interplanetary shocks and high-energy particles.

A solar stereoscopic and heliospheric mission would command interest from a wider community of scientists, as it would allow a detailed study of the solar forcing of the Earth's climate. Moreover, the mission naturally lends itself to international cooperation, as another agency might provide a third spacecraft at a phase angle of 180°.

Mission to Mercury

Mercury is still largely an unknown planet, both in terms of geology, tectonics and surface morphology, and with respect to its magnetosphere, the focus of our interest here.

The features that single out Mercury and make it particularly interesting among magnetised celestial bodies are the absence of an ionosphere and of a conductive surface, in combination with a relatively strong magnetic field of its own and the smallness of the magnetosphere with respect to the ion-gyro radius.

The time scale of response to changes of the interplanetary conditions (e.g. orientation of the interplanetary magnetic field) is expected to be very short (approximately 1 min) compared with the coherence time under these conditions.

A mission to Mercury should include observations of the planet. It might also include observations of the Sun which, together with Earth-based measurements, would give stereoscopic observations. The inclusion of solar measurements would, however, add considerably to the complexity of the payload and to the integration difficulties. Consequently, it is felt that solar measurements should be added only if the main objectives of the Mission to Mercury would not be significantly limited in the process.

Solar Probe

The scientific potential of a Solar Probe was already recognised by the Horizon 2000 plan, which classified it as one of its three major long-term objectives, or 'green dreams'. Since then, interest in a Solar Probe has only increased in the international solar science community.

A close approach to the Sun, besides providing observations of the solar surface features with unprecedented resolution, would allow disentangling of the signatures of the fundamental plasma processes responsible for the heating of the solar corona and acceleration of the solar wind.

There is a general consensus that meaningful observations can be performed only if the Probe reaches regions within the Alfvènic point and possibly within the sonic point. As the location of the latter is unknown, the precise choice of perihelion for the Probe is open to debate.

The Probe could carry a scientific payload including in-situ field and particle measuring devices, imaging and spectroscopic instruments, and gravitational experiments. It could be a single-shot or a multiple-pass mission.

The minimum mission would be a single-shot mission with a capability for in-situ measurements only. The expected scientific output of such a mission, however, would not justify the large investment involved, other than as a precursor for a later mission.

Even the minimum mission would be very demanding from a technological point of view, as it would require:

- a Jupiter gravity-assist manoeuvre (common to all mission profiles); moreover, a small final value for the perihelion would require further passages close to planets (possibly using aero-deflection) or the use of a solar sail or an electric propulsion device
- an effective thermal shield
- an energy-generation system that could perform well both at Jupiter-like distances and very close to the Sun. Only RTGs presently offer such performance.

Despite all of the above problems, a long-lived Solar Probe would be an exciting and fundamental step in solar science, apart from being a formidable technological challenge. Its scale, however, surpasses the limits imposed on a Cornerstone, and therefore it can only be conceived in the framework of an inter-agency joint effort. ESA should be encouraged to play a primary role in promoting such an ambitious and fascinating project.

Other programme elements

Potential medium-size missions

The following areas were identified as offering the best opportunities for medium-size missions:

- *Auroral Polar Orbiter,* aiming at a global mapping of the auroral energy input into the ionosphere and lower thermosphere, and the analysis at high resolution in time (1 s) and space (1 km) of the aurora, especially during substorms. Dayside observations through X-ray observations and improved fast-particle detectors make it possible to measure the energy input with unprecedented precision.
- *Multi-spacecraft plasma studies* with simple instrumentation to study the auroral acceleration process and stress balance of the magnetospheric plasma, objectives which cannot be achieved by the Cluster mission.
- *Solar sounding through radio waves,* to provide tomographic images of the corona. A typical mission would require one or two dedicated spacecraft carrying very stable transmitters, to be placed near the anti-Earth position at 1 AU from the Sun.

Small missions

No specific *small missions* were proposed for Horizon 2000 Plus. That does not mean that there are no ideas for small missions in this area; rather, the community apparently prefers to propose them within national programmes.

Missions involving *small spacecraft* were proposed, but they belonged to one of the following types:

- small spacecraft requiring big launchers for the study of the solar wind or the solar corona
- multi-spacecraft missions, mostly for plasma investigations.

Both types belong rather to the medium-size mission class.

Monitoring satellite missions

There is a class of missions involving monitoring of the total and spectral solar irradiance, the solar wind or the response of the upper atmosphere, which constitute a most important service to the solar-terrestrial research field, but whose execution is not attractive for any one space agency because they offer little novel science in themselves. Without such monitoring satellites in the future, those research programmes that depend, for instance, on solar-wind monitoring will come to an end when IMP 8 ceases functioning.

Monitoring satellite missions should therefore be undertaken as joint projects by the various space agencies.

Topical Team 3

High-Energy Astrophysics

A.C. Fabian (Chairman), *G. Bignami, T. Courvoisier, W. Hermsen, B. McBreen, J. Schmitt & G. Vedrenne*

Introduction

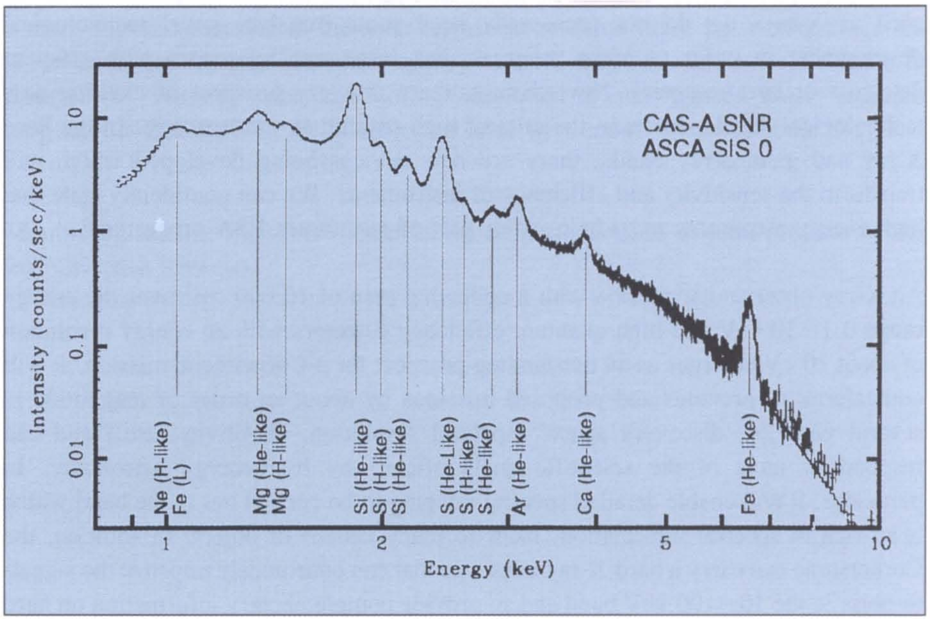
The energy band covered by high-energy astronomy is vast, ranging from soft X-rays below 0.1 keV to hard gamma-rays above 100 GeV, which is at least nine decades of photon energy. The dominant cosmic sources change with energy and the detection techniques also change. No one telescope can cover the whole band. The scientific reasons for covering significant parts of the band are overwhelming. It is in this band that much of the power of the most efficient cosmic sources – active galactic nuclei – emerges, where many of the dominant resonant and fluorescent atomic lines of the elements above helium emerge, where the nuclear lines of radioactive elements appear, and where much of the hot gas in the Universe becomes detectable. The diagnostic power of parts of the spectrum, in particular the 0.1–10 keV X-ray band, is very high and the information so obtained both supplements and complements that available from the traditional optical and radio windows. Other parts such as the gamma-ray bands are still in an exploratory discovery phase.

The high-energy band is *only* accessible to detectors placed above the atmosphere. Astronomers worldwide are now eager to continue and expand their work in high-energy astrophysics. Following on from the planned ESA missions XMM and Integral, NASA missions such as AXAF and Japan's Astro-E mission, it is reasonable to expect that the resulting large user community will demand further continuity of access. Much of the work will concentrate on the cosmic objects and be less wavelength-specific than at present. The understanding of a particular object or phenomenon will require all aspects of it to be observed. X-ray and gamma-ray observations will be as essential a part of the practice of astronomy as ground-based optical observations.

This obviously requires *continued access* to the high-energy band. In parts of the band such as X-rays we do not necessarily need some massive, novel technological development in order to make progress, just large-area telescopes with efficient detectors or spectrometers. Nevertheless, there is every prospect of exciting new technological developments in the area of high-resolution spectroscopy. In the hard X-ray and gamma-ray bands, there are new devices being developed which will transform the sensitivity and efficiency of instruments. We can confidently state that high-energy astronomy *must* be a major part of any future ESA programme.

An X-ray observatory mission with a collecting area of 10 000 cm^2 over the energy range 0.1–10 keV and high quantum efficiency detectors with an energy resolution of about 10 eV emerges as an outstanding prospect for a Cornerstone mission. It will outperform all previous and proposed missions by about an order of magnitude in several parts of 'discovery space' (spectral resolution, sensitivity, etc.) and can respond to most of the scientific goals offered by high-energy astronomy. In particular, it will enable detailed spectral imaging to be carried out in the band which is so rich in spectral information, from so many classes of object. In addition, the Cornerstone can carry a hard X-ray telescope that can enormously improve the signal-to-noise in the 10–100 keV band and so provide complementary information on hard

Image of the nuclear region of the active galaxy NGC1068, at a distance of approximately 60 million light-years, obtained with the Hubble Space Telescope's Faint Object Camera (FOC). The most likely source for the enormous energy release is a massive black-hole with a total mass of 100 million stars like the Sun. The image shows a number of hot, gaseous clouds ionised and heated by the intense radiation from the nuclear source. A wealth of new and previously unsuspected filamentary detail is also revealed in this near-nuclear gas, embedded within the diffuse emission. Study of the knots and streamers of emission will enable the physics of the mysterious nuclear region to be disentangled.



Pulse-height spectrum of the bright NW knot of the Cassiopeia-A supernova remnant from the ASCA spacecraft (from S. Holt, E.V. Gotthelf, H. Tsunemi & H. Negoro, *Publi. Astron. Soc. Japan*) sources such as accreting black holes in both galactic binaries and active galactic nuclei.

Scientific and Programmatic Considerations

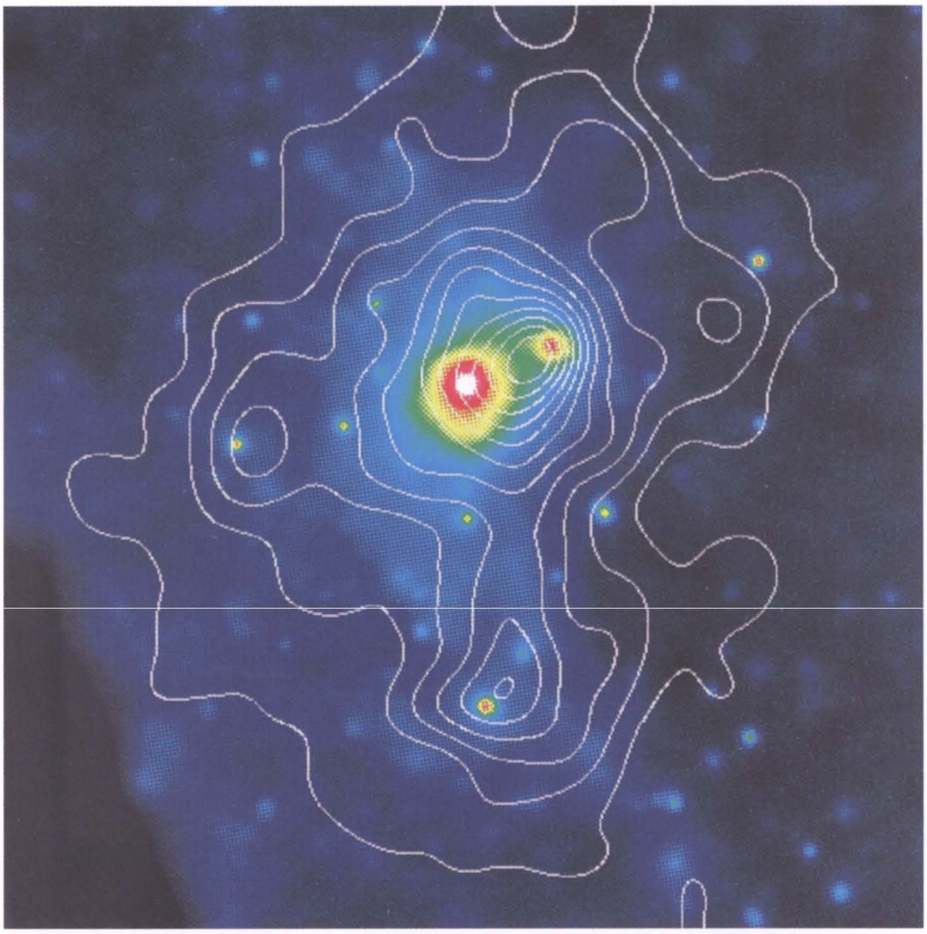
Scientific goals

The high-energy band begins in the soft X-rays where the immediate galactic hydrogen is becoming transparent to photoelectric absorption and atomic lines are common, stretches past the soft gamma-ray band where photons have the electron rest-mass energy and nuclear lines appear, and continues above the point where the photon energy exceeds the proton rest mass. Most types of cosmic objects known to exist, from dwarf stars to the most distant quasars, have now been detected in X-rays. As the band is traversed, so the kinds of objects that are observable change. Stars, the interstellar medium and supernova remnants dominate at the lowest energies. Then active galactic nuclei (AGN) are the most abundant objects, with galaxies – including the many varieties of active binaries – and clusters of galaxies being among the prominent brighter objects. Galactic binaries which are black-hole candidates (BHCs) emerge as the brightest sources in the hard X-ray band, at which point their output power peaks. At still higher energies, pulsars and the interstellar medium light up the sky.

