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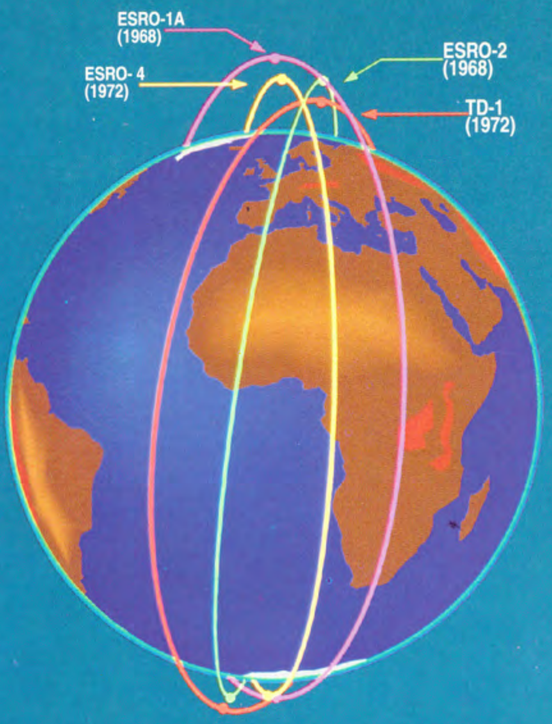
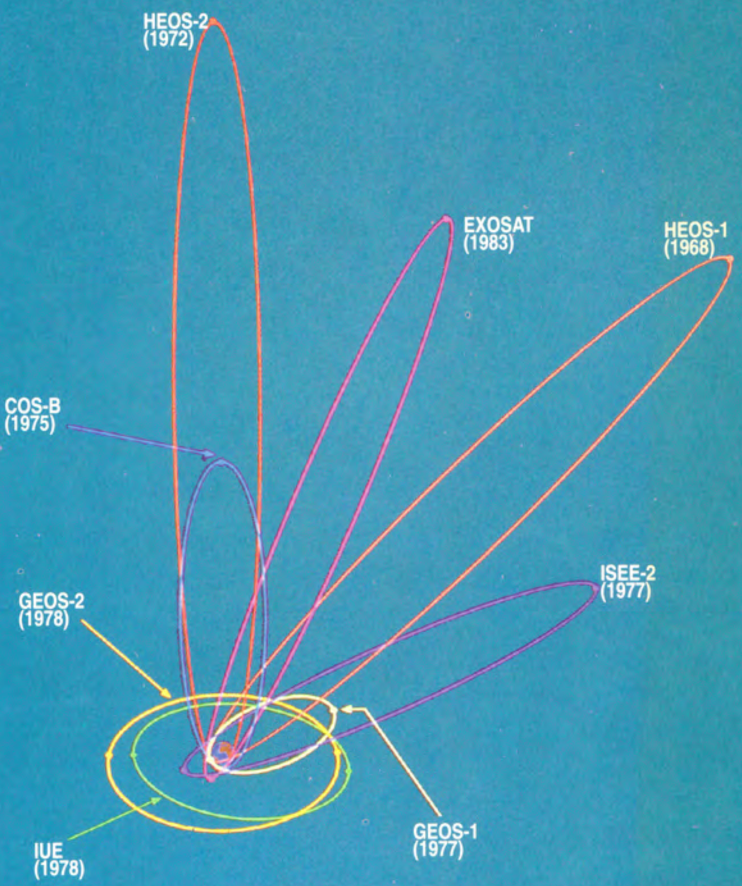
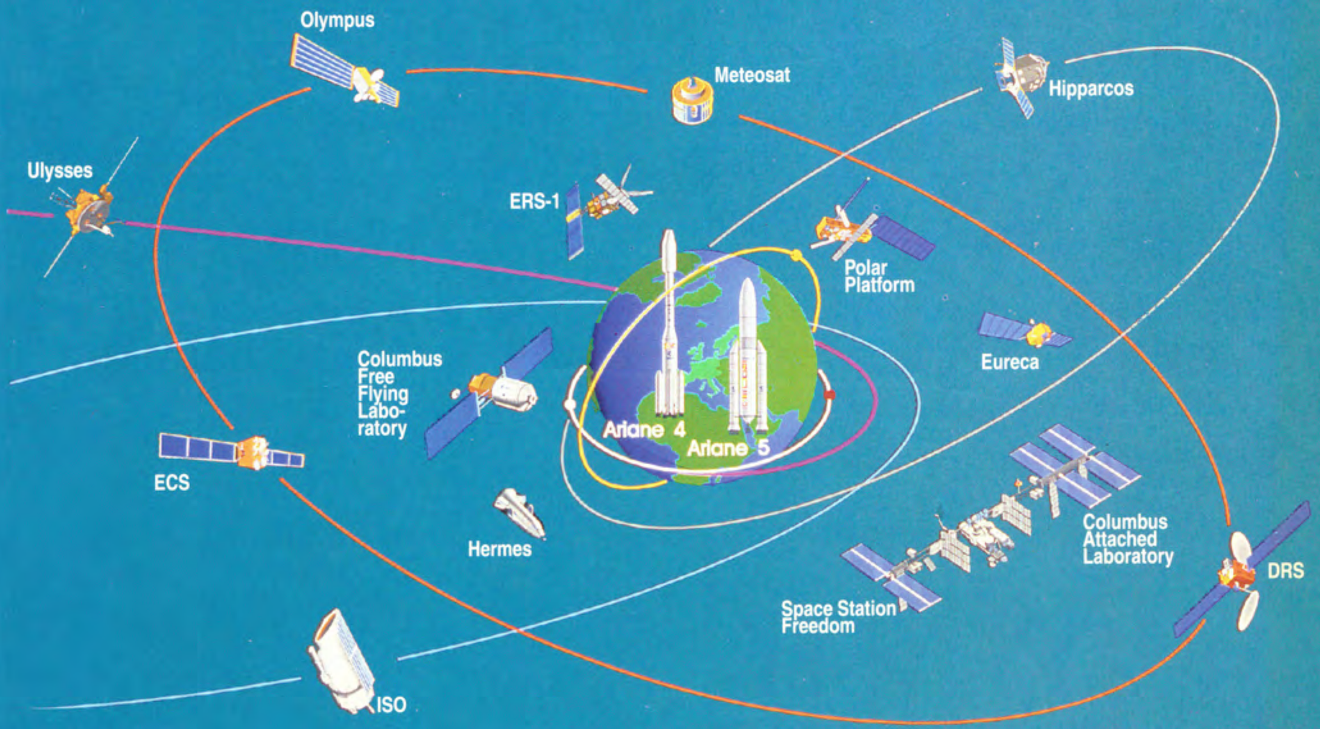




Science beyond the atmosphere: The history of space research in Europe

Proceedings of a symposium
held in Palermo
5 - 7 November 1992

Edited by Arturo Russo



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The ESA History Study Reports are preliminary reports of studies carried out within the framework of an ESA contract. As such they will form the basis of a comprehensive study of European Space activities covering the period 1959-87. The authors would welcome comments and criticism which should be sent to them at the appropriate address below.

The opinions and comments expressed and the conclusions reached are those of the authors, and do not necessarily reflect the policy of the Agency.

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SCIENCE BEYOND THE ATMOSPHERE: THE HISTORY OF SPACE RESEARCH IN EUROPE

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Preface

The papers comprising this collection were prepared for a symposium held in Palermo in early November 1992. The aim of the symposium, organized in the framework of the ESA History Project, was to discuss critically the development of the scientific programmes of ESRO and ESA by bringing together some of the main protagonists of the history of space research in Europe and a few historians working in the field. The symposium was sponsored by the University of Palermo, the Italian National Research Council and the European Space Agency. A financial contribution was also made by the Palermo bank Sicilcassa.

The meeting was a fruitful occasion to unite the liveliness of personal recollections with the rigour of scholarly historical research. It also offered all participants the occasion for friendly discussions and social events, while enjoying the beatiful landscape of the beach of Palermo-Mondello. And, last but not least, the historians engaged in the job of studying the history of Europe in space, were given the splendid opportunity of spending a few days with some of the people who made this history.

The papers presented at the symposium were very different from each other, reflecting the participants' various experiences, interests and methodological approaches. It would hardly be possible in this preface to offer any unifying guideline for reading this collection, and we will not attempt to find one. A few introductory remarks, however, can help the interested reader to place these papers in their proper historic and thematic framework.

A first element to be considered is obviously the chronology of the main events that have marked the development of space research in Europe. Limiting ourselves to the collaborative efforts within the framework of ESRO and ESA, we should recall that the former organization, whose name in full was the *European Space Research Organization*, was created in the early 1960s, as a result of the initiatives of a group of European scientists and science policymakers. It was intended to be an organization devoted solely to space science, with no involvement in any application fields, such as satellite telecommunications or meteorology. Ten European countries founded ESRO, whose convention came officially into force in March 1964, after a three-year interim period in which space activities were coordinated by the *Commission Préparatoire Européenne des Recherches Spatiales* (COPERS). It must also be recalled that alongside ESRO a second space organization was created, the *European Launcher Development Organization* (ELDO). It comprised six European countries plus Australia, and its aim was to design and build a rocket capable of launching heavy satellites into near earth orbit.[1]

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[1] ESRO's member states were Belgium, Denmark, France, Germany, Italy, Netherlands, Spain, Sweden, Switzerland, United Kingdom. ELDO's European member states were Belgium, France, Germany, Italy, Netherlands and United Kingdom.

ESRO launched its first satellite (ESRO-2) in May 1968. Two more satellites (ESRO-1A and HEOS-1) were successfully put in orbit later that year and another (ESRO-1B) in 1969. In 1972, ESRO launched three new satellites (HEOS-2, TD-1 and ESRO-4), while the following spacecraft, COS-B, launched in the summer 1975, carried the flag of the *European Space Agency*, the organization that had succeeded ESRO and ELDO earlier that year. Since then, several scientific satellites have been launched by ESA, and their results have contributed significantly to the understanding of celestial phenomena.[2] While in this logic we should also mention the results obtained by the 183 sounding rockets launched between 1964 and 1972.

The second important element to be considered is the peculiar relationship between ESRO/ESA and the European space science community. Apart from a small group of scientists integrated into ESRO's and ESA's technical organization, this community remained essentially external, being spread over many scientific institutions and universities in ESRO's and ESA's member states. European space scientists, however, were involved in a set of advisory committees and expert groups which provided ideas and made recommendations about possible missions to be included in the ESRO/ESA programme. They were also responsible for building the necessary hardware and software to operate the instruments of approved payloads, and for analysing and interpreting the data and publishing the results. ESRO/ESA's task was that of providing the community with the commmon services such as spacecraft, launchers, ground stations, project management, long-term planning, feasiblity studies, etc. This "bottom-to-top" approach, as R. Bonnet defines it in his paper, left the actual definition of the programme to discussions and negotiations between interests groups outside the European organizations, the latter only being called to provide technical and economic studies.

The third element in the framework we are considering is the political dimension in which ESRO/ESA's scientific programme was discussed and eventually implemented. Space in fact is not only a scientific frontier but also an area of technological applications of great industrial, commercial and military interest. ESRO was soon requested to undertake studies on application satellites, in particular in the field of telecommunication. In the ensuing negotiations over a coherent European space policy, the needs and expectations of scientists were reduced to an element, and not the most important one, of a complex framework which also included application satellites and launchers. The actual development of ESRO/ESA's scientific programme was strongly affected by this process, whose outcome was in fact the transformation of the original space research organization into a space agency in which science demands no more than 10 per cent of the budget.

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[2] Following is a list with, in brackets, the date of launch: ISEE-2 (1977), GEOS-1 (1977), GEOS-2 (1978), Exosat (1983), Giotto (1985), Hipparcos (1989) and Ulysses (1990). ESA also contributed to NASA's scientific satellites IUE (1978) and HST (1990).

The papers we are introducing here deal with the development of ESRO's and ESA's scientific programme from three different point of view. The first is that of the internal dynamics in the variegated field of space science. The availability of rockets and satellites, in fact, opened new interesting areas of scientific investigation (e.g. magnetospheric studies or UV and X-ray astronomy) and offered new stimulating opportunities to established disciplines (e.g. ionospheric research or solar physics). The intelligent use of these technologies required the formation of a new scientific community interested in space research and mastering the relevant technology. The emergence of such a community out of the traditional fields of astronomy, physics and geophysics, however, was characterized not only by great intellectual vivacity but also by tensions and conflicts between different scientific interests, approaches and methodologies.

The second point of view looks at the political framework in which the scientific programme was defined, particularly in the difficult transition period 1968-1972. Scientists and science policymakers could not have an important role in a phase when ministers and top-level bureaucrats were trying to forge a coherent space policy for Europe out of the conflicting interests of European countries. Thanks to the achievements ESRO's scientific satellites, however, and the willingness of all parties to find a compromise, space science found itself at the core of the institutional framework which emerged after the crisis, and the only activity which was mandatory for all ESA member states.

The third point of view, finally, regards the relationship between ESRO/ESA's scientific programme and the space programmes pursued at national level. The European organization in fact was not the only means available for flying scientific payloads designed by European scientists. Important national space programmes existed in France, Germany Italy, Netherlands and United Kingdom, while interesting flight opportunities were also offered to European scientists by NASA. And even in those countries where space research was only pursued within the ESRO/ESA programme, the problem existed of planning this activity in the framework of national scientific and technological policies.

Regarding the choice of the papers presented here two remarks are called for. The first regards the programme of the Palermo symposium and the selection of speakers. This reflects of course the interests and ideas of the organizers and, in particular, of the author of this preface. The symposium, as we said above, was organized in the framework of the ESA history project and therefore its organization reflected the status of the project in mid-1992. This explains the predominance of the ESRO period in the programme and the contributions requested from people who were particularly active in this period, both in the research work and at the policymaking level. As the project continues, more contributions will be required in the future and, perhaps, another symposium.

The second consideration is of a more general nature, and involves the relationship between historians and those who make history. In recent years, especially in the field of history of science, several initiatives have been undertaken, sometimes in the form of wide-ranging projects, to record oral testimonies and to convene meetings where scientists and historians gather to discuss the history of a discipline or a scientific institution. This has proved to be a fruitful exercise when two conditions are met. The first is the clear consciousness of the historian that personal recollections and the testimony of the historical protagonists should only be an element, and not the most important one, of meaningful historical research. They can provide important help on some technical aspects of the discipline, useful information on "missing links" in the chain of historical documents, and lively accounts of the human aspects of the scientific enterprise. They cannot replace, however, the diligent study of published articles and archival documents, which should remain the historian's principal source.

The second condition regards the scientist's attitude towards the history of his discipline and the historian's work. He should avoid the natural tendency to rationally reconstruct the past in the light of latest results, overlooking the concrete dynamics of trials and errors, successes and failures, conflicts and negotiations which are characteristic of scientific research no less than of any other human enterprise. And he should look at the historian not as a passive recipient of information or a curious listener to anedoctes, but as a professional scholar whose theories and methodologies are derived from established academic standards.

If both conditions are respected, the meeting of scientists and historians can become a fruitful engagement of ideas and competences and provide useful new knowledge of the past. We think that these conditions were respected at the Palermo symposium and this, we hope, can make the reading of its proceedings as useful and pleasant as was that meeting.[3]

Arturo Russo

3 The editing of these proceedings has been realized thanks to the help of Ms Beatrijs de Hartogh.

Space science in ESRO and ESA: an overview

Roger M. Bonnet
European Space Agency, Paris

Introduction

I am very honoured to have been invited to present this overview. To make a speech like this one in front of such a distinguished audience where so many knowledgeable people are present, is indeed a challenge. Many among you here would have been much more qualified to give this paper. Hence, I will not attempt to present to you a comprehensive description of the content of the ESRO and of the ESA programmes. I will rather focus on some of the characteristics of the European space programme covering the past, the present and looking towards the future. Before that, let me analyse first what has not changed between the ESRO and the ESA eras.

1. The invariants

1.1. The diversity of European space science

This diversity which characterised the beginning of the ESRO era is still a dominant feature today. By diversity I mean the large number of scientists and institutes (fig. 1), as well as the spread of their interests in so many scientific domains such as solar physics, cosmology, space plasma physics and planetary science, without forgetting fundamental science, gravitation theories and relativity. This characteristic which indeed is not only proper to Europe, explains why the European scientists are so eager to get access to a larger number of flight opportunities. It clearly shows the need for ESRO and ESA to have a balanced programme between the various centres of interest.

1.2. The ambition of the scientists

Nothing better characterises this ambition than the fact that the famous "Blue Book" of the COPERS had proposed no less than 17 satellites (11 small, 4 probes and 2 large ones) to be launched over a period of 8 years, corresponding to 34 launches since 2 launches per mission were considered safer at that time due to the relative uncertainty of the rockets. Amazingly, indeed, ESRO and ESA have launched 17 satellites in space science but in ... 31 years. The scientists were at that time (1961) over optimistic by a factor of 4!

It is also in these early times that European scientists were able to propose the Large Astronomical Satellite, so-called LAS, which was at the origin of the first deep crisis of ESRO.

The project was under-evaluated, its technology was less than ready, at least in Europe at that time, and there was no rocket to launch it - even the Europa launcher was not powerful enough to put it into orbit.

The other side of the scientific community was no less ambitious when they defined the second large project, a mission to a comet. This power for imagination (and lack of realism) still today characterises the scientists in the ESA era — but also others in other parts of the world — and shows no tendency to diminish.

1.3. The ESRO and the ESA approaches

The approaches followed by ESRO and by ESA to introduce missions in their programme is still the same after so many years. It is what I would call a *Bottom-to-Top* approach whereby the community is the only source of the ideas and of the concepts of missions, the Agency being there only to transform these ideas and concepts into reality, ESRO and ESA providing the common services to the community, i.e. the satellites, the launcher and the ground stations required to operate them and the scientific community being responsible for providing the payloads and the necessary hardware and software to operate the instruments, and, of course, for analysing and interpreting the data and for publishing the results in the scientific press.

2. *What has changed*

Several major changes have however been observed in the past 30 years or so. First, ESA is no longer a purely scientific organisation, as ESRO was. In fact, ESA's Science budget is only a tiny 10% of the overall ESA budget, totally dominated by the optional activities, in particular by the big infrastructure programmes such as the Ariane 5 launcher, the Columbus programme (Europe's participation to the Space Station), and what remains of the Hermes space plane, essentially a technology programme (figs. 2, 3 and 4). The general basic activities, which in the ESRO times were totally devoted to the science programme, are now shared by the various programmes. To be perfectly clear however, the scientific programme gets the largest share of the technology research money and of the general study money used for the preparation of its future missions (approximately 20%, that is twice the proportion of the overall budget). In addition, since 1987, new Member states have joined ESA. They are Austria and Norway. Finland also is now an associate member and participates fully in the scientific activities. Canada has a special associateship with ESA but does not participate in the science programme.

We also observe a proliferation of national space agencies (fig.5), each with its own national programme, a tendency which can probably be explained by the good success of the first of them, CNES, in formulating a long term policy successfully implemented within ESA, at least until recently. For the science programme, these agencies are useful since they are the direct interface with ESA on matters concerning the financing of the scientists and of the payloads embarked on ESA spacecraft. Furthermore, since 1984, ESA has signed an agreement of reciprocity with NASA whereby ESA agrees to open its own flight opportunities to the American scientific community, reciprocating NASA's policy which, very early, made it possible for European scientists to participate to NASA's missions, a policy, however, not deprived of any parochial interests. Worth being mentioned in this context is the fact that the first time ESA implemented the reciprocity agreement, by opening its Announcement of Opportunity for the ISO mission to the US scientific community, NASA took the attitude of not supporting its scientists in spite of their keen interest in participating in this unique mission - in fact, the only infrared space observatory to fly before the end of this century.

Today, ESA opens its Announcements of Opportunity to the world-wide scientific community as can be illustrated by the participation in the Cluster mission of the Centre for Space Science and Applied Research, the CSSAR at Beijing, of Hungarian scientists in the same mission, and of many countries in the exploitation of the International Ultraviolet Explorer, IUE, and of the Hipparcos mission, without forgetting the increasing number of contacts with scientists in the former USSR, materialised by the participation of IKI scientists in the INTEGRAL study and of ESA in the MARS-94 mission, for which it builds the mass memory of the spacecraft.

Last, but by no means least, ESA engineers, after so many years, have acquired an experience and an expertise which place them far ahead of their colleagues in industry and which allows the Agency to master the cost and the schedule of the various missions and to constrain the ever increasing ambitions of the scientists, forcing them to keep their feet a little closer to the ground.

Let me now review a few of the most important characteristics of the ESRO and the ESA space science programme.

3. Characteristics of the ESRO and of the ESA programme

After the first crisis mentioned above, which saw the elimination of its first two large projects, ESRO adopted a much more modest approach, one which was recommended by Edoardo Amaldi himself : an approach which started with a learning period, during which the basic techniques and technologies were to be developed, and the engineers and the technicians would be formed which would make it possible, in the course of a second phase, for the scientists to undertake these more ambitious missions which they had in mind. This approach has indeed been followed and it has basically fulfilled the objectives set out by Amaldi in his visionary analysis. Figure 6 lists the science missions launched by ESRO and by ESA.

Several observations can be drawn from this list. First, the number of satellites launched is relatively small with only 17 missions launched (in fact the Japanese have launched more satellites than ESA itself). Second, the ambition and complexity of the missions is continuously increasing (fig.7). Starting with a modest set of spacecraft, which more or less copied what the Americans and the Soviets were doing at that time, i.e. studying cosmic rays, the Sun, the magnetosphere and the ionosphere of the Earth, the ultraviolet spectrum of stars, mostly launched in the ESRO times, and then followed by a set of increasingly complex missions such as TD-1, GEOS, Giotto, ISO, SOHO and Cluster. The continuous growth in weight, power and telemetry requirements of the ESRO/ESA satellites illustrates the tendency for Europeans to undertake more and more ambitious missions, with a view to reaching more and more ambitious scientific objectives.

The learning period of ESRO, based uniquely on purely European missions, with the exception of the use of American launchers, was followed by the ESA period during which, among other things, we can see the increasing role of international cooperation, in particular with NASA, illustrated by the case of the ISEE, IUE, Ulysses and HST missions, now followed by SOHO, Huygens and probably later by the Comet Nucleus Sample Return mission.

3.1. The unique character of some of the ESA missions

ESRO and ESA, forced to constrain themselves to a small number of scientific missions, have counterbalanced this relative scarcity by selecting missions of great originality, in particular during what I would call the "majority age" of European space science. It is of course dangerous to quote some examples of these missions since I may run the obvious risk of forgetting the pet mission of some of you here. Nevertheless, I cannot avoid showing a few examples, by no means an exhaustive list, which in their time placed, or now place ESA above all the other space agencies in the world.

- COS-B produced the first map of the emission of our galaxy in gamma rays.
- Giotto, the natural son of the second large project, was the jewel in the crown of all the space missions launched by the USSR and Japan to observe Halley in 1986. Besides providing the most detailed pictures of a comet nucleus, Giotto provided numerous data on the dust and gas content of the coma. The spacecraft, which represents one of the best illustrations of the compromise approach between modesty and visionary ambition that ESA has followed in order to remain realistic, survived its momentous and perilous encounter with Halley. So well indeed that it could recently observe, in July of this year, a second comet, Grigg-Skjellerup, which it passed by at a distance of only 200 km, providing new and high quality data, even though its multicolour camera remained blind. The excellency of the Giotto mission will make it a reference point in all future comet missions and certainly at the next passage of Halley at its perihelion in 2061.
- Hipparcos, the first space astrometry mission, in spite of having been placed on a wrong orbit as a result of the failure of its apogee boost motor, and of many technical problems (mostly

connected to the orbit), has achieved its nominal objectives of measuring the parallaxes, distances and proper motions of stars in our galaxy with an accuracy of 2 milliarcseconds, an objective that very few believed could be achieved when the mission was selected in 1980. Furthermore, the Tycho add-on experiment has provided a very accurate three-colour photometry of more than one million stars instead of the 400 000 originally foreseen. Many new double star systems have been discovered. The first complete solution of the equations for the whole celestial sphere based on 18 months of data, has now been achieved, putting in clear evidence the rudimentary character of many of the astrometric catalogues so far published. Hipparcos will remain as a unique reference for astronomers and astrophysicists, until a more accurate and performing mission will be undertaken, probably not before the beginning of the next century at the earliest.

- Ulysses. The unique character of this mission derives from its orbit, with the spacecraft evolving well outside the ecliptic plane. Launched in October 1990 by the American Shuttle, Ulysses encountered Jupiter in February of this year and will fly above the south pole of the Sun in the May-September 1994 time frame, and above the north pole one year later. The extreme precision of the launch has rendered unnecessary several trajectory manoeuvres and will allow the spacecraft to reach a record heliocentric latitude of more than 80 degrees for both passages. Already, the data acquired by the various instruments have provided new insights into the Jovian environment, the composition of the solar wind, the dust, and the penetration of neutral interstellar helium into the Solar System.
- ISO (fig. 8), the Infrared Space Observatory will be the only observatory of its category to fly during the rest of this century and probably for more years of the next century, since its successor, the American SIRTF, is now indefinitely postponed. Operating between 3 microns and 200 microns, ISO, a 2300 kg satellite, rests on complex cryogenic systems which are necessary in order to cool down the temperature of its mirror and of the focal plane instruments to only a few degrees above absolute zero, through the use of more than 2000 litres of liquid superfluid helium. ISO represents an extreme technological challenge which is the cause of more than 2 years of delay in the launch with respect to the planned date.
- Huygens, my last example in this list, is a probe to be launched on-board NASA's Cassini mission in 1997. It will explore the atmosphere of Titan, the only satellite of another planet to possess an atmosphere which, furthermore, seems to be very similar to that of our own planet just after it was formed 4.5 billion years ago. If every thing goes according to plan, Huygens should land on the surface of Titan in the spring of 2005... Planetary scientists, these days must be prepared for long rides! When it reaches the surface of Titan after having explored its atmosphere, Huygens will be the first ever man-built object to land so far away from the Earth.

I think you can agree with me that the "leadership" of Europe is clearly demonstrated by the few examples quoted above. But of course, in order to maintain such a position, ESA's missions have to compete with the more ambitious projects of NASA and of the Russians. This is another reason why they are following a dangerous tendency toward increased complexity.

3.2. The trend towards gigantism

This trend characterises not only ESA missions but all other missions in the world, including the Japanese. The real problem for ESA is that, with a science budget which is more or less constant (fig. 3), with more and more ambitious missions, costing more and more, it can undertake fewer and fewer of them. If nothing is done to counter this tendency, the future of the only coordinated space programme in Europe is running the risk of being asphyxiated.

The need for more complex missions is closely associated with the need for more advanced planning and for an earlier and therefore longer, technological preparation. No other example of this problem can best be offered than ISO. Since the second half of 1991, ESA engineers are confronted by a problem which nobody would have imagined. The valves which distribute the liquid helium in the cryostat and which were qualified in the frame of the Technology Research programme of ESA, refuse to operate properly and encounter many problems. Today, one could say that these valves were not properly qualified. This very disturbing problem is now the cause of the ISO project having a delay of more than two years, something never seen before in the ESA scientific programme. A posteriori, one can say that an earlier technological preparation might have contributed to avoid the problem.

ISO was the last of the ESA Science Programme missions to have been selected through the highly competitive (but also fairly severe) selection process followed by ESA in its programming prior to the Horizon 2000 era.

4. The need for long lead technological preparation: Horizon 2000

If one were to identify one single rationale for the establishment of Horizon 2000, it would be the need for more advanced long-lead technological developments, in order to introduce in the programme the missions claimed and needed by the scientific community which were never accepted in the programme because their feasibility could not be proven and continuously put in doubt. This is the case of missions such as the X-ray Multi-mirror Mission, XMM and the Far Infrared Space Telescope, FIRST.

4.1. The genesis of Horizon 2000

The need for this advanced and long-lead technological preparation, together with the need for scientific institutes involved in the development of the payloads and of industry in charge of realising the missions to prepare themselves sufficiently early, created a quasi natural rationale for the formulation of Horizon 2000 in 1983-1984. Every partner was eager that ESA should formulate for once, a long term programme providing the necessary reference around which one could eventually organise itself in order to take the best possible share of the missions.

The approach which was formerly followed for introducing the various projects into the programme was also followed in the formulation of Horizon 2000. The process started with a broad consultation of the scientific community in September 1983. By January 1984 77 mission concepts piled up on the desk of V. Manno and on mine. A Survey Committee, chaired by J. Bleeker, analysed the scientific content of these concepts, as well as the technology required to transform them into realistic projects.

The programme was formulated concretely at the last meeting of the Survey Committee, held in Venice at the end of June 1984. The consensus was not easy to reach. Why? This is probably related to the proposed structure of the Horizon 2000 plan and to the concept of cornerstones which were introduced first as the basic element of the plan, as their name indicates (fig. 9).

4.2. The cornerstones

Cornerstones correspond to missions which, like XMM and FIRST, were regularly proposed by the scientific community in response to the calls for ideas issued at regular intervals by ESA but which, regularly, could not be selected, in spite of their great scientific interest, because they were not ripe from the technological point of view and therefore could not be considered as realistic. The XMM and FIRST missions already mentioned were the first to be selected as cornerstones.

In the area of Solar System exploration, the consensus was longer to reach, the plasma and solar physics community not willing to abandon their favourite respective projects, Cluster and SOHO. Not able to compromise, it was decided to group the two missions under the title of the Solar Terrestrial Physics Programme, STSP, which later became the first cornerstone of Horizon 2000.

As far as planetary exploration was concerned, the Survey Committee confirmed that Europe should lead the way in the study of the small bodies of the Solar System, i.e. comets and asteroids, and recommended a mission to return pristine material from these small bodies as a cornerstone, a very ambitious mission indeed.

With these four major projects the balance was achieved, at least in a reasonable number of scientific areas.

4.3. The structure of Horizon 2000

In order to maintain a certain amount of innovation and to allow the scientific community to introduce new ideas on a regular basis, in other words, to allow the science programme to keep abreast of the progress in space science, it was decided to introduce a certain number of medium and small size missions to be selected "à la demande" following the competitive selection process in use before the concept of Horizon 2000 was invented. The financial balance was secured in allocating 60% of the budget envisaged for the 20 years spanned by Horizon 2000 to the cornerstones, and the remaining 40% to the so-called "blue" and "yellow" missions.

In the definition of Horizon 2000, has ESA deviated from the principle of depending on the scientific community to introduce the missions in the programme, since indeed the cornerstones are pre-selected? Certainly not, because even the cornerstones have been selected by representatives of the scientific community, the Survey Committee, and their concepts were proposed by the same community after a broad consultation. What is true, however, is that the method now in use applies to an entire programme rather than to each project individually (fig. 10).

Now that the budget is allocated to a complete menu (Horizon 2000), it is essential that each mission keeps within its allocated budget, i.e. 565 MAU for cornerstones and half this value for the medium size (blue) missions. In other words, each mission has to be designed according to a fixed cost at completion (fig. 11). This is far from easy and the Director of Science is neither popular among the scientists who want to draw the maximum out of the budget in order to get as much science as possible, nor among the project managers who have to adopt a very strict discipline with respect to industry, but also vis-à-vis their own troops at ESTEC and at ESOC.

4.4. Horizon 2000 : a reference programme

Soon after its formulation, Horizon 2000 was presented successively to the Council of ESA in Rome and in The Hague, in order to obtain the budget necessary for its implementation. I will not enter into the details of the difficulties we had with the UK delegation, but the net result of the exercise was an increase in the science budget of a net amount above inflation of 5% annually over 10 years. This was a remarkable success, since the science budget had not changed since 1971 and in view of the fact that most science budgets in the Member States were either stagnant or decreasing.

Horizon 2000 soon became an international reference in space science on which the space agencies of ESA Member States, but not only those, could base their own programmes. For the first time, ESA was in a position to coordinate its activities with the various national European and international programmes. At a series of meetings of the SPC organised on the island of Capri, ESA and its Member States regularly review the content of their respective space science programmes, on a bi-annual basis, trying to harmonise them for the sake of avoiding unnecessary duplication. This approach is proving to be quite successful after three such meetings. As shown in figures 13 and 14, the national programmes complement the ESA programme which is concerned with the bigger missions.

It is worth mentioning that, in spite of the increase in the scientific budget and taking into account the national budgets devoted to space science, the overall effort of Europe in this domain is less than one third that of the United States. This is very modest and should be stressed.

5. *The need to return to smaller missions*

Some consider that the present ESA programme gives too much importance to the cornerstones at the expense of the "flexible" element of Horizon 2000, the medium and small size missions. They want more frequent flight opportunities and consider that ESA should undertake a programme of small satellites. As is shown in figures 15 and 16, it is clear that the frequency of missions is decreasing, even though the budget has increased. This is due to the increasing complexity already mentioned, but also to that of the payloads. In fact, today, the payloads represent a non-negligible part of the missions (fig. 17) and, since they are paid by the Principal Investigators themselves, they represent a heavier burden on the national budgets.

It is indisputable that the missions of ESA, which represent the wishes of the scientists, become more and more ambitious themselves and, when the descoping which is made necessary by the design-to-cost approach cannot be implemented in a cost efficiency way, only the reliance on another partner, be it a Member State, NASA, Russia, or Japan, can make it possible to implement the mission in a satisfactory way which preserve the scientific objectives. This reliance on another partner and on its good will introduces an element of uncertainty and possibly an element of potential cost increase, another reason to claim for more simple missions.

In the course of 1991, ESA has made a study aimed at assessing the value and the realism of a small satellite programme. As usual, it consulted the scientific community, requesting it to provide ideas for small and cheap missions. The response was enthusiastic. However, a finer analysis showed that what scientists call "small" varies considerably from individual to individual! This study has shown that below 200 kg the satellites would not be able to carry a payload with sufficient scientific appeal. In addition, ESA in its present structure, with its set of very complex and stringent rules, is not the best qualified to undertake such a programme, unless the rules and structure are changed. Furthermore, from past experience, we can draw a lesson concerning the evolution of the size of missions. They seem to be well correlated with the capacities of the launching systems (which are not determined by the science missions) as can be seen in figure 15. Clearly, what would also make a small satellite programme easier to implement would be to dispose of a small launcher such as the Pegasus or San Marco Scout rockets.

Conclusion

Throughout the ESRO and the ESA eras, European space science has considerably progressed. From a state of childhood it is now at the age of majority and has reached maturity. As we have seen, European scientists are now first in several domains of space science :

- Cometary exploration,
- Astrometry,
- Heliospheric, plasma and solar physics,
- Infrared astronomy
- Planetary science.

In addition, ESA is a reliable partner. Horizon 2000 provides a reference plan and the budget structure of ESA, with a level of resources voted every three years for five years, provides a stability which has no equivalent in the world today. Given the international context, this does represent a unique advantage.

The fairly favourable situation which the scientific activities presently enjoy — as compared to other programmes such as Hermes and Columbus — can be attributed in no negligible part to the excellent spirit of cooperation and frankness which exists between ESA, the delegations at the SPC and at Council and the scientific community. The support of industry should also be mentioned as an element of support to the science programme.

In spite of the heavy administration and bureaucracy of ESA, the science programme is also an example for other programmes such as the Earth sciences and microgravity and for other agencies as well. We can be proud of this success.

SPACE SCIENCE COMMUNITY IN EUROPE

1961	**10 – 15**	**Groups of scientists carrying out experiments with sounding rockets or satellites**
	300	**Scientists**

1986	**40 – 50**	**Hardware groups, plus more than**
	100	**Group involved in observation programmes of ESA satellites**
	2000	**Scientists**

Figure 1

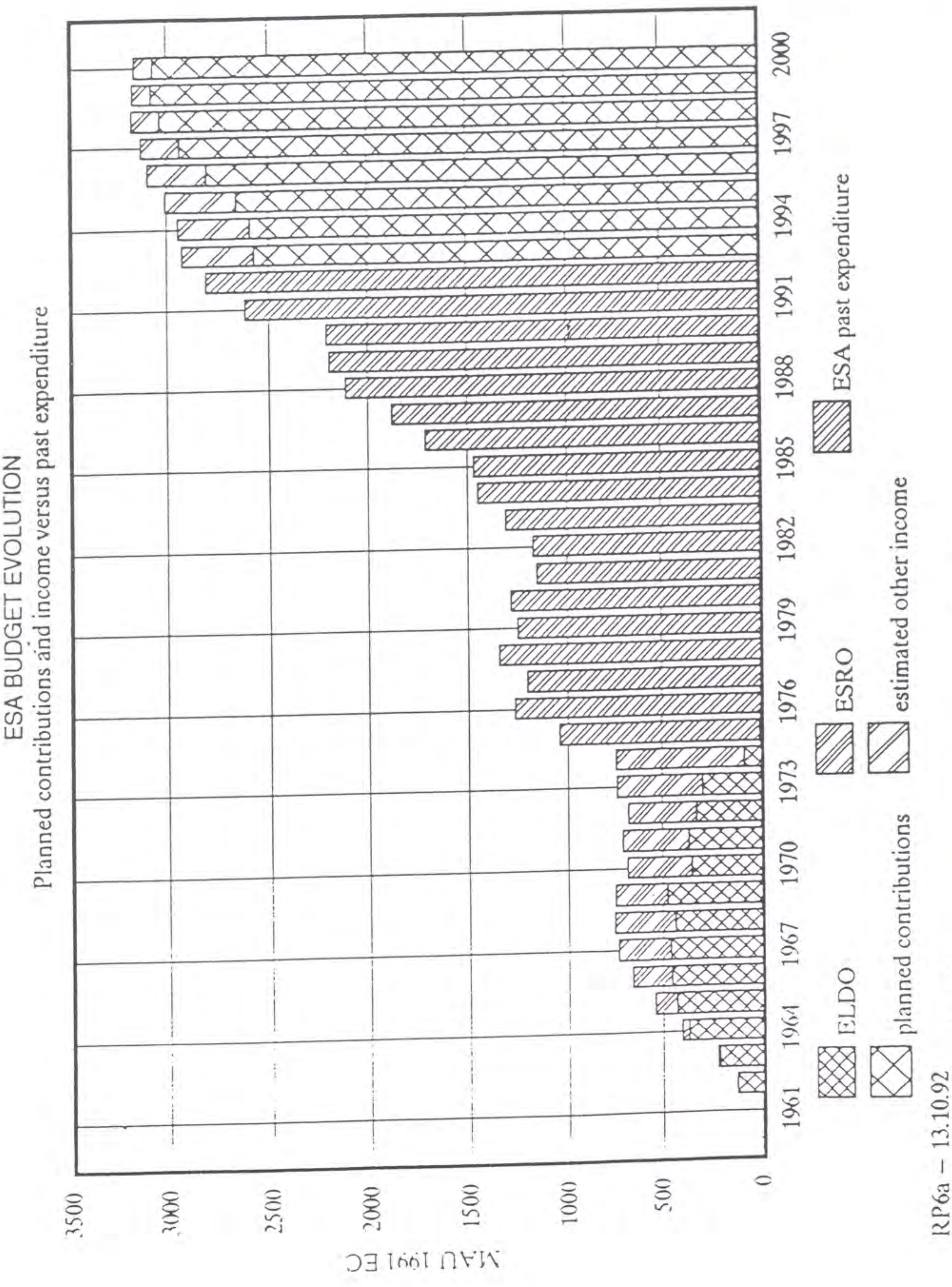

ESA BUDGET EVOLUTION
Planned contributions and income versus past expenditure
MAU 1991 EC
3500
3000
2500
2000
1500
1000
500
0
1961
1964
1967
1970
1973
1976
1979
1982
1985
1988
1991
1994
1997
2000
ELDO
ESRO
ESA past expenditure
planned contributions
estimated other income
RP6a – 13.10.92
DG/CMO/vp – 3.11.92, 13:06

Figure 2

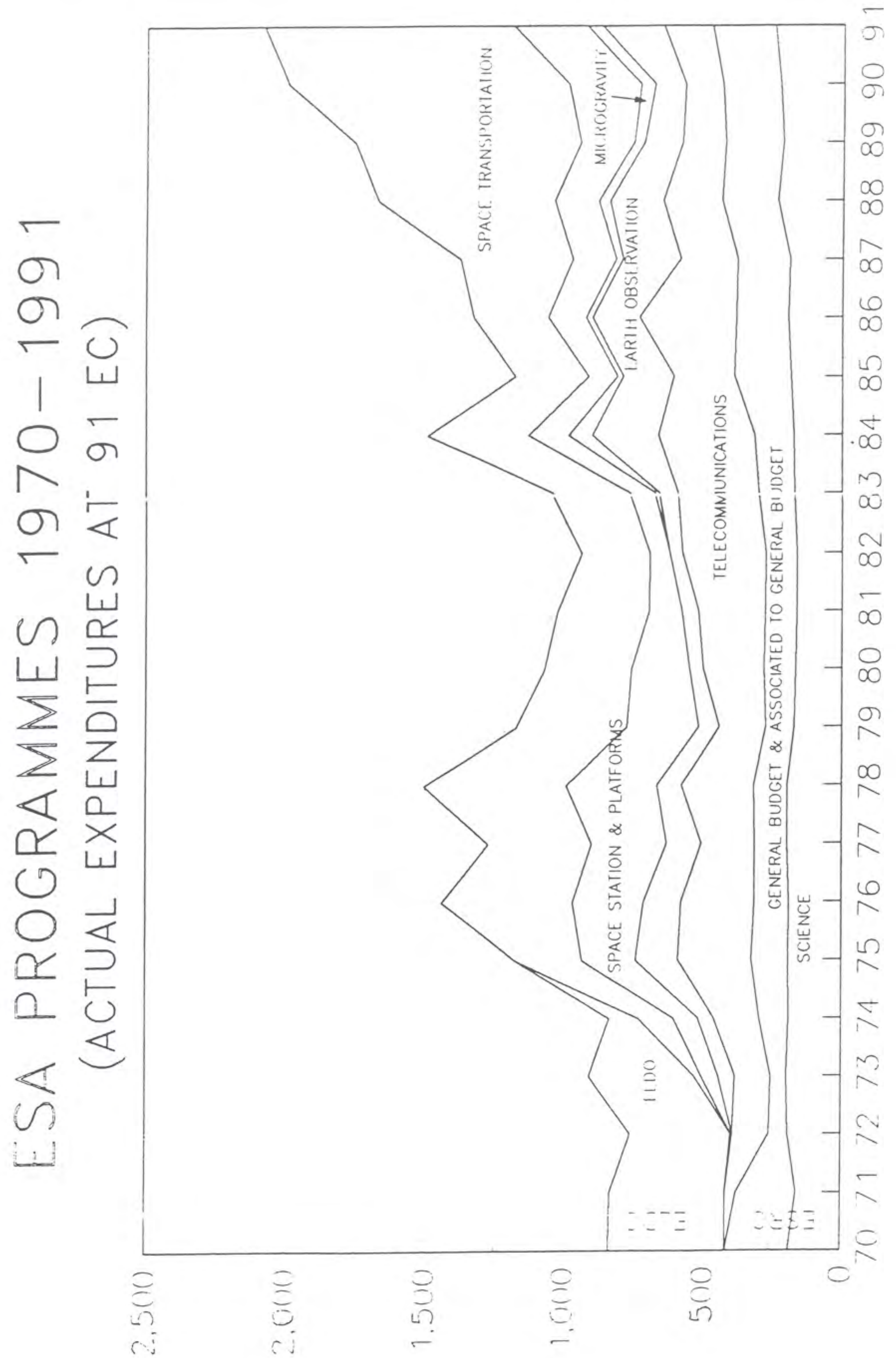

ESA PROGRAMMES 1970–1991
(ACTUAL EXPENDITURES AT 91 EC)
SPACE TRANSPORTATION
MICROGRAVITY
EARTH OBSERVATION
TELECOMMUNICATIONS
GENERAL BUDGET & ASSOCIATED TO GENERAL BUDGET
SCIENCE
SPACE STATION & PLATFORMS
ELDO
0
500
1,000
1,500
2,000
2,500
70 71 72 73 74 75 76 77 78 79 80 81 82 83 84 85 86 87 88 89 90 91

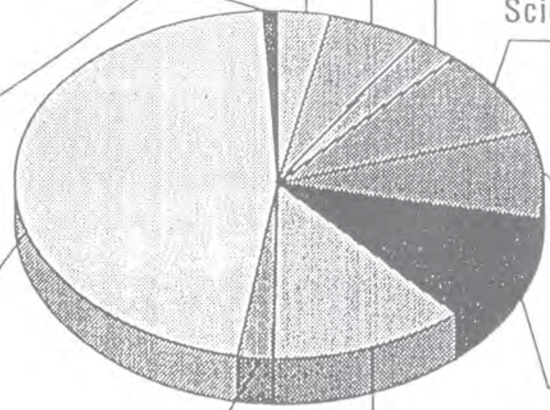
Figure 3

Figure 4

BUDGETS FOR 1992 (First revision)*

APPROVED PROGRAMMES + PROGRAMMES FINANCED BY THIRD PARTIES

TOTAL : 3229 MAU



LIKELY BUDGETS DEVELOPMENTS IN 1992	MAU
• Approved programmes	3122
• Programmes financed by third parties	107
Sub total	3229
• New budgets to be approved in 1992	39
Grand total	3268

* ESA/AF(91)30, rev.1 - June 1992

MAU : Million of Accounting Units (1 AU = 0,69 British Pounds, 1,15 US Dollars, 1,31 Canadian Dollars - Rates applicable to 1992 budgets)

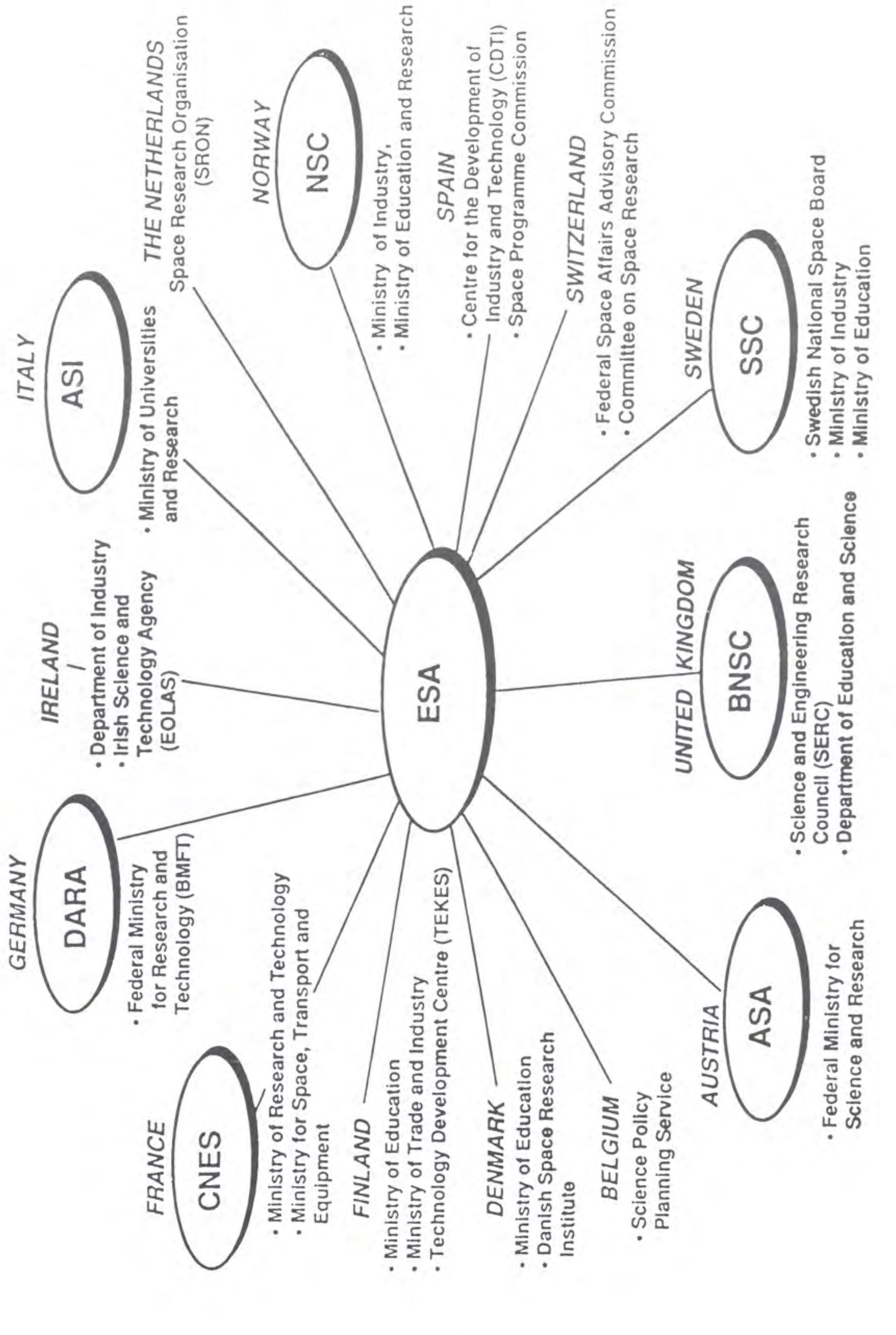

ESA
ITALY
ASI
· Ministry of Universities and Research
THE NETHERLANDS
Space Research Organisation (SRON)
NORWAY
NSC
· Ministry of Industry,
· Ministry of Education and Research
SPAIN
· Centre for the Development of Industry and Technology (CDTI)
· Space Programme Commission
SWITZERLAND
· Federal Space Affairs Advisory Commission
· Committee on Space Research
SWEDEN
SSC
· Swedish National Space Board
· Ministry of Industry
· Ministry of Education
UNITED KINGDOM
BNSC
· Science and Engineering Research Council (SERC)
· Department of Education and Science
AUSTRIA
ASA
· Federal Ministry for Science and Research
BELGIUM
· Science Policy Planning Service
DENMARK
· Ministry of Education
· Danish Space Research Institute
FINLAND
· Ministry of Education
· Ministry of Trade and Industry
· Technology Development Centre (TEKES)
FRANCE
CNES
· Ministry of Research and Technology
· Ministry for Space, Transport and Equipment
GERMANY
DARA
· Federal Ministry for Research and Technology (BMFT)
IRELAND
/
· Department of Industry
· Irish Science and Technology Agency (EOLAS)

Figure 5

Satellites scientifiques de l'ESRO déjà lancés

Satellite	Mission	Date de lancement	Fin de vie	Lanceur
ESRO-2B/ Iris	Rayons cosmiques et rayons X solaires	17.05.68	09.05.71	Scout
ESRO-1A/ Aurorae	Ionosphère polaire et phénomènes auroraux	03.10.68	26.06.70	Scout
HEOS-1	Vent solaire et particules solaires	05.12.68	25.10.75	Delta
ESRO-1B/ Boreas	Ionosphère polaire et phénomènes auroraux	01.10.69	23.11.69	Scout
HEOS-2	Magnetosphère polaire et milieu interplanétaire	31.01.72	02.08.74	Delta
TD-1	Astronomie dans l'ultraviolet	12.03.72	04.05.74	Delta
ESRO-4	Ionosphère et particules solaires	22.11.72	15.04.74	Scout

SCI 002-1 20.11.1991

Figure 6a

Satellites scientifiques de l'ESA déjà lancés

Satellite	Mission	Date de lancement	Fin de vie	Lanceur
COS-B	Astronomie des rayons gamma	09.08.75	25.04.82	Delta
GEOS-1	Magnetosphère	20.04.77	23.06.78	Delta
ISEE-2 *	Magnetosphère et relations Soleil-Terre	22.10.77	26.09.87	Delta
IUE *	Astronomie dans l'ultraviolet	26.01.78		Delta
GEOS-2	Magnetosphère	14.07.78	25.08.85	Delta
Exosat	Etude des sources célestes de rayons X	26.05.83	09.04.86	Delta
Giotto	Survol Comète de Halley (le 13.03.86, réactivé le 19.02.90)	02.07.85		Ariane
Hipparcos	Mesures d'astrométrie	08.08.89		Ariane
Télescope spatial Hubble *	Télescope astronomique de longue durée	24.04.90		Shuttle
Ulysses *	Etudes des propriétés du milieu interplanétaire et du vent solaire	06.10.90		Shuttle

* PROJET ESA-NASA

SCI 002-1 20.11.1991

Figure 6b

EVOLUTION OF SCIENTIFIC SATELLITES

SATELLITE	ORBIT (Km)	WEIGHT (Kg)	POWER (W)	BIT RATE (bits/sec)	LAUNCH DATE
ESRO-2	300/390	75	40	128	17.5.68
ESRO-1A	250/1500	86	21	320	3.10.68
ESRO-1B	300/390	"	"	"	1.10.69
HEOS-1	450/210000	108	55	12	5.12.68
HEOS-2	410/240000	117	55	32	31.1.72
TD-1	530/540	471	330	1700	12.3.72
ESRO-4	280/1100	115	60	10240	22.11.72
COS-B	350/1000	278	90	160	5.8.75
GEOS-1	35800	575	124	100000	20.4.77
GEOS-2	"	"	"	"	14.7.78
ISEE-B	283/141400	160	109	8000	22.10.77
EXOSAT	500/200000	510	330	8000	26.5.83
GIOTTO	0.9 AU	958	285	46080	2.7.85
HIPPARCOS	35800	1095	360	24000	JULY 1988
ULYSSES	Around Sun	365	285 RTG	8192	1989 ?
ISO	1000/40000	1800	792 *	47000	1992

* Solar Array provided to Space Telescope : 4.4 kW

17.07.86

Figure 7

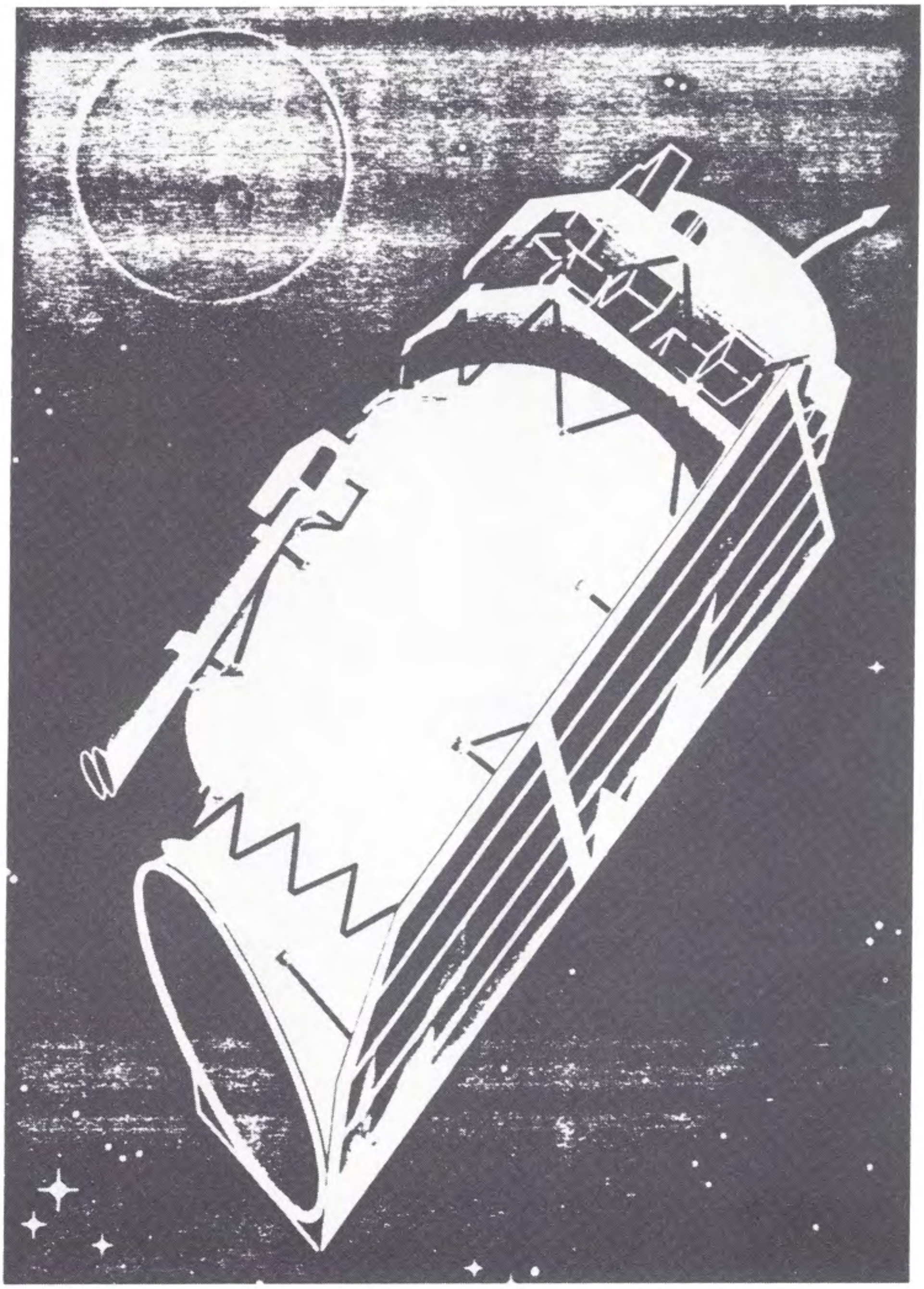


Figure 8

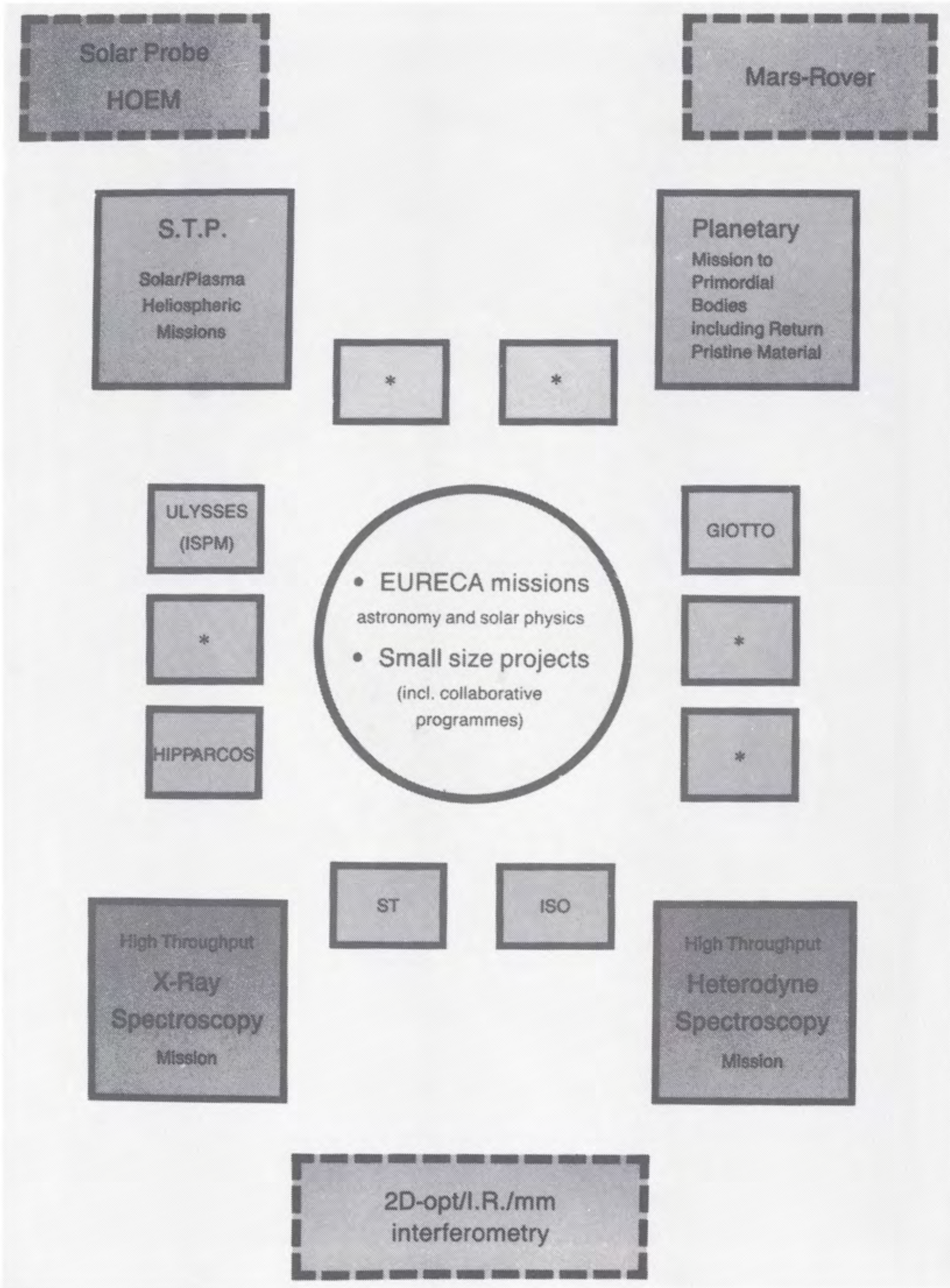

Solar Probe
HOEM
Mars-Rover
S.T.P.
Solar/Plasma
Heliospheric
Missions
Planetary
Mission to
Primordial
Bodies
including Return
Pristine Material
*
*
ULYSSES
(ISPM)
GIOTTO
• EURECA missions
astronomy and solar physics
• Small size projects
(incl. collaborative
programmes)
*
*
HIPPARCOS
*
ST
ISO
High Throughput
X-Ray
Spectroscopy
Mission
High Throughput
Heterodyne
Spectroscopy
Mission
2D-opt/I.R./mm
interferometry

Figure 9

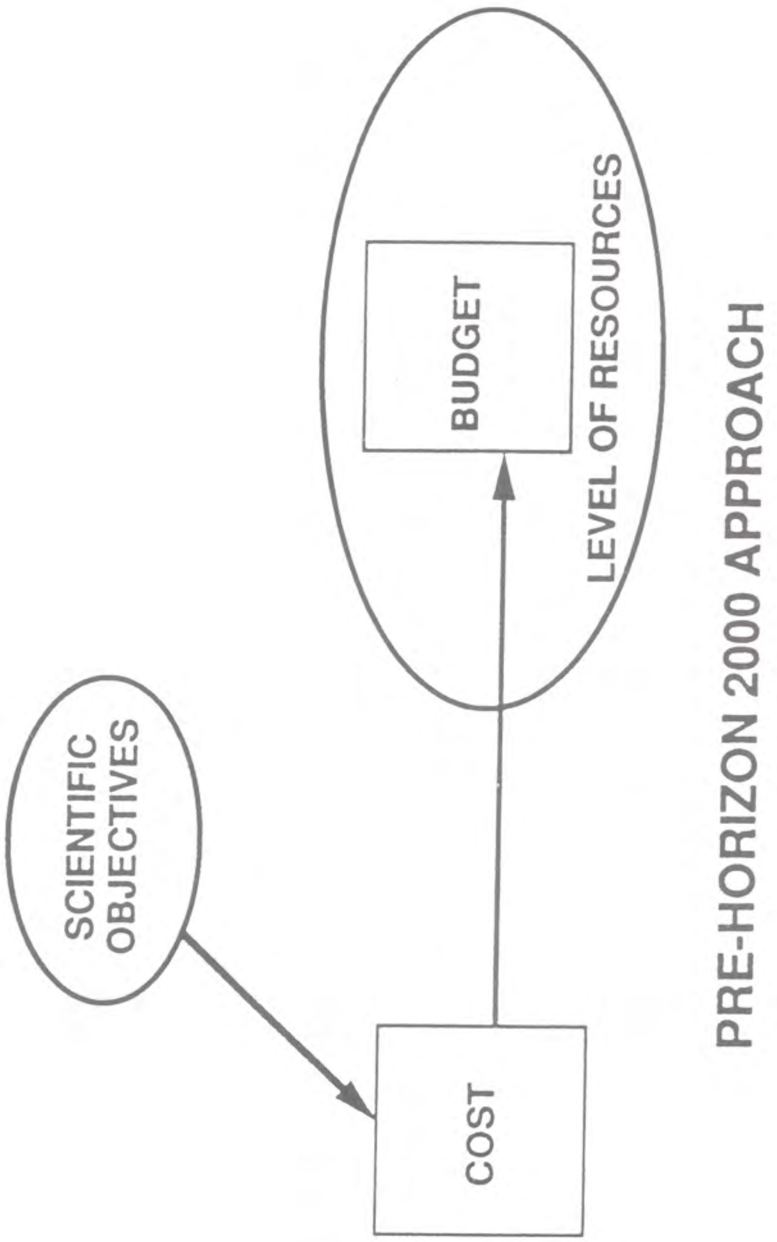

SCIENTIFIC OBJECTIVES
COST
BUDGET
LEVEL OF RESOURCES
PRE-HORIZON 2000 APPROACH

Figure 10

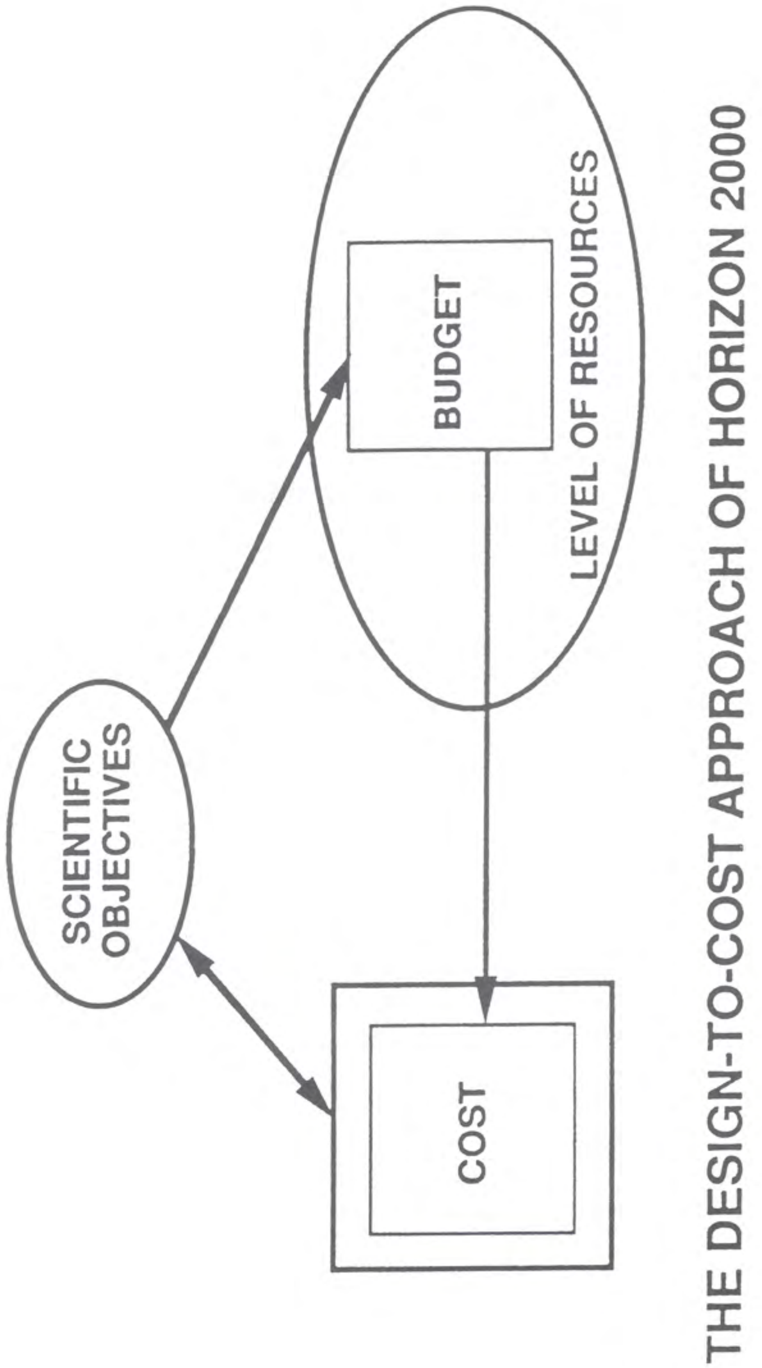

SCIENTIFIC OBJECTIVES
BUDGET
LEVEL OF RESOURCES
COST
THE DESIGN-TO-COST APPROACH OF HORIZON 2000

Figure 11

APPROVED NON-ESA ASTRONOMY MISSIONS
EUROPEAN PARTICIPATION

	A	B	CH	D	DK	E	EIR	F	GB	I	N	NL	S	SF
Radio														
- RADIOASTRON (USSR//US) (1994-95?)				d					gb			nl		sf
Visible/UV														
- IEH-Hitchhiker (I/US) 1992										i				
- GAUSS (D) 1992				d										
- FAUST (F/NASA) 1992								f						
- SPECTRUM-UV (I/USSR) 1994/95										i				sf?
- EVRIS/Mars-94 (USSR) 1994	a							f						
Visible/UV/EUV/X-Ray														
- ORFEUS (D/NASA) 1992				d										
EUV/X-Ray/Hard X-Ray/Gamma														
- ROSAT (D) 1990				d					gb					
- SAX (I/NL/D) 1993				d						i		nl		
- WATCH/EURECA (1993)					dk									
- SPECTRUM-X (USSR/NASA) 1994/95			ch	d	dk	e			gb	i				sf
- AXAF (NASA) 1998				d					gb	i		nl		
- FUSE (NASA) 1999								f						
Hard X-Ray/Gamma														
- EGRET 1990				d										
- GRANAT (USSR/F) 1990								f						
- GAMMA-1 (USSR/F)								f						
- COMPTEL/GRO (NASA)1990				d								nl		

Figure 13

APPROVED NON ESA SOLAR SYSTEM MISSIONS
EUROPEAN PARTICIPATION

	A	B	CH	D	DK	E	EIR	F	GB	I	N	NL	S	SF	ESA
Minor Bodies															
- CRAF (US/D)	a			d			eir	f	gb	i					esa
Plasma/Fields/ Particles															
- REGATTA (USSR)		b						f					s		
- FREJA (S/D)				d	dk						n		s	sf	
- WIND (USA)			ch	d				f							esa
- POLAR (USA)			ch					f	gb		n		s	sf	esa
- CRRES (USA)				d	dk				gb		n				
- INTERBALL (USSR/S)	a							f		i			s	sf	
- TIROS (USA/N)											n				
- WISP (USA)													s		
- GEOTAIL (J/USA)				d									s		
- TSS (USA/I)				d				f		i					esa
Planets															
- MAGELLAN (USA) 1990								f	gb						
- GALILEO (USA/D) 1990				d				f	gb	i					
- MARS-OBS (USA) 1992	a			d		e		f	gb						
- MARS 94 (USSR) 1994	a	b		d		e	eir	f	gb	i			s	sf	esa
Sun															
- SOLAR A (J)									gb						
- KORONAS (USSR)								f							
- ATLAS (USA)		b						f							esa

Figure 14

Figure 15

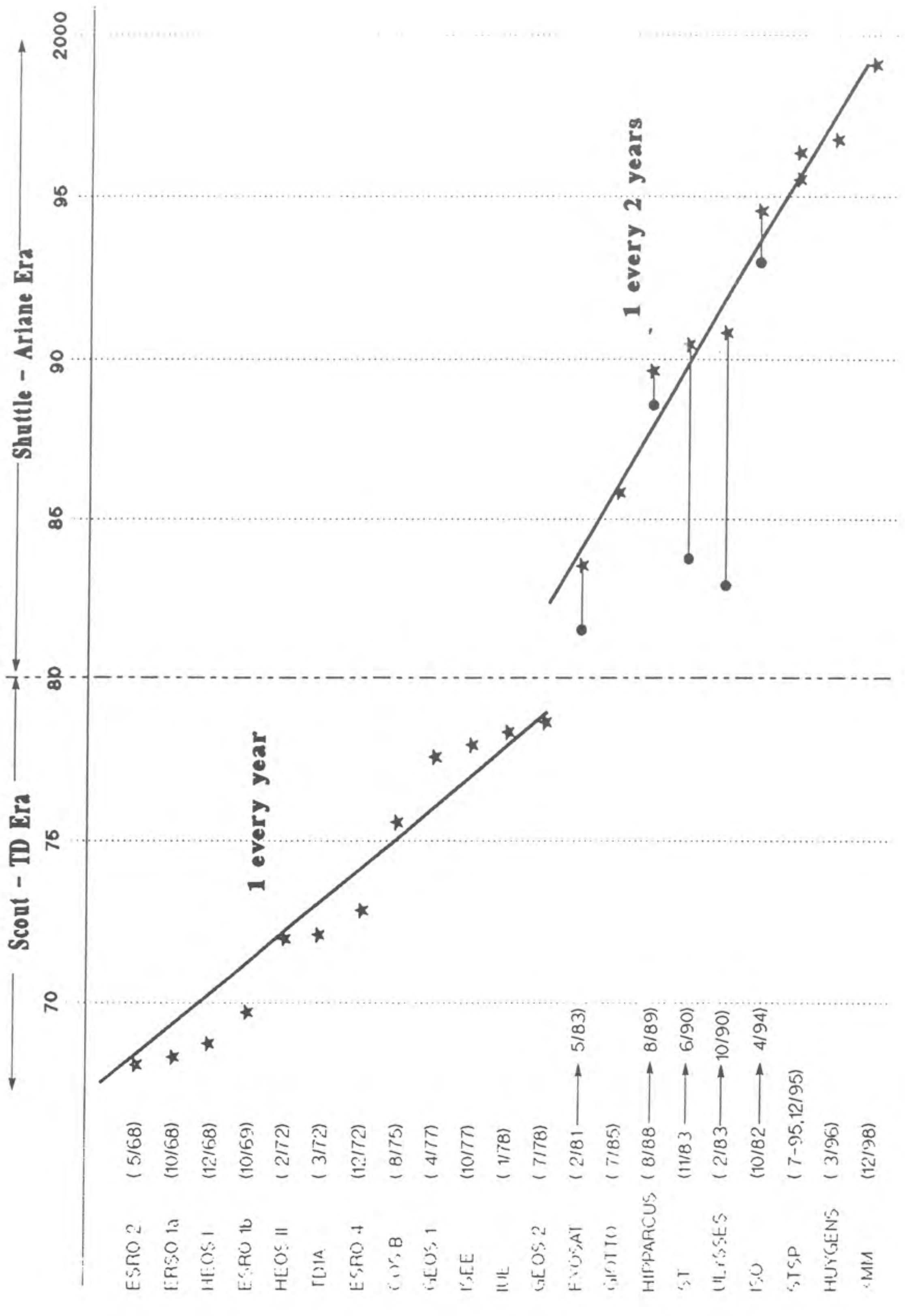
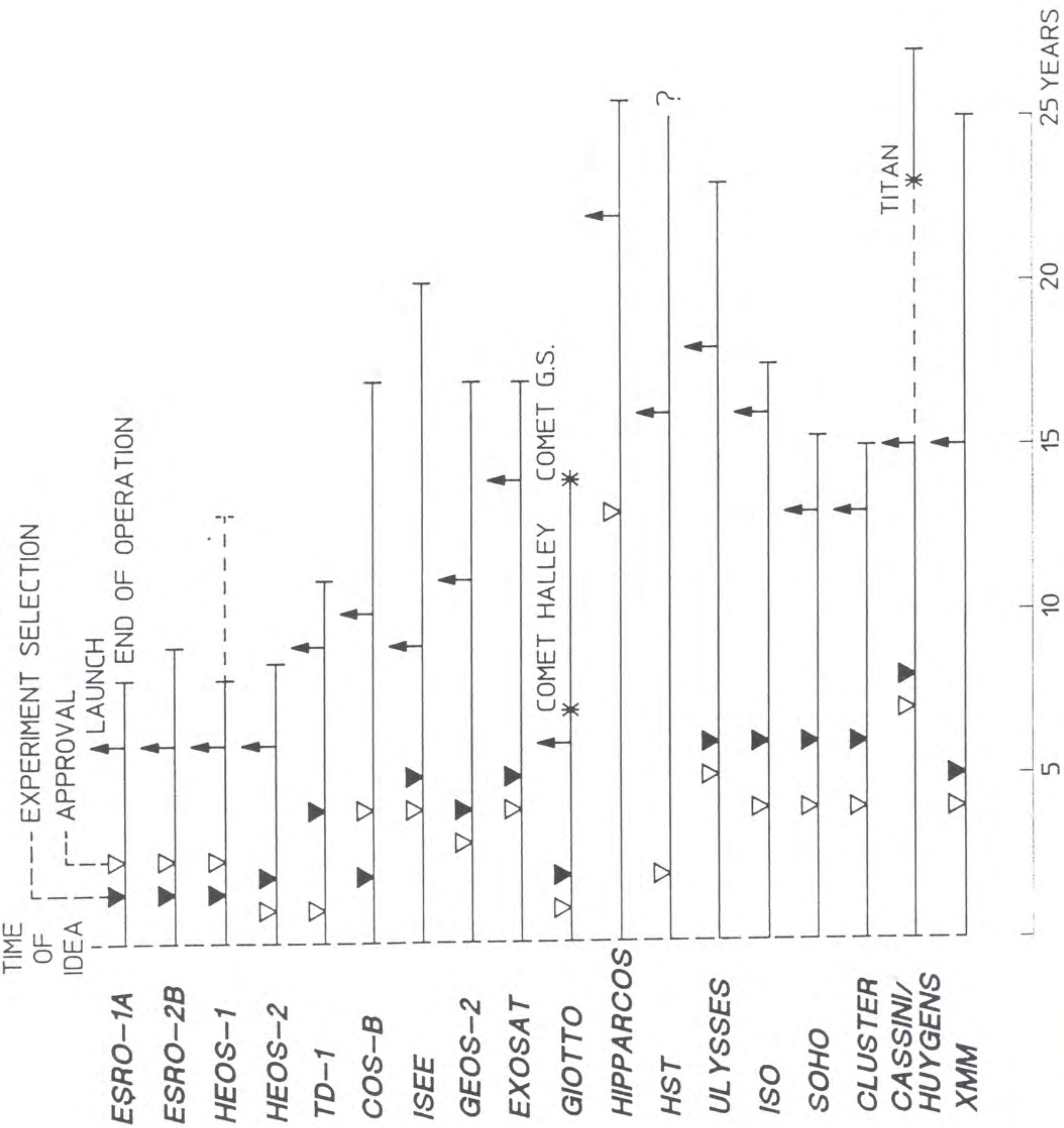

Scout - TD Era
Shuttle - Ariane Era
70
75
80
85
90
95
2000
1 every year
1 every 2 years
ESRO 2 (5/68)
ERSO 1a (10/68)
HEOS I (12/68)
ESRO 1b (10/69)
HEOS II (2/72)
TD1A (3/72)
ESRO 4 (12/72)
COS B (8/75)
GEOS 1 (4/77)
ISEE (10/77)
IUE (1/78)
GEOS 2 (7/78)
EXOSAT (2/81 → 5/83)
GIOTTO (7/85)
HIPPARCUS (8/88 → 8/89)
ST (11/83 → 6/90)
ULYSSES (2/83 → 10/90)
ISO (10/82 → 4/94)
STSP (7-95,12/95)
HUYGENS (3/96)
XMM (12/98)

Figure 16


TIME OF IDEA
EXPERIMENT SELECTION
APPROVAL
LAUNCH
END OF OPERATION
ESRO–1A
ESRO–2B
HEOS–1
HEOS–2
TD–1
COS–B
ISEE
GEOS–2
EXOSAT
GIOTTO
HIPPARCOS
HST
ULYSSES
ISO
SOHO
CLUSTER
CASSINI/HUYGENS
XMM
COMET HALLEY
COMET G.S.
TITAN
?
5
10
15
20
25 YEARS

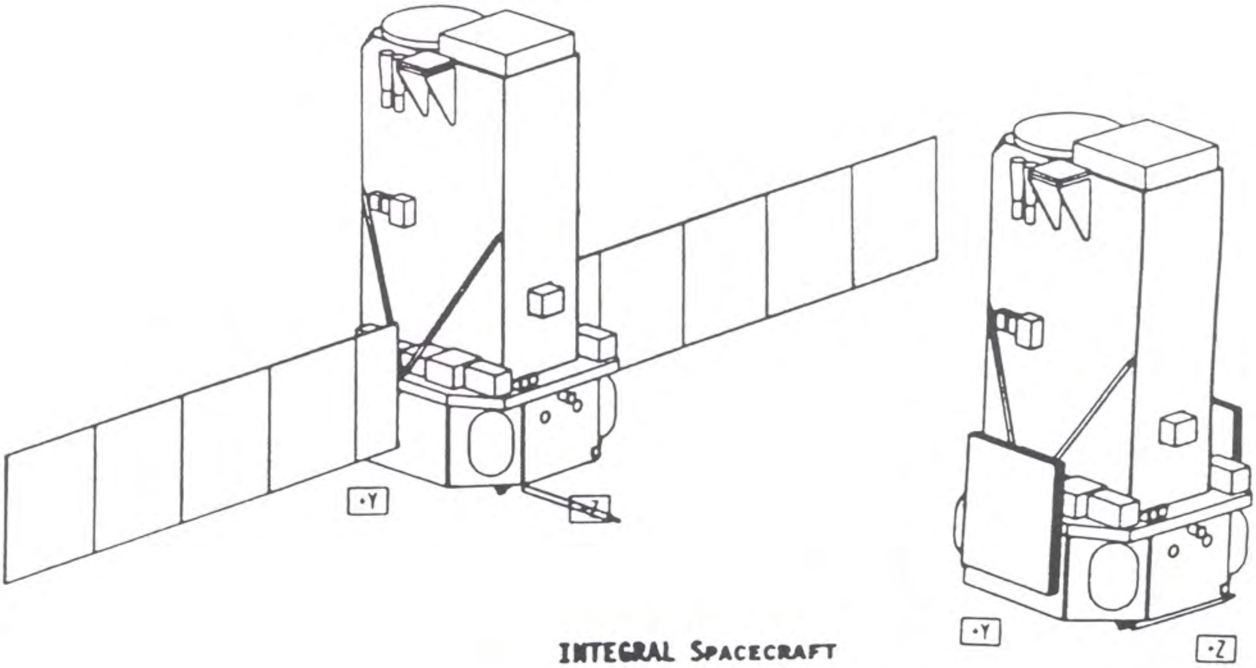
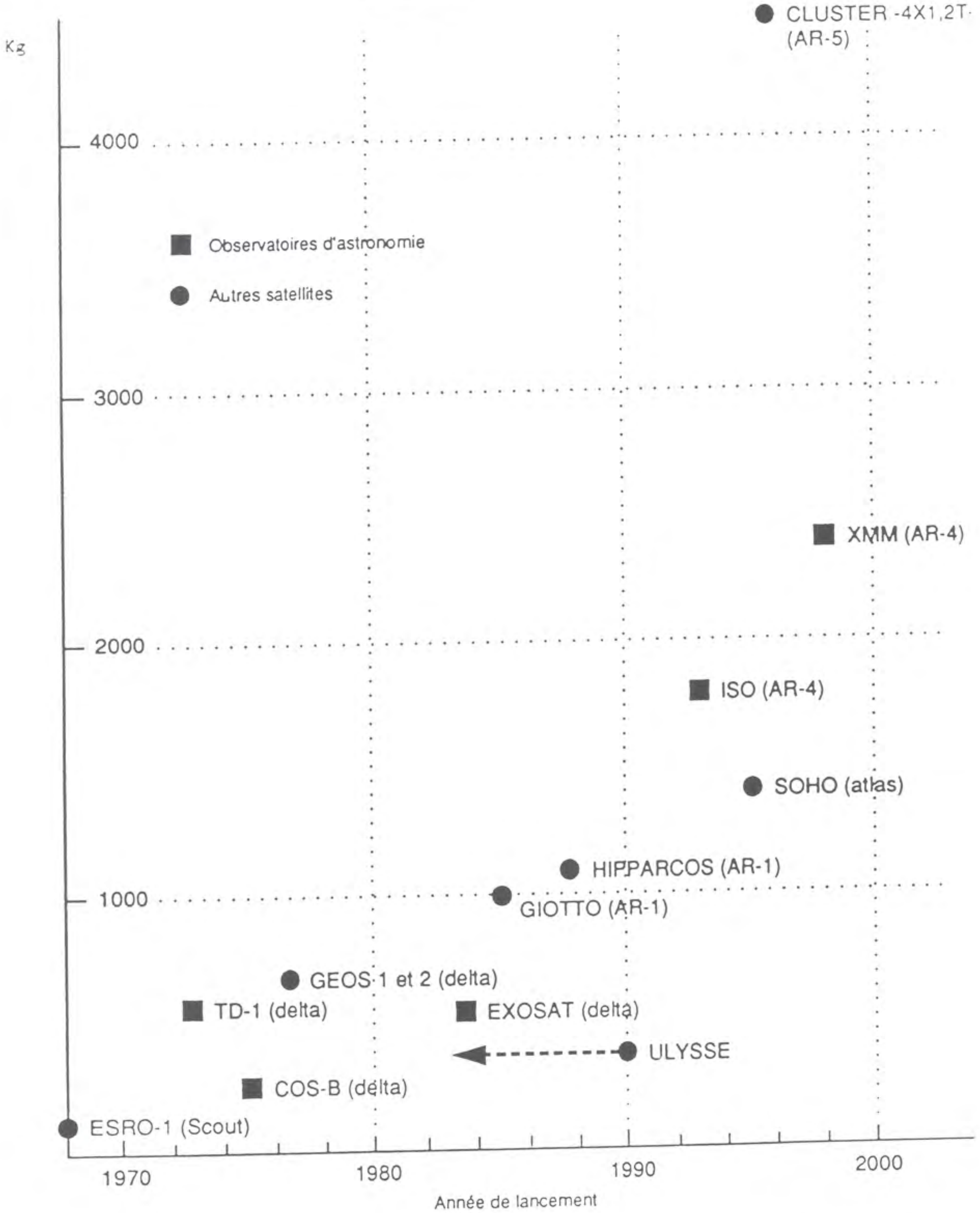


Figure 17

Figure 18



Evolution du poids des satellites scientifiques de l'ESA

How space scientists and governments saw ESRO in the early 1960s

John Krige
European University Institute, Florence

My aim in this brief paper is to identify and to analyse some of the key differences between ESRO and CERN in the early 1960s. Comparisons between these two 'sister' organizations are not new, of course. But they have tended simply to *list* similarities and differences, rather than to use them to answer specific historical questions. Massey and Robins provide an excellent case in point.[1] They identify four main differences between the space research organization and the high-energy physics laboratory:

- whereas high-energy physics is a single, highly demanding discipline, space research covers a number of very different disciplines;[2]
- whereas CERN had a central facility dedicated to scientific research which provided scientists with most of their experimental equipment, ESRO did not. Instruments were built in national institutes and the most important central establishment (ESTEC) was dedicated to satellite engineering and payload integration;
- whereas CERN was based on a single geographical site, ESRO's establishments were dispersed over several different sites;
- space research, unlike high-energy physics, had potentially profitable applications.

The tendency to draw up such lists is widespread — how many times have I not fallen into the trap myself! — and is to be resisted. For while undoubtedly suggestive, lists of "factors" are also extremely misleading. They are an ahistorical answer to an ahistorical question, the question: How does ESRO differ from CERN? The question is ahistorical because it overlooks the fact that the priorities of the key actors involved in shaping these organizations differed from one another, that there were no single, timeless objects called "CERN" or "ESRO" to be compared and contrasted. The list is ahistorical because it abstracts certain considerations from the flux of events and bestows on them a transhistorical importance. It detaches them from the persons and contexts in which they can, or did, shape behaviour and locates them in a disembodied, depersonalized world. For example, it is surely one thing to say, with Massey and Robins, that space research, unlike high-energy physics had potentially profitable applications. It is quite another to argue that that

---

1 Massey and Robins (1986), 111.

2 Russo develops this in his contribution to this collection. See also Russo (1992).

was of concern above all to governments when deciding how to distribute resources between European and national space efforts in the early 1960s. And that it was later to cause them, with the support of the senior management of ESRO, and to the considerable dismay of parts of the scientific community, to reorient ESRO's main mission. An abstract "difference" becomes concrete when related to the thinking of particular groups of actors taking decisions and formulating policies at a particular moment in time. As such it serves to relativize and contextualize their behaviour, to situate it historically.

The differences which I want to explore here are those in the attitudes of scientists and governments to the funding of ESRO when it was established, as compared to their attitudes to CERN at the same time. The point that I want to stress, and to explain, is that whereas governments, with scientific support, succeeded in imposing a set of firm ceilings on ESRO's expenditure, precisely the opposite happened at CERN. True, in the latter case, the UK government did try (once again) in 1961 to lay down a firm budget envelope for the laboratory. But they failed, and CERN, rather than being forced to adapt its programme to levels of expenditure fixed years in advance, planned on having a steady rate of growth for the foreseeable future. Some of the reasons for this are "hidden" in Massey and Robins' list. Others are peculiar to this debate. Indeed I will show that it was partly *because* of their defeat at CERN that the British, with the support of the other ESRO member states, took such an uncompromisingly firm attitude on the cost profile of the future space research organization.

A brief chronology[3]

In March 1961 about a dozen European governments set up a preparatory commission to plan ESRO (the COPERS). Its task was, above all, to define the scientific programme, the organization and the budget of the envisaged body, and to present the draft of a convention for signature by the participating member states. The commission in turn set up two working groups. One, composed primarily of scientists and engineers, was to define and to cost its scientific and technical programme (the STWG). The other, composed primarily of civil servants, was to handle legal, administrative and financial matters (the LAFWG).

Within a few weeks the scientists had sketched a scientific programme which, they felt, was suitable for a European collaborative effort, and had made a first estimate of expenditure over the first eight years of its life. It is shown in column 2 of Table 1. As we can see from columns 3 and 4 of this table, these initial figures were revised upwards in the months that followed, though by less than 10%. If we include the cost of a headquarters building in the scientists' figures, we arrive at a global estimate of some 1500MFF (million new French francs) for the eight years.

---

3 For a more detailed chronology see Krige (1992) and Krige (1993). The budget debate is detailed in section 4 of the latter.

Table 1 Estimates of the costs of ESRO (in million of new French francs) made in anticipation of the signature of the convention by the scientists in the interim STWG of the COPERS. The scientists did not include estimates of the costs of the headquarters in their figures; the figures arrived at by the LAFWG's budget subgroup (83.7 MFF over the eight years) have been added to the STWG's January 1962 estimates for completeness (column 5)

Year	STWG May 1961	STWG Blue Book October 1961	STWG January 1962	STWG January 1962 + Headquarters
1	61.9	69.7	73.8	82.4
2	123.0	121.1	104.9	115.5
3	150.6	148.7	157.5	169.0
4	156.0	162.2	168.2	177.7
5	172.5	181.2	187.7	198.5
6	232.1	243.2	262.0	272.8
7	232.1	243.1	259.4	270.3
8	232.1	242.6	259.6	270.6
TOT	1360.3	1411.9	1473.1	1556.8

Sources:
Column 2: COPERS/GTST/20, 11/5/61; Column 3: *Blue Book*, data laid before the third session of the COPERS in Munich on 24-25 October 1961, pp. 18-9; Column 4: COPERS/GTST/30, 22/1/62; Column 5: as for column 4, with LAFWG's data on headquarters taken from same source. All documents are in the ESA Archives, Villa Il Poggiolo, EUI, Florence.

The scientists' estimates were considered by the science administrators in the budget subgroup of the LAFWG. They were convinced that they were unrealistically low. On the basis of NASA's experience alone, they insisted, it was impossible to give meaningful estimates of forward expenditure beyond the third year of ESRO's life. If one wanted to arrive at "realistic" cost estimates one had to leave a margin for unexpected contingencies. One also had to make some allowance for growth in years 7 and 8, when ESRO would be laying the foundations for its next 8 year programme.

The budget subgroup's proposals are shown in Table 2. The administrators recommended an increase in overall expenditure above the scientists' proposals of about 30% to more than 2000 MFF.

Table 2 Estimates of the costs of ESRO made by the LAFWG's budget subgroup, as compared to those made by the scientists in the STWG. The last column is the budget levels calculated by the administrators assuming a 20% contingency for unforeseen developments, and increases of 15% in years 7 and 8 to allow for growth in the organization

Year	STWG January 1962 + Headquarters	LAFWG January 1962	LAFWG March 1962 + 20% (con) + 15% (7,8)
1	82.4	89.6	107.5
2	115.5	135.8	163.0
3	169.0	185.2	222.2
4	177.7	190.4	228.5
5	198.5	212.6	255.1
6	272.8	285.7	342.8
7	270.3	281.8	389.6
8	270.6	283.1	398.1
TOT	1556.8	1664.2	2106.8

Sources:
Column 2: COPERS/GTST/30, 22/1/62, with budget subgroup's data on headquarters taken from same source; Column 3: COPERS/GTST/30, 22/1/62; Column 4: Report of the budget subgroup, COPERS/AWG/II/2/Rev. 1, 19/3/62, Annex II, Summary Table S5. All documents are in the ESA Archives, Villa Il Poggiolo, EUI, Florence.

In parallel with these discussions the British delegation, with considerable support from the French and some of the small countries, was proposing that very strict measures of financial control be imposed on ESRO's budgets. In particular they wanted a firm ceiling for the eight years to be agreed between ministers, and within that ceiling they wanted equally difficult-to-dislodge ceilings on expenditure imposed for each three-year block of ESRO's life. The firm ceiling that they recommended was 1500 MFF — roughly the figure recommended by the scientists themselves, though far below the figure thought reasonable by the administrators.

How did the scientists react to this? An ad hoc committee, chaired by Henk van de Hulst, was set up to look into the matter. Their conclusion, delivered in March 1962, was that the ceiling of 1500MFF was acceptable (see Figure 1). More specifically, they emphasized that their initial programme was simply indicative, that their cost estimates were likely to vary in both directions and that it was "improbable but not impossible" that their draft launching programme could be achieved within the 1500 MFF ceiling. If economies were needed, they added, they could be made by reducing the launch rate, and without seriously jeopardising the European space effort.

At first sight this is a surprising reaction. In particular it is surprising when compared to the attitudes adopted by scientists and engineers at CERN to the attempts made by governments to impose limits on their expenditure. From 1957 onwards, as first the synchro-cyclotron and then the proton synchrotron, were commissioned, the CERN member states were faced by escalations in the costs of the laboratory far beyond what they or the inhouse staff had expected. The British were particularly concerned about this, trying time and again to get the CERN Council to impose a limit on forward expenditure. Faced with these threats to the build-up of their laboratory, the physicists and engineers went onto the attack. Far from accepting cuts, they insisted that reductions in expenditure would ruin the laboratory. To give the flavour of the debate here is part of a statement made by CERN DG Victor Weisskopf to the Council towards the end of 1962:

> "CERN, however, is a special laboratory with one definite purpose, namely the exploitation of two machines. If this is the case, there exists a natural expansion coefficient: the two machines must be exploited and this requires very definite steps to be taken — to build a second area, to construct a number of bubble chambers, to get beam transport equipment, etc. All this should be done as soon as possible, that is with an increase of manpower which the Laboratory can absorb. The last years have shown that 13 to 15% per annum is a reasonable increase and is in line with the recommendation of the Report of the Working Party on the CERN Programme and Budget: it is less than the actual growth in any year before 1961.
> This natural growth will now be retarded. We will not be able to serve European physics as well as we could and our position as equal partners to other high-energy laboratories will be severely threatened."[4]

Weisskopf's line of argument was typical of that adopted by the management for the previous five years.[5] Far from accepting that the CERN programme could be achieved within the reduced budgets being called for by the member states, they were emphatic that the laboratory

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[4] CERN Council Minutes, 1962, p. 57.

[5] I stress this point because some participants in the symposium suggested that it was inappropriate to expect the young space science community to argue with the same flair as the charismatic and persuasive Weisskopf. While this is true, it seems to me to be irrelevant to my point. The substance, if not the form of Weisskopf's speech (which may well have been mostly written by his right-hand man Mervyn Hine anyway) was completely coherent with the position adopted from 1957 onwards by far less charismatic members of the CERN management whenever they saw their budgets being threatened by the Finance Committee or the Council.

would he harmed. What is more they always tried to shift the responsibility for the damage which, they insisted, would ensue on budget cuts, onto the shoulders of the member states' delegates themselves. It was they, and not the CERN management, who would have to take the blame if the laboratory failed to achieve its objectives.

Why this difference in approach? Why were CERN physicists determined to squeeze every Swiss franc out of the participating states while the space science community were willing to accept figures for their new facility well below what the science administrators themselves in the COPERS were recommending? How is it possible that the scientific community, rather than fighting for the higher figure, could accept the lower one with such ease?[6]

The attitudes of the scientists

Three main considerations explain the relative compliancy of the space scientists regarding the ceilings imposed on ESRO's budget in 1962. Firstly, as they themselves have put it, this was a "euphoric" (van de Hulst) time, a time in which a group of relatively young people saw an entire new range of possibilities opening up for them. They were about to enter a world in which they would regularly rub shoulders with the highest government officials, a world in which their discipline would become explicitly associated with overall national objectives and policies. Excited and perhaps overawed by what lay ahead, they were, as Lüst — who was in his mid-30s at the time — put it, prepared "to take what they could get".[7] At the same time it must be added that the British sector of the community, led by Harrie Massey, who was chairman first of the COPERS and then of the ESRO Council, had made it quite clear that they would not support a figure of more than 1500MFF for ESRO's eight-year ceiling. Freddy Lines, from RAE Farnborough, who was to become ESRO's first Technical Director, and Robert Boyd, from University College, London, who championed ESRO's biggest single project, the large astronomical satellite, were also convinced that the early figures proposed by the scientists should not be exceeded. Without the support of one of the most powerful and influential groups within the community, space scientists on the

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6 One suggestion made at the symposium must be dismissed immediately. It was that whereas the space programme comprised a number of separate elements — over 400 sounding rocket launchings, plus some 18 satellite launchings — the CERN programme was an integrated whole ("90% of a proton synchrotron is not a viable proton synchrotron; 90% of a space programme is a viable programme".) This observation misses the point. The debate at CERN, as we can see from Weisskopf's statement, was over exploiting the accelerators, not over building them, and the staff were determined to get the biggest and widest range of equipment that they could — even if that meant duplicating material being built by national groups in the member states and specifically destined for use at CERN! See Krige and Pestre (1986).

7 The comments by van de Hulst and Lüst were made in the debate on this paper at the Palermo conference.

continent could hardly hope to put up a meaningful fight for higher budget figures even if they had wanted to.

A second important factor shaping scientists' attitudes was the fact that ESRO (unlike CERN) was not to be a centre which could compete on an equal footing with similar facilities in Europe or in the United States. The determination to match what the Americans could do was one of the main forces driving scientists and governments alike to commit resources to CERN. The situation in ESRO was quite different. It was intended to provide engineering support for a community most of whose instruments would be paid for and built by universities and national institutes. The amount of inhouse scientific activity was to be kept to the minimum compatible with the need to provide a sound interface between the scientists in the member states and the engineers at the space technology centre (ESTEC). In particular the scientists who founded ESRO were emphatic that it should not compete with national research activities, only complement them. As for the United States, it was clear that Europe could not be an "equal partner" (Weisskopf), could not match the huge American space effort in the 1960s. Competition there would be, but in specific, relatively less ambitious sectors to be determined by scientific interest and available resources.

Finally, scientists knew that ESRO would not be the only organization through which they would be able to launch payloads. There were two other alternatives which also merited financial support: national programmes and cooperation with the USA. The need to preserve and to extend the former was central in Massey's thinking when, in summer 1961, he presented his government with the very earliest estimates of the cost of ESRO. That programme, he said, was to be funded in parallel with a national programme based on *Skylark* sounding rockets and the *Ariel* series of scientific satellites.[8] The Italians, too, felt that it was important to protect their national space effort, even if that meant holding back from the international initiative. As Bonetti, Dilworth and Occhialini wrote to Amaldi as late as December 1964,

> "A courageous and imaginative policy is called for. It would seem advisable, in the present situation, to postpone adhesion to ESRO and to add the money set aside for the Organization to a substantial independent Italian budget for funding a national experimental programme. At the same time, to support Italian research and Italian industry, one must organize a major project in which several billion [lire] are invested, and which, through being of general interest (i.e. not only for Italy), could guarantee our future adhesion to ESRO, and make our collaboration possible." [9]

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8 See Massey and Robins (1986), 111, 123-4.

9 Letter A. Bonetti, C. Dilworth and G. Occhialini to E. Amaldi, Box 119, Amaldi Archives, University of Rome "La Sapienza". (My translation.) I would like to thank Michelangelo De Maria for supplying this document.

Of course when CERN was set up major countries were also determined to have national programmes. But in some cases (e.g. France) the laboratory was not funded from the science budget, but was seen as an arm of foreign policy and paid for by the Quai d'Orsay. In others (e.g. Italy) there was a deliberate policy to treat CERN as the single most important centre for Italians to do physics. The national programme was to be subordinated to it. It was only in the case of Britain (among these three) that the national physics programme had to compete directly with the international programme. This partly explains the ambiguous approach by both UK scientists and their government to the laboratory, at least until the 1970s. That same ambiguity informed the attitudes of scientists in all three countries, Britain, France and Italy, when ESRO was set up.

There was also the American option. At an international meeting in the Hague in March 1959, the US delegate stated that his country favoured bilateral arrangements with scientists from other countries who wished to fly equipment on NASA rockets. Researchers who wanted to build single instruments to be integrated into a larger payload were "invited to work in a US laboratory on the construction, calibration, and installation of the necessary equipment in a US research vehicle." Those who wished to build an entire payload themselves could count on NASA's advice on the feasibility of the proposed experiments, on the design and construction of the payload package, and the necessary pre-flight environmental testing. NASA even went as far to suggest that it would be prepared to launch some European satellites free of charge.[10]

This was an immensely seductive offer. It not only gave European scientists access to American scientific and engineering know-how, it also gave them access to US launchers. It was a quick, cheap, and highly efficient way of entering scientific space research and developing national space capabilities. Scientists were quick to capitalize on it. Indeed in 1961 both Harrie Massey for Britain and Jacques Blamont for France had started discussions with the Americans on the possibilities for collaboration. A year later Luigi Broglio in Italy was working out a collaborative scheme for the *San Marco* programme with the USA. In sum, from the start funding for ESRO had to be balanced, at least for scientists in some of the more powerful member states, against funding for programmes both at home and across the Atlantic. The importance they attached to "fighting" for its budget was correspondingly muted.

The attitude of governments

Three main considerations explain the determination shown by governments to keep ESRO's budget firmly under control. Firstly, there was the experience they had had at CERN. For four or five years national bureaucracies had watched with dismay as the costs of the Geneva laboratory spiralled. Led by the British, some had waged a forceful campaign to impose a measure of finan-

10 For the American offer see Massey and Robins (1986), Appendix 4.

cial control over expenditure. Each time they were rebuffed. It was impossible, they were told, to estimate the costs of research and development in advance: imposing ceilings would be counter-productive and would stifle innovation. Increasingly frustrated by the failure of their Council delegates to win support for greater financial discipline, the UK government took the unprecedented step of suggesting that a three-year envelope of expenditure be set for CERN between governments themselves, leaving the Council simply to vote the annual budgets within the limit agreed. In December 1961 senior members of the Council, furious at this assault on their authority, humiliated and blackmailed the British delegation into submission.[11] ESRO's convention was being drafted as this debate was being held. The lesson was clear. This time the British were emphatic that some ceilings be legally enshrined in the financial protocol, and in particular that ESRO be constrained to spending 1500 MFF (in 1962 prices) over the first eight years of its life. They also insisted that the Council pass important budget votes unanimously or, in some cases, by a two-thirds majority. There was no question of having annual budgets agreed by a simply majority, as was the case at CERN. The British proposal was quickly accepted by the other member states, some of whom explicitly expressed the wish not to repeat the experience they had had in Geneva. Governments also learn!

The desire to build up an autonomous, national capability in space technology, also undoubtedly led governments to be cautious about how much they committed to international collaboration. Space, unlike high-energy physics, was a field of enormous potential commercial and military importance. Many countries, including some of the smaller ones, were keen to build up the inhouse engineering knowledge which could be readily transferred between science and applications. They saw ESRO as a means to a more ambitious end, as part of a national strategy to build industrial competitiveness through collaborating with others in the same area. For them, as for many of the scientists, it made no sense to build an international organization without at the same time devoting considerable resources to a national effort.

This leads me to one last, related comment. If CERN was able to flourish unimpeded for much of the 1960s, while ESRO was born in a financial straightjacket, it was also because at the core of its Council there was a set of delegates who saw their role, not simply as defending the interests of their governments at CERN, but also as defending the interests of CERN before their governments. ESRO never had the benefit of this "lobby", a mixed group of senior scientists and civil servants who were prepared to protect the laboratory from criticism, and to fight for its growth at all costs.[12] Nor could it. The stakes in space were far higher, and no government was going to relinquish its sovereignty over the benefits which it hoped to reap from membership of

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11 For this conflict see Krige in Hermann et al. (1990), section 10.3.

12 For more on the behaviour of this lobby see Pestre in Hermann et al (1990), section 7.1.

ESRO. There was no ambiguity here about the role of national delegates: they were there to protect their governments' interests. Keeping a firm control over the budget (and heavily penalizing ESRO for its lack of financial discipline) were two of the classic ways of doing so.

Bibliography

Krige (1992)
J. Krige, *The Prehistory of ESRO, 1959/60. From the First Initiatives to the Formation of the COPERS*, Working Paper ESA HSR-1 (Noordwijk: ESA, July 1992).

Krige (1993)
J. Krige, *The Early Activities of the COPERS and the Drafting of the ESRO Convention (1961/62)*, Working Paper ESA HSR-4 (Noordwijk: ESA, January 1993).

Hermann et al. (1990)
A. Hermann, J. Krige, U. Mersits, and D. Pestre, *History of CERN. Volume II. Building and Running the Laboratory 1954-1965* (Amsterdam: North Holland, 1990).

Krige and Pestre (1986)
J.Krige and D. Pestre, "The Choice of CERN's First Large Bubble Chambers for the Proton Synchrotron (1957-1958)", *Historical Studies in the Physical Sciences*, **16** (1986), 255-279.

Massey and Robins (1986)
H. Massey and M.O. Robins, *History of British Space Science* (Cambridge: Cambridge University Press, 1986).

Russo (1992)
A. Russo, *The Definition of ESRO's First Scientific Satellite Programme (1961-1966)*, Working Paper ESA HSR-2 (Noordwijk: ESA, October 1992).

Figure 1. Document COPERS/64, ESA archives. Villa Il Poggiolo, EUI, Florence.

COPERS/64
Paris, 14 March 1962

EUROPEAN PREPARATORY COMMISSION FOR SPACE RESEARCH

4th Session

Report of an ad-hoc Committee of Experts on the Consequences of the adopted overall Ceiling (1500 MNF) for the first eight Years

Chairman : Professor Van de Hulst

The main findings of the Committee may be summarized as follows :

1. The blue book with its amendments does not represent a detailed scientific programme; accordingly neither the budget estimates contained in it, nor the figures of the budget which are based on this book, can be regarded as highly precise figures.

2. Accurate cost estimates based on design studies, which will be needed in the future, may be expected to give substantial corrections, down as well as up, of individual cost items in the blue book. The estimates of many items contain a built-in contingency.

3. It is considered improbable but not impossible that the approximate programme as outlined in the blue book may actually be carried out within the adopted ceilings of 1500 MNF. This programme should be maintained as a definite aim.

4. In the event that it will be necessary to economize on the programme in order to stay within this ceiling, such economies will probably take the form of a slower build-up, i.e. of fewer launchings than estimated in the blue book.

5. The consensus of opinion was that a programme thus reduced would still yield valuable scientific results and would still answer to the aim of ESRO to promote European collaboration in space science and technology.

1960: How many European astronomers wanted a space telescope?

Marcel Golay
Observatoire de Genève

1. Introduction

The concept of the present European Space Agency (ESA) was born at CERN in Geneva during a conference held between November 28 and December 19, 1960, where the president of the Swiss Confederation, Max Petitpierre, had invited representatives of the CERN member states (Krige 1992a).

Apart from the high officials and diplomats representing each member state, which formed a culturally homogeneous population due to the relevant selection procedures, the delegations also consisted of experts in various fields: economists, engineers, physicists, astronomers, radio-- astronomers, meteorologists, geophysicists, etc. The latter group, though very diverse regarding the scientific and technical backgrounds of its members, was nevertheless also homogeneous due to their common interest in space research and its technological implications. That initial diversity among the scientific and technical branches enabled each participant to get an idea, very early during the conference, of the various pressure groups that were to act during the forthcoming years. A huge effort was to be deployed by everybody concerned during several years, in order to try to establish a coherent scientific and technical programme for the entire organisation as well as for each field of research in particular. The difficulty of that task was due to the diversity of subjects involved in space technology. No European (or simply national) project of a research organization can avoid that difficulty which is specific to the multi-- and interdisciplinary nature of space research.

2. From ground-based astronomy to space astronomy; a crisis of growth

In 1960, the stellar astronomers (I am limiting my discussion to stellar and extragalactic astronomy) who, even though very much concerned by the study of UV and EUV stellar radiation, were poorly represented during the initial phases of the first COPERS councils. In spite of that low quantitative representation, one of the main projects described in the final report of December 1991, known under the name "Blue Book", concerns the construction (at the earliest opportunity, according to the report) of a large astronomical satellite. The conception of that satellite was very similar to that projected by Dr. Robert Boyd of the UK's National Committee for Space Research (Krige, 1992b). This generous project in favour of stellar astronomy was to be finally abandoned by ESRO (Krige, 1992b and also Russo, 1992) after countless discussions reflecting profound differences of opinion among the scientists, and the astronomers in particular. That failure gave the impression of a strange lack of interest among the continental stellar astronomers regarding observations made from spacecraft. A failure that was sufficiently severe to imperil for some time the future of ESRO.

Regarding again the "Blue Book" after 30 years, one discovers that all the quarrels during the deep crisis of ESRO between 1961 and 1969 were most probably latent within the ambiguities of the 1961 report. The ambiguities are, however, not to be found in the 1961 report alone. They are also present in the internal reports of the European states involved in national programmes of space research. Indeed, during the years 1955--1960, government organizations with the aim of planning scientific research gained influence in several countries. Planning implies the making of choices, the definition of priorities, what is called science policy, a policy conducted and influenced only to a slight degree by scientists. The setting up of national space research programmes was everywhere very difficult to carry out. They were to develop mainly for the benefit of research subjects which had already secured a dominant position. Thus, with the exception of the Netherlands, astronomy does not appear to have been one of the dominant scientific activities at that time, even though people of consequence such as Heckmann, Lindblad, Danjon, Swings, Oort, Righini were very active and influential. The slow process leading to the creation of ESO (Blaauw, 1991), initiated by some of those personalities, shows how various national authorities, even though they may be able to recognize the cultural value of astronomical research, nevertheless remain generally reserved when concrete and important action has to be taken. Astronomy has an ambiguous picture in the minds of planners and administrators. That image has to be continually (today more than ever) corrected and adapted to the current situation so as to avoid the trimming of our budgets by nearby branches of research which do not share our objectives. Dr. Barbier, in 1960, in the international journal of scientific synthesis "Scientia", gives a good description of the evolutive character of the field of astronomy by comparing its state in 1960 as opposed to that in 1935, for example.

> "Enfin l'astronomie n'est plus une pure science d'observation. Elle est accessible à l'expérimentation: on a fait des étoiles filantes artificielles, l'envoi dans l'espace de satellites artificiels et d'engins cosmiques constitue de la mécanique céleste expérimentale, l'émission de sodium par ces engins est une expérience astronomique ainsi que l'étude du sol lunaire par radar. La haute atmosphère terrestre devient elle aussi le domaine de l'expérimentation: aurore polaire artificielle, nuages de sodium, réactions provoquées par injection de corps particuliers. L'emploi de la méthode expérimentale en astronomie est sans doute promis à un riche avenir et son

introduction, d'un point de vue philosophique, constitue une révolution dans les idées admises.

L'astronomie étend donc un peu son champ d'action et les limitations imposées à ses moyens disparaissent comme par enchantement. Sa situation actuelle est-elle donc particulièrement enviable? Le croire serait une erreur qui proviendrait d'une confusion dans les termes. Dans les pages précédentes nous avons utilisé le terme d'astronomie dans le sens étendu, son sens logique d'ailleurs, de science consacrée à l'étude des astres alors qu'en fait l'astronomie des astronomes et des observatoires en est resté pratiquement à la définition de 1935. De nouvelles sciences sont nées qui ne sont pas *vraiment* intégrées dans l'astronomie. Ces nouvelles sciences sont la *radioastronomie*, étude des astres par leur rayonnement radioélectrique, la *physique cosmique* ou étude des rayons cosmiques, qui en grande partie s'intéresse plus particulièrement à la nature de leurs particules constitutives et à leur génétique mais dont la partie astronomique doit se développer par la possibilité nouvelle d'étudier les rayons cosmiques hors de l'atmosphère, l'*aéronomie* qui veut réaliser la synthèse des données sur la haute atmosphère et qui comprend également l'observation optique des phénomènes qui s'y produisent, la *science de l'espace* qui comprend toutes les études qui peuvent se faire à partir de fusées, de satellites artificiels et d'engins cosmiques qinsi que leur observations à partir du sol et enfin l'*astronautique* qui concerne la propagation de ces engins dans l'espace, ce qui relève de l'art de l'ingénieur mais ce qui est aussi en relation avec la mécanique céleste."

The concern expressed above by Barbier is shared, among others, by E. Schatzman in *Astronautica Acta*, also in 1960:

"Ces sciences nouvelles seront naturellement de la plus grande importance pour l'astronomie, mais ne feront pas plus partie de l'astronomie que, par exemple, la géographie ne fait partie de la science de la navigation, sous prétexte que l'emploi des instruments de navigation a permis l'exploration du globe'".

3. Space astronomy does not belong to stellar astronomers alone

The long term projects proposed in the "Blue Book" concern "astronomical" observatories consisting of stabilized satellites having extremely varied aims such as stellar spectroscopy in the ultraviolet, the study of the "Gegenschein", UV and X--ray spectroheliography, the study of cosmic radiation, the study of the moon, of planets, comets etc. It is quite obvious that these projects express the desires of a much larger European community than that represented by European members of the International Astronomical Union (IAU) of 1960. Laboratories of physics, geophysics, meteorology also become interested in the study of stars and planetary bodies, of the interstellar and interplanetary media, of the Earth-Solar medium.

In each country, and according to the existing organizations and qualities of the responsible people, the prospect of a European center of space research tended to stimulate new cooperative efforts, to incite traditional observatories to expand their fields of activity (or to sometimes seek isolation, or better, to get rid of routine tasks that had become non-productive) by including the use of highly efficient techniques, very often destined for short term applications. The cost of research thus reached levels that administrative authorities were not used to encountering in observatory budgets. Moreover, the new generation of scientists engaged in astronomy-oriented space projects often came from physics of technological laboratories and were used to the preparation of costly experiments. This caused a new attitude in observatories during the early sixties. At that time also the astronomical community became acutely conscious of the necessity to best exploit the extraordinary possibilities offered by "astronomical observations from outside the atmosphere". It is highly significant that these words appear in the main title of commission 44 of the IAU: "Commission des observations astronomiques en dehors de l'atmosphère terrestre". Significant also is the fact that this commission was created by the executive committee of the IAU in September 1959, in between two General Assemblies. Its first president was P. Swings and the first members were well known for their research work in astronomy and radio-astronomy. Already in the first report of the commission, P. Swings is confronted by the choice among subjects and their relations with astronomical research.

> "Je me limiterai", dit-il, "à peu près strictement aux observations astronomiques. Il ne sera pas question des innombrables et importants résultats obtenus en géophysique (météorologie, haute atmosphère terrestre, champ géomagnétique, ionosphère), quoiqu'il soit parfois difficile d'éviter ces domaines ... Il ne sera pas question non plus des ceintures de Van Allen, quoique la découverte et l'étude de celles-ci soient d'une importance capitale pour l'astronomie".

This first report gives an excellent review of the situation in 1961 and, in accordance with the wish of P. Swings, its historical value is obvious today. P. Swings, who had developed in a remarkable manner an institute specially adapted for astrophysical research, does not fail to point out the implications of space technology for astronomy.

> "L'astronomie va profiter de plus en plus du fait que l'on parvient à lancer des charges utiles de plus en plus lourdes dans l'espace. Les nombreux résultats obtenus que nous allons exposer ne constituent qu'une introduction; les charges utiles beaucoup plus élevées qu'il est permis d'envisager pour l'avenir ouvriront des

possibilités énormes dont nous décrirons quelques exemples. Ces possibilités imposent un "planning" scientifique soigneux. Il suffit de penser au coût énorme du lancer d'un gros véhicule spatial (comparable au coût total d'un grand observatoire!) et à l'effort théorique et technique requis, pour se représenter la lourde obligation morale des groupes d'astronomes responsables, l'impérieux besoin d'une collaboration internationale et, sans doute aussi, la nécessité d'augmenter le nombre des jeunes astronomes professionnels."

4. *Difficulties encountered by astronomical research institutes adapting to space observations*

Most directors of institutes were conscious of the fact that an increase of the number of young professional astronomers was an absolute necessity. This was, however, not very easy to put into practice. It was indeed necessary to start by informing young students that, thanks to forthcoming space programmes, beautiful astronomical research could be done and that research positions would be open in the near future. Thus scientific careers could be considered, as was already the case in physics particularly since the creation of CERN in 1952. Three to four years would be necessary to start such a shift of interest (minimum duration for a Ph.D. thesis). Within these three to four years, ESRO would have been created and important national programmes would be underway. Thus, at the time when the most critical choices were being made, the astronomers of continental Europe did not have a sufficient number of experienced young colleagues able to commit themselves to the new pathways opened up by space research. Moreover, traditional stellar astronomy in Europe had been lacking ground-based observational facilities, as compared with those available in the United States. We may recall that the largest European telescope was at that time the 193 cm of the OHP (Observatoire de Haute-Provence of the CNRS); it had become operational in June 1958 and the first high resolution spectrogram was recorded during the night of July 20, 1959. This telescope was surpassed only in 1974 by the 6-meter at Zelenchuk. At the same time, young European astronomers were prospecting sites in South Africa, for ESO, in view of "installing a telescope of at least 3 meters (still to be constructed) and a 120 cm Schmidt camera" (declaration of the signatories at the creation of ESO on January 26, 1954). Thus, at the time when a space astronomy programme was conceived, Europe was only just beginning to endow itself with suitable equipment for doing ground-based astronomy.