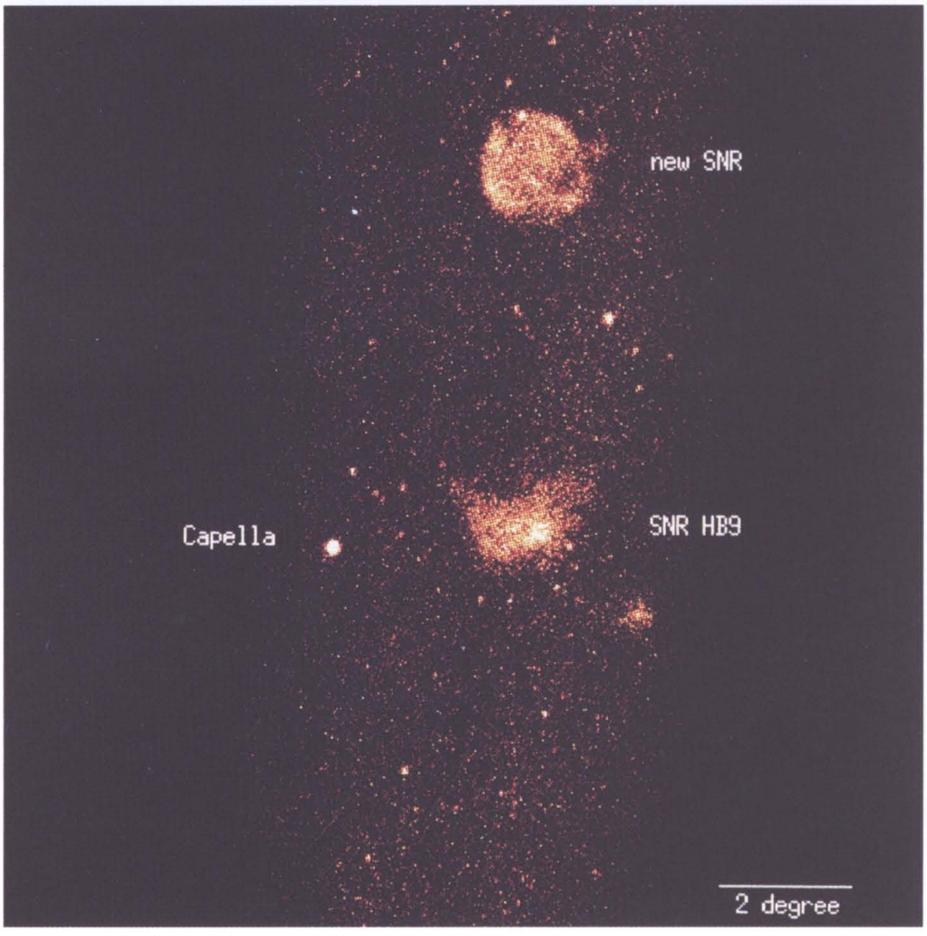
AGN do, however, continue to dominate the sky by number, and in some bands by flux, to the highest energy. The nature of the emission from AGN changes in the different wave bands. At the softest X-ray wavelengths, the emission appears to be from optically-thick matter, perhaps the gravitational energy released from an accretion disk. The X-ray and soft gamma-ray bands are dominated by a hard, variable, quasi-power-law spectrum which carries the main luminosity. The gamma-ray emission from radio-quiet objects appears to be small, but compact radio-loud objects are detectable to the highest energy. Here we see emission from their powerful jets, which are beamed towards us. We therefore probe both the direct primary radiation from the central engine as well as the kinetic power carried in the jets of the radio-loud objects.

Much of the power from AGN is reprocessed. It is only in the X-ray to soft gamma-ray bands that the primary emission from the central engine is directly observable. Only here is the rapid variability seen that is an expected characteristic of accretion onto a massive black hole, which is the most efficient energy production process known. The similarity of the spectra in both time and photon energy to those from the BHCs strongly indicates that we are dealing with the same phenomenon. Only in the situation where we can directly observe matter at a few Schwarzschild radii can we expect to test General Relativity in the strong field limit. This means either AGN and BHCs or the surfaces of neutron stars. All such observations require space-based, high-energy detectors. *High-energy astronomy provides us with access to a laboratory for the study of strong gravitational phenomena.*

In contrast, we can also consider the largest relaxed objects known, the clusters of galaxies, consisting of many hundreds of galaxies and an even larger mass of hot gas trapped in a gravitational well mostly composed of dark matter. The nature of this dark matter is another of the outstanding problems of current astronomy and it is likely to remain of great interest. If the standard inflation model of the Universe is correct, then



Rosat image of the cluster of galaxies in Virgo, obtained in the all-sky survey. Contour lines represent the galaxy distribution derived from optical surveys. The X-ray energy range is 0.4 – 2.4 keV, the field size is approx. $12° \times 12°$.



Part of the Auriga constellation. The two extended structures are supernova remnants, the gases of which glow in X-rays because of their temperatures (several million degrees). The upper remnant, previously unknown, was discovered with Rosat. The point source at the left is the well-known star Capella. The small spot to the right of the lower remnant is a cluster of hundreds of galaxies.

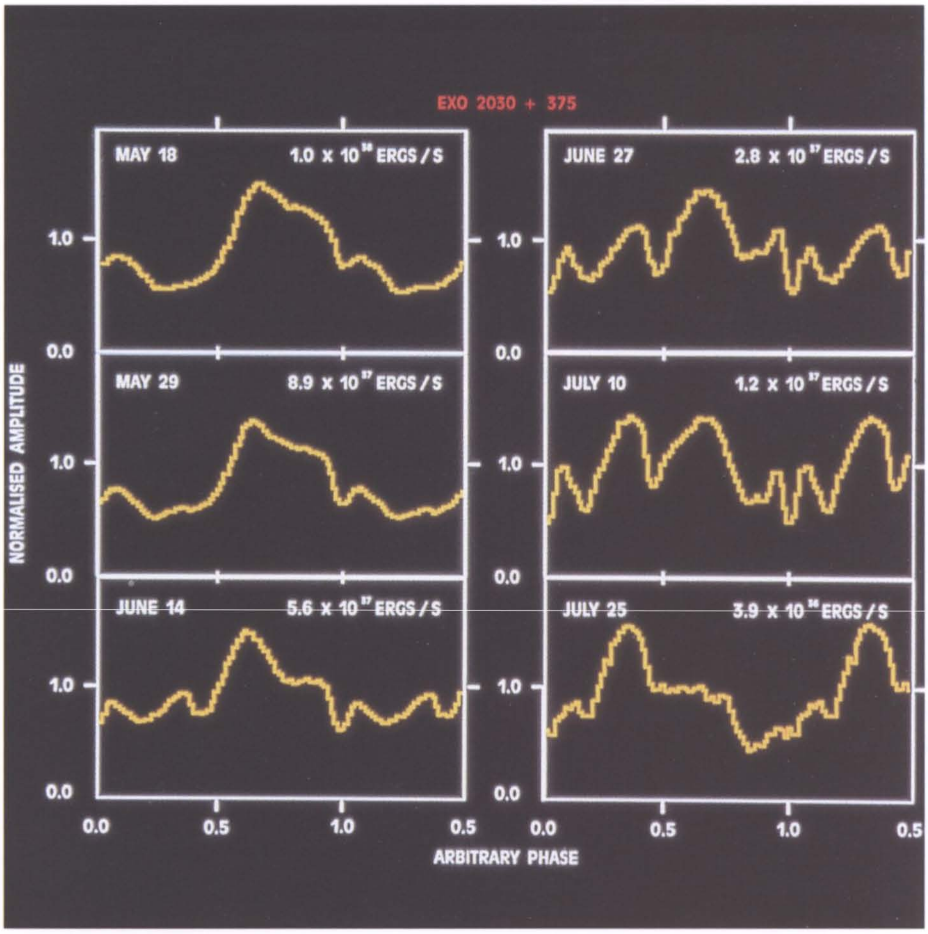
99% is dark matter. One of the best ways to map the dark matter in a cluster is to measure the radial profiles of the temperature and density of the hot gas by X-ray observations. Curiously, the fraction of the total mass of a cluster which X-ray data show to be in gas exceeds 20%, which is much larger than expected from current calculations of cosmic nucleo-synthesis and the standard inflation picture. The observations mean that in X-rays we see the dominant observable mass; galaxies are only a minor constituent of clusters. *High-energy astronomy provides us with a probe of dark matter and tests of cosmological models.*

The elements beyond helium in the atomic table have been made since the Big Bang, mostly in stars and ejected in supernovae and the winds of massive stars. Direct estimates of their abundances in elliptical galaxies and clusters of galaxies are now being made from X-ray spectra of the optically-thin plasma since many of the resonant emission lines of the elements above helium are observable in this band. We can thus begin to understand the chemical enrichment of galaxies, including in the case of clusters the mass lost by the galaxies but trapped in the deep potential well. In this band too, we can directly observe the metallicity of the ejecta of supernovae, in the gaseous remnants formed in the interstellar medium. At higher energies in the gamma-ray band, we can see the nuclear lines produced from radioactive elements: Co^{56} from very recent supernovae, Ti^{44} from ones centuries old and Al^{26} from million-year old remnants, for example. In this way, we can now map the pattern of enrichment of our galaxy and compare with models for massive star formation and supernova sites. *High-energy astronomy provides us with a direct test of the chemical history of galaxies and of the main source of enrichment, supernovae.*

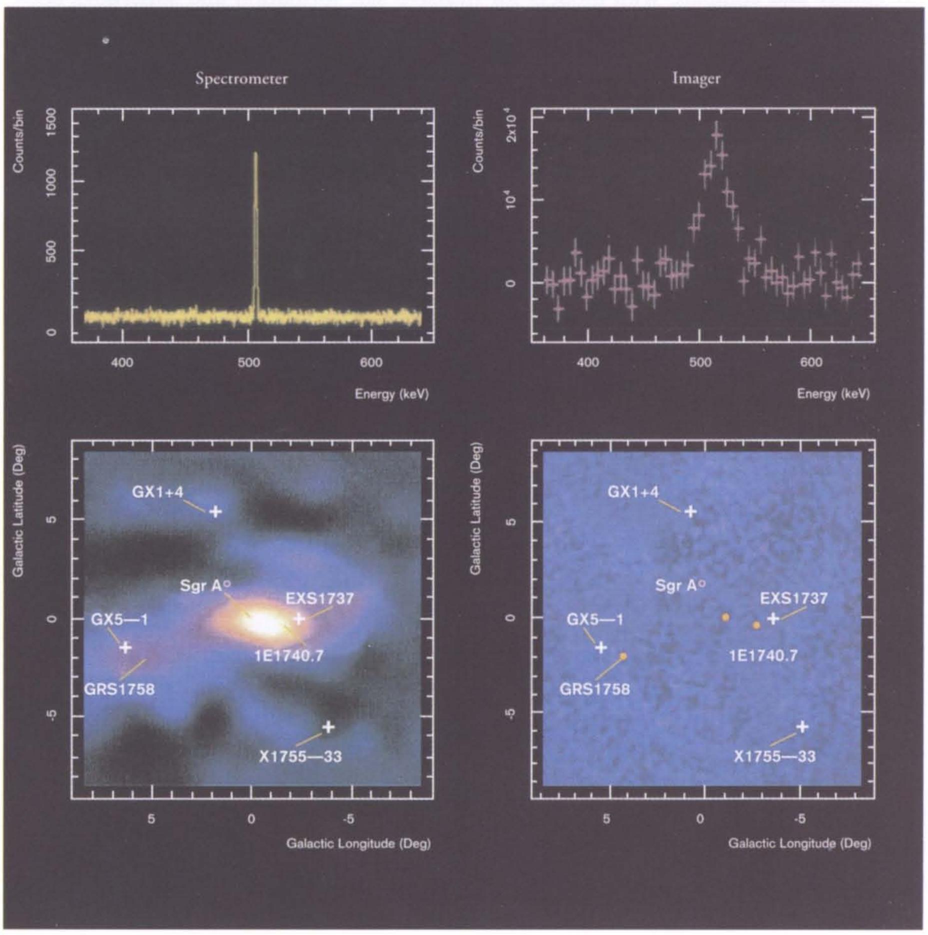
Transient phenomena are very common in high-energy astronomy: stellar flares, supernovae, and accretion in general. The time scales can range from milliseconds to years. Perhaps the most extreme variable phenomenon in this band is the gamma-ray burst. No reliable detection has yet been made of them at optical or radio wavelengths. *High-energy astronomy reveals new phenomena inaccessible at other wavelengths.*

The diagnostic power of the high-energy band is immense. The soft to hard X-ray band is extremely rich in lines. Apart from the abundance measurements mentioned above, the lines can be used to obtain temperatures, densities, ionisation states, velocities and redshifts. We can also determine departures from collisional ionisation equilibrium where relevant. When temperatures are measured from the spectra of hot objects, bolometric corrections are often small since we are observing where most of the power is emitted. Neutral gas leads also to photoelectric and resonance line absorption through which the line-of-sight medium can be studied and fluorescent lines by which the depth and geometry of gas surrounding a luminous source of X-rays becomes visible. Variability of hard emission gives us the compactness of the central source, which in turn governs whether electron–positron pair creation is taking place in the source. Regular variability is, of course, observed in both spin-down (radio) and spin-up (X-ray) pulsars and is a particularly rich source of information on size, mass and the moment of inertia of neutron stars. Cyclotron lines in the hard X-ray band also reveal the strength of the magnetic field in some accreting neutron stars.

In summary, high-energy astronomy is a rich source of information on the Universe, as well as of basic physics which is not accessible in the laboratory. It is easy to see



In May 1985, Exosat discovered a bright new transient 42 sec X-ray pulsar, EXO 2030+375. This figure shows the folded medium-energy light curves of the pulsar demonstrating the evolution in pulse profile as the luminosity of this transient system decreased by a factor of about 25 during an outburst.



Computed simulations of the possible appearance in Integral's instruments of the centremost part of the Milky Way, as seen by gamma-rays from annihilated positrons. The Spectrometer (left) will pick out the characteristic emission more cleanly, and the Imager (right) will be more exact about the locations of the sources. The labelled points are positions from which astronomers have previously reported gamma-rays or X-rays. (Images courtesy of G. Skinner, Univ. Birmingham, UK) that it will be a central source of information in the 21st Century. The science goals available in this band are diverse, as diverse as the whole of astronomy. It offers unique access to matter in strong gravitational and magnetic fields, probes dark matter and some important aspects of cosmology, and reveals the chemical enrichment of the Universe.

Proposals for a High-Energy Astrophysics Mission

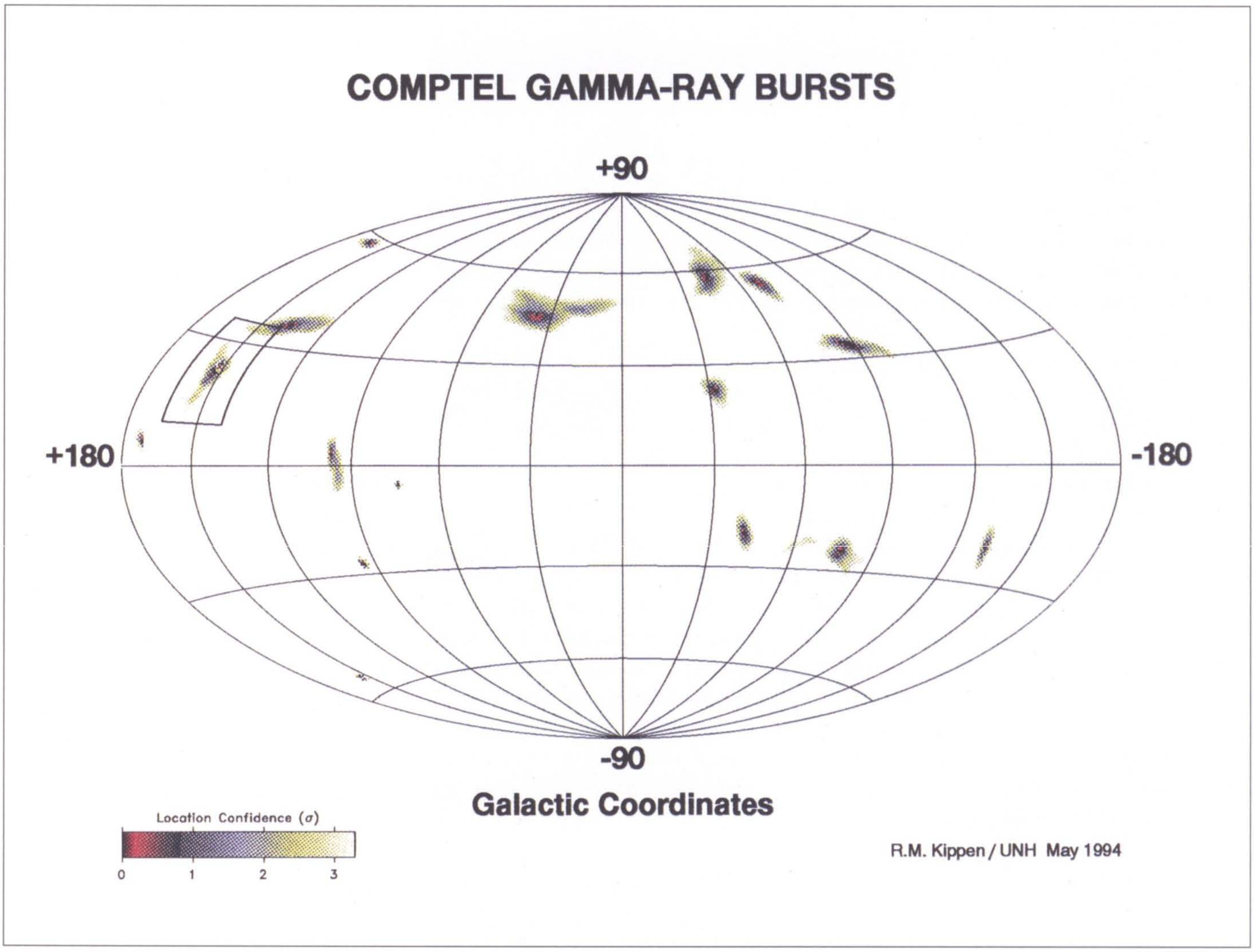
An X-ray Cornerstone

The Team has discussed the science and technology issues of high-energy astronomy and looked ahead to the decade following 2008. *It is clear from the analysis of the scientific potential of the field that ESA must give high priority to maintaining high-energy space astronomy in Europe.* Observations in the high-energy band now form a crucial element of mainstream astronomy and, in general, cannot be carried out from the ground. Although there are a number of missions yet to be launched in high-energy astronomy, including ESA's XMM and Integral missions, none are certain to be operating during the Horizon 2000 Plus era and *continued* access to high-energy telescopes will be vital to European astronomy. The completion of the new generation of optical telescopes such as the VLT and Gemini will only emphasise the need for the complementary information from the higher energy bands.

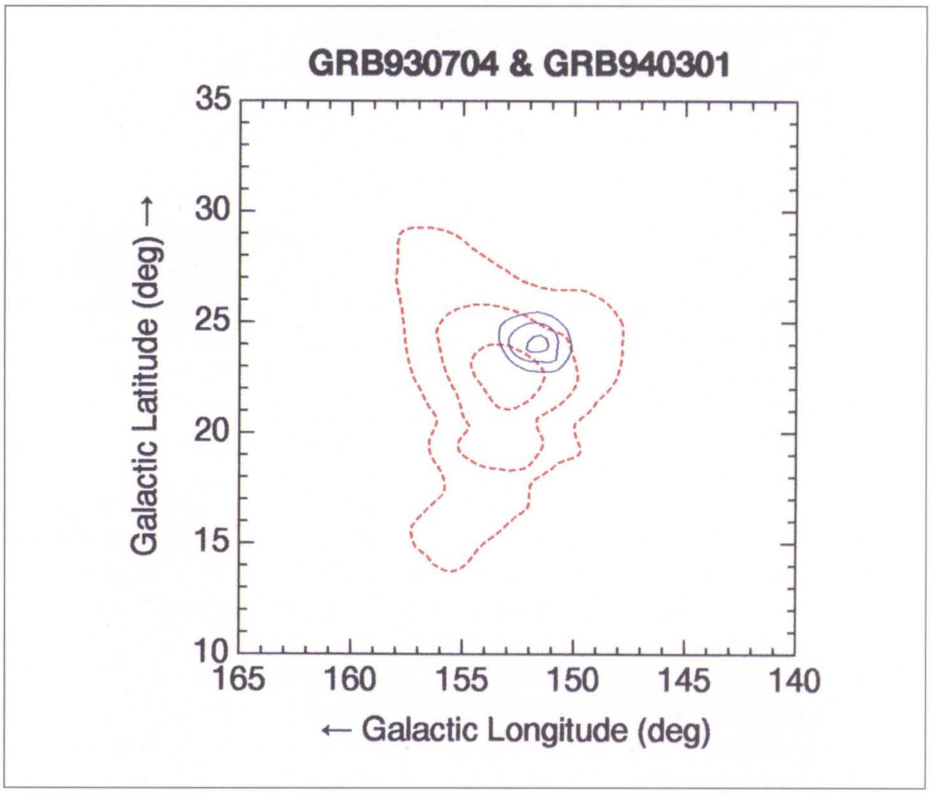
To respond to many of these goals requires a large collecting area of sensitive X-ray detectors with good spectral resolution. The Charge Coupled Device (CCD) is becoming the main detector in the field, giving a spectral resolution $E/\Delta E$ of 10−60 over the energy band 0.3−10 keV. Ten times higher spectral resolution over a wider band is expected to be delivered by other non-dispersive, imaging spectrometers now being developed, such as micro-calorimeters and Superconducting Tunnel Junctions (STJs). STJ research for astronomy is being pioneered in Europe and offers an interesting and exciting complement to a very-large-area X-ray telescope.

Such detectors must be placed at the focal plane of an X-ray telescope (or telescopes). Grazing-incidence X-ray telescopes give proven excellent performance over fields of view of a degree or more. The mirrors necessary for large collecting area would dominate the mass of any mission if high spatial resolutions are required; here we suggest that subarcmin resolution is a reasonable and achievable goal. Good spatial resolution is, of course, necessary for resolving detail in extended sources and in achieving the highest signal-to-noise for faint sources by excluding background and confusing sources, but an even more important factor for high spectral resolutions is grasp − the actual number of photons detected.

We have assumed that modest technological advances will be made over the next decade or two. We note as a *present-day* baseline, however, that a large array of lightweight, 1 arcmin half-power diameter (HPD), Serlemitsos foil telescopes (at least 10^4 cm^2) with CCDs in the focal planes would be readily achievable today (cf. ASCA which has a total area of about 300 cm^2 and 3 arcmin HPD for the CCDs). Such an instrument could study any of the 50 000 sources in the Rosat All-Sky Survey, and up to several times deeper (i.e. several 10^5 sources in the whole sky), without confusion problems (except in the inner regions of galaxies, etc.).



Distribution of gamma-ray bursts imaged by Comptel.



Two gamma-ray bursts possibly originating from the same source.

We unanimously support an X-ray Cornerstone which can respond to many of the scientific goals outlined earlier. In conceptual form, such a mission consists of a telescope with collecting area 10^4 cm^2 offering good spatial resolution combined with an imaging detector with a spectral resolution of ~ 10 eV, both operating over the band stretching from below 0.1 keV to above 10 keV. The telescope technology is envisaged as some form of lightweight grazing-incidence mirror and the main detectors as micro-calorimeters or STJs. The target payload mass would be about 3 t to be placed in High Earth Orbit with, as a baseline, a mission lifetime of at least five years. There would be a requirement for the satellite to have an arcsec pointing capability over (at any one time) a sizeable fraction of the sky. Such a payload would place Europe in the forefront of X-ray astronomy and complement the VLT and Gemini.

The adoption of the above as a Horizon 2000 Plus Cornerstone concept would provide a driver for European technological research in a number of crucial areas. The challenge would be set for the production of either a new generation of replicated mirror following-on from the XMM experience, or alternatively the exploration of more radical approaches akin to the very successful development of conical foil mirrors in the 1980s. Similarly, such a mission would provide a sustained stimulus to existing European efforts and expertise in both novel types of high-performance detector arrays and in space-qualified cryogenic technology.

In summary, we believe that the realisation of the X-ray Cornerstone concept as outlined above, although providing European astronomers, instrument scientists and technologists with some significant challenges, represents a fully achievable goal on the time scale of the Horizon 2000 Plus programme. As outlined in detail in this report, once in orbit there will be a whole host of scientific questions waiting to be addressed and a very wide European user community ready to exploit the facility to its limits.

Other Programme Elements

A major mission in the gamma-ray domain

The Team recognised that a major mission can be defined in the gamma-ray domain from about 100 keV up to 50 MeV. The astrophysics that can be studied specifically at these energies was addressed in the introduction, including the study of the radio-loud, gamma-ray bright AGN, and galactic objects like BHCs and radio pulsars. Very important is the rich field of gamma-ray spectroscopy. The envisaged major mission comprises a solid-state Compton telescope and a Laue diffraction gamma-ray telescope providing a novel third-generation gamma-ray mission. Such a mission can achieve a factor of 10 to 50 increase in sensitivity compared to existing (e.g. Comptel aboard CGRO) or planned missions (Integral). Application of innovative techniques also allows more than order-of-magnitude improvements in energy resolution and spatial resolution over part of the energy range. The Laue diffraction gamma-ray telescope could be optimised to study the 511 keV line, resolving a possible narrow line both energetically (ΔE=2 keV) and spatially (angular resolution better than 10").

Space cosmic-ray research

Electrically charged particles (elementary and complex) constitute what are traditionally called 'cosmic rays' when they reach the terrestrial environment with high energies. Despite nearly eighty years of study, their origin remains obscure. For the most part, they are stripped atomic nuclei (with some electrons, positrons and anti-protons). Because they are electrically charged, they are deflected and scattered by the magnetic field of the Galaxy and thus, except perhaps at the highest energies, their arrival directions at the Earth give no indication of their sources. However, much can and has been learnt by studying their composition and energy spectra. Conventional detector systems measure the square of the electrical charge, Z^2, and the energy of the particle, E. However, because the energy spectra are rapidly falling functions of energy, this is only possible from about 1 to 100 GeV per nucleon with detectors of moderate size.

There are at least three possible directions for future cosmic-ray research. In addition to charge resolution, the measurement of mass determines the individual isotopes and to some extent this has already been done. Using magnetic spectrometers, the charge Z can be determined and this allows study of the positron and anti-proton spectra (both important for propagation studies) and a search for heavy anti-nuclei. Finally, and most interestingly, an attempt can be made to push the measurements to higher energies. Ideally, to answer rather fundamental questions about cosmic-ray origin, the important measurement is to have charge-resolved energy spectra extending up to energies of 10^{16} eV per nucleon (i.e. through the so-called 'knee' in the all-particle spectrum at around 10^{15} eV). This is technically challenging and requires very large experiments with a geometrical factor of order 50 m^2sr to collect enough events in a few years. It is worth noting that as a byproduct such experiments can also generate significant high-energy gamma-ray astronomy data above 100 GeV.

Finally, consideration should be given to the possibility of astronomy with neutral complex particles. The recent detection of neutral interstellar gas atoms, molecules and dust particles penetrating into the Solar System shows that it is now possible to study directly a sample of the very local interstellar medium. In addition to chemical and isotopic analyses on a per-particle basis, this allows the direct determination of velocity distribution functions.

Small missions

Useful missions that are 'small' in terms of mass and spacecraft requirements are possible in the area of high-energy astronomy. For example, a small spin-stabilised satellite could be used to monitor the sky and search for new sources, bursts or new variations in known sources. The Team considers that such small missions could be an interesting addition to a high-energy astronomy programme, *provided that they do not compromise the basic Cornerstone plus Medium-Size Mission programme in this area.* Note that a small mission discovering new sources would require that there be a larger observatory in orbit to exploit such discoveries.

Topical Team 4

Ultraviolet to Radio Astronomy

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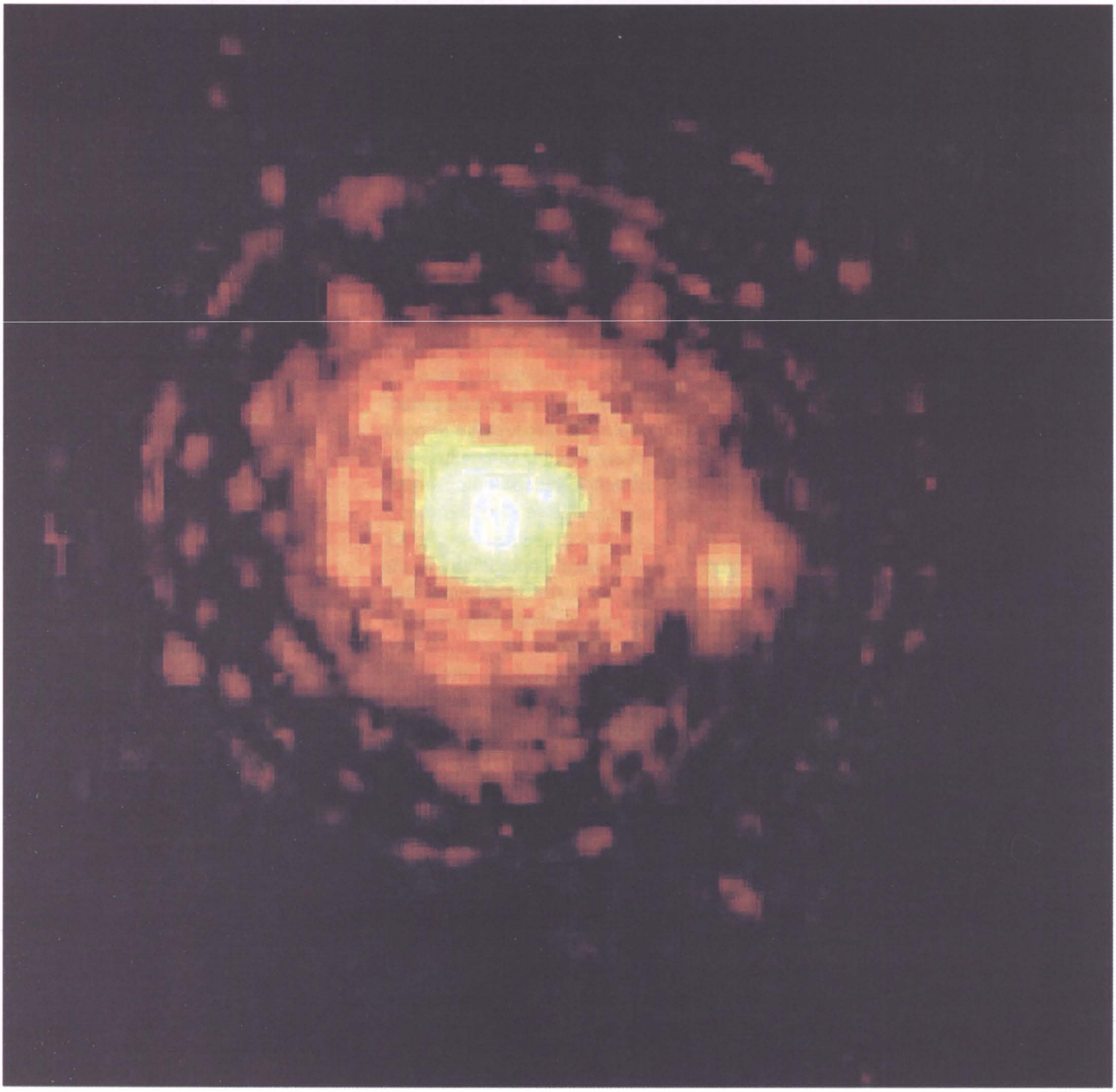
Introduction

The vast majority of our information about the Universe has come from observations between ultraviolet and radio wavelengths. The optical and radio bands alone are the oldest, most highly developed subfields in astrophysics. Many of these regions do not *require* spacecraft observations for meaningful research, yet the developments of the last decade now give a decided advantage to spaceborne telescopes even at visual wavelengths; they enable unique possibilities not realisable from the Earth's surface. Optical astronomy expects a revolution from the Hubble Space Telescope unlike any since Galileo's first use of small lenses to study the heavens. Radio astronomy with orbiting telescopes can yield angular resolution an order of magnitude greater than any other method yet devised, despite the very high resolution already obtained by whole-Earth VLBI techniques. For large portions of the ultraviolet, infrared, submillimetre and very-low-frequency radio bands, space-based observatories offer the only possibility for gathering data now recognised as vital to our understanding of the cosmos.