All the astronomers obviously understood that they were experiencing an exceptional period, i.e. that in all states involved in the ESRO adventure astronomy was going to gain status among the sciences. Like P. Swings, and conscious of their responsibilities, they had to choose realistic orientations that were highly dependent on local conditions (budget of the University, of the observatory, etc.), on the adopted national programmes, the immediately available personnel, the formation of the future scientists, the touchy reactions of the other fields of research that did not benefit from national or international support. It is also obvious that having invested much time and money into the acquisition of means allowing suitable observations to be made on a national scale, some people should have feared that space astronomy would be developed to the prejudice of ground-based programmes. That apprehension was (and still is) justified; we have all spent much time explaining to non-specialists the complementary nature of space - and ground-based astronomical research, that the radio-telescopes do not replace the optical telescopes, that the space telescope does not replace all ground-based telescopes and, to mention a very recent example, that the VLT does not render all smaller telescopes obsolete. Now, the people who hold the political power of decision were, and still are, to be found among the non-specialists which have to be convinced. So it is understandable that between 1959 and 1963 the community of stellar astronomers, a rather small community within the large family of sciences, found itself to be badly prepared for a sudden increase of its resources after a century of slow evolution, and that it should react with caution. Not only caution, however, but perhaps also mistrust regarding a future organization equipped with the means for launching into orbit an astronomical satellite costing, as we now know (the estimate given by P. Swings was very modest) several times as much as a national observatory and being, moreover, very short-lived. Some astronomers did fear that the financial contribution to the international organisation would be detrimental to the execution of the national astronomical programmes that had, at last, taken shape. Such apprehension is not new, neither is it unjustified; it occurs whenever an international programme is developed (LEP at CERN, VLT at ESO, ESA scientific programme).

We have just stated an "institutional" cause of division among the astronomical community, thereby reducing its capacity to save the projected large astronomical satellite.

5. *Stellar astronomy, one of the facets of astronomy*

Astronomy is represented by a small community in the scientific world, and is again subdivided into a certain number of fields characterized by very different objectives and technical means. These divisions tend to weaken still more the position of astronomers when important choices have to be made. Research devoted to the study of a star as close and bright as the Sun generally applies techniques quite different from those used for a star in a globular cluster for instance. When browsing through the "Reports on Astronomy" of the "Transactions of the International Astronomical Union" of 1958 and 1961, one can get an idea, via the different commissions, of the fields given priority by the astronomers of that period. We notice that we can locate four groups of equal importance, solar (activity, radiation, structure), stellar (spectroscopy, photometry, variability), galactic (structure, interstellar matter, radial velocities, parallaxes, star clusters) and planetary (planets, moon, meteorites, satellites, earth) astronomy. The commissions for radio-astronomy and galaxies still had very few members. The commission for radio-astronomy was created in 1952 but, in 1958, illustrating how long it takes for a new technique to find its place in a structured environment, its president J.L. Pawsey still felt the need to specify the role of his commission in the IAU:

> "The Radio-Astronomy Commission has a peculiar role to play in the I.A.U. because of the rapidity with which its techniques are developing into diverse fields in astronomy. These developments pose a problem in transmission of ideas between radio experimenters and astronomers who are each expert in their fields. This problem is accentuated by the fact that a large proportion of those contributing to radio astronomy have been trained as physicists or engineers and not as astronomers. There is need to inform astronomers of the fields in astronomy to which radio is contributing, or can contribute, and to provide clear statements on the experimental and the interpretative uncertainty in the available results. Similarly there is need to inform radio astronomers of relevant aspects of astronomy with which they may not be adequately familiar. This is a liaison job which, at this phase in the development of radio astronomy, Commission 40 should accept as one of its major responsibilities."

We may note that the two commissions "Radio-astronomy" and "Galaxies" (the latter was called "Extragalactic nebulae" until 1958) presently count together 750 and 530 members respectively and are by far the most important commissions of the IAU.

6. *Solitary and collective*

We have mentioned two sources of division within the community interested by the creation of a space telescope. One is institutional (choice of priorities according to new structures created in view of organizing scientific research), the other, by showing that only a fraction of the astronomical community is scientifically interested by the existence of a European programme responsible for placing a telescope into orbit. A third source of division seems to be quite specific to stellar and galactic astronomy, at least it was so in the days that are the object of the present study, and is probably the main reason why the LAS project was given up (following a series of debates that are very well documented and analysed by M. Krige). During many sessions one could notice a systematic division between the participants. On one side were those who favored high resolution UV spectroscopy applied to a small number of stars, and on the other the tenants of low resolution UV spectroscopy (or even ultimately a simple photometry with several passbands in the ultraviolet) applied to a very large sample of stars. The same divisions also emerged within national committees. Actually, this partition may be observed during the organization of any type of research, be it in the natural sciences or the humanities. To put it very schematically, it is the expression of two different intellectual approaches to the study of natural phenomena. To simplify the discussion I will call the one the "solitary" approach, whereby the whole effort is dedicated to the fine analysis of a given object, the other being the "collective" approach where the analysis is carried out for a population of objects distinguished by relatively rough indicators. Each researcher has a permanent cast of mind that links him more or less to one of these two approaches. I have always been surprised by the persistence of that character in each individual, whatever the subject he dealt with. Concerning stellar astronomy, the "solitary" approach is shared, for instance, by specialists in high resolution spectroscopy (often having acquired their basic training as physicists), those who measure and analyse eclipsing binary light curves (among other types), the orbits of double stars, the parallaxes of nearby stars, etc. The "collective" approach is favored most often by researchers who had their basic training in astronomy and thus are more interested in the properties of groups and populations. They are often more at ease when manipulating statistical parameters rather than laboratory equipment. They are often responsible for "surveys", studies of galactic structure or of distributions of interstellar matter, for example. In the natural sciences, these two approaches (though too schematic in this case) are complementary and necessary for the understanding of observable phenomena. These two approaches are, however, not equally regarded during the organization and planning of research. Generally, the "solitary" approach leads more rapidly to a publication than the "collective" one. In the latter case, the initial investment is costly in terms of time and money, the reductions long and often not very stimulating for the younger generations. However, who has never made use of a catalogue of spectral types, UV spectra, positions, variables, of star clusters or clusters of galaxies, of the distribution of interstellar matter, of HII regions, of the distribution of H_{21} or CO, etc.? Thanks to the sinking prices of computers and to their tremendous capacity for numerical calculation and storage of data, thanks also to the sensitivity of the new detectors and the automatisation of reduction procedures, the tedious part of the "collective" approach is rapidly disappearing. This approach nevertheless still requires long term planification and commitment, and these are generally much apprehended by our universities and national or international research organizations.

The two approaches, "solitary" and "collective", are complementary and this means that a research programme must usually carry out both of them in view of being properly balanced. However, the astronomical community being small, this balance must be achieved at least on a European scale. In 1960 all the actors were, I believe, conscious of the necessity to create a balanced "solitary" and "collective" programme for ESRO. The situation of conflict arose at the moment it was to be decided which type of programme to begin with. A difficult choice because, to carry it out, one should have been able to take into account the long term national and international programmes for development of space- and ground-based stellar research. Few national programmes were well established in 1960 (several states tended to wait until the orientation of ESRO would be clearly defined), whereas the main international programme, that of ESO, did not yet exist even though everyone was ready to take into account what was still but a project. Indeed, the agreement creating ESO was finally signed on October 5, 1962 (and ratified on January 17, 1964). The situation became even more complex as several member states of ESRO were not, or no longer, associated with the ESO project (the United Kingdom was a signatory of the provisional agreement of 1954, but rapidly withdrew from that project). We obviously do not have any official documents relating the waverings of each participant regarding the choice of priorities for the ESRO projects, the future ESO programme, the national programmes and the bi- and multilateral collaborations which are ever more frequently established. These hesitations were, of course, only apparent during the countless private discussions which preceded or followed the official meetings. Each of us has conserved numerous hand-written notes and now regrets, 30 years later, not having written them more clearly and in more detail.

7. *Lack of experience*

With the exception of the British astronomers, the great majority of European astronomers had never had the opportunity to acquire some experience with either balloon-borne or rocket-borne telescopes. Physicists, meteorologists, geophysicists and solar specialists, on the other hand, already had much experience with these techniques. The consequences of that situation, i.e. the absence of European results, are clearly visible in the International astrophysical colloquium held in July 1960 in Liège "Les spectres des astres dans l'ultra-violet lointain", or in the report given by P. Swings in 1961 while president of commission 44 "Observations en dehors de l'atmosphère terrestre" and also in the proceedings of that commission. The situation does not appear much better in 1964, if one is to judge by the communications presented at the IAU Symposium "Astronomical observations from space vehicles" held in Liège in August 1964. Let us take a look at some of those documents from 1960-61; they help us to understand the misgivings and hopes of their authors.

During the 1960 Liège colloquium, all the UV radiation stellar and interstellar experiments described are those of Boggess, Byram, Chubb, Friedman, Kupperian and Milligan, all from the USA. We find, on the other hand, the theoretical work of Pecker (France), research stimulated by the discrepancy between the predicted and observed UV spectra, the works of de Jager, Pottasch, Neven, Swings and Seaton (NL, Bel., UK). Theoretical research was more easily carried out by Europeans than the acquisition of new observations.

The report given by P. Swings for Commission 44 in 1961 does not refer to any observations of the stellar UV made by European groups. On the other hand, in the proceedings of the August 23, 1961 session, one learns of a very recent British experiment (May 1, 1961) and of a national programme for developing space astronomy that reflected the vitality and experience of the British scientists. Here is a quotation of the report given by Brück (British delegate):

> "A sky scan at 1700 Å with a set of photo--multipliers with peak sensitivity at 1700 Å. This experiment was flown on 1 May of this year, and preliminary analysis seems to show traces of certain O- and B-stars in the southern sky. (Like other rockets, this one was flown from Woomera, Australia).
>
> Sky photography at 1700 Å with a camera containing a fused quartz lens focussed for 1700 Å.
>
> Mention might also be made of plans which are to be undertaken in due course by a group at Harwell under Dr. Wilson. This group which studies, in the laboratory, the physics of plasmas through spectroscopy in the vacuum ultra-violet, intends — in collaboration with Professor Allen of London — to study the spectrum of the solar chromosphere and corona between 400 Å and 3000 Å.
>
> As regards satellites, I should like to mention the design study for an astronomical satellite carried out at Farnborough under Dr. Lines. The satellite is to carry a Cassegrain telescope of about 20 inches in diameter and a spectrograph with which spectra of individual stars are to be obtained within the range from about 1000 Å to 3000 Å with a resolution of about 1 Å.
>
> Finally I should like to mention the two Scout satellites for which the necessary instrumentation has been agreed on between NASA and the British Space Research Committee. Scout I, which is to be flown in 6 months' time, will contain two astronomical experiments, both due to Dr. Boyd, and both extensions of his rocket work, namely measurement of solar X-rays between 3 and 20 Å, and determination of the Lyman-alpha flux.
>
> Scout II, which will only be flown in two years' time, contains two experiments of direct astronomical interest. The first of these, due to Dr. Graham Smith of the Mullard Radio Radio Observatory, Cambridge, aims at extending ground measurements of galactic radio noise to the lowest possible frequency, and in particular to the range between 0.75 and 3 MHz. A test launch of the equipment has been made with Black Knight.
>
> The second astronomical experiment in Scout II is due to Dr. Jennison of the Nuffield Radio Laboratory at Jodrell Bank. Dr. Jennison intends to measure numbers and sizes of micrometeorites encountered by the satellite during its flight. The sensitivity of the relatively simple equipment is such that it should be possible to detect meteorites down to a diameter of one micron."

No stellar astronomical research was presented by the representatives of the other European states. The secretary of the session, J.C. Pecker, very briefly summarizes the positions of the French and Italian research workers:

> "Les chercheurs français et italiens, estimant que rien n'a été fait, en matière de recherche astronomique spatiale, en plus des recherches déjà mentionnées dans le *Draft Report*, renoncent à leur intervention."

One must indeed wait until April 4, 1967, for Courtès, Siran and Viton (Fr) to obtain images of stellar fields with a telescope mounted on bord of a Véronique rocket.

During that same session, R. Lüst gives an excellent summary of the situation of the ESRO projects. Here is an excerpt:

> "I would like to report very briefly on plans for European collaboration in space research. These plans go back to some unofficial preparations and planning during the past year. At the end of last year eleven European countries (Norway, Sweden, Denmark, The Netherlands, Belgium, United Kingdom, France, Germany, Switzerland, Italy and Spain) signed an agreement for establishing a preparatory commission. This preparatory commission (COPERS) has the task of proposing the plans and drafting a convention for a European Space Research Organization (ESRO). The President of this preparatory commission is Sir Harrie Massey and one of the Vice-Presidents is Professor van de Hulst. Its Executive Secretary is Professor Auger. The plans for this organization have been worked out by a scientific and technical working group, and quite a number of astronomers actively participated in this planning stage.
>
> In general terms, the purpose of the planned organization shall be to provide for and promote collaboration among European states in space research and technology. In doing this, the organization may operate facilities to design and construct payloads for sounding rockets, satellites and space probes. Furthermore, this organization shall provide launching vehicles, including their launching, and provide means for the reception, collection, reduction and analysis of data.
>
> This scientific programme should start with research by making use of sounding rockets. The upper atmosphere will be studied, particularly in the auroral zone, and also astronomical investigations will be carried out. In the fourth year after establishing the organization the first small satellites and space probes shall be launched, carrying payloads of about 200 kg. A number of astronomical studies have been proposed, including those for studying the interplanetary medium and the

comets. In the sixth year a first astronomical satellite with a stabilized platform should be launched, carrying a telescope of about 20 inches. The following is an indication of how large the effort may finally become when the ESRO is running at full activity: about 65 sounding rockets, 6 small satellites and space probes, and one large satellite should be launched each year."

The texts above bring well into perspective the great influence of the British projects regarding the choice of the characteristics of ESRO's future large satellite (project withdrawn in 1966).

8. Since 1960: Satellites and big telescopes assure the future of European astronomy

From 1960 onwards, the stellar astronomers participated in the creation of two international scientific organizations, ESRO and ESO, for which CERN was a magnificient example of success. Intellectually, they were perfectly conscious of the necessity to have an astronomical observatory in space. But certainly no European group had any practical experience comparable to that of the British groups. Contrarily to CERN where, at the time of its creation, practically each member state had experienced and financially relatively well provided groups, those of the stellar astronomers were generally very moderately staffed and, for the whole of Europe, had access to only one large telescope of less than 2 meters. Moreover, they represented but a small part of the community of astronomers who were also, traditionally, often engaged in routine tasks of a tedious and engrossing nature.

The discussions and disputes throughout the years 1960-1964 regarding the characteristics and performances of the LAS did help to accelerate the process of modernization of European stellar astronomy. One must not forget that many a European observatory had practically not evolved during half a century. Few astronomers could rely on an administrative and financial structure good enough to enable the involvement of a group in a LAS-type adventure. If some of us were surprised by the passivity of our community during that thrilling and impassioned period, the course of events has shown that that passivity was only apparent. A profound change was set in motion by the ESRO and ESO projects. It lasted for about 10 years and led to the intensive exploitation of IUE (launched on January 26, 1978, and still operational in 1992) followed by the Hipparcos astrometry mission (and the Tycho astrometry and photometry experiment) launched on the 8th of August 1989 by an Ariane 4 and, finally, the Faint Object Camera, one of the prime focal-plane instruments on board the Space Telescope (launched on the 4th of April 1990).

As for the pre-IUE period, it has been very well described by Bogges and Wilson (the latter played an important role in the LAS project) in "Exploring the Universe with the IUE satellite", 1987:

> "The first European Satellite to carry ultraviolet astronomy instrumentation was the European Space Research Organisation (ESRO) satellite TD-1, named, with some lack of imagination, after the acronym of the US Thor-Delta rocket which was to launch it in 1972. Of the seven experiments carried, two were devoted to cosmic ultraviolet astronomy. The first, which was the prime motivation for the mission and was carried out by groups in the UK (ROE and ARU) and in Belgium (Liège), conducted an all-sky ultraviolet spectrophotometric survey of objects down to visual magnitude of 9-10 (Boksenberg et al., 1973). The second carried out stellar spectroscopy in regions between 2000 and 3000 Å, with equipment provided by the Utrecht group (Hoekstra et al., 1972). Both were highly successful and enabled extensive studies to be made in stellar and interstellar astronomy. The former also made a contribution to the operation of IUE by allowing accurate estimates of the optimum exposure time for a large number of targets from the data in its all-sky catalogue (Thompson et al. 1978). The Astronomical Netherlands Satellite (ANS) also carried an ultraviolet telescope as its prime mission. Launched in 1974, it carried out broad band photometry of stars, clusters and galaxies and made a major contribution to ultraviolet astronomy."

During the night of November 7-8, 1976, i.e. 14 months before IUE, the Europeans gained access to the 3.6 meter telescope at La Silla. A few years earlier, in 1971 (almost simultaneously with TD1), the Schmidt Telescope became operational and began its survey of the southern sky. European stellar, galactic and extra-galactic astronomy has acquired over these last 20 years the means for doing high quality research (balancing the "solitary" and "collective" type approaches) from space as well as from the ground. Thanks to the stimulation generated by ESRO and ESO, talented young scientists now have fine prospects ahead of them.

9. Conclusions

Let us return to the title of this article: "1960: How many European astronomers wanted a space telescope?". I think none of the astronomers who participated in the creation of ESRO (first of COPERS) have met any opposition from colleagues of his own country. The importance of astronomical research carried out with spatial engines was obvious for anyone. However, prior to any involvement in space experiments, every participant had to fight first in order to obtain decent modern technical means (there were however some exceptions) even for the most basic research. Astronomers who participated actively in the creation of ESO were never (to the best of my knowledge) opposed to the creation of ESRO and vice versa. Thus the majority of astronomers who were not personally associated with the projects for ESRO or ESO were therefore mainly concerned with the survival of their own institute. That ESRO and ESO could exist simultaneously appeared to most people a dream and as such impossible to be achieved. If the dream became nevertheless true many astronomers feared that this might have a negative impact on the development of their national institute. Thus the majority of European astronomers showed, as it seems to me — but this is of course my own personal feeling — some hesitations, but at the same time accepted willingly that some colleagues spent considerable time for the preparation of a project which in their opinion, considering the administrative aspects, could not be realized. And yet, the dream and the determination of some has been beneficious for all.

Bibliography

Barbier D., 1960, "Les voies nouvelles de l'astronomie", *Scientia*, Revue internationale de synthèse scientifique, Vol. XCV, fasc. 8.

Blaauw A., 1991, "ESO's Early History", European Southern Observatory, Garching, 1991.

Boggess A., Wilson R., 1987, "The history of IUE", in *Exploring the universe with the IUE satellite*, **3**. Ed. Y. Kondo, D. Reidel Publishing Co.

Krige J., 1992a, "The prehistory of ESRO 1959/1960. From the first initiatives to the formation of the COPERS", ESA HSR-1, July 1992.

Krige J., 1992b, "The rise and fall of ESRO's first major scientific project, the large astronomical satellite (LAS)", in *History and Technology*, **9**, 1.

Russo A., 1992, "Choosing big projects in space research: the case of ESRO's scientific satellite COS-B", in *History and Technology*, **9**, 27.

Schatzman E., 1960, "Les applications de l'astronautique à l'astrophysique", *Astronautica Acta* vol. VI, Fasc. 2-3, 159.

Swings P., 1962, Report of "Commission des observations astronomiques au dehors de l'atmosphère terrestre", in *Transactions of the International Astronomical Union*, vol. XIA Report, 521.

VLT: Proposal for the construction of the 16 meter Very Large Telescope (VLT), ESO Garching, March 1987.

Solar research in the early days of COPERS/ESRO

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1. The situation in solar research around 1960

In 1939 and 1941 Grotrian and Edlén identified some coronal emission lines as due to highly ionized atoms, which required a coronal temperature of the order of a million K (MK). X-ray observations by means of rockets in the early fifties, mainly by the Naval Research Laboratory (Friedman et al., 1951)), fitted roughly with theoretical predictions, made later by Elwert (1954, Fig. 1) and these confirmed the value for the coronal temperature: slightly below 1 MK. I wrote a review (1959) in which the situation, including new observations by Friedman et al.(1957), were described. The data obtained closer to the solar maximum showed that X-ray emission associated with solar Active Regions can be detected down to wavelengths of a few Å. This was (correctly) ascribed to the hot coronal condensations (i.e. the coronal extensions of the solar Active Regions around sunspots). For these, a temperature of around 5 MK was estimated (Figure 2). It was also predicted that the corona, which is optically thin, even at the Sun's limb, should show a strong limb brightening (Fig. 3).

Already around 1953-55 it was clear that solar flares should have a high-temperature component. Indirect arguments were based on the observed height of the ionospheric D-layer which comes down as low as 60 km during flares. The quiet time D-layer is ionized by Lyman Alpha radiation, but that radiation does not increase sufficiently during flares to explain the observed strong lowering. Rightly, Friedman and Chubb (1955) ascribed it to short wave X-rays below 8 Å.

A number of rocket firings during flares (evidently always after maximum) were made by the N.R.L. Some of this data had been obtained from a 'rockoon': a rocket, suspended at 40 km altitude beneath a balloon and launched at command at the time of observation of a flare: thus the time between flare start and observation was reduced as compared to ground firings. They showed emission of radiation down to 1.5 Å. That would need temperatures of the order of or even above 10 MK. This high temperature confirmed some, at that time very recent, microwave observations which also showed that a very hot plasma occurs transiently during the early phase of flares.

2. *How to start*

At the start of the COPERS period the University College of London group, headed by Massey, had already a good experience in rocket firings. At that time this group was also well advanced with the construction of their instrumentation for the first British satellite, Ariel 1, which contained (among other things) detectors for monitoring the soft X radiation of the Sun. Ariel 1 was launched in spring 1962. I will never forget Massey's enthusiasm when he described the first results at the Washington COSPAR meeting. At the same time the SOLRAD satellites of the Naval Research Laboratory (U.S.) were also flying and were monitoring the Sun in various wavelengths, including Lyman Alpha and soft X-rays.

Another quick start was made by French scientists. The CNES was gaining experience with Véronique launchings. Jacques Blamont offered me some space in a Véronique, and this enabled the Utrecht group to measure the coronal X-radiation on Nov. 4, 1964 during a period of extreme solar quietness. The data yielded a lower limit for the quiet coronal temperature: they fit with a value of 0.9 MK.

Very important was a two-days COPERS symposium, held at UCL in December 1961, where all those who had submitted proposals for rocket experiments were invited. For the many new European groups, full of good will and not troubled by too much of experience, this was an eye-opening and highly instructive meeting. The heating system in the meeting room failed, but there was abundant warmth of enthusiasm among the participants.

Some efforts were made in the early sixties to get more information on the spatial distribution of X-radiation over and beyond the disk. In 1960 the Naval Research Lab (Chubb et al., 1961) got the first X-ray picture of the Sun by means of a pinhole camera. The Utrecht Laboratory tried to get a larger aperture by means of a Fresnel zone 'lens' and we got our first result in 1967. At about the same time Yngve Öhman from Stockholm obtained rocket images of the Sun in MGII by introducing a monochromatic polarizing filter in the light path. For studying the disk distribution of the solar short-wave radiation ESRO organized an eclipse expedition to Greece in order to launch a number of rockets during various phases of the solar eclipse of May 1966. The results were fairly meagre: getting the distribution of X-radiation over the disk by 'differentiating' a luminosity curve, consisting of a few discrete points, is a rather indeterminate process, with unclear results. More promising was the development of reflection optics for the X-range by means of parabolic frustrum mirrors. That development started in the U.S. in the second half of the 60s.

3. *The soft X-ray flare component*

In 1960 it was known that a typical flare has three components: the 'cool' one, visible in H Alpha and other emission lines of low ionisation; the hot component, from X-ray and microwave observations, and the interplanetary particle component. The latter component was dealt with by ESRO's spacecraft HEOS I, ESRO IV and ISEE, and it will not be discussed in this paper.

The hot flare component had been detected by means of incidental rocket launchings before 1960 but after that with satellites, initially equipped with proportional counters and therefore only suitable for observing soft X-rays. The NRL SOLRADs and the UK ARIEL were the forerunners; ESRO followed with ESRO II, the first European scientific satellite, launched 17 May, 1968, after an unsuccessful launching of a first original of the satellite in 1967. ESRO II had proportional counters for 1-20 Å (UCL) and 44-55 Å (Utrecht). It operated successfully until December 1986, when the tape recorder broke down. After that no continuous flare observations were possible.

What resulted was a good number of observations of X-ray flares and some statistics, but the hope to get conclusive information of the 'ignition' of a flare was not fulfilled. In the contrary, it was found that in many cases the flare emission had a 'gradual rise and fall' character, and did not show the hoped for impulsive start. Often the X-radiation persisted long after the visual flare, and it sometimes happened that the X-ray start occurred after the start of the flare in H Alpha. All these observations made clear that soft X-rays are not more 'basic' for understanding flares than visual observations and that the signature for the flare ignition had to be sought elsewhere. Unique was the case of a slowly rising and falling X ray event, associated with filament activity (Brinkman & Shaw, 1972). This may have been the first unnotified case of a 'giant loop' of the kind discovered later by Svestka (1982) with the X-ray imaging instrument aboard NASA's Solar Maximum Mission.

4. *The hard flare component*

In the mean time first indications were obtained that flares are also capable of emitting hard X-rays and that these do indeed occur at the very beginning of flares (Fig. 5). I was involved in the discussion of a spectacular one-minute burst of hard X-rays detected on 17 September 1963 by Legrand (Meudon) with balloon-borne instrumentation. The burst appeared to have photon energies in the 100 keV range and it occurred at the very beginning of a small flare.

These and similar observations were sufficient reasons to propose flare monitoring in hard X-rays, which was done by TD1A, launched March 12, 1972. Also here, lamentably enough, the tape recorder broke down after a few months of operation only. Subsequently ESRO made a giant effort to establish a world-wide system of mobile antennas for obtaining the satellite data, and that saved the mission. The observations led to the discovery that flares tend to emit a small number of 'Elementary Flare Bursts' during a one to two minutes interval at the beginning a flares. This interval was later baptized the 'Impulsive Phase' of a flare.

Further investigations of the various events in a flare during the Impulsive Phase and their causal relationships was left to the U.S. and Japan who had spectacular successes with NASA's Solar Maximum Mission and the Japanese spacecraft Hinotori and Johkoh. ESA is to restart a programme of solar research with SOHO.

5. *ESRO and the solar community*

When COPERS/ESRO started, the common expectation was that the scientific community as a whole would jump on the new possibilities that would give them access to new means to unravel the secrets of Nature. This was not true for the solar community (and not for other astronomical communities either). The apparently big threshold against entering a fully new field, of using instruments that are very different from the classical telescopes, and a general fairly conservative attitude, made the traditional community rather hesitant to embark on high energy solar observations. What actually happened was that a fully new generation had to be educated, a generation that came from nuclear and plasma physics and from laboratory spectroscopy, rather that from astrophysics. They had quickly to learn some basic elements of astrophysics while doing the job. I fear that in the European scheme I was one of the very few or maybe the only 'classical' solar astronomer (i.e. educated with telescopes and spectrographs) who entered the new field. Fully new groups emerged, like those in London, headed by the engineer Boyd or Paris-Meudon headed by the physicist Blamont. The same was the situation in the U.S. (Friedman, Tousey) and the U.S.S.R. (Mandelshtam). It was also so with X-ray astronomy, a discipline for which similar names and institutions can be written down.

In the early years (which is the topic of this paper) there was, though, one occasion when the traditional community showed up massively; that was in 1966 when a proposal was discussed for a large European solar space telescope, scheduled to do better from space what was done so far from the ground. This project, with the wonderful name 'Héloise' (invented by Jean Rösch) died the same dead as other similarly large projects like the Large Astronomical Telescope and the lunar spacecraft.

Bibliography

Brinkman, A.C. & M.L. Shaw: 1972, Solar Phys. 23, 120

Chubb, T.A., Friedman, H., Kreplin, R.w., Blake, R.L. & Unzicker, A.E.: 1961, Mém. Soc. Roy. Sci. Liège, 4, 228

De Jager, C.: 1955, Ann. Géophys. 11, 330

De Jager, 1959: Encyclopedia of Physics (Handbuch der Physik), 52, 80

De Jager, 1967, Solar Phys. 2, 347

De Jager, C. & Legrand, J.P.: 1967, Solar Phys. 2, 327

Elwert, G.: 1954, Z. Naturforsch. 99, 637

Elwert, 1958, Atm. Terr. Phys. 12, 187

Friedman, H. & Chubb, T.A.: 1955, 'Physics of the Ionosphere', London, p. 58

Friedman, H. Chubb, T.A., Kupperian, J.E., Kreplin, R.W. & Lindsay, J.C.: 1957, IGY Rocket Report Series 1, 183

Friedman, H., Lichtman, S.W. & Byram, C.T.: 1951, Phys. Rev. 83, 1025

Svestka, Z.: 1982, Solar Phys. 75, 305

Figure Captions:

Figure 1
The expected X-ray emission from the corona, calculated by Elwert in 1954. Calculations were made for three temperatures. Line intensities are given by crosses; series continue by circles. (From a review by De Jager, 1955).

Figure 2
Summary of observed and predicted emission by the corona below 3000 - . (De Jager, 1959, p. 264).

Figure 3
Disk distribution of solar X-radiation as predicted by Elwert (1958).

Figure 4
A 'gradual rise and fall' event observed by ESRO II on July 6, 1986 (Brinkman and Shaw, 1972)

Figure 5
Summary of information on hard X-burst available in 1967 (De Jager, 1967)

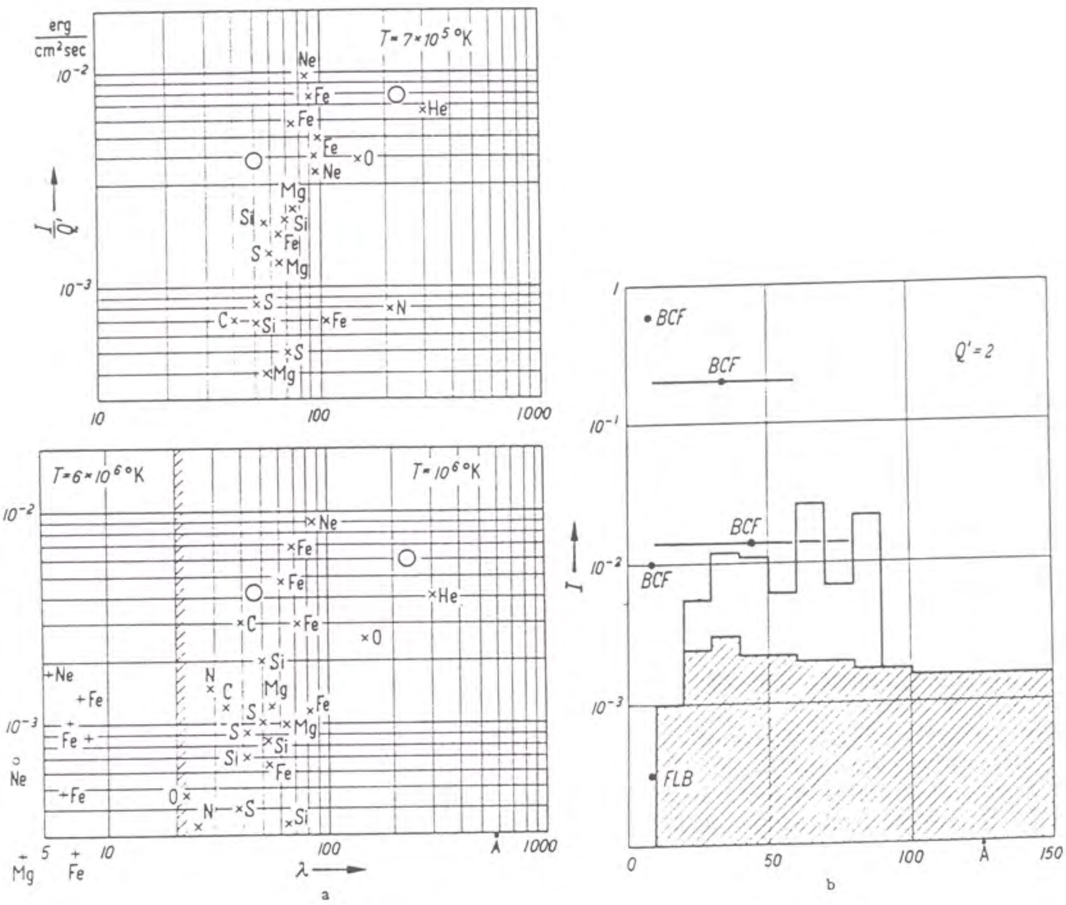

Figure 1

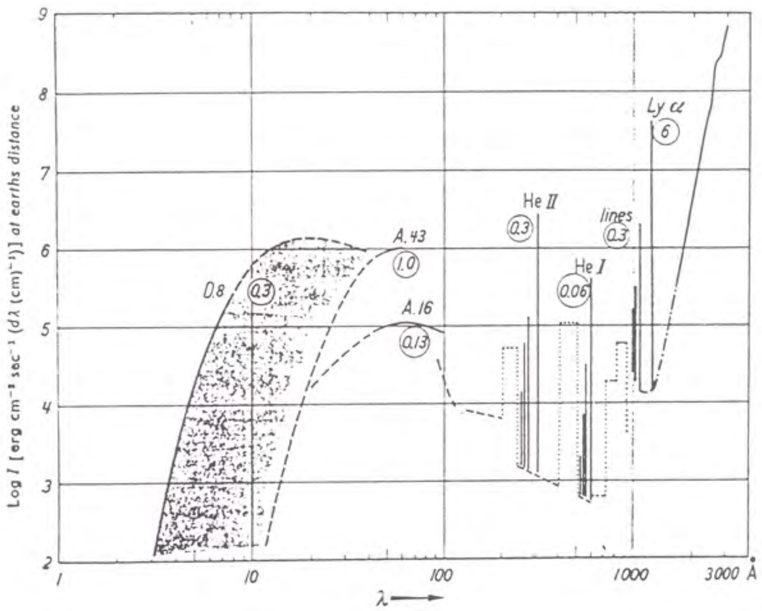

Figure 2

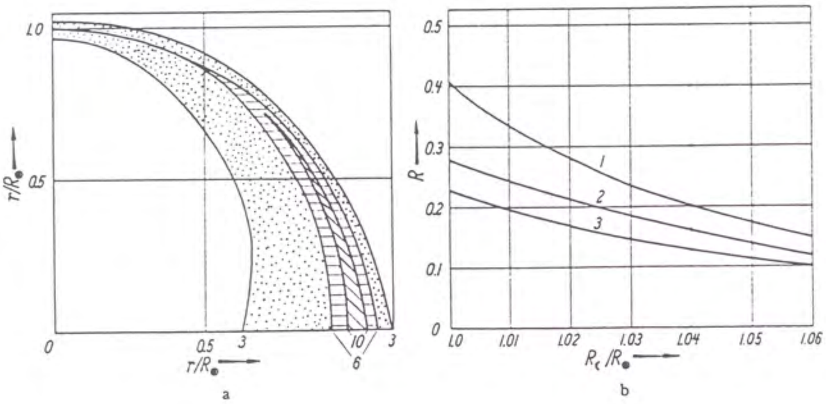

Figure 3

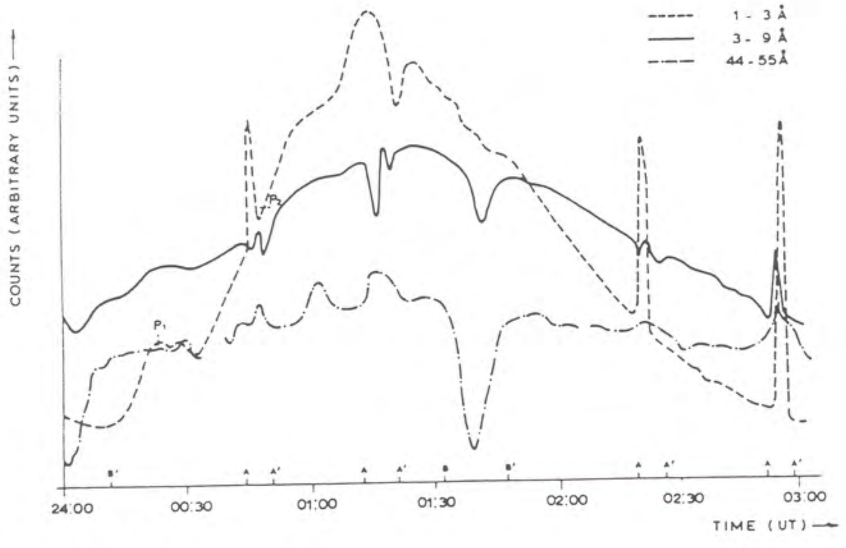

Figure 4

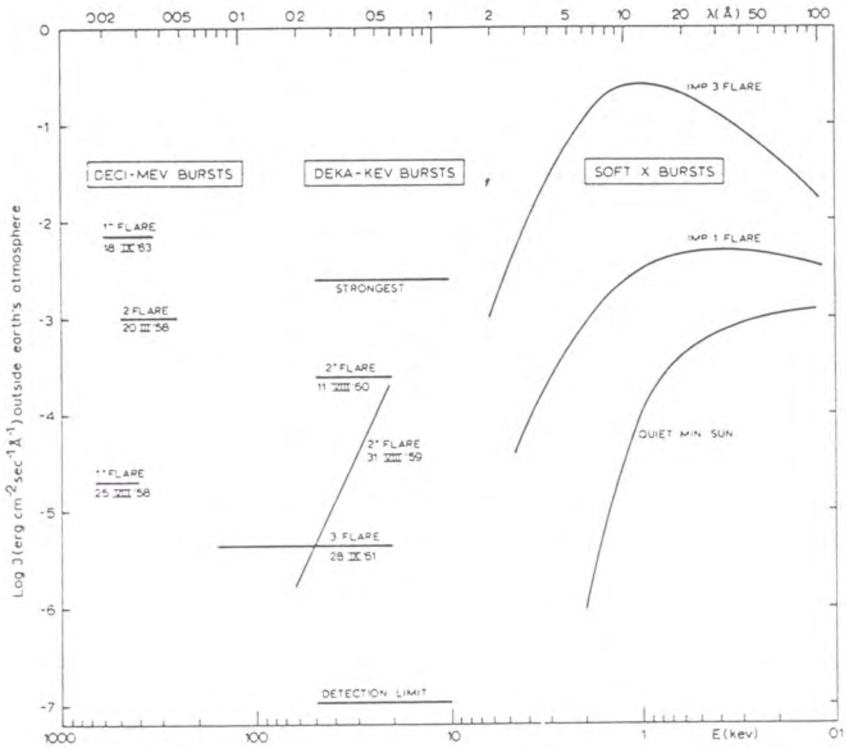
DECI-MEV BURSTS
DEKA-KEV BURSTS
SOFT X BURSTS
IMP 3 FLARE
IMP 1 FLARE
QUIET MIN SUN
STRONGEST
DETECTION LIMIT
2 FLARE
3 FLARE
28 IX 51
λ(Å)
E(kev)
Log J(erg cm^{-2}sec^{-1}Å^{-1}) outside earth's atmosphere

Figure 5

Ionospheric research by rockets and satellites in Europe in the 1960s

A. Peter Willmore
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1. Introduction

The ESRO period was notable for two things. The first was that the organisation started off with a quite unrealistic programme which had to be reduced to some sort of reality, at a time when its decision-making mechanisms were still in their infancy. The result of this was the series of debates about the TD series of satellites, the topside sounder and so on. There were, I think, two issues here — the first is what the ESRO programme was to be, and the second the criteria on which that should be decided — to what extent, for example, ESRO should be a responsive organisation, reacting to outside proposals, and to what extent a directive one. I was not central to that debate, in the way that others here today were; by the time I became chairman of the ION group in 1966, the battle was all over bar the shouting. This period, however, was also that in which a real programme of rocket and satellite launches of a considerable scale began, and when I look back on it, it is that aspect of ESRO that I remember most clearly, because of its direct importance for my working life. So that is what I decided to say something about, and I am especially glad I did so, when I look at some of the papers for this meeting, which seem to me to emphasise an important aspect, but perhaps not the only important aspect of ESRO's history.

Studies of the ionosphere, especially at high northern latitudes, was one of the most active areas of research in ESRO. There are several factors which brought this about. Study of the ionosphere by ground-based means was a very active area of science in Europe and indeed all over the world in the period after World War II, partly because of the direct importance of the ionosphere at that time for long distance communications. In addition, it turned out that ionospheric measurements were well adapted to relatively small instruments with modest technological requirements. One has only to recall the long history of the transformation of the LAS into IUE to see how technological requirements could be an important factor. It is therefore not at all surprising that ionospheric research was amongst the most lively of those topics which emerged in the newly-developing national programmes in Europe, as they were formulated in the 1950's, and in the ESRO programme from its formal start in 1964.

There was, however, a second important factor. There was in Scandinavia in particular a tradition of strong scientific interest in the study of auroral phenomena, which led both to the development of instrumentation and important facilities, especially the Kiruna Geophysical

Observatory. These factors naturally led to an active ESRO programme of ionospheric research which became centred on the study of the high latitude ionosphere, so that it rapidly acquired a character of its own which genuinely derived from the particular opportunities which a European activity offered.

In this talk, I would like to say something about the way in which the ESRO ionospheric programme developed from the foundations built by the national programmes, illustrating this from our own experience in the UK, leading first to an active rocket programme, and then to the development of a series of satellites, of which the first was ESRO I or *Aurorae*.

2. *The early growth of space science in the UK*

Scientific interest in this subject in the UK was greatly stimulated by a conference held in Oxford in August, 1953, the idea for which came from Sidney Chapman. This focussed on American work in this field using V-2's and *Aerobee* rockets but also addressed the potential of artificial satellites. This meeting brought together a number of people who later played an important part in getting the programme started, not only Sir Harrie Massey and Robert Boyd and others who played important roles in UK space science, but also rocket technologists in the UK and some key US scientists; all these links proved to be very productive in the following years.

The development of the Skylark sounding rocket, nominally at least for scientific purposes, started in 1955. The Skylark and its French equivalent, Centaure, of course became in due course the mainstay of the ESRO programme. The first scientific launch of Skylark was from Woomera in 1958, by which time quite a wide range of suitable instruments had been developed in the UK, including the design of Langmuir probe instruments for the study of the E- and F-regions of the ionosphere, as well as radio and radio-frequency probe experiments for studies of ionospheric electron density. Interest in this work and in the possible use of artificial earth satellites for scientific observations was greatly stimulated by the International Geophysical Year of 1957-8 not merely in the UK but world-wide. The fact that the earliest Skylark experiments took place during the IGY generated further scientific and public interest, partly in the IGY as a global scientific collaboration, and partly because of this new technology. This interest was of course dramatically intensified following the launch by the USSR of Sputnik I in 1957.

Events moved with astonishing speed under the combined pressure of these events, and COSPAR was formed, partly in recognition of the need for improved East-West scientific contacts with the satellite launcher development in US and USSR, and had its first meeting in The Hague in 1959.

That meeting saw another event which was to have a crucial impact on the development of space science in Europe and indeed in the whole world. The US delegate made on behalf of NASA an offer to open to the world-wide scientific community access to the NASA programme, including the launch of spacecraft from other countries. Whilst the view is sometimes expressed that this offer slowed the development of national programmes by diverting some of the pressure which scientists would otherwise have applied to their governments, my own view is that it opened a channel for the growth of space science outside the US to a level of maturity that otherwise could never have been achieved in the space only of a few years and so helped to bring into existence a viable space community in many countries where it might not otherwise have developed at all.

This offer was rapidly taken up in the UK, where it was seen by the scientists as a chance to work in the front rank in a way which national facilities would not have allowed for several years at the earliest. This led to an agreement on an initial programme of three satellites (though six were eventually launched), for which the US would provide the first spacecraft with a steady subsequent transfer of technological responsibility to the UK. I remember that I was in Paris in the autumn of 1959 when Sir Harrie Massey rang to ask if I could go to Washington in a couple of days; it turned out that I was expected to present the case for most of the experiments on the payload which eventually became Ariel I. As a young post-doc with two years' experience in the field, I felt that this was a heavy responsibility, especially as slides, etc, on the Birmingham experiment arrived only just before I left for the airport. These experiments included both Langmuir probe and radio-frequency measurements of ionospheric properties and also measurements of solar Lyman-alpha and soft X-ray emission, i.e., topics which both formed part of the early Skylark programme and also an important component of the early ESRO programme. The Ariel I experiments were taken to Goddard Space Flight Center in May, 1961 and launched in April, 1962.

I have dwelt a little on these events in the UK partly because when I look back on them now I am amazed at the rapidity with which they unfolded. In the space of a single decade, the foundations of space science as we see it today were firmly laid in a number of countries, a scientific union for the subject, surely an indication of maturity, was formed, and the collaborative habit which has been a major feature and also, I think, a significant strength of the way that space scientists work, was developed. From our point of view today, this period culminated in the meetings of the COPERS Working Groups in 1961, which laid down the framework of the future ESRO programme. These included a rocket programme with launchings in the northern auroral zone and a small satellite programme for the study of the ionosphere. It is not hard to spy the hand of Bengt Hultqvist, who was Chairman of the Scientific Programme group, in this. I wasn't involved in it; no doubt he will want to say something about these events.

3. *The ESRO rocket programme*

ESRO formally came into existence in 1964, and remarkably enough the first launches of both Skylark and Centaure rockets occurred during that same year — the first launch of all being a

Skylark from Salto di Quirra in Sardinia. This again is indicative of the speed with which events were moving in this period, though it also owes something to the fact that the initial operation of ESRO was really overlapped with that of COPERS, though that was nominally only a preparatory commission.

The mechanism by which the programme was put together was that the European scientific community was invited to submit proposals for instruments which they would then undertake to build — in effect the NASA Announcement of Opportunity system. An analysis of the responses received by 1967 shows the importance of the factors mentioned in the early part of this talk:

	Sounding Rockets	Satellites
Atmospheric structure and meteorology	28	13
Ionospheric and auroral phenomena	68	43
Solar astronomy	28	19
Moon, planets and interplanetary medium	21	16
Stellar astronomy	20	12
Cosmic rays and trapped radiation	12	32

There were two striking new developments during 1966. These were the Solar Eclipse campaign on the island of Euboeia in Greece, and the opening of Esrange.

The eclipse of 1966 was observed by a number of astronomers from Greece by conventional means, but the ESRO rocket campaign far eclipsed (!) these in its boldness. A temporary range was set up in a bay at Karystos, itself a major logistical task. Launchers were installed both for Centaures and for Arcas, a small American meteorological rocket. One of each of these was fired five days before the actual eclipse, both to demonstrate technical readiness and to provide a scientific baseline observation. Then at various phases of the eclipse, a further two Centaure and five Arcas rockets were fired. There is no doubt that this was a most ambitious and imaginative project for the new organisation to undertake, one which it carried through with great success.

I have a number of personal memories of this campaign. The first is of the reactions of the local villagers. 1966 was a period when relations between Greece and Turkey, usually strained, were particularly bad. The villagers noted that the launch direction of the rockets would carry them towards Turkey. Whilst they were clearly somewhat puzzled as to the precise significance of this, they had no doubt that some development of which they thoroughly would approve was imminent. In an effort to head off this mis-apprehension, one of the scientists — was it Hans Ortner, I don't remember — gave an account of the objectives of the campaign in the village hall, but the villagers were too wordly-wise to take any notice of what he said. A second is of the hardships endured by some of the scientific team, including myself. Hans Ortner hired a splendid yacht for about ten of us. During the campaign it moored in the bay off the launch site, and we went to work by dinghy in the morning. Afterwards, we went for a cruise for a few days around the Cyclades.

After the opening of Esrange at Kiruna in 1966, the programme there built up very rapidly, so that by 1967 50% of ESRO launches took place there: in addition, in 1967-8 almost 50% of ESRO payloads had an ionospheric theme. Ionospheric payloads were primarily directed towards Kiruna to take advantage of the auroral conditions which it afforded, and of the proximity of the Geophysical Observatory. This gave the background to the rocket observations, which were a snap-shot at a particular time; it also gave the particular signature by which auroral phenomena were conventionally recognised and provided in addition a range of important ground-based observations during the flight itself. Astronomical payloads was directed towards Sardinia because of the lower particle background. Nonetheless, a significant number of ionospheric payloads were still launched from Sardinia as well as from Andoya. The latter site was also capable of launching Dragon payloads which could reach rather greater altitudes, though in fact I don't think that any of these was successfully launched in the ESRO programme.

ION group payloads, were from the outset seen as closely related sets of experiments, i.e. for each an integrated scientific objective was set. Soon it was felt that the opportunity to design such objectives should not be restricted to the members of the ION group and "A0"s were issued to enable others to suggest topics for study, without any guarantee that any particular instrument would be used to carry them out, the ION group retaining the right to choose from those available the ones which it considered best suited.

This led to the payload objectives becoming more and more specialised and eventually brought with it a serious problem in that launch criteria became increasingly difficult to meet. This resulted in serious launch delays, of which the most celebrated were a Polar Cap Absorption campaign in 1968, where the Sun declined to provide suitable launch conditions for the campaign duration of two months, followed by a second in 1970 where the same happened in a campaign of duration five months. In the end, we had to accept the need for practical ways of resolving such problems.

4. The ESRO ionospheric satellite programme

The initial definition of the satellite programmes dated from the COPERS period. My recollections that the ION group started consideration of its first satellite payload from scratch, but came up with essentially the COPERS proposal as first priority. As might be expected from the background I have described, one of these, to become the *Aurorae* and *Boreas* satellites, was aimed at the study of the auroral ionosphere. The payload was quite sophisticated, consisting of no fewer than 20 particle detectors, covering the energy ranges 1 keV-1MeV for electrons and 1 keV-30MeV for protons, two auroral photometers and two instruments to measure the temperature and density of the ionosphere itself, provided by a total of seven scientific groups. *Aurorae* (ESRO I) was launched in October, 1968, and functioned quite well despite a tape recorder failure, until its re-entry in 1970. An important feature was a real time telemetric link over Tromso designed to give high spatial resolution over the auroral zone. It was intended to carry out two quasi-simultaneous experiments with rocket launches from Andoya as *Aurorae* passed over Kiruna at a time when an aurora was present, thus giving both a vertical profile at Andoya and a sweep in latitude at a more or less fixed height. However, the launch conditions — presence of an aurora and good weather at Andoya during a satellite pass over Kiruna — proved too restrictive to be practicable. One flight was made in 1968 when *Aurorae* was over the Southern auroral zone in conditions of a weak aurora over Andoya; the second became a normal auroral launch i.e. not tied to ESRO I.

The limitations of satellite measurements in height range (which were made very obvious by the beautiful height profiles — limited however to electron density — obtained by the top-side sounder Alouette) led to a proposal to launch the ESRO I flight spare into a very low orbit. This was the Boreas launch of October, 1969. During only one brief period, both Boreas and Aurora were both over the Northern polar cap at the same time in the decay phase of a solar particle event. Well-correlated variations in particle flux were observed, showing that they were changing coherently over a large region — probably the first occasion when spatial and temporal variations could be distinguished in satellite observations.

A great deal of valuable data was gathered by these two satellites, which in general tended to confirm the results obtained by those which had preceded them, but they were better instrumented than their predecessors and much new detail was obtained.

5. The end of the rocket programme

In 1968, the process of transforming ESRO and ELDO into ESA commenced, resulting in the 'Package Deal' of 1971 which included the transfer of Esrange to Sweden and led in practice to the termination of the rocket programme. The rocket programme reached its peak in 1971 when 43 rockets of all types were launched and then the last rockets of the programme were launched in 1972. The ending of the rocket programme was a response to the need to broaden the ESA programme beyond space science to include applications without at the same time requiring too large an increase in budget. It is unfortunate that the programme was brought to an end at a time when it was still scientifically vigorous and when scientific considerations alone would rather have suggested a continuation of the programme to capitalise fully on the very considerable investment that had been made in it. In fact, the second generation of instrument proposals, i.e. those developed since the start of ESRO, were just coming into the programme; an example would be the measurement of electric fields. On the other hand, it is notable that elsewhere in the world, but particularly in Europe, the exploitation of sounding rockets diminished considerably in the next decade, partly under financial pressures but also as rocket launches came increasingly to be seen as providing rather poor value for money.

Moreover, ionospheric science had itself advanced significantly in the 1960's. At the outset, it was, for historical reasons, very phenomenological and morphological in character. By 1970, however, interest was passing much more to physical mechanisms, to dynamic effects, and to the close linkage both to the magnetosphere and the neutral atmosphere. It was also being recognised that some development of plasma physics — such as identification of the process responsible for the "anomalous resistivity" which prevented the ionospheric plasma short circuiting electric field components along magnetic field lines, so permitting them to accelerate electrons to high energies, — was necessary. Thus the emphasis of ionospheric research was shifting, and it is not at all clear that the rocket programme could have continued to play the same role as hitherto, particularly as incoherent scatter radars were coming into use.

6. Conclusion

My outstanding impression is the speed with which space science developed in European national programmes and then into a European programme. In part this is because space is rather an enabling technology than a science, and the scientific foundations were already in place. It was certainly also due to some outstanding personalities, some of whom are at this meeting today. But I think it must also reflect the inherent vigour of European science and also technology in this period.

The Astrophysical Panel and HELOS

Constance Dilworth

The organizers of this meeting asked me to talk about the Astrophysical Panel. They also said that they wanted personal recollections, not a paper by an amateur historian. The trouble is, I found I had few recollections of the Panel. Checking dates, that is not surprising. The Panel functioned during the last four months of 1969, the period known in Italy as "l'autunno caldo". There was student revolt in the universities, strikes and demonstrations outside and a climate of increasing violence culminating in the bomb in Piazza Fontana in Milan that killed fourteen people. The memory bank is full of more serious things than ESRO affairs. So what I will give you is my interpretation of the minutes of the meeting, illustrated by occasional flashes of memory.

The Panel was set up, together with that on Geophysics, to advise the LPAC on a scientific programme for the next decade. The Geophysical Panel, under the chairmanship of Willmore was very efficient. It advised that ESRO concentrate on a few fields of research, renouncing for the next decade to the others. It was held up to us as a paragon to be imitated.

Unfortunately, the Astrophysical Panel was not an homogeneous body. It consisted of three cosmic ray physicists (Cocconi, Dilworth and Elliot, all old friends), a radio astronomer (Hewish), two optical astronomers (Houziaux for Starts and Kiepenheuer for the Sun), and an astrophysicist (Shatzmann), who came only to the first two meetings. The classical astronomers were a minority, and felt it.

The first meeting in Paris, at ESRO headquarters was mainly a briefing on what was expected of us, and sinks into a jumble of memories of meetings there. The second meeting, at Imperial College London on 21-22 October, was much more lively. Harry Elliot was our host. We sat around a long table and listened to surveys from experts in the various fields. There were also reports from two Mission Definition groups which had been set up to study possible ventures, one in general relativity reported by Guy Israel, the other in X-ray astronomy reported by Ken Pounds. The one in X-ray astronomy had been requested by the COS group, whose members, and particularly its chairman, were unhappy about the ousting of X-rays in the COS-B affair.

The problem in X-ray astronomy was that it was a very competitive field. Apart from various national programmes, NASA was about to launch SAS-A (later called UHURU) and had in programme the HEAO series which would give precise location, to ten arc seconds or better, of the sources found by SAS-A. The only immediately competitive technique was the classical one of lunar occultation; occasional rockets could be planned for this, but they would be very occasional.

The trouble with X-rays was that there are fewer photons per unit energy than at optical wavelengths, so to get arc second accuracies the full occultation time was needed for the weaker sources. This meant that the detector must not move faster than the moon. Now I happened to know the velocity of a satellite at apogee of a highly eccentric orbit like HEOS-A. During one of the discussions, I leaned over the table to Ken Pounds and showed him a piece of paper with some rough calculations. Ken jumped. This is how it came out in the minutes:

> "As regards spatial resolution, lunar occultation could be done by rockets with a short viewing time. On a highly eccentric orbiting satellite the viewing time is as long as the occultation time, the only limit being in determining the position of attitude of the satellite."[1]

The latter question was referred to the Mission Definition Group. I heard no more about it until — well, we will come to that later.

The third meeting of the Panel was at the Observatory of Nice. ESRO staff was there in full force, the Director General himself, the ever-persuasive Bondi, Dinkespiler, the quietly determined Director of Programmes and the not-to-be-underestimated Ortner with his Austrian charm. Hewish's contribution to the discussions was particularly welcome in that he made it clear that nothing short of an observatory on the Moon would take the radioastronomers into space. There was a lot of talk about the glorious future of optical telescopes on NASA space stations, but to no avail, the astronomers did not want 'pie in the sky'. They wanted an UVAS, OAO or OSO, now.[2]

When I told Lüst I could not get consensus he just told me to go back and do better. So we staggered on to the fourth meeting at CERN on 18 December (six days after the bomb in Piazza Fontana). My only vivid recollection of that meeting is of lunch in the canteen. A lot of people who had known me in my former rôles came to say 'hello'. One of the astronomers jumped up and was rather obviously asking a compatriot who I was. He had never understood why I should have been chosen as chairman of the Panel. Neither had I.

At that meeting we did reach some compromises and approved a draft report. The note on page 2 of that report records in diplomatic terms the tensions within the Panel:

> "One of the difficulties of the Panel lay in obtaining understanding between scientists of different disciplines. The differences are subtle but should not be underestimated,

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1 LPAC/52, Report of Second Meeting of the Astrophysics Panel held on 20 and 21 October 1969 at Imperial College London, p. 4.

2 LPAC/54, Report of the Third Meeting of the Astrophysics Panel held on 6 November at Nice, pp. 5, 7.

and, particularly on questions of experimental approach, can lead to incomprehension on vital points. The Panel feels that mixed committees of this type should be valuable in establishing the necessary rapport."[3]

The LPAC at its meeting in Darmstadt received the reports of both Panels and decided to refer them to the ad hoc groups. On 27 February, the LPAC called the Panels to a final account at Courchevel, a ski resort. Ski resorts are great fun for skiers, as most of those present were, but not for non-skiers. The only possible walk was down to the village between walls of snow two metres high. The first day there were the reports of the ad hoc groups' criticism of the Panels' work. That of the STAR group was particularly biting:

"... The STAR group concluded that the Astrophysical Panel had given priority to technique rather than the scientific aspect of fields of research ..."[4]

I felt I was a non-chairman as well as a non-skier.

The next session, ESTEC reported on the results of the X-ray astronomy Mission Definition Group. "Hell", I said, "why didn't you tell me it works?" Occhialini told me later that I had "strillato come un aquila" (screamed like an eagle). This is how it came out in the minutes:

"Mr Collette then informed the LPAC of the X-ray Mission Definition Group set up several months ago in ESRO, which had come to the preliminary conclusion that it was quite possible to carry out localisation of X-rays with an accuracy of better than 5 arc sec by using the lunar occultation method with a satellite. Investigations were taking place on an eccentric orbit of the HEOS A2 type with the possibility of correcting the orbital plane of the satellite. It was also thought possible to carry out additional observations in the low energy X-ray range. Prof. Dilworth-Occhialini underlined that the Astrophysical Panel and COS group reports would be modified by this information."[5]

The LPAC, in its "Report on Future Policy for the Scientific Space Programme of ESRO" (May 1970), adopted the selection of fields of research proposed by the Geophysical Panel. Since the Astrophysical Panel had limited itself to listing "the fields for which highest priority should be

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[3] LPAC/69, Report of the Astrophysics Panel, p. 2.

[4] LPAC/73, Report of the 31st Meeting of the Launching Programmes Advisory Committee, held at Courchevel on 27/28 February 1970, p. 2.

[5] *Ibid.*, p. 9.

given for all the areas within their competence", the LPAC made its own selection, recommending, for satellite launches in the years 1975-80, fundamental physics projects, X-ray and low energy gamma-ray astronomy, special cosmic ray projects. Stellar UV astronomy and solar high resolution astronomy were to await the NASA manned space stations. In the field of X-ray astronomy, specific mention was made of the lunar occulation satellite.[6]

In June 1970, at the LPAC meeting at Tegensee, a report by Rocchia of the Mission Definition Group (of which by then I was a member) caused the LPAC to strongly recommend "that a feasibility study should be carried out with all possible speed and that a note should appear in 'Nature' if the Director General and the Mission Definition Group felt it appropriate".[7] They did.[8]

For the feasibility study the group was enlarged to include representatives of X-ray astronomy groups and theoreticians. It was good clean fun, particularly for me because I had no chips in the game. By that time, by a quirk of Italian bureaucracy,* I no longer had an experimental group. This is one of the reasons I had no part in the realisation of two projects, COS-B and HELOS in whose design I had been engaged. Another reason I may put in a post scriptum.

As I said, the HELOS study group was good clean fun. The X-ray astronomy community encouraged us to go fast. We planned simple, light-weight detectors, putting first priority on the satellite performance. It was a very nice toy. There is a sense of power in this kind of project-making. Nothing you ask of the engineers is impossible, only 'more difficult' means more expensive. For the generation which grew up reading H.G. Wells it was heady stuff.

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6 LPAC/80, Report on Future Policy for the Scientific Space Programme of ESRO.

7 LPAC/88, Report of the 32nd Meeting of the Launching Programmes Advisory Committee held at Rottach-Egern am Tegernsee on 15/16 June 1970, p. 29.

8 J. Collett, C. Dilworth-Occhialini, R. Pacault, K. Pounds, R. Rocchia, E.R. Roth, P. Sanford, *Nature*, vol. 228, pp. 756-757 (21 November 1970).

* The quirk was this; the space groups had been converted from University groups funded by CNR (Consiglio Nazionale delle Ricerche) to CNR Laboratories. The Director of such a laboratory had direct administrative responsibility. Under the civil service rules on consanguinity, he could not sign orders of mission etc. for his wife. On the other hand the CNR refused to fund a university group operating in the same field as the local Laboratory . (The Castognolis in Turin had the same problem.) Following a carefully written essay entitled "The problem of a spouse of a Director of Laboratory" which exposed this Catch 22 situation, CNR relented to the extent of allowing a small funding of an essentially theoretical group.

In April 1971, the LPAC "recommended that this satellite should be the next scientific project to be adopted by ESRO".[9] Then came the crack-down. ESRO went into one of its recurrent financial crises. Funds were reduced, which meant that HELOS' launch would be delayed by four years or more, presumably after that of HEAO A.

Dinkespiler complained to me, privately, that ESRO scientists did not seem capable of having ideas more than four or five years ahead of NASA (a similar problem had arisen in gamma-ray astronomy). He was right. The kind of technical solutions of generally recognised problems we were dealing with were not truly original ideas. Those, like the 'out-of the ecliptic' and the tethered satellite would take twenty or so years to come to fruition. What ESRO needed at the time was a project it could afford, with a certain glamour, and competitive with NASA. The result was like a scene in a film of Tati, 'Jour de Fête'. Tati, as a French village postman, sees a documentary film on the efficiency of the U.S.A. mail, delivered even by helicopters. Spurred by the comments of his fellow villagers, he sets out to 'beat the Americans', pedalling furiously on his ancient bicycle, throwing letters and parcels into well-buckets, onto pitchforks and the butcher's block as he goes by. He ends up in a pond. Helos was a Tati adventure, but ESRO could not afford the bicycle.

The feasibility study group went back to work. It showed that the orbit and the attitude control of the satellite, designed for the occultation manoeuvres (which took up a small fraction of the orbital time) could be used to lock-in for long times of observation of the highly variable sources then being detected. So, changing the emphasis in the programme, it was still a viable project. In February 1973 it was presented at a symposium in Frascati together with the mother-and-daughter and the Venus orbiter projects. Ken Pounds and I agreed we would not push it, just present it in a 'take it or leave it' way. They took it. It became EXOSAT.

Postscript

The other reason for my exit from space research. From the nebulous talk of instruments on NASA Space Stations, there finally emerged a concrete proposal for European participation in the Shuttle programme. In the presentation by NASA in Paris, one of the speakers explained that a certain fraction of the Shuttle flights would be reserved to the US military organisations. Shortly after there was a conference in Frascati to test the response of the European space community to this new facility. To my memory, the majority opinion was contrary. The minutes of an LPAC meeting record that

> "Dr Haerendel, summarising the reaction of scientists at the symposium, said that the symposium had been useful in that it had stimulated positive comments on the use

[9] LPAC/110, Report of the Launching Programme Advisory Committee, held at Nyon on 28/29 April 1971, p. 4.

> of the space lab. However, there was scepticism regarding the spacelab in so much as there were many projects for which other means of flight would be preferred. The disadvantages such as short duration, restricted orbits and contamination had also to be considered."[10]

Nevertheless European participation in the Shuttle programme went ahead. It was the end of an illusion. In ESRO we had thought we were getting something like CERN, but CERN was based on a very compact scientific community and, strong in its name (nuclear research), could fool governments. Space research was different. They wanted missiles, meteosat and telecommunications. They fooled us. As a private, personal protest I came down to earth to build an infrared observatory at the Gornegratt.[11]

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[10] LPAC (73) 9, Minutes of the 45th Meeting of the Launching Programmes Advisory Committee, held at ESRO, Neuilly on 1 February 1973, p. 6.

[11] O. Citterio, C. Dilworth, N. Iucci, 'The Italian Infrared Telescope at the Gornegratt', *Sky and Telescope*, July 1981.

Cooperation and competition: The European space science community and ESRO*

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The scientists who, in 1959-1960, took the first initiatives to create an European organization for space research had the model of CERN in mind. This was an example of a successful organization of European countries dedicated to scientific research in a field where big and expensive technical equipment was required, equipment that no individual country could build by itself. International cooperation in space research, as in particle physics, seemed to be the only way in which Europe could compete with the United States and the Soviet Union in such a strategically important scientific, technological, and industrial field.

Space research, however, is quite different from particle physics, and if the CERN model showed that European scientific cooperation could actually work, the institutional framework and the scientific programme of the new organization were to be significantly different from that of CERN.

It is easy to identify three main differences between these two sectors of contemporary "big science." The first difference lies in the organization of the research work. In the case of particle physics this is arranged around an accelerator and supported by the facilities of a large laboratory. The laboratory, with its "big machine", its large detectors and its research staff, represents an intrinsic, stable and permanent component of the research organization. Space research, on the contrary, is conducted using scientific instrumentation carried by rockets or satellites and eventually destined to be lost with the spacecraft. Each space mission represents a definite and self-consistent element, involving at one and the same time the definition of a scientific objective, the building of the technical hardware, and the setting up of a specific managerial framework to link together scientific groups, technical teams, industrial firms, launching facilities, tracking stations, and data handling facilities.

The second difference between particle physics and space science regards their content. The former is a well defined research field, whose objectives and methods are continuously discussed

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* In this paper the author summarized the content of two reports he has published in the ESA HSR series: *The definition of ESRO's first satellite programme*, ESA HSR-2, October 1992, and *Choosing ESRO's first scientific satellites*, ESA HSR-3, November 1992.

and re-defined by a strongly homogeneous sector of the scientific community. Space science, on the contrary, is defined by its technique rather than its objectives: it includes, in fact, any kind of scientific investigation conducted by the use of rockets, earth-orbiting satellites, and deep-space probes. Each of these disciplines and research fields is characterized by its own aims and methods, by its own intellectual and institutional framework, by its own approach to the opportunities offered by space technologies.

Finally, one must mention the different roles of particle physics and space science within national scientific policies. Particle physics is pure research, with very limited, if any, possibilities for practical applications. Its large-scale development in the post-war period was mainly due to the prestige and influence that this sector of the physics community enjoyed thanks to their wartime work. Things are quite different for space research. In fact, the techniques that render this possible, rockets and satellites, have evident commercial and military applications and their development largely depends on political choices based on extra-scientific considerations.

In analysing the development of ESRO's scientific programme all these aspects must be taken into account. The scientists who contributed to this process, whether in their capacity as national delegates in the Organization's official bodies or as experts in advisory committees, were not members of one scientific community, with a well structured set of common cultural and professional values spread across national borders. And their task was not to choose the best instrument or the most promising experiment within the framework of a shared disciplinary paradigm. They represented instead various scientific and national interests, and were called to establish priorities and make choices between competing scientific disciplines and research programmes, between alternative technical options, and between different national interests and policies. They were not members of an international scientific elite who could confront the political decision-makers armed solely with well-defined research goals. They were rather advocates of a variegated set of new research fields who had to negotiate among themselves and with national governments to establish the place and fortunes of these fields in the wider framework of space activities.

In the title of this paper I have used the opposing terms *cooperation* and *competition* to define the internal dynamics of the European space science community in the early days of ESRO. Cooperation, on the one hand, was required to convince European governments of the necessity to pool resources for a joint effort in space, and to define a research programme competitive with those pursued in the United States and the Soviet Union. Competition, on the other hand, was inevitable when it became clear that the limited resources available did not allow a balanced development of all research fields and all technical options. Loyal cooperation was mandatory to implement satellite projects whose scientific payload was to be provided by groups from different countries and different institutional settings, speaking different languages and expressing different scientific cultures. Competition became fierce and impassioned when only one or two projects could be approved among several equally valid from the scientific point of view.

In this presentation I will review a few moments of ESRO's life, when important decisions were taken which affected significantly the development of the satellite programme of the Organization.

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Let's start from the very beginning. The first scientific programme for ESRO was described in the so-called *Blue Book*, a report prepared in 1961 by the Scientific and Technical Working Group of ESRO's precursor, the European Preparatory Commission for Space Research (known as COPERS, from its French initials).

Two aspects of this document must be stressed. The first regards the question of ESRO's in-house research laboratory. This had been a matter of controversy during the preparation of the *Blue Book*. The opinion of the majority of the Scientific and Technical Working Group was very clear: ESRO should not compete with universities and other research institutes in carrying out purely scientific research. All the scientific work had to remain the responsibility of scientific groups outside the Organization. The latter should only be responsible for the engineering development of satellites.

Three main arguments were given to support this idea:

(a) Firstly: if a central scientific establishment was set up, it would drain scientists from the national space programmes;
(b) Secondly: ESRO scientific groups would easily enjoy the best staff and the best facilities and therefore would take over the most sophisticated and interesting experiments;
(c) Finally: lacking a central research laboratory, the scientific activity supported by ESRO had to be distributed in the participating countries, and this would foster space research in Europe.

In other words, ESRO's founding fathers did not conceive the new Organization as a centre of scientific excellence, as CERN had been ten years before. While recognizing the need to pool technical resources, European space scientists also wanted to protect their national research programmes and could not accept a central research establishment when clear priorities between the various research fields had not yet been established. They looked at ESRO as an instrument to give international momentum to research programmes defined within the different sectors of the space science community, and as a technical institution not to be involved as a protagonist on the competitive ground of science.

This position was not held unanimously, however. Both within the scientific community and among the COPERS delegations it was felt that ESRO's role could not be limited to providing Member States with technical facilities. It was advocated that the Organization should also support vigorous research programmes in pure and applied science. In the event, the compromise that was reached and carefully worded in the *Blue Book* came close to the scientists' views. ESRO should have a small research group; its aim, however, was only to provide "opportunities for original research beyond those which exist in individual countries," and not to pool scientific resources to run scientific projects in competition with laboratories in member states.

Conceived and advocated as an international organization solely devoted to scientific research, ESRO did not in fact come out as a scientific institution, with its own scientific staff and a research programme, a recognised leadership role among other scientific institutions in member states, and a strong negotiating power *vis-à-vis* member states' governmental institutions. It eventually presented itself in the twofold aspect of a rather cumbersome multinational bureaucracy and a technical establishment conceived to use most of its operational budget for industrial contracts in member states according to the principle of *juste retour*. It rapidly became a sort of battleground where difficult and complex negotiations among various interest groups were required in order to reach compromise and agreement.

The second aspect of the *Blue Book* I wish to discuss here regards the scientific programme. This programme extended over eight years and presented three kinds of projects. The first category included experiments to be performed by sounding rockets. The second comprised small satellites in near earth orbits and small deep space probes. The third involved the realization of large and sophisticated spacecraft. The time schedule and launching rate were rather ambitious. The *Blue Book* in fact foresaw an annual launch rate of 65 sounding rockets (from the third year) and the successful launching of 17 spacecraft (from the fourth year), namely 11 small satellites, 4 space probes, and 2 large satellites.

The list of proposed fields of study included in the programme covered practically all fields of space science and no priority was explicitly given, either between different kinds of projects or between research fields. This was obviously the easiest course of action in a phase when space science in Europe was at its very beginning and no research group or disciplinary community had the experience and the prestige to advocate clear priority choices. A great deal of optimism about the technical and financial possibilities led to a gross overestimation of the number of spacecraft ESRO could actually realize, thus giving the illusion that this highly ambitious and unfocussed programme could be implemented.

In conclusion, ESRO's scientific programme was rather a *manifesto* of interests and expectations (should we say a book of dreams?) than a concrete working hypothesis. It reflected the intentions and hopes of important sectors of the European scientific community that lacked, however, the strength and the lucidity that can only derive from an established tradition, from a common patrimony of professional values, and from a substantial homogeneity of aims and methods. When ESRO moved forward from the inspired vision of a few pioneers to the hard political and financial realities of space policies, it was inevitable that the transformation of the *manifesto* into a true operational programme would be a long and laborious process and the results disappointing.

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The first occasion for conflict arose in the second half of 1966, when the ESRO Council refused to authorize the carrying forward to the second 3-year period (1967-69) of the unspent funds originally allocated to the first. This caused a severe financial crisis which called for a dramatic revision of the agreed programme. In particular, it became clear that there were not enough funds to proceed with work on both of the two medium-size satellites TD-1 and TD-2 and the Large Astronomical Satellite (LAS). A choice was required that had important implications for the Organization's scientific policy.

Three elements favoured the LAS. First, it was the kind of large project for which a pooling of resources and technical expertise from several countries was required: in other words it was just the kind of undertaking from which ESRO had derived its *raison d'être*. Secondly, it met the interests of a strong sector of the scientific community, the stellar astronomers, for which the access to space technologies meant the possibility to study stellar radiation in the ultra-violet range, where atmospheric absorption prevented the use of ground based telescopes. Finally, the LAS was advocated by the British space science community and was strongly supported by the United Kingdom delegation in ESRO.

Against these elements, others worked in favour of the two TD satellites. These two satellites, for which tenders had already been called, were multi-experiment satellites, carrying together as many as 18 different experiments built by 13 research groups in 7 member states, and covering fields as different as solar and stellar astronomy, X- and gamma-ray astronomy, cosmic-ray physics, atmospheric and ionospheric physics. TD-1 and TD-2, in other words, could count on widespread scientific and political support. Moreover, the TD-spacecraft was then considered as a sort of workhorse satellite for the implementation of ESRO's scientific programme. Any setback in this project, while helping astronomy, would significantly handicap other research fields.

Finally, if Britain supported the LAS, together with the Netherlands and Germany, France strongly opposed it, together with Belgium and Italy. The opposition was based on both scientific and political grounds. On the one hand, it reflected the position of a significant fraction of the astronomical community who, against the complex, high resolution telescope of the LAS design, had advocated a less sophisticated instrument, with lower resolution and more adapted to the technical and financial resources available in Europe. On the other hand, in the opinion of several delegations, engaging ESRO in a project like the LAS, which would absorb most of the resources of the Organization and which was so strongly dominated by the British, would jeopardize other parts of the programme and would undermine the multinational character of ESRO.

In the event, as you know and as my colleague John Krige has described in detail, the LAS was abandoned and European stellar astronomers lost their first chance to get involved in space research with ESRO.[1]

They lost the second chance two years later, in spring 1969, when ESRO's scientific advisory bodies were called on to recommend the new satellite project. Two main options were on the table. The first was UVAS, a satellite for UV astronomy which was nothing but a less sophisticated version of the LAS. The second option foresaw the realization of two satellites, COS-B and GEOS, the former carrying an instrument to investigate celestial gamma rays, the latter carrying several instruments into geostationary orbit to study magnetospheric phenomena.

I have discussed elsewhere the long and complex decision-making process which led to the choice of COS-B and GEOS against UVAS and therefore I will limit myself here to a couple of comments.[2] The first regards again the competition, in this case very harsh, within the space science community, facing the growing financial difficulties of ESRO and the rapid evolution of their research fields. The three projects were equally good and promised an assured return in scientific information. The choice among them was therefore not a matter of scientific merit but a matter of scientific policy, namely (a) whether to prefer UV astronomy or high energy astrophysics and space geophysics and (b) whether to invest ESRO's limited resources in one big project or in two less challenging projects. Again European astronomers lost the competition, and this affected the subsequent development of ESRO' scientific programme.

The second comment regards cooperation. COS-B and GEOS marked an important turning point in the development of ESRO's satellites. The former was the first single-experiment, observatory-type satellite of the organization and this implied that the success of the mission depended on the close coordination of all the different people responsible for the implementation of the project. This was successfully achieved in spite of the fact that the payload was built by several groups spread all over Europe who were funded independently. The organization and management of the COS-B project was a challenging problem for ESRO, whose solution had important implications for the further evolution of scientific satellite management in ESA.

GEOS too implied a good deal of cooperation. In fact, owing to the peculiarity of the satellite's scientific mission, the problem of cooperation among the different experimenters, as well as between them and the scientific community at large, arose in more urgent terms than previously. While the experiments in previous ESRO's satellites were relatively independent from one another and each group dealt with its own data on its own, the GEOS experimenters from the very

[1] J. Krige, "The rise and fall of ESRO's first major scientific project, the Large Astronomical Satellite (LAS)", *History and Technology*, vol. 9, pp. 1-26 (1992).

[2] A. Russo, "Choosing big projects in space research: the case of ESRO's scientific satellite COS-B", *History and Technology*, vol. 9, pp. 27-61 (1992).

beginning discussed ways to achieve the maximum scientific value from the satellite and defined common modes of operation and data handling. They also established official contacts with other scientists within the framework of the world-wide "International Magnetospheric Study."

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If stellar astronomers did not succeed in having their space telescope included among ESRO's spacecraft, their colleagues interested in solar phenomena were no more fortunate. In fact, the second spacecraft in the TD satellite programme was intended to be devoted entirely to solar astronomy, while a third was planned to be a ionospheric satellite. It was assumed that both satellites would be launched in time for the solar maximum of 1968-69, in order to study the correlation between solar activity and ionospheric phenomena. The TD programme, however, suffered a severe set-back in 1965, due to the financial difficulties of ESRO, and it was eventually decided to abandon the ionospheric satellite TD-3 and to re-define the scientific mission of TD-2, now to be considered as "a solar, ionospheric and geophysical satellite."

This was nothing but a compromise based on the idea that, in order to meet the interests of several sectors of the space science community, it would be possible to combine in a single spacecraft scientific objectives which pertained to very different scientific fields. Moreover, it was confirmed that the design of TD-2 should be the same as TD-1, the first satellite in the series, whose mission and payload had already been approved. It was hardly surprisingly that it soon emerged that this was a bad compromise, and quite unworkable.

The conflict in fact broke out a few months later, in the summer of 1965, when the Launching Programme Advisory Committee (LPAC) was asked for its recommendation on the TD-2 payload. On the one hand, the physicists involved in ionospheric research strongly advocated the inclusion of a special instrument to study the topside of the ionosphere (*topside sounder*). On the other hand, it appeared that the inclusion of such an instrument would be incompatible with the stabilization system designed for the TD spacecraft and therefore with the solar pointing experiments.

In the event, the LPAC recommended the exclusion of the topside sounder, as its members were convinced that it was essential for ESRO's scientific programme to have a standard design for a medium-size stabilized spacecraft and to foster solar physics studies. This decision, however, caused a strong reaction from the ionospheric physics community, whose spokesmen challenged the whole scientific policy of the LPAC and blamed the Committee for what appeared to them to be an unfair distribution of the experiments allocated in ESRO's satellites between astrophysical and geophysical investigations. They won support from the Scientific and Technical Committee (STC), who upset the LPAC's recommendation and decided, by a majority vote, to include the topside sounder in the TD-2 payload. They finally lost the battle on financial grounds, however. ESTEC engineers demonstrated that making two different spacecraft for TD-1 and TD-2 would have doubled the cost. A new vote was called for in the STC. The majority was now against the topside sounder and endorsed the payload originally recommended by the LPAC.

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This was not the end of the story of TD-2, however. By early 1968 it appeared evident that the financial costs of the joint project TD-1/TD-2 had been greatly underestimated. This circumstance, as well as the deep dissatisfaction of Italy towards the geographical distribution of ESRO's industrial contracts, led to the abandonment of TD-2 and the LPAC was asked to recommend a rescue operation for the TD-2 experiments. The real issue, of course, was the future of solar physics in ESRO. Only one solution appeared feasible both from the scientific and the financial point of view, namely to buy an OSO (*Orbiting Solar Observatory*) spacecraft from NASA to fly the experiments already approved for TD-2. This solution, however, was not acceptable from the political point of view, as it was of no technical interest to Europe and thus contrary to ESRO's industrial policy. Only the non solar-pointing experiments were thus rescued in the small satellite ESRO-IV, while the solar physics community could only advocate that ESRO should start an advanced solar satellite project in its future scientific programme.

They did not succeed. In the fall of 1969, in fact, the LPAC gave fresh consideration to the problem of ESRO's long-term scientific policy and it eventually recommended that solar and stellar astronomy be excluded from ESRO's satellite programme and confined to rocket experiments. According to the new scientific policy, primary consideration had to be given to a very limited number of research fields, with particular emphasis on high energy astrophysics (X-ray and low-energy gamma-ray astronomy) and plasma physics investigations in the magnetosphere.

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At the beginning of the 1970s ESRO was reaching maturity. Some of the muddled enthusiasm of its early period had faded away and a new awareness of the role and limits of the Organization had emerged from the difficult times of crisis. It was definitely recognized that ESRO could not support all fields of space science in a viable way; that the time when any scientific group could expect to get its experiment flown on board of one of ESRO's spacecraft was definitely over. The Organization had to develop mechanisms for selecting a few, well phased major projects according to agreed scientific guidelines.

Important changes in the organizational structure gave more flexibility and autonomy to the Organization. The scientific programme could now be planned on a more secure basis and member states reaffirmed their confidence in ESRO by agreeing on its extension beyond the 8-year period covered by the Convention. Moreover, they entrusted to ESRO the task of studying and eventually implementing a new programme on application satellites. In the difficult and controversial context of European cooperation in space, ESRO represented the most solid element.

The start of space research in Sweden during the COPERS and early ESRO years, with personal recollections

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1. Introduction

With space research I will here mean research carried out in space with the use of rockets and satellites. Such research was started in Sweden under the influence of, or as a direct consequence of, the European initiative by Amaldi and others, as I will describe below. But space research, with the meaning of research in the field of space physics, had already before the launch of Sputnik 1 long traditions in Sweden, with names such as Celsius and Hiorter in the first half of the eighteenth century and Hannes Alfvén in our time, not to mention many in between.

More systematic experimental or observational research in space physics was initiated after the second world war by the Royal Swedish Academy of Sciences in northernmost Sweden, which is located in the auroral zone. At the beginning of the International Geophysical Year, which covered the period 1 July 1957 to the end of 1958, the Academy opened a new geophysical observatory in Kiruna. Kiruna Geophysical Observatory was the first research organization in Sweden with a broad responsibility in the field of observational/experimental space physics research, but there were several other research groups at universities or research institutes, which carried out experimental research programs in certain parts of the space physics field, such as Chalmers Technical University and the Swedish Defense Research Institute in ionospheric physics and radio propagation, Uppsala University in cosmic ray physics, and the central Swedish agency responsible for shipping and navigation in geomagnetism. All these groups conducted measurements also in the Kiruna area.

The main reason for concentrating a large part of the Swedish space physics research to Kiruna, 140 km north of the Arctic Circle, was of course the fact that the auroral zone offers the possibility of investigating from the ground the hot plasma physics of the Earth's nearest environment, the magnetosphere. The most spectacular effect of the hot plasma in the magnetosphere is the aurora. The fact that Kiruna is located in the auroral zone in that part of the world where the climate at these latitudes is mildest, because of the Golf Stream, thus constitutes a "natural resource" for space physics research.

The efforts to exploit the above-mentioned natural resource after the end of the war were, as mentioned, led by the Royal Swedish Academy of Sciences, where Rolf Sievert was the main driving force. He was the founder and leader of the Swedish Radiophysics Institute in Stockholm and Swedish radiation protection activities and has given his name to one of the units of radiation dose. In the Academy he chaired an Interim Board for Northern Research Stations, which led the planning and preparation work for a geophysical observatory in Kiruna. Figure 1 shows a group of Scandinavian scientists participating in the preparatory work for Kiruna Geophysical Observatory, meeting in Abisko north of Kiruna in 1946, under the chairmanship of Rolf Sievert. Several of the leading Scandinavian geophysicists were present. The Norwegian auroral physicist L. Harang is found in the front row together with the Finnish and Danish geomagneticians, E. Sucksdorf and J. Olsen. No. 3 from the left in the back row is Hannes Alfvén, who was the dominating scientific inspirator in the space physics field. N. Ambolt (no. 2 from the left) was, next to Sievert, the strongest contributor to the efforts aiming at establishing Kiruna Geophysical Observatory.

In 1956 the Government provided a necessary main contribution of the investment funding for a main building, which was ready in the summer of 1957 and was officially opened on July 2.

I worked for Sievert from 1951 (with a years gap for military service) and took my Ph.D. in April 1956, after which I was offered the job as the head of the planned Kiruna Geophysical Observatory. During the winter 1956/57 I planned and acquired the equipment, furniture, and other equipment for the new observatory, and in May 1957 I moved to Kiruna with my family. At that time I had no idea of the revolution that satellites would bring about in the space physics field. My, and other Swedish scientists', plan was to study the physics from the ground only, using radiation of various kinds. Almost exactly three months after the inauguration of Kiruna Geophysical Observatory (KGO) Sputnik 1 was launched, on October 4, 1957. The small scientific group at KGO immediately engaged itself in the use of the radio transmissions from the satellites for determination of the total electron content of the ionosphere from the Faraday rotation of the polarization plane in the geomagnetic field. One of the very first Ph.D. theses that was based on research work at KGO dealt with this subject. Sputnik III, launched in early 1958, was the most important radio source for such studies in the first years of the space age.

The very first use of sounding rockets for upper atmosphere research in Sweden occurred at Kronogård, some 200 km south of Kiruna, in 1961. It was a USA-Swedish enterprise, lead on the Swedish side by Bert Bolin, professor of meteorology at the university of Stockholm. Rocket launchings were carried out at Kronogård also in the following years, 1962-1964.

At this time the planning for Swedish participation in the European cooperation for space research was well under way.

2. *The effect of Amaldi's European initiative in Sweden*

Stimulated by Amaldi's initiative in early 1959 and the following discussions in the leading circles of CERN, a Swedish Committee for Space Research was established in the spring of 1959. The person playing the leading role in this development was Gösta Funke (Figure 2), secretary general of both the Natural Sciences Research Council and the Atomic Physics Research Council. He was the leading Swedish representative in the Council of CERN. The Swedish Committee for Space Research was established jointly by the two above-mentioned research councils, the Technical Sciences Research Council and the Swedish Defense Research Institute.

Lamek Hulthén (Figure 3) was appointed chairman of the Space Research Committee. He was professor of mathematical physics at the Royal Institute of Technology (Kungl. Tekniska Högskolan, KTH) in Stockholm and at that time physics representative in the Natural Sciences Research Council, among other things. Secretary of the Committee became Ernst-Åke Brunberg (Figure 4), a young cosmic-ray physicist from Alfvén's group at KTH. He was secretary of that committee and its successors until 1973, when he resigned and was succeeded by Kerstin Fredga, now Director General of the Swedish National Space Board.

The main task of the Swedish Committee for Space Research during the initial period was to investigate the interest and needs for specific Swedish actions in response to the European ones. Some meetings with interested scientists were held, among other things. At one such meeting at KTH in the winter 1959/60 I suggested that Sweden should propose that a sounding rocket range for use in the European collaboration should be established in the Kiruna area. I may not have been the only one who had such an idea and it was brought into the European discussion by the Space Research Committee at a meeting in London in April 1960.

Lamek Hulthén and Ernst-Åke Brunberg participated actively in the European planning meetings and at a meeting in Paris in June 1960 Hulthén was elected one of three vice-chairmen of the then established GEERS (Group d'Etudes Européen pour la collaboration dans le domaine des Recherches Spatiales). When COPERS (Commission Préparatoire Européenne de Recherches Spatiales) came into force, Hulthén was asked to chair its Scientific and Technical Working Group (STWG), which was set up at the first session of COPERS, held in Paris on 13 and 14 March 1961. Reimar Lüst, who became the first scientific director of ESRO and later Director General of ESA in the 1980ies, was Secretary of the Working Group. Besides the Scientific and Technical Working Group there was another one which had the task to prepare the convention text and the financial and administrative rules of the European Space Research Organization (ESRO).

Hulthén called the first meeting of STWG at KTH in Stockholm in April 1961. I was asked by Brunberg to prepare a proposal for sounding rocket experiments and to present it at that meeting, which I did. That was the first time I was involved in planning work for ESRO and it was also the first time I met Reimar Lüst, Robert Boyd and several other leading European space physicists. They impressed me very much. Boyd was, as I remember it, the dominating personality at the meeting, very persuasive and, of course, mastering the common language of the meeting, broken English. We interacted a lot in the following years, sometimes with strongly different views. Boyd said many memorable things. One statement which I still remember with great pleasure, was when the official languages of the first international scientific meeting of COPERS (or ESRO) was discussed and the conclusion was that they should be English and French. Then Boyd stood up and said: "I propose that we shall allow the Americans to speak their own language". Reimar Lüst started his exceptional career in the European Space Research field at that meeting and he became very soon one of the most influential persons in the inner circles.

At the Stockholm meeting STWG established four subgroups, on "Scientific Programmes", "Technology", Tracking and Data Handling" and on "Vehicles and Ranges". To my great surprise I was appointed chairman of Subgroup I on scientific programmes. I was a few and thirty years old and had only four years earlier switched scientific field, from radiophysics to space physics. I certainly felt the heavy responsibility. But I did what most of us did, tried hard. Many of the participants at the meeting were young. I was only some years younger than Reimar Lüst, for instance. And we had really no great problems, for one reason because there were only unexperienced people available in Europe.

The first meeting of Subgroup I was held in Kiruna on 27 and 28 April 1961. I reported to the second meeting of STWG in London on 8-9 May 1961. As a result of discussions at the second plenary meeting of COPERS in the Hague on 17 and 18 May 1961 and at meetings of STWG in Munich on 25 and 26 May and in Paris on 12 and 13 June, Subgroup I met again on 20 June and on 6-8 July 1961 and prepared a revised text which was discussed by STWG in Paris on 27 and 28 July. The draft report of STWG was presented to the third Plenary Session of COPERS in Munich on 24 and 25 October 1961. The second edition of the STWG report was finally published as "the Blue Book" in December of 1961.

3. Recollections from the preparatory work

For a young man from the periphery of Europe, who was thrown into an intensive European planning work, travelling all over Europe and meeting colleagues as well as central bureaucrats and diplomats from all parts of Europe, the early sixties was an enormously exciting period. It was certainly a privilege to start a scientific career when a new research field was born, and it was another privilege to be allowed, as a very young man, to play a role in the birth process for European collaboration in space. The main reason for the low age of many of the members of the COPERS planning teams was, as mentioned before, that the field of space research was new and there existed very little experience of that field in Europe. There were, of course, some very senior persons involved at the top level of the preparatory work (e.g. Amaldi, Auger, Massey, de Jager, Hulthén) but they were not there because they had very much experience of space research but rather as leaders of the community in general. The young scientists, who wanted to participate themselves in space experiments, presented most of the views on what should be done and later on they came to dominate also the advisory and deciding bodies of COPERS, ESRO and the ESA scientific programmes. Allow me a few personal recollections of what it was like to plan and negotiate the European collaboration in space research.

One of the strongest experiences I remember from the first months is how much easier it was for a Swede to communicate with people from northern Europe (Scandinavians, Germans and Englishmen) than with Gallic and Latin people. I had been taught in school and everywhere to be concise and precise in expressing myself and I found that the Northeuropean representatives seemed to have learnt the same. The French, Italian and Spanish members of the COPERS planning organization clearly demonstrated to me a quite different "culture". I found them using so many more words and so much more sophisticated ways of expressing themselves than I would do in expressing the same thing, assuming that I had understood what they meant. But frequently I was not at all sure that I really understood what they meant, including the fine nuances. Professor Auger, in particular, was a master in leaving me, and hopefully also other northerners, with some feeling of not having quite understood everything.

Another strong experience for me was the difference in general attitudes between scientists in general and representatives of the ministries of foreign affairs. There were almost only scientists involved in the early phase of planning and negotiation work for ESRO. I found that we all, irrespective of from which country we came, showed good will and good faith in each other. The work progressed fast and with little difficulties. After a year or two diplomats were added to our groups and I remember quite vividly how with them suspicion was introduced. They started to ask us what representatives of other national delegations *really* meant with what they said. And when I said that I thought the members of the delegations meant what they said, my colleagues from the ministry of foreign affairs did not believe me. I learnt from them that the most serious mistake you can make in international negotiations is to be fooled. One reason for the differences in attitudes mentioned is probably that scientists have less possibilities of fooling each other than the participants in most other branches of human activities.

That scientists do not fool each other does certainly not mean that they always agree. Differences of opinions become stronger when the work proceeded from planning to executing space research programmes. I became the first chairman of the ION group, after the planning work for COPERS had been finished and the detailed planning of the ESRO scientific missions had started. The ION group was responsible for what today would be called magnetospheric physics or space plasma physics.

In the planning of the TD-2 mission the ION group, which consisted of about half a dozen leading representatives of the special research field, put highest priority on one special experiment being part of the payload. The Launching Programme Advisory Committee (LPAC), which collected the recommendations from the various advisory groups and made a payload proposal, did not include the experiment given highest priority by the ION group. The LPAC argued its right to introduce other considerations than scientific priority into the selection of a payload, in this case technical ones. The ION group on the other hand argued that it alone was to put priorities within its field of science. So it was a real disagreement on principle. The ESRO Scientific and Technical Committee (STC, which had replaced the COPERS' STWG) judged first in favour of the ION group, but later in the process the view of LPAC was accepted. So the ION group won the battle but it lost the war. And this happened in spite of the fact that Professor Auger, the first Director General of ESRO, after the first battle told me that the ION Group won because it was right. This also illustrates that Professor Auger did not influence some important ESRO processes very much, although he was the Director General.

4. Some comments on the scientific programme of the Blue Book

Looking back upon the proposed scientific programme in the Blue Book today one is struck by the very large numbers is contained. When the ESRO activity had reached its final level towards the end of the eight-years period for which plans were presented, 65 sounding rockets, four small satellites in near earth orbits, three space probes and one larger astronomical satellite were expected to be launched per year. In reality only a small fraction of the planned annual number of sounding rockets and a total of seven satellites were launch during the first eight years of ESRO, i.e. from 1964 to 1972. The total economic resources of some 300 million US dollars allocated for the first eight years of ESRO soon turned out to be much too low.

The scientific programme in the Blue Book thus was found to be exceedingly unrealistic. We who planned the programme were, of course, quite unexperienced of space research, but that may still not be the main explanation. The plans were based upon the limited experience that there was in the World. In the period October 1957 to early 1958 Soviet Union launched Sputnik I, II and III. A single university institute in USA, Van Allen's institute at the university of Iowa, launched one satellite per month in the early part of 1958: Explorer I on 1st of February, then the rocket carrying Explorer II failed, Explorer III was launched successfully on March 26, Explorer IV was sent up on July 26. In addition the Van Allen group supplied scientific instruments for two more missions in 1958, namely for Pioneer I and Pioneer III, which were sent to impact the moon (but failed).

It was this kind of experience that was behind the figures presented in the Blue Book. It did not seem unrealistic to us that the whole of Europe should be able to compete with Van Allen's group in terms of number of satellite launches per year. What we, and most likely all others in Europe, were unaware of was the enormously fast rise in sophistication and complexity of the new satellites that were required to carry the science further. The situation in which even the simplest instrument launched into a satellite orbit would produce interesting new scientific results was very short lived. It was essentially explored by Van Allen and by the Russians. Also in USA and USSR the frequency of launches of satellites providing exciting new results decreased very much in the sixties. But we who planned the programme in the Blue Book were not aware of that yet in 1961.

5. ESRANGE in Kiruna

From Swedish point of view the fact that the Blue Book contained the proposal to establish ESRO's sounding rocket launching range in the Kiruna area was most important. The proposal was the result of a study in which also other locations had been investigated, among them Narsarsuak on Greenland and Sardinia. That Esrange was placed in Sweden played a major role for the Swedish Government's decision to join ESRO. That decision was taken in March of 1962. The interest of the Government was otherwise low and it is not unlikely that Sweden, like Norway, would have decided not to join ESRO if Esrange had not been proposed to be located in northernmost Sweden. More about the attitudes of the Swedish Government towards space research can be found in the contribution to this volume by Jan Stiernstedt.

A few personal recollections from the planning and construction period of Esrange in the years 1962 to 1966 are the following. In the summer of 1962, two young men in leading positions in the COPERS secretariat in Paris, Messrs Blassell and Vandenkerckhove, came to Kiruna to establish the location of Esrange and its various parts. I brought them out to Vittangi river along an almost nonexistent road through the open country from Paksuniemi near Jukkasjärvi. We climbed what is now called the radar mountain and very quickly found the obvious locations for all the major parts of the facility: main building, launching area and radar station.

The impact area of Esrange contained no permanent residences, but the Lapps had and have temporary residences within the area and also some hunting huts existed. A few marshland meadows used long ago also existed within the boundaries. In the years 1963-65 I was somewhat involved in the negotiations with those who had economic interests within the rocket field and it was interesting to see how valuable all assets suddenly became. On the whole the negotiations went, however, very well. The Swedish Government compensated the Lapps and property owners for the disturbance that the rocket launching activity caused them. Also the safety problem was solved in a rational way. A number of shelters (18 originally) were built, where all who were in the field when a rocket was to be launched could find refuge. That the shelters only very rarely are used for their purpose is less important. In principle the security of all who are within the range can be protected. In order to be able to seek shelter in the periods when rockets are launched those in the field have to be informed about the impending launch. For this purpose radio receivers were provided and announcements were, and still are, distributed over one of the broadcasting channels covering the area. The first design study by ESRO of the portable radio receivers to be distributed to the persons who entered the range area is an amusing illustration of another kind of inexperience than the one demonstrated by the programme planners mentioned above. The radio receivers were specified by some ambitious young engineers in Paris. The specifications required the receivers to be able to work at -40° and to survive being dropped to the bottom of a 10 m deep lake. As a result the battery had to be so big and the casing so heavy that the total weight of the receiver became completely unrealistic: many kilograms. No Lap would agree to carry such a burden with him in the field. Finally the choice fell upon commercially available radio receivers of low weight.

Another example of overspecification by overambitious, unexperienced young men in the young ESRO organization is the meteorological tower that still stands at Esrange. The worst possible recorded climatic conditions in Antarctica, with very heavy ice cover were used as a basis for the specifications. As a result the tower was bought in France and transported to Kiruna on special wagons. It became some ten times more expensive than if an ordinary Swedish television tower had been used. But it has worked well for almost thirty years by now.

At the inauguration of Esrange in September 1966 I had the honor to officially open the range with the crashing of a bottle of champagne against a Centaur launcher (Figure 5).

6. The years 1963 to 1971

With the minister of higher education and research, Ragnar Edenman, as responsible minister, the Government on 2 March 1962 presented to Parliament the proposition that Sweden should join ESRO and that a special committee should study the future organization of Swedish space research and propose to the Government. The minister ended his declaration by stating that the setting up of a space committee did not mean that he had taken any stand in the question of future Swedish space activities.

The official Space Committee, under the chairmanship of Lamek Hulthén proposed in September 1963 to the Government that a space research board and a space institute should be established to lead Swedish space research and its coordination with ESRO. In March 1964 the Government rejected the proposal and left to the research councils to look after the support of Swedish space research and to a special ESRO Committee to be the contact agency with ESRO. The decision not to give any special support to the Swedish space research was based on a lack of interest within the political circles but also on the responses from various scientific bodies in Sweden to a question from the Government about their attitudes to the proposal from the Space Committee. Most answers were fairly negative. Space research was said to be too expensive and one could generally not accept that existing governmental research money was transferred to a special space research organization. This result was a real blow to the Space Committee and the Swedish space researchers.

The research councils formed in 1964 a joint space advisory group (called Rymdnämnden). The first chairman was Lamek Hulthén. When he resigned in 1967, I was appointed chairman. For the ESRO Committee the Under-Secretary of State of the Ministry of Education was first the chairman, but after a year or two Jan Stiernstedt (Figure 6), who was the next man in rank in the ministry (Permanent Under-Secretary of State), took over the chairmanship. Stiernstedt had been handling space matters in the ministry for several years and he became the leading official in the Swedish space organizations and played the main role in the process of giving Sweden a new organization for space activities in 1972. He was also chairman of the Council of ESA for a period in the seventies.

The second half of the sixties was in many ways a "desert walk" for Swedish space research. The amount of funds available was very limited. A small technical group (Rymdgruppen) was supported. Its tasks were to handle technical matters connected with the space experiments, such as sounding rocket payload integration. The funding was, however, so limited that the group could not be kept busy. Finally, in 1970, the entire Rymdnämnden resigned, stating that their task could not be met and that continuation of its activity was not meaningful. This was one reason for the change of organization of Swedish space activities that was decided by the Government in 1972, but that is another story and shall not be dealt with here.

The meagre funds available for Swedish space research in the years 1964-71 were enough to support the development of the few Swedish experiments that were accepted for ESRO satellites. The spacecraft launched in the period mentioned with Swedish experiments on board were ESRO 1a, ESRO 1b and ESRO 4. The only Swedish group, which carried out satellite experiments in the sixties and seventies, was that at Kiruna Geophysical Observatory (the name was changed to Kiruna Geophysical Institute in 1973 and to Swedish Institute of Space Physics in 1987).

The ESRO 1 satellites were extremely important for the Kiruna group. The experiment that the group developed for them worked very well and provided a scientific data base which the group worked on for some ten years. On ESRO 1 all participating groups were new beginners. We learnt the "trade" there and we got into the "club" of groups who had experience of developing and flying satellite experiments. As such experience later on was required by the major space agencies for acceptance of proposals, that first experiment was the key to the future.

It may be mentioned that ESRO 1 was developed before the era of integrated circuits. The Kiruna experiment was built with discrete components. As a result we could reduce the power consumption to half a watt for the entire experiment, which is an order of magnitude less than for modern experiments. But the available power on the satellites has increased very much since then, so the importance of limiting the power is now much less than it was on ESRO 1.

7. Some comments on the relationships between the different scientific disciplines

The organizers of this meeting in Palermo in November 1992 have suggested that I say something about the subject in the title, so I do that.

In Sweden, as in most of Europe, the space physicists, i.e. those interested in the physics of the upper atmosphere, the ionosphere, the aurora and the magnetosphere (of which the latter did not exist as an accepted concept in the early sixties), were ready and eager to use the new space technology when it first appeared. The astronomers were much less interested at that time. There were several reasons for such a situation. First of all the early space technology could satisfy most of the needs of the space physicists, but it could not satisfy the astronomers. Whereas the space physicists could observe from ground a lot of the consequences of the hot plasma processes in the Earth's environment, they needed to send instruments out into space to be able to measure the hot plasma itself. Any high-inclination orbit and any orbit apogee were interesting in the early period. Spinning spacecraft are most desirable for space physicists, and spinning was the first and simplest way of stabilizing spacecraft that was developed by the technologists. The astronomers could only hope that some time in the future sufficiently accurate pointing systems would be developed. They could generally not build their experiments themselves but depended upon the development by space agencies of their observatory satellites. There was a Large Astrononical Satellite (LAS) discussed already in the first years of ESRO, but it became quite soon clear that it was not realistic at that time.

In Sweden the National Space Board took the initiative to establish and support a special group for infrared astronomy, but that did not happen until the seventies. When the UV observatory, IUE, was in space ready to be used, the Swedish astronomers like the astronomers in most other countries rallied to use it and now in the nineties there is a strong interest in using IUE, Hubble space telescope and ISO. In the field of high energy astrophysics no significant interest has developed yet in Sweden. In the submillimeter wave length range a Swedish national satellite project, called Odin, is at present being studied. Swedish IR- and radioastronomy groups as well as aeronomy groups are involved in this study. So the interest in measurements from space is continuously increasing in the Swedish scientific community.

Within ESRO and ESA the sixties, and particularly the seventies, saw a sort of battle between magnetospheric physics/space plasma physics on one hand and astronomy on the other. For natural reasons there was an interest in the leading circles of the organization to increase the astronomy content of the scientific programme in phase with the development of the necessary technologies. In the space physics community, which was first to be served and established a strong and active program, we certainly understood the need of bringing astronomy into the program, but we were disturbed by unofficial statements that we had had our time and now it was the time for other disciplines. Although that was never an official line, the rumors were strong enough. In a period in the seventies we even experienced a virtual campaign against the space plasma physics community from leading persons at ESA Headquarters, leading to a strong reduction of representation in the advisory groups, among other things. We in the space physics field were not far from uproar. It was only the initiative by Roger Bonnet, when he became Director of Science of ESA, to appoint in 1982 (or was it 1983) a special committee, the Survey Committee, with the task of proposing a balanced long term scientific programme — Horizon 2000 — that removed these tensions between various disciplines. Horizon 2000 has become accepted by the European space science community, as well as by the more political ESA circles, as the right way for Europe to go into the future in the field of space science.

Figure 1

The Committee of Scandinavian scientists established by the Royal Swedish Academy of Sciences for the investigation of the possibilities of establishing a geophysical observatory in the Kiruna area. The meeting at which the photograph was taken was held in Abisko, 100 km north of Kiruna C, in 1946. From left in the back row are found: Dr. Malmfors, cosmic ray physicist, Stockholm; Dr. N. Ambolt, geomagnetician, Stockholm; Dr. C.J. Östman, meteorologist, Stockholm; Prof. H. Alfvén, space physicist, Stockholm; Prof. H. Köhler, meteorologist, Uppsala; Prof. R. Sievert, radio physicist, Stockholm, Chairman of the Committee; Prof. H. Norinder, electromagnetics, Uppsala; Prof. G. Ising, physicist, Stockholm; Dr. S. Åslund, geomagnetician, Stockholm; Dr. S. Werner, geophysicist, Stockholm. In the front row from left; Dr. E. Tönsberg, auroral physicist, Tromsö, Norway; Dr. E. Sucksdorff, geomagnetician, Helsinki, Finland; Dr. J. Olsen, geomagnetician, Copenhagen, Denmark; Prof. L. Harang, space physicist, Oslo, Norway.

Figure 2

Dr. Gösta Funke, Secretary General of the Swedish Natural Sciences Research Council and the Atomic Physics Research Council from the late fourties to the seventies. He initiated the establishment in 1959 of a Swedish Committee for Space Research.

Figure 3

Lamek Hulthén, professor of mathematical physics at the Royal Institute of Technology, Stockholm. He was chairman of the Swedish Committee of Space Research, established in 1959, and of the Space Committee from 1962 to 1964. He was the leading Swedish representative in the COPERS and early ESRO periods, Chairman of the Scientific and Technical Working Group of COPERS, which produced the "Blue Book", and leading member of several other groups in the preparatory period.

Figure 4

Dr. Ernst-Åke Brunberg, cosmic ray physicist from Hannes Alfvén's group at the Royal Institute of Technology in Stockholm, was the secretary of the Swedish Committee for Space Research in 1959, of the Space Committee in 1962 and of "Rymdnämnden" (the inter-research council advisory group on space research) from 1964 to 1973.

Figure 5

The "launching" of Esrange in September 1966 by the author.

Figure 6

Jan Stiernstedt, Permanent Under-Secretary of State in the Ministry of Education, where he handled space matters from the early sixties. He was Chairman of the Swedish ESRO Committee from the middle of the sixties and the main designer of the new Swedish organization for space activities, which was introduced in 1972. From that year to the end of the eighties he was the Chairman of the Swedish National Board for Space Activities and he was also Chairman of the ESA Council for a period in the seventies.

Personal recollections

Jan Stiernstedt

I was Swedish delegate to COPERS and ESRO/ESA for more that 25 years, but I was not involved in COPERS until 1963. Here I will restrict myself to talk about the COPERS/ESRO period. I will never forget my first meeting. In May 1963 I attended a session of the Committee for Higher Education and Research in the Council of Europe in Strasbourg. One day I received a phone call from the Ministry of Education and Research where I then was a young civil servant. It was the Under-Secretary of State who asked me to travel via the Hague on my way home and there attend a meeting of the Ad Hoc Finance Group of COPERS. I had myself not been involved in space affairs earlier. The ordinary Swedish delegate to the group could not come to the meeting which in Stockholm was considered rather important, as one of the main items of the agenda was the COPERS budget for the coming months, including the budget for ESRANGE, the planned sounding rocket range in Lapland in northern Sweden. I received only one instruction - to protect the ESRANGE budget and look out for the Italians who wanted to use ESRANGE money for their sounding rocket facility Salto di Quirra at Sardinia.

When I arrived at the meeting it was like entering a new world. The group was rather small, all the delegates knew each other and they had a language which to me was almost incomprehensible. It was the specific space lingo and I understood only one thing — they wanted to cut the ESRANGE budget in favour of Salto di Quirra. As I did not understand their arguments, I had nothing else to do than to take a hard grip of the edge of the table and just say no, no, no. I still remember the Italian delegate, general Cigerza, and the Director of Administration, mr Crowley, despairing over the hopeless Swedish delegate who was absolutely unsusceptible to their probably logical arguments. In the end some kind of compromise was achieved.

That was my start, and when I went home I was rather happy that — as I believed — I would never see these people again. But destiny sometimes plays odd tricks with us, and in fact I was back already in September when I attended my first meeting in the COPERS administrative working group corresponding to the AFC in ESRO/ESA. Rather soon afterwards I became Swedish government delegate to COPERS and later to ESRO/ESA up to the end of 1989. In spite of many problems, many long and difficult sessions I have never had reason to regret this — on the contrary it was always a very interesting and stimulating task. And pretty soon I also could speak the special space lingo!

The AFC and the Council meetings in the sixties were exhausting. The agendas were long, the discussions still longer. Especially those delegates who, like myself, were direct representatives of their ministries had a tendency of getting stuck in administrative or financial details for hours and hours. At times it was almost a rule that only half the AFC agenda was consumed during a meeting and the other half left to the next meeting, which was not a very efficient way to run things except in the few very rare cases where the problems mysteriously managed to solve themselves in the meantime, without intervention of delegates.

The Swedish attitude towards ESRO was complicated. In 1961 the government had declared that Sweden was not to join the common market, the EEC, mainly for reasons of neutrality. As a consequence the government wanted to show itself as European as possible and, as often as it could, show a positive attitude towards European cooperation in other more uncontroversial areas. So, when the scientists came and proposed participation in the future ESRO in combination with the building of a sounding rocket range at Kiruna in northern Sweden, the government was in principle positive. But at the same time there was a certain hesitation and suspicion towards the planned activities — the step between civil and military space activities was considered to be short. And there was also the attitude that space was something only for the superpowers and for mad scientists who wanted to go to the moon.

In his proposal to start ESRO Edoardo Amaldi had underlined the peaceful purpose of the new organization. I am convinced that if he had not written so strongly in this respect, Sweden would never have participated. When the convention was written the Swedish delegates had a standing instruction — to work for unanimity with regard to cooperation with other international organizations. Behind the proposal regarding international cooperation was a strong Swedish suspicion that ELDO rockets in a crisis could be used for military purposes. In 1960 Peter Thorneycroft, member of the British government, had visited Stockholm and tried to persuade Sweden to join ELDO, but the answer was negative and he did not even manage to convince his Swedish colleagues of the peaceful character of the proposed launcher organisation. Years later when I was chairman of the ESA Council, I was not so happy with the requirements of unanimous decisions which ESA inherited from ESRO — there was always at least one member country which made use of them in order to achieve some national goal at the expense of other member countries.

In 1962, however, the Swedish government decided to join COPERS and ESRO. A bill was presented to the Parliament and it was passed rather unnoticed and without much debate. But that was only for the moment. A discussion started within the scientific community and specifically within the government's advisory science policy committee, chaired by the Prime Minister. Within this committee there was a hot debate between two future Nobel laureates, Arne Tiselius, professor of biochemistry at Uppsala University, and Hannes Alfvén at the Royal Institute of Technology in Stockholm. The debate concerned priorities in science policy in general but the participation in CERN and ESRO was involved in the discussions. Tiselius who was an advocate for life sciences was, to some extent, supported by such physicists who were not interested in the work of the organisations but wanted new equipment for their own laboratories. Alfvén lost the debate because, in the end, the government decided to give priority to life sciences. This did of course not mean that the physicists did not receive anything, but the main priority was given to new laboratories and equipment in life sciences. And it meant that space sailed against the wind.

The consequences came in 1964 when the government had to decide whether to start a national space programme and whether finally to ratify the ESRO convention. The decision was to say **yes** to the ESRO convention but **no** to a proposal of a national space agency and a national space budget. Space science should, it was declared, compete within the framework of the budget of the science research council against all other natural sciences. Space research in the sense research with the help of satellites or sounding rockets was young and it was expensive. The competition was uneven and Sweden entered ESRO with an unbalanced space budget where almost all the money went to ESRO, but in principle no money was assigned to experiments on board ESRO satellites or sounding rockets. It was a difficult situation for the space scientists but Sweden had anyway joined ESRO, that was always something. And furthermore, the research council decided on its own to support space science by earmarking a small annual amount for a national programme. This helped the scientists to survive but no more than that.

The ratification meant that the building of ESRANGE started. In the summer of 1964 Bert Bolin, who later was to be scientific director of ESRO, persuaded a small delegation consisting of civil servants in the Ministry of Education and Research, the Ministry of Commerce and the Ministry of Finance to visit Lapland and have a look at a national sounding rocket campaign and to visit the site of ESRANGE. I suppose that Bolin's idea was to convince those who were not space enthusiasts, in particular the Under-Secretary of State in the Ministry of Education and Research who was very critical. I will never forget this trip. Late one evening we arrived at an old farm in southern Lapland from which an Arcas rocket would be launched with the payload coming down in the safety area of a military rocket range. The purpose was to study noctilucent clouds. We waited for hours and hours the whole night long and no rocket was launched, something went wrong, and the only thing that happened was that we all were bitten by angry Lapland mosquitoes. After that we all went further north to visit the future ESRANGE, hoping to see something more spectacular. The weather was bad but we all went up in a helicopter and all there was to see were some digging machines together with a few melancholy looking elks and reindeer in a sad grey Laplandic landscape. The Under-Secretary went home unconvinced. But nevertheless, two years later we all went there again when ESRANGE was inaugurated.

The period after 1964 could be characterized as the silent years in the history of Swedish space activities. The small national budget was used in a clever way and the programme was scientifically successful. The Swedish ESRO delegation had many problems to solve in connection with ESRANGE. There were endless negotiations regarding salaries at the range compared with those in the town of Kiruna and at other ESRO establishments, freedom from income tax and from customs duties etc., etc. The Laps asked for special reimbursements for theoretically killed reindeer during the launch campaigns and I had to learn everything about reindeer herding and, furthermore, to carry my knowledge forward to Mr Kaltenecker, ESRO's legal adviser. Up to now, 25 years later, no reindeer has been killed, but the reimbursement is still paid to the Laps.

In 1966 the first rocket was launched from ESRANGE. With the campaigns came new problems. The rockets tended to land everywhere except in the scientifically calculated safety areas. Northern Norway and northern Finland were favourite targets. This happened so often that we developed a method of presenting our excuses and regrets to the Norwegians and the Finns without even involving the foreign ministries concerned. One rocket disappeared entirely but was later found as a decoration in a garden on the coast of northern Norway with beautiful flowers winding around it. Another time a rocket came down exactly at the same latitude as the town hall of Kiruna. In the end I had to forbid the launching of one specific type of rocket. Hermann Bondi was then Director General of ESRO and I expected a thunder storm from his side, but he took it graciously and we managed to solve the problems with the help of a few technical improvements of this specific rocket.

As regards general ESRO policy during this period we had one standing instruction from the government: you can do what you like, or rather what the scientists like, as long as you stay within the 8-year ceiling of the budget and as long as you do not involve yourself too much with ELDO. In fact this meant that the general Swedish attitude was very loyal towards the agency and its Director General. But it was also characterized by fear of all new ideas, everything outside the framework laid down in the Convention and the Finance Protocol. I remember for instance when the first proposal of a broader cooperation between the US and Europe was presented. ESRO was visited by a NASA delegation headed by Homer Newell and Arnold Frutkin, and when I presented their proposals in my ministry, the immediate reaction was that if this means a budget increase, then we had better withdraw from ESRO when the first 8-year period will expire in 1972.

Another problem was the industrial return. Swedish industry was in fact rather ignorant with regard to space activities but offered themselves optimistically as main contractors for the first ESRO satellites. When they did not receive the contracts, some of them went to the government and said "why are we members of ESRO, wouldn't it be much cheaper to leave ESRO and start a national programme which should include Swedish-built satellites?". The government listened rather willingly — at least to the negative signals regarding ESRO — and I am still convinced that if Swedish industry had not joined the different industrial consortia which were formed in the middle of the 60ies and if the Mesh consortium which included SAAB had not received the TD-contract, then Sweden would have left the agency in 1972.