The challenge to Topical Team 4 was to sift through the many opportunities made possible by space and concentrate on those offering unique, necessary and compelling scientific and technical advances in these well-developed areas. It was necessary to make some strategic decisions about the course of science over the next twenty years, given the rapid advance of ground-based astronomy. Many currently interesting problems will cease to be important within the time frame of Horizon 2000 Plus. From this standpoint, a large increase in observational capability was considered a strong justification in itself for a future mission, especially if that increase would bring our capabilities to the limits imposed by nature herself, rendering future advances extremely difficult. Such observational capability will be relevant to *all* problems addressable within these wavebands, even ones currently unforeseen. In all cases, there must be clearly identifiable scientific problems that will benefit only from such an increase.

The Topical Team also recognises that benefits to science are best if also benefitting society as a whole, furthering the technical and economic conditions in the countries that provide such generous support for ESA's programmes. Consequently, the Team favoured projects requiring technology developments that appear now to hold promise for a wider application in industry and space exploration.

To this end, it is clear that improvements in resolution afforded by developing *interferometry* in space holds tremendous promise for space science in the 21st Century and can address some of the most compelling questions in all of science. The potential technological spin-offs from developing interferometric capabilities in the optical and infrared wavebands are also very great, as has traditionally been the case with all very high precision measurement techniques. The recommendations below include technical areas in which modest investments should yield rich benefits for Horizon 2000 Plus.



This picture, taken in visible light on 11 June 1994 with the Hubble Space Telescope's Faint Object Camera, resolves one of the smallest stars in our Milky Way galaxy for the first time. Named Gliese 623b, the diminutive star (right of centre) is 10 times less massive than the Sun and 60 000 times fainter. Located 25 light-years away in the Hercules constellation, Gl623b is the smaller component of a double-star system, where the separation between the two members is only twice the distance between the Earth and the Sun (approximately 300 million km). Gl623b is too dim and too close to its companion star to be seen by ground-based telescopes. The new FOC observations will allow the intrinsic brightness and mass of Gl623b to be measured, leading to a better understanding of the formation and evolution of the smallest stars currently known.

Scientific and Programmatic Considerations

Science progresses rapidly. Although broad themes such as the origin of the Universe, the creation of life, the evolutionary histories of galaxies, stars, planets, and exotic objects, remain important, many of the details change quickly and it is difficult to predict with any confidence which details will still be significant over several decades. Nevertheless, there are a few general principles that can guide planning for the long term. Developments in UV and optical astronomy will be primarily based on observations with the Hubble Space Telescope (HST) and the large ground-based telescopes currently under construction. High-resolution radio astronomy will be dominated by observations with global interferometric networks, shortly to be extended by space-based radio telescopes under development by Japan (VSOP) and Russia (Radioastron). We list four major areas which are likely to remain important as foci of infrared astronomical research well into the next century.

The early Universe

Objects are currently seen to redshifts z of order 5, a time when the Universe was no more than 10% of its present age. Many of these objects already have heavy elements, implying that star formation and nucleo-synthesis took place even earlier. For $5 \leq z \leq 10$, the rest-frame visual spectra appear in the infrared. The prominent hydrogen lines, Hα and Hβ, are shifted out of the 2 μm terrestrial window at $z \gtrsim 4$. To study objects at these very high redshifts, it is almost certain that we will need to observe them in the thermal infrared, from 2 to 20 μm.

These primeval objects consist of elliptical galaxies, globular clusters, the first quasars, and the very first stars, the so-called 'Population III'. The galaxies are very faint and likely to be several arcseconds or more in extent, easily resolved by medium-sized infrared telescopes. Under these circumstances, an infrared space telescope with only modest cooling (less than 150 K) will substantially outperform even the largest ground-based telescopes, and will do so for wavelengths longward of about 2.5 μm. For both the discovery and subsequent studies of such objects, it will be necessary to have a long-life mission. In fact, the primary characteristics desirable for such a mission are long-life, large aperture and large-format detectors. No currently planned mission has sufficient sensitivity, longevity, and sky coverage to study the thermal-infrared light from these high-redshift objects.

The nature and evolution of galaxies

Many, perhaps all, spiral galaxies were created by ongoing processes of collapse and merging starting at high redshifts and progressing to redshifts z of order 1. There is substantial evidence that we are seeing this phase of galaxy formation right now in the form of luminous galaxies at $1 \leq z \leq 4$. The rest-frame ultraviolet lines appear in the visual band, and the near-infrared bands contain the rest-frame visual spectra. These objects are already seen with 4 m-class telescopes and will be the focus of many programmes using the new generation of large ground-based telescopes.

Young galaxies, particularly those with ongoing star formation, often emit most of their luminosity in the far-infrared. This radiation is an important adjunct to the short-wavelength spectra, because its overall luminosity is essential to assessing rates of star formation and subsequent evolution.

A deep survey of the sort proposed for Edison in the thermal infrared (4 μm to 100 μm) will exceed the sensitivity of all planned missions by a factor of 1000 (ISO will not do a comparable survey) and it should reveal at least 10 million galaxies, a large number at very high redshifts.

Only a cooled space telescope will have the sensitivity and angular resolution needed to measure these young galaxies at far-infrared wavelengths. FIRST could do this in the submillimetre region, but is not presently foreseen to operate below a wavelength of 100 μm. ISO provides insufficient angular resolution to isolate colliding galaxies and will cease operations before the first large European telescope goes into operation, although ISO is likely to open the frontiers of this problem. Ideally, one wants a large aperture, long-life telescope with sufficient sensitivity to reach the confusion limit in modest integration times (hours). It would be highly desirable to extend the imaging capability of FIRST down to 50 μm to cover this important region.

The birth and evolution of stars

Stars are born enshrouded in the dust in which they were created: many of them die enshrouded in newly created dust from their own atmospheres. The study of the earliest and latest stages of stellar evolution takes place at wavelengths longer than about 10 μm because of the necessity to penetrate this dust and reveal the processes taking place within these shrouds. Star formation has been studied actively for many decades, and it is likely to be several more decades before major questions are resolved.

Supernovae emit the main fraction of their total energy in the near- and far-infrared. Observation of fine-structure lines from iron and cobalt makes a detailed study of the radioactive isotopes in distant supernovae possible, in the same way as has been demonstrated for SN 1987A. The formation of dust in supernovae can best be studied in the infrared.

Young and evolved stars, even relatively distant ones, tend to be bright sources of infrared radiation at all wavelengths. Great sensitivity is not required to detect and study these objects. However, the angular scales needed to separate out various components within protostellar and giant-star envelopes are small; indeed, it will be some time before infrared telescopes achieve the angular resolution needed to discriminate important structures at thermal and far-infrared wavelengths. The spectra of these envelopes, especially very high-resolution spectra, may hold the key to the dynamics governing cloud collapse, the formation of individual stars and disks, and the production of dust particles in stellar envelopes.

This subject will almost certainly continue to be important even twenty years hence. What is needed is a long-life mission with the highest possible angular resolution and reasonably good sensitivity for spectra. A large-aperture telescope, possibly a binocular or an orbiting interferometer, would be well-suited to the requirements of the 21st Century.

The creation of planetary systems and life

Our current prejudices are that life needs planets for its existence, and these planets should have characteristic temperatures and chemistry not dissimilar from Earth. Although we admit ignorance about extra-terrestrial life, it is reasonable to take the only known example as a model for what we will find around nearby stars. The discovery and study of extra-solar planetary systems is central both to the search for extra-terrestrial life and to the understanding of our own origins.

There are a number of ongoing searches for extra-solar planets from terrestrial observatories and several proposed space missions with the same objectives. Some of these projects have the sensitivity to detect the signatures of Jupiter-like planets orbiting nearby stars, and we fully expect other planetary systems to be discovered by one of these methods during the next twenty years. Furthermore, the study of nascent planetary systems via observations of the pre-planetary disks is currently both popular and productive, but will not be completed during the next two decades.

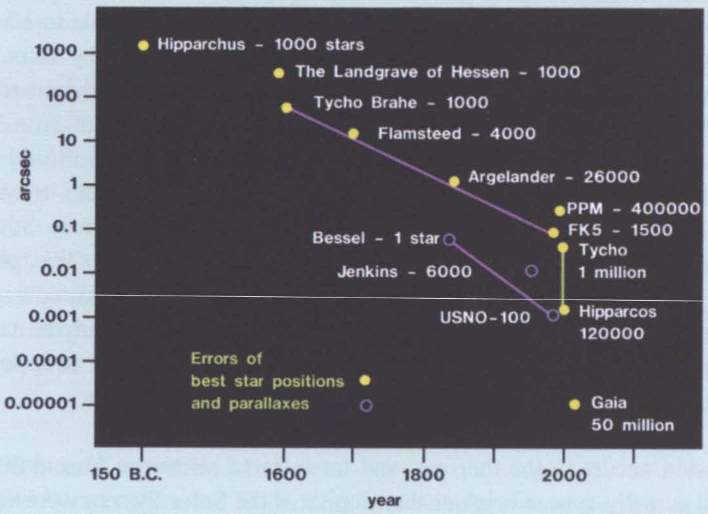
Planetary emission occurs in the thermal- and far-infrared. Between 12 and 20 μm, the Earth would actually appear brighter than Jupiter if the Solar System were viewed from a distance. The contrast between Earth or Jupiter and the Sun at these wavelengths is several orders of magnitude less than the corresponding contrast in the visible (for $\lambda \leq 10$ μm, the Sun is approximately 10^9 times brighter than Jupiter). With the very stable point-spread functions available in space and the reduced contrast, it should be possible to detect Jupiter-like planets around nearby stars. This can be accomplished with very modest cooling of a large-aperture or interferometric telescope with a sufficiently long life for long integration times and the characterisation of point-spread functions leading to 'super-resolution'.

To study forming planetary systems, longer wavelengths will be equally important. There are enough candidates in nearby star-forming regions that we can state with assurance that very high sensitivity is not essential; the candidates are bright enough to be revealed by IRAS; the brightest ones might be studied with SOFIA. What is most important is good angular and spectral resolution. ISO will make great progress in this area, but it is unlikely to settle the most important questions during its limited lifetime.

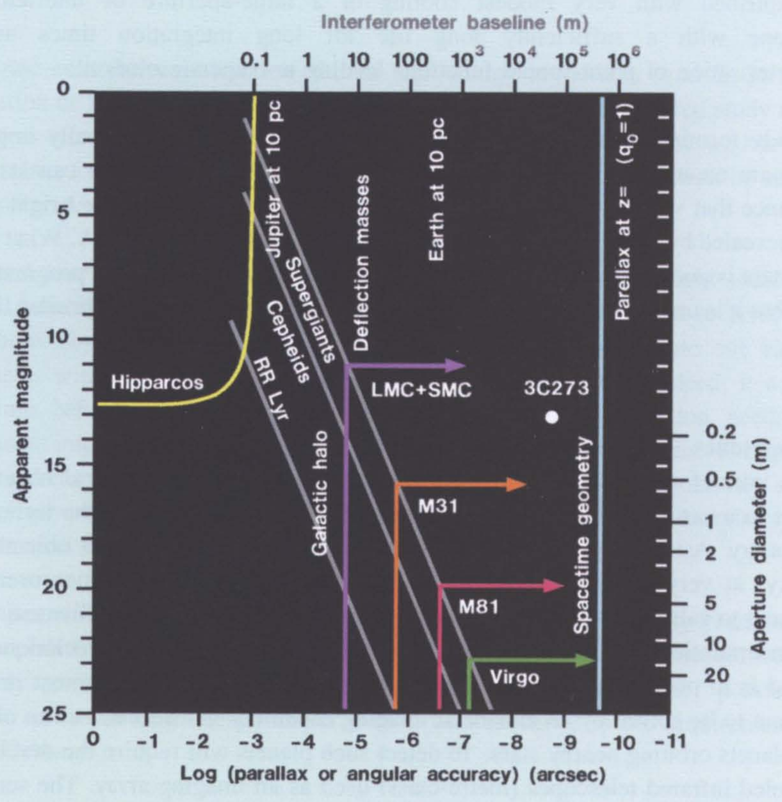
Proposals for Space Astronomy Missions

Cornerstones

Space interferometry has two complementary benefits for astronomical observation. One is to measure the positions of objects relative to one another – the technique of *astrometry*. Astrometry allows the direct measurement of distance to objects in the Galaxy; at very high precision, it should enable the geometrical measurement of distances to galaxies at modest distance, establishing the scale of the Universe without the uncertainties that have plagued all other indirect measurement techniques. The second is to provide high-resolution *images* of celestial targets. The most important problem to be solved by an enhanced imaging capability is direct detection of Earth-like planets orbiting nearby stars. To detect such planets will require the development of cooled infrared telescopes (metre-class) used as an imaging array. The sensitivity of such cooled telescopes will itself allow the problems of galaxy formation in the early Universe and a host of other outstanding astronomical issues to be addressed.



Errors in star positions in the most accurate catalogues. Tycho Brahe achieved a jump in accuracy through the first 'big science' in history. After four centuries with more gradual improvement, another much larger jump was obtained by Hipparcos, with the Hipparcos and Tycho Catalogues containing a total of one million stars. (Courtesy of E. Hoeg, Univ. of Copenhagen Observatory)



Overview of objects and phenomena that can be studied by optical astrometry. The increase in precision by a factor 100 enlarges the volume accessible for studies by a factor of 10^6 compared to Hipparcos. This allows one to make accurate distance measurements to 10 kpc and provides the possibility to obtain accurately the distances and proper motions for most stars in the bulge and the halo of the Galaxy, allowing study of galactic dynamics and rare and exotic objects such as Cepheids, RR-Lyrae stars, supergiants and planetary nebulae.

The basic instrumental concepts are:

- A high-precision astrometric interferometer capable of *absolute* positional accuracies of order 10 μs of arc (μas) or better with a sensitivity limit down to at least 17th visual magnitude. This concept represents an increase of two orders of magnitude in positional accuracy with respect to Hipparcos, enabling entirely new problems to be addressed with astrometric measurements. It most probably requires two orthogonal interferometers, each with apertures of order 1 m and baselines of order 10 m.
- A set of cooled telescopes with sufficient aperture and interferometric baseline to allow direct imaging of planets around nearby stars at thermal-infrared wavelengths ($\sim$5 to 30 μm). The apertures required are of order 1 m or larger with baselines of order 10 m. At least two telescopes are required for interferometry.