Nevertheless, the general attitude was loyal as long as everything went according to the initial plans and rules. Consequently, my ministry was not very positive, when the discussion started to change the ESRO programme towards applications, especially as the potential users showed little or no enthusiasm. But the Ministry of Industry was more interested, and in the autumn of 1971 all the ministries concerned had long and difficult discussions about the future Swedish space policy where one of the options still was to withdraw entirely from the European cooperation. I can still recall my feelings during the Council meeting just before Christmas 1971. When the meeting started in the morning I still did not know whether Sweden should join the package deal which was to be decided at this meeting. For hours I did not hear anything from Stockholm and I became more and more pessimistic. When at last the phone call came, I did not believe my ears — we were instructed to join all three applications projects, the OTS, the METEOSAT and the AEROSAT. I remember also the reactions around the table when I had the pleasure of announcing this, I still believe that the other participants had already started to calculate the costs of the deal without a Swedish participation, including the costs of closing down ESRANGE.

The 1971 package deal also meant that ESRANGE was transferred to Sweden. Someone had to be responsible for the running of the range in accordance with the rules of the ESRANGE Special Project, which we — supported by the UK delegate Jim Hosie and the French delegate Maurice Lévy — had managed to carry through. In the spring of 1972 the government decided to create a space agency with responsibility for the national programme and for the Swedish ESRO participation and also a state-owned company responsible for the technical execution of the programme, including ESRANGE.

On the night between 30 June and 1 July 1972, the midnight sun of Lapland together with members of ESRO Council and its chairman, professor Puppi, could witness the solemn handing over of ESRANGE to Sweden, together with the birth of the Swedish National Space Board and the Swedish Space Corporation.

Italy in space: the take-off of the Italian space programme and the birth of ESRIN[1]

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1. Amaldi's 'Euroluna' project: first initiatives

In a previous paper[2] I analysed the crucial role played by Edoardo Amaldi since July 1958 in conceiving, diffusing and defending in influential European scientific circles the project of realizing an "*Euroluna*" (Euromoon), i.e., a satellite for scientific research made and launched jointly by western European countries.

The kind of space organization initially conceived by Amaldi for Europe was to be in charge not only of the construction of scientific satellites, but also of the development and firing of their launchers. Amaldi's project was based on one main pillar: the decisional process and programme control of the future European space organization should be left exclusively in the hands of the scientists, both space scientists and experts in missile technology, while the military, in particular, were to be kept out. He conceived, in fact, the conquest of space as a peaceful enterprise open to all mankind and considered the military environment as an obstacle to that open co-operation and free exchange of ideas characterizing the best development of science.

One of the founding fathers of CERN, Amaldi saw it as the prototype to follow for space too as it represented a very successful example of an international organization conceived and organized "only on the basis of *scientific and technical principles* and not on the basis of *political and commercial arguments*".[3]

From the summer of 1958 to the spring of 1959 Amaldi sounded out a number of authoritative members of the European physics community, in particular some top-rank high-energy physicists and administrators, in order to convince them of the concrete feasibility of his

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1 This paper is mostly based on documents in the archive of Edoardo Amaldi. This archive, comprising several hundreds of boxes, is located at the Department of Physics, Università di Roma "La Sapienza". Work for its re-arrangement, ordering and cataloguing started a few months ago under the direction of Prof. F. Sebastiani, and is still in progress. The numbering of boxes and folders reported in the footnotes is therefore provisional.

2 M. De Maria, "Europe in space: Edoardo Amaldi and the inception of ESRO", Working Paper ESA HSR-5 (Noordwijk: ESA, March 1993).

3 E. Amaldi to J.B. Adams, 15/12/61, Amaldi Archive, box 210, folder 1; Amaldi's emphasis.

project. He succeeded in stimulating in the majority of his *confrères* a genuine interest and enthusiasm for his ideas, but also received quite a unanimous chorus of warnings against the possibility of applying *ad litteram* the CERN model to space. In particular, a certain amount of involvement with the military seemed to many unavoidable in the strategically relevant field of satellite launchers, if only because they already controlled it. [4]

As to the best way of launching the new organization, Amaldi initially thought it would be enough to start with "a small group of experts who consider the problem with sufficient enthusiasm and can make propaganda in their own countries". This group should be in charge of preparing in a few months a plan of development for the future space organization and a first definition of its scientific programmes, to be approved later by western European governments.[5] But in February 1959 in Paris he met his old friend and ally in the early battles for the foundation of CERN,[6] the French cosmic-ray physicist Pierre Auger, with whom he discussed his project at length. On that occasion he was informed by Auger that in early January of that year the *Comité des Recherches Spatiales* (CRS), chaired by Auger himself, had been created in France with the task of establishing a national research programme for space and the high atmosphere. Amaldi immediately became convinced that the foundation of national space committees in the various European countries, on the model just established in France, was "the only way of facing the problem adequately", since it would provide the correct institutional framework for a more official take-off of European cooperation in space.[7]

A few weeks after his meeting with Auger, Amaldi completed the first draft of his well-known paper, *Space Research in Europe*, explicitly "prepared to give a start to the discussions on the creation of the European Organization for Space Research", and sent ten copies each to a small group of influential colleagues in Europe, so that they could "diffuse it" in the European scientific community.[8] The two main pillars of Amaldi's proposal were still the same: a) the creation of a European Space Organization, "pooling the resources of, say, *ten European Countries*", was "*essential and urgent*" in order to fill the scientific, technological and industrial gap with the

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[4] Note 2, pp. 5-14.

[5] E. Amaldi to P. Auger, 6/2/59, Amaldi Archive, box 212, folder 6.

[6] Pestre in A. Hermann, J. Krige, U. Mersits and D. Pestre, *History of CERN. Volume I* (Amsterdam: North Holland, 1987), chapter 4.

[7] E. Amaldi to P. Auger, 5/3/59; P. Auger to E. Amaldi, 12/3/59; E. Amaldi to P. Auger, 18/3/59; E. Amaldi to B.N. Cacciapuoti, 18/3/59, Amaldi Archive, box 212, folder 6.

[8] E. Amaldi, Introduction to the Discussion on Space Research in Europe, 30 April 1959; E. Amaldi to P. Auger, C.J. Bakker, J.H. Bannier, J. Hirsh, A. Hocker, J. Willems, F. Giordani, L. Broglio, 22/5/59, Amaldi Archive, *ibid.*

Superpowers; b) this Organization "should have no other purpose than research and should, therefore, be *independent of any kind of military organization and free from any Official Secrets Act*" (Amaldi's emphasis). As the procedure to be followed for creating such an organization, Amaldi, after the example of the French CRS, envisaged the setting up of "national commissions to examine the problems of space research". These commissions, composed of experts in launcher and satellite technology on the one hand, and of space scientists on the other, should then discuss their proposals at an international conference, "so as to work out a detailed programme for submission to the governments of the countries concerned".[9]

During the second half of 1959 and the first months of 1960 Amaldi was very active in organizing the start of his project. Using his report as a tool he kept mobilizing his extended network of personal and scientific relations, in particular the CERN 'lobby', in order to foster the foundation of national space committees both in Italy and in other western European countries.[10]

Amaldi's project was discussed in public for the first time in two informal meetings of a small number of European scientists, which took place respectively in Nice, in the occasion of the general assembly of COSPAR (9-16 January 1960), and at Auger's home in Paris on 29 February 1960. The real novelty of these meetings consisted in the enthusiasm with which the British sponsored the project. In particular, Sir Harrie Massey, chairman of the British National Committee for Space Research (NCSR) set up by the Royal Society in December 1958, was "very enthusiastic about a European collaboration" and played a major role in defining the possible programmes of the future organization. Moreover, he offered the services of the Royal Society "to take the initiative" in calling the next meeting, to be held in London on 29 April 1960, "for the constitution of a more formal body". The choice of the Royal Society as the institutional 'cradle' of the project represented a dramatic change in the external 'visibility' and credibility of the project itself, finally offering a prestigious framework for the take-off of European co-operation in space.[11]

Soon after this first Royal Society meeting, Amaldi stepped aside from the European space scenario. In a previous paper[12] I tried to give a tentative answer for his quite abrupt withdrawal

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9 *Ibid.*, pp. 3-7. See also J. Krige, "The prehistory of ESRO 1959/60", Working Paper ESA HSR-1 (Noordwijk: ESA, July 1992), pp. 2-10.

10 J. Willems to E. Amaldi, 22/6/59; J.H. Bannier to E. Amaldi, 24/7/59; E. Amaldi to J.H. Bannier, 30/7/59; E. Amaldi to P. Auger, 15/12/59; P. Scherrer to E. Amaldi, 21/3/60; E. Amaldi to P. Scherrer, 26/3/60, Amaldi Archive, box 212, folder 6.

11 P. Auger to E. Amaldi, 26/1/60, Amaldi Archive, box 270, folder "Spazio Europa — Corrispondenza e Relazioni, 1960-1962 "; P. Auger to E. Amaldi, 16/2/60, *ibid.*; W.V.D. Hodge to E. Amaldi, 30/3/60, Amaldi Archive, box 248, folder "Spazio Europa — Corrispondenza fino al 1961". See also note 2, pp. 21-24.

12 Note 2, pp. 28-29.

from the forefront of the new European space organization, in whose promotion he had initially played so determinant a role: as an active scientist Amaldi had never been involved in space science, his research interests being focused mainly on high-energy and nuclear physics, his old interests in cosmic-ray physics apart. Therefore, he elaborated his "*Euroluna*" project mainly on the grounds of political and moral considerations; as he himself anticipated as early as December 1958, he wanted to limit his initiative simply to "the launching of the idea", i.e., to mobilize a number of European experts who could push forward his project with competence and "enthusiasm", and eventually "a few years later, if the idea had borne its fruits, to participate in the collection of data obtainable with this kind of activity".[13] But he did not remain idle at home: in fact, as we shall see in the next section, he played a central role in mobilizing cosmic-ray physicists and astrophysicists from a number of Italian universities and in starting the first national space programme.

2. *Amaldi and the development of space science in Italy*

Soon after conceiving his "*Euroluna*" idea at the end of July 1958 Amaldi, in search of support and advice, wrote to his colleague Luigi Broglio, one of the few Italian experts in rockets, who was at that time director of the Institute of Aeronautical Engineering at the University of Rome "La Sapienza" and a colonel of the Italian Air Force. As Amaldi later put it, Broglio initially expressed "a substantial agreement on the theoretical formulation of the problem but also a noteworthy skepticism regarding the feasibility of a concrete project". In fact, Broglio considered Amaldi's initiative "worthy of attention both for its important scientific aspects and for its high moral effects"; however, he expressed all his doubts about the difficulties that could arise in the launching of an international scientific organization in a field where "the military interest is highly pre-eminent, particularly in the case of an extra-military management".[14] But a few months later, after receiving the first positive replies from a number of European colleagues, Amaldi succeeded in convincing Broglio of the concrete "feasibility" of his project, so that in February 1959 he could inform Auger that Broglio was "ready" to represent Italy, in his quality of "expert", in the future European space organization.

During his first round of contacts and enquiries Amaldi sounded out the reaction of Italian industrial circles and, in particular, discussed his project also with a number of top-level FIAT managers who expressed their strong interest "to collaborate in this project from an industrial point of view".[15]

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13 E. Amaldi to L. Crocco, 16/12/58, Amaldi Archive, box 212, folder 6.

14 *Ibid.*; L. Broglio to E. Amaldi, 28/8/58, Amaldi Archive, box 248, folder "Corrispondenza con Broglio, 1958-1961".

15 E. Amaldi to P. Auger, 6/2/59, note 5. One of the FIAT managers with whom Amaldi discussed his project was the engineer Giuseppe Gabrielli, director of FIAT Aviation, a former student of Theodore

As we have already seen, in February 1959 Amaldi had been convinced by Auger of the necessity of creating a national space research committee in each western European country, modelled on the French CRS, as the best start of his project. After sending his report *Space Research in Europe* to Francesco Giordani, President of the Italian *Consiglio Nazionale delle Ricerche* (CNR), he therefore at the end of June 1959 took the initiative with Broglio of meeting Giordani in order to discuss with him the constitution of such a national committee inside the CNR. Early in September 1959 the President of the CNR officially set up the *Commissione per le Ricerche Spaziali* (CRS) whose task it would be to assess "the national capabilities" in the space field and to "achieve a collaboration among western European countries in order to realize a common programme of work". Broglio was its president and Amaldi one of its seven members, all university professors who, in line with Amaldi's initial idea, could guarantee that the scientists would have complete control of the field from its very inception. The French committee, on the contrary, had a much more official status: besides some influential scientists like Auger, it was also composed of representatives from the various ministries interested in space, such as higher education, aeronautics, foreign affairs, national defence, telecommunications and finances.[16]

Ten days later the members of the Italian CRS met the scientific director of NASA, Hugh Dryden, with whom they discussed for the first time possible forms of co-operation.[17] Three months later the CRS, in its second session held on 14 December 1959, following the proposal of Amaldi and Broglio, unanimously approved the project of creating a European organization for space research. The next day Amaldi wrote to Auger, informing him of the Italian news and asking him to organize a meeting of European scientists on the occasion of the COSPAR Conference to be

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von Karman at Aachen. His name was suggested to Amaldi by his friend Luigi Crocco, Gabrielli's brother-in-law and professor of "Aerospace Propulsion" at Princeton University. Amaldi started to discuss his "Euroluna" project with Crocco from the very beginning and used him as *trait d'union* with von Karman. E. Amaldi to L. Crocco, 16/12/58; L. Crocco to E. Amaldi, 2/1/59; E. Amaldi to L. Crocco, 9/1/59; L. Crocco to E. Amaldi, 27/1/59, Amaldi Archive, box 212, folder 6.

16 F. Giordani to E. Amaldi, 5/9/59; E. Amaldi to F. Giordani, 8/9/59, Amaldi Archive, box 248, folder "Ricerche spaziali — Corrispondenza con Broglio 1958-1961". The other members of CRS, besides Broglio and Amaldi, were M. Boella, professor of "Electrical Communications", Polytechnic of Turin; N. Carrara, professor of "Electromagnetic Waves", University of Florence; C. Casci, professor of "Motors for Aeromobiles", Polytechnic of Milan; G. Puppi, professor of "Physics", University of Bologna. The composition of the French CRS and a brief description of the first French national programme is in the document presented at the Royal Society meeting in April 1960, NCSP/79(60), Amaldi Archive, box 248, folder "Spazio Europa — Corrispondenza fino al 1961"; see also P. Auger to E. Amaldi, 12/3/59, note 7.

17 L. Broglio to E. Amaldi, 17/9/59, Amaldi Archive, box 248, folder "Corrispondenza con Broglio 1958-1961".

held in Nice in January 1960. Amaldi, who could not participate in the meeting, informed Auger that "the Italian point of view would be well exposed by Broglio". [18]

Amaldi was perfectly aware of the necessity of developing a sound national space programme in order to guarantee an active presence of Italian scientists in the future European space organization. In order to define this programme he therefore started to mobilize from autumn 1959 onwards, under the aegis of the CRS, cosmic-ray physicists and astrophysicists from the universities of Rome, Turin, Bologna, Milan, Florence and Parma. A few months later, in February 1960, Amaldi informed his colleagues attending the meeting at Auger's house of the initiatives taken by the CRS to promote the first national space programme. It included the launching, by the end of 1960, of a few sounding rockets up to 200-250 km for the study of primary cosmic rays both with G-M counters and with emulsions. For the financial year 1 July 1959-30 June 1960 the CNR had allocated 300 million lire (i.e., about $190.000 at 1960 exchange rates) to the CRS for the construction of scientific instrumentation to be placed in these rockets. Moreover, the CRS had obtained a further 300 million lire from the Italian Air Forces for the purchase of sounding rockets and equipment for tracking and telemetering, as well as some 200 million lire for technical services and free use of the facilities in the military launching base of Salto di Quirra in Sardinia.[19]

As early as May 1960, the Italian cosmic-ray physicists, under the guidance of Amaldi, had already drawn up an articulated programme of experiments, to be accomplished in the fiscal year 1 July 1960-30 June 1961, on cosmic rays and energetic radiations at very high altitude. These experiments were to be jointly realized by various universities and involved the launching of both sounding balloons and rockets. In February 1961 the cosmic-ray groups of the universities of Bologna and Rome carried out the first tests with sounding balloons from a base near Bologna; then, in April 1961, the Rome group started a programme of balloon launchings from a base near Frascati, and in June 1961 from Selva di Fasano (Bari), in collaboration with the cosmic-ray group of the University of Bristol. [20]

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18 E. Amaldi to P. Auger, 15/12/59, Amaldi Archive, box 212, folder 6. Amaldi could not be present at the Nice meeting because during the same period he had to attend a CERN meeting and a meeting of EURATOM in Bruxelles.

19 Amaldi's report is a typewritten document entitled "Meeting at prof. Auger's house the 29th of February 1960", Amaldi Archive, box 248, folder "Spazio Europa — Corrispondenza fino al 1961".

20 E. Amaldi, "Preliminary Research Program of Italian Universities on Energetic Radiations at very high Altitudes", 15/3/60, *ibid.* See also "Programma sulle radiazioni e sui campi magnetici in alta quota (1960-1961), 7/5/60, *ibid.*; see also "Promemoria al prof. M. Giorgi", 6/7/61, Amaldi Archive, box 301, folder "Commissione Ricerche Spaziali, 1960-61-62". The planned experiments regarded: a) study of the neutron component of cosmic rays with emulsions (Universities of Rome, Bologna and Parma); b) measurement of neutron intensity after large solar flares as well as in undisturbed conditions (Rome and Bologna); c) measurement of the total ionizing radiation of cosmic rays as a function of height (Bologna); d) measurement of charge spectrum of heavy primary cosmic rays (Rome and Parma); e)

The quick start of the first national space programme was due to a very efficient 'division of labour' between Amaldi and Broglio: while Amaldi mobilized the Italian cosmic-ray physicists to realize a fully fledged space research programme, Broglio and his engineers started to develop an ambitious national civil programme on rockets. As early as December 1959 Broglio had announced the launching of some five to six bi-stage sounding rockets between 1 July 1960 and 30 June 1961 as a joint project of the CNR and the *Comitato Razzi e Missili*, chaired by Broglio himself, of the Italian Ministry of Defence. In April 1960, after contacts and discussions between Broglio and NASA officials, Italy signed an agreement with NASA on a sounding rocket programme aimed at studying high altitude winds using sodium 'clouds'. In April 1960 Broglio's engineers achieved, from the base of Salto di Quirra, the first (secret) successful launching of a *Nike* sounding rocket, made entirely by the Italian corporation BPD on US licence; on 9 July 1960 Broglio's team launched its second *Nike*.[21]

Amaldi's concept of the relation between science and the military may be considered ambiguous. Although he considered it in Europe, as we have seen, "an essential point" that the future European organization "should be only civil and with a strictly scientific character, without military links or problems of secrecy in general",[22] in Italy he did not have any hesitation to accept funds and the use of facilities from the military. In order to understand this apparent contradiction, one should bear in mind that Amaldi's views on scientific internationalism and on the relation between science and the military were not naively pacifist, but rather characterized by pragmatism. He supported the foundation of a space organization free from any military influence since their "control" would have made the realization of its scientific programmes "more difficult and complicated". At the same time he replied "to those who worry about European defence [...] that once the techniques of satellite launching were known in Europe, correspondingly the military structure of each country would automatically result strengthened".[23] According to Amaldi, the real point at stake was the "control" of space activities, while in Italy the CRS — owing to its composition (all its members were university professors) and nature (the CRS was an organ of CNR, i.e., a strictly civil national research agency) — would guarantee that the Italian space programme would be kept firmly in the scientists' hands both with regard to the selection of scientific experiments and their realization. Moreover, the president of CRS Broglio — who was the *trait d'union* between the CRS and the Italian Air Force — shared Amaldi's idea that space

measurements of gamma and X-rays in coincidence with higher solar activity (Rome and Bologna); f) measurement of X-ray and electron intensity (Rome); g) measurements of the intensity of X-rays in the region 1-100 Å and of the radiation intensity between 1100 Å and 1345 Å (Florence and Bologna).

21 *Il Corriere della Sera*, 18/12/59; *Aviation Week and Space Technology* (26/8/63), 76-81.

22 E. Amaldi to P. Auger, 6/2/59, note 5.

23 E. Amaldi to L. Crocco, 16/12/58; E. Amaldi to L. Crocco, 9/1/59, note 15.

research activities should be under the exclusive control of the scientific community and represented a guarantee that the use of military money and facilities would not imply any form of control on their part. Therefore, Amaldi did not hesitate, in the autumn of 1961, to ask the Italian minister of defence Giulio Andreotti "to co-operate and support the launching of sounding balloons with scientific instruments [...] to be used in a near future aboard Italian-made rockets and satellites."[24]

In his letter to Andreotti Amaldi was hinting at the *San Marco* project, on which Broglio and his team began to work from May 1961 onwards, when preliminary designs of the first Italian satellite were started. This project was the core of the triennial national space programme for 1961-1963, presented by G. Polvani and by Broglio at a ministerial meeting held on 31 August 1961 and approved by the Italian government with an allocation of 4.5 billion lire. The programme included: a) the realization of a mobile near-equatorial platform to be anchored offshore the coasts of Somalia; b) the launching, within two years, of an Italian satellite with a useful payload of 100 kg by a vector purchased abroad.[25]

At the meeting of CRS on 18 September 1961 Broglio informed Amaldi of the recent developments of the Italian space programme and asked him to set up as soon as possible with the Italian space physicists interested in space research, a programme of experiments to be put on board the Italian satellite. This urgency was due to the fact that Broglio was leaving for the United States on 30 September to meet NASA officials in order to define in detail the collaboration between Italy and the US in the *San Marco* project. Amaldi immediately wrote to his cosmic-ray colleagues and convened a meeting which duly took place at the University of Rome on 25 September 1961. Four cosmic-ray groups from the universities of Bologna, Milan, Rome and Turin, a total of 28 physicists, attended this meeting and defined a number of ambitious experiments to be put aboard "small satellites".[26] By October 1961, Broglio reached an informal

[24] E. Amaldi to G. Andreotti, 25/10/61, Amaldi Archive, box 301, folder "Commissione Ricerche Spaziali — 1960/61/62". In his reply Andreotti authorized the use of the base of Salto di Quirra and assured Amaldi that the Italian armed forces would offer "the most efficient collaboration"; G. Andreotti to E. Amaldi, 16/11/61, *ibid.*

[25] *Il Corriere della Sera*, 1/9/61; more details on the inception of the *San Marco* project are in two interviews with L. Broglio (22/6/92) and C. Buongiorno (23/6/92) respectively, made by dr. Lorenza Sebesta in the framework of the ESA History Project.

[26] E. Amaldi to G.P. Puppi, G. Occhialini, C. Castagnoli and A.M. Conforto, 18/9/61; "Verbale della riunione tenuta a Roma il 25 Settembre 1961 presso l'Istituto di Fisica dell'Università", Amaldi Archive, box 210, folder 1; see also "Notes for Prof. Broglio on a preliminary draft of an Italian program on space research", Sept. 1961, Amaldi Archive, box 248. The list of experiments defined in that meeting included: 1) measurements of flux, energy spectra and sign ratio of primary cosmic ray electrons; 2) study of heavy primary cosmic rays; 3) study of mass spectrum of low energy particles in the Van Allen belts; 4) study of energy spectrum of electrons in the Van Allen belts and ionospheric layers; 5) study of cosmic ray neutrons; 6) study of solar gamma rays; 7) study of magnetic fields; *ibid.*

agreement with NASA for the launching within two years of an Italian satellite by a US *Scout* booster from an Italian near-equatorial platform. Thus for Italy the procurement of satellite launchers for space research was resolved by NASA's offer of co-operation; this explains, at least in part, the lasting opposition of Amaldi and Broglio against the "*Blue Streak* project". [27]

3. Amaldi and the birth of ESRIN[28]

Another major problem that Amaldi had to face from the very beginning in his effort of organizing space research in Italy was the structural weakness of this sector. This was also due to the fact that since 1959, with the construction of the 1 GeV synchrotron in Frascati, many Italian cosmic-ray physicists, including Amaldi himself, had shifted their research interests towards high-energy physics. To find a remedy for this situation Amaldi started to invite a number of top-level space scientists from abroad to come to Italy in order to provide quick training in space science to young Italian physicists. The first to accept Amaldi's invitation was his old friend, the cosmic-ray physicist Bruno Rossi, then at MIT, Cambridge Massachusetts. Rossi came to Rome in October 1960 to give a course of lectures on space physics, and on that occasion the cosmic-ray physicists of the Rome group could "discuss future programmes with him at length". Other space scientists invited to Italy by Amaldi on behalf of CRS were S. Hayakawa, from Nagoya University, Japan, who came to Rome in March 1961 and V. Ferraro, from Queen Mary College, University of London, who lectured in Rome in April 1961 on the theory of magnetic storms and the physics of interplanetary space. Amaldi also invited R. Lüst, then at MIT, to give a course of lectures on "elements of space physics" at the *Scuola di Perfezionamento in Fisica* of Rome University in spring 1962. Finally, in June 1962 a summer school on space physics was organized on Amaldi's initiative at Varenna (near Como) by the *Società Italiana di Fisica*. The school was directed by Bruno Rossi and Van Allen, Hayakawa and Lüst were among the lecturers.[29]

In Varenna Bruno Rossi had detailed discussions with the members of five Italian groups engaged in space research from the universities of Bari, Bologna, Milan, Rome and Turin on "the

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[27] This argument has been thoroughly analyzed by M. De Maria (note 2), pp. 32-34. The official agreement on the *San Marco* project was signed on 5 September 1962 by the US Vice-President Lyndon B. Johnson and by the Italian minister of foreign affairs Pietro Piccioni, "San Marco satellite to probe air density", *Aviation Week and Space Technology*, (26/8/63), 76-81. See also "Progetto San Marco — Relazione sul Poligono Equatoriale San Marco", 30/9/63, Amaldi Archive, box 110.

[28] For the birth of ESRIN see also J. Krige, "Europe into Space: The Auger Years (1959-1967)", Working Paper ESA HSR-8 (Noordwijk: ESA, May 1993).

[29] E. Amaldi to V. Ferraro, 31/10/60; V. Ferraro to E. Amaldi, 18/11/60; E. Amaldi to V. Ferraro, 23/11/60; N. Mancini, F. Mariani to B. Rossi, 2/12/60; E. Amaldi to S. Hayakawa, 18/1/61; S. Hayakawa to E. Amaldi, 17/2/61; E. Amaldi to S. Hayakawa, 7/3/61; S. Hayakawa to E. Amaldi, 11/3/61; R. Lüst to E. Amaldi, 1/12/61; E. Amaldi to R. Lüst, 6/12/61, box 301, folder "Commissione Ricerche Spaziali — 1960-61-62".

necessity of undertaking a national project in which all the members of these groups would collaborate under a single, unified direction". This led to the proposal, advanced by these groups to the CRS in October 1962, of a "national satellite for the measurement of interplanetary plasma". The project would be developed with the assistance of NASA and its associated laboratories in order to guarantee "the integration of this experiment in the wider US programme". The US laboratory indicated by the Italian groups was Rossi's laboratory at MIT, "both for its activity in the field and for the close personal and scientific relations already existing between Prof. Rossi's group and members of the Italian groups". In fact, two Italian physicists — G. Occhialini in Milan with C. Dilworth, and Bonetti in Bari — had already participated in the MIT plasma experiment. Moreover, because of the "relative inexperience" of the Italian groups in satellite instrumentation, the type of equipment envisaged for the Italian satellite was a development of that successfully employed in *Explorer 10* for plasma studies. Also in this case Amaldi sponsored the national satellite project with Broglio and the president of CNR Polvani. [30]

However, space research in Italy remained still very weak and uncompetitive at a European level, as testified by Occhialini, who informed his colleagues at a meeting of the *Commissione Raggi Cosmici* of the *Istituto Nazionale di Fisica Nucleare* held in Milan on 20 October 1962, that out of a total of 70 projects of experiments submitted to ESRO only two came from Italian groups, one from Rome and one from Turin. [31]

Amaldi, perfectly aware that the backwardness of Italian space research could not be overcome simply by inviting experts from abroad to lecture on the more recent developments of the field, therefore took the initiative of sending a number of young Italian physicists to be trained in US laboratories, in particular at MIT and at the NASA's Goddard Space Flight Center.[32]

But this was not the only initiative he took: more important, Amaldi was also the 'grey eminence' behind the Italian proposal of setting up in Italy an ESRO establishment for advanced space research that would focus on the study of advanced programmes to be carried out in future ESRO satellites. Needless to say, in Amaldi's view the foundation of a European laboratory of this kind would have represented a unique occasion for the development of space research in Italy.

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30 "Proposal for a National Satellite for the Measurement of Interplanetary Plasma", Oct. 1962; see also C. Castagnoli to E. Amaldi and G. P. Puppi, 2/10/62; E. Amaldi to G. Polvani, 19/10/62; E. Amaldi to L. Broglio, 19/10/62, Amaldi Archive, box 269.

31 "Riunione della Commissione Raggi Cosmici dell'INFN tenuta all'Istituto di Fisica dell'Università di Milano il 20/10/1962", Amaldi Archive, box 301, folder "Commissione Ricerche Spaziali — 1960-61-62".

32 E. Amaldi to J.P. Heppner, 26/9/62, E. Amaldi to A.W. Frutkin, 6/10/62. Amaldi Archive, box 270, folder "Spazio Europa — Corrispondenze e relazioni, 1960-1962". Heppner was the Head of the Magnetic Fields Section at the Goddard Space Flight Center; Frutkin was the Director of NASA's Office of International Programs.

The Italian delegation to COPERS advanced this proposal for the first time at the special meetings of the Bureau attended by the heads of delegations, which took place in Paris on 25-27 March and 4 April1962 to deal with the problem of locating the ESRO establishments. Broglio illustrated the Italian proposal in detail at the 6th meeting of the COPERS Scientific and Technical Working Group (STWG) held in Rome on 9 May 1962. Amaldi (who had not taken part in any other meeting after the Royal Society meeting held in London in April 1960) attended this meeting as "adviser" to the Italian delegation.[33]

According to the project presented by the Italian scientists, the envisaged laboratory (the future ESRIN, initially called ESLAR, the European Space Laboratory for Advanced Research) should be independent of ESTEC, in order not to interfere with the programmes already approved or being implemented there. Its institutional tasks would be: a) feasibility studies on possible future proposals; b) survey of special requirements for experiments in future space-borne systems; c) basic theoretical and experimental research on the various advanced systems to be used. Its staff requirements amounted to a total of 100 people (with 40 scientists and 30 technicians). The total cost of ESLAR for eight years of operation was estimated at 102.75 million NF, a figure considered "compatible" with the fixed ceiling of 1500 million NF established for ESRO's first eight years of existence. [34]

Consequently, at the plenary session of COPERS held in Rome on 10-11 May 1962, it was decided that the Conference of Plenipotentiaries, to be held in Paris on 14-15 June, should recommend, in addition to the other ESRO establishments mentioned in the Convention, that "[...] a laboratory be established in Italy. The size and the scope of the laboratory shall be decided by the Council of the Organization". Moreover, the STWG was asked to report on the elements of the project still to be defined.[35]

One month later the Bureau of COPERS, at its meeting held in Paris on 13 June 1962, asked the Chairman of the STWG, L. Hulthén (Sweden) to take immediate steps for initiating the work requested by COPERS in its May plenary session. An *ad hoc* group was set up for studying the question of "the size and the scope" of ESLAR. Both Amaldi and Broglio were members of this group.[36]

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33 "Comments on the scope and the size of the Laboratory for Advanced Space Research (ESLAR) to be located in Italy (prepared by the Italian Delegation)", COPERS/89 (rev. 1), 9/5/62.

34 *Ibid.*

35 "European Preparatory Commission for Space Research -Draft resolutions to be submitted to the Conference of Plenipotentiaries", COPERS/AWG/34 (rev. 6), 17/5/62.

36 "Special meeting of an 'ad hoc' group for studying the proposal of the laboratory (ESLAR) to be installed in Italy", COPERS/GTST/43 (rev. 1), 10/10/62. The other members of the "ad hoc" group were: Bartels (chairman), Auger, Hulthén, Lüst, de Jager, Dorleac, Lines and Vandenkerckhove. See

An important re-orientation of the "scope" of ESLAR was proposed by Sir Harrie Massey during the 7th meeting of STWG held in Paris on 29-30 October 1962. Massey suggested that, besides the study of future advanced programmes as initially suggested by the Italian delegation, other tasks more directly connected to present ESRO programmes should be carried out by ESLAR in order to ensure "the best scientific efficiency" of planned satellite experiments. In particular, he suggested that ESLAR should also take up laboratory and theoretical work on plasma flow in the interplanetary magnetic field, on disturbances created by satellites in rarefied ionized gases, and, more generally, on reaction rates and processes occurring in upper atmosphere and stars. [37]

The "ad hoc" group and the Italian delegation took Massey's suggestions fully into account and accordingly modified its proposals, by also including in ESRIN's institutional tasks "theoretical and experimental work for the interpretation of the experiments carried in *present* satellites and space probes from the point of view of the interaction between the space-borne systems and the surrounding fields and media".[38]

The gestation of ESRIN, however, revealed itself much more fraught of obstacles than was initially thought, and in May 1963 Amaldi was again obliged to take action in defence of the project. In fact, at the 9th meeting of COPERS held in Paris on 3 May 1963, the Italian delegation had formally raised the problem of "a solicit and rapid restarting of the work for the institution of ESRIN". After "a very strong resistance of the Secretariat and all the other delegations except the Belgian one", COPERS finally accepted the Italian request and a new "working group" was established with the task of defining in detail "the scientific and financiary dimensions of ESRIN". This new working group, formed by five "scientists" and two financial experts had to submit its conclusions to the STWG before the 10th meeting of COPERS, to be held by the end of June 1963, for the final approval of ESRO's eight-year programme. Both Amaldi and Broglio were members of this working group and in this capacity they could give the final push to the birth of ESRIN.[39]

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also P. Auger to E. Amaldi, 12/10/62, Amaldi Archive, box 270, folder "European Preparatory Commission".

37 "Remarks of Professor Massey concerning the new laboratory to be established in Italy", COPERS/GTST/55, 30/10/62.

38 "Revised edition of COPERS/89 (rev. 1) taking into account document COPERS/GTST/55" (undated).

39 E. Ortona to E. Amaldi, 6/5/63; E. Amaldi to E. Ortona, 11/5/63, Amaldi Archive, box 231, folder 2. Ambassador Ortona, Director General of economic affairs, Ministry of Foreign Affairs, was the Head of the Italian delegation to COPERS. The other scientists of this "working group" were Golay, Coulomb and van de Hulst, *ibid.*

Seizing opportunities: some comments on the Dutch national space science programme of the sixties and seventies

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1. Setting

1.1 Introduction

As a boy I noticed that my brother, who later became a professional artist, always had more exciting adventures in his holidays than I had. I also noticed that his stories often sounded more heroic when told for the fifth time. This was my first remembered confrontation with the problem of writing recent history. Later I have noticed from concrete examples in the early history of radio astronomy that even taped and documented eye-witness reports made 10 or 30 years later, may contain inconsistencies in important details. Many kings had one or more historians, whose task it was to destill from the actual facts a canonized, embellished, and sometimes fully distorted history. The formal history in Saddam Hussein's version is that he won the Gulf war.

Since flattery is pernicious and can in the long run endanger mental health, wise kings had it counteracted by the remarks of the court jester. I like to think that I feel more at ease in the role of the jester but it is by no means certain that I manage to avoid the role of the canonizer, making up a good story from selected historical facts. I have in fact done so consciously several times in editing for publication the confused discussion sessions of a scientific symposium. But what about the written record as an objective source? I do not believe much in such records. They tell part of the story, but contain nothing of the bull sessions, the phone calls, the meetings halfway the corridor, where the opinions and decisions were engendered, if not finalized. Even between the formal sessions and the well-written minutes thereof, there always were (in the time I was active) the Mannos and the Dattners doing an excellent editing job.

I suppose this suffices as an explanation of my approach and as disclaimer for any errors I shall make. I plan to speak mostly from "an honest memory", partly supported by facts I looked up in my own archive and only very little by facts from the formal record. This method is also dictated by limited preparation time, for, unlike some of my colleagues emeriti, I wish to keep the time spent on writing history *far* below one half of my active working time. I presume that this meeting is called with the idea that reminiscing may not only be pleasant but also to some extent helpful. If you think so, please remember that any pretence that the lessons learned then can be applied verbatim to the present situation, would be misplaced. Too much has changed: in science, in economics and in politics.

1.2 The relations of science and government

One aspect of this relation, the pressure of industry brought to bear on the governments, in particular on the departments of economic or industrial affairs, will automatically pop up in my account below. But let me first say something about time-scales. Although rumour has it that the scientists are the impatient ones, always pressing for new data and new missions, I feel that the actual scales are different. Industrialists are the most impatient; they have to worry about next month's jobs. Politicians come next; they worry about next year's election. The scientists, although they may have drifted away from the old ideal of working for the next generation, still are accustomed to look 10 years ahead.

The following true anecdote may illustrate this point. During one of the early consultations about the second "national Dutch satellite", with representatives from half a dozen ministries present, the chairman, representing the ministry of science and education, asks: "What will the astronomers give up if they get IRAS?" The answer is: "Sir, we shall give up plans we do not even know yet, for the universities are requested to submit 5-year plans, and this satellite will fly well beyond that time."

Within ESA and its precursors, most member-countries symbolized the link between science and government by appointing into the SPC two delegates, a representative of the relevant ministry, and a representative of science, sitting side by side. How did — with only a few well-remembered exceptions — those two persons manage to come to one vote? The widely different ways in which the member countries answered this question shows that Europe still has a many-coloured map.

My feeling is that we in Holland were fairly well off. The situation was always complex and sometimes confusing but I never felt it approaching a Kafka-like, impenetrable mesh. For one thing: (a) Officials of the 8 or so ministries involved in various aspects of the space program, *did* talk together, and met regularly in the interdepartmental committee on space affairs (ICR). (b) We as space scientists had a fair access to the talking or bickering going on in this body, because we had been invited to sit in on the ICR as an observer. The ICR's task included very important sessions for preparing position papers for a ministerial conference, or finalizing the draft of an important policy paper to be issued by one or several ministers. Our formal influence was nil. But I think I could still trace some of the last-minute "suggested corrections" which I delivered on bicycle the evening before the meeting finalizing the "Ruimtevaartnota" of 1982.

A far less spectacular task of the ICR was the preparation for the ordinary ESA Council, SPC, and further committee meetings. These preparatory sessions, with 2 to 6 people in attendance, started by a routine check like: "Do you have rev.2 or rev.3 of the agenda?" They were conducted in good spirit and often brief but never ineffective.

This section would in my feeling be incomplete without a reflection on some personalities which impressed me during those years. The list would be long but let me mention just three from my own country.

Van der Maas, the senior professor of aeronautics (aircraft-building, as it read literally), was also heading the Netherlands Institute for Aerospace Development (NIVR), which performed a kind of buffer function between the ministry of economic affairs and the Netherlands aircraft industry with their naturally rather different dynamisms. He fought for a full Netherlands participation in the European space effort from the beginning and taught me that the basic motive that would impress government was, that our country should not be permitted to slide gradually into the status of an industrially underdeveloped country. When we both attended the founding meeting of COPERS at Meyrin (1960), we had to put up a real struggle to keep the wish 'to develop technology' in the preamble of the resolution. Presumably other delegations already at that time feared competition with ELDO or had other petty reasons for wishing to limit the task of the new organization to basic science.

Bannier was not himself a pusher for the space effort. But his long years in guiding CERN and in heading the Netherlands Organization for Pure Research had made him a grand master in the field of big science, both nationally and internationally and a valuable source of advice. In the early years of COPERS (1963) he was asked to head a committee to advise on possible shifts in the distribution of tasks among the member countries. They recommended — aiming at a better internal equilibrium — that a substantial number of tasks be removed from ESTEC at Noordwijk to ESOC at Darmstadt. This advice was followed, a wise decision indeed.

Piekaar was another of those persons with a very erudite background. He had studied colonial law. He had been a government official in Atjeh (now in Indonesia) for many years and people still visited him occasionally as a world expert on the Atjeh language. When I knew him, he headed the division of higher education and research in the Ministry. He chose his advisors carefully and when the space programme required an urgent decision, he always found a slot in his busy agenda. But what impressed me most was his willingness to listen and to make an effort to understand. So many government officials with a training in a different field take the attitude: 'Science is not my province; I won't even try to understand'.

1.3 A multi-faceted programme

Before addressing my subject, the Dutch national space science program, I must draw your attention to the fact that 'national' may mean two things, which are not in conflict but should nevertheless be distinguished for good understanding.

First: national (opposite: provincial or local) means working together as scientists and engineers regardless of university or station. As a formal tradition this was mostly absent in Dutch universities in spite of the small geographic distances. Informally, this attitude was so ingrained that it was self-evident. A celebrated example is that the professor of theoretical physics Lorentz spent years of calculation on the future enclosure dam of the Zuiderzee and, incidentally, established the need for founding a good technological university. Another example, which I have witnessed myself, is how the telecommunication agency (PTT), Philips Research, and two universities joined hands in the post-war years and gave radio astronomy a headstart, compared to the separate initiatives that flourished in other countries. As a final example I recall with pleasure how Oort (a full-fledged astronomer from Leiden) and Burgers (a full-fledged aerodynamicist from Delft), both hiding from the occupying force, had long conversations on the uncharted problems of cosmical aerodynamics. This led many years later to an exciting international symposium (Paris, 1949) and, again much later, very indirectly gave me an introduction to talk during a reception somewhere to Dryden, who had headed NACA, the precursor to NASA. This may have been the first international conversation on the Dutch national satellite ANS.

The second meaning of 'national' has as its opposite something like 'unofficial, done by private initiative'. It means that a program or goal has been specifically adopted by government, i.e., the cabinet of ministers, as a policy aim. During the period I report on there were two such 'national' satellites, ANS and IRAS. However, they certainly did not appear out of the blue. Space experiments on balloons and rockets and by instruments placed on American and European satellites had preceded them. These experiments were 'national' enough in the first sense. For the scientists who prepared these experiments or took part in them thoroughly interacted in the space science committee (GROC) of the Academy of Sciences, which laid out the programme and through which all money from government was channelled. But these early experiments did not carry the label 'national' in the second sense. I present in Table 1 the full list of such experiments until 1984.

2. *Chronicle*

A narrow definition of history is: a chronological list of events with some explanatory comments. If we take that narrow definition, the history in my talk starts only here. I have separated it into four rather unequal episodes and present a selected list of events in some separate tables.

2.1 The pre-ANS period

This period hardly falls within my subject, but a few words about it may be helpful. For, no matter how strong industry would have pushed for the ANS satellite, it would not have materialized without healthy and eager science groups to give this plan a scientific authenticity. These groups had had an early start, within the second year after Sputnik I, when, along with everyone else, we were asking ourselves: what can we do in space?

One conscious decision we then took was not to worry about an elaborate institutional framework. It became a committee of the Academy of Sciences (GROC, March 1960-December 1983) and remained so until nearly 24 years later it was replaced by the Space Research Organization Netherlands (SRON) under the national organization for scientific research (ZWO, soon changed into NWO). Another conscious decision was not to worry about how difficult or impossible a project seemed, but to aim right away at measurements that would be scientifically important and would have a natural link with existing expertise or scientific interest in the Netherlands. We collected a dozen initial suggestions, of which about five passed the first scrutiny. But let me comment only on the astronomical plans.

A typical difference in policy between the Utrecht and Leiden groups developed. One might seek the explanation in tradition or in personalities but I feel that it was largely dictated by the subjects chosen. Utrecht (De Jager) chose the initial aim to observe X-rays from the Sun, with the explicit intention to see where the path would lead from there, eventually to X-rays from the stars. This meant a step-by step advance with a lot of in-house building and testing, and led to the one fully equipped Laboratory for Space Research in the Netherlands. Leiden (Wapstra, Tanaka, Van de Hulst) chose for the 'impossibly difficult' aim of measuring cosmic-ray electrons, simply because those same electrons were held responsible for the non-thermal radio emission from the galaxy, a subject of prime interest in the research program of the Leiden astronomers. An experiment with this aim received a place on NASA's OGO-5. When we approached the national industry (Philips) with our conceptual design, the answer was: we can make this but it will take learning time, so we cannot deliver within the time frame requested by NASA. So the contract went to Ball Brothers.

Let me also briefly mention Groningen (Borgman, Van Duinen). They started a few years later, preparing thoroughly for far UV photometry of stars, but, finally, out of desperation that no flight opportunities materialized, shifted their attention to the infrared. This happened in 1967. The UV preparation was finally honoured by a relatively small experiment on ANS, and the move to the infrared came to full fruition only with the IRAS satellite in 1983.

Recounting this history provides the background for two further remarks. First, when NASA had to decide whether the Netherlands, represented by NIVR, was a trustworthy partner to embark on the bilateral program ANS, it evidently had to examine the credentials both at the industrial and at the astronomical side. My distinct impression from conversations I had around that time is, that both the fine record of work at the Utrecht laboratory and the flawless performance of the Leiden Group in OGO-5 (although Leiden was not involved in ANS) helped NASA to come to a positive decision. The second remark is that, of course, the black-white distinction between a group specialized in in-house construction, and one with out-house contracts was soon softened up. Utrecht did a lot of work, notably on the UV spectroscopy experiment S59, which was ESRO's most complicated experiment to date, with the assistance of the semi-industrial organisation TNO-TPD. The result was of excellent quality but the fairly open-ended contract sometimes caused raised eyebrows

It is clear from Table 1, that the scientists in our country did not care where they got a launch, provided they got one. If you want to go skiing, you wish to go up. You may inquire about the quality and capacity of the lift and the price of a ticket, but which agency runs the lift is not a point of consideration. This attitude was shared by the Dutch government officials until the advent of the 'national satellites' ANS and IRAS. Elsewhere I have remarked that the proper word for this attitude is 'outright opportunism' but without the nasty tone usually associated with that word. After all, since the early sixties NASA graciously showered the world with 'announcements of opportunity', and from that time on, opportunism has become a practical art.

To complete this story, although stepping farther away from my main subject, let me add that *none* of the other potential flight opportunities was neglected. With our Japanese friends we thoroughly explored common interests but did not find ground for a common proposal. Signing (in 1982) a sufficiently formal document between the space science agencies of the USSR Akademia Nauk and the Koninklijke Nederlandse Akademie van Wetenschappen had some aspects of a comic opera. But it worked to secure a place for an Utrecht experiment COMIS flown in 1987 on the module KVANT attached to the Space Station MIR and, incidentally, opened similar possibilities for ESA (because ESTEC is in The Netherlands). Finally, prolonged talks with Italy led to the Dutch participation in SAX, with an MOU signed in 1990 and a launch now expected in 1994.

2.2 The first national satellite ANS

The drive for a national satellite came from industry. For good reasons, but unlike the great example CERN, ESRO insisted on placing industrial contracts under the principle of fair geographical distribution. This often led to strange ad-hoc consortia, in which by and large our industries have been fairly successful in their bids. The early hassle about the argument that the concrete poured for the ESTEC buildings should not count one-to-one against the electronics provided by others, was soon forgotten. But one sore point remained: the limited size of our country (and hence of its financial contribution) prohibited our industry from ever obtaining the leading role in a major contract. Therefore, our industry argued, they could never prove their real competence. This was the chief argument that won over the Dutch government to go ahead with ANS.

Table 2 contains my selection of the principal events. I fully realize that other insiders might make a different selection. Few comments are needed. The warning signs of 1966 were visible at both sides of the ocean. ESA cancelled its most prestigious plan, the Large Astronomical Satellite (LAS), and NASA, in selecting the experiments for OSO-H, appeared to drop the principle of selecting only on basis of merit and gave preference to USA proposals. This year was what in fairy tales is described as 'the day when the king saw the bottom of his treasure chest'. The diminishing chances elsewhere provided a good argument for creating chances nationally.

The cost (at the Netherlands side) of ANS had been largely borne by the ministry of economic (read industrial) affairs. There was some logic, therefore, to close down the relatively expensive operations (engaging a team of a dozen persons full time) once the industrial point had been proven. But the storm of public protest against throwing away the opportunity to do unique science prevailed, and led to a last-minute extension in order to observe a number of objects that could only be reached at that time. A proper review should also contain a record and appraisal of the doctor's theses and other publications that have come out of the ANS satellite, and of their scientific impact. But I have not done my homework and therefore skip that point.

2.3 The ARTISS interlude

This interlude, which lasted only a year, appears important enough for a separate section. The facts are sober and can be told without a chronological table.

The Dutch cabinet of ministers, hearing the plea from industry, was favourably inclined towards another national satellite project. However, their clear decision was that a satellite for agricultural purposes would have the highest priority. After all, industry would find such a satellite an equally worthy challenge, the astronomers had had their share, and aiding third-world countries was high on the government's policy list. A pre-study of such a satellite had appeared by the name ARTISS. A clever feature in its design concept was that each participant country (tropical or subtropical) would be handed a software key that would limit the unscrambling of the remote sensing images to those of their own territory. Neighbourly distrust or conflict would thus be circumvented.

The Academy of Sciences was asked to arrange a feasibility study and, therefore, created an ad-hoc committee. At my suggestion they sought a true agricultural expert as chairman. They found professor Buringh of the Agricultural University at Wageningen willing. He was not too interested in technical details but at the right moments would bring to bear his own experience from visits and consultations in many tropical countries. When we were engrossed in details of angular, spectral, or time resolution, he explained to us how the widely cited test results from the USA had very limited relevance: "In many of those countries", I remember him saying, "agriculture is mostly a one-family affair, with a mixed crop on a small area, and if they can manage, they will try to grow it under trees in order to get some shade."

Another problem that became clear is that "handing a key" could technically be done, but would still leave the operating agency (which was envisaged to be in the Netherlands) in the position of a 'concierge' that might in time be viewed with equal suspicion as an unfriendly neighbour country.

Anyhow, the committee's final advice was that this plan was neither diplomatically nor technically ripe for implementation. This cleared the way for IRAS. Several years later a related plan TERS (Tropical Earth Resources Satellite) was discussed at length in bilateral talks between Indonesia and the Netherlands but I am not familiar with the details. In any case, it failed to raise sufficient interest to embark on its construction.

2.4 A brief history of IRAS

The first claim of industry that a follow-up of the ANS satellite was needed, preceded even the launch of ANS. It was clear that the astronomers kept their eyes wide open to explore, nationally and internationally, what scientific aim should be chosen if a positive decision to construct another astronomical satellite would be taken. The development of gigantic Helium Dewars at Ball Brothers caught Van Duinen's fancy and led to the first design concept of IRAS in 1973, soon followed by a more complete design study. When the cabinet had decided that a second astronomical satellite was OK, IRAS was already well in the lead as the most likely candidate. GROC then took the precaution of a round of open suggestions, after which the choice of IRAS became firm.

The industrial preparation, in the form of a Phase-B study paid by NIVR, proceeded with great speed. Since at the heart of the design were American detector technology and an American Dewar, the study involved many (and sometimes not very easy) consultations. But NASA was favourably inclined from the start. The volumes of the industrial 'voorstudie' appeared at the end of 1975, in time for a positive decision by the Cabinet. But I well remember my disappointment when I looked inside and still found so many pages essentially blank, chiefly stating that further study must be made.

At the astronomical side, a pioneering set of rocket flights by AFCRL (later called AFGL) had already mapped part of the sky at the same infrared frequencies. It became clear, first by rumour, then by open studies, that a substantial fraction of the sources thus discovered were spurious, because no effective distinction against glitches had been built into the project. The prudent answer to this experience were the painstaking internal checks by repetition at several time intervals built into the design of the IRAS satellite and into the entire IRAS survey mission.

It was obvious that the social problem of keeping everybody on the user side happy, was not a simple one either. Our USA colleagues were not at all pleased with their shotgun marriage to the Dutch, and later also the English, astronomers. Moreover, in negotiating the many drafts of the MOU, spelling out a casuistic tree of what course should be taken if something went wrong, proved quite impossible. The wise solution was to place a bigger than usual power of decision in the hands of the two co-chairmen of the Joint IRAS Scientific Working Group (JISWG), hoping that they would act in wisdom and that in critical times they would be able to stand the pressure. These two persons were Neugebauer and Van Duinen. The need to have yet a third one from Britain was deftly avoided by keeping the project formally bilateral, with a sub-MOU spelling out the division of labour between Holland and England.

Inside Holland we spotted another problem. We felt that the project, once in full swing, would turn out far too demanding on the astronomers of one university (Groningen) only. GROC therefore created the new function of 'science coordinator' for IRAS. We found Habing willing to adopt this role, without any formal power based in the MOU, with the main task to round up and channel the interests of astronomers (professors and students) at the other universities, Leiden, Utrecht and Amsterdam. This move brought another potential source of conflict to the surface, which required great diplomacy in its handling. Our USA colleagues (in my estimate by a combination of tradition, economic necessity, and personality) exercised a caution bordering on paranoia when it came to letting others profit from the data for which they had done all the work. Our tradition was exactly the opposite: "the more brains, the better". As history went, the formal authority of the Dutch science coordinator never had to be tested. For, in October 1981, when the satellite had been built and shipped to the USA for integration, Van Duinen accepted a different position, whereby his formal ties with IRAS were severed. Habing became the Dutch co-chairman of JISWG, who with his USA counterpart Neugebauer now had to argue out whatever dispute might arise.

Was IRAS ready for launch? The flight-readiness review of december 1982 decided yes, and why quibble since the results came out so well? Well, permit me to add as a last personal memory that I returned from that meeting as a more cynical man. The 200 viewgraphs flashed before us in one and a half day were a parody to the professed management policy that every detail of the performance specifications had been assessed and found well by an independent panel of experts. I knew from earlier meetings that parts of the detector array were at fault, but that it would be far too costly and too risky to take months letting the satellite warm up again and take it apart. But even the leakage of Helium from the Dewar, the vital factor in the expected lifetime of the IRAS mission, was far from understood. The overriding reason for NASA yet to go ahead with the launch was not this panel's review but NASA's desire not to let the expensive industrial and launch teams wait forever.

I end my story here, knowing full well that the next chapter of gathering, exploiting, analysing and interpreting the data, is equally interesting on the technical, anecdotal, and diplomatic side, as the pre-launch history. I hope that someone else will find the courage to write that part of the story before the canonizers have distorted it too strongly. One thing is beyond doubt: the success of IRAS triggered ESA's decision (1983) to go ahead with ISO and we all hope that this even more ambitious infrared satellite will be a success.

3. Lesson

3.1 Conclusions

Normally, such a study should be concluded by a summary of what we have learned: what errors to avoid, what wise decisions to copy. But I really do not feel like making such an attempt, for the situation was so unique that it is hard to imagine another moment of decision which has more than a faint resemblance with those I described. Two points I wish to stress toward my younger colleagues:

1. Be alert and listen carefully, trying to understand and watching not only the scientific horizon but also the economic and political forces at play.
2. Do not leave the formulation of advice, or the writing of policy papers, exclusively to supposedly wiser persons. In spite of all appearance you may have something important to add to their wisdom.

Bibliography

I have not made a systematic bibliography, nor an archival study for this lecture. Nevertheless, I list some references (mostly in Dutch), which I was happy to consult and which also might be valuable to a future serious researcher.

Jaarverslagen (Annual reports) Nederlandse Commissie voor Geofysica en Ruimteonderzoek (GROC) 1961-1983; the same, Stichting Ruimteonderzoek Nederland (SRON) 1984-1991.

J.H.de Koomen, "Overzicht van het IRAS projekt", Ruimtevaart **32**, 103-119, 1983.

H.C.van de Hulst, "Nederland in de Ruimte", Zenit **11**, 292-304, 1984.

Koninklijke Nederlandse Akademie van Wetenschappen, "Verslag van de bijzondere bijeenkomst der Afdeling Natuurkunde op Maandag 19 December 1988 tijdens welke de Gilles Holst-Medaille werd uitgereikt aan Dr. R.J. van Duinen en Prof.dr. H.J. Habing.

J.H.de Koomen, "Een terugblik op de ontwikkeling van de Nederlandse industriële ruimtevaart", Ruimtevaart **40**, 9-31, 1991.

Table 1 Dutch astronomical satellite experiments until 1984

The first 3 columns refer to a particular satellite; the last two columns refer to one or several experiments in that satellite.

Satellite	Agency	Launch	Purpose of Experiment	Group*
OGO-5	NASA	1968	electrons in C.R.	L
ESRO-2	ESRO	1968	solar soft X-rays	U
TD-1	ESRO	1972	solar hard X-rays UV stellar spectra	U U
ESRO-4	ESRO	1972	solar flare protons	U
ANS	'National'	1974	UV stellar photometry Galactic X-ray sources	G U
COS-B	ESA	1975	Gamma-ray sources	L et al.
ISEE-3	NASA	1978	Solar flare particles	U
Einstein	NASA	1978	X-ray grating spectra	U
SMM	NASA	1980	Imaging solar flares in X-rays	U et al.
IRAS	'National'	1983	IR source survey IR spectr.classification	G et al. G
EXOSAT	ESA	1983	Soft X-ray sources	U, L, et al.

* Group contributing to instrument design, test, or construction: U=Utrecht, L=Leiden, G=Groningen; et al. refers to a variety of foreign partners *in the same experiment*. The relative importance of the partners in a collaboration *cannot* be read from this table. Analysis of results often involved further groups.

Table 2 ANS in brief

1965	Exploratory conversations
1966	First signs that money for space science is not infinite. Coordinated proposals of GROC to Ministry of Education and of NIVR to Ministry of Economic Affairs
1968	Elaborate studies and dito reports
Dec. 1968	Cabinet gives the green light
1969	Formal start of project
1970	MOU signed
1971	ANS project Scientist starts his work
Aug. 1974	Launch by Scout rocket
Sep. 1975	Discovery of X-ray bursters by ANS
Dec. 1975	Ministry stops support for ANS operation
Mar. 1976	Operations are resumed
25 Apr. 1976	End of astronomical observations. Several technical tests by NASA still follow

Table 3 IRAS in brief

Oct.1971	Industry writes to Ministry of Economic Affairs that a successor to ANS is needed
1973	Design concept
1974	Van Duinen, invited by Dutch industry makes first design study for IRAS
1975	Cabinet decides that an astronomical satellite is OK GROC confirms preference for IRAS. NIVR places study contract with Dutch industry consortium ICIRAS and initiates formal talks with NASA
1976	JISWG constituted; Dutch user community broadened
end 1976	Cabinet gives the green light
1977	MOU signed between NASA and NIVR, containing the usual provision of no exchange of funds. A second MOU is signed between NIVR and SERC with the same provision
Oct. 1981	Van Duinen accepts a different job. Habing takes over as co-chairman of JISWG
Dec. 1982	Flight readiness review
Jan. 1983	Launch
Dec. 1983	End of useful life; Helium has evaporated

NOTE: The story does not end here. Reaping the scientific harvest continues until today.