Precision Astrometric Interferometer

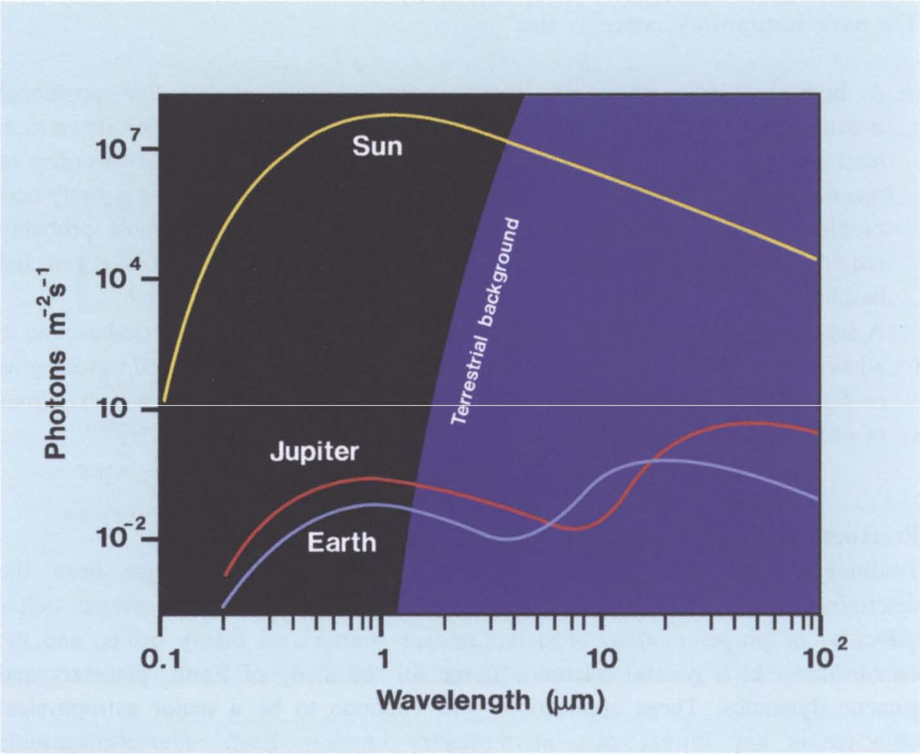
Traditionally the most important applications of astrometry have been the determination of stellar distances by trigonometric parallax, of transverse stellar velocities by proper motion, of stellar masses from visual binary orbits, and the establishment of a general reference frame for the study of Earth, planetary and galactic dynamics. These applications will continue to be a major astrophysical objective of any future space-interferometry mission. Each order-of-magnitude improvement in angular accuracy and sensitivity is also likely to bring new classes of phenomena into the parameter region of study, just as the astrometric detection of the deflection of light rays by the Sun in 1919 was important support for General Relativity. With angular measurements at the microsecond of arc level, the same phenomenon could be routinely utilised for the determination of stellar masses. This and a few other new applications of astrometry can be predicted with confidence by extrapolation of current knowledge, but the important ones may result from some presently unforeseen combination of high-accuracy measurements and advanced theory.

Astrometric measurements have two quite distinct domains: *differential* or *small-field* astrometry, which measures the positional difference between two nearby objects, and *global* astrometry which establishes positional differences between objects widely separated in the sky. It is global astrometry that is enabled by spacecraft.

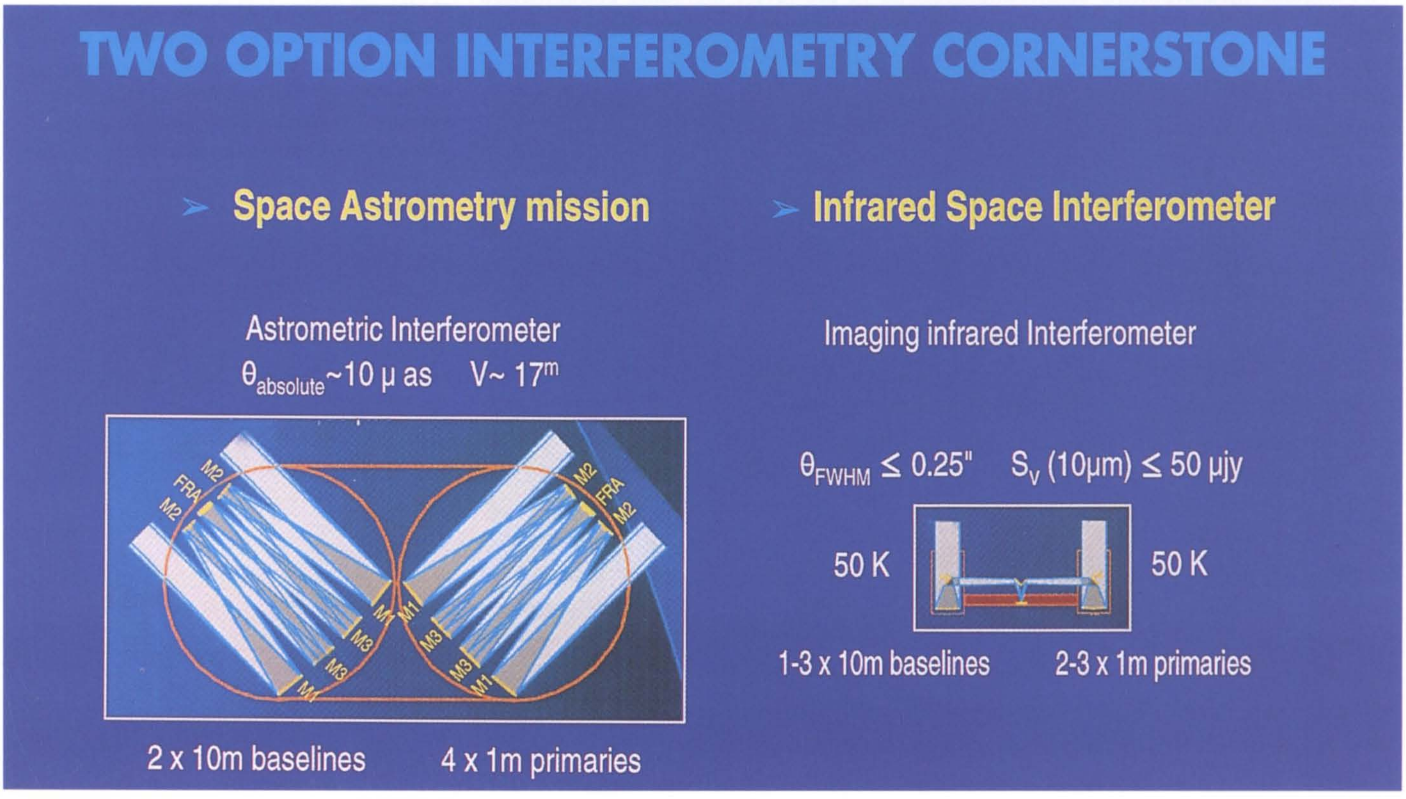
Global astrometry is essential to establish accurate kinematic frames of reference at the submillisecond of arc level. At this level, uncertainties in the distances to objects close together on the sky pose a fundamental limitation for the small-field technique. The next generation of astrometric missions must necessarily concentrate on the science made possible through global position measurements.

Baselines of a few metres and metre-class apertures are sufficient to achieve the accuracy level of 10 μas proposed for Horizon 2000 Plus; indeed, there are a number of concepts for free-flying instruments that explore this region. A further improvement by another order of magnitude brings entirely new fields into study, such as the extragalactic distance scale and kinematics. Such increases in accuracy are likely to come about via the steady development of technology over the next several decades, including lightweight telescope mirrors and precision interferometric references. The

Photon fluxes at low spectral resolution and diffraction-limited angular resolution of the Sun, Jupiter and Earth viewed at a distance of 5 pc, a median distance for our nearest neighbours. The photon detection rates for the planets themselves are small; apertures of the order of 1 m or more are required just to gather light for imaging. The contrast between the planets and the Sun is the most difficult problem for imaging and the reason why an interferometer with a baseline of the order of 10 m is required.



There are two options for the interferometry Cornerstone. One is a global astrometric mission based on two interferometers at right angles, consisting of two 1 m apertures with a baseline of 10 m. The other option is an infrared interferometer with a 10 m baseline and two apertures. The need for sensitivity requires that both the focal-plane arrays and the telescopes be cooled to 50 K by passive means to ensure long lifetimes.



next mission is an important step to develop this fundamental field and will be a necessary step to full-scale interferometric imaging missions at visible or infrared wavelengths.

Infrared Imaging Interferometer

Infrared astronomy has recently become technologically mature and is beginning to address the entire suite of important astronomical problems. It is widely regarded as one of the most dynamic of the new areas in astronomy and is expected to remain important for the coming decades. To a large extent, the advances have come about from enormous improvements in infrared detectors, mainly as the result of massive investment by the United States' military, but significant progress also required airborne and orbiting telescopes, since the Earth's atmosphere is opaque over a certain range of wavelengths. The atmosphere is the source of significant infrared radiation in its own right, even at those wavelengths to which it is almost transparent. Over the entire spectrum between 2 and 200 μm, the Earth's atmosphere sets the natural limits to wavelength accessibility, sensitivity, angular and time resolution. With a few exceptions, these limitations cannot be significantly changed using ground-based telescopes with any technology forseeable in the next twenty years. It is therefore important to plan for space-based observatories that operate in the infrared.

Scientifically, there are two strong drivers for improved capabilities at thermal-infrared wavelengths (2–30 μm). First, the region between about 10 and 25 μm is potentially the best place to directly detect planets around nearby stars; the wavelengths at which the relative contrast between planetary and stellar radiation is dramatically reduced yet the angular resolution for imaging is still relatively good.

The second driver is the study of the first generation of stars and galaxies in the early Universe. Objects with redshifts exceeding 5 have their rest-frame visible spectra shifted into the thermal-infrared; ground-based facilities have orders of magnitude less sensitivity than a cooled, space telescope for studying these faint, extended galaxies.

Technical developments

Each mission concept can benefit greatly from technology development in specific areas. In some cases, the development is necessary to ensure that the mission will achieve its stated goals. In other cases, advanced technology is likely to substantially lower the mission costs and make possible future missions going well beyond those recommended. The areas of technology development deemed most germane to these missions are:

1. Materials development for lightweight optical-quality mirrors several metres in size: Use of new materials could substantially reduce the weight and cost and increase the reliability of large, precision structures and have wider applications in industry and science.
2. Active optical correctors: Along with material development, active optics to correct residual errors in telescope reflectors may provide an inexpensive and low-risk means of correcting remaining large-scale errors in large optical systems. Such systems could provide large pay-offs for almost all future missions.

3. Large-scale non-cryogenic cooling systems: Passive cooling is an important adjunct to mechanical cooling systems; it can be developed primarily by research into surface emissivities of materials (especially uniformity over large areas) and sophisticated engineering models of, for example, transients induced by changing orientation. New mechanical cooling concepts can hold rich dividends for future space missions.
4. Standard platforms for pointed experiments: The cost of most astronomy missions is driven first and foremost by the stringent pointing requirements. Any low-cost means of providing pointing will be extremely helpful in driving down the cost of astronomical missions.

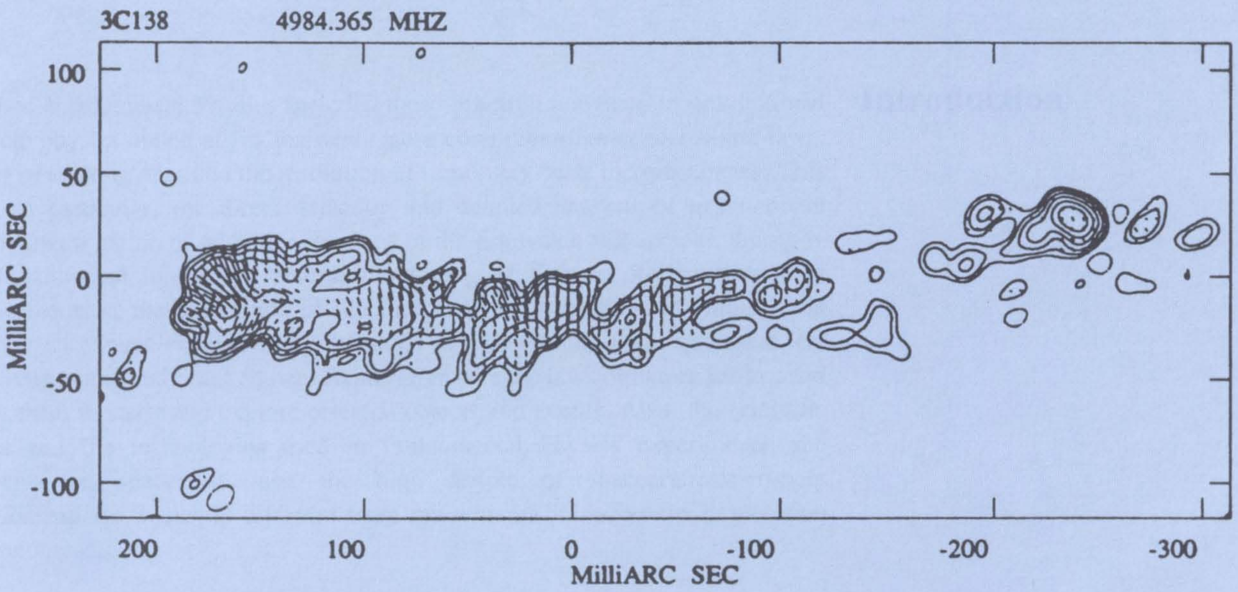
Medium missions

Several areas will clearly benefit from medium missions in the Horizon 2000 Plus time frame. Some further current areas of interest, the ultraviolet and optical observations presently being made with IUE and HST, for example, while some pioneer entirely new fields, such as asteroseismology. Many of the medium missions currently being looked at could be launched within the current Horizon 2000 Programme:

1. *Ultraviolet/visual astronomy:* An advanced camera for HST, covering especially the ultraviolet wavelengths, is of highest priority for a large fraction of the European scientific community. It would secure a future involvement for Europe in HST, assuming HST will still be operational in twenty years' time. A mission similar to the proposed EUVSPEC to explore the extreme ultraviolet is another example of a good medium mission.
2. *Asteroseismology:* Detection of seismic waves on the Sun has revolutionised solar astronomy; similar studies of other stars can be considered a fundamental new area for their understanding. STARS is the only example of a mission to pioneer seismic studies of other stars in the same manner as the Sun is now studied.
3. *Very-low-frequency radio observations:* Radio-frequency observations down to ~500 kHz are possible in principle, but these very low frequencies are as yet unexplored. It is possible that the exploratory observations might be made with inexpensive, lightweight radio receivers perhaps piggy-backed on other spacecraft.
4. *Cosmic background radiation:* This will continue to be among the most fundamental of extragalactic radiations, and its characterisation, particularly its structure, is likely to be important into the next century.
5. *Space VLBI radioastronomy:* An orbiting radio telescope in combination with ground-based radio telescopes would allow higher angular resolutions and better quality images than are possible with ground-based radio astronomy to be achieved.

Small missions

The principle advantage of a small mission is the possibility of proposing, constructing and flying an experiment more quickly compared with the rather extended periods currently needed to accomplish medium and large projects. It is therefore almost impossible to consider small missions at this time which would be flown at the time of Horizon 2000 Plus. In any case, the Topical Team considered specific recommendations to be unwise, but it is possible to comment on the overall desirability of a small mission programme from a somewhat more general standpoint.



Tapered VLBI image of 3C138 at 5 GHz restored with a 10 mas circular beam. The polarised emission is dominated by the main jet, about 1 kpc in length. Contour levels are −5, 5, 10, 20, 40, 80, 150 times the noise of 0.7 mJy/beam in the Stokes-I image. The peak flux density is 190.4 mJy/beam (core region). The superimposed E-vectors have lengths proportional to the polarised flux density. (D. Dallacasa et al. 1995, *Astron. & Astrophys.*, in press)

To justify the expense of even small space missions, these must provide a contribution to astrophysics that is both unique and fundamental. Almost all regions of the electromagnetic spectrum have now been explored however, and the room for making fundamental discoveries with small missions is considerably less than was the case one or two decades ago. At the present time, there are few missions that could be justified in terms of the cost involved compared with the expenditure of similar amounts for ground-based astronomy.

A principle cost driver for new missions is the need for platforms pointed with accuracies often greatly exceeding those of much larger ground-based facilities. The minimum cost of pointing platforms forces most astrophysics missions into the medium- or large-size category. If it were possible to develop an inexpensive pointing system, this objection would be overcome, and many missions currently in the medium category might well become small missions. Yet even with such a platform, ESA would have to provide launch vehicles for small missions at low enough cost to really enable the small mission category. At present, there is no obvious way to carry out a small mission within the rules governing ESA's procurement and launch of spacecraft.

Although the Topical Team does not feel able to recommend a specific programme for small missions, it would emphasise that ESA should always be willing to consider proposals for small missions as and when they arise.

Topical Team 5

Fundamental Physics

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Introduction

The field of Fundamental Physics includes those research activities in gravitational and particle physics aimed at finding new, more comprehensive concepts and laws, the testing of existing ones and the resolution of some very basic inconsistencies. This includes, in particular, the direct detection and detailed analysis of gravitational waves, the investigation of possible violations of the Equivalence Principle, the study of new hypothetical long-range forces, the testing of General Relativity and its alternative theories, the unification of the fundamental interactions of Nature, and particle physics. Space experiments are crucial in investigating these questions. By contrast, Astronomy and Solar System Exploration take the laws of nature for granted and apply them to study and explore celestial objects and events. Also, the scientific objectives and the technologies used in Fundamental Physics experiments, the requirements on spacecraft, and the high degree of spacecraft/experiment interrelationship are distinctly different from missions in Solar-System Exploration and Astronomy.

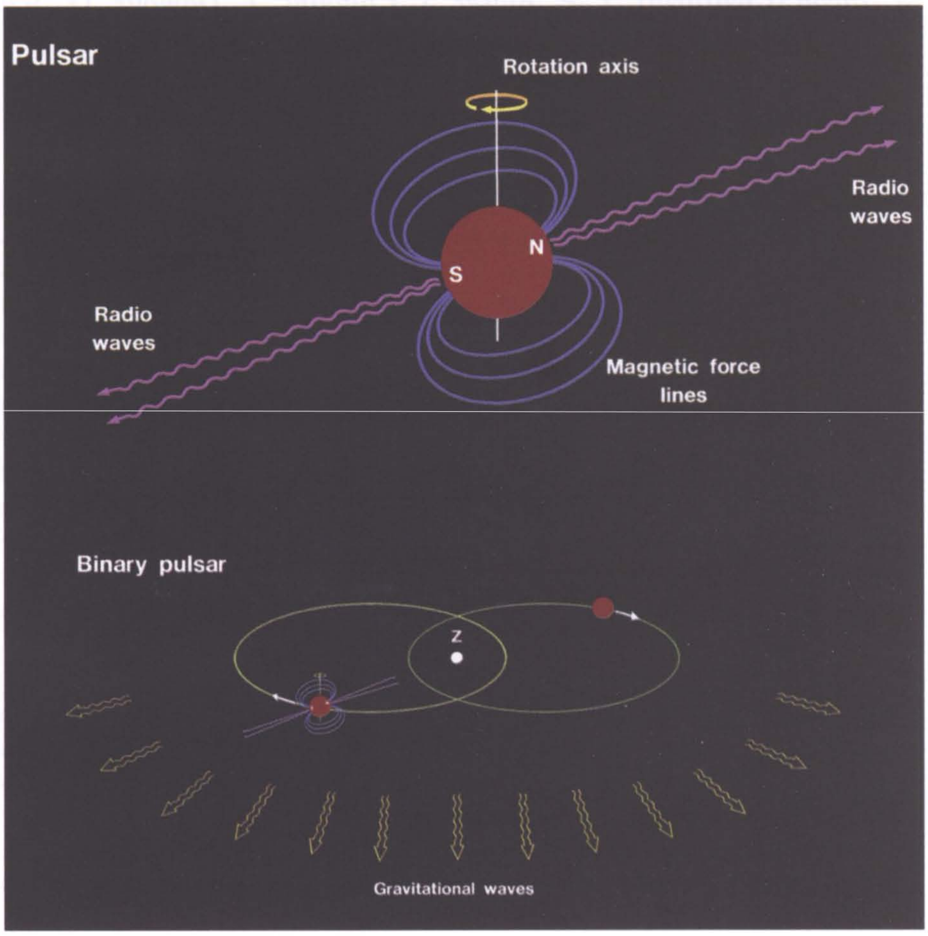
Some crucial Fundamental Physics experiments can be carried out in space to a precision many orders of magnitude higher than on the ground because of the much more benign environment. Interest in a space project in Fundamental Physics has existed in the space science community since the early days of ESRO, but it is only now that the technology has matured sufficiently for space missions in this new field. This has manifested itself in a strongly increasing number of mission proposals to ESA in Fundamental Physics over the last few years.

Scientific and Programmatic Considerations

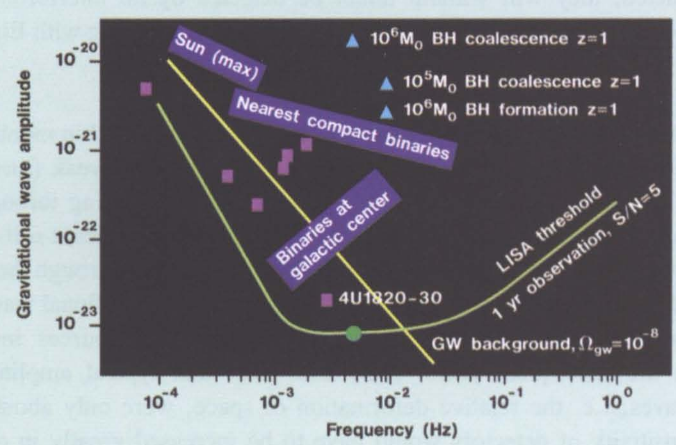
Selection of a Cornerstone project

Although gravitational waves are an intrinsic part of Einstein's theory of gravity, they have not yet been directly detected. If these gravitational waves exist and have the properties predicted, they will without doubt be detected by an interferometer in space. If they do not exist, there would be something seriously wrong with Einstein's theory.

Einstein himself laid the foundations of gravitational wave theory within months after his final formulation of General Relativity, restricting himself to weak (linearised) waves emitted by bodies with negligible self-gravity and propagating through flat, empty spacetime. However, it was clear that for sources with significant self-gravity (e.g. binary systems), the linearised theory was invalid. It was only through the efforts of H. Bondi and others, in the early 1960s, that the theory of gravitational waves was put on a sound basis. As the understanding of astrophysical sources improved dramatically in the subsequent years, it became clear that typical amplitudes of gravitational waves, i.e. the relative deformation of space, were only about 10^{-21} and that the sensitivity of detectors would have to be increased greatly in order to detect them.



The radio waves from a pulsar are emitted in two bunches which sweep across space at the same rate as the pulsar rotates (top). From a binary pulsar, gravitational waves are also emitted (bottom). (Illustration by H. Nilsson)



Strength of various sources and the sensitivity curve for LISA, for an integration time of 1 year and S/N=5 (an appropriate threshold) with an allowance for averaging over source directions. Squares denote known galactic binary sources, including 4U1820-30. The circle lying on the sensitivity curve is the strongest expected binary black-hole system in the Virgo cluster. Triangles denote massive black-hole sources in a galaxy at a redshift of 1. Also shown are the present upper limits on disturbances produced by g-modes of the Sun, and expected populations of compact star binaries in the galaxy. A cosmological background of gravitational waves with 10^{-8} of the closure density would appear as extra noise in the data, at the level shown by the yellow line.

Up to now, our understanding of the Universe has been limited as it is based only on observations of electromagnetic waves, ranging from radio to gamma-ray wavelengths. Electromagnetic waves come almost entirely from weak-gravity, low-velocity regions. By contrast, gravitational waves are emitted most strongly in regions of spacetime where gravity is relativistic and where the velocities of bulk motion are close to the speed of light. Cosmic gravitational waves should be emitted by, and carry detailed information about, coherent bulk motions of matter (e.g. collapsing stellar cores) or coherent vibrations of spacetime curvature (e.g. black holes), whereas cosmic electromagnetic waves, due to their much shorter wavelengths, are usually incoherent superpositions of emission from individual atoms, molecules and charged particles. Gravitational waves pass through surrounding matter with impunity, in contrast to electromagnetic waves, which are easily absorbed and scattered, and even in contrast to neutrinos which, although they easily penetrate normal matter, should scatter thousands of times while leaving the core of a supernova. These differences make it likely that, if cosmic gravitational waves can be detected and studied, they will revolutionise our view of the Universe.

A gravitational wave detector in space will investigate the fundamental nature of gravity by providing access to the behaviour of gravity in a strong-field regime (black-hole/black-hole coalescenses), where the ultimate test of the validity of General Relativity could be conducted or deviations from it could be revealed.

The direct detection of gravitational waves is particularly exciting because we are relatively sure that they exist – the decay in the orbit of the Taylor-Hulse binary pulsar (PSR 1913+16) due to gravitational radiative damping agrees to 0.35% with the predictions of General Relativity – but they have not yet been observed as such. Their direct detection and analysis would doubtless be one of the most important discoveries in physics in the coming decades. Consequently, Topical Team 5 strongly and unanimously recommended a gravitational-wave project in space as a Cornerstone in Fundamental Physics.

Sources of gravitational waves

Non-spherically symmetric accelerations of mass cause gravitational waves. The time dependence of the quadrupole moment is the main term. Thus a binary system will always radiate. While a perfectly symmetrical collapse of a supernova will produce no waves, a non-spherically-symmetric one will emit gravitational radiation. The types of gravitational waves are bursts, periodic waves, and stochastic backgrounds due to compact binaries, primordial waves and cosmic strings. Bursts due to the coalescence of neutron-star binaries can be observed during the final stages (minutes and seconds) of coalescence. Consequently, the frequency is high ($1-10^4$ Hz) and both the frequency and amplitude increase quickly with time. Such bursts, and those due to supernovae, are most suitable for detection by ground-based interferometers. By contrast, the frequencies of periodic waves from large numbers of galactic binaries with frequencies of 10^{-2} to 10^{-4} Hz are stable over hundreds to millions of years, and they are only observable in space.

Galactic binaries

The sources that can be detected in our Galaxy by an interferometer in space include a wide variety of close binaries. White-dwarf binaries are difficult to observe optically, but should be rather common. At least, several hundred should be detectable. There may even be so many that a confusion-limited background could be produced, which would make it more difficult to detect other very weak sources. Neutron star binaries should be detectable throughout the galaxy with a high signal-to-noise ratio and several hundred or more should be found. Some black-hole binaries may exist and should be easily detectable, perhaps even in the Virgo cluster.

In addition to the above types of binaries, it seems likely that signals from at least a few cataclysmic variables, contact binaries, and X-ray binaries will be detected. Some known systems are indicated in Figure 1, especially the X-ray binary 4U1820-30, which is so well studied that it is one of the most reliable sources in our catalogue.

Massive black holes
There is now strong evidence for the existence of black holes in the nuclei of galaxies. There is also much evidence for mergers of galaxies. Consequently, the formation and eventual coalescence of black-hole binaries should be expected. Coalescence rates of several per year in the Universe may be anticipated, with black holes of between 10^3 and 10^7 solar masses radiating in the observable frequency range. Both the quasi-periodic gravitational-wave signals before coalescence and the terminal signals near coalescence should be detectable with high signal-to-noise ratio wherever the event occurred in the Universe.

By comparing the detailed wave forms of observed gravitational wave bursts with those predicted for the coalescence of black-hole binaries, one could verify that certain bursts are indeed produced by black-hole coalescences and, as a consequence, verify unequivocally the existence of black holes and General Relativity's predictions of their behaviour in highly dynamical circumstances. The analysis of such events is probably the only way to test Einstein's theory of General Relativity up to the high-field regime.

Primordial gravitational waves
Primordial gravitational waves originated much closer in time to the Big Bang than the 3 K microwave background. Their study would give us an unprecedented glimpse into the earliest moments of the Big Bang. Photons coming from the Big Bang last scattered off matter at a cosmological redshift $z \sim 1000$, when the Universe was roughly one million years old; and neutrinos last scattered at $z \sim 10^{10}$, when it was about 0.1 s old. An order-of-magnitude calculation shows that gravitons, by contrast, last scattered at roughly the Planck time, i.e. during the first 10^{-43} s when spacetime was quantised and the laws of physics were exceedingly different from today. Thus, in studying primordial gravitational waves, one can usually ignore their subsequent interactions with matter.

Not so for their subsequent interactions with the background spacetime curvature of the Universe. As the primordial perturbations that give rise to present-day waves 'come inside the cosmological horizon' – and also before they enter the horizon – they can be parametrically amplified by their interaction with the dynamical background spacetime curvature; in other words, they can trigger further graviton creation. In this way, exceedingly small initial fluctuations can be amplified into an interestingly strong stochastic background today. Just how much stochastic background is produced depends crucially on ill-understood aspects of the initial singularity and on the equation-of-state-dependent and vacuum-dependent expansion rate in the very early Universe. It is interesting that if the electro-weak phase transition produced a period of inflation, the gravitational wave signature of this event would peak in the frequency range accessible from space.