Crisis and achievement: ESRO 1967-1971

Sir Hermann Bondi

1. This article summarises my recollections of my period as Director General of ESRO from October 1967 to the end of February 1971. Although my scientific interests were largely in astronomical subjects, my researches were purely theoretical. My involvement in space matters began in earnest when in late 1964 the U.K. Ministry of Defence asked me, as an academic, to chair a committee to look at British defence interests in space. From what I had learned in that task it was a long intellectual step to take responsibility for ESRO, but I had at least acquired some understanding that major projects can flourish only with a powerful unified centralised management.

The inheritance I took on at ESRO was basically excellent but the system had not by then had a successful satellite to its credit, the only completed one having been lost in a U.S. launch failure. Yet the achievements of our predecessors (the top management was almost wholly changed, in the division of responsibilities as well as in personalities) was astounding. They had, from nothing, recruited staff, put up buildings and facilities, negotiated agreements with NASA, run a successful sounding rocket programme, started three small satellites times for completion in 1967/8, and begun work on a pair of medium sized ones (TD1/2). Yet there were also tensions which I inherited: Spain felt, with some justice, that she had not benefitted sufficiently from her membership, Italy was severely critical of the contract award for TD1/2, and overshadowing it all was the sound suspicion of the continental member states that the U.K., which had engineered the ELDO agreement, had lost interest in this enterprise. My own view on ELDO was formed well before I came to ESRO and was that the treaty setting it up violated every principle of project management by doing without a strong central authority, and by dividing the financial burdens between countries both by task and in fixed ratios, a perfect machine for cost escalation. it was a remarkable achievement of its staff that, in spite of these crippling handicaps, the project came so close to success.

It must be appreciated that, some 30 years ago, the international management of high tech projects was only just beginning to be understood. The scientists (notably Professors Auger and Amaldi) brought their CERN experience to bear, and so the ESRO treaty was basically sound in the powers it gave to the organisation. These were further improved, just as I began to work there, as a result of the Bannier report. No such experiences informed the ESRO negotiators who moreover had to handle politically far more sensitive issues.

2. The first task the new management team had was to ensure that the second flight model of the mislaunched ESRO 2 satellite could be made launchworthy in good time. This was not easy and involved setting up a special management coordination led by an outstanding engineer. Perhaps this was a sledgehammer to crack a nut, but ESRO could not hope to live with a failure here. An international organisation with ten masters can have only one source of strength, and that is proven engineering excellence. Throughout my period of service, I was fortified in every difficult situation by the knowledge that, thanks to our super staff, I could rely on their engineering judgement, and our member countries had to accept this.

A very unsatisfactory feature of ESRO as I found it was the appalling underspending. At that time, of the funds planned to be spent in external contracts, less than one fifth was in fact paid out. The reason was that managers at every level put into their estimates a cushion against any chance of overspending. These cushions added up to an absurd total. Moreover, industry was often overoptimistic about its delivery times. All this not only made nonsense of the budget estimates, not only fell foul of several member countries' unwillingness to allow unspent balance to be carried over, but above all showed that the organisation had fallen far behind in carrying out its tasks. This was quite unacceptable and we resolved to improve this situation drastically. Indeed, three years later the underspent was well below 10%.

During 1968 we had delivered to NASA for launch and successfully operated in orbit not only ESRO 2, but also the somewhat delayed ESRO 1 and, later in the year, the HEOS satellite which had been completed within time and cost estimates, a remarkable achievement. Against this background of technical success, even the most awkward political problems became soluble. First to be dealt with was the danger of a Spanish withdrawal. To ameliorate Spanish concerns, we therefore devised a reasonable compromise between 'juste retour' and free competition, secondly we persuaded the other member states to agree to allow some easement in Spanish payments, and thirdly ensured through appropriate management attention that contracts within the then rather limited capacity of Spanish industry went there.

Next came the problem of working within our Convention. This specified that the member states had to agree unanimously every three years a three-year level of resources for ESRO. In early 1967 they had in fact failed to agree, and we lived on a one-year budget. Moreover, with the Convention not obeyed, every Council decision had to be unanimous. This situation was due to the shadow of the ELDO crisis, and the concern that the U.K. was quite prepared to rely on U.S. good will for launchings, while others wanted Europe to be independent. (This was linked to the applications satellite question to be discussed below.) Yet it was essential for ESRO that its Convention should be honoured by its member states.

Originally the programme of ESRO had contained as its most ambitious and important project the Large Astronomical Satellite (LAS). To get clear about whether this was indeed feasible, I asked one of our best young engineers to brief me on it. At the end of his presentation I asked him: "So you think LAS may be beyond ESRO's strength?" He responded by expressing doubts whether ESRO had the capacity to manage the medium sized project TD1/2. This made my alarm bells ring. Indeed this satellite pair TD1/2 was far larger, more complex, and more expensive than anything ESRO had attempted before. Naturally we asked this engineer to study and report on the state of the project. In the midst of these labours he suddenly died of a heart attack, having kept secret his knowledge of his heart condition. This tragedy was the prelude to our worst crisis.

In fact, in the work on this project we had gone awry. The contracts had been awarded, and a considerable sum already spent, on the basis of cost estimate that turned out to be wildly wrong. We had to rescue a situation made more difficult by Italy's understandable hostility to the consortium that did not include an Italian firm and had won the tender action to some extent on the basis of these unreliable cost estimates. After many vicissitudes a solution was arrived at. The satellite TD2 was cancelled (a most painful decision for several of our scientific customers), TD1 would be completed to a new cost estimate (and a modified design so it bore the name TD1A) and financed by the member states other than Italy, but when in orbit would have its costs of operation met by all. But rescuing ESRO's own part of the effort was by no means all that had to be done. The experiments in all our satellites were managed by groups working in the universities and laboratories of the member states, and nationally funded. It emerged that the chief experiment in TD1A was the responsibility of a university group that had not appreciated at all the difficulty of managing this task. This was then put, not without crises, on to a sound footing through excellent cooperation with the national authorities concerned and with industry.

With these problems out of the way and our technical successes with the smaller satellites, we entered the November 1968 Ministerial Space Conference in good heart. Indeed we got approval for all we wanted, including unanimous agreement to our three-year level of resources. However, a few weeks later the U.K. split from the others over ELDO, causing great resentment, which clouded the scene for a long time. Yet we managed our own scientific affairs in good order, working on TD1A, completing other satellites, and generally progressing well.

3. About that time the question arose whether ESRO should have its mandate widened to include applications satellites. On the one hand this was obviously right: such satellites were by their nature regional rather than national, and it would be absurd for Europe to have two international management organisations dealing with the same technology. On the other hand, ESRO's scientific character had shielded it to some extent from political hassles, and the scientists rightly regarded it as their creation to serve their purposes. Again, the bigger national agencies had their own aspirations in the field. Thus a lot of discussion and negotiation was required, but eventually the main principles were all settled in a reasonable manner, with ESRO given the

authority to handle applications satellites. We then started preliminary studies on four types of such satellites: communications, meteorological, aeronautical and earth observations. The first two lines went forward well, though of course no satellite was completed till well after my period of service. Earth observations went forward rather more slowly but, very much to my regret, the capacity of satellites to control air traffic over the oceans was never exploited.

Several problems arose at this time with our telecommunications ambitions. First, the operation of such satellites (after early experimental ones) clearly had to be a matter for the PTT authorities who showed little appetite for any space hardware other perhaps than U.S. satellites under Intelsat. This took some sorting out. Then the question of competition with American commercial interests arose. Would our good friends in NASA be able to launch for us such competitive satellites? We got confuse signals on this in awkward negotiations, with the result that most of our member countries now saw more strongly than before the need for a European launcher. But of course this did not cure the technical and managerial problems of ELDO.

Applications satellites, however, strengthened the fissiparous tendencies of Europe. The TD1 solution whetted the appetite of some member states for an *à la carte* system, with members participating or not in projects as they wishes. This was quite contrary to the original idea of a scientific organisation, that scientists could and should agree on an order of projects irrespective of the country they came from. An obligatory programme has been maintained in science, but in my view everybody would have benefitted from a wider employment of this principle.

4. There were plenty of difficulties throughout my time originating in general pressures on space budgets, in the continuing rumbles of the ELDO crisis, in wide differences of view about participation in the U.S. Spacelab programme, etc. But through all these crises we managed to preserve a joint European management organisation of proven engineering competence. That we succeeded in this is owed in no small way to the loyalty and cohesion of the team of Directors it was my good fortune to lead: Marcel Depasse, Jean-Albert Dinkespiler, the late Professor Wernher Kleen, and Umberto Montalenti.

The ESRO scientific programme during the transition period, 1971-1975

Maurice Lévy

1. Introduction

In the spirit of the majority of presentations at this meeting, I shall rely mainly on personal recollections of the period covered here rather than on documents.

As was emphasized earlier, the creation of ESRO, strongly influenced by people like Pierre Auger, Edoardo Amaldi, Sir Harrie Massey etc., was very much patterned on the structure of CERN, in which the same persons had been involved in the fifties. The emphasis was therefore essentially on a *scientific* organization. During the early ESRO period (1962-70), there was very little interest for space applications and for industrial development.

In France, during the same period, CNES had established itself. It was a strong organization, where a balance between science and launchers development prevailed. Scientific laboratories were not part of CNES itself, but rather *associated institutions*, supported by other organizations (CNRS, CEA, etc.) and funded by CNES only in relation with specifically approved projects. This meant that French space policy was not much influenced by scientific preoccupations. After 1966, the importance of space telecommunications was recognized in France, mostly because the United States, through Intelsat, managed by COMSAT, a U.S. company, were taking strong international positions in this area. This led to the "Symphonie" programme, a German-French enterprise.

2. Science vs. applications in the sixties

In a way, the great chance of the ESRO scientific programme was the total lack of interests at that time, of European PTTs for satellite communications. Space projects were considered expensive and in most countries, the PTTs preferred to have them funded by scientific or industrial ministerial departments. In France, the PTTs had very much the same attitude, but CNES saw in satellite telecommunications the possibility of big launchers development. Also, the French (Gaullist) government did not see kindly the U.S. domination in this area. In the German Federal Republic, and also in Belgium, there was an interest in launcher development and in satellite telecommunications, mostly for technological reasons (to improve the industrial know-how in this area). This led to the so-called three countries programme (*programme à trois*) on which CNES, at the end of the sixties, thought that it would base its European activities. The outcome was the denunciation by France (followed by Denmark) of the ESRO convention at the end of 1970, the withdrawal becoming effective on 31 December 1971, if no agreement could be reached by then on a redesigned ESRO programme, particularly in favour of applications.

3. *The 1971 negotiations*

At the beginning of 1971, I had come back from a three years tour of duty in Washington and was asked by the French Minister for Scientific and Industrial Development to create a small unit, closely related to his cabinet, to follow the large civilian programmes (space, oceanography, nuclear energy). At the same time, I became French representative on the "Committee of alternates" and head of the French delegation at ESRO and ELDO. I was then in the position to participate actively in the informal negotiations which had started, under the chairmanship of G. Puppi, to try to find a solution to the ESRO crisis.

I felt that it was unsound to conceive a national or bilateral application programme. On the other hand, a European application programme was really needed: this implied a reduction of the scientific programme, because the total budget could not go beyond limits.

Here, I shall not go into the details of the negotiations but simply state that during the second part of 1971 a "package deal" was formulated which eventually obtained the approval of all ESRO members. This included

- a limitation of the scientific programme to 27 MAU;
- a global envelope for space applications of 60 MAU;
- a limitation of general expenses to 10 MAU.

However, and this is essential, the scientific programme was *mandatory*, whereas the application envelope was optional (each country could decide in which proportion it would participate). The consequence was that the scientific programme, although reduced, was saved by the "package deal" of 1971. Over the years, it was again allowed to grow prudently and covered most of the areas of space science.

4. *The scientific programme after 1971*

In 1972-73, I became President of the ESRO Council and Chairman of the Scientific Programme Committee. This gave me an insight into the space scientific programme which I found very different from other areas of scientific research:

a) Space programmes were very long: it often took ten to fifteen years between the conception of a particular experiment and the final exploitation of its results. Consequently, it was vital for a given laboratory to have one of its experiments accepted as part of a particular payload: it really made the difference between life and death for many scientific teams.

b) The consequence was that negotiations among scientists interested in different ares (astronomy, plasma physics, solar science, planetology, etc.) were very long and very difficult. It was, in the end, necessary to decide on three different satellites simultaneously, which meant that each project would become even longer ...

c) The areas where the scientists were a minority became neglected. This was the case, for example, of planetology where only scientists from few countries (including France) had a real interest. This in turn led France into bilateral cooperation with the USSR and, to a lesser extent, to the United States.

d) On the other hand, this situation had some positive aspects. Since money was scarce and competition among scientific teams fierce, it became necessary to propose very original missions (which was not always the case in bilateral cooperation). The quality of the ESRO-ESA programme therefore grew considerably.

e) Finally, the evolution of space science brought it near earth-based programmes (space telescopes, for example). There was therefore a banalization of space science, which became much more an integral part of the sciences of the universe. This, in my opinion, is a very good evolution.

5. Conclusion

In retrospect, one can say that the 1971 reform, which was at the time considered with hostility and bitterness by the scientists, actually had a very beneficial effect on the scientific programme. Since the overall European programme, largely because of Ariane and the application projects, grew considerably, the scientific part of that programme, because of its compulsory character, was also able to grow appreciably.

One can ask oneself, considering the present difficulties of all science research projects and the high cost of each space mission, what would have happened if ESRO had remained a small purely scientific organization. It probably would have suffered from many more crises over the last twenty years.

From ESRO to ESA: some personal recollections

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On September 1, 1984, I took up my duties at ESA and for the next six years I was very actively involved again in space matters. I had not had much direct contact with space activities for the previous 12 years during which time I had been President of the Max-Planck-Gesellschaft.

When I arrived in Paris in the fall of 1984, I looked back to the 12 year period from 1960 to 1972 during which I helped to establish and shape European cooperation in the field of space science. In COPERS, from 1961 to 1964, the way for ESRO to come into being was prepared, and in 1964 ESRO could formally start its life as an organization with its main task that of promoting space science. But already in 1968, the discussions had started whether concentration on space science was sufficient for European space activities to exploit the opportunities that space offers. The discussions culminated in the fourth European Space Conference held by the Ministers in November 1970 in Brussels. This was the most disastrous session of European Space Ministers I have ever experienced. During the whole year of 1971, many people, I was among them, were very busy trying to find a solution. Giuseppe Puppi was the chairman of the Council at that time and under his guidance, the first "package deal" was adopted in order to accommodate the application program within ESRO's activities. I shall never forget the two council meetings, one on July 14, 1971 at ESTEC and the other on December 20, 1971 in Paris. A compromise had been worked out, and the level of resources for the period of 1972 to 1974 was decided. During this period, I was chairman of the scientific and technical committee, and in this capacity, a member of the so-called Bureau, which met quite often in 1971 to prepare the necessary decisions of the Council.

As chairman of the STC, I was a spokesman for the scientific community and had to defend science against the growing pressure of the application program. My main counterpart was Maurice Lévy from CNES. Although he wanted to support science, he was under very strict instructions from his Government. Also on the French side was Michel Bignier, at that time Director General of CNES. Lévy succeeded Giuseppe Puppi as chairman of the Council for the final transition period of ESRO to ESA in 1973 to 1975.

I regarded the "package deal" with only 27 Million Accounting Units left for the scientific program as a defeat for science, although one could claim it was a success, as the original sum proposed for science was much less and the French delegation had even threatened not to participate in the science program at all. But it was not only this very low financial ceiling for the scientific program, in addition to this the sounding rocket program and the scientific activities at ESRIN in Frascati had to be abolished in ESRO's undertaking as a consequence of the "package deal".

At the time, I took up duty in ESA in 1984 and often during the following six years in Paris, I tried to compare ESA with ESRO in many aspects, in order to understand and to learn how to handle problems in a better way. A few of these comparisons I will describe here.

Of course, for both ESRO and ESA, the most important resource is the people working in the organization and their devotion. There is one striking difference between ESRO and ESA. When we started ESRO, most of the people who joined ESRO were very young and had very little experience, if any at all, in space techniques. Now, at ESA, they were twenty years older and had gained tremendous experience. Many of them I had known in the past, working in our Headquarters in Paris, or at ESTEC in Noordwijk or at ESOC in Darmstadt. Some of them I had met for the first time while sitting on numerous interview boards from 1962 to 1964 when they had applied for posts at ESRO. With their knowledge and experience, they formed the backbone of ESA. Without them I could not have carried out my task during these six years.

When I arrived in Paris, I was very pleased that I was received by Michel Bignier, at that time Director of Space Transportation Systems, and by Georges van Reeth, Director of Administration, who knew everything about ESRO and ESA as he had started at ESTEC in 1964 as a contracts officer. It was an additional piece of good fortune that Roger Bonnet was also there as Director of the Science Program. We had known each other since the beginning of the sixties, launching sounding rockets together from Hammaguir in the Sahara Desert.

However, the organization of ESA was rather different from the days at ESRO. At the beginning of ESRO, there were three Directors under the Director General: the Scientific Director, the Technical Director and the Administrative Director. The Scientific Director was responsible for the preparation of, and planning of the scientific program and for the Data Center, ESDAC, in Darmstadt. The Technical Director was responsible for the execution of the agreed projects, for ESTEC in Noordwijk, for ESRANGE in Kiruna and for ESTRACK, the tracking stations. However, as it turned out, this organization scheme was not really in balance, and the member states were becoming more and more dissatisfied with it. As a consequence, in 1968 there was a change in the structure of the organization as recommended by the Bannier Group. At Headquarters, the Director General had next to him only the Director of Program and Planning and, of course, the Director of Administration. The Director of ESTEC became responsible for all technical matters, including the execution of the projects. The operation centre was shifted to Darmstadt and called ESOC, responsible for the data acquisition and distribution and also for ESRANGE in Kiruna.

This organization proved to work quite well and was sufficient until the application program was included. My predecessor, Erik Quistgaard, abandoned the Directorate of Programs and Planning and put the whole responsibility for each program from the planning to the execution under a Director, while the new Technical Director was responsible only for running ESTEC, including the Technology Program and the testing facilities. As a consequence of the Ministerial Conference in Rome, I imposed only minor changes to the overall structure by bringing in some new people in order to have a good mixture between older and younger co-workers. For me, it was crucial that the Director of Science, the Director of Earth Observation, the Director of Telecommunication, the Director of Space Station and Platforms and the Director of Space Transportation Systems had the responsibility for a project from the planning to the end. For the overall planning in the Agency, the Director General was supported by the coordination and monitoring office.

This brings me to the comparison of programs between ESRO and ESA. I arrived at ESA at a very fortunate time: I was able to start with the first ideas of a new overall plan because the projects decided at the beginning of the seventies had been executed very successfully, namely Ariane, Meteosat, the communications satellites OTS, MARECS and ECS, SpaceLab and last but not least, a number of scientific projects including IUE, COS-B and EXOSAT. New compared to the time of ESRO were the three independent Organizations EUTELSAT, EUMETSAT and ARIANESPACE, being responsible for the operations of the communications satellites, the meteorological satellites, and the Ariane launchers, respectively.

Most advanced in the planning was the scientific program called "Horizon 2000". It was most admirable how Roger Bonnet was able to gain the full support and harmony of the scientific community for this concept embracing all traditional space science disciplines. It was also quite a contrast to the practice which had developed in ESRO. Although one had prepared a long-term scientific plan for ESRO at the beginning of the sixties, the projects themselves had been decided one at a time, while Horizon 2000 was built on four essential cornerstones which had been agreed by the scientific community. On this basis, the scientific community as well as ESTEC and industry could prepare the necessary developments well in advance.

There was only one problem for getting Horizon 2000 started and this was its financing. The famous 27 Million Accounting Units - which were only brought up-to-date according to the inflation rate - still represented the financial basis for the science program. The fight for an increase started at the Ministerial Conference in Rome in January of 1985 and continued through the Meeting in The Hague in 1987 up to the end of my term of office in 1990. I regard it as my greatest success while working at ESA that I was able to achieve, together with Roger Bonnet, an annual 5% real increase for the scientific program over a period of ten years. Parts of the scientific community had not even taken note of this or even appreciated this remarkable gain for space science in a period when all other scientific disciplines in Europe did not achieve any real increase in their funding. At present, some of them forget that this was only possible due to the expansion of the other programs, particularly the manned program.

When ESRO was started, there was almost an emotional dispute whether or not ESRO should be permitted to carry out its own scientific activities. At ESA, this was no longer a controversial question and I was very happy about this. ESLAB, as it was called originally, later it became the Space Science Department (SSD) at ESTEC, had proved its usefulness for the scientific community, giving very efficient support to the satellite projects. But this was not at all. The scientists there had also contributed very valuable scientific results which were recognized in Europe and elsewhere. The only thing I have regretted, was that there was not a greater rotation between those working at the laboratory in Noordwijk and the institutes of the member states.

It is quite interesting to compare the situation between ESRO and ESA as far as the projects themselves are concerned and their impact on European industry. This is related to the fact that the science projects are a part of the mandatory program while all other programs with the exception of technology are embedded in the optional program.

For every scientific satellite project, there was in ESRO as well as in ESA competition from industry to obtain the contracts. In the sixties, three consortia existed which competed with one another. These consortia not longer exist, particularly due to the restructuring of industries in several member states, partially also due to the way in which the satellite projects of the optional program were initiated. Here, as in practically all cases, the selection of the major contractor was predetermined by the subscribing rate of the member states for a particular program. Competition was almost excluded and existed only on the subcontractor level. The return factor for each country gained more and more weight - certainly not a very healthy situation. At the Ministerial Conference in The Hague in 1987, we finally had to accept a guarantee of 95% as a return factor for every country. It was only due to the very high competence of the technical staff and contracts officers of ESA, having twenty years and more of experience, that this worked without loss of too much efficiency, and price escalation.

Finally, let me turn to relations with the member states. It is clear that within the much larger program of ESA as compared to that of ESRO, the political weight of the European space activities is much greater than in the sixties. A sign of this is not only the involvement of member states in the Ministerial Conference, but also the celebration of the 20th and 25th anniversary, having the Queen of the Netherlands, President Mitterrand from France, Bundeskanzler Kohl from Germany and the Prime Minister of Belgium Mr. Martens at ESTEC or Paris, respectively. But dealing with 13 member states was sometimes like dancing with an octopus.

As Director General of ESA, I could not have been successful, without the support of the Council. It was very satisfying to see how involved and helpful most of the Council members were already during my time at ESRO, first when I was a staff member as the scientific director, and later when I became a member of the Council itself. In ESRO's time, there was good mixture of people from the governments and independent scientists representing the member states. In ESA's time, this ratio was no longer really balanced, but what has grown over twenty years and more, has been the feeling of most delegates belonging to a special club and having a particular responsibility, not only towards their own member states, but also towards the entire organization. In connection with the 20th anniversary in 1984, Roy Gibson wrote the following: "Often the positive decisions have been due to the personal dedication of individual delegates who consistently interpreted their instructions in as favourable a light for ESA as was humanly possible. Without betraying their national matters, they voted whenever possible for the European solution. It is indeed interesting to reflect how much of ESA's success has been due to the tenacity of a handful of delegates over the years". I experienced this even more during my period and I could see the many personal friendships which had developed among the delegates.

One of the most satisfying aspects of my time with ESRO and later with ESA were that I had the chance to meet so many interesting people from different countries and to build up good friendships with many of them. I am very pleased that so many are here at the symposium.

From sounding rockets to small satellites. A way of complementing ESRO/ESA flight opportunities

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1. Securing traces

As we are attending this conference, do we write history? I do not think that this is the right expression. History writing is digging into the unknown, interpreting silent witnesses, such as documents, stones, pieces of art, and ordering these pieces of evidence in a political, economical, cultural or other context. The very fact that the protagonists of the early years of European space research are assembled in this room, that they are not silent, but rather eloquent, precludes the word "history" as label for this conference. All the same, we feel that what we are doing here has to do with history. It is more than telling anecdotes at a dinner table. For instance, there is the confrontation with real historians. They have already done a good amount of the digging and ordering and are now thrilled by the opportunity to question the heroes directly. We have a different attitude towards history than our ancestors had hundreds of years ago. For us it is a valuable, almost necessary science. Almost while the events that we want to record are still going on, we take care to secure the traces. Imagine the Spaniards had allowed Athahualpa to tell the story of the Inca Empire in 1533, explain their way of life, their beliefs, their customs and rituals, just for a few hours, and somebody had recorded it. What an immense treasure this would have constituted. But the Spaniards did not care, the history and way of life of these heathens were irrelevant to them. How differently we feel now. At this conference we devote about 15 hours to the comparatively unexciting history of space research in Europe, although it is already recorded in mountains of documents.

When I received the invitation to speak at this symposium with the remark that some of the main protagonists had being invited, I asked myself why I was on the list. Certainly, I was none of the protagonists, at best I was one of the early users. My first inclination was to decline the invitation. But the event seemed to be too attractive to resist the temptation to accept. However, I felt inside myself a slowly growing uneasiness about what aspect of my doings could be of interest to this group. Almost all of the speakers at this conference helped constructing or running ESRO/ESA. Hence the history of these organizations is in the focus of this conference. But the history of ESRO/ESA is not the history of European space research, very much unlike the history of NASA which is essentially the history of US American space research. My professional doings bear witness to the limitations of ESA's space program, that this program needed complementation. So, I decided to make this very aspect, the need for complementation, the subject of my talk. This will not be done in a general way, but rather from the point of view of my personal history.

2. *The early sounding rocket program (1962-1972)*

In 1961 Reimar Lüst set out to develop a new technique, the barium plasma cloud technique, a way to make the near-Earth magnetic field and plasma motions visible, with the ultimate goal to go out into interplanetary space and create an artificial comet. It took 23 years to achieve the latter. Reimar Lüst had left our group for higher offices already 12 years before the first artificial comet was born on December 27, 1984. The beginning was in the Sahara, with experiments accommodated on Centaure rocket flights within the French National Space Program. These were offered to us by Jacques Blamont. Our art of experimenting, the way we filled the barium mixture into the release containers, and how we performed our optical observations was completely amateur-like, irresponsible, but very enthusiastic. After initial failures we had the first success in November 1964. A plasma cloud, aligned along the magnetic field, emerged from the released spherical neutral gas cloud and followed its own path as dictated by the ambient electric field (Fig. 1). A new technique to record the difficult to measure electric field was born. It gave us much work for the future.

By 1972 we had launched altogether 65 rockets, out of which 7 were failures. Among the successful ones were the very first ESRO rocket launch from Sardinia on July 6, 1964, the release from *HEOS I* at 12 R_E in March 1969, and the NASA *Scout* launched up to 5.6 R_E in September 1971 (Fig. 2). I remember two disturbing predictions from the early days, one from Jacques Blamont: 'You will never produce an ion cloud, but it is good that you are getting into the sounding rocket business this way'; the other, a few years later, from Karl Wurm, co-experimenter on the first ESRO rockets: 'You will never produce an artificial comet; that needs tons and tons of material to be lifted into interplanetary space'. Fortunately, both were wrong. The first *artificial comet*, on December 27, 1984, needed just 27 kg and produced a tail visible to a length of ~ 10,000 km (Fig. 3). But I always felt great respect for Jacques Blamont who gave us so much initial support in spite of his doubts in our scientific goal.

Already during these early years of experimenting with sounding rockets it was apparent that ESRO's program could not satisfy the needs of an active experimental group. It was not meant to do so. Only 16 out of the 65 launches until 1972 were part of the ESRO program (Pedersen, 1973). All the same many of us were disappointed when the ESRO Council, in December 1971, decided to abandon the sounding rocket program and to establish instead the ESRANGE Special Project. Why were we disappointed? The ESRO sounding rocket program constituted a platform for young experimenters to meet, to discuss their experiences, their instrumentation and agree on joint projects. Secondly, much competence was assembled at ESTEC, scientific and engineering, which was invaluable for young experimental groups. We in Germany could not complain, a strong National Sounding Rocket Program was defined in 1972/3 and executed through the seventies, but for some of the ESRO/ESA member states — like France — the days of sounding rockets were essentially over. And this was a pity, since the short lead times of such projects, as well as the allowance of a higher risk factor (10% failures were considered to be permissible) made it an ideal testing ground of instrumentation, an evolutionary way for its development. No wonder that a very high fraction of the leading space experimenters of today went through sounding rocket training.

3. From sounding rockets to small satellites

The German national sounding rocket program from 1973 to 1977 contained 45 launches, 7 for astronomy, 29 for aeronomy and 9 for magnetosphere (Joneleit, 1975). Klaus Pinkau, Ulf von Zahn and I were the respective project scientists. Launches were performed at Woomera, Huelva, ESRANGE and Andøya. My program was dedicated to the IMS, the International Magnetospheric Study. Our goal was to compare the plasma physics of the aurora borealis in the upper ionosphere (up to ~ 600 km) with that in the source region, the plasma sheet, where ESA's GEOS satellite was supposed to be positioned in 1976. For this matter we were fighting for permission to overfly Norway after a launch from ESRANGE, in order to obtain good magnetic conjugacy with GEOS and the auroral source region (Fig. 4). Our vehicle was the Aries rocket, derived from the 2nd stage of the Minuteman I system. The young Swedish Space Corporation headed by Frederik Engström decided to build a comfortable assembly building. (Nowadays, after extension in height, it is being used for the MAXUS program in μ-gravity research.) The payloads, named Porcupine, because of the many sting-like booms, contained more than 20 single experiments, some of them on ejectable subpayloads, supplied form the UK, USA, Austria, ESTEC, France, USSR and Germany (Fig. 5). For two years Sweden was negotiating with Norway for the permission of overflight, although strictly legally speaking, such permission was not necessary. Finally, the Norwegian Government decided against it. The flights had to be essentially vertically up and down inside ESRANGE, i.e. within a horizontal range of 60 km. Unfortunately, the first rocket launched in March 1976 exploded a few seconds before burnout. Pieces flew ballistically along the predicted trajectory, but spreading widely, so that a major chunk ended up in the backyard of a Norwegian customs officer, an enormous diplomatic incidence. The reason for the rocket failure was soon found, and the IMS program was successfully completed in the middle and late seventies with three Aries and four Skylarks launches.

By this time we felt more and more that sounding rockets did not provide us with enough time for measurements, even for the short-lived active plasma experiments. We directed our efforts towards satellite programs, which meant towards ESA and NASA. Fig. 6 illustrates this tendency at the example of the launch frequencies for rockets, balloons and satellites on which our institute had some experiment. In the sixties, all our sounding rockets were in space plasma physics, in the seventies astrophysics was added, although balloons still dominated. However, for both disciplines satellite experiments became increasingly important. Fig. 7 gives an account of the various projects, the number of experiments/instruments in which we participated, and their operation times. The shading shows the origin. The ESRO/ESA contribution (fine dots) is quite prominent. For my area of research HEOS 1 and 2 were particularly fruitful missions during the ESRO period, later ISEE and GEOS. The graph shows as well that ESA was by far not the only source of satellite flight opportunities. NASA added very substantially (vertical bars), both in space physics (like IMP) and astrophysics (SMM and, lately, COMPEL and EGRET on GRO). The third most important program line was the German national program, early through AZUR, then very successfully through HELIOS 1 + 2 and later through AMPTE and ROSAT. The cross-hatched shading shows missions for which our institute contributed largely to the spacecraft or central facilities. In my field, this line grew directly out of the sounding rocket program. By the time of the last flights of the PORCUPINE program we felt sufficiently competent to dare building entire satellites ourselves.

The first opportunity was offered to us in June 1977 through M. Lebeau of ESA. The test flights of the Ariane 1 rocket allowed the accommodation of non-expensive scientific payloads. We conceived Firewheel, a mother spacecraft with 12 plasma release containers and 4 separable subpayloads heavily instrumented with plasma and field diagnostics (Fig. 8). Three of the latter were instrumented in Berkeley, Ottawa and at RAL (UK). From the first thought to launch slightly less than 3 years passed, a wonderful program, except for the launch! Ariane L02 exploded on May 20, 1980; the Firewheel turned, as some people said, into a waterwheel.

We slowly recovered from this blow and began our next satellite project, AMPTE (for Active Magnetospheric Particle Tracer Explorers) jointly with NASA, which supplied through the Applied Physics Laboratory of The Johns Hopkins University the Charge Composition Explorer (CCE). On the German spacecraft, the Ion Release Module (IRM), there were again chemical release experiments (16 containers) and plasma diagnostics. When the program was already underway, in a phase when it is customary to undergo descoping exercises, we even managed to squeeze in another spacecraft built around the adapter between IRM and CCE, the UK subsatellite (Fig. 9). Fortunately, this mission was a success. Among a lot more, it produced the first man-made comet (Fig. 3).

On the list of Fig. 7 there are two further special MPE satellite projects, ROSAT, about which I do not have to say much, its fame has spread widely, and FREJA, mainly a Swedish satellite with 25% German contribution. It was developed very much along the lines of AMPTE--IRM, even contained some derivations of that program supplied by MPE, and was successfully launched from China on October 6, 1992.

I just tried to illustrate that an active lab in Europe was served with flight opportunities not only by ESRO/ESA. There were great chances to be selected for a NASA mission, there were possibilities within the national programs, often in bi- or multilateral programs. This spectrum was needed to secure a reasonable flight frequency. A specialty exists for our lab. The short duration of plasma release experiments and the need for extensive on-board diagnostics made us decide to develop inhouse complex rocket payloads and later on entire satellites or major spacecraft subsystems. This created additional flight opportunities for comparatively little money. Table 1 contains numbers. The sums apply to spacecraft costs only, salaries of the project team at MPE included. Some overhead may have been forgotten, but it is an honest account of our expenses. The projects of Table 1 constitute the MPE line of "Small Scientific Satellites".

4. Crisis is your best friend

Besides the diversity of launch opportunities available to a typical European space research lab, Fig. 7 brings out another aspect, the decreasing frequency of events in ESA's program and elsewhere. For example, from the launch of ISEE 2 in 1977, eight years passed till the launch of Giotto which had at least some plasma physics instruments. The next particles and fields mission, Ulysses, came 13 years later, the next magnetospheric mission, CLUSTER, is going to be launched in 1995/6, i.e. 18 years later. In X-ray astronomy, between EXOSAT and XMM 15 years are at least going to pass. IR-astronomy is still waiting for its first mission in 1995. γ-astronomy has not seen anything since COS-B. The concept of a Cornerstone Program is beautiful, but quite deceptive. The nice graphic shown by R. Bonnet which looks like a castle with 4 towers lacks the temporal element. If this were in, it would look like a piece of the Chinese Wall, a few towers in sight, others disappearing beyond the horizon. The tendency is clearly towards fewer and bigger missions.

NASA got worried about this fact and started to think about installing a small satellite program, distinctly smaller than the EXPLORER program, the SMEX (Small-Scale Explorer) program. A workshop was held at GSFC in February '87 in which scientists identified 40 potential candidates for small missions. In May 1988 an AO went out for the SMEX-program. The goal was no more than 30 M$ for spacecraft plus experiments. 51 proposals were received, three were finally selected (Jones and Rasch, 1989). The first was launched in July 1992 (SAMPEX). A second AO has been recently released by NASA.

In Europe similar considerations went on. The International Academy of Astronautics had launched a study (IAA 1990) on small scientific satellites. In it was also considered the potential role of ESA with the conclusion that ESA`s structure was not geared for such a program. However, the 1989 Science Programme Review Team chaired by K. Pinkau thought differently and urged ESA to rethink its position. So, ESA issued a call for ideas for small scientific missions. 52 proposals were received and classified (Table 2) (Olthof, 1991). The outcome of this effort is, however, uncertain.

The central problem is not the efficiency of small versus large satellites, nor the financial capability of national agencies to realize them on their own, the problem has to do with the career expectations of a scientist. Would he be satisfied with having just two or three chances in his lifetime to build an experiment? Can he develop at all the necessary skills to do so? Look at the instrument builders in our community. Most of them are in their fifties. Why? Because the playground for young experimenters, the sounding rockets, have faded away in our field, and lead-times for satellite experiments are forbiddingly long. There is little opportunity and little attraction for training graduate students or post-docs in the art of building space instrumentation. The situation is very different in ground-based astronomy or, for instance, with the EISCAT radar facility both of which have a strong young constituency, because there is something to plan, to build, to operate and to analyze in a reasonable time span.

I believe that European space research is in a *crisis*, or is at least approaching it. Huge expenditures for the space infrastructure vis-à-vis a deteriorating economic situation and a noticeable change in public and political opinion about space activities in general combine to put pressure on the national space science programs. Ways must be found out of this impasse. For ESA it may be the best to go on and provide the backbone of a viable program, i.e. major space missions. But the community at large needs new ideas how to pave the way for a healthy and continuous program. I do not intend to give a recipe here how to reinstall smaller and much less expensive missions and how ESA could contribute to this end. This conference is about history. But it happens to be held at a moment when European space researchers and their program managers up to the political decision makers are asked to reconsider how to approach the future. If we acknowledge the existence of a crisis, we should remember that a crisis is a chance for readjustment, for rejuvenation. The president of SAS wrote recently in the SCANORAMA magazine: 'Success is your worst enemy, but crisis is your best friend'. Let us see it that way and act accordingly.

Bibliography

1. International Academy of Astronautics (IAA): Cosmic Planning Study on 'Inexpensive Scientific Satellite Missions', IAA Space Sciences Committee, June 1990.

2. D. Joneleit: German National Scientific Balloon and Sounding-Rocket Programme, ESRO Special Publication, No. 107, p. 11 (1975).

3. H. Olthof: 'Small Satellites: Results of ESA's Enquiry', 42nd IAF Congress, Montreal, Canada (1991) IAA-91-725.

4. A. Pedersen: Experience of Co-Operation in the ESRO Sounding-Rocket Programme, ESRO SP-97, p. 295 (1973).

5. W. Vernon Jones and N.O. Rasch: 'NASA's Small Explorer Program', 40th IAF Congress, Malaga, Spain (1989), IAA-89-760.

Table 1

PAST and PRESENT MPE Satellite Program

FIREWHEEL: (1977-1980)	PLASMA INJECTION EXPERIMENTS IN THE MAGNETOSPHERE WITH MULTIPOINT DIAGNOSTICS . MOTHER S/C + 4 DAUGHTERS . COLL. WITH USA, CANADA, UK . TOTAL MASS: 1010 KG . 19 EXPERIMENTS . ARIANE L02 - FAILURE MAY 1980 . GERMAN COSTS: ~ 5 MIO DM
AMPTE-IRM: (1980-1984)	ION TRACING IN SOLAR WIND AND MAGNETOSPHERE, ARTIFICIAL COMETS . ONE OUT OF THREE S/C . COLL. WITH USA AND UK . MASS OF IRM: 705 KG . 6 EXPERIMENTS INCL. 16 RELEASE CONT. . LAUNCH: DELTA 3294 . OPERATIONS: 2 YEARS . GERMAN COSTS OF S/C: 18 MDEM
FREJA: (1989-1992)	HIGH RESOLUTION AURORAL PLASMA PHYSICS . SWEDISH-GERMAN SATELLITE . MASS: 259 KG . 8 EXPERIMENTS (2 GERMAN) . LAUNCH: LONG MARCH 2, OCT. 6, 1992 . COST OF S/C: ~ 20 MDEM . GERMAN CONTR. (7 MDEM)

RESPONSE TO ESA'S CALL FOR IDEAS FOR SMALL MISSIONS (1990)

TABLE 2: SMALL MISSION EVALUATION

COMPLEXITY ANALYSIS

CLASSIFICATION:	SIMPLE	MODERATE	COMPLEX	SUMS
SUM OF WEIGHTING FACTORS:	5 - 7	7 - 11	11 +	
ASTRONOMY	1	6	3	10
SOLAR PHYSICS	3	2	2	7
PLANETARY/EARTH SCIENCE	7	3	7	17
SPACE PLASMA PHYSICS	6	3	3	12
	17	14	15	46

other missions 6

(Olthof ,1991)

Figure captions

Figure 1
First production of a barium plasma cloud on November 27, 1964 above the Sahara desert.

Figure 2
Barium plasma cloud at 5.6 R_E distance. The plasma cloud has disintegrated into many magnetic field-aligned striations (Sept. 20, 1971).

Figure 3
First man-made comet; barium plasma cloud in interplanetary space (Dec 27, 1984).

Figure 4
Desired flight path of the Aries rockets in the Porcupine program and conjugate points in the plasma sheet.

Figure 5
Sounding rocket payload "Porcupine".

Figure 6
Number of rocket, balloon and satellite launches per year carrying MPE instrumentation.

Figure 7
MPE satellite experiments and their operation periods.

Figure 8
The Firewheel spacecraft with 12 containers for Li and Ba releases and four ejectable diagnostic subsatellites.

Figure 9
The three AMPTE spacecraft on the launch vehicle. From top to bottom: CCE (USA), UKS (UK) and IRM (FRG).

Figure 1

Figures 2 and 3

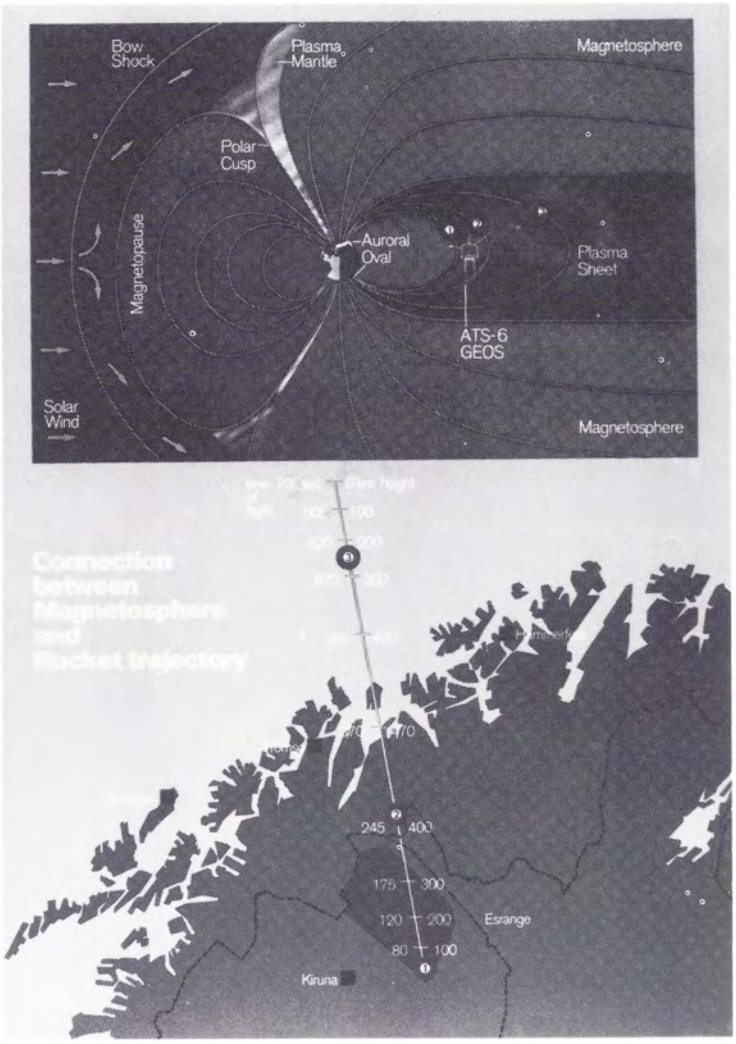
Bow Shock
Plasma Mantle
Magnetosphere
Polar Cusp
Magnetopause
Auroral Oval
Plasma Sheet
ATS-6
GEOS
Solar Wind
Magnetosphere
Connection between Magnetosphere and Rocket trajectory
245 400
175 300
120 200
80 100
Esrange
Kiruna

Figure 4

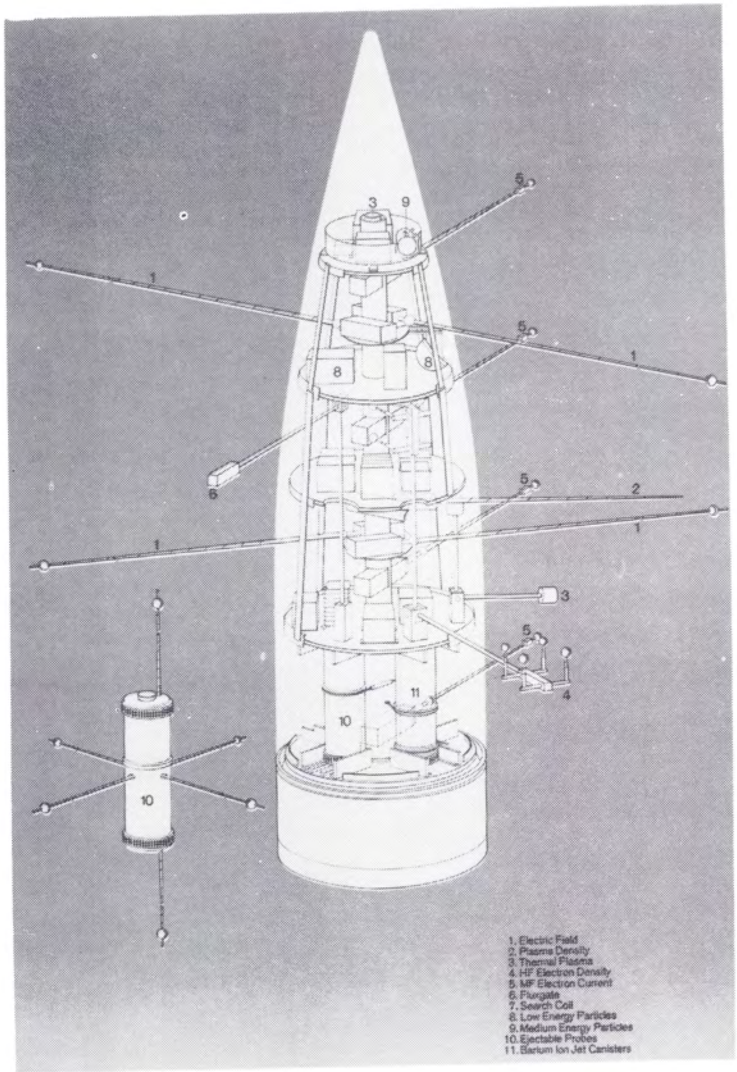
1. Electric Field
2. Plasma Density
3. Thermal Plasma
4. HF Electron Density
5. MF Electron Current
6. Fluxgate
7. Search Coil
8. Low Energy Particles
9. Medium Energy Particles
10. Ejectable Probes
11. Barium Ion Jet Canisters

Figure 5

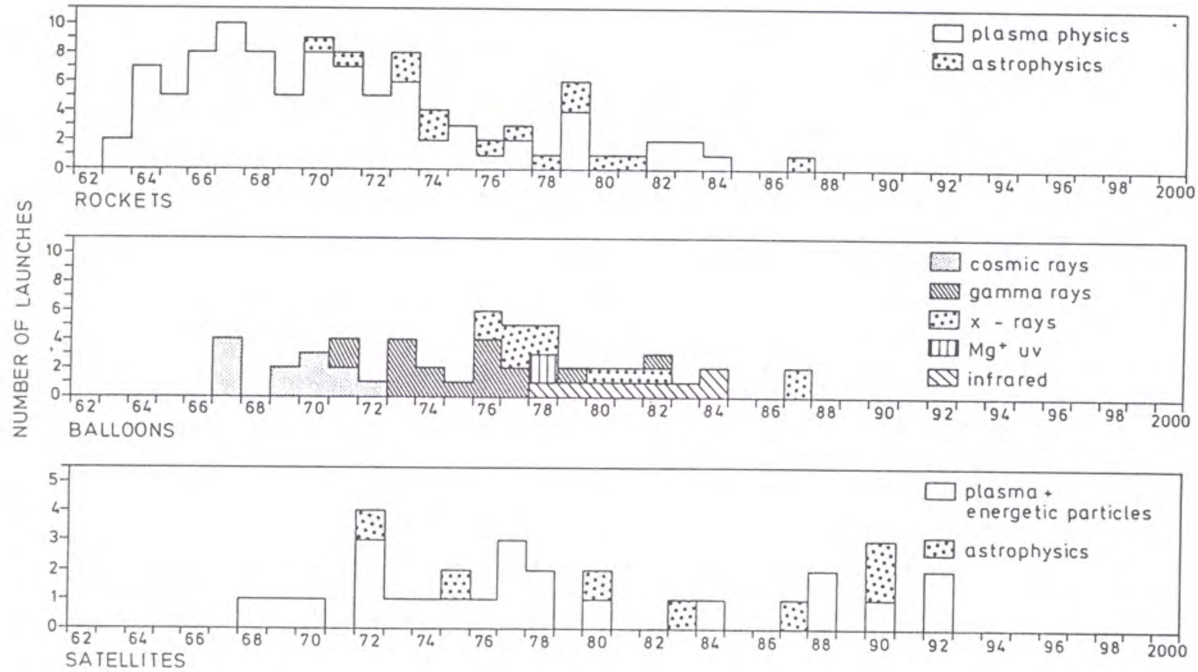
NUMBER OF LAUNCHES
ROCKETS
plasma physics
astrophysics
BALLOONS
cosmic rays
gamma rays
x - rays
Mg⁺ uv
infrared
SATELLITES
plasma + energetic particles
astrophysics

Figure 6

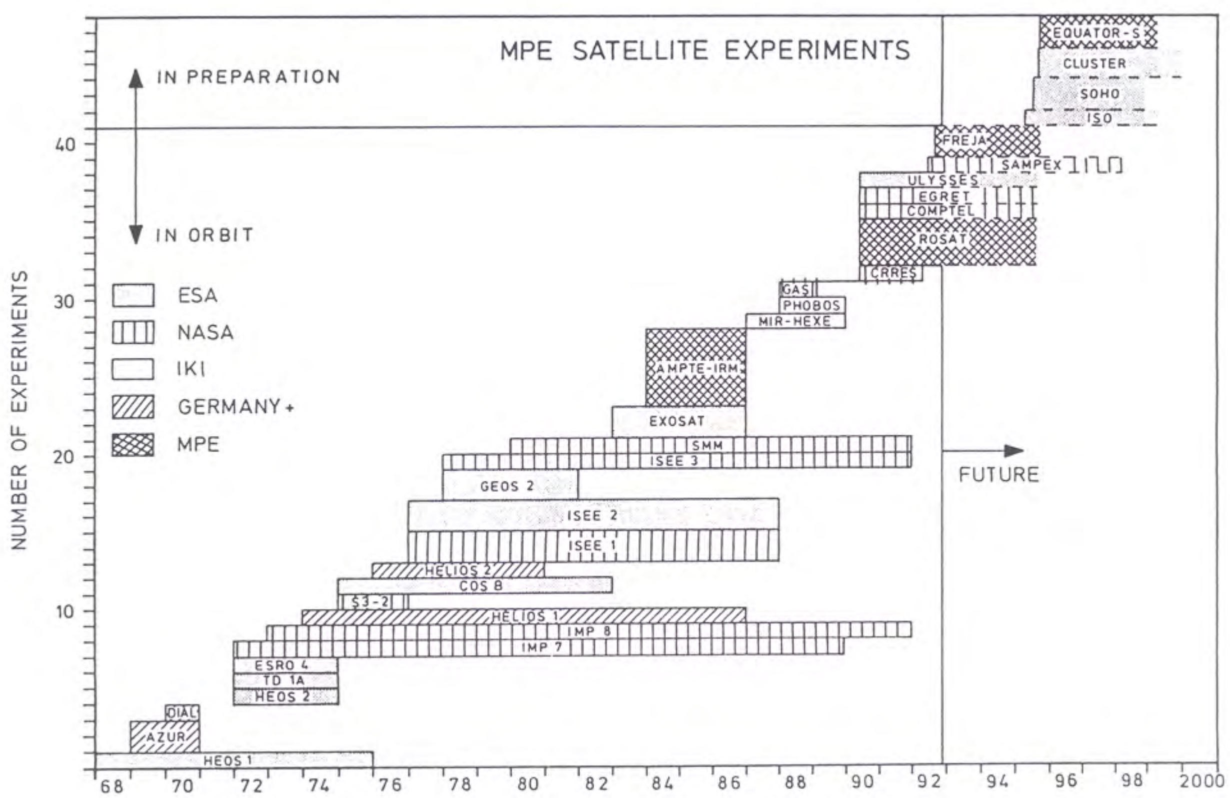
MPE SATELLITE EXPERIMENTS
IN PREPARATION
IN ORBIT
ESA
NASA
IKI
GERMANY +
MPE
NUMBER OF EXPERIMENTS
10
20
30
40
FUTURE
EQUATOR-S
CLUSTER
SOHO
ISO
FREJA
SAMPEX
ULYSSES
EGRET
COMPTEL
ROSAT
CRRES
GAS
PHOBOS
MIR-HEXE
AMPTE-IRM
EXOSAT
SMM
ISEE 3
GEOS 2
ISEE 2
ISEE 1
HELIOS 2
COS B
S3-2
HELIOS 1
IMP 8
IMP 7
ESRO 4
TD 1A
HEOS 2
DIAL
AZUR
HEOS 1
68
70
72
74
76
78
80
82
84
86
88
90
92
94
96
98
2000

Figure 7

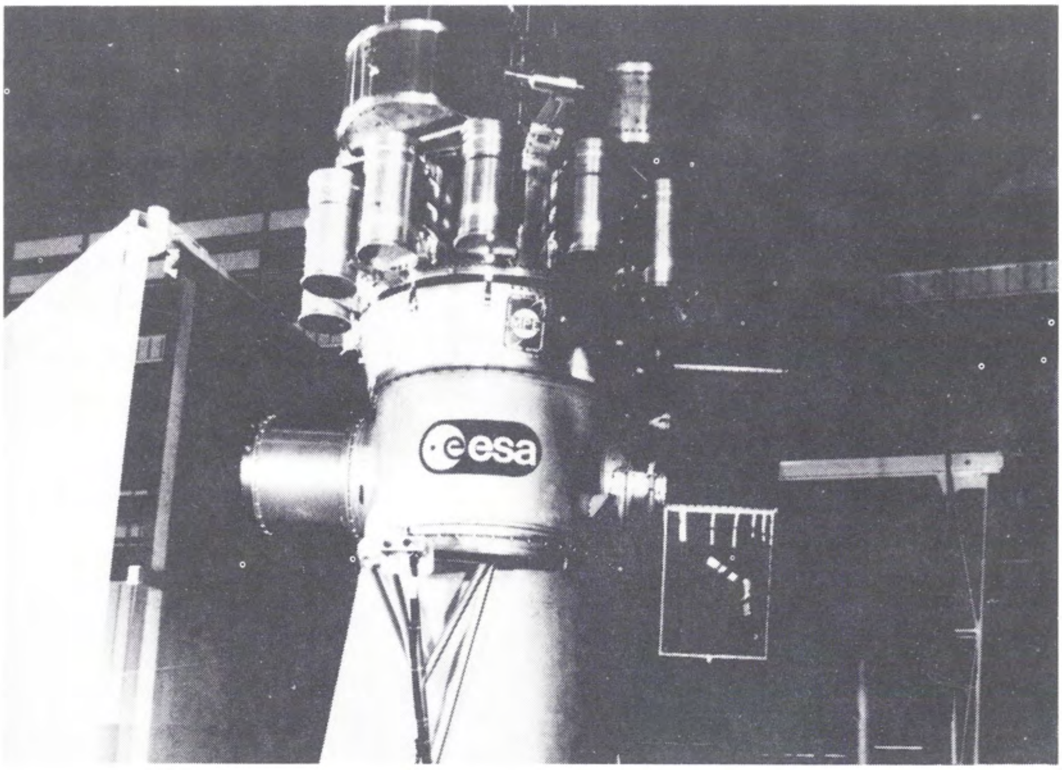

Figure 8

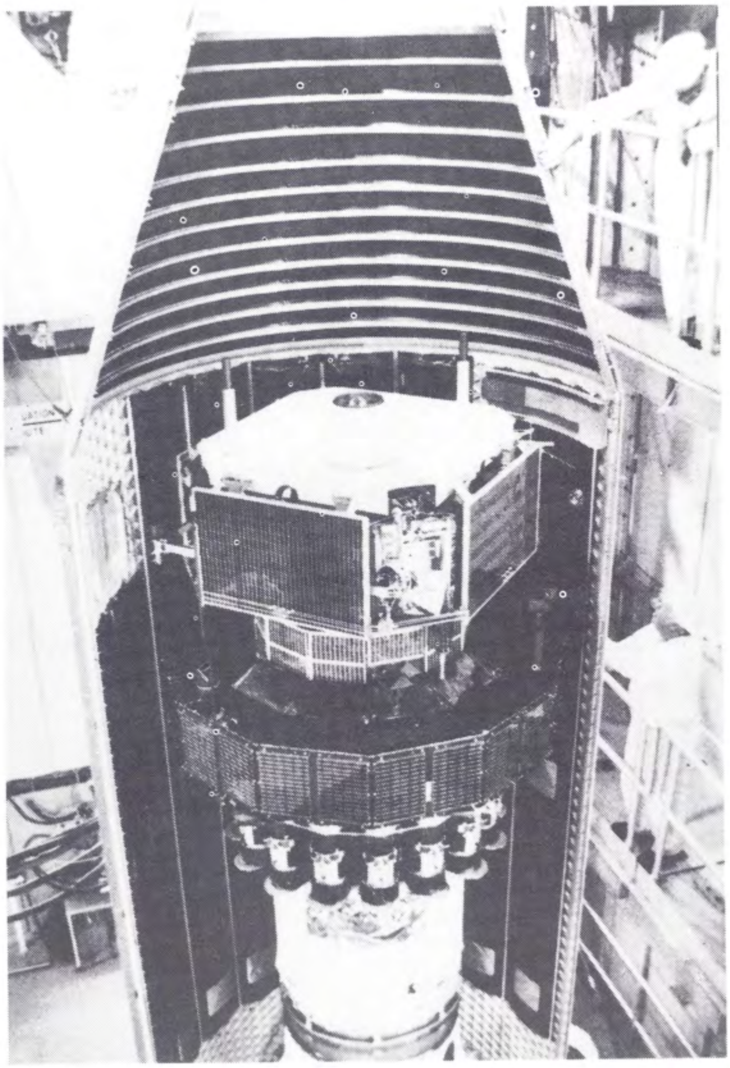

Figure 9

European space scientists and the genesis of the Ulysses mission, 1965-1979

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University of California, Irvine

Introduction

In 1965 Ludwig Biermann, who was then Germany's leading astrophysicist, published the first article to examine the scientific merits of "out-of-the-ecliptic studies." He built a case that in the near future it was "important to extend ... studies in interplanetary space ... to those regions [beyond] the ecliptic plane which can be reached without undue effort." He also expressed the hope that ultimately it would be possible to conduct "experiments at higher heliographic latitudes."[1] As things turned out, space scientists are just now on the verge of making the kinds of observations that Biermann envisioned more than a quarter of a century ago. In February 1992 ESA's *Ulysses* swung around Jupiter to begin a journey away from the ecliptic plane that will carry the spacecraft below the sun's southern pole for six months in 1994 and on around above the sun's northern pole for three months in 1995.[2] The mission's thirteen science teams are eager to learn whether the results secured at these high heliographic latitudes will justify their many, often frustrating years preparing for this opportunity. Likewise ESA and NASA administrators are anxious to ascertain whether *Ulysses's* scientific and other returns will justify their agencies' respective investments of $250 and $500 million in the mission.[3]

Acknowledgments: My interest in the origins of the out-of-ecliptic mission goes back to 1986 when I was working on contract for the NASA History Office on what became my *Exploring the Sun: Solar Science since Galileo* (Baltimore MD: Johns Hopkins University Press, 1991). As I pursued this interest, I received much help from Craig Waff, who was then working on the origins of the Galileo Mission for the NASA History Office. In particular, he supplied me with many pertinent documents from NASA, JPL, and the Van Allen Papers. Besides Waff, I am indebted to the following for documents used in this paper: Harry Elliot, Harold Glaser, Reimar Lüst, Larry A. Manning, Richard G. Marsden, D. Edgar Page, and John J. Quenby. Before and during the Palermo Symposium, I had helpful discussions with Michelangelo De Maria, Arturo Russo, Martin Huber, Maurice Lévy, and G. Haerendel.