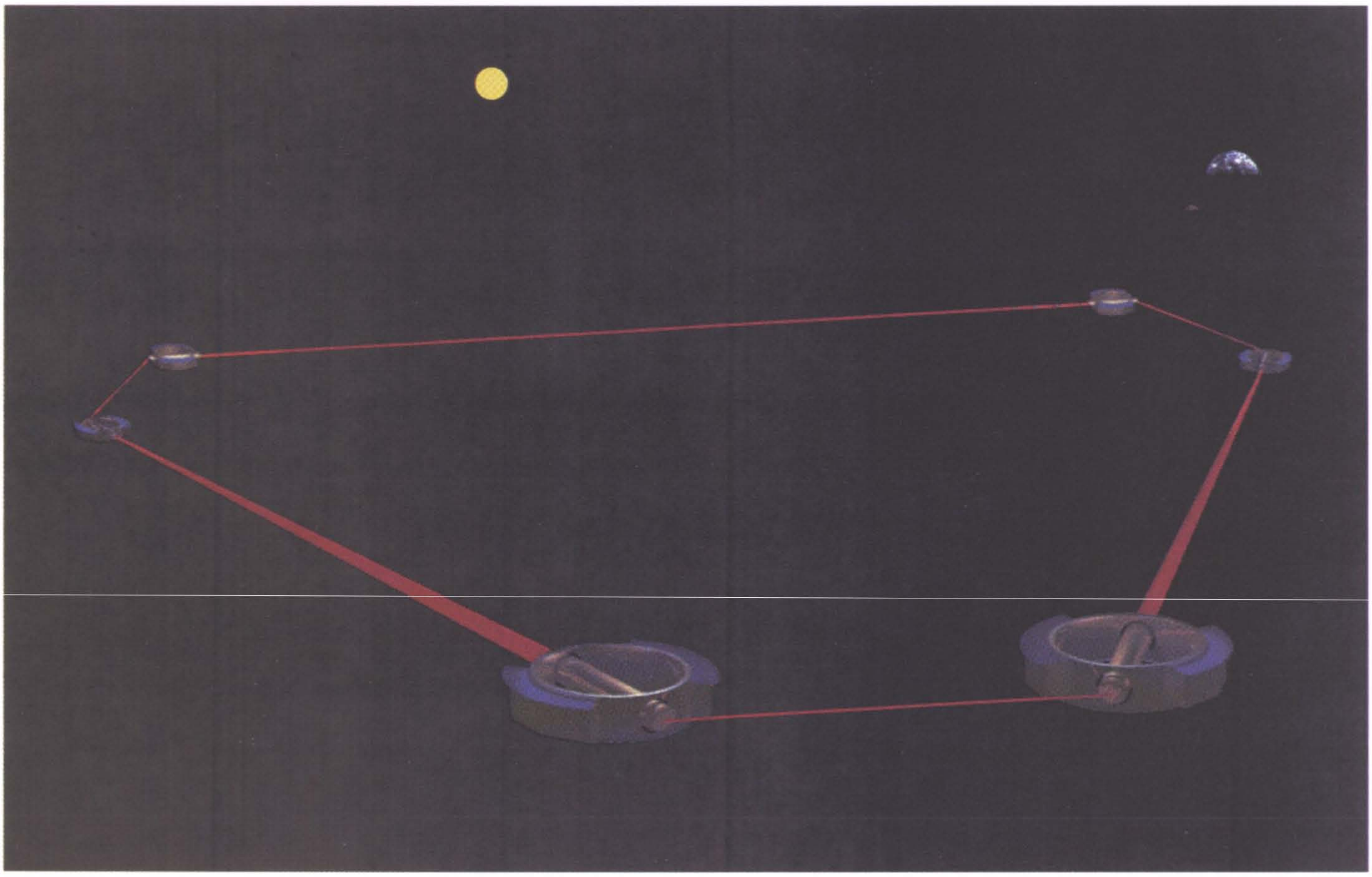
Cosmic strings
Long before the QCD and electro-weak phase transitions – i.e. nearer the initial singularity – there may have been a phase transition associated with the grand-unified interactions, and that transition may have created cosmic strings, one-dimensional 'defects' in the vacuum with gravitational mass per unit length estimated to be $\sim 10^{-6}$, and with tension equal to mass per unit length. As the Universe's horizon expands to uncover the stochastic inhomogeneities in a string's shape, those inhomogeneities should begin to vibrate with speeds up to the speed of light. By self-intersection of the string, closed loops should form; those loops could have acted as seeds for the condensation of galaxies and galaxy clusters. The vibrations of the strings should generate possibly detectable gravitational waves in the relevant frequency range.

Gravitational perturbations from the Sun
A space-based interferometer could also see other things, including possibly the induction-zone gravitational disturbances produced by g-mode oscillations of the Sun. The g-modes are standing internal gravity-driven (hydrostatic) waves in the Sun. They depend on buoyancy, whose magnitude is determined by the stratification of density and pressure and is sensitive to the distribution of chemical elements within the Sun. The Golf and Virgo experiments on Soho will attempt to measure g-modes by observing mass motions of the solar surface and brightness fluctuations, respectively, with periods from about 1 to 12 h. However, these measurements are likely to be limited by other solar phenomena. In the solar interior, the mass motions are predicted to be much larger (up to 100 m/s for some modes), causing gravitational perturbations that might be detectable by a space-based interferometer. These observations would be important for solar modelling.

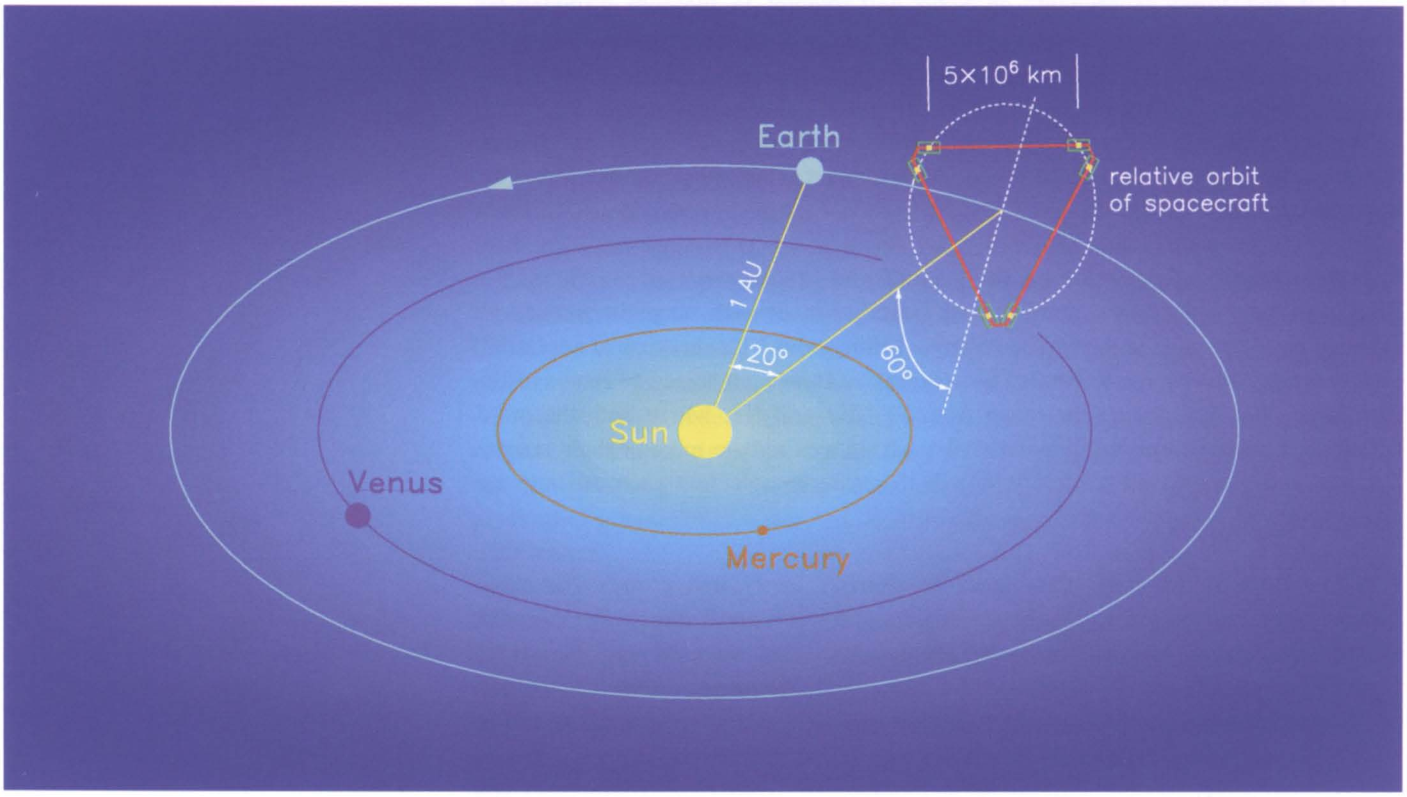
Complementarity of detection on the ground and in space
Like electromagnetic waves, gravitational waves span a wide range in frequency. Ground-based detectors will never be sensitive below about 1 Hz, because of terrestrial gravity-gradient noise. A space-based detector is free from such noise and can be made very large, thereby opening the range from 10^{-4} Hz to 1 Hz, where both the most certain and the most exciting gravitational-wave sources radiate most of their power. Ground- and space-based observations will therefore complement each other in an essential way.

For ground-based detectors, the higher frequencies imply that even stellar-mass systems must last for short periods, and so these detectors will search for sporadic short-lived catastrophic events (supernovae, coalescing neutron-star binaries). Normally, several detectors are required for directional information. If such events are not detected in the expected way, this will upset the astrophysical models assumed for such systems, but not necessarily contradict gravitation theory.

By contrast, if a space-based interferometer does not detect the gravitational waves from known binaries with the intensity and polarisation predicted by General



The six-spacecraft LISA configuration in space, forming an equilateral triangle with a baseline of 5×10^6 km; the two spacecraft at each vertex are 200 km apart



Perspective view of the LISA relative orbit plane, inclined at 60° to the ecliptic. The six-spacecraft LISA configuration is at least 20° behind the Earth to reduce gravitational perturbations.

Relativity, it will undermine the very foundations of gravitational physics. Furthermore, even some highly relativistic events, like massive black-hole coalescences with masses below 10^5 $M_\odot$, last roughly a year or longer. This allows a single space-based detector to provide directional information as it orbits the Sun during the observation.

Both ground- and space-based detectors will also search for a cosmological background of gravitational waves. Since both kinds of detectors have similar energy sensitivities, their different observing frequencies are ideally complementary: observations can provide crucial spectral information.

Proposal for a Fundamental-Physics Cornerstone Mission

There are two great advantages of space for gravitational-wave detection: firstly, it is empty, so one can build very large interferometers, which makes it easier to measure the small strain induced by gravitational waves; and secondly, space is relatively quiet, especially at low frequencies. The Earth is anything but quiet below about 1 Hz. Even if one could screen a ground-based detector from all mechanical disturbances, one could never shield it from gravitational perturbations. The terrestrial environment is full of gravitational disturbances on timescales greater than 1 s, and therefore at frequencies below 1 Hz. These are caused by disturbances such as ground density variations from microseisms and moving atmospheric-density variations. It is likely that these disturbances would mask anticipated incoming gravitational waves. Going into space is the only answer.

Precise, multi-frequency transponding to interplanetary probes, such as the Ulysses, Galileo and Cassini spacecraft, can set upper limits on gravitational waves. These appear as irregularities in the time-of-communication residuals after the orbit of the spacecraft has been fitted. The irregularities have a particular signature. Searches for gravitational waves have produced only upper limits so far, but this is not surprising: the sensitivities are far short of predicted wave amplitudes.

Conceptual ideas for interferometers in space using separate spacecraft were suggested in the USA as long ago as 1978 and 1981. The concept was further developed in the 1980s, leading ultimately to the LISA (Laser Interferometer Space Antenna) proposal to ESA in 1993. A four-spacecraft LISA mission was studied at assessment level as an M3 mission candidate, but it turned out that the cost for LISA was clearly above the limit for a medium-size project.

LISA now consists of six identical spacecraft, forming an equilateral triangle in space with two closely spaced (200 km) 'near' spacecraft at each vertex. In principle, one spacecraft at each vertex would be sufficient, but the optical system and attitude-control requirements would be more complicated. The distance to the 'far' spacecraft is 5×10^6 km, which defines the interferometer arm length. Each spacecraft sends out a 1 W laser beam (at 1 μm wavelength) to its corresponding far spacecraft, and a 10 mW laser beam to its neighbouring near spacecraft. The lasers at each pair of near spacecraft are phase-locked together, thus behaving effectively as a single laser. Each spacecraft receives the laser light through a f/1 Cassegrain telescope with a 38 cm aperture and sends out a beam of its own through the telescope. For the two main arms of the interferometer, the far spacecraft transmits back beams which are phase-locked with a small frequency offset from the incoming beam. When a gravity wave passes through the system, it causes a strain distortion of space, which is detected by measuring the fluctuations in distance between proof masses that are floating freely inside the spacecraft. The incoming light from the telescope is reflected off the proof mass and superimposed with the local laser on a phase-measuring diode. The distance fluctuations are measured to sub-Angstrom precision. Each proof mass is shielded from external disturbances (e.g. solar radiation pressure) by the spacecraft in which it is accommodated. The position signals from capacitative sensors are used in a feedback loop for drag compensation using FEEP (Field Emission Electric Propulsion) thrusters to enable the spacecraft to follow its proof mass precisely. LISA is designed to detect gravitational-wave strains down to a level of order 10^{-23} in one year of observation, with a signal-to-noise ratio of 5.

The six spacecraft, including three propulsion modules for the transfer from Earth orbit to the final position in interplanetary space, can be launched by a single Ariane-5. The three pairs of spacecraft are positioned in individual heliocentric orbits of specific inclination and eccentricity in such a way that the three spacecraft pairs move relative to each other on a circular orbit inclined at 60° to the ecliptic. This keeps the distances between them (the interferometer arm lengths of 5×10^6 km) constant. To ensure that the gravitational perturbations stay small enough for this, the system is placed at least 20° behind the Earth. As this configuration orbits the Sun in the course of one year, the observed gravitational waves get Doppler-shifted. For periodic waves with sufficient signal-to-noise, this allows the direction of the source to be determined. The three arms give two almost independent interferometers, and also provide redundancy in case of the failure of up to two spacecraft (not at the same vertex).

In summary, LISA is basically a Michelson interferometer placed in space, with the advantages of a longer baseline and a quieter environment than on the ground. LISA will benefit substantially from the ongoing development of ground-based interferometers (LIGO, VIRGO and GEO 600).

Other Programme Elements

Medium-size missions

Our present understanding of the fundamental laws of physics has serious shortcomings. Future progress is expected to involve new interactions at a level which may only be detectable by experiments in space. Various ideas proposed for Horizon 2000 Plus aim at testing the Equivalence Principle, searching for spin-dependent interactions, testing Einstein's theory, testing Newton's inverse square law of gravity, and carrying out particle-physics studies in space.

Gravitation, the electromagnetic and weak (unified to the electro-weak interaction) and the strong interactions constitute the four fundamental forces existing in Nature. Einstein's theory of General Relativity provides the basis of our description of the Big Bang, the cosmological expansion, gravitational collapse, neutron stars, black holes, and gravitational waves. General Relativity is a 'classical' non-quantum-field theory of curved spacetime, and it constitutes an as yet unchallenged description of gravitational interactions at macroscopic scales. The other three interactions are described by a quantum-field theory called the 'Standard Model' of particle physics, which accurately describes physics at short distances where quantum effects play a crucial role. At present, however, no realistic theory of quantum gravity exists. This fact is the most fundamental motivation for pursuing our quest into the nature of gravity. The Equivalence Principle – the contention that different bodies fall with exactly the same acceleration in a gravitational field – is both the historical foundation stone of General Relativity and its most precisely testable element.

The Standard Model accounts with surprising success for all existing non-gravitational particle data. However, just as in the case of General Relativity, it is not a fully satisfactory theory. Its complicated structure lacks an underlying rationale. Even worse, it suffers from unresolved problems concerning the violation of the charge conjugation parity (CP) symmetry between matter and anti-matter and the various unexplained mass scales.

The only known tenable solution to the CP problem requires the existence of a new particle, the 'axion', which is also an appealing candidate constituent of the 'dark matter' making up most of the Universe. Axion exchange would mediate forces involving a coupling to quantum spin. One proposal for a medium-size mission aims at searching for a possible interaction between quantum-mechanical spin and ordinary matter at a range of 1 mm with a precision that is seven orders of magnitude better than present ground-based experiments, allowing an important step towards the possible detection of the axion.

The construction of a Grand Unified Theory of weak, electromagnetic and strong interactions seems to require – for theoretical as well as experimental reasons – the existence of a supersymmetry between particles of different spins. This framework suggests the existence of new interactions beyond those of the Standard Model. In particular, the exchange of new spin-one particles could lead to a new repulsive force between macroscopic bodies, which might be detected through small deviations from the Equivalence Principle or the $1/r^2$ law of gravitation.

However, the truly outstanding problem remains the construction of a consistent quantum theory of gravity, a necessary ingredient for a complete and unified description of all interactions in Nature. Superstring theories – in which elementary particles would no longer appear as point-like – are the only known candidates for such a grand construction. They systematically require the existence of spinless partners of the graviton, namely dilaton- and axion-like particles. The dilaton, in particular, could remain nearly massless and induce violations of the Equivalence Principle at a level that may well be within the reach of an experiment in space. The STEP (Satellite Test of the Equivalence Principle) mission is currently under study by ESA at Phase-A level as a candidate for the next medium-size project (M3). It would test the Equivalence Principle to a level of 1 part in 10^{17}, which is five orders of magnitude more precise than is achievable on the ground.

Other medium-size proposals address the relationship between spacetime curvature and matter, usually described by the post-Newtonian parameter γ. In Einstein's theory of gravitation $\gamma=1$; other theories assume a possible admixture of a scalar interaction to the dominant tensor, which would result in a small deviation from 1. In space, such a possible deviation could be tested to a level of 1 part in 10^7 (a factor 10^4 improvement over current knowledge) by measuring the deflection or the time delay of laser light passing close to the Sun.

Small missions

As Fundamental Physics experiments in space have extreme requirements, which typically require drag-free spacecraft and often a cryogenic environment, small missions in Fundamental Physics are usually not feasible. To reduce cost, collaboration between space agencies is then required. The only mission proposal in Fundamental Physics that was shortlisted as a small mission was the Lageos III (Laser Geodynamics Satellite) gravito-magnetic experiment. In combination with Lageos I, launched in 1976, Lageos III is aimed at testing gravito-magnetism (the effect predicted by General Relativity that a massive rotating body, in this case the Earth, drags space and time around with it – also called the Lense-Thirring effect) to a precision of about 3%. However, NASA's much more expensive GP-B project (superconducting gyroscopes on board a drag-free spacecraft) is aimed at measuring this effect to about 1% accuracy and will probably be launched around the year 2000.

Space Station utilisation

The International Space Station offers the opportunity to conduct experiments in near-zero-g conditions. For most Fundamental Physics experiments, however, not only zero-g is required, but also high-level isolation from vibrations over the whole frequency range, as well as extreme attitude stability. These disturbances are present on the Space Station at a level higher than a micro-g, making it considerably noisier than a laboratory on ground. If not for actual measurements, the Space Station could on the other hand be very useful for conducting technological tests and even pilot measurements which are not possible in 1-g conditions on the ground. As an example, development tests on ultra-sensitive superconducting accelerometers could be of interest. In some special cases, such as work on high-precision atomic frequency standards, weightless conditions as on the Space Station could allow one to reach the ultimate limit on precision, which is not attainable on the ground.

Annexes

Annex 1. Responses to the Call for Mission Concepts for Horizon 2000 Plus

No	Names	Proposers	Organisations	Description	Field
1	Aurio/Apafo	Stadsnes	Univ. Bergen, N		Solar System
2	Ballerina	Lund	DSRI, DK	Gamma-Ray Bursts, X-ray Transients, Multiwavelength Studies	Astrophysics
3	CASP	Vessot	Harvard Univ., USA	Close-Approach Solar Probe	Fund. Phys.
4	CHIC	Lallement/Bougeret	CNRS, SA, F	Combined Heliospheric Interstellar and Coronal Mission	Solar System
5	CNSR	Grün	MPI, Kernphysik, D	Comet-Nucleus Sample-Return Mission	Solar System
6.	Co-GRO	Tümer	Univ. Calif., USA	Continuous Gamma-Ray Observatory for Gamma-Ray Astronomy	Astrophysics
7	CONE	Leon	LAEFF, INTA, SP	Cosmic Neutrino Explorer	Fund. Phys.
8	COSINE	Weber	Univ. Maryland, USA	Coherent Scattering of Neutrinos in Space	Fund. Phys.
9	CRONOS	Busca	Obs. Neuchatel, CH	Clock Relativity Observations of Nature of Spacetime	Fund. Phys.
10	CUBE	Deharveng	CNRS, LAS, Marseille, F	Cosmic Ultraviolet Background Experiment	Astrophysics
11	DARWIN	Léger	IAS, F	IR observatory with interferometric rejection to search for primitive life on extra-solar planets	Solar System
12	DCEP	Bertaux	CNRS, SA, F	Detection and Characterisation of Extra-solar Planets	Solar System
13	DHEP	Schneider	Obs. Meudon, F	Detection of Habitaable Extra-solar Planets	Solar System
14	DSFU	Buat	Marseille, F	Deep Sky for UV Survey	Astrophysics
15	EDISON	Penny Cesarsky	RAL, UK CEA	International Infrared Space Observatory	Astrophysics
16	EGL	Cruise	RAL, UK	The European Gravitational Laboratory	Fund. Phys.
17	ELISA	Drury	Dublin, Irl.	European Large Ionisation Spectrometer for Astrophysics	Astrophysics
18	EPOS	Kunow	Univ. Kiel, D	Electron Positron Space Mission	Solar/Astro.
19	ETOS	Neubauer	Univ. Cologne, D	Electrodynamic Tethered Orbiting Satellites	Solar System
20	ETM	Seboldt	DLR, D	Electrodynamic Tether Missions, on Sounding Rockets	Solar System
21	EUROPA	Gautier	Obs. Meudon, F	The Europa Mission towards the Jovian System	Solar System
22	EUROPE	Blanc	Obs. Midi Pyren., F	Mars Exploration	Solar System
23	EUVSPEC	Fraser	Univ. Leicester, UK	Cosmic Extreme-Ultraviolet Spectrometer	Astrophysics
24	FOCAL	Maccone	Turin, I	Gravitational Lens of the Sun	Astrophysics
25	FOCUS	Lund	DSRI, DK	Point Gamma-Ray Sources	Astrophysics
26	FOURTY TWO	Cruise	RAL, UK	Mission to Place Limits on the Rotation of the Universe	Fund. Phys.
27	GAIA	Lindegren	Lund Obs., S	Global Astrometric Interferometer for Astrophysics	Astrophysics
28	Galileo Galilei	Nobili	Univ. Pisa, I	Test of Equivalence Principle	Fund. Phys.
29	GISAT	Anandan	Univ. South Carolina, USA	Test of Gravitational Inverse Square Law and Torsion	Fund. Phys.
30	GMIS	Sims	Univ. Leicester, UK	Global Magnetosphere Imaging System	Solar System
31	GRA	Aartes	SRON, NL	Gamma-Ray Astronomy	Astrophysics
32	GRCM	Dean	Univ. Southampton, UK	Gamma-Ray Cornerstone Mission	Astrophysics
33	GRT	Von Ballmoos	CESR, F	Gamma-Ray Telescope using a Crystal Lens for High Angular and Spectral Resolution	Astrophysics
34	GSE	Paterno	Ist. di Astronomia, I	Global Solar Explorer	Solar System
35	GWAS	Anandan	Univ. South Carolina, USA	Gravitational-Wave Detection using a Superconducting Circuit	Fund. Phys.
36	HELEX	Grün	MPI für Kernphysik, D	Investigation of the Local Interstellar Medium and the Outer Heliosphere	Solar System
37	HICW	Bochsler	Univ. Bern, CH	High-Energy Determinatin of Isotopic Composition of the Solar Wind	Solar System
38	HXRI	Dean	Univ. Southampton, UK	Hard X-Ray Imager	Astrophysics
39	HXT	Turner	Univ. Leicester, UK	Hard X-Ray Telescope	Astrophysics
40	ISLAND	Lockerbie	Univ. Strathclyde, UK	Inverse Square Law using Inertial Drift	Fund. Phys.
41	J3S	Prangé, Zarka	IAS, F	Jupiter and Saturn Systems Survey	Solar System
42	Jupiter...	Neubauer	Univ. Cologne, D	Investigation — Jupiter Inner Magnetosphere Orbiter (Io, Jupiter)	Solar System
43	KON TIKI	Milani	Univ. Pisa, I	Exploration of the Asteroid Belt	Solar System

No	Names	Proposers	Organisations	Description	Field
44	KUIPER	Drossart	Obs. Meudon, F	A Deep Survey of the Outer Solar System	Solar System
45	Lageos III	Ciufolini	CNR, Frascati, I	Lageos III Gravitomagnetic Experiment – Geodesy	Fund. Phys.
46	Lagrange	Schmidt	MPI für Aeronomie, D	The Sun and Inner Heliosphere in Three Dimensions	Solar System
47	LARGO	Hellings	JPL, USA	Long Arm-Length Relativistic Gravitational Observatory	Fund. Phys.
48	LISA	Rüdiger	MPI für Quantenoptik, D	Laser Interferometer Space Antenna for Gravitational Wave Measurements	Fund. Phys.
49	LORE	Paik	Univ. Maryland, USA	Lunar Orbiting Relativity Experiment	Fund. Phys.
50	LOVLI	Labeyrie	Obs. Calern, F	Lunar Optical Very Large Interferometer	Astrophysics
51	LPE GaAs	Sumner	Imperial College, UK	LPE GaAs as an X-ray Detector for Astronomy	Astrophysics
52	LRPP	Seboldt	DLR, D	Lunar Resource Prospecting and Processing	Solar System
53	MAGICS	Sauvaud	CESR, FF	Magnetospheric Imaging Circumterrestrial Satellite	Solar System
54	MENDELEEV	Pillinger	Open Univ., UK	Solar-Wind Sample Return	Solar System
55	MEPS	Ginenthal	Univ. New York, USA	Measurements of Electromagnetic Properties of Space	Fund. Phys.
56	MER COR	Peale	Univ. Calif., USA	Determination of the Nature of Mercury's Core	Solar System
57	MERCURY MAPPING ORBITER	Arnold	DLR, D	Surface Mapping and Investigation of Mercury	Solar System
58	MERCURY ORBITER WITH A SOLAR SAIL SPACECRAFT	Leipold/Seboldt	DLR, D	Mercury Orbiter with a Solar Sail Spacecraft	Solar System
59	MGM	Theiss	Univ. Stuttgart, D	Measurement of the Earth's Gravito-Magnetic Field	Fund. Phys.
60	MODEST	De Graauw	SRON, NL	Molecular Oxygen Detection Experiment with Space Technology	Astrophysics
61	NAIADES	Schmitt	CNRS, F	Neptune: Atmosphere, Interior, Arcs, Dynamics Environment and Satellites	Solar System
62	NEDHEM	Anandan	Univ. South Carolina, USA	Test of a New Force on a Dipole due to Homogeneous Electric and Magnetic Fields	Fund. Phys.
63	NEP	Loeb	Univ. Giessen, D	High-Energy Interplanetary Missions with Nuclear Electric Propulsion	Solar System
64	NSGRA	Schönfelder	MPI, D	The Next Steps in Gamma-Ray Astronomy after Integral and GRO	Astrophysics
65	OHK	Kirsch	MPI, D	Outer Heliosphere and the Kuiper Comet Belt	Solar System
66	OLBERS	Désert	IAS, F	Interplanetary Probe to Study Visible and Infrared Diffuse Backgrounds	Astrophysics
67	ORPHEUS/SCP	Marsch	MPI, D	Solar Coronal Probe	Solar System
68	ORT	Booth	Onsala, Chalmers, S	Orbiting Radio Telescope	Astrophysics
69	PLURES	Ragazzoni	Obs. Padua, I	Payload for Ultraviolet Research on Early Supernovae	Astrophysics
70	PROMISE	Hall	SERC, UK	Programme of Many Inexpensive Space Experiments	Small missions package
71	QUEST	Karim	St. John Fisher College, USA	Test of Quantum Electrodynamics in Curved Space-Time	Fund. Phys.
72	QUILT	Anandan	Univ. South Carolina, USA	Quantum Interferometric Test of the Lense-Thirring Field	Fund. Phys.
73	RLS	Arnold	Univ. Leicester, UK	Radio Limb Sounder	Solar System
74	RULER	Owens	Univ. Leicester, UK	Instrument to Measure Gamma-ray Burster Distances	Astrophysics
75	SAMBA	Puget	IAS, F	Satellite for Measurements of Background Anisotropies	Astrophysics
76	SCS	Neubauer	Univ. Cologne, D	Solar Corona Sounders	Solar System
77	SIHR	Vial	IAS, F	Solar Imager at High Resolution	Solar System
78	SIMURIS	Damé	CNRS, SA, F	Solar and Solar System Interferometric Mission for Ultrahigh-Resolution Imaging and Spectroscopy	Solar System

No	Names	Proposers	Organisations	Description	Field
79	SMRPM	Ciufolini	CNR, Frascati, I	Small Mercury Relativity and Planetology Mission	Solar System/ Fund. Phys.
80	SO	Ferrari	Univ. Turin, I	Solar Observer	Solar System
81	SOLACE	Lester	Univ. Leicester, UK	Solar Atmospheric Coupling Experiment	Solar System
82	SOPHI	Lipa	Stanford Univ., USA	Test of the Theory of Second-Order Phase Transitions in Condensed Matter	Fund. Phys.
83	SORT	Veillet	OCA, F	Solar Orbit Relativity Test	Fund. Phys.
84	SPARXS	Bleeker	SRON, NL	Spatially Resolved High-Resolution X-Ray Spectroscopy	Astrophysics
85	SPICE	Rouan	Obs. Meudon, F	Spectro-Photometric Infrared Celestial Exploration	Astrophysics
86	SQUARE	Speake	Un. Birmingham, UK	Satellite Quantum Gravity and Relativity Experiment	Fund. Phys.
87	SREP	Paik	Univ. Maryland, USA	Short-Range Equivalence Principle	Fund. Phys.
88	SSCE	Xu	Univ. Strathclyde, UK	Spin−Spin Coupling Experiment	Fund. Phys.
89	SSPIN	Sumner	Imperial College, UK	Satellite Search for Pseudo-scalar Interactions	Fund. Phys.
90	SSH	Lang	SERC, UK	Stereoscopic View of the Sun and Heliosphere	Solar System
91	STARS	Jones	Kapteyn Obs., NL	Seismic Telescope for Astrophysical Research from Space	Astrophysics
92	STEP	Cruise	SERC, UK	Satellite Test of the Equivalence Principle	Fund. Phys.
93	STRESS	Lockwood	SERC, UK	Stress-Test Research Satellites	Solar System
94	STUFF	Nordvedt	Bozeman, USA	Strong Test of the Universality of Free-Fall	Fund. Phys.
95	Super IUE	Bleeker	SRON, NL	IUE, Spectrocsopy	Astrophysics
96	TRT (Mouse/Trust)	Viotti	Ist. di Astrofisic., I	Three-Reflection Telescope (UV)	Astrophysics
97	UHPM	Lipa	Stanford Univ., USA	New test of the universality hypothesis using order parameter measurements near the lambda transition of helium	Fund. Phys.
98	VELIX	Zehnder	PSI, CH	Very Large Imaging X-Ray Satellite for High-Resolution Spectroscopy	Astrophysics
99	VIVALDI	Stalio	CARSO, I	Grand Tour through the Inner Solar System using the Solar-Sail Technique	Solar System
100	VLO	Bougeret	Obs. Meudon, F	Very-Low-Frequency Lunar Observatory	Astrophysics
101	VULCAN	Roxburgh	Univ. London, UK	Solar Probe	Solar System
102	LATOR	Sandford	SERC, RAL, UK	Laser Astrometric Test of Relativity	Fund. Phys.
Miscellaneous					
		Lundin	IRF, S	Follow-up to Horizon 2000 − Solar System, Space Plasma Physics (4 proposals)	Solar System
	OWS	Someria	SRPE, F	Ocean Waves Spectrum	Earth Observ.
	GOAL	Waddington	Univ. Minnesota, USA	Galactic Origins and the Acceleration Limit	Astrophysics

Some of the contributors, photographed during a Survey Committee meeting in Rome in September 1994

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