1 L. Biermann, "Some Aspects of the Physics of Interplanetary Space Related to Out-of-the-Ecliptic Studies," *Advances in Space Science and Technology*, 7 (1965), 437-47, on 438f.

2 E. J. Smith, K.-P. Wenzel, and D. E. Page, "Ulysses at Jupiter: An Overview of the Encounter," *Science*, 257 (1992), 1503-07.

3 For the figures on mission costs, see H. Gavaghan, "Ulysses heads for the Sun," *New Scientist*, 128 (1990), 8. Some ESA administrators believe that NASA's costs would come in substantially lower if

Although we cannot now be sure how these issues will be resolved, we can inquire how early visions of an out-of-ecliptic probe eventually found expression in the Ulysses Mission. A full account of this mission's origins would run from the 1960s to 1990, considering all the main individual and institutional actors, their agendas and priorities, their access not only to funds but also to expertise and facilities, and their larger fiscal and political environments. In this paper I shall simplify the task in two ways. I confine my attention to the period from Biermann's article of 1965 to the ESA/NASA memorandum of understanding for the mission of 1979. And I tell the story from the vantage point of those European space scientists who helped promote, define, and/or prepare for the mission. In particular, I consider three questions: Why did some European space scientists think in 1965-72 that an out-of-ecliptic mission would yield significant scientific results? How did they participate in 1973-76 in the building of a constituency strong enough to secure joint European-American definition and feasibility studies? And how, despite a worsening fiscal climate, did they and their allies obtain space-agency commitments in 1977-79 for funding and cooperation on a dual-spacecraft, Jupiter-swingby, solar-polar mission.

To my knowledge, two authors have devoted studies to the origins of the Ulysses Mission. Harry Elliot, an English space scientist who was a particularly persistent and effective advocate of an out-of-ecliptic probe (see below), has recently provided an account of the mission's "tortuous prelude ... as seen from the European side of the Atlantic." This narrative, which is evidently based on Elliot's files as well as his recollections, provides a vivid picture of all ESA's difficulties in collaborating with NASA.[4] Joan Johnson-Freese, an American political scientist working on the subject of international cooperation, has focused on a key episode in the story — the events leading to NASA's 1981 cancellation of its spacecraft for the mission and ESA's unsuccessful efforts to reverse this decision. She concludes that this unhappy affair played a major role in shaping ESA's approach to subsequent cooperative ventures with NASA.[5] The present paper with its focus on the role of European space scientists in advocating, planning, and winning approval for the mission between 1965 and 1979 complements the articles of Elliot and Johnson-Freese in two main ways. First, it provides a fuller account of the steps leading up to the mission's approval. Second, it reveals that European space scientists, always an essential group of mission proponents, played an increasingly circumscribed role in the campaign that culminated in the ESA/NASA memorandum of understanding for the International Solar Polar Mission.

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they were limited strictly to the incremental costs attributable to this mission.

4 H. Elliot, "The Genesis and Evolution of the International Solar Polar (Ulysses) Mission," *COSPAR Information Bulletin*, no. 122 (Dec. 1991), 82-89.

5 J. Johnson-Freese, "Cancelling the US Solar-Polar Spacecraft: Implications for International Cooperation in Space," *Space Policy*, 3 (1987), 24-37.

Developing a scientific rationale for an out-of-ecliptic mission, 1965-72

Beginning in the mid-1960s, a few European space scientists — like some of their American counterparts — started discussing the scientific advantages of sending a probe to high heliographic latitudes. They did so for two main reasons. First, the dramatic development of space technology in the years since *Sputnik* gave them confidence that it would soon be feasible to get a spacecraft out of the ecliptic plane. Space engineers were, indeed, already considering the prospects of using solar-powered ion engines or a planetary swingby to send a probe into this hitherto inaccessible region of interplanetary space. Second, the rapid advances in knowledge that had resulted from numerous missions near the ecliptic plane led space scientists to anticipate that comparable advances might well be forthcoming from out-of-ecliptic observations. In particular, studies at high heliographic latitudes seemed likely to contribute in important ways to understanding of the particles, fields, and dust that comprised the interplanetary medium.

Ludwig Biermann of the Max-Planck-Institut für Physik und Astrophysik in Munich was, as mentioned at the outset, the first to canvass the scientific arguments for *in situ* observations away from the ecliptic plane. His interest in this prospect was a natural outgrowth of his pioneering theoretical work on the solar wind, the supersonic efflux from the corona responsible for the anti-solar orientation of cometary plasma tails.[6] Writing within months of the first international solar-wind conference in 1964, Biermann considered how out-of-ecliptic studies might improve knowledge of the solar wind, the magnetic fields embedded in it, the influence of these fields on solar and galactic cosmic rays, and the distribution of interplanetary dust. He concluded that the time was ripe for direct investigations of the solar wind's plasma properties and associated magnetic fields because various indirect inquiries had already established their "essentially three-dimensional nature." Believing, however, that there was still much to be learned about cosmic-ray modulation and interplanetary dust in the ecliptic plane, he advised against adding anything more than "a small device for measuring energetic particles" to the first out-of-ecliptic probe's solar-wind instruments.[7]

The second European space scientist to take a close look at the scientific merits of an out-of-ecliptic mission was Harry Elliot of Imperial College, London. Having gotten cosmic-ray experiments into space on *Ariel 1* (1962), *ESRO 2* (1968), and *HEOS 1* (1968), he was one of the

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6 K. Hufbauer, *Exploring the Sun: Solar Science since Galileo* (Baltimore: Johns Hopkins University Press, 1991), pp. 215-21. Biermann's interest in discussing an out-of-ecliptic mission may have been stimulated by his appointment in the spring of 1964 to head ESRO's PLA working group which was responsible for proposals concerning the moon, planets, comets, and interplanetary medium — see A. Russo, "The Definition of ESRO's First Scientific Satellite Programme (1961-1966)," ESA HSR-2 (Oct. 1992), pp. 23-24, 47.

7 Biermann (n. 1), 446.

leading spokesmen for British space science.[8] In 1968, for instance, he was appointed chair of the British National Committee on Space Research's Working Group 3, the charge of which was "to examine that group of observations which demand the use of deep orbit satellites or space probes and to propose projects which could carried out in the period 1970-1980."[9] Elliot guided his committee to the conclusion that an out-of-ecliptic mission to 45° heliographic latitude would best meet the dual desiderata of yielding novel scientific results and stimulating the nation's aerospace industry. On the scientific side, he and his colleagues argued that such a mission would obtain the first observations of the solar wind and solar energetic particles originating from "the zones of maximum sunspot activity." It would also, they maintained, enable observations of importance "for furthering our understanding of the modulation of the cosmic ray flux by the interplanetary field." On the technological side, they thought such a mission would give British industry the opportunity to develop the first solar-powered ion-propulsion system, which would be used-after launch by NASA-to send the spacecraft away from the ecliptic plane.[10]

After the report's completion in June 1969, Elliot sought for over a year to generate interest in a joint UK/NASA out-of-ecliptic mission.[11] Unsuccessful, he took the idea to ESRO planning committees in January 1971 with the aid of John Quenby, a close colleague at Imperial College. Quenby spoke for the mission at ESRO's working group for cosmic rays, persuading its members to recommend a mission-definition study.[12] And Elliot informed the Launching Programmes

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8 For Elliot's roles in the British and European space programs, see H. Massey and M. O. Robbins, *History of British Space Science* (Cambridge: Cambridge University Press, 1986), pp. 75-76, 83-84, 144-45, 219, 221, 333-35, 436, 467; and A. Russo, "Choosing ESRO's First Scientific Satellites," ESA HSR-3 (Nov. 1992), pp. 42, 44-45, 49.

9 BNSCR: Working Group No. 3, "Minutes [of first Meeting on 23 July 1968]," NCSP/44 (68), Royal Society Archives, London, England. This and two other *ad hoc* working groups on space science and technology were established on the recommendation of the organizer of a day-long discussion of British space policy and programs that was held by the Royal Society on 27 Feb. 1968. At this discussion, Elliot had spoken in favor of an out-of-ecliptic mission-see *Proceedings of the Royal Society*, A308 (1968), 155, 241.

10 BNSCR: Working Group No. 3, "Report to the British National Committee on Space Research " (13 June 1969), NCSP/36 (69), Royal Society Archives.

11 When Elliot presented his proposal to the BNSCR in November 1969, the committee was unwilling to go further than state the importance of securing "an official view on the wisdom of the U.K. investment" in ion-propulsion technology—see BNSCR, "Meeting-24 November 1969: MINUTES," NCSP/57 (69), Royal Society Archives.

12 ESRO: Scientific Working Group COS on Cosmic Rays and Trapped Radiation, "Report of the 27th Meeting ... on 8 January 1971," COS/58 (25 May 1971), courtesy of J. J. Quenby. COS also recommended that Quenby and one other COS member — G. Wibberenz — be appointed to the mission-definition group. Quenby's success in winning COS's endorsement was very important, for as A. Russo has shown, this working group was in the ascendancy at the time — see his "Choosing Big Projects in Space Research: The Case of ESRO's Scientific Satellite COS-B," *History and Technology*,

Advisory Committee (LPAC) of the potential for ESRO/NASA cooperation on a joint out-of-ecliptic mission.[13] Three months later, he also discussed the possibility of a cooperative mission at the Symposium on International Collaboration in the Next Generation of World Space Projects.[14] The outcome of Elliot's campaign was that ESRO's Directorate of Programmes and Planning established a mission-definition group in June 1971 to report on "the general scientific goals that might be achieved with experiments on board a spacecraft going to a high solar latitude," to recommend "the most advantageous orbit configuration," and to propose "a model payload." The group's scientific leaders were Quenby, Richard Giese (a specialist on interplanetary dust at the Ruhr University in Bochum), and Gerd Wibberenz (a cosmic-ray physicist at Kiel University).[15] In addition, it included five ESRO technical and administrative employees.

The mission-definition study of April 1972 was the most thorough discussion of the scientific rationale for observations at high heliographic latitudes yet to appear, not only in Europe but also in the United States.[16] Its authors analyzed an out-of-ecliptic mission's potential contributions to six main problem areas, proposing a model payload of 18 kilograms (see Table 1, at end). They also compared the scientific and technical merits of an inclined-orbit mission to 40° heliographic latitude with a Jupiter-swingby mission that would eventually reach much higher latitudes. In doing so, they conceded that the swingby mission would provide better data on galactic cosmic rays and the chance to secure information on the Jovian radiation belts. But they preferred the inclined-orbit mission, partly because this mission would provide an early opportunity to secure high-latitude data on the solar wind, solar energetic particles, solar X-rays,

9 (1992), 27-61.

[13] ESRO: Launching Programmes Advisory Committee, "Report of the 35th Meeting ... on 19/20 January 1971," LPAC/100 (7 Apr. 1971), ESA Archives in the Historical Archives of the European Communities, European University Institute, Florence, Italy. Elliot was appointed to LPAC in 1970 — see A. Russo, "The Definition of a Scientific Policy: ESRO's Satellite Programme in 1969-1973," ESA HSR-7 (Feb. 1993), p. 50.

[14] For Elliot's paper of 21 Apr. 1971, see "Scientific Deep Space Probes," *Aeronautical Journal,* 76 (1972), 27-31.

[15] K. Wilhelm to J. J. Quenby with copies to Giese and Wibberenz (17 June 1971), courtesy of J. J. Quenby. In selecting Quenby and Wibberenz for the study group, the Directorate was following the advice of the cosmic-ray working group — see n. 12. In selecting Giese, it was probably motivated, at least in part, by his brief discussion of the possibilities of examining the distribution of interplanetary dust with an out-of-ecliptic probe — see R. H. Giese and C. v.Dziembowski, "Suggested Zodiacal Light Measurements from Space Probes," *Planetary and Space Science,* 17 (1969), 949-56, on 949 and 956.

[16] ESRO: Mission Definition Group, "Preliminary Mission Definition of an 'Out of the Ecliptic Plane' Mission for Scientific Investigations," MS/318, rev. 2 (11 Apr. 1972), courtesy of J. J. Quenby. The only earlier report that came at all close to offering as comprehensive an analysis of the scientific case for such a mission was prepared by scientists and engineers at NASA Ames Research Center — see "Pioneer H Jupiter Swingby Out-of-the-Ecliptic Mission Study: Final Report" (July [20 Aug.] 1971), courtesy of L. A. Manning.

and zodiacal light, partly because it would be compatible with ESRO's existing reliability standards, and partly because it would stimulate the development of ion-propulsion technology. In concluding, the study's authors asserted that

> There can be no doubt that a number of important scientific problems in Solar-Terrestrial physics require the launch of an off-Ecliptic probe for their solution. [A] spacecraft carrying about 20 kg of experiments can make a significant contribution to new knowledge in this field. Comparison of a Jupiter swing-by and a 1 AU out of ecliptic mission suggests that rather more scientific questions are specifically answered by the latter, which furthermore is technically more attractive to ESRO.

Their closing recommendation was that ESRO conduct a feasibility study "as soon as possible ... in order to give a sound support to this scientifically appealing mission."[17]

Thus, starting with Biermann, a small yet growing number of European space scientists developed a good case for observations at high heliographic latitudes. However, notwithstanding the cogency of the mission-definition study of April 1972, these advocates could not realistically expect that ESRO's science planners would give serious consideration to an out-of-ecliptic mission until the next round of planning in the mid-1970s.[18] Impatient, Quenby convened a group of eight Dutch, French, German, and British space scientists in July 1972 to propose that a consortium of European nations, rather than ESRO, collaborate with NASA on such a mission.[19] However, their proposal elicited no more interest from NASA than had the mission-definition study from ESRO.[20] The basic problem was that the mission did not yet have a scientific-technical constituency of sufficient size and clout for space-agency science planners to treat it as a serious option.

Building a constituency for an out-of-ecliptic mission, 1973-76

In 1972-1973 two developments created an environment that was more favorable than hitherto for building the sort of constituency needed for an out-of-ecliptic mission. On the administrative side,

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17 *Ibid.*, 39-40.

18 During 1972, ESRO's science planners were deeply involved in the process that culminated with the selection the missions that became ISEE-2 and Exosat respectively — see Russo (n. 13).

19 C. de Jager, L. D. de Feiter, J. J. Burger, G. Weill, G. Wibberenz, K. Wilhelm, R. Dalziel, and J. J. Quenby (convenor and lead author), "Off-Ecliptic Probe: A Possible Joint Venture involving British, Dutch, French and German Collaboration with NASA" (Aug. 1972), courtesy of J. J. Quenby. At the end of the proposal, they listed another 14 European space scientists who they thought would be interested in an out-of-ecliptic mission.

20 J. J Quenby to J. M. Wilcox (Nov. 1973), John M. Wilcox Papers, Center for Space Science and Astrophysics, Stanford University, Stanford CA.

ESRO and NASA responded to an increasingly tight budget climate by agreeing to cooperate more closely in science-program planning.[21] This agreement gave the two agencies' science planners extra incentive to nurture cooperation between European and American space scientists interested in new missions. And on the technical side, NASA controllers used a gravitational assist from Jupiter in early December 1973 to line up *Pioneer 10* for its rendezvous with Saturn. In doing so, they demonstrated the feasibility of using a Jovian swingby to get out of the ecliptic plane.

Even as this more favorable environment was coming into being, key ESRO advisory committees were considering mission priorities for the early 1980s. In fall 1973 the Solar System Working Group (SSWG) and the Astrophysics Working Group reviewed possible missions in their respective domains for the Launching Programmes Advisory Committee.[22] At the SSWG meeting[23] Peter Hedgecock, a magnetometer specialist who was at Imperial College with Harry Elliot and John Quenby,[24] laid out the scientific case for an out-of-ecliptic mission. His oral presentation was essentially an abbreviated version of the preceding year's mission-definition study. Although the SSWG declined to get into the delicate business of recommending priorities, Hedgecock took advantage of a supplemental report to make a bid for the support of Jean-Louis Steinberg, a solar radio astronomer at Meudon who served as LPAC's representative to the SSWG. Steinberg was eager to make stereoscopic studies of coronal phenomena by coordinating observations from a deep space probe with observations from earth, or earth orbit.[25] In his supplemental report, Hedgecock pointed out that an out-of-ecliptic probe would be ideal for such stereoscopic research.

In January 1974, LPAC made a first crack at prioritizing the twenty missions discussed by its two working groups.[26] Of its five members, two — Elliot and Steinberg — could see direct

[21] Formal consultation between ESRO and NASA science-program planners began sometime in the 1960s. Such meetings occurred in April 1969, 1-2 February 1971, and 18 April 1972 — see LPAC/100 (n. 13) and Russo (n. 13), pp. 38-39.

[22] For the origins in 1971 of these two working groups, which replaced ESRO's six former, more specialized working groups, see Russo (n. 12), p. 48; and Russo (n. 13), pp. 26-27, 53.

[23] ESRO: Solar System Working Group, "Report to LPAC on Priorities for the 1980s" (Dec. 1973), SOL (73) 16, ESA Archives.

[24] P. C. Hedgecock to K. Hufbauer (12 Oct. 1992). For early evidence of his interest in an out-of-ecliptic mission, see P. C. Hedgecock, J. J. Quenby, and S. Webb, "Off-Ecliptic Control of Cosmic Ray Modulation," *Nature Physical Science*, 240 (1972), 173-75, on 175.

[25] For Steinberg's interest in stereoscopic studies, see his comments in ESRO: Astrophysics Panel, "Report of Second Meeting ... on 20 and 21 October 1969 at Imperial College London," LPAC/52 (9 Jan. 1970); and J.-L. Steinberg and C. Caroubalos, "Space Radioastronomy of Solar Bursts at all Frequencies: The Stereo-Project," *Astronomy and Astrophysics*, 9 (1970), 329-38.

[26] ESRO: Launching Programmes Advisory Committee, "Guidelines for ESRO Scientific Mission

links between an out-of-ecliptic mission and their individual research interests. Not surprisingly, therefore, LPAC included such a mission in the list of eleven recommended to the Director General for immediate mission-definition studies. In doing so, the committee was stipulating that the mission satisfied four criteria — it promised to be (1) "new, attractive, and scientifically sound," (2) of interest to several European research groups, (3) an opportunity for "meaningful ... ESRO participation," and (4) affordable. LPAC's discussion of an out-of-ecliptic mission itself stressed the desirability of securing a three-dimensional picture of the interplanetary medium, increasing knowledge of the way in which the solar wind modulated cosmic rays, and tracing solar-wind properties back to solar features. In addition, LPAC claimed that an out-of-ecliptic mission "alone [would] be of interest to at least fifteen scientific groups" in Europe and that one with stereoscopic capabilities "would be of considerable interest to a broad scientific community covering at least twenty-five groups." It also maintained, however, that the high "launcher costs" obliged ESRO to think of the mission "as a cooperative project with another space agency."[27]

In these last remarks LPAC was anticipating the forthcoming ESRO/NASA science-program review, which was held at the European Space Technology Center (ESTEC) in February 1974. The meeting resulted, among other things, in an agreement for a joint study of "two planetary missions ... in order to establish the best mode of cooperation in the development of these projects."[28] One of these missions was an out-of-ecliptic probe.[29] Regarding such a mission, LPAC members and other ESRO negotiators seem easily to have agreed with their NASA counterparts that the scientific rationale was sound, that ion propulsion should be used for escape from the ecliptic plane, and that launch should occur in 1979-80. Both sides also agreed that it could "be effected in a cooperative mode with relatively clear engineering and management interfaces" — NASA would be responsible for the launch rockets and ion-propulsion system; ESRO, for the spacecraft; and both agencies, for experiment selection.[30] This was, insofar as most European advocates of an out-of-ecliptic mission were concerned, an ideal agreement. The only dissent, indeed, was a request from the UK's Science Research Council that the mission-definition

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Studies" (Jan. 1974), LPAC (74) 4, ESA Archives.

27 *Ibid.*, 13,.20.

28 A. W. Frutkin to A. Hecker (7 Mar. 1974) with "Agreements and Action Items arising from the NASA/ESRO Science Program Review held at ESTEC, Noordwijk on 11th February 1974," D. H. Herman File, NASA Headquarters, Washington DC, courtesy of C. Waff.

29 The other planetary mission covered by the agreement eventually became *Galileo*.

30 *Op. cit.* (n. 20). By this time, as the assignment of the ion-propulsion system to NASA indicates, ESRO planners had concluded that developing this new technology was beyond the European agency's means. For the American aerospace press's view of this new stage of ESRO/NASA cooperation, see C. Covault, "NASA Presses Planetary Aid in Europe," *Aviation Week and Space Technology*, 100 (1974), 38-39.

study "be extended to take into account the possibility of a mission over the solar poles ... via a Jupiter swingby."[31]

Both European and the American positions had shifted somewhat by the summer of 1974 when ESRO and NASA science planners began working out the details for the joint definition study. European space scientists and ESRO wanted to give the out-of-ecliptic mission a stereoscopic capability so as to enlist the support of Steinberg and researchers with like interests. Meanwhile, in deference to many American space scientists, NASA was now insisting that the joint study give equal scrutiny to the swingby and ion-propulsion options for getting away from the ecliptic plane. In early July, forty-seven European and seven American space scientists and space-science planners articulated their respective positions during ESRO's two-day "Symposium on The Sun and Solar System in Three Dimensions."[32] According to the two NASA-Headquarters participants, the symposium's final panel discussion

> resulted in an understanding that this mission would have the best chance of implementation if it appealed to a broad scientific and technological constituency. Dr. Page of the ESTEC Space Science Department, stated rather strongly that the only chance of implementing the solar stereo mission as well as an out-of-the-ecliptic mission was to merge these two missions and to effect them as a cooperative NASA/ ESRO project using the [ion] propulsion mode. Dr. Haskell of ESRO Headquarters, stated categorically that ESRO would not implement the solar stereo mission if it were divorced from out-of-the-ecliptic mission.

In an executive session following the symposium, the space-agency officials present decided "to form a joint NASA/ESRO Science Working Group to define the optimum mission mode and a typical payload." And, eager to comply with ESRO's deadlines, they scheduled the group's first meeting for just five weeks hence.[33]

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31 ESRO: Scientific Programme Board: U.K. Delegation, "ESRO Scientific Programme — LPAC's Recommendations," PB-S (74) 17, Herman File, NASA Headquarters. Many American advocates of an out-of-ecliptic mission were outraged by NASA's willingness even to consider ion propulsion for this mission-see, for instance, D. H. Herman to J. A. Van Allen (8 Mar. 1974), Herman File.

32 ESRO, "Symposium on the Sun and Solar System in Three Dimensions, 2-3 July 1974, Frascati (Italy): Programme" (26 June 1974), ESA Archives. Among the twenty speakers and session chairs, the six following European space scientists have already appeared in this paper's text as advocates of an out-of-ecliptic mission: Elliot (since mid-1969), Quenby (since early 1971), Wibberenz and Giese (since mid-1971), Hedgecock (since late 1973), and, Steinberg (since early 1974).

33 S. I. Rasool and D. H. Herman, "Trip Report: Symposium on the Sun and Solar System in Three Dimensions" (18 July 1974), SL/Chron. File, NASA History Office, Washington DC, courtesy of C. Waff.

The joint group met first at the Jet Propulsion Laboratory (JPL) in early August 1974 and then at ESTEC in late September. Its seven members included a minority of three Europeans — Steinberg, D. Edgar Page (a cosmic-ray physicist at ESTEC), and W. Ian Axford (an interplanetary-medium theorist who had just assumed the directorship of the Max-Planck-Institut für Aeronomie in Katlenburg-Lindau).[34] The group soon reached consensus on the desirability of giving the mission a stereoscopic capability. Attempting to build on this consensus, Axford proposed that two spacecraft — one built by ESRO, the other by NASA — be sent out of the ecliptic plane by gravitational assists during Jupiter swingbys.[35] The group's American members were quick to support Axford's proposal. But the final report, the writing of which was supervised by Page, indicates that Page and Steinberg persisted in thinking it was preferable to send one spacecraft into a 1-A.U. inclined orbit with ion propulsion. Their main argument was that such an orbit

> permits the sorting out of distance-latitude-time variations, allows recalibration against known conditions each time the spacecraft crosses the ecliptic plane, and in the end makes possible the long term statistical averaging so important in establishing background magnetic field configurations.[36]

Although the joint group agreed to disagree about the mission options, NASA Headquarters settled the matter unilaterally even as Page was drafting the final report. In November, responding to pressure from American space scientists who favored the swingby approach, NASA terminated JPL's program to develop ion propulsion for the out-of-ecliptic mission.[37]

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34 According to Rasool and Herman (*ibid.*), Elliot, Page, and Steinberg were announced as the European representatives to the joint study group in the executive session following the ESRO symposium. Elliot and Steinberg were, given the earlier history of European advocacy of an out-of-ecliptic mission, obvious choices. Page, who had attended the SSWG meeting of Nov. 1973 in which Hedgecock pitched the mission, may have been chosen because of his forceful remarks during the symposium's concluding discussion. By the time the joint study began, Elliot had been replaced with Axford. A New Zealander by birth, Axford had taken his Ph.D. at Manchester and worked in Canada (2 years) and the USA (11 years) before taking his position in Germany in 1974. He was probably chosen for the study group because he had been among the American advocates of an out-of-ecliptic mission since at least 1971 — see NASA: Science Advisory Group: Outer Solar System Missions: Particles and Fields Subgroup, [Minutes of Meeting] (30 July 1971), SAG Folder, James A. Van Allen Papers, University of Iowa, courtesy of C. Waff.

35 For Axford's role in proposing this option, see D. E. Page, "Exploratory Journey out of the Ecliptic Plane," *Science*, 190 (1975), 845-50, on 850.

36 ESRO, "Out of Ecliptic and Solar Stereoscopic Mission" (16 Dec. 1974), MS (74) 34, on p. 14, courtesy of D. E. Page.

37 D. H. Herman, "Memorandum re Termination of SEP Out-of-the-Ecliptic Activity at JPL" (18 Nov.

No matter how much NASA's action irritated them, those European space scientists who wanted an out-of-ecliptic mission with stereoscopic capabilities were in no position to urge ESRO to proceed on its own. So, putting aside their annoyance, they set about advocating a joint ESRO/NASA feasibility study. Their campaign was successful. In late January and early February 1975, ESRO science planners informed their NASA counterparts that the European agency regarded the dual-spacecraft, Jupiter-swingby, out-of-ecliptic, solar-stereoscopic mission as the most suitable current prospect for a cooperative project.[38] Finally in late March, Harry Elliot and the other delegates to ESRO's Scientific Programme Board formally approved a joint study of the mission's technical feasibility.[39] This approval was one of ESRO's last legacies to ESA which in April began taking over ESRO's functions.

During ESA's first year, European space scientists helped improve the prospects of an out-of-ecliptic mission in two main ways. First, twenty-two researchers and planners from ESA nations attended the NASA/ESA "Symposium on the Study of the Sun and Interplanetary Medium in Three Dimensions" at Goddard Space Flight Center in May 1975. Their number included most of the recent leaders of the European campaign for the out-of-ecliptic mission — Quenby (since early 1971), Wibberenz (since mid-1971), Hedgecock (since late 1973), Steinberg (since early 1974), Page (since mid-1974), and Axford (since mid-1974).[40] Through participating in the symposium, these men and their colleagues enriched the analysis of the ways in which the mission's observations might contribute to the understanding of solar phenomena, the solar wind, interplanetary magnetic fields, cosmic rays and their modulation, and interplanetary dust and neutral gas. Their participation also attested to European interest in the mission for American space scientists and NASA science planners. Second, several European space scientists — most notably, Axford, Steinberg, and Klaus-Peter Wenzel (a cosmic-ray physicist at ESTEC) — served as advisors during the joint feasibility study carried out by British Aircraft Corporation and TRW between September 1975 and April 1976.[41] In doing so, they kept the study's engineers and

1974), OOE File, NASA History Office, courtesy of C. Waff.

[38] D. H. Herman to N. W. Hinners re "Telephone Conversation with Dr. George Haskell on January 20, 1975, regarding NASA/ESRO Meeting of February 4 and 5, 1975" (23 Jan. 1975), Galileo File, NASA History Office; and Herman to J. E. Naugle (7 Feb. 1975), SL/Chron. File, NASA History Office, courtesy of C. Waff.

[39] ESRO: Scientific Programme Board, "Eleventh meeting held at Neuilly on 26 March 1975: Draft Minutes," ESRO/PB-S/MIN/11, ESA Archives.

[40] *Proceedings of the Symposium on the Study of the Sun and Interplanetary Medium in Three Dimensions held at Goddard Space Flight Center on May 15-16, 1975*, eds. L. A. Fisk and W. I. Axford (Greenbelt MD: NASA Goddard, 1976) = NASA Pub. X-660-87-53.

[41] For the role of European space scientists in the feasibility study, see ESA, "Out of Ecliptic and Solar Stereo-scopy Mission: Report on the Phase A Study," DP/PS (76) 12, on pp. 74-75, courtesy of R. G.

managers mindful of the mission's scientific *raison d'être*. The result was a fairly detailed, and optimistic, analysis of a dual-spacecraft mission — a larger, spin-stabilized NASA probe with a payload including optical instruments and a smaller ESA probe with a payload consisting mainly of particles-and-fields instruments — that would commence with a shuttle launch into earth orbit and then proceed to rocket launches from earth orbit toward Jupiter followed by Jovian swingbys to send the spacecraft on separate trajectories above the sun's north and south poles.[42]

In 1973, to sum up, a favorable environment for an out-of-ecliptic mission was created by the space agencies' decision to promote cooperative endeavors more vigorously than heretofore and by *Pioneer 10*'s untroubled Jovian flyby. During the mid-1970s, consequently, the leaders of the European campaign for an out-of-ecliptic mission (Elliot, Quenby, Wibberenz, and Giese) had little difficulty recruiting other leaders (Hedgecock, Steinberg, Page, Axford, and Wenzel) and, with their aid, a sizable following. The mission's growing European constituency allied itself with American space scientists and space-science planners to secure joint definition and feasibility studies. As these studies progressed, the Europeans who favored an inclined-orbit mission were outmaneuvered by the Americans who favored a Jovian-swingby mission. Nonetheless, virtually all of the mission's European advocates rallied to Axford's dual-spacecraft version of this swingby option by the time of the feasibility study's completion in April 1976.

Obtaining commitments for the international solar-polar mission, 1976-79

As the mission's constituency became larger and more diverse during the mid-1970s, those European space scientists who were active in the campaign found that their roles were increasingly limited. This trend continued during the three years extending from the completion of the feasibility study to the signing of the ESA/NASA memorandum of understanding for the mission. To be sure, their steadfast support was indispensable for the mission's ultimate authorization. But it was not sufficient, even when combined with that from the mission's proponents among ESA science planners and American space scientists. To secure approval for such an ambitious mission required determined support from NASA Headquarters. Through 1975, however, such support was absent because the Planetary Division, which was officially responsible for the joint studies of the out-of-ecliptic mission, was preoccupied with trying to satisfy the demands of its primary

Marsden.

42 *Ibid.*; and TRW Systems Group and NASA: Ames Research Center, "Jupiter-out-of-Ecliptic Mission: Final Report" (7 Apr. 1976), 27059-6007-RU-00, courtesy of L. A. Manning. It was in the summer of 1975, just as the feasibility study was commencing, that NASA decided to use the shuttle rather than Titan rockets to get the out-of-ecliptic mission started — see, for instance, D. E. Fink, "Scientists Studying Shuttle Application, Payload Plans," *Aviation Week and Space Technology*, 103 (1975), 20-21; and P. Barnett, "Joint NASA/ESA Out of Ecliptic Mission Utilizing the NASA Space Transportation System: Presented to European Space Agency September 11, 1975," Jet Propulsion Laboratory #662-6.

constituency. In early 1976, the mission finally attracted a champion among NASA's science planners — Harold Glaser, the first director of the Solar-Terrestrial Division that NASA had just established so as to protect its flanks from the increasingly vociferous and powerful environmental movement. Glaser leapt at the chance to assume responsibility for the mission as soon as the joint feasibility study was completed. He anticipated that the mission would contribute substantially not only to his division's scientific program but also to its overall budget and viability.[43] The upshot was that NASA's Solar-Terrestrial Division emerged in April 1976 as the lead actor in the campaign for space-agency commitments to the out-of-ecliptic mission.

Over the next year, European space scientists contributed to this campaign in two chief ways. One way was that a few of the leading spokesmen for the mission served as scientific liaisons between ESA and NASA. In April 1976, for instance, Ian Axford attended a preliminary meeting of what later became the Solar-Terrestrial Division's Out-of-Ecliptic Science Working Group. He provided Glaser and the others present with the European perspective on the mission. According to Glaser's notes, Axford emphasized ESA's tight schedule for making decisions, ESA's insistence on "clean interfaces" between the two agencies, the importance in ESA's approval process of choosing principal investigators form several European countries, ESA's interest in the observations made during the Jovian swingby, ESA's conviction that two spacecraft were scientifically essential, and ESA's desire that each probe spend as long as possible above 50° heliographic latitude.[44] Two months later, Edgar Page participated in a high level ESA/NASA meeting at which tentative agreement was reached on five points: (1) a joint announcement of opportunity would be released in early 1977; (2) a joint screening committee would nominate the principal investigators; (3) a joint science working team would be formed for the development phase; (4) NASA would seek new-start status for the mission in FY 1978; and (5) the two agencies would ultimately enter into a memorandum of understanding for the mission.[45] And from August 1975 through January 1976, Ian Axford, Jean-Louis Steinberg, and Klaus-Peter Wenzel variously attended meetings of the Out-of-Ecliptic Science Working Group as ESA observers, helping this advisory body update the mission's rationale and draft the scientific parts of the announcement of

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[43] My account of H. Glaser's emergence as the champion for the out-of-ecliptic mission at NASA Headquarters is based on interviews with him during 1987-88 and his NASA Record Notebook II (entries for 26, 30 Jan., 6 Feb. 1976).

[44] Glaser, *ibid.* (entry for 16 Apr. 1976). That Glaser's notes on this meeting focused on Axford's remarks reflected his awareness that NASA's top planners were unwilling to consider the mission without ESA's participation — see *ibid.*, (entry for 10 Mar. 1976).

[45] *Ibid.* (entry for 15 June 1976, including A. Lebeau to N. W. Hinners (6 June 1976)). In agreeing to proceed with a joint AO and joint experiment selection prior to mission approval, ESA and NASA were following a precedent worked out in 1971 during the planning process for what became the ISEE mission — see Russo (n. 13), pp. 38-39, 43.

opportunity.[46]

The other main way in which European space scientists contributed to the campaign for the out-of-ecliptic mission during the year following the joint feasibility study was through a series of votes in ESA's severe winnowing process. In early July 1976, ESA's Science Advisory Committee (SAC) considered three missions involving cooperation with NASA, giving the Space Telescope its highest priority, the out-of-ecliptic mission second priority, and an infra-red telescope on Spacelab third priority.[47] That summer, national committees and caucuses reviewed SAC's report and voted on instructions for their delegates to ESA's Science Programme Committee (SPC). For example, British space scientists discussed ESA's program and priorities in a day-long meeting at the Royal Society. Like SAC, they gave their highest priority to the Space Telescope and second priority to the out-of-ecliptic mission.[48] Then in early October, the SPC took up the issue of ESA/NASA cooperative projects. Although most of the delegations spoke in favor of both the Space Telescope and the out-of-ecliptic mission, there was considerable dismay that NASA had just postponed the latter's launch from 1981 to 1983 on account of difficulties in the Shuttle program. In the end, Harry Elliot along with the other delegates decided that prudence dictated a wait-and-see approach. Accordingly, they authorized ESA's managers to continue working with NASA on the out-of-ecliptic mission until the SPB could again consider the matter the following spring.[49]

In April 1977, some twelve years after Biermann first discussed what instruments might profitably be sent to high heliographic latitudes, ESA and NASA issued the announcement of opportunity for the out-of-ecliptic mission. It encouraged space scientists in Europe and the United States to propose payload components for either or both spacecraft or interpretive investigations. Twenty-six European space scientists were among the 78 principal investigators who managed to get proposals in by the August deadline. While they, their collaborators, and their American counterparts awaited the outcome, separate ESA and NASA science-screening committees independently evaluated all the proposals. Then in November a joint ESA/NASA scientific

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46 H. Glaser to N. W. Hinners (4 Apr. 1977), Harold Glaser NASA Papers. For what Glaser called the group's "advocacy document," see JPL-NASA Out-of-Ecliptic Science Working Group, "The NASA/ESA Dual Spacecraft Out-of-Ecliptic Mission: A Scientific Summary" (20 Apr. 1977), Jet Propulsion Laboratory #660-53.

47 A. Lebeau to N. W. Hinners (21 July 1976), Harold Glaser NASA Papers.

48 D. W. Hughes, "Britain in European Space," *Nature*, 262 (1976), 538-40.

49 ESA: Scientific Programme Committee, "Sixième Réunion tenue à Neuilly les 4 et 5 Octobre 1976: Projet de procès-verbal," ESA/SPC/MIN/6, ESA Archives; and ESA, "Two New Scientific Projects for European Space Agency," ESA News Release (6 Oct. 1976), OOE File, NASA History Office.

committee reached agreement on strawman payloads for both probes.[50] ESA officials immediately presented the tentative results to the Solar System Working Group and Science Advisory Committee. Soon the grapevine carried the news throughout the European and American space-science communities, triggering protests from some disappointed proposers and, as a consequence, a stiff NASA request to ESA to do a better job of maintaining confidentiality.[51] A final round of ESA/NASA negotiations generated the list of successful proposals in mid-February 1978.[52]

European space scientists were well represented among the principal investigators chosen for the out-of-ecliptic mission. They were principal (or co-principal) investigators for almost half of the 27 successful proposals — five of the eight experiments selected for the ESA spacecraft, four of the eleven experiments selected for the NASA spacecraft, one of the two gravity-wave experiments, and three of the six interdisciplinary and theoretical projects (see Table 2, at end). The relative objectivity of the selection process is suggested by two facts about the twelve European PI's. First, although three — Axford, Giese, and Hedgecock — had been prominent in the campaign for an out-of-ecliptic mission, the desire to recognize such activists was not so great as prevent the selection of nine PI's having modest or no connections with this campaign. Second, although the twelve successful proposers worked in six different European countries, the concern about equal national representation was not so great as to prevent the selection of six PI's from Germany and two from Italy.

All the while during the proposal-evaluation process, NASA and ESA science planners and managers were finally making firm commitments to the out-of-ecliptic mission. In August 1977, just as the proposals were coming in, Glaser managed to persuade his NASA superiors that the agency should, despite its funding problems, seek Congressional approval. He did so by emphasizing that the space-science community strongly supported the mission, stressing that ESA would not tolerate further delays, promising that NASA's share of the mission could be kept below $250 million, and by agreeing that the mission's name should be changed to the Solar Polar Mission so that NASA's new Administrator Robert A. Frosch would not be obliged to explain the

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[50] N. W. Hinners described the proposal and review process to a Congressional committee on 2 Feb. 1978 — see Ninety-fifth Congress: U.S. House of Representatives: Committee on Science and Technology: Subcommittee on Space Science and Applications, *1979 NASA Authorization Hearings*, vol. I: part 2 (Washington: U.S. Government Printing Office, 1978), p. 1393. According to Martin Huber, who was a member of ESA's science-screening committee, the other members included Mayo Greenberg, George Haskell, Michelle Petrie, Livio Scarsi, H. J. Völk, and Peter Willmore — conversation, 7 Nov. 1992.

[51] H. Glaser, draft of telegram from N. W. Hinners to A. Lebeau with copies to E. Page and E. A. Trendelenburg (5 Dec. 1977), Harold Glaser NASA Papers.

[52] Glaser was provided with a list of the mission's payloads and principal investigators around 23 Feb. 1978 — see Glaser, NASA Record Notebook V (sheets pasted between entries for 22 and 24 Feb. 1978).

ecliptic to Congress.[53] In November, ESA's Science Programme Committee followed up on NASA's decision by approving, despite some irritation about the unilateral name change, the Solar Polar Mission.[54] A month later, NASA secured permission from the Office of Management and Budget to seek authorization for the mission by arguing that this would be the agency's only new start in 1978 and that $13 million would suffice for the initial budget.[55]

The fight for Congressional approval began in January 1978. Edward Boland, the powerful chair of the House subcommittee overseeing NASA appropriations, insisted that the funding needs of the beleaguered Shuttle program precluded giving more than $5 million to the Solar Polar Mission. Glaser and his allies in the American space-science community feared that such a low start-up budget would kill the mission by forcing another launch delay which, in turn, would induce the Europeans to pull out. They mounted an intense lobbying effort that was finally successful in mid-September. With President Jimmy Carter's signature shortly afterwards, the Solar Polar Mission was, at long last, approved.[56]

European space scientists and ESA managers were virtually sidelined throughout the protracted struggle in the United States over the Solar Polar Mission. This experience increased their resolve that the NASA/ESA Memorandum of Understanding (MOU) for the mission would assure mutual consent in all subsequent decisions of consequence. As drafted by the two agencies' officials, then recommended by the Science Programme Committee (14 Nov. 1978), and finally approved by the ESA Council (12-13 Dec. 1978),[57] the MOU addressed the issue of mutuality time and again. For example, the MOU provided for a joint Science Working Team that would be

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53 This account of Glaser's persuasion of Hinners and then Frosch to back the mission is based on interviews with him in Aug. 1987; his NASA Record Notebook IV (entries for 4, 5, 8, 16, 20 Aug. 1977); and *op. cit.* (n. 50), p. 1548.

54 ESA: Science Programme Committee, "Fourteenth meeting ... on 8 November 1977," ESA/SPC/MIN/14 (6 Dec. 1977), ESA Archives. The vote was hardly a resounding victory for those who favored the out-of-ecliptic and GEOS-2 missions, whose fate had been coupled by ESA's Directorate in order to assure a majority: 5 national delegations were in favor (Denmark, Germany, the Netherlands, Sweden, and Switzerland), 2 abstained because they lacked instructions (Italy and the United Kingdom), and 3 abstained by instruction (Belgium, France, and Spain). Various participants at the Palermo Symposium told me of the annoyance at NASA's high-handed change in the mission's name.

55 Glaser NASA Record Notebook (entry for 5 Dec. 1977).

56 This short account of the complex fight to win Congressional approval is based on numerous items in Glaser's NASA Record Notebook V; the *Aerospace Daily;* and *Aviation Week and Space Technology.*

57 ESA: Council, "Draft Memorandum of Understanding between NASA and ESA on the International Solar/Polar Mission," ESA/C(78)145 (27 Nov. 1978), and "Twenty-eighth meeting ... on 12 and 13 December 1978: Draft Minutes," ESA/C/MIN/28 (26 Jan. 1979; trans. from French 8 Feb. 1979), both in ESA Archives.

chaired by the two agencies' Project Scientists and composed of all the principal investigators. This team would be responsible for helping the two agencies' Project Managers and the Joint Working Group achieve the mission's scientific objectives, coordinating exchange of data among all the investigators, and facilitating the use of the data by the larger interplanetary, solar, and Jovian communities. Its meetings would occur "at appropriate intervals during the lifetime of the mission" and "be scheduled equally in the U.S. and Europe." These and like measures engendered hopes that the MOU would foster a genuine partnership both in the preparations for and conduct of the mission. Bearing such hopes, ESA's Director General Roy Gibson went to Washington in late March 1979. There he joined with Frosch in signing the MOU.[58] In doing so, Gibson and Frosch formally transformed the Solar Polar Mission into the International Solar Polar Mission (ISPM) and, it then seemed, expressed their respective agencies' firm commitments to launching the dual-spacecraft, out-of-ecliptic mission in 1983.

Although, as things turned out, NASA's commitments to this plan were not at all firm, the ISPM MOU was the major turning point in the background history of what eventually became *Ulysses*. Before the MOU, those European space scientists who wanted the mission were busy promoting its scientific advantages, building its constituency, lobbying for favorable committee votes and administrative decisions, and preparing investigative proposals. Afterwards, although circumstances forced the resumption of such activities more than once, those European space scientists who were still or newly associated with the mission were mainly oriented to payload development/maintenance and to operations planning. The MOU marked, that is, both the culmination of an increasingly complex campaign on behalf of an out-of-ecliptic mission and the inauguration of the detailed technical preparations required by this mission.

Conclusion

European space scientists were essential participants in the long campaign for a mission to high heliographic latitudes that was initiated by Biermann's 1965 article and Elliot's 1969 committee report. In the early 1970s, those who liked the idea persuaded ESRO first that an out-of-ecliptic mission merited a definition study and then that it deserved consideration for an ESRO/NASA cooperative venture. In the mid-1970s, their involvement in joint conferences and studies assured the formation of a sufficiently broad mission constituency for ESA and NASA to begin taking the matter seriously. And finally in the late 1970s, their continued interest in the mission — as expressed by the number of their investigative proposals and by their insistence, through ESA, on an early launch — gave NASA's science planners a crucial argument in their difficult struggle to win first the agency's and then Congress's approval.

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[58] "G12. Memorandum of Understanding between NASA and ESA on the International Solar/Polar Mission" (29 Mar. 1979), courtesy of R. Lüst.

Yet, while European space scientists were always essential, their relative importance necessarily diminished as other essential groups — American space scientists, ESA science planners, and eventually NASA science planners — joined into the campaign. Indeed, none of these groups can be given more than modest credit for the creation of this coalition. It was *Pioneer 10* 's Jovian flyby in late 1973 that convinced space scientists and planners that there was a much cheaper and more reliable way than ion propulsion for getting a spacecraft out of the ecliptic plane. It was NASA's post-Apollo budget difficulties in the mid-1970s that made the American agency more eager than before to enter into cooperative ventures with ESRO. It was NASA's way of responding to the burgeoning environmental movement that led to the emergence in 1976 of a Headquarter's champion for the out-of-ecliptic mission. Without all three of these exogenous developments, the broad-based coalition that won support for the mission would certainly not have come into being during the 1970s. Thus, as is so often the case with Big Science, the campaign for an out-of-ecliptic mission which European space scientists initiated in the 1960s eventually culminated in the NASA/ESA ISPM MOU because the technical, economic, and political contexts were all propititious — for a time at least — for a cooperative, dual-spacecraft, Jovian-swingby, solar-polar mission.

Table 1

Problem areas and model payload proposed for an Out-of-Ecliptic mission by ESRO's Mission-Definition Group, April 1972

Problem areas	*Payload instruments*
• Solar sources and 3-D structure of magnetic fields embedded in the solar wind	• Magnetometer
• Solar sources and latitude dependence of solar-wind plasma properties	• Plasma probe
• Solar acceleration and interplanetary propagation of solar energetic particles	• Energetic-proton detector (0.1-10 MeV) • Energetic-electron detector (up to 40 keV)
• Interplanetary modulation of galactic cosmic rays	• Cosmic-ray detector
• Mechanism underlying the close association of solar hard X-ray bursts with radio bursts	• Solar X-ray detector (1-10 keV)
• Distribution of interplanetary dust particles	• Dust detector • Zodiacal-light photometer

Source: See n. 16.

Table 2

Successful proposals announced for the Solar Polar Mission, February 1978*

NASA spacecraft	*ESA spacecraft*
Structure/Evolution of the solar corona	
• White Light Coronagraph & X-Ray/XUV Telescope R. M. MacQueen/HAO, U.S.A.	
Solar X-rays & cosmic gamma-ray bursts	
• Solar X-Ray Flare & Cosmic Gamma-Ray Gamma-Burst Monitor T. L. Cline/GSFC, U.S.A.	• Solar Flare X-Radiation, Cosmic Ray Bursts & Jovian X-Rays K. C. Hurley/CSR, France
Interplanetary magnetic fields	
• Magnetic Field Experiment M. H. Acuna/GSFC, U.S.A.	• Magnetic Field Measurements in Heliosphere P. C. Hedgecock/Imperial College, Eng. E. J. Smith/JPL, U.S.A.
Cosmic rays	
• Energetic Particles Investigation E. C. Stone/Caltech., U.S.A.	• Cosmic Ray & Solar Charged Particle Investigations J. A. Simpson/U. Chicago, U.S.A. H. Kunow/U. Kiel, Germany
Solar wind & ion composition	
• Mass Separating Solar Wind Experiment H. Rosenbauer/MPI, Germany	• Plasma Spectrometer Experiment S. J. Bame/Los Alamos, U.S.A. • Solar Wind Ion Studies G. Gloeckler/U. Maryland, U.S.A. J. Geiss/U. Bern, Switzerland
Plasma waves & solar radio astronomy	
• Unified Radio & Plasma Wave Experiment R. G. Stone/GSFC, U.S.A.	• Unified Radio & Plasma Wave Experiment R. G. Stone/GSFC, U.S.A.
Dust, zodiacal light & neutral gas	
• Zodiacal Light Experiment R. H. Giese/U. Bochum, Germany J. L. Weinberg/SUNY Albany, U.S.A. • Fluid Parameters of the Interstellar Gas, using Helium as Tracer H. Rosenbauer/MPI, Germany	• Cosmic Dust Experiment E. Grün/MPI, Germany

(cont.)

Radio science (with spacecraft-to-ground signals)

- Solar Corona Electron Density Distribution
  P. B. Esposito/JPL, U.S.A.
- Changes in Electron Columnar Content
  P. S. Callahan/JPL, U.S.A.
- S/X-Band Faraday Rotation & Dispersion Measurements during Solar Occultation
  H. Volland/U. Bonn, Germany

Both spacecraft

Gravity waves (definition study only)

- Experimental Gravity: Gravitational Radiation and Celestial Mechanics
  H. D. Wahlquist/JPL, U.S.A.
- Detection of Gravitational-Wave Bursts from the Nuclei of Galaxies & Quasars
  B. Bertotti/U. Pavia, Italy

Interdisciplinary and theoretical studies

- Out-of-Ecliptic Interdisciplinary & Theoretical Investigations
  C. P. Sonett/U. Arizona, U.S.A.
- Studies of Cosmic Rays & their Interaction in the Heliosphere & the Galaxy
  J. R. Jokipii/U. Arizona, U.S.A.
- Studies of the Dynamics of the Solar Wind
  A. Barnes/Ames Res. Center, U.S.A.
- Dependence of Mass Flow & Ion Composition of the Solar Wind on Heliographic Latitude
  G. Noci/U. Firenze, Italy
- Discontinuities & Current Sheets in the Solar Wind
  J. Lemaire/Institute for Space Aeronomy, Belgium
- Theoretical Team to Support the Solar Polar Mission
  W. I. Axford/MPI, Germany
  L. A. Fisk/U. New Hampshire, U.S.A.

Source: See n. 52.

* Note: There were several modications in the Solar Polar Mission's Experiments and Studies during the next year — see K.-P. Wenzel, "The scientific objectives of the Internaltion Solar Polar Mission," *Philosophical Transactions of the Royal Society,* A297 (1980), 565-73.

Solar system science for Horizon 2000

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1. Introduction

With the launch of the first mission still two and a half years away, reporting on the history of 'Space Science Horizon 2000' may seem premature. Yet, ESA's Council, meeting at Ministerial Level in Granada in 1992, has already asked the Executive to start planning beyond 'Horizon 2000'. Also, given the coverage of the ESA history study up to 1987, a description of events in the period 1983/4 (that led to 'Horizon 2000', the first scientific long-term programme of the Agency) fits well into the present context. Indeed, a record of the preparations for Horizon 2000 is timely. In particular a critical review of the discussions and recommendations concerning the solar-system space sciences — the more complex and more controversial part of the exercise — is of current interest: similar and related scientific and science-policy choices will have to be made in 1993/4, as the new long-term plan is being elaborated.

We will start this report by reflecting upon the status of solar-system sciences when the long-term plan 'Horizon 2000' was started. The next section therefore presents the composition of the Solar System Working Group (SSWG), the activities of the International Solar Terrestrial Physics (ISTP) Joint Working Group as well as the studies and projects under way at the time. The subsequent section is devoted to introducing the 'Survey Committee' as well as the 'Mission Concepts', that had been solicited from the community and were evaluated by the Survey Committee's 'Topical Teams'. The 'Venice Meeting' is the subject of section 4, where we will emphasise the change from three to four 'Cornerstones' and report how the planetary — or, as it turned out, the cometary — Cornerstone was defined. We will also briefly comment on the so-called 'Green Dreams', i.e. the themes chosen for longer-term prospects. In section 5, we will look at the initial implementation of solar-system projects in the framework of 'Horizon 2000', and present our conclusions in section 6.

2. Status of solar system science in ESA before 'Horizon 2000' was initiated

When — towards the end of 1982 — I was asked by the then Director of Scientific Programmes, Dr. Ernst A. Trendelenburg, to chair the Solar System Working Group[1], neither he nor I realised

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[1] The peer group set up to review, and advise on, scientific policy in solar-system space sciences.

that, within less than a year, I would also have to coordinate the solar-system part of Horizon 2000.[2] Guiding the deliberations of the Working Group was already quite difficult, for the group's members came from a wide range of fields, and most of them would claim that ESA had not yet flown a mission relevant to their own field. This was probably not only 'a fault of the system'; the Solar System Working Group had repeatedly raised unrealistic expectations — especially in the field of missions to planets and asteroids.[3]

Moreover, from early 1983 on, after the Infrared Space Observatory (ISO) had been selected as the next project, many space scientists working in solar-system disciplines felt somewhat shortchanged. In their view, astronomers had won another expensive ESA project at a time when they had access to two satellite observatories (IUE, the International Ultraviolet Explorer and, as of June 1983, Exosat) and already had two projects of their own — the Hubble Space Telescope (HST) and the rather costly astrometry satellite Hipparcos — under development. At the same time, there was only one ESA solar system mission (ISEE, the International Sun-Earth Explorer) operated within the mandatory programme (another, Geos-2, was operated under an optional programme), and only two relatively modest missions, as far as cost was concerned — Ulysses and Giotto — were under development.

2.1 The Solar System Working Group (SSWG)

The Solar System Working Group (SSWG) was well-balanced at the time (cf. Table 1): it had 16 members, four of them solar physicists, four space plasma physicists and eight planetary scientists; the latter were subdivided again into two quartets, one with scientists working with solid bodies and the other with experts in planetary atmospheres. The Group had been chaired for several years by Alan H. Gabriel, a senior solar physicist. Their secretary was George P. Haskell, originally a space plasma physicist, who had acquired a remarkable familiarity with the issues and communities of all the disciplines represented in the SSWG, and thus acted with competence and fairness.

Some of the members making up the strong representation of the planetary community were involved in NASA's planetary exploration programmes, be it as Experimenters or Science Team Members. Nevertheless, there was still little hardware experience. There was also a certain amount of competition and disagreement between scientists who promoted the study of different planets. At the time, these divergent tendencies could be held in check thanks to a study which was being completed by a Joint Working Group of the European Science Foundation (ESF) and the

2 Dr. Trendelenburg — in contrast to his successor as of May 1983, Prof. Roger M. Bonnet — took a rather dim view of the chances for a long-term programme.

3 A major reorganisation of the Solar System Working Group (SSWG) had therefore taken place a few years earlier: the Director of Science had assumed the right to appoint half of the Group's members.

U.S. National Academy of Sciences (NAS).[4] Twelve leading planetary scientists from both sides of the Atlantic had, by the end of April 1984, established a priority list of missions to planets and asteroids. This helped to focus the ideas of the planetary community representatives in the Working Group.[5]

The four SSWG Members representing space plasma physics, or more accurately the 'particles and fields' community, had acquired extensive hardware experience during earlier ESRO/ESA missions. In fact, their community had to a large extent started space research in Europe. They formed perhaps the most confident and realistic part of the Working Group.

The solar physicists in the SSWG were in a similar situation as the planetary scientists: no solar physics mission had been flown yet within the ESRO/ESA programmes (Huber 1993). In this instance, however, there were several solar physicists who had gained extensive experience in building hardware for sounding rockets flown within national programmes, and for experiments flown on NASA satellites. In addition, some of the solar physicists in the Working Group had participated in scientific operations of NASA missions.

Most SSWG members had good connections to US scientists and, of course, knew their European colleagues very well. Many of them had spent some time abroad, within Europe and overseas.[6]

2.2 The Joint Planning Group for the International Solar-Terrestrial Physics Programme (ISTP)

After he became Director of Science, Prof. Bonnet regularly met with the Associate NASA Administrator for Space Science and Applications (then Dr. Burton I. Edelson). Key staff members from ESA and NASA also attended these meetings, which were held once or twice a year. During the first such meeting, on 27-29 June 1983 in Paris, it was agreed that a need existed to take an integrated look at the large number of missions under study in the USA, Europe and Japan in the area of solar terrestrial physics. NASA and ESA therefore set up a preparatory meeting, to which ISAS (the Japanese Institute for Space and Astronautical Science) was invited (see Annex 1 of

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4 Co-chaired by Prof. Hugo Fechtig and Dr. Eugene H. Levy.

5 The selection of the Cassini/Huygens mission to Saturn and its moon Titan, by ESA and NASA is a concrete result of the earlier deliberations of the ESF/NAS Joint Working Group (JWG). The choice of a Mars Rover as theme of a 'Green Dream' also had its roots in the priority list arrived at by the JWG (Space Science Board (NRC/NAS) and Space Science Committee (ESF) 1986).

6 A workshop on 'Solar and Space Plasma Physics', jointly organised by the Space Science Committee of ESF (chair: J. Geiss) and the Space Science Board of NRS/NAS (chair: T. Donahue) on Hilton Head Island in 1983 had highlighted the importance of the field as well as the maturity and capabilities of the partners (Geiss 1993, cf. SSAC 1983).

SSWG 1983). This preparatory trilateral meeting on Solar-Terrestrial Science took place in Washington on 26 and 27 September 1983.[7]

After extensive discussion and a rather painful rationalisation process, the 'International Solar Terrestrial Physics (ISTP) Programme', as shown in Figure 1, was formulated. It contained a reduced version of the previous 'Open' programme, now consisting of four spacecraft - 'Wind' (measuring the solar-wind near Lagrange point L1), 'Equator' and 'Polar' (in respective near-Earth orbits) and 'Geotail', and as 'add-on's' Cluster and Soho. It was argued[8] that Soho and Cluster should be flown together: they both were addressing the same physical structures and processes by remotely sensing the coronal plasma, by in-situ measurements of the solar wind and by in-situ investigations in three dimensions of the magnetospheric plasma! It was also pointed out, however, that the two missions had only just completed their Assessment studies and — given the usual ESA selection process — that it would be almost impossible to select both Cluster and Soho as projects. Nonetheless both SSWG and SSAC endorsed the idea of the ISTP Programme and took account of this in their recommendations made for the selection of Phase-A studies at their October 1983 meetings (cf. section 2.3. below).

Some of the scientists who had helped compose the ISTP Programme at the preparatory meeting in Washington were asked to form a Joint Planning Group for ISTP. This Group met for the first time in June 1984 in Paris. The first agreement taking shape was the bilateral ISAS/NASA collaboration which eventually resulted in the launch of Geotail in July 1992. The origins of the ESA/NASA collaboration on the Solar Terrestrial Science Programme, which later became the first Cornerstone of 'Horizon 2000', can also be traced to the Joint Planning Group, particularly since their interrelation was declared desirable for the first time at the preparatory trilateral ISTP meeting of September 1983.[9]

2.3 Studies in solar system space sciences

For a review of the studies in all areas of solar-system space sciences we must now return to early 1983. Two Phase-A Studies for solar-system missions had just been completed: Disco (a helioseismology mission) and Kepler (a mission with the aim of investigating Mars, and in particular Martian aeronomy). In the selection of a new project, which took place at Scheveningen in February 1983, Disco and Kepler were thus the candidate missions in the field of Solar System

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7 Dr. Stan Shawhan, who had just joined NASA, and was on leave from the University of Iowa, chaired that meeting. It is fair to say that the participants of the preparatory meeting preceived him as the 'spiritus rector' of the ISTP Programme.

8 By Dr. Gerhard Haerendel.

9 The Phase-A Studies of Cluster and Soho were already joint ESA/NASA studies at that time: both had NASA sponsored participants in their Science Study Teams.

Sciences. The SSWG had preferred Disco over Kepler. However, the Space Science Advisory Committee (SSAC) which had to make the final choice recommended the selection of an Infrared Space Observatory, ISO, i.e. the project put forward by the Astronomy Working Group (AWG) rather than that advocated by the SSWG (namely Disco). The selection of ISO was approved by the Scientific Programme Committee at the end of March 1983 and this then led - as already indicated - to a certain uneasiness among the solar-system community. They felt that their missions were not only less expensive than those of astronomers, they now also had fewer missions under development.[10]

Another decision point came in October 1983, when new Phase-A Studies had to be chosen. A presentation of Assessment Studies took place in Frascati, followed by meetings of both Working Groups as well as the Space Science Advisory Committee. They had to formulate the priorities, advising the Director of Science on which of the missions coming out of Assessment Studies should be subjected to the Phase-A Studies. Candidate missions in the solar-system area were Agora (Asteroid Gravity, Optical and Radar Analysis), Cluster[11], Saturn/Titan Orbiter/Probe and Soho.[12] The Solar System Working Group recommended that Phase-A Studies be performed on Cluster and Soho. In addition, the SSWG asked that the Phase-A Study for Kepler[13] be updated in time for the selection of the next project, which was then planned for mid-1985. Proponents of the previously studied Polar Lunar Orbiter, Polo, wanted the same treatment for this mission; the SSWG however explicitly excluded Polo from a Phase-A or an Assessment Study. Further, the SSWG recommended re-assessments of both the Saturn/Titan and Agora missions in joint studies with NASA. It was requested that the latter study be based on an ion-propulsion system.

The Space Science Advisory Committee endorsed these recommendations and (following a suggestion from the SSWG) wrote: "To maintain open the possibility of including these (scil. Cluster and Soho) missions in the International Solar Terrestrial Physics Programme with NASA

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[10] Before, it had been customary to maintain the balance between solar-system and astrophysical sciences by making a simultaneous or near-simultaneous selection of one mission each for astronomy and for solar system sciences.

[11] Four-spacecraft multi-point mission in an eccentric near- polar orbit for studying small-scale plasma processes in three dimensions (Schmidt 1988).

[12] Soho had by that time changed its full name from Solar High-resolution Observatory to Solar and Heliospheric Observatory: previously a solar-coronal and fields-and-particles investigation in a low-Earth orbit, it now comprised a payload complement devoted to helioseismology, and was going to be placed in a halo orbit around Lagrange point L1, where it could measure the solar wind. Thus it could, in principle, also fulfil the scientific aims of Disco (Domingo 1989, see also Huber and Malinovsky 1992).

[13] A Mars orbiter with scientific objectives embracing the disciplines of planetary interiors, geophysics, geology, climatology, atmospheric physics, aeronomy and the particle and field environment of Mars (Haskell 1982).

and Japan," (cf. section 2.2 above), "the two missions should be compatible with Ariane and the Shuttle and these two options considered in the Phase-A study" (SSAC 1983). In putting forward the Saturn/Titan and the Agora missions for a re-assessment, the SSAC noted that their "recommendations regarding planetary sciences were in agreement with the preliminary results of the joint ESF/NAS Joint Working Group" (cf. section 2.1). The recommendations resulting from the October 1983 SSAC meeting are well summarised in Figure 2, which shows the general schedule for the Phase-A studies and the joint ESA/NASA Re-assessment Studies (as well as preparatory studies leading to technical study phases with subsequent Phase-A studies for two major proposed astronomy missions, XMM and First).

The following incident may serve to illustrate the then perceived bias of ESA's Scientific Programmes towards astronomy. At that same October 1983 meeting, the Space Science Advisory Committee also had to formulate a recommendation regarding the flight of solar system and astronomy experiments on Eureca. The Director of Space Transportation Systems, Mr. Bignier, had offered flight opportunities on the Eureca platform (the European Retrievable Carrier, designed primarily to carry microgravity experiments): several experiments coming out of the community that is normally served by the Scientific Programme would be included on the platform.[14] When the proposals put forward by the two Working Groups (SSWG and AWG) were discussed in the SSAC, the chair of SSWG emphasised that the Eureca platform was especially suited for solar system experiments. He argued that in view of the lack of flight opportunities for solar system sciences, only experiments from these disciplines should be included in the Eureca payload. A 'tour de table' seemed to result in unanimity — until the chair of AWG[15] (very appropriately) protested against this argument, because there had been a solicitation for other experiments as well (SSAC 1983). A hard X-ray survey-instrument was then included among the experiments recommended for flight. The point about the imbalance was however not lost!

3. The Survey Committee

The Survey Committee (cf. Table 2) was established in 1983 to advise the Director of Scientific Programmes during the new long-term planning exercise. The Committee consisted of the Space Science Advisory Committee, the chairman of its political authority, namely the Scientific Programme Committee, and the chair of ESA's Earth Observation Advisory Committee. Further

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[14] The Announcement of Opportunity (AO) for these flights, as well as the earlier one for the First Spacecraft Payload (FSLP) were the only ones which so far have been consummated. An AO for flight of space science experiments on the first Polar Platform (a development under the purview of the Directorate for the Observation of the Earth and its Environment) later turned out to be fruitless. Similarly the call for Space Station Precursor Missions has not yet resulted in any concrete prospects for flight opportunities for solar system science nor for astronomy.

[15] Dr. Johan Bleeker.

members of the Survey Committee represented relevant international foundations, scientific organisations and unions.

The Survey Committee's first meeting was chaired by the outgoing SSAC Chairman, Prof. Amaldi. Subsequently, Dr. Johan Bleeker, the outgoing chair of the AWG, took over the chairmanship of the SSAC and Prof. Franco Pacini became the new chairman of the Astronomy Working Group. The past chairman of the Solar System Working Group, A.H. Gabriel (who had been succeeded by the author) remained in the Survey Committee as a former SSAC member. The secretary of the Survey Committee was Dr. Vittorio Manno, a veteran of ESA, who knew the Agency, the scientific community and the delegations to the SPC very well. Given his experience as closest collaborator of the previous as well as the current Director of Science, Dr. Manno also provided a highly useful 'corporate memory' to the Survey Committee.

At the first meeting of the Survey Committee, a snap decision was made to keep the Sun under the purview of the SSWG. The question, which was raised by Prof. de Jager, reflected the fact that the current system had not yet led to a solar-physics mission within ESA and that other organisations, like CNES and NASA, where the Sun 'belonged' to the equivalent of the Astronomy Working Group, had strongly supported solar physics missions. It turned out to be a more crucial decision than realised at the time: it facilitated the insertion of the first Cornerstone, namely the Solar Terrestrial Science Programme, into Horizon 2000, and thus enabled ESA to again bring together the magnetospheric and solar communities.[16]

3.1 Working methods

The work of the Survey Committee was initiated when, in September 1983, the Director of Science, Prof. R.M. Bonnet, sent a letter to the wide community, in which he announced his intention to elaborate a long-term plan for his Programme, and asked the European Scientific Community interested in space research to submit 'Mission Concepts', i.e. very brief descriptions of potential missions they wanted to see flown by ESA.

The 'Mission Concepts' submitted in response to this call (together with the studies already underway) formed the basis of the Survey Committee's deliberations. A summary of the response is given in Table 3. About the same number of concepts for solar system space science and space astronomy had been received. The concepts concerning the disciplines within the competence of the SSWG could be assigned about one third each to the fields of solar and heliospheric physics, of magnetospheric and space-plasma physics as well as of planetary sciences. To analyse these

[16] Close links had existed until the fifties between these two communities. It is interesting to note that in the present organisational structure of NASA, the Space Physics Division pursues the same aim of enhancing the dialogue between magnetospheric and solar plasma physicists.

mission concepts, the Survey Committee established Topical Teams for the above three disciplines.

3.2 Topical teams and model programmes

The composition of the Topical Teams is given in Table 4. Each of the three Teams generated a model programme that extended from the mid- eighties to ca. 2010. The model programmes were based on an analysis of mission concepts and, of course, had to fit into the international context. The long-term programmes are shown in Figure 3.

In the field of astronomy, the Topical Teams were replaced by a core team and a technical support team, the latter consisting of four experts who covered the various domains extending from the gamma-ray region to the radio part of the spectrum. This reflected the fact that astronomy missions are almost exclusively determined by energy-specific observing techniques: a modern space observatory may be pointed at nearly all the objects in the sky (with thermal and other constraints usually leading to the exclusion of only the Sun and possibly Earth and Moon). In contrast, in solar-system space sciences, the widely different experimental approaches ranging from remote sensing over in-situ exploration and measurement, to sample returns, required the involvement — and coordination — of a much larger number of experts.

The three Topical Teams — like the discipline groups in the SSWG (cf. section 2.1) — had quite different outlooks: while the solar and heliospheric team and the team for space plasma physics considered their topics to be mature subjects, with more in-depth studies being required, the planetary panel put their accent on exploration. The plasma physics team also stressed the importance of numerical simulations as a third element between theory and observation. The planetary team drew attention to the need of synoptic observations of solar-system objects from space observatories as an important complement to in-situ exploration and close-up viewing.

After the Solar System Working Group had heard, at their October 1983 meeting, a report on the progress in long-term planning, some members expressed concern about the balance of solar-system disciplines in view of the three Topical Teams. This concern was not warranted, as the Teams had a very broad outlook: the Topical Team on space plasma physics, for example, supported not only multi-point measurements but also the exploration of magnetospheres of other planets and the study of solar plasmas. The final outcome also showed that it was reasonable to keep the planetary community within one Topical Team, under one very able chairman.[17]

After the Topical Teams had produced their draft plans and the accompanying text for the Survey Committee Report, a meeting with all members of the Topical Teams was held at the Kulm Hotel in St. Moritz in March 1984. During the three days of this meeting, the final plans and texts

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[17] Prof. Siegfried J. Bauer.

were elaborated and the conclusions (which are briefly summarised in the three following paragraphs) were discussed in a final plenary session with the members of all three Teams present.

The Topical Team on Solar and Heliospheric Physics concluded that "a vigorous solar and heliospheric research programme commensurate with the capability, vitality and needs of the corresponding European community should be given strong support" and that "the Soho mission... would provide a major support for the International Solar Terrestrial Physics programme" (Delache and Hoyng et al. 1984). They also invented a synoptic array consisting of an observatory in Lagrange point L1 (i.e. Soho), a 'Solar Occulter' placed 'behind the Sun' on the Earth orbit 180° away from the Earth and two additional 'Stereo' observatories located ± 90° from the Earth. A Heliosynchronous Out-of- Ecliptic Mission, a Solar Probe as well as free-flyers in Earth orbit were further components of their model programme (see also Huber 1993).

The Space-Plasma Topical Team stated that "of the missions under study, Soho and Cluster (were) the ones of most interest to the plasma physicist" (Haerendel 1984). They added: "Although each of them can stand on its own, much is to be gained if they are carried out as a European contribution to the International Solar-Terrestrial Physics programme...." In a table entitled 'Summary of Some Potential Programme Elements after Soho and Cluster' the Topical team gave two lists: one containing mainly exploratory missions (including solar missions and missions to inner and outer planets) and the other with process-oriented missions (i.e., studies of small-scale structures by multiple spacecraft and active experiments like tethered, multi- diagnostic probes and VHF radar).

In the end, the planetary model programme was the most comprehensive, as it tried to include virtually all the objects orbiting the Sun. The Team's recommendation, however, already contained a hint at what could eventually be selected as a Cornerstone in the final 'Horizon 2000' programme: "In view of the Giotto effort, a follow-up rendezvous mission to a short-period comet is particularly appealing to the European planetary and space-science community" (Bauer et al. 1984).[18]

The model long-term programmes were directly submitted to the Survey Committee without involving the SSWG. (Similarly, in astronomy, the long-term programme covering all the

[18] It is worthwhile mentioning here that there had been a disagreement between the Executive and the SSWG in February 1984 (SSWG 1984, SSAC 1984, SPC 1984). During their meeting of 13-14 February 1984 at NASA Headquarters, ESA and NASA had agreed — pending the concurrence of ESA's advisory committees — "to jointly undertake a broadly based exploratory study of primitive body mission opportunities utilizing electrical propulsion", including "Agora, a Comet Nucleus Sample Return mission,..." The SSWG in its subsequent recommendations, however, always stressed that this study be quite specifically for Agora - an unfortunate insistence, as NASA in 1985 stopped supporting the Agora study (SSAC 1985, SSWG 1985). The Survey Committee, which eventually recommended a Comet Nucleus Sample Return as the scientific aim for a planetary cornerstone thus extended its vision far beyond that of the SSWG, but also somewhat beyond that of the Topical Team on Planetary Science.

wavelength domains went from the Core Group to the Survey Committee without being re-evaluated by the AWG.) The synthesis of the various model programmes into one consolidated long-term plan — later to be called the 'Horizon 2000' programme — was the task of the Survey Committee at their Venice meeting, and to this we will now turn.

4. Venice

The meeting of the Survey Committee, which was held at the Fondazione Cini on the Isola San Giorgio in Venice, was perceived by all participants as being the culmination of the entire long-term planning exercise. Everybody realised the high stakes: several billion accounting units worth of scientific missions were going to be discussed and a significant part of this was going to be locked into Cornerstones — and their topics would be decided in Venice!

The basic tenet for a Cornerstone — the need for long-term technological developments — was obvious in X-ray and infrared/sub-mm astronomy and almost naturally led to the X-ray multi-mirror mission (XMM) and to the far-infrared space telescope (First) as Cornerstones.[19] In fact, however, Cornerstones were not defined as missions, but as scientific goals; XMM and First were therefore circumscribed as 'High Throughput X-ray Mission (for Spectroscopic Studies between 0.1 and 20 keV)' and 'High Throughput Heterodyne Spectroscopy Mission', respectively (Bleeker et al. 1984).

In contrast to astronomy, only one Cornerstone was foreseen for the solar system community at the beginning of the Venice meeting: this was thought to be a major planetary mission. There was, however, one principal concern: how to satisfy the planetary community with a mission to one specific planet? It was felt (and feared) by several Survey Committee members that considerable opposition would develop among the advocates of missions to other planets.

Given the leadership of Europe in cometary research, as exemplified by the (then still anticipated!) Giotto mission, and in view of the vigorous community of European laboratory investigators who were working on meteorites and lunar samples, it was decided to define 'A Mission to Primitive Bodies including Return of Pristine Materials' as the theme for the 'planetary' Cornerstone. J. Geiss, W. Fechtig, A.H. Gabriel, H.J. Völk and the author were perhaps those most intimately involved in the formulation of this subject. Although the choice between asteroids and comets was, in principle, left open by the formulation adopted, there was no doubt in the minds of those around a table in a trattoria on the waterfront of the Isola di Giudecca in the evening before the final day of the Venice meeting, that a sample return from a comet (which, it was pointed out by H. Fechtig, even may contain 'stardust') was the actual aim.

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[19] As shown in Figure 2, preparatory studies for these two mission were already underway at the time.

4.1 From three to four cornerstones

The introduction of the other solar-system Cornerstones was somewhat of a surprise. It was originally brought into the plan after a remark by L. van Hove, who questioned why there was only one Cornerstone for the solar system community, although — in his view — they had prepared their model programmes with much more effort and care than the astronomers. The 'Solar-Terrestrial Physics' Cornerstone was then suggested by the chair of the SSWG and this idea drew instant support by K. Fredga[20] and B. Hultqvist. It may well be that it had not been quite clear to all meeting participants what the STP Cornerstone actually was, in particular that it took up G. Haerendel's idea to combine Soho and Cluster in one programme. In any case, the Executive returned next morning, with their original plan of three Cornerstones and thus was immediately criticised by Fredga and Hultqvist and others, who under questioning forced the admission of the feasibility from the point of view of cost and schedule. The STP programme was thus reinserted and quite naturally became the first Cornerstone; it was later renamed the 'Solar-Terrestrial Science Programme (STSP)', in order to make it a distinct element of the much larger ISTP Programme. Regarding the technical requirements for the STSP theme, it was stated that "a special effort will be required to prepare for the operations and data handling of multi-spacecraft missions." At the time this was meant as including not only the four Cluster spacecraft but also Soho; in fact the data question had always been listed as one of the major issues in the International Solar-Terrestrial Physics Programme.

The original Horizon-2000 schedule (Fig. 4) which was included in both documents presenting the conclusions of Venice (Bleeker and Bonnet 1984, Longdon and Olthof 1984) shows three Cornerstones only, but two nearly simultaneous blue missions representing Soho and Cluster, but not labelled as such. In fact, Soho and Cluster are mentioned as blue, i.e. medium-size missions to be selected competitively in the graphical presentation of Horizon 2000 (Fig. 5). As Kepler (the Mars mission) was simultaneously under Phase-A study, and thus formally competing with the two STSP mission, this was, of course, the correct way of dealing with the situation.

It was realised at the time that both Cornerstones in solar-system science had to be implemented in collaboration with partner agencies. While this was already underway with both Soho and Cluster (where the Phase-A Science Study Teams comprised NASA representatives and scientists from the US community), there were no arrangements for the Comet Nucleus Sample Return Cornerstone yet. (In fact, all efforts to get a partner for his Cornerstone theme have failed up to now, and therefore a redefinition of the theme, presumably excluding a sample return, is underway at the time of writing.)

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[20] I later learned from K. Fredga that she was going to suggest the STP Cornerstone at that same moment.

4.2 The green dreams

The Survey Committee also identified the major thrusts beyond the horizon of 2004 (the then perceived completion date for Horizon 2000). The associated mission concepts are often referred to as the 'green dreams': they were summarised in green boxes on the Horizon 2000 graphics. Most likely they would remain dreams for some time to come, as they were beyond the programme for technical and financial reasons. It was stated at the time that "some of these projects could be brought within the Horizon 2000 programme, if additional funds could be made available in the frame of cooperative projects." In the event, however, the topics for the green dreams have just been studied, to different depths, since.

In the field of solar-terrestrial physics, the Survey Committee included the Solar Probe and the Heliosynchronous Out-of-Ecliptic Mission among the green dreams. These two missions, which had been identified and supported by the Topical Teams on Solar and Heliospheric Physics and on Space Plasma Physics, later led to the amalgamated concept of 'Vulcan',[21] a spacecraft in a highly eccentric, out-of- ecliptic orbit which would approach the Sun to within four solar radii (Hoyng et al 1988). The orbital period of Vulcan would be of the order of three to five months, and the payload would comprise in-situ plasma experiments and a high-accuracy clock for relativistic investigations, aimed at verifying the metric hypothesis of general relativity and a measurement of the effect of gravity on photon propagation. It has also been proposed to include cameras for very high-resolution solar imaging during closest approach. The merits of this technique will, however, have to be weighed against those of interferometric observations from near-Earth space.

For planetary sciences the Mars Rover concept was introduced as a long- term thrust, i.e. as a Green Dream. This corresponded to the long-term aim in future international cooperation that had been identified by the Joint Working Group of the ESF/NAS (cf. section 2.1). A Mars Rover was also among the future projects advocated by the Planetary Topical Team. After an initial study of a Mars Rover mission, ESA later set up a Mars Exploration Study Team, whose task it was to evaluate an ESA participation in an international Mars mission (Chicarro, Scoon and Coradini 1989). One of the options discussed in that report, namely a network of semi-hard landers on Mars, 'Marsnet', was then proposed as an international cooperation for the second medium-size mission within Horizon 2000. At the time of writing, Marsnet was in the final stages of its Phase-A Study.

5. Starting to implement Horizon 2000

The start of the first Cornerstone of Horizon 2000 was recommended at a meeting of the Working Groups and the Space Science Advisory Committee in Darmstadt on 6 and 7 January 1986. Cluster, Kepler and Soho had been presented in competition, and the SSWG recommended

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[21] Vulcan's name is derived from that of a hypothetical planet orbiting the Sun inside Mercury's orbit: the orbiting solar probe will be an artificial 'Vulcan'.

unanimously to start Horizon 2000 by approving STSP as a project and, as far as Phase-A Studies were concerned, that Cassini enter into Phase-A. In preparation for this decision, a workshop on 'Future Missions in Solar, Heliospheric and Space Plasma Physics' was held in early May 1985 in Garmisch-Partenkirchen (Haerendel and Huber 1985). Similar workshops were later held for all Cornerstones. In his opening remarks, Gerhard Haerendel — reflecting on the changing relations, over the years, between Garmisch and Partenkirchen, and alluding to the local license-plate sign 'GAP' — summoned his audience to act in the sense of STSP: "Let us close the gap between in-situ and remote sensing, between plasma physicists and solar astronomers, between the faint nightly glow of the aurora and the brilliant light of the Sun."

A major difficulty faced in starting to implement Horizon 2000 was the large potential cost of the Solar-Terrestrial Science Programme. Although the SSAC had recommended that the canonical envelope of this first Cornerstone, consisting in fact of two projects, be raised by 15%, a serious cost-reduction exercise had to be undertaken. NASA could not make a commitment for a 'new start' for the STSP. A more modest collaboration between ESA and NASA thus had to be elaborated. This eventually comprised the Soho and Cluster spacecraft to be built under ESA's management and with ESA funding and NASA's contributions comprising the launch and the in-orbit operations of Soho as well as the provision of several experiments and extended experiment hardware (like tape recorders, high power amplifiers etc.) for both Soho and Cluster. It could also be agreed that Cluster would be launched for a nominal charge on one of the Ariane-5 test flights. While the spacecraft and mission operations of Cluster were to be taken on by ESA, the scientific operations were to be handled by the participating community, who were to build up a Cluster Science Data System based on electronic networks.

A major descoping exercise involving two committees[22] led to further cost savings by deciding to use four identical (rather than one mother and three daughter) spacecraft in the case of Cluster. For Soho, it was decided to abandon the long antennae that would have enabled wave measurements but would have severely compromised the one-arc-second pointing stability of the spacecraft. Also, no magnetometer was included on Soho, as it would have been difficult and costly to maintain the required magnetic cleanliness with the many mechanisms operating in the big spectroscopic telescopes and coronagraphs. As there was no magnetometer, plasma experiments were not included in the Soho payload either.

In the end, the cost-sharing between Europe and the US for STSP (including the experiments which in Europe are nationally funded) was of the order of 2:1 — a novel ratio since ESA herewith for the first time became senior partner in a joint project with NASA!

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[22] The 'Southwood' and the 'Balsiger' ad-hoc committees.

As mentioned above no partner agency could be found for the Comet Nucleus Sample Return (CNSR) Cornerstone. A descoping exercise of this mission, which obtained the name 'Rosetta', is only now, at the time of writing, converging on a cometary rendez-vous (after a couple of asteriod fly-bys). A sample return thus seems to be excluded; it might be replaced by in-situ investigations of the comet's composition.

Since in the meantime XMM is being implemented as the second Cornerstone, First and Rosetta will become Cornerstones numbers three and four with the sequence to be determined in the second half of 1993.

6. Conclusions

The 'Horizon 2000' exercise, indeed, resulted in a well-balanced programme for solar system sciences. By adding a fourth Cornerstone to the long-term programme it was not only possible to carry out the first solar and heliospheric mission within ESA's Scientific Programme, but also to include the first three-dimensional study of the structure of and processes in space plasma. The combination of these two missions in the STSP now offers the opportunity to perform a comprehensive study of solar-terrestrial physics by in-situ as well as remote-sensing investigations. This 'fourth' Cornerstone actually became the first to be implemented within Horizon 2000. It is also the first joint ESA/NASA undertaking where ESA is the senior partner. The timely implementation affords the associated ESA community the opportunity to participate in a privileged position in the Inter-Agency Solar-Terrestrial Physics programme, the Inter-Agency Consultative Group's follow-on project with ca. 25 spacecraft to be coordinated (IACG 1992).

Further balance to the programme was added with the selection of the Cassini/Huygens mission, i.e. a Saturn orbiter/Titan probe to be flown as a joint NASA/ESA undertaking (with NASA being the senior partner). This was the first mission to a planet (and one of its moons) to be included in ESA's Scientific Programme.

It is interesting to observe that electric propulsion — strongly promoted in the nineteen-eighties — has not had the expected impact: all the missions currently under study, including the comet rendez-vous of Rosetta are based solely on chemical propulsion and gravity assists.

The solar system community is now well served by ESA into the beginning of the next century, as all segments of the community have a share. To obtain this balance was, of course, only possibly owing to the increased funding which in the final analysis may be the most successful consequence of the Horizon 2000 programme as it has helped both solar-system space sciences and astronomy to elaborate a long-term programme which has the support of both communities involved.

Sources and acknowledgements

In preparing the above report, I looked through all the documents I had collected at the time. Only occasionally did I mention personal discussions, of which no verbatim record exists. (It was indeed rather surprising to realise that only ten years ago there were relatively few telephone conversations and that comparatively formal telex messages were still the fast means of communication, rather than today's more transitory electronic-mail and fax transmissions!) I thus hope to have given an accurate and balanced account.

Thanks are due to the Director of Scientific Programmes, R.M. Bonnet, and several of my colleagues, especially H. Balsiger, A.H. Gabriel and J. Geiss, who have generously helped me to clarify matters and to check the record.

Bibliography

Bauer, S., Fulchignoni, M., Ip, W.-H., Langevin, Y., Taylor, F.W. and von Zahn, U. 1984, Report of ESA's Topical Team on Planetary Science, chapter 5 in Longdon and Olthof (1984), pp. 50-58.

Bleeker, J. and Bonnet, R.M. 1984, 'European Space Science — Horizon 2000, Report of the Survey Committee' (Paris: European Space Agency), with cover letter D.Sci/RMB/ga/6961 of 8 August 1984.

Bleeker, J., Fechtig, H., Fredga, K., Gabriel, A.H., Huber, M.C.E., Lequeux, J., Pacini, F., Völk, H.J., Geiss, J., van Hove, L., Lefevbre, M., Hultqvist, B., de Jager, C., Setti, G.C. and West, R.M. 1984, 'Actions, Proposals, Conclusions — Survey Committee Report', chapter 1 in Longdon and Olthof (1984), pp. 2-15.

Chicarro, A.F., Scoon, G.E.N. and Coradini, M. 1983, 'Mission to Mars — Report of the Mars Exploration Study Team', ed. B. Battrick, ESA SP-1117 (Paris: European Space Agency), 138 pp.

Delache, Ph., Hoyng, P., Christensen-Dalsgaard, J., Priest, E.R., Schwenn, R. and Stenflo, J.O. 1984, 'Report of ESA's Topical Team on Solar and Heliospheric Physics', chapter 3 in Longdon and Olthof (1984), pp. 26-36.

Domingo, V. 1989, ed., 'The SOHO Mission — Scientific and Technical Aspects of the Instruments', ESA SP-1104 (Paris: European Space Agency).

Geiss, J. 1993, personal communication.

Haerendel, G., Dobrowolny, M., Eliasson, L., Gendrin, R., Johnstone, A., McKenna, S., Morfill, G. and Vasyliunas, V. 1984, 'Report on ESA's Topical Team on Space Plasma Physics', chapter 4 in Longdon and Olthof (1984), pp. 38-48.

Haerendel, G. and Huber, M.C.E. 1985, 'Future Mission in Solar, Heliospheric and Space Plasma Physics', Proc. ESA Workshop Garmisch-Partenkirchen, 30 April-3 May 1985, eds. E. Rolfe and B. Battrick, ESA SP-235 (Paris: European Space Agency), 256 pp.

Haskell, G.P. 1982, ed., 'Kepler — an Interdisciplinary Mars Orbiter Mission', Report on the Phase-A Study, SCI (83)5 (Paris: European Space Agency).

Hoyng, P., Andersen, B., Bertotti, B., Coradini, M., Cornelisse, K., Richter, A.K., Roxburgh, I.W. Schüssler, M., Stenflo, J.O., Wenzel, K.P. 1988, 'The Vulcan Mission', Interime Report of ESA Science and Technology Definition Team, SCI (88) 7 (Paris: European Space Agency).

Huber, M.C.E. and Malinovsky-Arduini, M. 1992, 'The Soho Concept and its Realisation', Space Sci. Rev. **61**, 301-334.

Huber, M.C.E. 1993, *Solar Physics within the Planning of ESA*, in 'Mini-Workshop on Solar Physics Requirements for Future Space Missions', ed. P. Maltby, ESA SP-1157 (Paris: European Space Agency), in press.

IACG 1992, 'Understanding the Sun-Earth Environment — Multiple Mission Campaign Coordination through the Inter-Agency Consultative Group for Space Science' (Paris: European Space Agency).

Longdon, N. and Olthof, H. 1984, eds., 'Space Science — Horizon 2000', ESA SP-1070 (Paris: European Space Agency).

Schmidt, R. 1988, ed., 'The Cluster Mission — Scientific and Technical Aspects of the Instruments', ESA SP-1103 (Paris: European Space Agency).

Schwehm, G.H. and Langevin, Y., Ahrens, T.J., Atzei, A., Grün, E., McDonnell, J.A.M., Sekanina, Z. and Stöffler, D. 1991, 'Rosetta/CNSR — a Comet-Nucleus Sample Return Mission', eds. E. Rolfe and B. Battrick, ESA SP-1125.

Space Science Board (NRC/NAS) and Space Science Committee (ESF) 1986, 'United States and Western Europe Cooperation in Planetary Cooperation', Report of the Joint Working Group on Cooperation in Planetary Exploration (Washington, D.C.: National Academy Press).

SPC 1984, Minutes of the thirty-sixth meeting of the Science Programme Committee held in Paris on 12 and 13 March 1984, ESA/SPC/MIN/36 (Paris: European Space Agency), p. 8.

SSAC 1983, Minutes of the 32nd meeting of the Space Science Advisory Committee held in Frascati on 12 and 13 October 1983, SSAC/MIN/32 (Paris: European Space Agency).

SSAC 1984, Minutes of the 34nd meeting of the Space Science Advisory Committee held in Paris on 29 February 1984, SSAC/MIN/34 (Paris: European Space Agency), p. 6.

SSAC 1985, Minutes of the 39nd meeting of the Space Science Advisory Committee held on 14 and 15 March 1985 at ESRIN, Frascati , SSAC/MIN/39 (Paris: European Space Agency), item 5.

SSWG 1983, Report on the 44th Meeting of the Solar System Working Group held on 12 October 1983 at ESRIN, Frascati, SOL(83)9 (Paris: European Space Agency).

SSWG 1984, Report on the 45th Meeting of the Solar System Working Group held on 27/28 February 1984 at ESA Head Office, Paris, SOL(84)7 (Paris: European Space Agency), p. 2.

SSWG 1985, Report on the 48th Meeting of the Solar System Working Group held on 21/22 May 1985 at ETH, Zürich, SOL(85)4 (Paris: European Space Agency), item 9.

Table 1. The Solar System Working Group before the Horizon-2000 exercise was started

Solar Physics	4
Particles and Fields	4
Planetary Sciences, Solid Bodies	4
Atmospheres	4
	16

Chair: A.H. Gabriel
Secretary: G.P. Haskell

Table 2. The Survey Committee

SSAC	J. Bleeker, Chair	(NL)
	H. Fechtig	(D)
	K. Fredga	(S)
	A.H. Gabriel	(GB/F)*
	M.C.E. Huber, SSWG Chair	(CH)
	J. Lequeux	(F)
	F. Pacini, AWG Chair	(I)
	H.J. Völk	(D)
SPC	C. de Jager	(NL)
EOAC	M. Lefebvre	(F)
ESF/SSC	J. Geiss	(CH)
	B. Hultqvist	(S)
CERN	L. Van Hove	(B)
ESO	G.C. Setti	(I)
IAU	R.M. West	(DK/ESO)

(Secretary: V. Manno)

The abbreviations used are:

SSAC: Space Science Advisory Committee
SPC: Scientific Programme Committee
EOAC: Earth Observation Advisory Commitee
ESF/SSC: European Science Foundation/Space Sciences Committee
CERN: Conseil Européen pour la Recherche Nucléaire
ESO: European Southern Observatory
IAU: International Astronomical Union

* A.H. Gabriel joined the SSAC while he was working in the United Kingdom, but later took up a position in France.

Table 3. Summary of mission concepts

SOLAR SYSTEM MISSION CONCEPTS	
Solar Physics	6
Heliospheric Physics	3
Magnetospheric and Plasma Physics	8
Planetology	8
Comets and Small Bodies	5
The Solid Earth	1
Atmospheric and Plasma	1
ASTRONOMY MISSION CONCEPTS	
Gamma Ray Astronomy	3
X-Ray Astronomy	11
UV Astronomy	8
Astronomy in the Visible	2
Infrared Astronomy	2
Interferometry	4
Others	2

NOTE: Some concepts suggested re-examination of earlier proposals or were based on missions already under study.

Table 4. Topical teams for solar system science

- SOLAR AND HELIOSPHERIC PHYSICS

Chair:	Ph. Delache/P. Hoyng*
Members:	J. Christensen-Dalsgaard
	E.R. Priest
	R. Schwenn
	J.O. Stenflo

- SPACE PLASMA PHYSICS

Chair:	G. Haerendel
Members:	M. Dobrowolny
	J. Eliasson
	R. Gendrin
	A.D. Johnstone
	S. McKenna-Lawlor
	G. Morfill
	V. Vasyliunas

- PLANETARY SCIENCE (SOLID BODIES AND ATMOSPHERES)

Chair:	S.J. Bauer
Members:	M. Fulchignoni
	W.-H. Ip
	Y. Langevin
	F.W. Taylor
	U. von Zahn

* P. Hoyng replaced Ph. Delache, who had to resign for health reasons, in February 1984.

Figure captions

Figure 1

International Solar Terrestrial Physics Programme as elaborated at the trilateral ESA/NASA/ISAS preparatory meeting on Solar Terrestrial Science, held in Washington on 26 and 27 September 1983. The two last missions, designated (ESA) Solar and Multipoint, are the equivalent of Soho and Cluster.

Figure 2

Schedule of studies as recommended by the Space Science Advisory Committee in Octoebr 1984. (The abbreviations XMM, First, Quasat and Columbus stand for then proposed astronomy missions, namely the X-ray Multi-Mirror Mission, the Far Infrared Space Telescope, a Very-Long Baseline Interferometry (VLBI) mission and a far ultraviolet observatory mission, which later became Lyman (FUSE). Note that the mission labelled Titan is now Cassini/Huygens. The Technical Studies on Ion Propulsion have had the expected importance, since none of the currently planned planetary missions depends on an ion drive.

Figure 3

Missions in Solar System Science: the long term plans elaborated by the three Topical Teams

Figure 4

The original schedule for Horizon 2000 (from Bleeker and Bonnet 1984)

Figure 5

The European Long-Term Programme as first presented in Bleeker and Bonnet (1984)

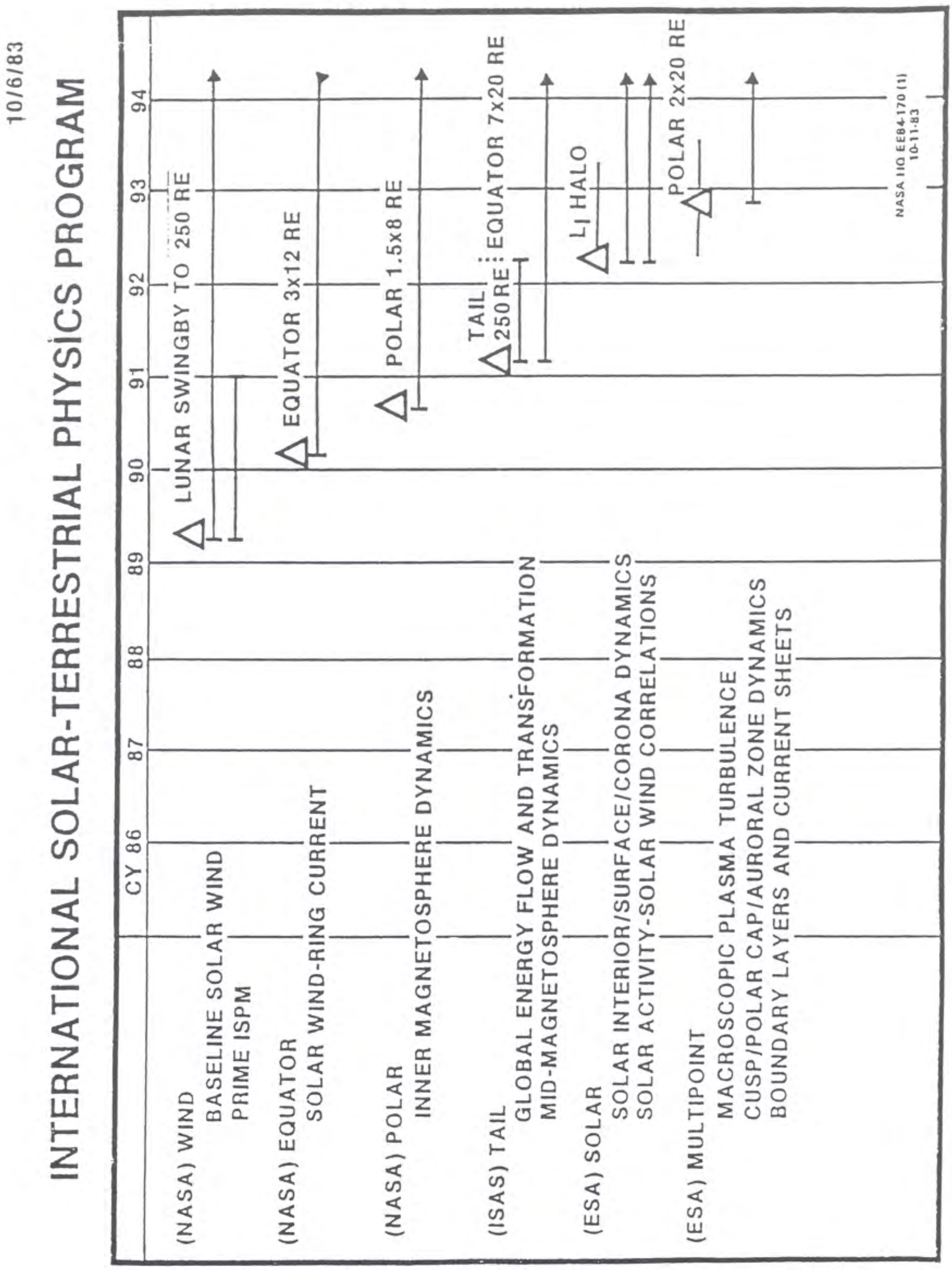
10/6/83
INTERNATIONAL SOLAR-TERRESTRIAL PHYSICS PROGRAM
CY 86
87
88
89
90
91
92
93
94
(NASA) WIND
BASELINE SOLAR WIND
PRIME ISPM
LUNAR SWINGBY TO 250 RE
(NASA) EQUATOR
SOLAR WIND-RING CURRENT
EQUATOR 3x12 RE
(NASA) POLAR
INNER MAGNETOSPHERE DYNAMICS
POLAR 1.5x8 RE
(ISAS) TAIL
GLOBAL ENERGY FLOW AND TRANSFORMATION
MID-MAGNETOSPHERE DYNAMICS
TAIL 250 RE
EQUATOR 7x20 RE
(ESA) SOLAR
SOLAR INTERIOR/SURFACE/CORONA DYNAMICS
SOLAR ACTIVITY-SOLAR WIND CORRELATIONS
L1 HALO
(ESA) MULTIPOINT
MACROSCOPIC PLASMA TURBULENCE
CUSP/POLAR CAP/AURORAL ZONE DYNAMICS
BOUNDARY LAYERS AND CURRENT SHEETS
POLAR 2x20 RE
NASA HQ EE84-170 (1)
10-11-83

Figure 1

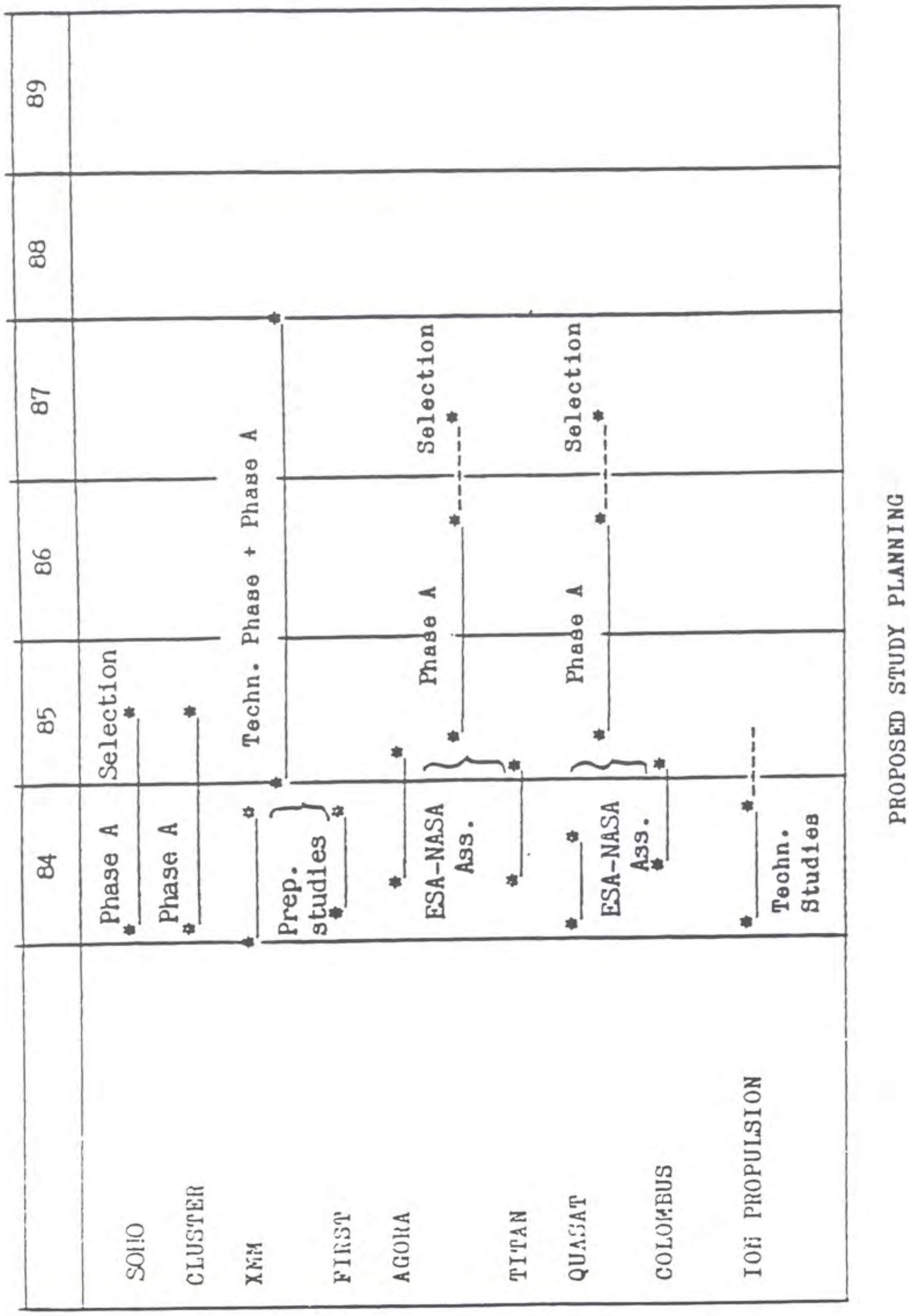
PROPOSED STUDY PLANNING
84
85
86
87
88
89
SOHO
CLUSTER
XMM
FIRST
AGORA
TITAN
QUASAT
COLOMBUS
ION PROPULSION
Phase A
Selection
Phase A
Techn. Phase + Phase A
Prep. studies
ESA-NASA Ass.
Phase A
Selection
ESA-NASA Ass.
Phase A
Selection
Techn. Studies

Figure 2

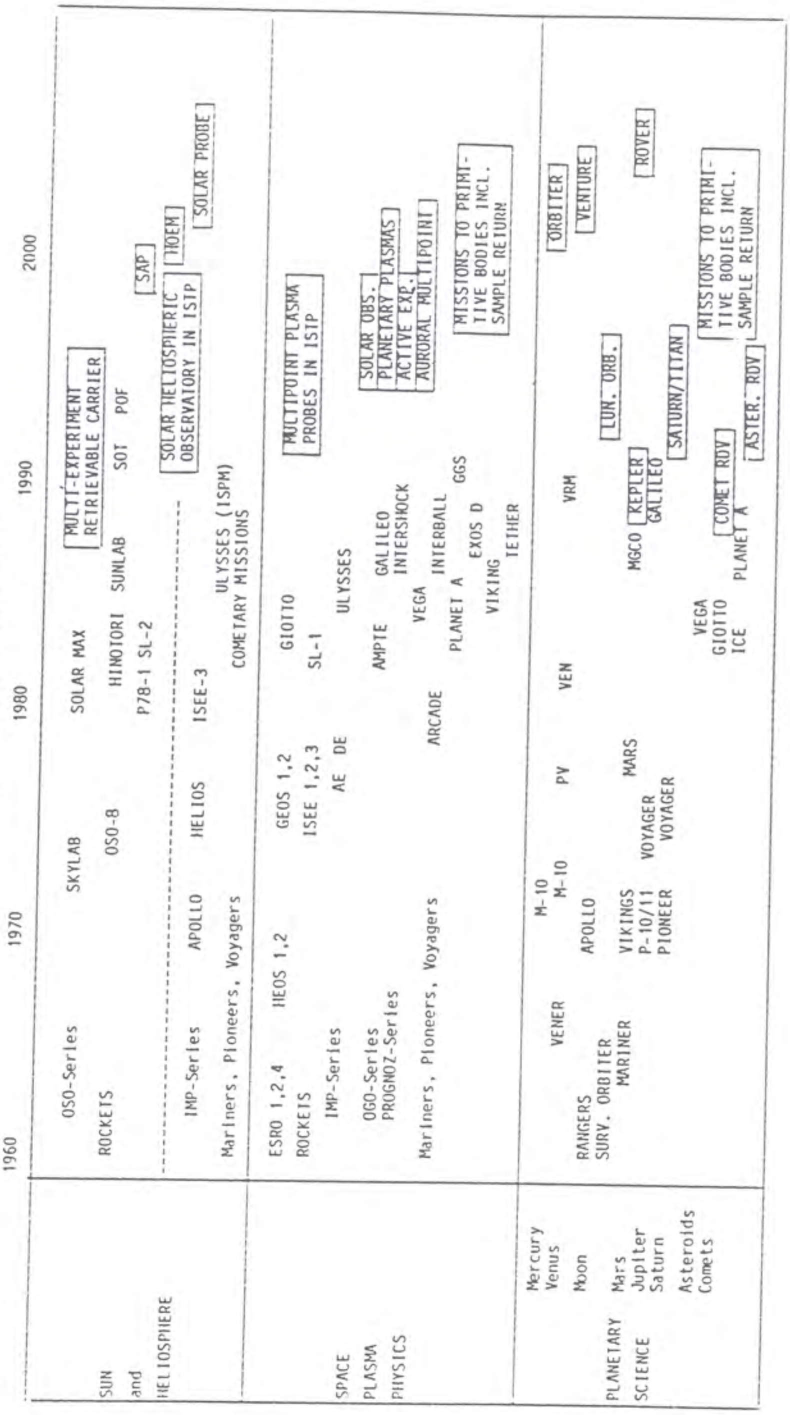
FIGURE 1 : MISSIONS IN SOLAR SYSTEM SCIENCE
1960
1970
1980
1990
2000
SUN and HELIOSPHERE
OSO-Series
ROCKETS
SKYLAB
OSO-8
SOLAR MAX
HINOTORI
P78-1 SL-2
SUNLAB
SOT POF
MULTI-EXPERIMENT RETRIEVABLE CARRIER
SAP
IMP-Series
APOLLO
HELIOS
ISEE-3
ULYSSES (ISPM)
SOLAR HELIOSPHERIC OBSERVATORY IN ISTP
HOEM
SOLAR PROBE
Mariners, Pioneers, Voyagers
COMETARY MISSIONS
SPACE PLASMA PHYSICS
ESRO 1,2,4
ROCKETS
HEOS 1,2
IMP-Series
OGO-Series
PROGNOZ-Series
GEOS 1,2
ISEE 1,2,3
AE DE
GIOTTO
SL-1
ULYSSES
AMPTE
GALILEO
INTERSHOCK
VEGA
INTERBALL
ARCADE
PLANET A
EXOS D
VIKING
TETHER
GGS
MULTIPOINT PLASMA PROBES IN ISTP
SOLAR OBS.
PLANETARY PLASMAS
ACTIVE EXP.
AURORAL MULTIPOINT
MISSIONS TO PRIMITIVE BODIES INCL. SAMPLE RETURN
Mariners, Pioneers, Voyagers
PLANETARY SCIENCE
Mercury
Venus
Moon
Mars
Jupiter
Saturn
Asteroids
Comets
RANGERS
SURV. ORBITER
MARINER
VENER
APOLLO
M-10
VIKINGS
P-10/11
PIONEER
PV
VOYAGER
VOYAGER
MARS
VEN
VEGA
GIOTTO
ICE
MGCO
VRM
KEPLER
GALILEO
PLANET A
COMET RDV
LUN. ORB.
SATURN/TITAN
ASTER. RDV
ORBITER
VENTURE
ROVER
MISSIONS TO PRIMITIVE BODIES INCL. SAMPLE RETURN

Figure 3

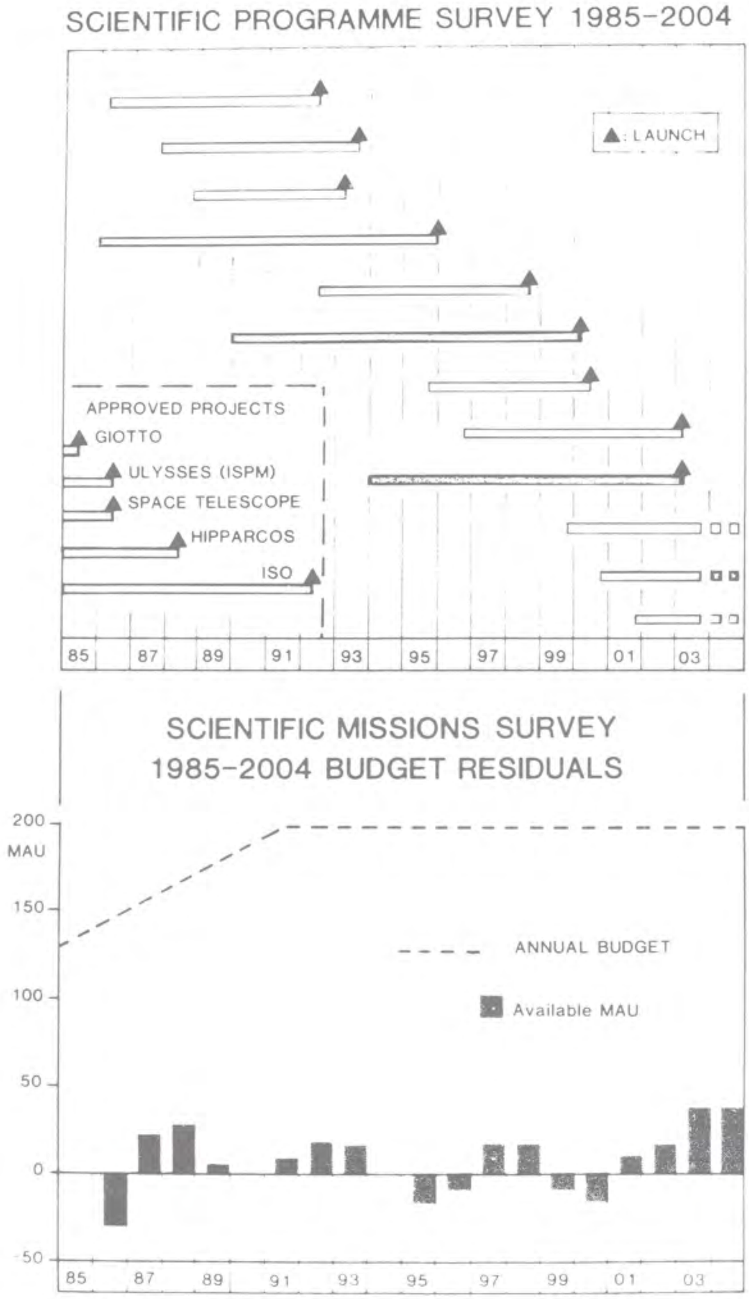
SCIENTIFIC PROGRAMME SURVEY 1985-2004
▲: LAUNCH
APPROVED PROJECTS
GIOTTO
ULYSSES (ISPM)
SPACE TELESCOPE
HIPPARCOS
ISO
85
87
89
91
93
95
97
99
01
03
SCIENTIFIC MISSIONS SURVEY
1985-2004 BUDGET RESIDUALS
200
MAU
150
100
50
0
-50
ANNUAL BUDGET
Available MAU
85
87
89
91
93
95
97
99
01
03

Figure 4

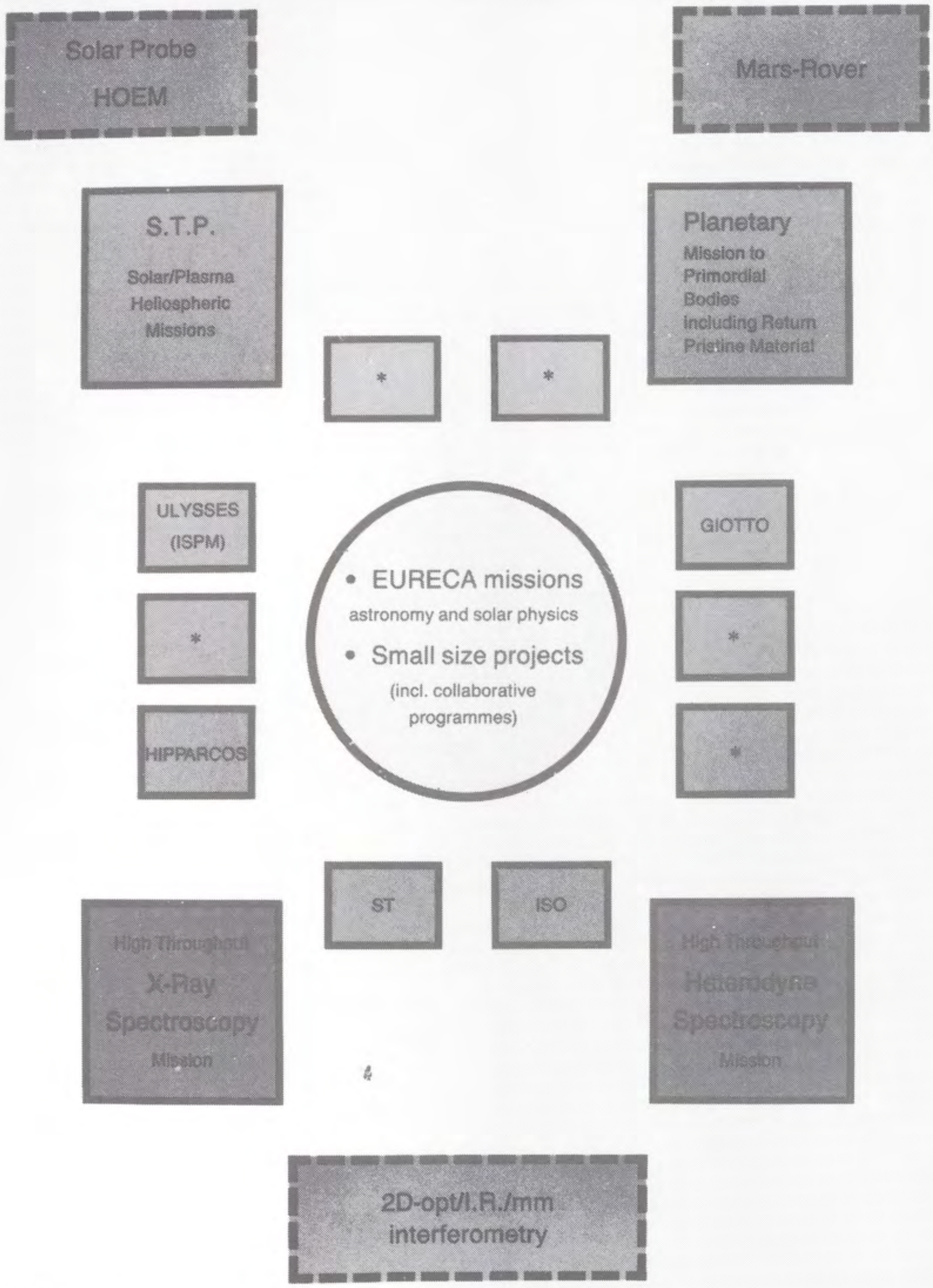

FIGURE 3 : THE EUROPEAN LONG TERM PROGRAMME

[*e.g. : - Solar Heliospheric (Soho), Multipoint Probes (Cluster), Auroral Multiprobes, etc., in Solar/Heliospheric/Plasma Physics; - Venus (Venture), Mars (Kepler), Lunar (Selene) Orbiters, etc., in Planetary Science; - Space VLBI, UV Spectroscopy, Stellar Seismology, etc., in Astronomy.]

16

Figure 5